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

CHARLES HUTTON, LL D. F.R.S.
GEORGE SHAW, M.D. F.R.S. F.L.S.
RICHARD PEARSON, M.D. F.S.A.

VOL. XII.

FROM 1763 TO 1769.

LONDON:
PRINTED BY AND FOR C. AND R. BALDWIN, NEW BRIDGE-STREET, BLACKFRIARS.
1809.

CONTENTS OF VOLUME TWELFTH.

THE CONTENTS CLASSED UNDER GENERAL HEADS.

Class I. Mathematics.

1. *Arithmetic, Annuities, Political Arithmetic.*

2. *Algebra, Analysis, Fluxions.*

3. *Geometry, Trigonometry, Land-surveying.*

Class II. Mechanical Philosophy.

1. *Dynamics.*

2. *Astronomy, Chronology, Navigation.*

3. *Hydrostatics.*

4. *Pneumatics.*

5. *Optics.*

6. *Electricity, Magnetism, Pyrotechny, Thermometry, &c.*

Class III. Natural History.

1. *Zoology.*

2. *Botany.*

3. *Mineralogy.*

4. *Geography, and Topography.*

5. *Hydrology.*

Class IV. Chemical Philosophy.

1. *Chemistry.*

2. *Meteorology.*

Class VI. The Arts.

1. *Mechanical.*

2. *Chemical.*

3. *Antiquities.*

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

REFERENCES TO THE PLATES IN VOLUME XII.

THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON:

ABRIDGED.

XXXII. On the Success of the Bark of the Willow in the Cure of Agues. By the Rev. Edm. Stone, of Chipping-Norton, Oxfordshire. Dated April 25th, 1763. p. 195.

About 6 years prior to the above date, Mr. S. tasted the willow bark, and was surprised at its extraordinary bitterness, which immediately raised in him a suspicion of its having the properties of the Peruvian bark. As this tree delights in a moist or wet soil, where agues chiefly abound, the general maxim that many natural maladies carry their cures along with them, or that their remedies lie not far from their causes, was so very apposite to this particular case, that he could not help applying it; and that this might be the intention of Providence he owns had some weight with him. The plenty of this bark furnished him, in his speculative disquisitions on it, with an argument both for and against these imaginary qualities of it; for on one hand, as intermittents are very common, it was reasonable to suppose, that what was designed for their cure should be as common and as easy to be procured. But then, on the other hand, it seemed probable, that if there was any considerable virtue in this bark, it must have been discovered from its plenty. His curiosity prompted him to look into the dispensatories and books of botany, and examine what they said concerning it; but there it existed only by name. He could not find, that it ever had any place in pharmacy, or any such qualities as he suspected, ascribed to it by the botanists.

However he determined to make some experiments with it; and for this purpose he gathered that summer near 1 lb. weight of it, which he dried in a bag, on the outside of a baker's oven, for more than 3 months, at which time it was to be reduced to a powder by pounding and sifting, after the manner that other barks are pulverised. It was not long before he had an opportunity of making a trial of it; but being an entire stranger to its nature, he gave it in very small quantities, he thinks it was about 20 grains of the powder at a dose, and repeated it every 4 hours between the fits; but with great caution, and the strictest attention to its effects; the fits were considerably abated, but did not entirely cease. Not perceiving the least ill consequences, he became bolder with it, and in a few days increased the dose to 2 scr. and the ague was soon removed. It was then given to several others with the same success, but he found it better answered the intention, when 1 dram of it was taken every 4 hours in the intervals of the paroxysms.

He had continued to use it with success, as a remedy for agues and intermitting disorders for 5 years successively. It had been given he believed to 50 persons, and never failed in the cure, except in a few autumnal and quartan agues, with which the patients had been long and severely afflicted; these it reduced in a great degree, but did not wholly take them off; the patient, at the usual time for the return of his fit, felt some smattering of his distemper, which the incessant repetition of these powders could not conquer: it seemed as if their power could reach thus far and no farther; and he supposed that it would not have long continued to reach so far, and that the distemper would have soon returned with its pristine violence; but he did not stay to see the issue: he added one-fifth part of the Peruvian bark to it, and with this small auxiliary it totally routed its adversary. It was found necessary also, in one or two obstinate cases, at other times of the year, to mix the same quantity of that bark with it; but these were cases where the patient went abroad imprudently, and caught cold, as a post-chaise boy did, who being almost recovered from an inveterate tertian ague, would follow his business; by which means he not only neglected his powders, but meeting with bad weather renewed his distemper.

One-fifth part was the largest, and indeed the only proportion of the quinquina made use of in this composition, and this only on extraordinary occasions; the patient was never prepared, either by vomiting, bleeding, purging, or any medicines of a similar intention, for the reception of this bark, but he entered upon it abruptly and immediately, and it was always given in powders, with any common vehicle, as water, tea, small beer, and such like. This was done purely to ascertain its effects; and that he might be assured the changes wrought in the patient could not be attributed to any other thing: though, had there been a due preparation, the most obstinate intermittents would probably have yielded to this bark without any foreign assistance: and by all he could judge from 5 years experience of it on a number of persons, it appeared to be a powerful absorbent, astringent, and febrifuge in intermitting cases, of the same nature and kind with the Peruvian bark, and to have all its properties, though perhaps not always in the same degree. It seemed likewise to have this additional quality, viz. to be a safe medicine, for he never could perceive the least ill effect from it, though it had been always given without any preparation of the patient.

The tree from which this bark was taken, is stiled by Ray, in his Synopsis, salix alba vulgaris, the common white willow. Hæc omnium nobis cognitarum maxima est, et in satis crassam et proceram arborem adolescit. It is called in these parts by the common people, the willow, and sometimes the Dutch willow; but if it be of a foreign extraction, it has been so long naturalized to this climate, that it thrives as well in it as if it was in its original soil. It is easily distinguished by the notable bitterness, and the free running of its bark, which may be readily separated from it all the summer months, while the sap is up. He took it from the shoots of 3 or 4 years growth, that sprung from pollard trees, the diameters of which shoots, at their larger end, were from 1 to 4 or 5 inches: it is possible, and indeed not improbable, that this cortex, taken from larger or older shoots, or from the trunk of the tree itself, may be stronger; but he had not had time nor opportunities to make the experiments which ought to be made upon it. The bark he had, was gathered in the northern parts of Oxfordshire, which are chiefly of dry and gravelly nature, affording few moist or moory places for this tree to grow in; and therefore, he suspected that it was not so good here as in some other parts. Few vegetables are equal in every place; all have their peculiar soils, where they arrive to a greater perfection than elsewhere: the best and strongest mustard-seed is gathered in the county of Durham; the finest saffron-flowers are produced in some particular spots of Essex and Cambridgeshire; the best cyder-apples grow in Herefordshire, Devonshire and the adjacent counties; the roots of valerian are esteemed most medicinal which are dug up in Oxfordshire and Gloucestershire: and therefore why may not the cortex salignus, or cortex anglicanus, have its favourite soil, where it may flourish most, and attain to its highest perfection? It is very probable that it has; and perhaps it may be in the fens of Lincolnshire, Cambridgeshire, Essex, Kent, or some such like situations; and though the bark which grew in the county of Oxford, may seem in some particular cases to be a little inferior to the quinquina, yet in other places it may equal, if not exceed it. The powders made from this bark are at first of a light brown, tinged with a dusky yellow; and the longer they are kept, the more they incline to a cinnamon or lateritious colour, which he believed was the case with the Peruvian bark and powders.

XXXIII. Of an Earthquake in Siberia. In a Letter from Mons. Weymarn to Dr. Mounsey, Principal Physician of the Emperor of Russia, F.R.S. Translated from the French. p. 201.

This earthquake, or rather these several earthquakes, happened in the months of October, November, and December, 1761. They shook the buildings a little, but did no damage. It is said that such earthquakes happen there frequently, almost every year; and the inhabitants, being thus accustomed to them, take little notice of them.

XXXIV. Roman Inscriptions at Tunis in Africa, copied about the Year 1730 by Dr. Carilos, a Native of Madrid, then Physician to the Bey of Tunis. Communicated by John Locke, Esq. F. R. S. p. 211.

These numerous Roman inscriptions are in honour of different Roman emperors, and other distinguished persons.

XXXV. A Letter from Mr. George Edwards, F. R. S. to Thomas Birch, D. D., Sec. R. S. concerning an Observation made by him in Optics. p. 229.

Having lately accidentally discovered that the shadows of things floating in water, a little below its surface, are reflected from the air above the water more strongly than objects above the surface of the water are reflected from the water, and consequently, that fishes playing beneath the surface of a still water may see their images distinctly playing in the air, with this advantage over men who view their faces in the water; for things in air that are reflected from the water, must have, when placed over the water, their dark or shadowed sides reflected from it, which renders the images obscure. On the contrary, the inhabitants of the waters have almost a hemisphere of light falling on their upper sides, which are the sides that are reflected from the air, which consequently renders such images lighter, and more striking to the eye, than reflections of obscured things in air, when reflected from the water. As I have never heard of, or read, any account of this discovery, I imagine it may be new; but you, Sir, in far more extensive reading, may be acquainted with such a discovery.

XXXVI. Two Remarkable Cases in Surgery. By Mr. Francis Geach, Surgeon, Plymouth.* p. 231.

XXXVII. Of a New Dye, from the Berries of a Weed in South Carolina. By Mr. Moses Lindo, dated at Charles Town, Sept. 2, 1763. p. 238.

In August 1757, Mr. L. observed the mocking bird fond of a berry, which grows on a weed called pouck, represented to him as of a poisonous quality; the juice of this berry being a blooming crimson. He was several times inclined to try if he could extract a dye from it; yet the very thoughts of its quality prevented him from proceeding, till observing these birds to void their excrement of the same colour as the berry on the Chinese rails in his garden, convinced him it was not of the quality represented. He therefore made a trial in the following

* May be consulted in the vol. of Medical and Surgical Observations, published by this author, in 1768.

manner. 1st. He ordered one of his negroes to gather him a pint of those berries, from which he extracted almost ¾ of a pint of a juice, and boiled it with a pint of Bristol water, ¼ of an hour. 2dly. He then took 2 pieces of flannel, and numbered them 1 and 2, boiled them in a separate tin-pot with alum ¼ of an hour, and rinced them in cold water. 3dly. He then dipped the piece of flannel N° 1 into the pot where the juice was, and left it to simmer 5 minutes; then took it out, and rinced it in cold water, when, to his surprise, he found a superior crimson dye fixed on the flannel than the juice of the berry. 4thly. He then dipped the piece of flannel N° 2 in the same juice, and being desirous to clean his hands from the stain which N° 1 had caused, he ordered some lime water to be brought him, such as he used to settle the indigo, and found the colour of the stain change to a bright yellow. This unexpected change urged him to throw a wine glass full of lime water into the pot, where the piece of flannel N° 2 was simmering; on which all the juice, as well as the flannel, became of a bright yellow; by which he found alum fix the crimson, and lime the yellow. 5thly. Having then put a quart of fresh juice in 2 pint decanters, in one of which he put a small quantity of powdered alum, he laid them up; about 6 weeks after he examined them, and found the juice in the decanter, which had no alum, was turned black, and the other retained its colour.

XXXVIII. On the Eclipse of the Sun, April 1, 1764. *By Mr. James Ferguson, F. R. S.* p. 240.

This was a projection of the eclipse of the sun, which was to happen on the 1st of April 1764. The diagram shows the time and phases of that eclipse, for the Royal Observatory at Greenwich, and the calculation is from Meyer's tables. As these tables gave the appearance and the times very different from those of Flamsteed, Halley, and La Caille, Mr. F. offers this projection as a means of proving which tables are the more accurate.

If the motions of the sun and moon were equable, any given eclipse would always return in a course of 223 lunations, which would consist of 18 years 11 days 7 hours 43 minutes 20 seconds (as was observed by the ancients) for 1388 years; and would for ever do so, if, at the end of each period, the sun and moon should be in conjunction either in the same node, or at the same distance from it as before. But that is not the case: for if the sun and moon are once in conjunction at 18° distance from the node, which is the greatest distance at which the moon's shadow can touch the earth, at the next period of 18 years 11 days, &c. the sun and moon will be 28′ 12″ of a degree nearer the same node than they were at the period last before. And so by falling gradually nearer and nearer the same node every time, the moon's shadow will pass over the centre of the earth's enlightened disk, at the end of the 38th periodical return of the eclipse, from the time of its first coming in at either of the earth's poles; because the conjunction falls in the node at the end of the 38th period. In each succeeding period, the conjunctions of the sun and moon will be gradually farther and farther from the node, by the quantity of 28′ 12″; which will cause the moon's shadow to pass over the disk of the earth farther and farther on the opposite side from its centre, till it quite leaves the earth, and travels in expansion for about 12,492 years, before it can come upon the earth again at the same pole as before.

The reason of this will be plain, when we consider, that 18° from either of the nodes of the moon's orbit is the greatest distance at which her shadow can touch the earth at either of its poles. And as there are 18° on each side of the node, within the limits of a solar eclipse; and twice 18 make 36, these are all of the 360 degrees of the moon's orbit about either of the nodes, within which there can be an eclipse of the sun: and as these eclipses shift through 28′ 12″ of these 36 degrees, in every Chaldean or Plinian period, they will shift through the whole limit in 77 periods, which include 1388 years and 3 months. And then the periods have the remaining 324 degrees of the moon's orbit to shift through, at the rate of only 28′ 12″ in each period, before they can be near enough to the same node again, for the moon's shadow to touch the earth; and this cannot be gone through in less than 12,492 years; for as 36 is to 1388, so is 324 to 12,492.

The eclipse, April 1st, 1764, fell in the open space, quite clear of the earth at each return, ever since the creation, till A. D. 1295, June 13th, old stile, at 12^h 52^m 59^s P. M. when it touched the earth at the north pole, according to the mean (or supposed equable) motions of the sun and moon; their conjunction being then 17° 48′ 27″ from the moon's ascending node, in the northern part of her orbit. In each period since that time, the conjunction of the sun and moon has been 28′ 12″ nearer and nearer the same node, and the moon's shadow has therefore gone more and more southerly over the earth. In the year 1962, July 18th old stile, at 10^h 36^m 21^s P. M. the same eclipse will have returned 38 times; and as the conjunction will then be only 24′ 45″ from the node, the centre of the moon's shadow will fall but a little northward of the centre of the earth's enlightened disk. At the end of the next following period, the conjunction of the sun and moon will have receded back 3′ 27″ from the moon's ascending node, into the southern part of her orbit; which will cause the centre of her shadow to pass a very small matter south of the centre of the earth's disk. After which in every following period, the conjunction of the sun and moon will fall 28′ 12″ farther and farther back from the node, and the moon's shadow will go still farther and farther southward on the earth, till A. D. 2665, September 12, old stile, at 23^h 46^m 22^s P. M. when the eclipse will have finished its 77th period, and will finally leave the earth at the south pole; and cannot begin the same course over the earth again in less than 12,492 years, as above mentioned. And thus, if the motions of the sun and moon were equable, the same eclipse would always return in 18 Julian years, 11 days, 7 hours, 43 minutes, 20 seconds, when the last day of February in leap years is 4 times included in the period; but when it is 5 times included, the period is one day less, or 18 years, 10 days, 7 hours, 43 minutes, 20 seconds.

But on account of the various anomalies of the sun and moon, arising from their moving in eliptic orbits, and the sun's different attractions of the moon in different parts of her orbit, the conjunctions of the sun and moon never succeed one another at equal intervals of time; but differ sometimes by no less than 14, 15, or 16 hours: and therefore, in order to know the true times of the returns of any eclipse, recourse must be had to long and tedious calculations. In order to show both the mean and true times of the above mentioned eclipse, through all its periods, while it is visible on this earth, together with the mean anomalies of the sun and moon, the true distance of each conjunction from the ascending node, with the true latitude of the moon at the time of each of her true conjunctions with the sun, according to the old stile, Mr. F. calculated the 4 following tables.

According to the mean (or supposed equable) motions of the sun, moon, and nodes, the moon's shadow in this eclipse would have first touched the earth at the north pole, on the 13th of June A. D. 1295; and would quite leave the earth at the south pole, on the 12th of September, A. D. 2665, at the completion of its 77th period; as shown in the first and 2d tables. But on account of the true (or unequable) motions of the sun, moon, and nodes, the true lines of conjunctions of the sun and moon, and the sun's true distance from the moon's ascending node, are as set down in the 3d and 4th tables; and the moon's true latitude is too great at the end of the first mean period, to allow her shadow to touch the earth. So that the first time of the coming-in of this eclipse was at the end of its 2d mean period; and the true time was on the 24th of June, A. D. 1313, at $3^h\ 57^m\ 3^s$ past noon at London: and it will finally leave the earth on the 31st of July, A. D. 2593, at $10^h\ 25^m\ 31^s$ past noon, at the completion of its 72d period. So that the true motions do not only alter the true times from the mean, but they also cut off 5 periods from those of the mean returns of this eclipse.

In this and all other eclipses of the sun, which happen about the ascending node of the moon's orbit, the moon's shadow first touches the earth at or about the north pole; and goes more and more southerly over the earth in each return, till it quite leaves the earth at or near the south pole. But when eclipses happen about the descending node, (as that of July 14th A.D. 1748 did) the moon's shadow first touches the earth at or near the south pole; and goes gradually more and more northward in each periodical return, till it finally leaves the earth at the north pole. And as the obliquity of the moon's orbit to the ecliptic is the same about both the nodes, there must be the same number of eclipses about the one as about the other.

TABLE I.

The mean time of new moon, with the mean anomalies of the sun and moon, and the sun's mean distance from the moon's ascending node, at the mean time of each periodical return of the sun's eclipse, March 21st, 1764, from the time of its first coming upon the earth since the creation, till it falls right against the earth's centre, according to the old style.

Periods.	Years of Christ.	Mean time of new moon.	Sun's mean anomaly.	Moon's mean anomaly.	Sun's mean distance from the node.
0	1277	June 2d 5h 9m 39s	11s 17° 57′ 41″	1s 26° 31′ 42″	0s 18° 16′ 40″
1	1295	June 13 12 52 58	11 28 27 38	1 23 40 19	0 17 48 27
2	1313	June 23 20 36 19	0 8 57 35	1 20 48 56	0 17 20 15
3	1331	July 5 4 19 39	0 19 27 32	1 17 57 33	0 16 52 2
4	1349	July 15 12 2 59	0 29 57 29	1 15 6 10	0 16 23 50
5	1367	July 26 19 46 19	1 10 27 26	1 12 14 47	0 15 55 37
6	1385	Aug. 6 3 29 39	1 20 57 23	1 9 23 24	0 15 27 25
7	1403	Aug. 17 11 12 59	2 1 27 20	1 6 32 1	0 14 59 12
8	1421	Aug. 27 18 56 19	2 11 57 17	1 3 40 38	0 14 31 0
9	1439	Sept. 8 2 39 39	2 22 27 14	1 0 49 15	0 14 2 47
10	1457	Sept. 18 10 2 59	3 2 57 11	0 27 57 52	0 13 34 35
11	1475	Sept. 29 18 6 19	3 13 27 8	0 25 6 29	0 13 6 22
12	1493	Oct. 10 1 49 39	3 23 57 5	0 22 15 6	0 12 38 10
13	1511	Oct. 21 9 32 59	4 4 27 2	0 19 23 43	0 12 9 57
14	1529	Oct. 31 17 16 19	4 14 56 59	0 16 32 20	0 11 41 45
15	1547	Nov. 12 0 59 40	4 25 26 56	0 13 40 57	0 11 13 32
16	1565	Nov. 22 8 43 0	5 5 56 53	0 10 49 34	0 10 45 20
17	1583	Dec. 3 16 26 20	5 16 26 50	0 7 58 9	0 10 17 7
18	1601	Dec. 14 0 9 40	5 26 56 47	0 5 6 48	0 9 48 55
19	1619	Dec. 25 7 53 0	6 7 26 44	0 2 15 25	0 9 20 42
20	1638	Jan. 4 15 36 20	6 17 56 41	11 29 24 2	0 8 52 30
21	1656	Jan. 15 23 19 40	6 28 26 38	11 26 32 39	0 8 24 17
22	1674	Jan. 26 7 3 0	7 8 56 35	11 23 41 14	0 7 56 5
23	1692	Feb. 6 14 46 20	7 19 26 32	11 20 49 53	0 7 27 52
24	1710	Feb. 16 22 29 40	7 29 56 29	11 17 58 30	0 6 59 40
25	1728	Feb. 28 6 13 0	8 10 26 26	11 15 7 7	0 6 31 27
26	1746	March 10 13 56 20	8 20 56 53	11 12 15 44	0 6 3 15
27	1764	March 20 21 39 40	9 1 26 20	11 9 24 21	0 5 35 2
28	1782	April 1 5 23 0	9 11 56 17	11 6 32 58	0 5 6 50
29	1800	April 11 13 6 20	9 22 26 14	11 3 41 35	0 4 38 37
30	1818	April 22 20 49 40	10 2 56 11	11 0 50 12	0 4 10 25
31	1836	May 3 4 33 0	10 13 26 8	10 27 58 49	0 3 42 12
32	1854	May 14 12 16 20	10 23 56 5	10 25 7 26	0 3 14 0
33	1872	May 24 19 59 40	11 4 26 2	10 22 16 3	0 2 45 47
34	1890	June 5 3 43 0	11 14 55 59	10 19 24 40	0 2 17 35
35	1908	June 15 11 26 20	11 25 25 56	10 16 33 17	0 1 49 22
36	1926	June 26 19 9 40	0 5 55 53	10 13 41 54	0 1 21 10
37	1944	July 7 2 53 0	0 16 25 50	10 10 50 31	0 0 52 57
38	1962	July 18 10 36 21	0 26 55 47	10 7 59 8	0 0 24 45

TABLE II.

The mean time of new moon, with the mean anomalies of the sun and moon, and the sun's mean distance from the moon's ascending node, at the mean time of each periodical return of the sun's eclipse, March 21st, 1764, from the time of its falling right against the earth's centre, till it finally leaves the earth; according to the old style.

Periods.	Years of Christ.	Mean time of new moon.	Sun's mean anomaly.	Moon's mean anomaly.	Sun's mean distance from the node.
39	1980	July 28ᵈ 18ʰ 19ᵐ 41ˢ	1ˢ 7° 25′ 44″	10ˢ 5° 7′ 45″	11ˢ 29° 56′ 33″
40	1998	Aug. 9 2 3 1	1 17 55 41	10 2 16 22	11 29 8 20
41	2016	Aug. 19 9 46 21	1 28 25 38	9 29 24 59	11 29 0 8
42	2034	Aug. 30 17 29 41	2 8 55 36	9 26 33 36	11 28 31 55
43	2052	Sept. 10 1 13 1	2 19 25 33	9 23 42 13	11 28 3 43
44	2070	Sept. 21 8 56 21	2 29 55 30	9 20 50 50	11 27 35 30
45	2088	Oct. 1 16 39 41	3 10 25 27	9 17 59 27	11 27 7 18
46	2106	Oct. 13 0 23 1	3 20 55 24	9 15 8 4	11 26 39 5
47	2124	Oct. 23 8 6 21	4 1 25 21	9 12 16 41	11 26 10 53
48	2142	Nov. 3 15 49 41	4 11 55 18	9 9 25 18	11 25 42 40
49	2160	Nov. 13 23 31 1	4 22 25 15	9 6 33 56	11 25 14 28
50	2178	Nov. 25 7 16 21	5 2 55 12	9 3 42 33	11 24 46 15
51	2196	Dec. 5 14 59 41	5 13 25 9	9 0 51 10	11 24 18 3
52	2214	Dec. 16 22 43 1	5 23 55 7	8 27 59 47	11 23 49 50
53	2232	Dec. 27 6 26 21	6 4 25 4	8 25 8 24	11 23 21 38
54	2251	Jan. 7 14 9 41	6 14 55 1	8 22 17 1	11 22 53 25
55	2269	Jan. 17 21 53 1	6 25 24 58	8 19 25 38	11 22 15 13
56	2287	Jan. 29 5 36 21	7 5 54 55	8 16 31 15	11 21 57 0
57	2305	Feb. 8 13 19 41	7 16 24 52	8 13 42 52	11 21 28 48
58	2323	Feb. 19 21 3 1	7 26 54 49	8 10 51 29	11 21 0 35
59	2341	March 2 4 46 21	8 7 24 46	8 8 0 6	11 20 32 23
60	2359	March 13 12 29 42	8 17 54 43	8 5 8 43	11 20 4 10
61	2377	March 23 20 13 2	8 28 24 40	8 2 17 20	11 19 35 58
62	2395	April 4 3 56 22	9 8 54 37	7 29 25 57	11 19 7 45
63	2413	April 14 11 39 42	9 19 24 34	7 26 34 34	11 18 39 33
64	2431	April 25 19 23 2	9 29 54 31	7 23 43 11	11 18 11 20
65	2449	May 6 3 6 22	10 10 24 28	7 20 51 48	11 17 43 8
66	2467	May 17 10 49 42	10 20 54 25	7 18 0 25	11 17 14 54
67	2485	May 27 18 33 2	11 1 24 22	7 15 9 2	11 16 46 43
68	2503	June 8 2 16 22	11 11 54 19	7 12 17 39	11 16 18 31
69	2521	June 18 9 59 42	11 22 24 17	7 9 26 16	11 15 50 18
70	2539	June 29 17 43 2	0 2 54 14	7 6 34 53	11 15 22 6
71	2557	July 10 1 26 22	0 13 24 11	7 3 43 30	11 14 53 54
72	2575	July 21 9 9 42	0 23 54 8	7 0 52 7	11 14 25 41
73	2593	July 31 16 53 2	1 4 24 5	6 28 0 44	11 13 57 28
74	2611	Aug. 12 0 36 22	1 14 54 2	6 25 9 21	11 13 29 16
75	2629	Aug. 22 8 19 42	1 25 23 59	6 22 17 58	11 13 1 3
76	2647	Sept. 2 16 3 2	2 5 53 56	6 19 26 35	11 12 32 51
77	2665	Sept. 12 23 46 22	2 16 23 53	6 16 35 12	11 12 4 38
0	2683	Sept. 24 7 29 24	2 26 53 50	6 13 43 39	11 11 36 26

TABLE III.

The true time of new moon, with the sun's true distance from the moon's ascending node, and the moon's true latitude, at the true time of each periodical return of the sun's eclipse, March 21, 1764, old stile, from the time of its first coming on the earth since the creation, till it falls right against the earth's centre.

Periods.	Years of Christ.	True time of new moon.					Sun's true distance from the node.				Moon's true latitude. North.			
			d	h	m	s	s	°	′	″	°	′	″	
0	1277	June	2	15	9	36	0	19	5	40	1	37	50	N. A.
1	1295	June	13	21	54	32	0	18	40	54	1	33	45	N. A.
2	1313	June	24	3	57	3	0	17	20	22	1	29	34	N. A.
3	1331	July	5	10	42	8	0	16	29	35	1	25	20	N. A.
4	1349	July	15	17	14	15	0	15	34	18	1	20	45	N. A.
5	1367	July	26	23	49	24	0	14	46	8	1	16	39	N. A.
6	1385	August	6	6	41	17	0	13	59	43	1	12	43	N. A.
7	1403	August	17	13	32	19	0	13	16	44	1	9	3	N. A.
8	1421	August	27	20	30	17	0	12	37	4	1	5	42	N. A.
9	1439	Sept.	8	3	51	46	0	12	1	54	1	2	41	N. A.
10	1457	Sept.	18	10	23	11	0	11	30	27	0	58	53	N. A.
11	1475	Sept.	29	17	57	7	0	11	3	56	0	57	43	N. A.
12	1493	Oct.	10	1	44	3	0	10	41	55	0	55	49	N. A.
13	1511	Oct.	21	9	29	53	0	10	25	11	0	54	28	N. A.
14	1529	Oct.	31	17	9	18	0	10	11	27	0	53	12	N. A.
15	1547	Nov.	12	0	51	25	0	10	1	10	0	52	19	N. A.
16	1565	Nov.	22	8	54	56	0	9	52	49	0	51	46	N. A.
17	1583	Dec.	3	16	48	17	0	9	48	4	0	51	11	N. A.
18	1601	Dec.	14	0	51	5	0	9	43	42	0	50	49	N. A.
19	1619	Dec.	25	8	54	59	0	9	40	23	0	50	31	N. A.
20	1638	Jan.	4	16	56	1	0	9	34	57	0	50	3	N. A.
21	1656	Jan.	15	0	54	41	0	9	29	24	0	49	57	N. A.
22	1674	Jan.	26	8	48	24	0	9	19	44	0	48	44	N. A.
23	1692	Feb.	6	16	36	28	0	9	8	58	0	47	49	N. A.
24	1710	Feb.	17	8	8	37	0	8	54	20	0	45	43	N. A.
25	1728	Feb.	28	7	43	40	0	8	34	53	0	44	52	N. A.
26	1746	March	10	15	14	33	0	8	10	38	0	42	46	N. A.
27	1764	March	20	22	30	26	0	7	42	14	0	40	18	N. A.
28	1782	April	1	5	37	4	0	7	9	27	0	37	28	N. A.
29	1800	April	11	12	36	38	0	6	35	30	0	34	31	N. A.
30	1818	April	22	19	27	34	0	5	51	48	0	30	43	N. A.
31	1836	May	3	2	12	7	0	5	5	5	0	26	40	N. A.
32	1854	May	14	8	50	40	0	4	19	45	0	22	42	N. A.
33	1872	May	24	15	28	15	0	3	26	3	0	18	1	N. A.
34	1890	June	4	22	8	0	0	2	35	5	0	13	34	N. A.
35	1908	June	15	4	38	23	0	1	41	43	0	8	54	N. A.
36	1926	June	26	11	13	5	0	0	47	38	0	4	10	N. A.
37	1944	July	6	17	50	35	11	29	55	28	0	0	24	S. A.
38	1962	July	18	0	31	38	11	29	2	35	0	5	2	S. A.

By the true motions of the sun, moon, and nodes, the moon's shadow falls even with the earth's centre 2 periods sooner than by their mean motions.

TABLE IV.

The true time of new moon, with the sun's true distance from the moon's ascending node, and the moon's true latitude, at the true time of each periodical return of the sun's eclipse, March 21, 1764, old stile, from the time of its falling right against the earth's centre, till it finally leaves the earth for upwards of 12,492 years.

Periods.	Years of Christ.	True time of new moon.					Sun's true distance from the node.				Moon's true latitude. South.			
39	1980	July	28d	7h	18m	53s	11s	28°	11′	32″	0°	9′	29″	S. A.
40	1998	August	8	14	12	22	11	27	26	41	0	13	25	S. A.
41	2016	August	18	21	14	53	11	26	42	16	0	17	18	S. A.
42	2034	August	30	4	25	45	11	26	2	0	0	20	48	S. A.
43	2052	Sept.	9	11	45	17	11	25	26	46	0	23	53	S. A.
44	2070	Sept.	20	19	17	26	11	24	55	4	0	26	39	S. A.
45	2088	Oct.	1	2	57	8	11	24	27	43	0	28	58	S. A.
46	2106	Oct.	12	10	47	39	11	24	4	38	0	31	2	S. A.
47	2124	Oct.	22	18	37	39	11	23	48	28	0	32	26	S. A.
48	2142	Nov.	3	2	[illegible]	19	11	23	35	11	0	33	53	S. A.
49	2160	Nov.	13	11	11	20	11	23	22	22	0	34	42	S. A.
50	2178	Nov.	24	19	36	14	11	23	18	57	0	35	0	S. A.
51	2196	Dec.	5	4	4	9	11	23	14	40	0	35	22	S. A.
52	2214	Dec.	16	12	35	48	11	23	10	43	0	35	43	S. A.
53	2232	Dec.	26	20	29	9	11	23	6	47	0	36	1	S. A.
54	2251	Jan.	7	5	42	9	11	23	4	27	0	36	16	S. A.
55	2269	Jan.	17	14	14	8	11	23	0	41	0	36	35	S. A.
56	2287	Jan.	28	22	43	34	11	22	53	58	0	37	10	S. A.
57	2305	Feb.	8	7	8	30	11	22	44	44	0	37	59	S. A.
58	2323	Feb.	19	15	7	10	11	22	31	1	0	39	8	S. A.
59	2341	March	2	0	6	5	11	22	17	46	0	40	28	S. A.
60	2359	March	13	7	59	17	11	21	55	29	0	42	9	S. A.
61	2377	March	23	15	51	59	11	21	39	40	0	43	41	S. A.
62	2395	April	3	23	45	7	11	21	0	53	0	46	58	S. A.
63	2413	April	14	7	32	40	11	20	26	22	0	49	48	S. A.
64	2431	April	25	15	12	57	11	19	47	34	0	53	17	S. A.
65	2449	May	5	22	45	14	11	19	6	22	0	56	50	S. A.
66	2467	May	17	6	17	30	11	18	21	16	1	0	40	S. A.
67	2485	May	27	13	46	30	11	17	34	20	1	4	42	S. A.
68	2503	June	7	21	10	31	11	16	43	17	1	9	3	S. A.
69	2521	June	18	4	24	42	11	15	51	48	1	13	26	S. A.
70	2539	June	29	11	58	46	11	15	1	12	1	17	43	S. A.
71	2557	July	9	19	24	7	11	14	9	13	1	22	6	S. A.
72	2575	July	21	2	52	34	11	13	19	22	1	26	16	S. A.
73	2593	July	31	10	25	31	11	12	13	43	1	31	44	S. A.
74	2611	August	11	17	58	39	11	11	45	13	1	36	13	S. A.
75	2629	August	22	1	41	37	11	11	1	49	1	39	50	S. A.
76	2647	Sept.	2	9	29	37	11	10	22	59	1	42	0	S. A.
77	2665	Sept.	12	17	25	13	11	9	46	48	1	45	45	S. A.
0	2683	Sept.	24	1	29	1	11	9	15	49	1	47	58	S. A.

The true motions carry off the eclipse 4 periods sooner than the mean.

XXXIX. Of an Earthquake at Chattigaon. Translated from the Persian by Mr. Edward Gulston, in the Service of the East India Company. p. 251.

This earthquake happened in the region of Islamabad on the 22d of the month Chytt 1168 Bengal æra, answering to the 2d of April, 1762, on Friday about 5 o'clock in the afternoon. Many houses were thrown down: and in various places the land, and even the hills, rent and sunk a considerable depth; at the same time water sprung up, and overflowed many parts of the country.

XL. Of the same Earthquake in the East Indies; and of two Eclipses of the Sun and Moon, observed at Calcutta. By the Rev. William Hirst, M. A., F. R. S. p. 256.

This earthquake, which happened April 2, 1761, was very violent in the kingdoms of Bengal, Aracan, and Pegu, but particularly at the metropolis of Aracan, where, according to the accounts of an English merchant residing there, the effects have been as fatal as at Lisbon, and where it is thought the chief force of the earthquake vented itself. At Dacca, in this kingdom of Bengal, the consequences have been terrible; the rise of the waters in the river was so very sudden and violent, that some hundreds of large country boats were driven ashore, or lost, and great numbers of lives lost in them. No less deplorable are the accounts from Chattigaon in this same kingdom. The same earthquake was also very alarming at Chirotty, where Colonel Coote with his Majesty's troops are in cantonment about 18 miles up the river from this place. The waters in the river and tanks there were violently agitated, and in many places rose to more than 6 feet perpendicular height. Nearly at the same time was this earthquake felt at Calcutta, where the agitation of the waters in the tanks rose upwards of 6 feet, and was in the direction north and south. A subsequent earthquake was felt at Calcutta the 13th of July following, at half past 2 in the afternoon.

Observations of a solar eclipse made at Ghyrotty, about the latitude 22° 51′ north, the watch adjusted to apparent time.

Beginning of the lunar immersion.	2^h	49^m	30^s
Greatest visible obscurity near 11 digits eclipsed	4	5	38
End of the eclipse.	5	12	20
Total duration	2	22	50

The next observation was of the eclipse of the moon, which he made Nov. 2, 1762, in conjunction with Mr. Hancock, at his house in Calcutta, who supplied him with some excellent astronomical instruments, particularly with a large land quadrant of 2 feet radius, made by Cole in Fleet-street, with which he took the correspondent altitudes of the sun to adjust his watch (which was furnished with a hand to distinguish seconds) to the apparent time. Mr. Hancock himself marking the times while Mr. Hirst observed. The telescope he used was a reflector made by Dollond, in perfect order, being sent out of England by the last ships, and about 22 inches in length.

Observation of the lunar eclipse Nov. 2, 1762, made at Calcutta in the kingdom of Bengal, latitude 22° 30′ N.

	By the watch.			Apparent time.		
The beginning of the eclipse at	1^h	15^m	10^s	1^h	7^m	40^s
End of the eclipse	4	3	2	3	55	32
Total duration	2	47	52	2	40	22

Near 8 digits eclipsed by occular demonstration.

XLI. On the foregoing Earthquake. By Mr. Edward Gulston, at Chittigong. p. 263.

The violent earthquake which was felt here on April 2, 1762, at 5 in the afternoon, lasted the space of 4 minutes. The factory, a brick building, is totally spoiled, so as not to be safely habitable; for thereabouts, and in many other places, the earth opened and the water gushed out prodigiously. At the time of the first shake, great explosions were heard like the noise of cannons, of which they counted 15. All the tanks overflowed their banks, fish were cast up, and the river rushed upon the shore like the surf of the sea.

XLII. Of the Earthquakes that have been felt in the Province of Islamabad, with the Damages attending them, from the 2d to the 19th of April, 1762. p. 265.

The weather being very close and warm for some days preceding, on the 2d of April, about 5 in the afternoon, they were alarmed by an earthquake, which beginning with a gentle emotion, increased to so violent a degree, for about 2 minutes, that the trees, hills, and houses shook so severely, that it was with difficulty many could keep their feet. On the plains, by the rivers, and near the sea, it was chiefly felt with great severity. There is not a brick wall or house but is either greatly damaged or fallen. The ground opened in several places in the town, throwing up water of a very sulphureous smell; and several ditches and tanks were filled up, which are now level dry land. The emotions were so complicated, that we could not well determine their direction, being sometimes from west to east, and again from east to west: and the tanks in some places overflowed north and south. In Purgunnah Deang, Bursea Gong, the ground in several places opened 10 and 12 cubits wide; and in some parts so deep, that they could not fathom its bottom; the water immediately overflowing the whole town, which is sunk about 7 cubits. Deep Gong, a village near the other, is also sunk, and now lies 7 cubits under water. From Patter Gottah to Howlah, about 8 cess distance, the ground opened, and a great quantity of water was immediately thrown out, and in several places the ground entirely sunk. At Bans Burreah, Akul Poor, near the sea, the earth opened in 7 places like wells, throwing up the water 10 cubits high; the great Cutcherry there, with brick walls, is cracked and shivered to pieces. At Hulda Creek, near Sancharam Conguy's house, 12 don of ground is entirely sunk; and many others shared the same fate.

At Bar Chara near the sea, 5 or 6 cess of ground immediately sunk, and out of 4 or 5 hundred people, above 200 were lost, with all their cattle; and the greatest part of the remaining inhabitants who ran into the woods have not yet been heard of. At Lafettee Silcope Chuckla the ground in some places opened and threw up great quantities of salt water, and in others entirely sunk; the channels of several creeks and little valleys between the hills, were filled up with great quantities of sand; in some parts the water still continues 20 cubits deep, and in others unfathomable. Silluk creak, and Issamuttee river are both stopped up; several boats laden with goods then coming down are not now able to get out of them; the country around there opened greatly in some places, and in others entirely sunk: and a great many tanks filled with sand. Bur Coller hill opened about 40 cubits wide. Cess Lung Joom hill, one of the Mug mountains, is entirely sunk. Chunggee hill opened between 20 and 30 cubits. Puddaoah creek, at that time without water, opened and threw up 2 hills of sand; and all the houses in these parts were broken down. Joom Chater Pedea hill is sunk so low, that its top is now on a level with the plains. Rigerree hill, which was very large, opened 30 cubits wide. Joom Palang hill opened 25 cubits. By the accounts already come in there are 120 *dons of ground lost in different parts of the province; but these it is feared will not be $\frac{1}{8}$ part of the whole damages, as further reports are coming in every hour.

As they were informed that 2 volcanoes were opened, they were in great hopes these will prove a sufficient vent to discharge all the remaining sulphureous matter in the bowels of these countries, and put a stop to any further earthquakes there; at least for many years to come.

XLIII. On certain Infinite Series. By the late Rev. Thomas Bayes, F. R. S. p. 269.

It has been asserted by some eminent mathematicians, that the sum of the logarithms of the numbers 1, 2, 3, 4, &c. to z, is equal to $\frac{1}{2}$ log. $c + (z + \frac{1}{2}) \times$ log. z lessened by the series $z - \frac{1}{12z} + \frac{1}{360z^3} - \frac{1}{1260z^5} + \frac{1}{1680z^7} - \frac{1}{1188z^9} +$ &c. if c denote the circumference of a circle whose radius is unity. And it is true that this expression will approach very nearly to the value of that sum when z is large,

* One sye don of ground is 1920 cubits long and 1600 cubits broad.—Orig.

and we take in only a proper number of the first terms of the foregoing series; but the whole series can never properly express any quantity at all; because after the 5th term the coefficients begin to increase, and they afterwards increase at a greater rate than what can be compensated by the increase of the powers of z, though z represent a number ever so large; as will be evident by considering the following manner in which the coefficients of that series may be formed. Take $a = \frac{1}{12}$; $5b = a^2$; $7c = 2ba$; $9d = 2ca + b^2$; $11e = 2da + 2cb$ $13f = 2ea + 2db + c^2$; $15g = 2fa + 2eb + 2dc$, and so on: then take $A = a$, $B = 2b$, $C = 2 \times 3 \times 4c$; $D = 2 \times 3 \times 4 \times 5 \times 6d$; $E = 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8e$, and so on: then A, B, C, D, E, F, &c. will be the coefficients of the foregoing series: whence it easily follows, that if any term in the series after the first 3 be called y, and its distance from the first term n, the next term immediately following will be greater than $\frac{n \times (2n - 1)}{6n + 9} \times \frac{y}{z^2}$. Therefore at length the subsequent terms of this series are greater than the preceding ones, and increase in infinitum, and therefore the whole series can have no ultimate value whatever.

Much less can that series have any ultimate value, which is deduced from it by taking $z = 1$, and is supposed to be equal to the logarithm of the square root of the periphery of a circle whose radius is unity; and what is said concerning the foregoing series is true, and appears to be so, much in the same manner, concerning the series for finding the sum of the logarithms of the odd numbers 3, 5, 7, &c. .. z, and those that are given for finding the sum of the infinite progressions, in which the several terms have the same numerator, while their denominators are any certain power of numbers increasing in arithmetical proportion. But it is needless particularly to insist upon these, because one instance is sufficient to show that those methods are not to be depended on, from which a conclusion follows that is not exact.

XLIV. Of the Insect called the Vegetable Fly. By William Watson, M. D., F. R. S.* p. 271.

The beginning of Oct. 1763, Dr. W. received a letter from Dr. Huxham of Plymouth, in which, among other things, he informed him that he lately had obtained a sight of what is called the vegetable fly, with the following description of it; both of which he had from Mr. Newman, an officer of General Duroure's regiment, who came from the island Dominica. As this description seemed to the doctor exceedingly curious, he sent it to Dr. W., exactly transcribed from Mr. Newman's account, which is as follows.

"The vegetable fly is found in the island Dominica, and (excepting that it has no wings) resembles the drone both in size and colour more than any other En-

* Fungi, not only of the genus Clavaria, but several others occasionally spring from the bodies of dead insects, the seed of such Fungi having accidentally fallen on the insects.

lish insect. In the mouth of May it buries itself in the earth and begins to vegetate. By the latter end of July the tree is arrived at its full growth, and resembles a coral branch; and is about 3 inches high, and bears several little pods, which dropping off become worms, and from thence flies like the English caterpillar." An account of this extraordinary production similar to the above, was given to Dr. Huxham by Capt. Gascoign, who lately commanded the Dublin man of war, which had been at Dominica. The doctor subjoins, that possibly Dr. W. might have heard of this fly, or seen it in the collections of the British Museum, or Royal Society; but if it was in neither, he believed he could procure it to be sent to the Royal Society.

Though Dr. H. could by no means think the above relation true in all its circumstances, yet he was persuaded there was something of reality in it; which perhaps further accounts and observations might set in a full and true light; though as above represented, it seemed quite repugnant to the usual order of nature. As Dr. W. had never seen this production himself, but had been informed that Dr. Hill had had the examination of some of them, he wrote to that gentleman to desire to be informed of the result of his inquiries. To which he very obligingly sent the following answer.

"When Colonel Melvil brought these flies from Guadaloupe, Lord Bute sent me the box of them to examine. The result was this. There is in Martinique a fungus of the Clavaria kind, different in species from those hitherto known. It produces soboles from its sides. I call it therefore Clavaria Sobolifera. It grows on putrid animal bodies, as our fungus ex pede equino from the dead horse's hoof. The Cicada is common in Martinique, and in its nympha state, in which the old authors call it Tettigometra, it buries itself under dead leaves to wait its change; and when the season is unfavourable many perish. The seeds of the Clavaria find a proper bed on this dead insect, and grow. The Tettigometra is among the Cicadæ in the British Museum: the Clavaria is just now known. This you may be assured is the fact, and all the fact: though the untaught inhabitants suppose a fly to vegetate; and though there exists a Spanish drawing of the plant's growing into a tri-foliate tree: and it has been figured with the creature flying with this tree on its back. So wild are the imaginations of man: so chaste and uniform is nature!"

Commissioner Rogers, at Dr. Huxham's desire, presented this extraordinary production to the Royal Society. A careful examination of it (says Dr. Watson) seems to confirm, to me at least, Dr. Hill's opinion of the manner of this phenomenon's being produced. Mr. Edwards has taken notice of this extraordinary production in his Gleanings of Natural History, and has given us a figure of it in that elegant work.

There is in the British Museum among the Cicadæ, one nearly resembling the animal part of this production, but it came from the East Indies. There is likewise from the West Indies, in its perfect or winged state, the insect of which this production is believed to be the nympha. See fig. 1, pl. 1.

XLV. An Attempt to explain a Punic Inscription, lately discovered in the Island of Malta. By the Rev. John Swinton, B. D., F. R. S. p. 274.

This inscription was sent from Rome to Mr, S. by Signor Venuti. And from the observations made by Mr. S. it most evidently appears, that the annexed arrangement of the words

חדר כת עלם קבר ננעל
נקח בכלת חות רח-
-ם רף אם בשת חנב-
-על כן כרמלך

forming this inscription may be considered as not very remote from truth. The Latin and English versions of which words may, as he conceives, be appositely enough drawn up in the following terms.

Penetrale domvs secvli (sive domvs perpetvæ)—sepvlchrvm depositi (hic) clari (viri) consvmmationibvs (i. e. omnino, plane, vel, arctissime) dormientis—intime diligens (eum) commotvs (est) popvlvs qvvm poneretvr scil. in terra (i. e. sepeliretvr) Hannibal Barmelec (Barmilc Bormilc vel Barmeleci) filivs.

The interior part of the house of long duration (or long home i. e. the grave) —she sepulchre of an upright man deposited (here) in a most sound (or dead) sleep—The people having a great affection for him were vastly concerned when Hannibal the son of Barmelec (Barmilc or Bormilc) was put into the earth or interred.

It ought to be here remarked, that the word רחם terminates the second line, and begins the third; as also that the proper name חנבעל, Hannibal, by a similar kind of bissection belongs both to the third and fourth lines. But this is by no means to be wondered at. The Greeks observed the same method of writing in their inscriptions, both of an earlier and a later date. From this inscription, Mr. S. forms a Maltese-Punic alphabet of 17 characters, very different in shape from the ancient Phœnician or Samaritan.

Who Hannibal the son of Barmelec, Barmelc, or Bormilc, was, or when he lived, Mr. S. cannot take upon him precisely to determine. We may however, he thinks, rest assured that he died a considerable time (perhaps several centuries) after the Citiean inscriptions, or at least the earliest of them, first appeared. The forms of several of the letters, particularly of the Aleph, Ghimel, He, Heth, Caph, Ajin, Koph, Schin, and Thau, so considerably differing from those of the same elements in the earlier Phœnician times, seem, he thinks, to render this incontestably clear.

Mr. S. adds that the Punic and Phœnician alphabets were originally the very same, and doubtless continued so, or nearly so, long after the foundation of Carthage. And this is rendered highly probable by the letters preserved on many Carthaginian coins. To what then can we so properly ascribe the variations in them as to distance of time, since the letters so varied in the Carthaginian territories had undoubtedly the same forms with those of the correspondent elements in the more ancient Phœnician alphabet, (used both there and at Tyre, Sidon, Citium, &c.) several ages before? In fine, the same characters at first prevailed both at Carthage and in Phœnicia; though these, or at least several of them, in after ages, assumed pretty different forms. So that the more any Punic or Phœnician literary characters, in whatever country found, recede from those that formed the Samaritan or earliest Phœnician alphabet, the later they ought undoubtedly to be deemed.

After the Carthaginian provinces had been subdued by the Romans, the people still retained the use of their ancient proper names, and spoke the Punic tongue. Nay, we have good reason to believe, that the Phœnician or Punic language was spoken and understood in some of those provinces even to the days of St. Austin. With regard to the island of Malta in particular, which was so long subject to the Carthaginians, it may not be improper to remark, that the entire reduction of it seems scarcely to have been effected before the time of Julius Cæsar by the Romans. For though the people of that island were obliged to submit to the Roman power after the destruction of Carthage; yet they found means afterwards to assert their independency, and shake off the Roman yoke. But notwithstanding they had been rendered a formidable maritime power, by the extensive commerce which they enjoyed, they were finally* subjugated by Cæsar, though with no small difficulty, about 45 years before the birth of Christ. It may justly therefore be questioned whether the Latin tongue was ever much used in Malta before the death of that conqueror, or rather before the commencement of the Christian æra, which was but little posterior to it. Be that however as it may, that the use of the Punic language and the Punic proper names were retained in Malta, as an ancient part of the Carthaginian territories, at least 3 or 4 centuries after the last-mentioned period, if not much longer, from what has been here advanced, is abundantly clear. Nay, that the Punic tongue is even at this day the vernacular language of the lower part of the Maltese, though deformed by many corruptions, and disguised by the accession of various foreign words, after perusing what has been communicated on that head to the learned world by Canonico Agius, Mr. S. is strongly inclined to believe.

* Appian. Alexandrin. apud Burchard. Niedersted. in Malta Vet. et Nov. lib. ii. c. vi. p. 69. Helmestadii, 1660.—Orig.

Since therefore the ducts of several of the letters indicate this inscription to be of a later date, we cannot but suppose it to have been many years (perhaps several centuries) posterior to the conclusion of the first Punic war. And since Hannibal Ben Barmelec, or Bormilc, is mentioned therein as a person of consideration, whose death was greatly lamented by the people; perhaps he was either a popular senator of Malta, or one of the sufferers there, (the Punic form of government not improbably prevailing in that island, even when dependent on the Romans, as it did in other places that had been subject to the Carthaginian state) a century at least after Julius Cæsar had given the finishing stroke to the liberties of the Maltese.

XLVI. Algebraical and Geometrical Problems. By Edward Waring, M. A., and Lucasian Professor of Mathematics, Cambridge, and F. R. S. From the Latin.* p. 294.

PROB. I. To find how many impossible roots has the given biquadratical equation $x^4 + qx^2 - rx + s = 0$.

* Edward Waring, M. D., was born near Shrewsbury, about the year 1736; where also he died, Aug. 15, 1798, in the 63d year of his age. He was the son of a wealthy farmer of the Old Heath, near that place, and where he received the early part of his education: whence he was removed to Cambridge, and was admitted in 1753 a member of Magdalen College. Here his talents for abstruse calculations soon distinguished him; so that at the time of taking his first or bachelor's degree, in 1757, he was considered as a prodigy in those sciences which make the subject of the examination on such occasions, when he was distinguished as senior wrangler, or the first student of the year, John Jebb being the 2d on the list. The Lucasian professorship of mathematics in the University becoming vacant, by the death of Mr. John Colson, in 1759, before Mr. Waring was of sufficient standing for the next or Master of Arts degree, which is a necessary qualification for that office, for which he was desirous to become a candidate; this defect was supplied by a royal mandate, conferring that degree, and he was elected to the professorship in Jan. 1760. On this occasion some remarkable circumstances occurred. Mr. W. before his election, gave a small specimen of his abilities, as a proof of his fitness for that office, by the publication of the first chapter of his Miscellanea Analytica. This specimen was attacked, and his election opposed, by Dr. Powell, of St. James's College, partly from a principle of regular conformity to the academic rules, and partly to serve his friend Mr. Masseres (the present cursitor baron of the exchequer) then a candidate also for the vacant professorship. This opposition produced several pamphlets between the two parties, by Dr. Powell and Mr. Masseres on the one part, and by Mr. Waring, assisted by his friend Mr. Wilson (afterwards one of the judges, Sir John Wilson) on the other part; which however ended in the success and election of the latter.

In 1762 Mr. W. published complete his Miscellanea Analytica, one of the most abstruse books, written on the abstrusest parts of algebra; which at least had the effect of extending the author's fame for ingenuity, though it might not otherwise be of any real use. Mathematics however did not engross the whole of his attention. He could allow some part of his time to the study of medicine; and in 1767 he was admitted to the degree of M.D., though he never after practised as a physician Mathematics again engaged his chief attention, whence he successively produced a number of pieces, of the same abstruse kind as the former; several of which were inserted in different vols.

1st, If $256s^3 - 128q^2s^2 + (144r^2q + 16q^4) \times s - 27r^4 - 4r^2q^3$ be a negative quantity; then the equation has two, and not more impossible roots.

2d, If that quantity be affirmative, and either $-q$ or $q^2 - 4s$ be a negative quantity; then the given equation will have 4 impossible roots.

3d, If it be equal to nothing, and either $-q$ or $q^2 - 4s$ be a negative quantity; then the two unequal roots of the given equation will be impossible.

PROB. II. To find how many impossible roots are in the given equation $x^5 + qx^3 - rx^2 + sx - t = 0$.

1st. If the signs of the terms of the equation $w^{10} + 10qw^9 + (39q^2 + 10s) \times w^8 + (80q^3 + 50qs + 25r^2) \times w^7 + (95q^4 + 124q^2s - 95s^2 + 92qr^2 + 200rt) \times w^6 + (66q^5 - 360qs^2 + 196q^3s + 118q^2r^3 + 260r^2s + 625t^2 + 400qrt) \times w^5 + (25q^6 + 40s^3 - 53r^4 + 52q^3r^2 - 522q^2s^2 + 194q^4s + 708qr^2s + 240q^2rt + 1750qt^2 - 950srt) \times w^4 + (4q^7 + 106q^5s - 80qs^3 - 308q^3s^2 - 102qr^4 - 7q^4r^2 + 570r^2s^2 + 612q^2r^2s + 700r^2t - 3750t^2s + 2500t^2q^2 + 80rtq^3 - 2150qrst) \times w^3 + (400s - 360q^2s^3 - 15q^4s^2 + 24q^6s - 8q^5r^2 - 45q^2r^4 - 270r^4s + 140r^2sq^3 + 960r^2s^2q + 1875t^2r^2 + 1000trs^2 - 5000t^2qs + 1750t^2q^3 + 40trq^4 + 600tr^3q - 1650trsq^2) + w^2 + (36q^5s^2 - 224q^3s^2 + 320qs^4 + 4q^3r^4 + 27r^6 - 40r^2s^2 + 434r^2q^2s^2 - 24r^2sq^4 - 198r^4qs + 5000t^2s^2 - 450tr^3s - 6250t^3r + 675t^2q^4 - 3750t^2q^2s + 3000t^2r^2q + 60tr^3q^2 + 200trs^2q - 330trq^3s) \times w + 3125t^4 - 3750qrt^3 + (2000s^2q + 2250r^2s - 900sq^3 + 825r^2q^2 + 108q^5) \times t^2 - (1600s^3r - 560rq^2s^2 - 16r^3q^3 + 630r^3qs + 72rsq^4 - 108r^5) \times t + 256s^5 - 128q^2s^4 + 144r^2qs^3 + 16q^4s^3 - 27r^4s^2 - 4r^2q^3s^2 = 0$, be continually changed from + to −, and from − to +; the given equation has no impossible roots.

2d. If the signs of the terms of the equation be not continually changed from + to − and − to +; then 2 or 4 of the roots of the given equation will be impossible, according as the last term of it is negative or affirmative.

of the Phil. Trans., and others he published in separate works: as, the Meditationes Algebraicæ, in 1770; the Proprietates Algebraicarum Curvarum, in 1772; and the Meditationes Analyticæ, in 1776. To these might be added a work written in his retirement, on morals and metaphysics; of which a few copies only were printed, and presented to his friends. For his various ingenious papers in the Phil. Trans., Dr. W. was, in 1784, deservedly honoured by the R. S. with their gold medal. And most of these essays give strong proofs of the powers of his mind, both in abstract science, and its application to philosophy: though they labour, in common with his other works, under the disadvantage of being conveyed in a very unattractive form. As to the contents of the separate volumes above mentioned, a very full analysis of them may be seen in Dr. Hutton's Dictionary, vol. 2, p. 717, as communicated by Dr. W. himself. In his disposition and character Dr. W. is represented as of inflexible integrity, great modesty, plainness, and simplicity of manners; of a meekness and diffidence of manner to such a degree, as to be always embarrassed before strangers. His extreme short-sightedness too, joined to the natural want of order and method in his mind, which appeared remarkably even in his hand-writing, rendered his mathematical compositions so confused and embarrassed, that in manuscript they were often utterly inexplicable: a circumstance which may account for the numerous typographical errors in his publications.

3d. If the last term of it be equal to nothing, and the signs of the terms of the equation be not continually changed from + to − and − to +; then either 4 or 2 roots of the given equation will be impossible, according as 2, and not more of the last terms of the given equation are equal to nothing, or the contrary.

PROB. III. Let x, y, v, be the abscissa, ordinate, and area of a given curve; and let $y^n + (a + bx) \times y^{n-1} + (c + dx + ex^2) \times y^{n-2} + (f + gx + hx^2 + kx^3) \times y^{n-3} + \&c. = 0$: to find whether the area (v) can be squared or not.

Suppose the equation to the area to be $v^n + (A + Bx + Cx^2)\, v^{n-1} + (D + Ex + Fx^2 + Gx^3 + Hx^4) \times v^{n-2} + (I + Kx + Lx^2 + Mx^3 + Nx^4 + Ox^5 + Px^6) \times v^{n-3}\ \&c. = 0$: consequently it will be $nyv^{n-1} + (n-1) \times (A + Bx + Cx^2)\, yv^{n-2} + (n-2) \times (D + Ex + Fx^2 + Gx^3 + Hx^4\ B + 2Cx)\, v^{n-1} + (E + 2Fx + 3Gx^2 + 4Hx^3) \times yv^{n-3} + \&c. \times v^{n-2} + \&c. \Big\} = 0$.

If from these equations, by the known methods, v be exterminated, there will result an equation expressing the relation between x and y. Then the coefficients of this equation must be equal to the coefficients of the given equation $y^n + (a + bx)\, y^{n-1} + (c + dx + ex^2)\, y^{n-2} + \&c. = 0$; and if the quantities A, B, C, &c. can be hence determined, the curve is quadrable, for it is $v^n + (A + Bx + Cx^2) \times v^{n-1} + (D + Ex + Fx^2 + Gx^3 + Hx^4) \times v^{n-2} + \&c. = 0$; otherwise, it is not quadrable.

Exam. Let the given equation be $y^2 + x^2 - 1 = 0$, and suppose the equation to the area be $y^2 + D + Ex + Fx^2 + Gx^3 + Hx^4 = 0$; then will $2vy + E + 2Fx + 3Gx^2 + 4Hx^3 = 0$; hence reducing these two equations into one, to exterminate v, and there results the equation $y^2 +$

$$\frac{16H^2x^6 + 24HGx^5 + (16HF + 9G^2)\,x^4 + (8EH + 12FG)\,x^3 + (6GE + 4F^2)\,x^2 + 4FEx + E^2}{4 \times (Hx^4 + Gx^3 + Fx^2 + Ex + D)} = 0.$$

But the fraction $\frac{16H^2x^6 + 24HGx^5 + (16HF + 9G^2)\,x^4 + (8EH + 12FG)\,x^3 + (6GE + 4F^2)\,x^2 + 4FEx + E^2}{4 \times (Hx^4 + Gx^3 + Fx^2 + Ex + D)}$ ought to be $x^2 - 1$; and consequently

$$\begin{aligned}
4H &= 16H^2 \\
4G &= 24HG \\
4F - 4H &= 16HF + 9G^2 \\
4E - 4G &= 8HE + 12FG \\
4D - 4F &= 6GE + 4F^2 \\
-4E &= 4FE \\
-4D &= E^2
\end{aligned}$$

But, by the method of finding common divisors, it appears that these equations are contradictory to each other; and consequently the curve is not generally quadrable.

THEOREM. Let x, y, v, be the abscissas and ordinates of the curves ABCDEFG

HI &c. and Aβγδε &c. fig. 2, pl. 1, and let $y = px^n$, and $v = \frac{n}{2.3} pa^{n-1} x - \frac{n \times (n-1) \times (n-2)}{30 \times 2 \times 3} pa^{n-3} x^3 + \frac{n \times (n-1) \times (n-2) \times (n-3) \times (n-4)}{42 \times 2 \times 3 \times 4 \times 5} pa^{n-5} x^5 - \frac{n \times (n-1) \times (n-2) \times (n-3) \times (n-4) \times (n-5) \times (n-6)}{30 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7} pa^{n-7} x^7 + \frac{5n \times (n-1) \times (n-2) \times (n-3) \times (n-4) \times (n-5) \times (n-6) \times (n-7) \times (n-8)}{66 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9} pa^{n-9} x^9 - \frac{691 \times n \times (n-1) \times (n-2) \times (n-3) \times (n-4) \times (n-5) \times (n-6) \times (n-7) \times (n-8) \times (n-9) \times (n-10)}{2730 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7 \times 8 \times 9 \times 10 \times 11} pa^{n-11} x^{11} +$ &c. the last term of which ought to be x^{n-1} or x^{n-2}, according as n is an even or an odd number.

Let $x =$ AP $= a$, bisect AP in T, and draw the lines ETδ; and if AE, EM, AM, be joined; then will the triangle AEM be equal to the area TPεδT.

Again, bisect TP, AT in R and V, and draw RG, CVγ; and joining AC, CE, EG, GM; then will the two triangles ACE + EGM = the area VTδγV.

In like manner, if the parts AV, VT, TR, RP, be again bisected in W, U, S, Q, and there be drawn the lines BWβ, UD, SF, QH; and joining AB, BC, CD, DE, EF, FG, GH, HM; then will the 4 triangles ABC + CDE + EFG + GHM be = the area WVγβW. And so on.

Cor. 1. If the curve be ABC, and CM be a conic parabola, or $y = pix^2$; then will $v = \frac{1}{3} pax$, and ABγδ &c. will be a right line; and the proposition is the same with the known proposition of the quadrature of the parabola.

Cor. 2. If $y = px^3$; it will be $v = \frac{1}{2} pax$, and ABγδ &c. again a right line.

Cor. 3. Given a curve, whose equation is $y = px_{2n}$, there can be found another curve, whose dimensions are $2n - 1$, in which the sum of the triangles, at every bisection, will be respectively equal to the sum of the triangles of the given curve.

To these may be added, that if, instead of bisecting the abscissa AP, it be divided into unequal parts in any other ratio; the sums of the triangles of the curve ABCD &c. will be equal to each of the divisions of the segments of the curve Aβγδ.

XCVII. Second Paper concerning the Parallax of the Sun determined from the Observations of the late Transit of Venus; in which this Subject is treated of more at Length, and the Quantity of the Parallax more fully ascertained. By James Short, M.A., F.R.S. p. 300.

In the last volume of the Memoirs of the Royal Academy at Paris, for the year 1761, there is a memoir by M. Pingré, who went to the island of Rodrigues, and observed the transit of Venus there; in this memoir M. Pingré endeavours to determine the parallax of the sun, by the observation of the late transit of Venus, to be = 10″, by the observed durations, as well as the least distance of the centres, and by the internal contact at the egress; and seems to think that there must be some mistake in the observation of Mr. Mason at the Cape of Good Hope, particularly with regard to the difference of longitude between Mr. Mason's observatory and Paris, because by comparing the observation of Mr. Mason at the Cape with the European observations, he finds the parallax of the sun, thence resulting, to be between 8″ and 9″, consequently differing from the determination by the observation at Rodrigues when compared with the same places. Mr. S. therefore in this paper endeavours to prove, beyond all doubt, by a comparison of the observations on this side of the equinoctial line alone, that the sun's parallax is between 8″ and 9″, and that this determination is the same, or very nearly the same, as when the observation at the Cape is compared with the same places. He also endeavours to prove, that there is a mistake of one minute in time in writing down the time of the internal contact at the egress at Rodrigues, and that this being corrected, the results of the sun's parallax, by a comparison of the observation at Rodrigues with the observations at the several places on this side of the line, is the same with that which results from all the rest; and this agreement is also an argument that there must have been such a mistake in setting down the time of the internal contact at the egress at Rodrigues. Mr. S. also shows that the parallax of the sun, determined from the observed durations, and from the least distance of the centres, is very nearly the same as that which is determined from the internal contact at the egress, though these last determinations cannot be so much depended on, because of the minute elements from which they are drawn.

He proceeds to compare the observations of the internal contact made on this side of the line only, and thence determines the sun's parallax. To do this, it is necessary that the differences of longitude between the places of observation, compared together, be well ascertained: and in doing this, in all places where the ingress was observed, he has been much obliged to a very ingenious method, proposed by M. Pingré in his aforesaid memoir, to which Mr. S. refers, and for

Tobolsk and	Abo	3h	4m	37s	w.
	Bologna	3	47	46	w.
	Calcutta	1	20	38	E.
	Cajaneburg	2	42	12	w.
	Calmar	3	27	32	w.
	Cape of Good Hope	3	19	32	w.
	Florence	3	49	3	w.
	Gottingen	3	53	35	w.
	Grand Mount	0	46	26	E.
	Greenwich	4	33	7	w.
	Hernosand	3	21	55	w.
	Leskeard	4	51	39	w.
	Madras	0	47	11	E.
	Paris	4	23	51	w.
	Rodrigues	0	20	25	w.
	Rome	3	43	14	w.

Tobolsk

the longitudes of other places he has consulted the Phil. Trans. and the Connoisance des Temps. These differences of longitude are as annexed.

Tobolsk and Savile-house,* London	4^h	33^m	37^s	W.
Stokolm	3	20	41	W.
Tornea	2	56	7	W.
Tranquebar	0	46	9	E.
Upsal	3	22	40	W.

Mr. S. has deviated from the above-mentioned method of M. Pingré in settling the longitude of Stokolm, by the ingress, because it appears clear to him that there must have been a mistake in the observation of the internal contact at the ingress at Stokolm, owing, as mentioned in his former paper, to the small altitude of the sun at the time of the ingress: for by comparing the times of ingress and egress observed at Stokolm and Upsal, we find that the difference of longitude between these two places is $1^m\ 39^s$, and $1^m\ 59^s$, and as we are sure that the observation at the egress gives the difference of longitude the most certain in this case, therefore it follows that the error was at the ingress, and it is easy to prove that the error is in the observation at Stokolm.

To avoid all uncertainty, and to be as clear and distinct as possible, he sets down, in the following table, the observation at the egress at each place compared, the difference of longitude between each place compared, the effect of the parallaxes resulting from the comparison, and also the effect of the parallaxes computed on a supposition that the sun's parallax is = 8″.5, in order that if there is any mistake it may the more easily be discovered. He compares Cajaneburg with 18 places, Bologna with 17 places, and Tobolsk with 18 places. He has explained the manner of these in his former paper on this subject, to which he refers. He then sets down the result from each comparison in the following order, that they may be the more easily seen.

	Sun's parallax.		Sun's parallax.		Sun's parallax.
Cajan. and Stokolm	8.″50	Bolog. and Stokolm	8.″65	Tobolsk and Stokolm	8.″50
Upsal	8.50	Upsal	8.35	Upsal	8.80
Abo	7.33	Abo	8.71	Abo	8.40
Calmar	8.64	Calmar	8.31	Calmar	8.82
Hernosand	9.27	Hernosand	8.36	Hernosand	9.02
Tobolsk	9.06	Tobolsk	8.58	Gottingen	10.57
Gottingen	9.25	Gottingen	7.80	Greenwich	9.11
Greenwich	9.09	Greenwich	7.77	Savile-house	8.66
Savile-house	8.50	Savile-house	8.30	Leskeard	8.34
Leskeard	8.06	Leskeard	9.71	Paris	8.60
Paris	8.43	Paris	8.50	Florence	7.99
Bologna	8.44	Calcutta	8.28	Bologna	8.58
Rome	8.14	G. Mount	9.87	Rome	8.34
Florence	7.68	Tranquebar	8.86	Calcutta	9.64
Calcutta	10.34	Madras	5.76	G. Mount	8.34
G. Mount	8.07	Cajaneburg	8.44	Tranquebar	8.55
Tranquebar	8.36	Tornea	8.23	Madras	9.54
Madras	9.71			Cajaneburg	9.07

* The latitude of Savile-house, London, is = 51° 30′ 50″ N. The latitude of Florence is = 43° 46′ 30″ N. and that of Gottingen = 51° 31′ 54″ N. The latitudes of the rest of the places are set down in Mr. Short's former paper on this subject, only that of Tranquebar should be = 11° 30′ 0″. —Orig.

The mean of these 53 comparisons gives the sun's parallax = 8″.61. Rejecting all those results which differ more than one second from the mean of the whole, the mean of the remaining 45 results gives the sun's parallax = 8″.55. Rejecting all those results which differ more than half a second from the mean of the whole, the mean of the remaining 37 results gives the sun's parallax = 8″.57. And the mean of these three means gives the sun's parallax = 8″.58.

He next compares the observations of the internal contact at the egress made at Paris, Greenwich, Savile-house, Bologna, Madras, Grand Mount, and Tranquebar, with those made at Stokolm, Upsal, Tornea, Cajaneburg, Tobolsk, Abo, Calmar, Hernosand, and Calcutta. The results are as in the following table.

	Stok.	Upsal.	Torn.	Cajan.	Tobo.	Abo.	Calm.	Hern.	Calcu.	Sun's mean par.
Paris	8″.70	8″.60	7″.92	8″.43	8″.60	9″.03	8″.63	8″.42	7″.83	8″.46
Greenwich	9.66	9.61	8.42	9.09	9.11	10.04	10.16	9.20	8.62	9.32
Savile-house	8.50	8 50	7.75	8.50	8.66	9.04	8.50	8.16	7.54	8.36
Bologna	8.65	8 35	8.23	8.44	8.58	8.71	8.31	8.36	8.28	8.43
Madras	10.33	10.15	8.90	9.71	9.54	10.48	10.80	9.77	9.32	9.89
Grand Mount	8.07	7.77	7.35	8.07	8.34	8.31	7.50	7.52	6.96	7.76
Tranquebar	8.50	8.23	7.68	8.36	8.55	8.67	8.12	7.96	7.[illegible]7	8.17
Sun's par. mean . .	8.91	8.75	8.03	8.66	8.77	9.18	8.86	8.48	8.00	8.63

The mean of these 63 results gives the sun's parallax = 8″.63; and if we reject all those which differ more than one second from the mean of the whole, the mean of the remaining 49 results gives the sun's parallax = 8″.50. And if we reject all those which differ more than half a second from the mean of the whole, the mean of the remaining 37 results gives the sun's parallax = 8″.535: the mean therefore of these three means gives the sun's parallax = 8″.55.

Thus by the mean of 53 comparisons the sun's parallax is determined to be = 8″.58, and by the mean of 63 comparisons it is determined to be = 8″.55. The mean of these two means gives 8″.565 for the parallax of the sun on the day of the transit. It may be objected, that this determination cannot be depended on to a very great precision, because the greatest difference of the effect of the parallaxes in any of these comparisons does not exceed 3′ 31″: consequently that this is too small a base, from which we can expect any great exactness in the determination of the sun's parallax. But if we consider the great number of comparisons (no less than 116), the certainty of the differences of longitude of most of the places of observation, and the small differences in the results themselves, Mr. S. thinks that the force of this objection is in some measure removed; and that this determination of the sun's parallax, by the observations at places on this side of the line only, must be very near the truth.

In order therefore to remove the force of this objection entirely, let us next consider the observation at the Cape of Good Hope, by which we shall have a base very near 3 times greater than the former, and also the observation at Rodrigues, by which the base is nearly double of the former. But first he takes notice, that in the Memoir by M. Pingré, before mentioned, the time of the internal contact at the egress at Rodrigues is set down at $0^h\ 36^m\ 49^s$. But in the same volume there is an account of M. Pingré's observation sent to the Royal Academy before his arrival in Europe, and the time of the internal contact is therein set down at $0^h\ 34^m\ 47^s$. Also in a letter from him to the Royal Society, on his arrival in Europe at Lisbon, and dated the 6th of March, 1762, and which letter is printed in the Phil. Trans., vol. lii, the time of the internal contact is therein set down at $0^h\ 34^m\ 47^s$ true time. In another letter from him at Lisbon to the Royal Society, dated 14th of March 1762, the time of the internal contact is again set down at $0^h\ 34^m\ 47^s$ true time, and he ends this letter in these words, 'Notez que l'attouchement interne des bords s'est faite à $0^h\ 34^m\ 47^s$. Je fais cette remarque, parceque, vû la proximité de prononciation, qui dans notre langue est entre 30 et 40, cette attouchement se trouvoit marqué 10^s plutot qu'il ne devoit l'être, dans une copie que j'ai faite pour mon usage: cette erreur aura peut etre passé dans quelque autre copie. Mais, selon l'original, il faut absolument lire $0^h\ 34^m\ 47^s$.' M. Pingré has no where, in the said memoir, given any reason for this alteration of the time of the contact. If the internal contact at the egress at Rodrigues happened at $0^h\ 34^m\ 47^s$; and if this is compared with the same observation at Tobolsk, the parallax of the sun comes out $= 7''.36$. If the time of the contact at Rodrigues was at $0^h\ 35^m\ 47^s$, and if this is compared with the same observation at Tobolsk, then the parallax of the sun is found $= 8''.62$. Again if the time of the contact at Rodrigues was at $0^h\ 36^m\ 49^s$, and if this is compared with the observation at Tobolsk, the parallax of the sun will be found $= 9''.93$. But to return.

M. Pingré, in his letter to the Royal Society, dated at Lisbon March 14, 1762, sets down the time of the internal contact at the egress at $0^h\ 34^m\ 47^s$ true time: and with regard to the time of the external contact expresses himself thus: à $0^h\ 53^m\ 18^s$ le soleil a paru pendant 3 ou 4 secondes. Je n'ai pas vu le disque du soleil bien fermé, il me paroissoit un peu alteré au lieu de la sortie de Venus. M. Thuillier ne voyoit rien avec la lunette de 9 pieds. J'ai de la peine à me persuader que Venus soit sortie plutot.' It is plain from these words that M. Pingré believed that the external contact did not happen before $0^h\ 53^m\ 18^s$. This being allowed, let us compute the duration of the egress at Rodrigues, which we shall find $= 17^m\ 55^s$. It follows therefore that the internal contact happened at $0^h\ 35^m\ 23^s$. But this supposes that the observer could see the very last contact of Venus with the sun's limb, the contrary of which Mr. S. has shown in a former paper on this subject. We are therefore certain that the external contact happened later than $0^h\ 53^m\ 18^s$, by several seconds, consequently the internal contact happened later than $0^h\ 35^m\ 23^s$, by several seconds. On the whole therefore, we may safely conclude that there is a mistake of one minute in setting down the time of the internal contact at the egress at Rodrigues, and that instead of $0^h\ 34^m\ 47^s$, it should be $0^h\ 35^m\ 47^s$. This sort of mistake has happened several times in the observations of this transit, but they are easily discovered.

Mr. S. now proceeds to compare the observation of the internal contact at the Cape, with the observation of the same contact at Rodrigues, and at 20 places to the north of the line; and also the observation at Rodrigues with the same 20 places; and the results of the sun's parallax from these several comparisons are as follow:

	Sun's parallax.		Sun's parallax.
Cape of Good Hope and Rodrigues	8″.54	Rodrigues and Cape of Good Hope	8″.54
Paris	8.56	Paris	8.58
Bologna	8.54	Bologna	8 54
Rome	8.74	Rome	8.94 r
Florence	8.91 r	Florence	9.24 r
Gottingen	8.40	Gottingen	8.30 r
Greenwich	8.40	Greenwich	8.33 r
Savile-house	8.57	Savile-house	8.59
Leskeard	8.69	Leskeard	8.81 r
Calmar	8.55	Calmar	8.56
Hernosand	8.51	Hernosand	8.50
Upsal	8.57	Upsal	8.58
Stokolm	8.58	Stokolm	8.58
Abo	8.63	Abo	8.68
Cajaneburg	8.56	Cajaneburg	8.57
Tornea	8.41	Tornea	8.33 r
Tobolsk	8.64	Tobolsk	8.62
Calcutta	8.43	Calcutta	8.37
Madras	8.28 r	Madras	8.03 r
G. Mount	8.70	G. Mount	8.85 r
Tranquebar	8.60	Tranquebar	8.74

The mean of the 21 comparisons with the observation at the Cape, gives the sun's parallax = 8″.56. There are only 2 of these 21 comparisons, marked with the letter r, which differ more than $\frac{3}{10}$ of a second from the mean of the whole; let these be rejected, and the mean of the remaining 19 results gives the sun's parallax = 8″.56.

If we select out of these 21 comparisons those places whose difference of longitude may be supposed to be the best determined, the mean of these may be regarded as the most exact determination, viz. Paris, Bologna, Greenwich, Savile-house, Upsal, Stokolm, Cajaneburg and Tobolsk; the mean of these gives the sun's parallax = 8″.55; and if we leave out the results of Greenwich and Tobolsk, which differ the most from the rest, the mean of the remaining 6 results gives it = 8″.56, the same as before.

The mean of the 21 comparisons with the observation at Rodrigues gives the sun's parallax = 8″.57; and if we reject 8 of them, which are marked with the letter r, and which differ more than $\frac{2}{10}$ of a second from the mean of the whole, the mean of the remaining 13 results gives the sun's parallax 8″.57, differing only one hundredth part of a second from that which was determined from the observation at the Cape, and agreeing in a most surprizing manner with what was formerly determined by the comparisons of the observations at places on this side of the line only, where the base was so small, as before said; a most convincing proof of the great precision with which the parallax of the sun is determined by the late transit of Venus.

We shall now inquire into the limits of the error that may attend the determination of the parallax by the observation of the internal contact. An error of $1^m\ 10^s$ of time in the observation at Tobolsk, when compared with the observation at the Cape, will produce an error of 1″ in the sun's parallax: and if we suppose an error of 35^s of time in the observation at Tobolsk, and an error of the same quantity in the observation at the Cape, and both in contrary directions, this also will produce an error of only 1″ in the sun's parallax. If therefore no greater error could be committed in the observations at Tobolsk and the Cape, we are certain that the comparison of Tobolsk and the Cape gives the sun's parallax so exact, that the error does not exceed one second from the true parallax. But this is too great an error to be supposed in the observations, because Mr. S. has shown, in the former paper, that an error of only 6^s in time was committed in the observation of the contact by persons observing even in the same place; therefore if we suppose an error of 6^s of time in the observation at Tobolsk, and an error of the same quantity in the observation at the Cape, and both in contrary directions, the error produced in the parallax by those 12^s, will amount only to $\frac{1}{6}$ of a second, even though we had only these two observations to determine the sun's parallax: but since we have a great number of very good observations, made at other places, it follows that the mean of all these must give the sun's parallax to a less error than $\frac{1}{6}$ of a second, and consequently very near the truth.

In all places where the internal contact at the egress was observed, and where there were more observers than one, we find a difference in the time of each observer; the observation at Greenwich is an exception to this, as the 3 observers all agree to the same second, in the observation of the contact of Venus with the sun's limb; which is the more surprizing as they used telescopes of different constructions and of different magnifying powers. This coincidence not only surprized Mr. S., but also the Rev. Mr. Hornsby, now Savilian professor of astronomy at Oxford. Mr. Hornsby went to Greenwich in the beginning of the year 1762, and on his return told Mr. S. that his surprize was at an end, for he had been informed at Greenwich, that Mr. Green, the assistant observer there, as soon as he judged that the internal contact was formed, called out *now*

This must certainly have caused some disturbance to the other observers, and might possibly influence their judgment: and the fact it seems was, that each observer had a second watch in his hand, and they instantly stopped their watches, each having his hand at his watch ready to stop. This problem therefore is easily solved, and the surprize at the coincidence entirely vanishes; so that this observation can be looked on as no more than the observation of one person, and he too not much practised in observing. It is proper further to observe, that another person was present at this observation, who confirmed the above account.

The very near coincidence of the three observers at Greenwich, in the time of the external contact, remains now to be accounted for. Mr. Green did not call out at this time, because he was forbid by Dr. Bradley, who was present, though not in a condition to observe because of his bad state of health. This problem therefore may be solved in the following manner. The observation of the external contact was undoubtedly more uncertain than the former, and yet we find two of the observers agreeing to the same second, and the third differing only one second from them. If we attend to the following circumstances, we shall be immediately satisfied by them. Each observer had a second-watch in his hand; the 3 observers were at the same window of the same room, one of them on the leads immediately without the window, and the other two within the window; therefore each observer was within hearing and seeing of each other; consequently the instant one of the observers stopped his watch, may it not be presumed that the noise of the nicking of it might be heard by the rest? especially as there was a profound silence during the time of the observation. Mr. S. has thought proper to take notice of these facts, because several persons both at home and abroad have expressed their surprize at this coincidence, and that such an exactness may not be established as a precedent in these sorts of observations; and because he thought it essentially necessary, in all sorts of observations, especially in one of so much importance in astronomy as this, that every the minutest circumstance should be particularly related.

We are now to find the limits of the error arising from the difference of longitude between Tobolsk and the Cape. Mr. S. found that an error of $1^m\ 10^s$ in time, in the difference of longitude between these two places, will cause an error of $1''$ in the sun's parallax. But as we are certain that this error in longitude does not take place; therefore we are certain that the error in the parallax is within one second of the truth. The difference of longitude between the Cape of Good Hope and Paris is determined, both by the observations of M. de la Caille and Mr. Mason; the difference of longitude between Paris and Upsal in Sweden is settled by the observations of Jupiter's first satellite, and the difference of longitude between Upsal and Tobolsk is settled, by the observations of the contact at the ingress at both places, by the method of M. Pingré above mentioned. Therefore the difference of longitude between the Cape and Tobolsk is very exactly settled, so exactly, that Mr. S. was persuaded that the error does not amount to 5 or 6 seconds. Therefore the error in the parallax arising from the error of the difference of longitude is extremely small, scarcely amounting to $\frac{1}{18}$ part of a second. Therefore we are certain that the error in the sun's parallax, arising both from the error of observation and the error of longitude, does not exceed $\frac{1}{4}$ of a second in the comparison of the observations of the internal contact at Tobolsk and at the Cape, even though we had no more observations to determine the sun's parallax; but the mean of a great many more must bring it very near the truth.

Mr. S. now proceeds to determine the parallax of the sun from the total durations observed at different places. If therefore we compare the durations observed at Tobolsk, Cajaneburg, Abo, and Tornea, with the durations observed at Madras, Grand Mount, and Tranquebar, which give the greatest differences, the results of the sun's parallax will be as follow.

	Sun's Par.	Dif. of du.		Sun's Par.	Dif. of du.
Tobolsk and Madras .. =	9″.61	2^m 50^s	Abo and Madras ...... =	10″.66*r*.	1^m 34^s
G. Mount =	8.33	2 27	G. Mount =	8.33	1 11
Tranquebar =	8.52	2 40	Tranquebar =	8.40	1 24
Cajaneburg and Madras =	10.09*r*.	1 49	Tornea and Madras .... =	9.20	1 34
G. Mount =	8.00	1 26	G. Mount =	7.00*r*.	1 11
Tranquebar =	8.33	1 39	Tranquebar =	7.50*r*.	1 24

The mean of these 12 results gives the sun's parallax = 8″.68; and if we reject 4 of them, which are marked with the letter *r*, and which differ the most from the rest, the mean of the remaining 8 gives the sun's parallax = 8″.61. This determination of the sun's parallax cannot be depended on to any great precision, because of the small differences between the durations compared, the greatest of which amounts only to 2^m 50^s, and also because of the small number of comparisons. It serves only to show nearly what is the quantity of the sun's parallax.

We are now to determine the limits of the error in the determination of the sun's parallax by the durations observed at two different places. The greatest difference of duration is between Tobolsk and Madras, which amounts only to 2^m 50^s. If therefore an error of 20^s in time is committed in the observations of the ingress and egress, at both the places compared, this error of 20^s in time will cause an error of 1″ in the result of the sun's parallax; and in the comparisons of these places where the difference of duration is less, will occasion a greater error; and therefore the determination of the parallax, by this method, cannot be depended on to any great exactness, because of the small differences of the durations compared. In this method however we are free from the uncertainty arising from the difference of longitude not being exactly known.

Mr. S. now proceeds to the determination of the sun's parallax by the least distance of the centres. There came to his hands only two measurements of the greatest distance of the limbs of the Sun and Venus, one at Tobolsk and the other at Rodrigues. He only considers the measurement at Rodrigues, because there seems to be some mistake in the measurement at Tobolsk. If we suppose the sun's parallax = 8″.5, then the apparent middle of the transit happened at Rodrigues at $9^h\ 37^m\ 30^s$. There is a measurement by M. Pingré of the greatest distance of the limbs at $9^h\ 38^m\ 13^s$, which is so near the middle of the transit that we may safely take this quantity, viz. 5′ 54″.6,* for the greatest distance of the limbs of the Sun and Venus, and especially as it is marked an exact observation. This measurement therefore gives the apparent least distance of the centre of the Sun and Venus at Rodrigues = 9′ 21″.4. Supposing then this measurement to be exact, here follows an irrefragable argument, independent of all other methods, to prove that the parallax of the sun is very nearly = 8″.5. Let us suppose the sun's parallax = 10″; and let us compute by the following method the apparent least distance of the centres at Tobolsk; thence we shall find that the geocentric least distance of the centres at Tobolsk is 567″.416, and by the observation at Rodrigues the geocentric least distance of the centres is = 572″.612; so that on this supposition we have two different geocentric least distances of the centres, which being absurd, it follows that the sun's parallax is not 10″. Again let us suppose that the sun's parallax is = 7″; we shall find that the geocentric least distance of the centres by the observation at Tobolsk is = 575″.356, and by the observation at Rodrigues it is = 569″.248. Thus then again we have two different geocentric least distances of the centres, which being absurd, it follows that the parallax of the sun is not 7″. Again if we suppose the sun's parallax = 8″ or 9″, we shall find that the same absurdity will follow, but in these last two suppositions we shall find that the differences of the geocentric least distances of the centres are not so great as on the suppositions of 10″ and 7″, it therefore follows that the parallax of the sun is less than 9″ and more than 8″. And if we continue to reason in the same manner, we shall find, that on the supposition that the sun's parallax is = 8″.5, the geocentric least distances of the centres, severally found by the observation at Tobolsk and at Rodrigues, is very nearly the same, consequently that the sun's parallax is very nearly = 8″.5. If we pursue this subject to a greater precision, and suppose that the measurement of the greatest distance of the limbs of the sun and Venus, taken by M. Pingré, to be perfectly exact, and compute on true † principles the apparent least distances of the centres, from the

* Mr. S. all along considers the observation of M. Pingre at Rodrigues as it is printed, from his own letter, in the Phil. Trans.—Orig.

† He says on true principles, because he had reason to think that there is a mistake in the method given by M. Pingré in the aforesaid memoir.—Orig.

durations observed at the different places in the north (the method of which is afterwards given) the parallax of the sun will come out as follows, when they are compared with that measured at Rodrigues:

	Cajan.	Calm.	Tobol.	Torn.	Upsal.	Stock.	Abo.	Herno.
Rodrigues	8″.60	8″.58	8″.65	8″.48	8″.60	8″.40	8″.63	8″.55

The mean of these 8 comparisons gives the sun's parallax = 8″.56, being the very same as that which we found before by the comparisons of the internal contacts.

Again let us reduce the observed durations, at the following several places, to the centre, on the supposition that the sun's parallax is = 8″.56, as in the following table.

1. Tobolsk.	2. Cajaneburg.	3. Tornea.	4. Upsal.
5h 48m 53s = Obs. Du.	5h 49m 54s = Obs. Du.	5h 50m 9s = Obs. Du.	5h 50m 26s = Obs. Du.
0 9 6 = Parallax.	0 8 8 = Parallax	0 8 3 = Parallax.	0 7 36 = Parallax.
5 58 59 = Cent. D.	5 58 2 = Cent. D.	5 58 12 = Cent. D.	5 58 2 = Cent D.
5. Stockholm.	**6. Abo.**	**7. Hernosand.**	**8. Calmar.**
5 50 42 = Obs. Du.	5 50 9 = Obs. Du	5 50 26 = Obs. Du.	5 50 39 = Obs. Du.
0 7 37 = Parallax.	0 7 49 = Parallax.	0 7 39 = Parallax.	0 7 24 = Parallax.
5 58 19 = Cent. D.	5 57 58 = Cent. D.	5 58 5 = Cent. D.	5 58 3 = Cent. D.
9. Calcutta.	**10. Madras.**	**11. Grant Mount.**	**12. Abo.**
5 50 36 = Obs. Du.	5 51 43 = Obs. Du.	5 51 20 = Obs. Du.	5 51 33 = Obs. Du.
0 7 35 = Parallax.	0 6 35 = Parallax.	0 6 35 = Parallax.	0 6 26 = Parallax.
5 58 11 = Cent. D.	5 58 18 = Cent. D.	5 57 55 = Cent. D.	5 57 55 = Cent D.

The mean of these 12 central durations gives the mean central duration = 5^h 58^m 5^s. From this central duration we shall find that the geocentric least distance of the centres is = 571″ or 9′ 31″. Let us compare the above apparent least distance of the centres measured at Rodrigues, with this geocentric least distance of the centres, and we shall find that the parallax of the sun thence resulting is = 8″.56, the same as before. These results of the parallax, arising from the comparisons of the apparent least distances of the centres, agreeing with the former determinations of the parallax by the internal contacts, are a proof of the accuracy of this measurement of the greatest distance of the limbs made by M. Pingré at Rodrigues.

There are 12 places at which the total duration was observed, 3 of these had a northern parallax of latitude at the middle of the transit, the other 9 had a southern parallax of latitude. Let the apparent least distance of the centres at each place of observation be found by the following method; let these be compared together, and we shall have the parallax of the sun resulting from them. For this purpose Mr. S. computed the apparent least distance of the centres at the 8 following places, and compared them with the apparent least distance of the centres at the 4 following places, and from each comparison he computed the parallax of the sun, which are as in the following table.

	Cajan.	Calm.	Tobol.	Tornea.	Upsal.	Stok.	Abo.	Herno.	Sun's P. mean.
Tranquebar	8″.48	8″.45	8″.54	8″.31	8″.48	8″.20	8″.52	8″.42	8″.42
Madras	8.79	8.76	8.93	8.61	8.79	8.50	8.82	8.73	8.74
G. Mount	8.42	8.38	8.45	8.24	8.42	8.12	8.45	8.35	8.35
Calcutta	8.69	8.65	8.81	8.43	8.68	8.35	8.73	8.61	8.62
Sun's Par. mean	8.59	8.56	8.68	8.40	8.59	8.29	8.63	8.53	8.53

The mean of these 32 comparisons gives the sun's parallax = 8″.53. This very near agreement with the former determinations is somewhat surprising, when we consider the smallness of the base from which they are computed, the greatest scarcely exceeding 20″ of an angle. But we are also to consider that the apparent least distance of the centres may be found from the duration observed to a very great exactness, and nothing affects the accuracy of it but the errors in the observation. Let us suppose that an error of 6ˢ in time happened in each of the observations of the ingress and egress, both in contrary directions; the sum of the errors therefore in each comparison will amount to 24ˢ of time: this will produce an error of 1″ of space in the apparent least distance of the centres by computation; but this error of 1″ cannot produce an error of so much as half a second in the determination of the sun's parallax. It therefore follows on the above supposition of an error of 24ˢ of time in the observation, that though we had no other observations of the transit of Venus than two of the above total durations, (suppose that of Cajaneburg and Madras) yet we should have been absolutely certain of the parallax of the sun within less than an error of half a second, and therefore of course it follows that the mean of so great a number of results must be very near the truth.

This determination of the sun's parallax by the least distance of the centres, is also a convincing proof that there is no mistake in the observation of Mr. Mason at the Cape, as alleged by M. Pingré; and that there must be a mistake of 1ᵐ in setting down the time of the internal contact at the egress at Rodrigues, notwithstanding M. Pingré, in the aforesaid memoir, prefers his observation to that of Mr. Mason, because, as he says, that after a strict examination of all the circumstances attending his observation, he could not find any mistake in it, but also because he has proved that no mistake could possibly be committed. In this determination of the parallax by the apparent least distance of the centres, we are not embarrassed with an exact knowledge of the difference of longitude between the places compared; which therefore in some measure compensates for the smallness of the base.

The same irrefragable argument, made use of in the apparent least distance of the centres measured at Rodrigues, to prove that the parallax of the sun is very nearly = 8″.5, may likewise be deduced from the apparent least distance of the centres computed from the total durations observed at these 12 places, but with more certainty; because the determination of the apparent least distances of the centres, from the observed total durations, may be depended on to a very great precision: but the same cannot be said with regard to the apparent least distance of the centres measured at Rodrigues. For M. Pingré tells us that he used a very good micrometer, fitted to a refracting telescope of 9 feet focus, the object-glass of which was but an indifferent one; and we are very certain that in measuring with a micrometer of this sort, dark objects on a white field or ground, if the image is any way indistinct, the angle measured will be less than the true angle, and vice versa when a bright object is measured on a dark ground. As a proof of this remark, we find that M. Pingré measured and found the diameter of Venus when on the sun, = 54″.7, whereas we are certain that it was above 58″; and therefore we may presume that the measurements of the greatest distance of the limbs might be greater than the true distance; and, as a further proof of the uncertainty of the measurements made with this instrument, we find that M. Pingré makes the distance of the limbs greatest, several minutes after it was past the greatest.

Mr. S. now produces at one view the means of the several determinations of the sun's parallax, by the before-mentioned 3 several methods, which contain the substance of this whole paper.

1^{mo} The mean of 116 comparisons of the internal contacts observed at places to the north of the line only, gives the sun's parallax. . . . . = 8.565

2^{do} The mean of 21 comparisons of the internal contacts, with that at the Cape, gives the sun's parallax . . . . = 8.56

3^{tio} The mean of 21 comparisons of the internal contacts with that at Rodrigues, gives the sun's parallax . . . . = 8.57

4^{to} The mean of the comparisons of the total durations gives the sun's parallax. . . . . = 8.61

5^{to} The mean of the apparent least distances of the centres compared with that measured at Rodrigues, gives the sun's parallax. . . . . = 8.56

6^{to} The mean of the apparent least distances of the centres by computation from the total durations compared together, gives the sun's parallax. . . . . = 8.53

The mean of these 6 means gives the sun's parallax . . . . = 8.566

And if we reject the mean arising from the comparisons of the total durations, which is the least certain, the mean of the other 5 means gives the sun's parallax. . . . . = 8.557

Thus is the sun's parallax on the day of the transit concluded to be = 8″.56, and that from 3 different modes of comparing together a great number of observations variously combined, the several results so nearly coinciding, that it seems impossible that the mean of them all can err $\frac{1}{10}$ of a second, and that probably the error does not exceed $\frac{1}{500}$ part of the whole quantity, as Dr. Halley had many years since confidently presaged.

P. S. M. Pingré, in his aforesaid memoir, seems to think that there must be some mistake in Mr. Mason's observation at the Cape, because by comparing the observations of Jupiter's satellites made by Mr. Mason at the Cape, with those made by M. Messier at Paris, he finds the difference of longitude between these two places less, by 1^m of time, than between Paris and the observatory of M. de la Caille at the Cape; and therefore imagines that Mr. Mason's observatory was to the west of M. de la Caille's. If M. Pingré had looked into the map of the Cape by M. de la Caille, he would have seen that if Mr. Mason's observatory had been 1^m of time to the west of M. de la Caille's, it must have been in the ocean. Mr. S. is not at all surprised to see a difference or error of 1^m of time in deducing the difference of longitude between Paris and the Cape, by comparing Mr. Mason's observations with those of M. Messier: for he finds in the last volume of the memoirs for 1761, a difference of $1^m\ 5^s$ between M. de la Lande and M. Messier in an immersion of the first satellite of Jupiter, both of these gentlemen observing at Paris, owing he supposes to the different goodness of the telescopes used on this occasion; for M. de la Lande says that he used an 18 foot refractor, the object-glass of which was tolerably good; and that M. Messier made use of a very good reflector of 30 inches. If M. Pingré had taken the trouble of looking into the Philos. Trans., vol. 52, he would have found there observations made at the Cape, and in Surrey-street, London, of the immersions of the first and 2d satellites of Jupiter, with reflecting telescopes of equal goodness, of 2 feet focal length, where the difference of determination of the longitude of these 2 places does not exceed one second in those of the first satellite, and not above 16^s in those of the second satellite. Mr. Mason's observatory at the Cape was about half a mile to the south of M. de la Caille's, and about 10 or 12 yards to the west of the meridian of the same.

M. Pingré also seems to think that the time shown by Mr. Mason's clock was taken from a false meridian. When M. Pingré shall read the account given by Mr. Mason of his observations at the Cape, which he says in his memoir he has not seen, Mr. S. is persuaded he will be fully satisfied, from the many equal-altitudes taken by Mr. Mason, that there can be no doubt of the times of his observations being found from a true meridian.

Mr. S. takes notice of a remarkable expression in the history of the memoirs of the Royal Academy at Paris, p. 96, for the year 1757. It is there said, that the English intended to send an astronomer to North America, to observe the transit of Venus (according to the plan laid down by Dr. Halley) before they saw the map of the transit by M. de L'isle; and the authority produced for this assertion are the English news-papers, which, if they had understood the nature of these papers, can be no authority at all. Mr. S. therefore on the best authority informs the gentlemen who are the compilers of the history of these memoirs, that the Royal Society never once thought of sending an observer to North America, even before they saw the map of the transit by M. de L'isle.

N. B. In this paper Mr. S. has employed the same elements as in his former paper on this subject, except that in reducing time to space, he has made use of 4′ 0″.03 for the horary motion of Venus in her path.

Mr. S. adds the following method of determining the apparent least distance of the centres of the sun and Venus from the observation of the total duration of the transit observed at any one place, and also the geocentric least distance of the centres.

Let BCPL, (fig. 3, pl. 1,) represent the sun's disk; LSP the ecliptic; OR the geocentric path of Venus over the sun; AD the apparent path at any place, to the north of the plane of Venus's orbit; SM the geocentric least distance of the centres; AK the parallax of latitude at the internal contact at the ingress; ND the parallax of latitude at the internal contact at the egress; A*b* the parallax of longitude at the ingress; and CD the parallax of longitude at the egress. It is required to find SF, a perpendicular let fall from the centre of the sun on the apparent path, and thence to find SM the geocentric least distance of the centres of the sun and Venus.

If the parallax of longitude at the ingress retard, and the parallax of longitude at the egress accelerate, the total duration will be shortened by the sum of these 2 parallaxes of longitude, viz. by A*b* and CD; and if we make no allowance for these parallaxes, the apparent path will appear to have been BC, consequently a perpendicular from the sun's centre on BC will be SE, longer than the perpendicular on the true apparent path, by FE. Now since it is certain that the parallaxes of longitude do not depress or elevate the planet, but only alter the position of the planet in a direction perpendicular to the axis of its orbit, therefore the parallaxes of longitude, in time, are in this case to be added to the observed time of the total duration: in consequence of which, the observed time of total duration, *bc* + A*b* + CD are = to the chord described by the planet in its passage over the sun; and if the semidiameters of the sun and Venus are known, their difference is known, which is = to the line AS: AF, from what has been said, is also known, therefore SF may be found. But this SF is not the apparent least distance of the centres; for if we compute the parallax of latitude for the apparent middle of the transit, we shall find it greater than MF, which MF is only a mean between the parallaxes of latitude at the ingress and egress. Let therefore the difference between MF and the parallax of latitude, computed for the middle of the transit, be added to SF, and the sum will be = to the apparent least distance of the centres nearly; and if from this sum we subtract the parallax of latitude, computed for the middle of the transit, the remainder will be the geocentric least distance of the centres nearly.

A true, and more ready method, to find the geocentric least distance of the centres, consequently the apparent least distance of the centres, at any place where the total duration has been observed, is thus:—

Reduce the total duration observed to the centre; reduce the central semi-duration, in time, into space; then in the right-angled triangle SMA, fig. 4, or SMa, we have given the two sides SA or Sa, and AM or aM: therefore the third side SM may be found; therefore SM the geocentric least distance of the centres is found; and if to SM we add or subtract the parallax of latitude for the apparent middle of the transit, the sum or difference will be the apparent least distance of the centres.

EXAMPLE. *To find the apparent least distance of the centres at Tobolsk.*

The total duration observed at Tobolsk, was $5^h\ 48^m\ 53^s$, add $9^m\ 3^s$ (= to the effect of the parallaxes of longitude and latitude, both for the ingress and egress, on the supposition that the sun's parallax is = 8″.5) to this total duration, the sum $5^h\ 55^m\ 56^s$ is = to the central duration; consequently $2^h\ 58^m\ 58^s$ is = the central semi-duration, reduce this time into space, and it will be found = to 715″.956 = AM or aM, and SA or Sa (= difference of the semi-diameters of the sun and Venus) = 916″: therefore SM will be found = 571″.37 = the geocentric least distance of the centres of the sun and Venus. The parallax of latitude computed on the above supposition of the sun's parallax, for the apparent middle of the transit at Tobolsk, will be found = 14″.13, which being added to the geocentric least distance of the centres above found, the sum 585″.50 will be the apparent least distance of the centres at Tobolsk.

XLVIII. *Of a Case in which Green Hemlock was applied. By Mr. Josiah Colebrook, F.R.S.* p. 346.

Ann James, of the parish of Boughton Monchelsey in Kent, aged 55 years, a married woman, had for some years complained of a pain and hard lump in each breast. In Sept. 1762 she asked Mr. C.'s advice about them: on examining them, he found a very hard schirrus in each breast: that in the left breast had the mamillary glands indurated and knobbed, like ramifications toward the axilla, a little adhesion to the pectoral muscle, was as large as a turkey's egg, and she was under daily apprehensions that it would break. That in the right breast was not near so large, nor had ramifications, nor adhered like the other. She complained of most excruciating stabbing pains in both breasts, which prevented her having any rest in the night, and made her so very miserable all day, whether she lay down, stood, sat, or walked, that she was unable not only to go out to work, but even to do any thing for her family at home, not even to make her own bed: and she had totally lost her appetite: her usual employ was spinning, washing, brewing, and what in London is called the business of a chairwoman. The breasts were but little discoloured, but the pains she described, and the ramifications attending the schirrus in the left breast, induced him to pronounce it a cancer.

He advised her to take the green hemlock, viz. cicuta major vulgaris caule maculoso; to mince it with parsley, to disguise the taste, and eat it with bread and butter twice or three times in a day, the 3d part of a leaf, or one of the 3 divisions, which are in each leaf, at a time; that her constant drink should be lime water and milk; that she should take as many millepedes every day, as her stomach would bear, or she could get; that her body should be kept open by rhubarb or magnesia, as occasion required; and that she should have an issue in her arm, and lose 6 or 8 oz. of blood once in 6 or 8 weeks, if her pains continued. He desired a leaf might be weighed, that he might ascertain the quantity of each dose, and found she took 15 grs. of the green plant 3 times in a day: finding it agree with her stomach, and that it eased her pains, though it caused a tingling to her fingers' ends, she increased the quantity. In the beginning of Nov. she had a very large menstrual discharge, which had not happened to her for many years before; the schirrus was much lessened, and her pains were considerably abated.

About the end of Nov. she found her breast more swelled, and the pain more acute than it had been for 6 weeks before; had a restlessness, giddiness in her head, and weight over her eyes; the discharge of the issue stopped, and a violent humour came all round the orifice. As he had desired a little blood might be taken away, if occasion required it, she was bled about the last day of Nov. on which she fainted away, and afterwards had fainting fits 2 or 3 times in a day, great sickness at her stomach, and sometimes bled at the nose. On these symptoms coming on, though she had taken somewhat purgative twice in a week, from her first beginning to take the hemlock, it was thought proper to suspend the taking the hemlock for some days. He then ordered her an infusion of the cortex Peruvianus 1 oz. in powder, to a quart of spring water, to let it stand 3 or 4 days, shaking it every day; and then that she should take 3 spoonfuls twice in a day; that she should repeat the hemlock in the same quantity she took at the first; that she should not again exceed that quantity on any account; and that she should continue the lime-water and the millepedes.

About the latter end of Dec. she had a regular appearance of her menses, but very moderate, her pains were very much abated, and the schirrus much less, though she often complained of a swimming in her head, and a restlessness in the night. From this time, viz. the end of Dec. she continued mending in all respects so much that he heard nothing of her till March 1763; when he was told that Ann James was surprisingly recovered, that her cancer was much lessened, that she could use her arms, work for herself and family, and that her pains were so much abated that she was quite happy.

In Sept. he was at Boughton, saw her, and examined her breasts: the schirrus in her left breast was not half so large as when he saw it before; the ramifications were all gone, and it did not at all adhere to the pectoral muscle; her appetite was good, and she was able to do her business as usual; she said she sometimes felt some of those stabbing pains she before complained of, but they were not frequent nor very severe.

The beginning of last Nov. he had a further account of her, then stating that the lump in her breast, which she expected would break, was not half so large as it was, and continued decreasing; that she had great spirits; and from being one of the most miserable of the human species, she then enjoyed ease and happiness, and could without much pain do all her usual business, as washing, brewing, baking, and needle-work, except spinning, that motion still giving her great pain: she continued to take $\frac{1}{2}$ dr. of dry hemlock twice in a day, but took the green, when she could get it, in larger quantities.

XLIX. *Of a Remarkable Meteor. By Mr. Sam. Dunn.* p. 351.

In Sept. and Oct. 1763, on many different days, but always in the afternoon, when the sun was nearly at the same height above the horizon, Mr. D. was amused with the appearance of a kind of meteor, not before noticed. As it appeared under nearly the same circumstances at other times, and therefore may contribute towards the better understanding the theory of a parhelion, he gives the description of this meteor, as it appeared the 6th of Oct. last, at 5 o'clock afternoon. A kind of mock sun appeared of equal altitude with the real sun, about $22\frac{1}{2}^{\circ}$ southerly from him. A little above the mock sun the sky was clear, but the phenomenon was in the midst of clouds that were not very dense. The diameter of this phenomenon was nearly like that of the real sun, and a remarkable red stream of light pointed from it, as at all other times towards the real sun, which shined clearly at the same time. As there was no descending rain, nor any other colour of the rainbow, he thinks this a meteor not yet registered among meteorological observations.

L. *Of a Blow on the Heart, and of its Effects. By Mark Akenside, M.D., F.R.S.* p. 353.

Sept. 11, 1762, Richard Bennet, a lad about 14 years of age, was brought to a consultation at St. Thomas's Hospital. His disorder was a palpitation of the heart, so very violent to the touch, that they all concluded it to be an aneurysm, and without remedy. He had a frequent cough. His pulse was quick, weak, and uneven, but not properly intermitting. It was apparent that nothing could be done, further than by letting blood in small quantities; and by the use of emollient pectoral medicines, to lessen now and then, however inconsiderably, the extreme danger to which he was continually subject. He was taken into the hospital that same day, being Saturday, and treated according to what had been agreed on. But on the Tuesday morning following he died, without any previous alarm or alteration.

The origin of his complaint was a blow, which he had received 6 months before, from the master whom he served as waiter in a public-house. The master had owned that he had pushed him slightly on the left side with his hand. The boy informed them that he himself was then carrying a plate under his arm; and that the blow or push from his master drove the edge of the plate forcibly between 2 of his ribs. He was immediately very ill of the hurt, sick, and in great pain. His mother also informed them, that she thought the palpitation was more violent about a fortnight after the accident than when they examined him. The day after the blow, they took 8 oz. of blood from his arm; about 3 weeks after that, they again opened a vein, but got not much from it: and 3 weeks after, they let him blood the last time, to the amount of 8 oz. He began to have a cough soon after the hurt, with frequent spittings of blood in very large quantities; and had nocturnal sweats almost the whole 6 months, during which he survived the blow. About 4 months after it, there came over the umbilical region of the abdomen a livid appearance, like a mortification: but it went off gradually, and at length vanished. He had nothing particular in his habit of body, or state of health; save that, about a year before this accident, he had been crippled with the rheumatism. He was, when they saw him, a good deal reduced, but had not a hectic nor consumptive look.

On the day of his death, Mr. Cowell opened him; when to their great surprize, they found no aneurysm, nor the least extravasation of blood, either from the cavities of the heart or the large vessels. But on the left ventricle of the heart, near its apex, there was a livid spot, almost as large as a half-crown piece, bruised and jelly like; the part underneath being mortified quite to the cavity of the ventricle. Thence upward, towards the auricle, there went several livid specks and traces of inflammation, tending in like manner to gangrene. The heart also, throughout its whole surface, adhered very closely to the pericardium; and the whole outer surface of the pericardium, as closely to the lungs. The other viscera were quite sound. So that the mischief here was properly a contusion of the heart; the edge of the plate having struck it, probably at the instant of its greatest diastole. This produced an inflammation on its surface, followed by a gangrene, and terminating in that double adhesion, by which the whole heart was fast tied up, till on this account, as well as by reason of the mortification, it was no longer able to circulate the blood.

LI. The Method of Making Nitre in Podolia. By —— Wolf, M. D. p. 356.

In the present improved state of chemical knowledge, it would be of little use to give a translation or abstract of this paper on the artificial preparation of nitre, or to notice the author's conjectures concerning the origin of this salt.

LII. An Essay towards Solving a Problem in the Doctrine of Chances. By the late Rev. Mr. Bayes, F. R. S. Communicated by Mr. Price. p. 370.

This problem is to this effect: "Having given the number of times an unknown event has happened and failed; to find the chance that the probability of its happening should lie somewhere between any two named degrees of probability." In its full extent and perfect mathematical solution, this problem is much too long and intricate, to be at all materially and practically useful, and such as to authorize the reprinting it here; especially as the solution of a kindred problem in Demoivre's Doctrine of Chances, p. 243, and the rules there given, may furnish a shorter way of solving this problem. See also the demonstration of these rules at the end of Mr. Simpson's treatise on "The Nature and Laws of Chance."

LIII. Of the Sea Pen, or Pennatula Phosphorea of Linnæus; also a Description of a New Species of Sea Pen, found on the Coast of South Carolina; with Observations on Sea Pens in General. By J. Ellis, Esq., F.R.S. p. 419.

This animal was well known to the ancients by the name of the sea pen; many of the old authors took it for a fucus or sea plant. This species has been found in the ocean from the coast of Norway to the most remote parts of the Mediterranean sea, and not only dragged up in trawls from great depths of the sea, but often found floating near the surface. Dr. Shaw, in his History of Algiers, remarks that they afford so great a light in the night to the fishermen, that they can plainly discover the fish swimming about in various depths of the sea. From this extraordinary property Linnæus calls this species of sea pen, pennatula phosphorea, and remarks, after giving the synonyms of other authors, habitat in oceano fundum illuminans.

The outward appearance of this animal is not unlike one of the quill feathers of a bird's wing, but they are found of different sizes, from 4 to 8 inches in length; the lower half of it is naked, round, and white, not unlike the quill part of a writing pen; the upper part represents that of the feathered part of the pen, and is of a reddish colour. This upper half, which arises from the quill and is feathered on both sides, is a little compressed, and becomes smaller and smaller till it ends in a point at the top; along the back of this, in the same manner as in the inner side of a common writing pen, there is a groove in the middle, from the quill to the extremity; from each side of this upper part of the stem proceed little parallel featherlike fins; these begin at the top of the quill part, very small on each side at first, but lengthen as they advance towards the middle; hence they shorten gradually on each side, till they end in a point at the top, their terminations preserving on each side the figure of the segment of a circle.

To come now to consider more minutely those pinnulæ, or feather-like fins, that project on each side and form the upper part of this animal. These are evidently designed by nature to move the animal backward or forward in the sea, consequently to do the office of fins, while at the same time, by the appearance of the suckers or mouths furnished with filaments or claws, they were certainly intended to provide food for its support; for notwithstanding what Linnæus has said in regard to its mouth, in his system of nature, viz. Os baseos commune rotundum, Mr. E. could not, with the help of the best glasses, discover that the point of the base was penetrated in the least, so that he is clearly of opinion, that this animal, like the hydra arctica or Greenland polype, described in his Essay on Corallines, nourishes and supports itself by these suckers or polype-like filaments; that by these both kinds take in their food, and have no other visible means of discharging the exuviæ of the animals they feed on, than by the same way which they take them in; and that, from attentively considering the structure and manner of living of both these animals, he classes them in the same genus of pennatula, though they vary very much in their exterior form and size, and consequently are of very different species. The stem of the suckers of this animal is of a cylindrical form; from the upper part proceed 8 fine white filaments or claws, to catch their food: when they retreat on the alarm of danger, they draw themselves into their cases, which are formed like the denticles of the corallines, but here each denticle is furnished with spiculæ, which close together round the entrance of the denticle, and protect this tender part from external injuries.

Some of the most curious remarks of Dr. Bohadsch on the anatomy of this animal, as also on the appearance of it while alive in sea water, are as follow: "When the trunk is opened lengthwise, a saltish liquor flows out of it, so viscid as to hang down an inch; the whole trunk of the stem is hollow; its outward coriaceous membrane is more than a line thick, and forms a strong covering to it: between this and another thinner membrane of the pinnated part of the trunk, are innumerable little yellowish eggs, floating in a whitish liquor, about the size of a white poppy seed; these are best seen when the trunk is cut across; this thin membrane lines the whole inside of the trunk, in which we observe nothing but a kind of yellowish bone, which takes up 3 parts of the cavity. This bone, in some of these animals, is above 2½ inches long, and about half a line thick; in the middle part of it, it is quadrangular; towards each end it grows round and very taper: that end is smallest which is nearest the top of the pinnated trunk. The whole bone is covered with a yellowish clear skin, which at each end changes into a ligament; one of which is inserted in the top of the pinnated trunk, the other in the top of the naked trunk; by the help of this upper ligament, the end of this little bone is either contracted into a very narrow arch, or disposed into a straight line, according to the motion of the trunk. The fins likewise are composed of 2 skins: the outer one strong and leathery, and covered over with an infinite number of crimson streaks, the inner skin is thin and clear: the cylindrical part of the suckers are in the same manner, only with this difference, their outer skins may be softer. Both the fins and suckers are hollow, so that the cavity of the suckers may communicate with the fins, as their cavity does with the trunk.

"We now come to the appearance which this animal makes when alive in sea water. The trunk then was contracted circularly at the bottom of the naked part of the stem, and by this contraction formed a zone of the most intense purple, which moved upwards and downwards successively: when it moved upwards through the length of the pinnated trunk, it there became paler, and at length terminated at the top: the motion being scarcely finished, a like zone appeared at the end of the naked trunk, which finished its motion in the same manner as the former. When this zone becomes very much constricted on every side, the trunk above it swells and acquires the form of an onion; and then it appears as if a compressed globe moved along through the whole space of the trunk; this constriction of the trunk gives that fine red colour to the zone; for when the skin of the trunk is outwardly full of purple papillæ, the intermediate spaces are of a whitish colour. In this constriction then of the skin, the intermediate spaces are obliterated, and the papillæ are brought nearer together; consequently only the purple colour presents itself to the eyes and appears more bright.

"The end or apex of the naked trunk is sometimes curved like a hook, and sometimes extended in a right line; both these motions then must be directed by the little bone in the inside, and from this motion of this little internal bone, that sinus or cavity at the lower end of the trunk (thought by authors heretofore to be the mouth) seems plainly to be formed; for sometimes it is deeper, sometimes shallower; it is deeper while the moveable globe appears in the middle of the pinnated part of the trunk, and shallower when it is in the bottom of the naked trunk, at which time the bone is most extended. The fins or pinnulæ have 4 different motions; they are moved both towards the naked stem, and towards the pinnated stem; sometimes they are drawn in very much to the belly, a little after they are inclined to the back; further, the fleshy filaments or claws move in all directions, and the cylindrical part with the filaments is either extended out or drawn in and hid in the fins."

In the following 3 chapters Dr. Bohadsch describes 3 other kinds of sea pens. One he calls penna grisea or the grey sea pen with crenated fins; this is figured and described from a dry specimen in Seba's Museum, tom. 3. The next is a very singular one, without fins, having a square bony stem 2 feet 10 inches long, covered with a skin, and furnished on 3 sides with tentacula or suckers: he says, the fishermen call it penna del pesche de pavone, or the feather of the peacock fish. To these he has added the alcyonium, called Manus marina; he calls it Penna ramosa pinnis carens, tentaculis in ramis positis; and in another place, Penna exos. The figures of these pennatula, or sea pens, may be seen in the works of Bohadsch, Linnæus, and elsewhere.

Mr. E. concludes with a short account of a new discovered species of pennatula, which his friend John Greg, Esq. of Charles Town in South Carolina, discovered on that coast, and presented to him. This beautiful purple animal is of a compressed kidney shape. The body is about an inch long, and half an inch across the narrowest part, it has a small roundish tail of an inch long, proceeding from the middle of the body: its tail is full of rings, from one end to the other, like an earth-worm; and, along the middle of the upper and under part of it, there is a small groove which runs from one end to the other. He examined carefully the point of the tail, but could find no perforation in it; which is agreeable to what he has observed in the rest of this genus. The upper part of the body is convex and near a quarter of an inch thick; the whole surface of it is covered over with minute yellow starry openings, through which are protruded little suckers like polypes, each furnished with 6 tentacles or filaments, like what we observe on some of the corals, and which seem to be the proper mouths of the animal. The under part of the body is quite flat: this surface is full of the ramifications of fleshy fibres, which, proceeding from the insertion of the tail, as their common centre, branch out so as to communicate with the starry openings on the exterior edge and upper surface of this uncommon animal.

Fig. 5, pl. 1, is the kidney-shaped purple sea-pen from South-Carolina, in its natural size. This upper part is full of starry openings, which send out small suckers, like polypes, by which it feeds. Fig. 6 is the under part of the same, with its ramifying fibres, that lead from the insertion of the stem, as from a centre to the circumference, and correspond with all the starry openings on the edge and back of it. Fig. 7 is a part of the exterior edge highly magnified, to show the form of the starry openings and suckers, which consist of 6 rays and claws.

LIV. A Letter from Mr. B. Wilson, F. R. S. to Mr. Æpinus. p. 436.

This is a tedious and uninteresting dispute between Mr. Æpinus and Mr. Wilson, concerning the nature of plus and minus electricity; also the permeability of glass to that fluid; and on the electricity of the tourmalin.

LV. On the Parallax of the Sun. By the Rev. Thomas Hornsby, M. A., Savilian Professor of Astronomy, Oxford, and F. R. S. p. 467.

The quantity of the sun's parallax is of such importance, both to the theory and practical part of astronomy, that many methods of determining it have been employed by the astronomers of every age. Mr. Flamsteed informs us, in the 92d and 96th numbers of the Philos. Trans., that from some observations made on the planet Mars, he had found the sun's parallax not to exceed 10 seconds; and Dr. Halley, in a memoir written expressly with a view to ascertain the exact quantity of it, supposes it not to be greater than 12¼″. When we consider the imperfect state of astronomy at the time when Mr. Horrox lived, we cannot sufficiently admire the wonderful genius of that young gentleman, who at the age of 24 could collect, from his own observations, that the parallax of the sun did not exceed 14 seconds; while many celebrated astronomers, whose tables were then in the greatest repute, had assigned a parallax of more than 2 minutes to the sun, which Kepler had supposed could not be less than 59 seconds, and which Hevelius, who published the admirable treatise of Mr. Horrox, entitled, Venus in Sole visa, fixed at 41 seconds.

In the year 1719, Dr. Pound and his nephew, that illustrious astronomer, Mr. Bradley, did, when Mars was in opposition to the sun, demonstrate (to use the words of Dr. Halley, Phil. Trans., N° 366,) the extreme minuteness of the sun's parallax, and that it was not more than 12″, nor less than 9″, on many repeated trials. At the same time, and by the same kind of observations, Maraldi determined this parallax to be 10″, the result of his observations agreeing exactly with those deduced from the correspondent observations by Richer at Cayenne, and by Cassini at Paris, in the year 1672.

The voyage which the Abbé de la Caille undertook, to perfect a catalogue of some of the principal fixed stars, furnished the astronomers with the means of determining the sun's parallax by corresponding altitudes of the planets Mars and Venus, to be observed on each side of the equator, with all the accuracy of which that method is capable. The astronomers here in Europe were invited to determine the distances of the planets from particular stars on stated days, while the Abbè himself proposed to make the corresponding observations on the southernmost part of Africa at the Cape of Good Hope. By the differences of the altitudes of the northern limb of Mars, and of such stars as were nearly in the same parallel, observed on the same day at the Cape with a sextant of 6 f. radius; at Greenwich by Dr. Bradley with a mural quadrant of 8 f.; at Bologna in Italy by M. Zanotti with a similar instrument of 5 f.; at the Royal Observatory at Paris by Messieurs Cassini de Thury and Gentil, with a moveable quadrant of 6 f.; and in Sweden by Messieurs Wargentin, Strommer and Schemmark, with telescopes of 7 and 8 f. armed with micrometers; it was found, when every reduction is made, that according to each observation, the dates of which are given below, the horizontal parallax of the sun, when at its mean distance from the earth, was as is represented in the following table.

Greenwich.		Bologna.		Paris.		Stockholm.		Upsal.		Hernosand.	
1751	″	1751	″	1751	″	1751	″	1751	″	1751	″
Aug.... 30	9.677	Aug.31	9.753	Sept.13	9.134	Sept. 1	10.466	Sept. 2	9.438	Sept.25	9.933
Sept.... 13	9.324	Sept. 1	9.895	14	9.715	25	10.504	+24	12.255	27	10.018
14	9.096	13	9.971	+24	11.912	Oct+3	12.864	25	9.715		
Oct..... 3	10.161	14	10.238	Oct. 8	9.895	5	10.085	Oct. 6	9.13		
4	10.504	Oct+7	11.075			6	9.735				
7	9.515										
+9	10.961										
Mean of all	9.891		10.186		10.164		10.734		10.135		10.275
Mean rej. +	9.712		9.964		9.581		10.262		9.421		

By taking a mean of all these observations, it follows that the sun's mean horizontal parallax is 10″.2; and if we reject the observations which differ most in excess from the rest, the mean will give 9.842 for the sun's mean horizontal parallax. Besides these 27 determinations, the Abbè de la Caille compared 41 observations, the mean of which is given in the following table.

N° of Obs.	Observations.	Instruments.	Places.	⊙ Par.
7	The late M. Cassini and M. Maraldi........	Quadrant 2 f. rad.	Thury	8″.982
6	Mr. Delisle—at the Hotel de Clugny........	Mur. circle 2 f.	Paris †	11.532
3	Father Beraud..............................	Refr. tel. 7 f.	Lyons	9.020
6	M. M. Garipuy and d'Arquier	Ditto	Toulouse	8.944
12	M. Sabatelli and Father Carcani...........	Quad. 4 f. diag. div.	Naples	9.933
7	M. Bose	Tel. of 6 and 8 f.	Wittemberg	10.999
	Mean of all observ. according to A. Caille			10.210
	Mean of Results (rejecting the 2d)			9.575

Few observations of Venus near the inferior conjunction with the sun on Oct. 31, 1751, were made, on account of the unfavourable weather here in Europe. By an observation made at Greenwich, on Oct. 25, the mean horizontal parallax was 9″.8; but according to the observation made at Paris on the same day at the Royal Observatory, that parallax was 11″.4. On Oct. 27, by an observation made at Paris, the sun's mean horizontal parallax was 9″.85; but by an observation at Bologna on the same day it was found to be 10″.4. By the observation at Paris on Nov. 17, the sun's mean parallax was 10″.5. By a mean of all the observations of Venus, the sun's mean parallax is 10″.38; and if we reject the Paris observation on Oct. 25, that parallax is 10″.13.* We see then that, according to these observations, the sun's mean horizontal parallax is not less than 8″.94. If we take a mean of the whole, that quantity is 10.09: but if we reject the observations that differ most in excess, the sun's mean horizontal parallax will be found to be 9″.92; a determination in which every astronomer

* See the Abbè de la Caille's Introduction to his Ephemerides Celestes from 1765 to 1774.

might readily acquiesce, when he considers the accuracy of the observers, and the nice agreement of almost all the observations.

And such was the state of the sun's parallax as deduced from the latest and best observations, when the approaching transit of Venus in 1761 engaged the attention of the curious of all nations. Dr. Halley, in Philos. Trans., N° 348, had proposed a method of determining the sun's parallax, by procuring observations to be made on this transit in such places where the difference of time between the ingress and egress would be the greatest possible; namely near the mouth of the Ganges, where the sun would be vertical at the middle of the transit, and at Port Nelson in Hudson's Bay, where the planet would enter on the sun's disk about the time of sun-set, and leave it soon after sun-rising; for in the former place, says Dr. Halley, the planet would be equally distant from noon both at ingress and egress, and the apparent motion of Venus on the sun would be accelerated by almost double the quantity of the horizontal parallax of Venus from the sun: because Venus is at that time retrograde, and moves in a direction contrary to that of the eye of an observer on the earth's surface. Whereas in Hudson's Bay, under an opposite meridian, the eye of an observer will be carried, while the sun seems to move under the pole from setting to rising, in a direction contrary to the motion of the observer's eye at the Ganges; that is, in the direction of the planet's retrograde motion from east to west.—From these considerations, and supposing with Dr. Halley the axis of the planet's path to be inclined to the axis of the equator in an angle of 2° 18′ only, the interval between the two contacts would have been $15^m\ 10^s$ longer in Hudson's Bay than at the mouth of the Ganges.

But on examination, the case is found to be somewhat different. The axis of the equator on the 6th of June 1761 made an angle of 6° 10′ with the axis of the ecliptic on one side, and the axis of the planet's path an angle of 8° 30′ 10″ on the other; the axis of the planet's path therefore made an angle with the equator of 14° 40′ 10″.—The planet's latitude was $5\frac{1}{4}$ minutes greater, both from observation and the Doctor's own tables, than he had supposed, in his calculation made from the Rodolphine tables corrected: and therefore the planet's egress could not have been observed at Port Nelson. Having made a computation for a place in North America situated $5^h\ 30^m$ to the west of Greenwich, and in the 60th degree of latitude; and also for a place to the east of the Ganges, and $6^h\ 30^m$ to the east of Greenwich, in the latitude of 22° 42′ north, that the places might be nearly situated in the same circumstances with the mouth of the Ganges and Port Nelson, it appears that the interval between the two contacts would be but $4^m\ 56^s$ longer in America than in the East Indies, supposing the sun's parallax 12″.5, and the inclination of Venus's path 14° 40′ to the equator.

And here perhaps it may not be altogether unnecessary to inquire, how far the mistake which Dr. Halley committed, by using the difference of the two angles instead of their sum, would influence the times of the transit as seen at the Ganges and Port Nelson. For this purpose Mr. H. made use of the same elements which Dr. Halley has given in his paper, and calculated the angle of the vertical with the orbit of Venus at the two internal contacts at both places, supposing the orbit to be inclined first only 2° 18′ to the equator, agreeably to Dr. Halley's supposition, and also 14° 40′, and he found that the duration would be 15^m 13^s longer at Hudson's Bay than at the Ganges, on the first supposition; and 14^m 44^s, if the circles be duly inclined to each other; the difference being only 29 seconds. It has already been found by calculation, supposing the latitude of Venus to be about 9¼ minutes, that the difference of duration at the two places would have been only 4^m 56^s. It may fairly therefore be concluded, that the transposition of the circles contributed very little towards giving so different a result, the reason of which need not here be mentioned; and Dr. Halley seems to have been led into the mistake entirely from supposing the latitude of Venus to be about 4′ 0″, according to the tables which he then used, constructed on the principle that the nodes of that planet were fixed.—Having determined that the difference of duration at the two places above mentioned would be 15^m 10^s (differing only 3^s from the method Mr. H. used, which is independent of projection) the doctor proceeds to show, that if Venus had no latitude at the time of the middle of the transit, the difference would be 18^m 40^s; and if the planet should pass 4′ 0″ to the north of the sun's centre, that difference would be 21^m 40^s; and would become still greater, if the planet's north latitude should be further increased. And such would have been the event had the motion of the nodes been progressive. But, agreeably to the principles of universal attraction, their motion is really retrograde, and this Dr. Halley says he himself suspected. And therefore it is somewhat surprising that he did not determine by calculation, what would have been the difference in the whole duration between the two places, if Venus should pass more to the southward of the sun's centre, than he had supposed. He would then immediately have perceived that the two stations were not so advantageously placed as the solution of the problem required.

Observers were therefore to be sent to other places, in order to determine the sun's parallax agreeably to the method proposed by Dr. Halley. The city of Tobolski in Siberia is so situated, that the interval between the two contacts was perhaps as short as could possibly be observed on any part of the earth's surface: to this place was sent the Abbé Chappe d'Auteroches, one of the French astronomers. Near Hudson's Bay, and in 60° of latitude, the duration would have been 5 minutes longer, supposing the sun's parallax = 9″. At

Bencoolen, where it was first proposed to send Messrs. Mason and Dixon, the difference would have been about 4¼ minutes. At the island of Rodrigues, where Mr. Pingré could only observe the last internal contact, the difference would have been about 7½ minutes. On the southern coast of New Holland, it would have been somewhat more than 10 minutes. And in the great Indian Ocean, under 115¼° of absolute longitude from the Isle of Ferro, and in 57° of south latitude, where the beginning of the transit would happen soon after sun-rising, and the end just before sun-set, the difference would amount to 13¼ minutes. The greatest difference between the interval of the two internal contacts, as determined by actual observation on the 6th of June, was $2^m\ 49^s.75$, a quantity hardly sufficient to determine the sun's parallax agreeably to the method proposed by Dr. Halley.

Mr. H. has however made the necessary calculations, and compared the duration of the transit observed at several places with the duration as observed at Tobolski. The parallax resulting from each observation is contained in the following table, in which the 3d column contains the observed duration, the 4th the difference of each observed duration; the next contains that difference as deduced by computation on a supposition that the sun's parallax is 9″. In the last column is given the horizontal parallax on the day of the transit, resulting from a comparison of the 4th and 5th columns.

Places.	Observers.	Observed duration.	Diff. of observed dura.	Diff. by calculation.	Sun's parallax.
Tobolski	Abbe Chappe	$5^h\ 48^m\ 53^s.25$			
Cajaneburg	Mr. Planmann	5 49 54	$1^m\ 00^s.75$	$1^m\ 00^s.88$	8″.980
Tornea°	Hellant	5 50 09	1 15.75	1 05.27	10.444
Tornea°	Lagerborn	5 50 21	1 27.75	1 05.27	†12.098
Upsal	Bergman	5 50 26	1 32.75	1 33.78	8.901
Upsal	Mallet	5 50 07	1 13.75	1 33.78	† 7.077
Upsal	Stromer	5 50 02	1 08.75	1 33.78	† 6.597
Hernosand	Gister	5 50 26	1 32.75	1 25.76	9.733
Abo	Justander	5 50 09	1 15.75	1 20.68	8.450
Stockholm	Wargentin	5 50 45	1 51.75	1 34.21	10.675
Stockholm	Klingenstiern	5 50 42	1 48.75	1 34.21	10.389
Calcutta	Magee	5 50 31	1 37.75	1 37.02	9.067
Madras	Hirst	5 51 43	2 49.75	2 39 50	9.577

Mean of the whole.. 9.332
Mean, rejecting 2 observations at Upsal and 1 at Tornea................ 9.579

The duration at Cajaneburg was the shortest, except at Tobolski; with which if we compare the duration observed at Madras, the parallax is 9″.948: and by taking a mean of the parallax deduced from a comparison of the observation at Madras with those of Tobolski and Cajaneburg, the parallax is 9″.762. The observations at the above places agree as well together as can be expected from such small differences in the duration, which must in some measure be influenced by the necessary and unavoidable errors in observation. If the quantity of the sun's diameter, and the least distance of the centres were very exactly known, the sun's parallax might safely be determined, by comparing the duration of the transit, as observed at different places, with the duration as supposed to be seen from the earth's centre. According to this method, supposing the least distance of the centres to be $9' 29\frac{1}{2}''$, which is a mean between the Greenwich, Shirburn, and Paris observations, and the difference of the semidiameters of the sun and Venus = $916''.4$, the duration as observed at Tobolski was more than 10 minutes shorter, than if seen without parallax; at Tornea°, at Stockholm, at Cajaneburg, at Astracan, and indeed in almost every part of Europe and Asia, the duration was considerably shortened; and if a number of good observations made in several of those parts were procured, the quantity of the sun's parallax might be well enough ascertained, as the difference in duration, for a difference of one second in the sun's parallax, will be found very considerable. Though this method should not be practised, unless the necessary requisites for the computation be known with some degree of precision, Mr. H. has ventured to compare the durations observed chiefly in the northern parts of Europe, and some in Asia, with the duration as seen from the earth's centre, = $5^h\ 59^m\ 19^s.10$ mean time, or $5^h\ 59^m\ 16^s.64$ apparent time, and calculated from the elements above mentioned.

Places.	Observed durations.	Duration without parallax.	Differ. of duration.	Diff. for 1″ of parallax.	☉'s parallax.
Tobolski	$5^h\ 48^m\ 53^s.25$	$5^h\ 59^m\ 16^s.64$	$10^m\ 23^s.39$	$64^s.09$	$9''.726$
Cajaneburg	5 49 54		9 22.64	57.32	9.815
Tornea°	5 50 09		9 17.64	56.83	9.636
Tornea°	5 50 21		8 55.64	56.83	9.425
Abo	5 50 09		9 07.64	55.12	9.935
Upsal	5 50 26		8 50.64	53.67	9.888
Upsal	5 50 07		9 09.64	53.67	10.241
Upsal	5 50 02		9 15.64	53.67	10.352
Hernosand	5 50 26		8 50.64	53.65	9.890
Stockholm	5 50 42		8 34.64	53 62	9.579
Stockholm	5 50 45		8 31.64	53.62	9.523
Calcutta	5 50 36		8 40.64	53.31	9.766
Madras	5 51 43		7 33.64	46.36	9.785

The 4th column contains the difference between the observed and calculated duration; in the 5th is given the difference in the duration for a difference of 1″ in the sun's parallax, and the 6th column is obtained by dividing the 4th by the 5th. The mean of all the results is $9''.812$: and if we reject two of the observations at Upsal, which differ most in excess, the sun's parallax is $9''.724$, agreeing very nearly with the quantity resulting from a comparison of some of the observed durations with the shortest observed at Tobolski and Cajaneburg.

We may also proceed to find the sun's parallax by means of the least distance of the centres, as observed in two or more places where the effect of parallax was contrary; or if the least distance of the centres was only determined at one place, it may be found by calculation at any other place, where the total duration was observed. But in this and the last case the elements of calculation are required with so rigorous an exactness, that perhaps these methods are only to be called in to illustrate and confirm the others.

Mr. Pingré confined himself principally to the determination of the least distance of the centres. At $21^h\ 43^m\ 11^s$ he found the distance between the nearest limbs of Venus and the sun to be the greatest $= 5'\ 57''.2$, or $5'\ 57''.4$ when corrected by refraction. This distance being subtracted from $15'\ 19''.5$, the difference of the semidiameters, leaves $9'\ 22''.1$ for the least apparent distance of the centres. But as that observation was made rather too late, when the distance of the centres was greater than it ought to be, he found by calculation that it should be diminished by $0''.22$. The true apparent least distance of the centres by actual observation was therefore $9'\ 21''.88$. In order to be more secure of this result, Mr. Pingré compared a large number of observed distances, both at the beginning and towards the middle of the transit, with the distance determined by internal contact, and after excluding every doubtful observation, he found the least apparent distance of the centres to be $9'\ 21''.69$. By comparing this distance with the distance deduced from the total duration, as observed at any place (the method of finding which he has given at large in his memoir inserted in the Memoirs of the Academy of Sciences for 1761) and by knowing from calculation what influence a parallax of $10''$ for instance would have on those distances, he found the sun's parallax as in the following table:

Places.	Observed durations.	L. distance of centres from the durations.	L. dist. deduced from calculation.	Sun's parallax.
Tobolski	$5^h\ 48^m\ 53^s.25$	$9'\ 51''.53$	$9'\ 52''.24$	$10''.125$
Stockholm	5 50 43.5	9 54.85	9 55.83	10.03
Upsal	5 50 26	9 55.62	9 55.95	10.23
Cajaneburg	5 49 54	9 55.61	9 55.61	10.00
Tornea°	5 50 09	9 55.28	9 56.08	10.09

By taking a mean of these determinations, we find the sun's parallax to be $10''.1$. In the above calculations the sun's semidiameter was supposed $= 15'\ 48''.5$, and that of Venus $29''$. Observers, says Mr. Pingré, have found the former to be about $2''$ less, and the latter on the contrary half a second larger. By calculating on the supposition of a difference of $2''$ in the difference of the semidiameters of the sun and Venus, the least distance of the centres at Tobolski, Stockholm, Upsal, Tornea°, and Cajaneburg, ought to be $3''.12$ less, and at Rodrigues $2''.56$ or $2''.60$, and the sun's horizontal parallax ought also to be 0″.17 less. If then this correction be admitted, which is warranted by the best observations, the sun's horizontal parallax will be 9″.92.

There is still another method by which we are enabled to determine the sun's parallax, by comparing the observations made in different places, where the effect of parallax on the planet is considerable at the times of the two contacts. It was more convenient to make use of the 2d internal contact for this purpose, and the observers were very advantageously stationed at St. Helena and the Cape of Good Hope: for by comparing the observations made there, with those at Tornea°, Tobolski, and in some of the eastern parts of Asia, the difference of the times of the contacts, when reduced to the same meridian, will be found to be very considerable, amounting to more than 9½ minutes at the first two places above mentioned, and being greater, as the places are farther situated to the north-east. But if this method be used, it is absolutely necessary that the longitudes of the places should be determined with the utmost accuracy, since an error of a few seconds would have a considerable influence on the result, and would increase or diminish the quantity of the sun's parallax, in proportion. The unfavourable state of the heavens at the time of the internal contact prevented the Rev. Mr. Maskelyne from making an observation at the Isle of St. Helena; which is the more to be lamented, as his observation would have confirmed or corrected the observation at the Cape if necessary; since the effect of parallax at both places would have been very nearly the same. The observers at the Cape were more fortunate, and differed only 4ˢ in their observation of the internal contact. But before we proceed to deduce the quantity of the sun's parallax, by comparing as well the observation made at Greenwich as those at other places, with the observation at the Cape, it will be necessary to lay before the reader the authorities on which the longitude of each place has been determined.

The longitude of the Cape of Good Hope was not even nearly known till the Abbé de la Caille went thither in the year 1751. By a comparison of 9 eclipses of Jupiter's satellites, as well immersions as emersions, observed at the Cape, with the corresponding observations made at Paris, the Cape was found, by the Abbé de la Caille himself, to be $1^h\ 4^m\ 14^s$ to the east of Paris, or $1^h\ 13^m\ 31^s$ to the east of Greenwich. Messrs. Mason and Dixon observed many eclipses of Jupiter's satellites at the Cape, but the weather was not so favourable here in England. However, by comparing 4 observations made in Surry-street, and one at Greenwich, with those made at the Cape, the difference of longitude at a mean is found to be $1^h\ 13^m\ 28^s$, which Mr. H. has used in the following computations.

The internal contact, as reduced from sidereal to apparent time by Mr. Mason, happened at $21^h\ 39^m\ 52^s$. But on examination it will be found to have happened later; for whether we make use of the sun's mean R. ascension from the best solar tables extant, or the sun's apparent R. ascension reduced to the meridian of the place, as determined by actual observation on the day of the transit, the true apparent time of the contact will be found to have happened at $21^h\ 39'\ 54''$ — or at $21^h\ 39^m\ 54''\frac{1}{2}$ if the time by the star Antares be used, whose situation was more favourable to an observer in 34° of south latitude. Mr. H. therefore supposes the internal contact to have happened at $21^h\ 39^m\ 52^s$, by taking a mean of the two observations.*

The Royal Observatory at Paris was supposed by Sir Isaac Newton, in his Principia, to be $9^m\ 20^s$ to the east of Greenwich. And the editor of Dr. Halley's tables has followed that determination, which has also been generally used by the English astronomers. The French astronomers have till very lately imagined the difference of meridian to be $9^m\ 10^s$, as deduced from a single observation of an eclipse of Jupiter's first satellite made by M. Cassini when in London, with a telescope of similar size and construction with that used at Paris when the same eclipse was observed. In the year 1734 M. Maraldi published a comparison of 33 eclipses observed at Greenwich by Mr. Flamsteed, and at Paris by the French astronomers, 19 of which are immersions, and the rest emersions. The longitudes resulting from each correspondent observation differ widely from each other, the two observatories being $11^m\ 27^s$ distant by an immersion of the 2d satellite, and only $7^m\ 43^s$ by an emersion of the first. But if we take a mean of the whole, the difference of longitude will be $9^m\ 24^s$; and if we exclude the observation of the 2d satellite above mentioned, which must be very faulty, the difference of meridians will be $9^m\ 22^s$, a result which in all probability is but a very few seconds from the truth. It may be observed that the immersions all give the difference of longitude too great, and almost all the emersions too little; a circumstance owing either to the badness of the air here in England, or to an inequality in the goodness of the telescopes, or perhaps to both; for whatever was the advantage in observing the immersions, was balanced by the emersions: for which reason, whenever the eclipses of Jupiter's satellites are used, the longitude should, if possible, be deduced both from immersions and emersions.

As the observations of transits of Mercury may be very useful in settling the longitudes of places which are not far distant, Mr. H. examined the several observations made at Paris, and either immediately at Greenwich, or in such parts of London whose longitude from Greenwich is known within one second of time. And the result of such comparisons is as follows. On the 29th of October 1723, Dr. Halley observed the first interior contact of the limbs of Mercury and the

* Mr. Mason, before he left England, acknowledged, in a letter to Mr. H., that he had committed a mistake in his calculations, by forgetting to apply to the sun's place the equation of precession, which on the day of the transit amounted to — 15″.6.—Orig.

sun at $2^h\ 42^m\ 26^s$ apparent time at Greenwich. Mr. Bradley observed the same at $2^h\ 42^m\ 38^s$, at Wansted in Essex (10^s to the east of Greenwich) or at $2^h\ 42^m\ 28^s$ when reduced to the meridian of Greenwich. Mr. Graham in Fleet-street observed the same at $2^h\ 42^m\ 19^s$, or at $2^h\ 42^m\ 44^s$, when reduced to Greenwich. The mean of these is $2^h\ 42^m\ 32^s.7$. In the observatory at Paris M. Maraldi observed the same at $2^h\ 51^m\ 48^s$ apparent time; and M. Delisle at $2^h\ 51^m\ 37^s$, but suspects it might have been some few seconds later. Mr. H. supposes it to have happened at $2^h\ 51^m\ 43^s.5$. The difference of meridians therefore is $9^m\ 10^s.8$. If we take a mean of Dr. Halley's and Mr. Bradley's observations only, the difference of meridians is $9^m\ 16^s.5$.

In 1736, Dr. Bevis observed the last contacts of the limbs of Mercury and the sun at $0^h\ 8^m\ 33^s$ at Greenwich. The same was observed at Paris by M. Maraldi and M. Cassini de Thury, and at Thury by M. Cassini, at $0^h\ 18^m\ 5^s.5$ by a mean of the 3 observations. The difference of longitude therefore is $9^m\ 32^s.5$. In 1743, the last internal contact of the limbs was observed by Mr. Graham, in Fleet-street, at $1^h\ 0^m\ 42^s$, and by Dr. Bevis at Beaufort-Buildings, in the Strand, at $1^h\ 0^m\ 33^s$; or by a mean of both, when reduced to the meridian of Greenwich, at $1^h\ 1^m\ 04^s$.—The same was observed by the Abbe de la Caille, Mess. Maraldi, Monnier, and Cassini the son, at Paris, and by M. Cassini at Thury; which observations when reduced to the meridian of the Royal Observatory, give $1^h 10^m\ 15^s.5$ for the time of the internal contact; the difference of meridians is therefore $9^m\ 12^s.5$.——By a mean of the observations of Mr. Graham and Dr. Bevis, when reduced to Greenwich, the last external contact on the same day happened at $1^h\ 2^m\ 42^s$; and by a mean of the observations in France, the same happened there at $1^h\ 12^m\ 10^s$. The difference of longitude therefore is $9^m\ 28^s$.

In 1753 was another transit of Mercury, when the unfavourable state of the heavens, a few seconds before the time of the internal contact, prevented any observations from being made at Greenwich, as appears from a paper communicated to Mr. H. by the executors of the late Dr. Bradley. Both contacts however were luckily very well observed by Mr. Short, Dr. Bevis, and Mr. Bird; by a mean of whose observations, reduced to the meridian of Greenwich, the internal contact happened at $10^h\ 9^m\ 37^s.5$. The same contact was observed by 13 observers at Paris, and was found not to happen sooner than $10^h\ 18^m\ 36^s$, nor later than $10^h\ 19^m\ 3^s$. But by a mean of all, at $10^h\ 18^m\ 45^s$. The difference of meridians therefore is $9^m\ 7^s.5$. By a mean of the observations of Mr. Short, Dr. Bevis, Mr. Bird, Mr. Canton, and Mr. Sisson, all reduced to the meridians of Greenwich, the external contact happened at $10^h\ 12^m\ 17^s.5$; and at the Royal Observatory, by a mean of all the observations at Paris, at $10^h\ 21^m\ 33^s$. The difference of longitude therefore is $9^m\ 15^s.5$. And if we take a mean of these 7 results, the Royal observatory at Paris will be found to be $9^m\ 17^s\frac{1}{4}$ to the east of the Royal observatory at Greenwich; a determination very nearly agreeing with that mentioned by Sir Isaac Newton, and which Mr. H. believes was deduced from a comparison of Dr. Halley's and Mr. Cassini's observations.

The Abbe de la Caille, in his memoir on the parallax of the moon, supposes the difference of meridians to be $9^m\ 17^s$, though he has not mentioned from what authority he drew that conclusion. Mr. H. therefore supposes the difference of meridians to be $9^m\ 17^s$.—The last internal contact was observed at Paris by M. de la Lande at $20^h\ 28^m\ 25^s$ or 26^s; at $20^h\ 28^m\ 26^s$ by Father Clouet, and by M. Maraldi and M. Barros separately at $20^h\ 28^m\ 42^s$. Mr. Pingre, in a very curious memoir on the sun's parallax already referred to, supposes the internal contact to have happened at Paris at $20^h\ 28^m\ 38^s$. Mr. H. therefore makes use of the Abbe de la Caille's observation at $20^h\ 28^m\ 37^s\frac{1}{2}$.

The difference of meridians between Paris and Stockholm, says Mr. Wargentin, is $1^h\ 2^m\ 51^s$ or 52^s at most. M. de la Lande, from a comparison of 17 observations of the first satellite of Jupiter, made from 1750 to 1759, and communicated to him by Mr. Wargentin, determines the difference of longitude to be $1^h\ 3^m\ 10^s$. And the Abbe de la Caille, in his memoir on the moon's parallax, supposes it to be $1^h\ 3^m\ 13^s$. As these last two determinations agree so nearly together, Mr. H. supposes Stockholm to be $1^h\ 3^m\ 10^s$ to the east of Paris, and $1^h\ 12^m\ 27^s$ to the east of Greenwich; and the last internal contact to have happened at $21^h\ 30^m\ 09^s.5$, which is a mean between the observations of Mess. Wargentin and Klingenstiern.

The city of Cajaneburg in Sweden is $38^m\ 40^s$ to the east of Stockholm, according to very late observations; and therefore Cajaneburg is $1^h\ 51^m\ 7^s$ to the east of Greenwich. The 2d internal contact happened at $22^h\ 7^m\ 59^s$, when the error in writing down the minutes is corrected according to the instruction given in Philos. Trans. for 1761. Indeed (supposing the longitude of Cajaneburg as above set down to be exact) it is very easy to prove that the error of one minute was made at the egress, rather than at the ingress.

The city of Tobolski in Siberia (according to the observation of the end of the solar eclipse on June 3d, by Mr. Chappe and Mr. Planmann at Cajaneburg, and calculated by Mr. Pingré) is $2^h\ 42^m\ 11^s$ to the east of Cajaneburg; and this determination is also confirmed by Mr. Wargentin's observation of the same phase. Tobolski therefore is $4^h\ 33^m\ 18^s$ to the east of Greenwich; and Mr. H. supposes Mr. Chappe to have observed the last internal contact at $0^h\ 49^m\ 23^s\frac{1}{2}$, without making any allowance for the luminous ring which appeared round Venus in his telescope.

The observatory at Upsal (according to Mr. Wargentin in the Philos. Trans.,) is $1^h\ 1^m\ 10^s$ to the east of Paris, and is therefore $1^h\ 10^m\ 27^s$ to the east of Greenwich. By taking a mean of the 3 observations made there, the internal contact happened at $21^h\ 28^m\ 6^s$.

Tornea° has been generally supposed to be $1^h\ 27^m\ 30^s$ to the east of Paris; but with this difference of meridians, the observations at Tornea°, though made by Mr. Hellant, a very excellent observer, will give a parallax of the sun much less than the other observations made in high northern latitudes. In order to settle the longitude of this place, Mr. H. is of opinion that we may have recourse with safety, and without incurring the charge of reasoning in a circle, to the observation of the transit itself; viz. the observation of the internal contact at the ingress. Whether we suppose the sun's parallax to be 8″ or 10″, the first internal contact would have happened sooner at Tornea° than at Stockholm 19^s or 24^s. As the sun's parallax will readily be allowed to be more than 8″, Mr. H. supposes the first internal contact to have happened 21^s sooner. Tornea° is therefore $24^m\ 55^s$ to the east of Stockholm, and consequently $1^h\ 37^m\ 22^s$ to the east of Greenwich. Mr. H. makes use of Mr. Hellant's observation of the internal contact, at $21^h\ 54^m\ 8^s$, in preference to that of Mr. Lagerbom.

Abo, the capital of Finland, where Mr. Justander observed the last internal contact, at $21^h\ 45^m\ 19^s$ (when a correction is made in the minutes) is $1^h\ 11^m\ 29^s$ to the east of Paris, and $1^h\ 28^m\ 34^s$ to the east of Greenwich. At Hernosand, which is $1^h\ 11^m\ 29^s$ to the east of Greenwich, Mr. H. supposes the 2d internal contact was observed at $21^h\ 28^m\ 52^s$, as published in the Philos. Trans. by Mr. Short, from the Swedish acts. Mr. H. finds the island of Rodrigues, by comparing 3 observations of eclipses of Jupiter's satellites, with others made in England and at the Cape, to be $4^h\ 12^m\ 38^s$ to the east of Greenwich; and this determination is exactly confirmed by Mr. Pingré's comparison of the same eclipses. The observation of the occultation of a fixed star gives the longitude 6^s or 7^s greater. In the Philos. Trans., and even in the former part of the volume of the Memoirs of the Academy of Sciences for 1761, we find the internal contact was observed at Rodrigues at $0^h\ 34^m\ 47^s$. And yet in the memoir on the sun's parallax it is said to have happened at $0^h\ 36^m\ 49^s$. On comparing this latter with the time by the clock, it should seem that Mr. Pingré had committed a mistake in subtracting the error of his clock instead of adding it. But he has no where mentioned any reason for this difference.

Gottingen, where the celebrated Mr. Mayer observed the first internal contact at $20^h\ 58^m\ 26^s$ to the east of Paris, is $30^m\ 16^s$ or $39^m\ 33^s$ to the east of Greenwich. The Abbé de la Caille has placed Bologna $36^m\ 3^s$ to the east of Paris. By comparing the observations of the transit of Mercury, Mr. H. finds by a mean of 3 results, agreeing very nearly together, that Bologna is $45^m\ 15^s$ to the east of Greenwich. Zanotti observed there the 2d internal contact at $21^h\ 4^m\ 34^s$. But as he used a refracting telescope of $2\frac{1}{2}$ feet, and as two other observers with telescopes of 10 and 22 feet saw the contact 24^s later, Mr. H. supposes it to have happened at 21^h 4^m 58^s. At Florence, the internal contact was observed with a reflector of more than 4 feet, at 21^h 4^m 28^s by Father Ximenes. The longitude of this place is 34^m 48^s to the east of Paris, according to the table in the Connoissance des Mouvemens Celestes, or 35^m 58^s according to the table in the Elemens d'Astronomie by M. Cassini. By taking a mean of both, Florence is 44^m 40^s to the east of Greenwich. The longitude of St. Peter's at Rome is 49^m 54^s according to the French astronomers. The internal contact was observed to happen at 21^h 9^m 36^s. But as it is not said where this observation was made, the longitude given above will be found to be somewhat inaccurate.

Observations were also made at Madrid and Lisbon: at the former, the internal contact happened at 20^h 6^m 56^s apparent time; and at Lisbon at 19^h 44^m 26^s. The longitude of Madrid as given in the Philos. Trans. is certainly erroneous; being more than a minute and a half too little, if the observation of the transit can be depended on. At Lisbon, the longitude of the place was not determined by M. Ciera, who observed the transit, when Mr. Pingré, from whom Mr. H. had taken the observation, left it in his way from Rodrigues. From the best accounts that he can collect, particularly from the 385th number of the Philos. Trans., and from an account of some observations by Mr. Short, Lisbon is about 36^m 26^s to the west of Greenwich.

Now in order to deduce the sun's parallax from the observations related above, Mr. H. proceeded in the following manner. Having subtracted the difference of longitude between Greenwich and the Cape, = 1^h 13^m 28^s, from 21^h 39^m 52^s the mean of the observed times at the Cape, and compared the remainder with the observed time at Greenwich, he finds that the internal contact was observed 7^m 24^s later at the Cape than at Greenwich on account of the parallax. He then calculated what would be the effect of parallax at each place, supposing the sun's parallax to be 9 seconds; and found that the time of the internal contact would be accelerated 1^m 16^s.63 at Greenwich, and retarded 6^m 31^s.9 at the Cape: the whole effect of parallax therefore is 7^{m}47^s.72. But the difference in time as found by observation, is only 7^m 24^s: and therefore the difference by calculation is to the difference by observation, as the assumed parallax is to the true parallax on the day of the transit, which by this observation is 8″.543. The parallax resulting from each observation will be found in the following table, which will be sufficiently explained by the foregoing example.

Places.	Diff. of calculated times.	Diff. of observed times.	Sun's Parallax.	Places.	Diff. of calculated times.	Diff. of observed times.	Sun's Parallax.
Greenwich......	7^m $47^s.72$..	7^m 24^s ..	$8''.543$	Abo	9^m $11^s.16$..	8^m 59^s ..	$8''.301$
Paris	7 28.40 ..	7 3.5..	8.49	Hernosand	9 21.17 ..	9 1 ..	8.676
Stockholm......	8 53.72 ..	8 41.5..	8.712	Rodrigues......	3 19.72 ..	2 13 ..	5.993
Upsal..........	9 1.83 ..	8 45 ..	8.727	Gottingen	7 54.36 ..	7 31 ..	8.558
Cajaneburg	9 42.30 ..	9 32 ..	8.841	Bologna........	7 3.31 ..	6 41 ..	8.525
Tobolski......	10 29. 6..	10 18.5..	8.848	Florence	6 57.79 ..	6 36 ..	8.536
Tornea°........	9 48.95 ..	9 38 ..	8.832	Rome........	6 45.16 ..	6 41 ..	8.907

Such is the result of a comparison of the best observations, made in places whose longitudes are as accurately ascertained as the present state of astronomy will permit; by a mean of the whole, rejecting only the observation at Rodrigues, the sun's parallax on the day of the transit is $8''.692$.—Mr. H. has excluded the comparison of the observation at Rodrigues, because the parallax resulting from it differs so considerably from the rest. If we suppose the internal contact to have really happened one minute sooner, through a mistake in writing down the observation, the parallax will then be $8''.697$.

This observation made at Rodrigues, supposing it exact, will furnish another term with which to compare the several observations made in Europe. The sun's parallax resulting from each observation may be seen in the following table.

ces.	Diff. of calculated times.	Diff. of observed times.	Sun's parallax	Diff. of observed times.	Sun's parallax.
Greenwich	$4^m 28^s.00$	5^m 11^s.	$10''.444$	$4^m 11^s$.	$8''.429$
Paris..........................	4 8.98	4 50.5	10.500	3 50.5	8.332
Stockholm	5 39.00	6 28.5	10.314	5 28.5	8.721
Upsal	5 42.11	6 32	10.312	5 32	8.734
Cajaneburg	6 22.58	7 19	10.327	6 19	8.915
Tobolski	7 9.34	8 5.5	10.177	7 5.5	8.919
Tornea°	6 29.23	7 25	10.289	6 25	8.902
Abo	5 51.46	6 47	10.422	5 47	8.886
Hernosand	6 1.45	6 49	10.183	5 49	8.690
Gottingen	4 34.64	5 18	10.421	4 18	8.454
Bologna	3 43.59	4 33	10.787	3 33	8.372
Florence	3 38. 7	4 23	10.854	3 23	8.449
Cape of Good Hope	3 10.72	2 13	5.993	3 13	8.697

The mean of the whole, rejecting the comparison of the Cape, is $10''.419$; supposing the internal contact to have happened at 0^h 36^m 49^s. But if a mistake of one minute was really committed, the 3d column will receive a considerable alteration, and the parallax resulting from each observation will be represented in the last column, the mean of which is $8''.654$, agreeing as nearly as possible with the parallax resulting from all the best observations compared with the Cape.

Mr. Pingré finding the parallax resulting from his own observation, to differ so widely from that deduced from the Cape, and that both observations might be made to agree by supposing an error of one minute in the observation at Rodrigues, has examined every source of error that might be committed; and on the whole sees reason to prefer his own observation to that of Mr. Mason, not because he could find no mistake in his own, but because he has proved that no mistake could possibly be committed. His observation indeed is in some measure confirmed by comparing all the observations with that at Lisbon; from which comparison, if the longitude above laid down may be depended on, the sun's parallax is somewhat more than 10 seconds. The several observations that have been compared with the observations both of the Cape and Rodrigues, may also be compared together; and by combining some of them we may obtain different results, on which we may more or less depend, as the differences between the observed times are greater or less.

Places compared.	Diff. of calculated times.	Diff of observed times.	Sun's parallax.	Places compared.	Diff. of calculated times.	Diff. of observed times.	Sun's parallax.
Tobolski and Greenwich	2m41s.34	2m54s.5	9″.734	Cajaneburg & Gottingen	1m47s.94	2m 1s.	10″.088
Tobolski and Paris....	3 0.36	3 15	9.736	Cajaneburg and Florence	2 44.51	2 56	9.628
Tobolski and Gottingen	2 34.70	2 47.5	9.744	Cajaneburg and Bologna	2 38.99	2 51	9.679
Tobolski and Stockholm	1 30.34	1 37.0	9.663	Upsal and Paris......	1 33.13	1 4 1	9.808
Tobolski and Upsal....	1 27.23	1 33.5	9.646	Upsal and Greenwich..	1 14.11	1 21	9.836
Tobolski and Bologna..	3 25.75	3 37.5	9.513	Hernosand and Paris ..	1 52.47	1 57.5	9.402
Tobolski and Florence	3 31.27	3 42.5	9.525	Hernosand & Greenwich	1 33.45	1 37	9.342
Stockholm & Greenwich	1 11. 0	1 17.5	9.824	Hernosand and Bologna	2 17.86	2 20	9.139
Stockholm and Paris..	1 30.02	1 38.0	9.797	Hernosand and Florence	2 23.38	2 25	9.101
Stockholm and Bologna	1 55.41	2 0.5	9.396	Abo and Paris........	1 42.46	1 55.5	10.145
Stockholm and Florence	2 0.93	2 5.5	9.340	Abo and Greenwich ..	1 23 44	1 33	10.031
Tornea° and Gottingen	1 54.59	2 7	9.974	Abo and Bologna	2 7.85	2 18	9.714
Tornea° and Paris	2 20.25	2 34.5	9.914	Abo and Florence	2 13.37	2 23	9.649
Tornea° and Greenwich	2 1.23	2 14	9.948	Tornea and Bologna..	2 45.64	2 57	9.617
Cajaneburg & Greenwich	1 54.58	2 8	10.054	Tornea° and Florence..	2 51.16	3 2	9.569
Cajaneburg and Paris..	2 13.60	2 28.5	10.003	Greenwich and Paris..	0 19.02	0 20.5	9.700

The mean of the whole is 9″.695.

It has been shewn that the parallax resulting from the total durations is.................... 9″.579
— from a comparison of the observation at Madras with those of Tobolski and Cajaneburg is 9.763
— from a comparison of the observed, with a calculated duration without parallax is 9.724
— from the least distance of the centres.. 9.920
— from the observations combined together is... 9.695

It can hardly be supposed, as such different methods give a parallax of the sun on the day of the transit equal to 9″.736, that this parallax should yet be only 8″.692, as deduced from a comparison of the observations with the Cape, while the same observations compared with those of Rodrigues and Lisbon show that the parallax exceeds 10 seconds. Let us therefore suppose that the observers at the Cape have set down their observation one minute too soon, (though it must be confessed that the time of the duration at the egress cannot warrant such a correction,) and that the time of the internal contact should have been observed at $21^h 40^m 52$; the parallax by taking a mean will then be $9''.732$, exactly agreeing with a mean of all the other determinations. And in this quantity of the sun's parallax we must either acquiesce, or remain as ignorant of the true quantity of it as we were before, till we can have recourse to the next transit on June 3d, 1769, when the planet Venus will again pass over the sun's disk, having something more than 10 minutes of north latitude; and will be so favourably circumstanced, that if the errors in observing each contact do not exceed 4^s or 5^s, the quantity of the sun's parallax may be determined within less than $\frac{1}{100}$ part of the whole; as the total duration, or the interval between the two internal contacts, will be found to be about 18 minutes longer at Tornea° than at Mexico. But the several circumstances of that transit must be the subject of a future paper. Let it suffice at present to observe, that it will in part be visible to the inhabitants of this island, as Venus will be seen wholly entered on the sun's disk more than half an hour before the time of sun-set at Greenwich.

LVI. On the Locus for Three and Four Lines, celebrated among the Ancient Geometers. By H. Pemberton, M. D., R. S. Lond. et R. A. Berol. S. p. 496.

My worthy friend, and associate in my early studies, the collector* of the late Mr. Robins's mathematical tracts, thought it conducive to a more complete vindication of the memory of his friend against an insinuation prejudicial to his candour, to make some mention of the course I took in my early mathematical pursuits, and how soon I became attached to the ancient manner of treating geometrical subjects. This gave occasion to my looking into some of my old papers, among which I found a discussion of the problem relating to the locus ad tres et quatuor lineas, celebrated among the ancients, which I then communicated to a friend or two, whose sentiments of those ancient sages were the same with mine. What I had drawn up on this subject is contained in the papers following.

The describing a conic section through the angles of a quadrilateral with two parallel sides, is so ready a means of assigning loci for the solution of solid problems, that it cannot be doubted but this gave rise to the general problem concerning 3 and 4 lines mentioned by Apollonius, and described by Pappus; and it may be learned from Sir Isaac Newton, who has considered the problem, how easily the most extensive form of it is reducible to the case which probably gave rise to it.

Sir Isaac Newton refers the general problem to this: any quadrilateral ABCD, (fig. 1, pl. 2) being proposed, to find the locus of the point P, whereby PRQ being drawn parallel to AC, and SPT parallel to AB, the ratio of the rectangle

* Dr. James Wilson.

contained under QP, PR to that under SP, PT, shall be given; and this by pursuing the steps, by which he proves that the point P will in every quadrilateral be in a conic section, may be readily reduced to the case of a quadrilateral with 2 sides parallel, after this manner. Draw B*t* and DN parallel to AC; then find the point M in ND, so that the rectangle under MDN be to that under ANB in the ratio given; and draw C*r* M*d*. Here R*r* will be to AQ, or SP, as MD to AN, and B*t*, or QP, to T*t* as ND to NB; whence the rectangle under R*r*, QP, will be to that under SP, T*t*, as that under MDN to that under ANB, that is, in the ratio given of the rectangle under RPQ to that under SPT. Therefore, by taking the sum of the antecedents and of the consequents, the rectangle under *r*PQ will be to that under SP*t*, that is, to the rectangle under AQB, in the quadrilateral ABC*d*, whose two sides AC, B*d*, are parallel, in the given ratio.

In like manner, if 3 of the given lines passed through one point, as the lines CA, CB, CD, fig. 2, and the rectangle under QPR be to that under SPT in a given ratio, this case is with the same facility reduced to the like quadrilateral thus.

Draw BE parallel to AC, that shall cut ST produced in *t*; and let the point F be taken, so that the rectangle under CA, EF, be to the square of AB, in the ratio given; then C*r*F being drawn, B*t*, or QP, will be to T*t*, as AC to AB, and R*r* to AQ, or SP, as EF to AB; whence the rectangle under QP, R*r* will be to that under T*t*, SP, as that under AC, EF, to the square of AB, that is, in the given ratio of the rectangle under QPR to that under SPT; and the rectangle under QP*r* will be to that under SP*t* or AQB, in the quadrilateral ABCF, whose two sides AC, BF are parallel, in the same given ratio.

Now let ABCD be a quadrilateral having the two sides AC, BD parallel, with any conic section passing through the 4 points A, B, C, D, fig. 3, 4, 5; also, the point E being taken in the section, and EFG being drawn parallel to AC or BD, let the ratio of the rectangle under AGB to the rectangle under FEG be given: then the conic section will be given.—Let the sides AB, CD meet in M, and draw MI bisecting AC and BD in K and L. Then the diameter of the section, to which AC and BD are lines ordinately applied, will be in the line MI; and if NP, QS be tangents to the section, and parallel to AC and BD, fig. 3, 4, the points O, R, in which they intersect MI, will be the points of their contact, and the vertexes of that diameter. But the square of NO is to the rectangle under ANB, and the square of QR to the rectangle under AQB, as the rectangle under EGH or FEG, to that under AGB, therefore in a given ratio; but the ratio of NM to NO, the same as that of QM to QR, is also given; whence the ratio of the square of NM to the rectangle under ANB, or of the square of OM to the rectangle under KOL, is given, as also the ratio of the square of RM to the rectangle under KRL.

Now in the ellipsis the square of MO, the distance of the remoter vertex of the diameter OR from M, fig. 3, is greater than the rectangle under KOL; that is, the given ratio of the rectangle under FEG to that under AGB, must be greater than the ratio of the square of half the difference between AC and BD to the square of AB. But in the hyperbola the square of MO is less than the rectangle under KOL, by which the ratio of the rectangle under FEG to that under AGB, shall be less than that of the square of half the difference between AC and BD to the square of AB,* fig. 4.

In both cases, if the point T be such, that the rectangle under MOT be equal to that under LOK, fig. 3, 4, by which MO shall be to OT in the given ratio of the square of MO to the rectangle under LOK, the given rectangle under KML will be to the rectangle under LTK (by prop. 35, l. 7, Papp.) in this given ratio, and therefore given; consequently the points T and O will be given. In like manner, if the rectangle under MRV be equal to that under LRK, so that MR be to RV in the given ratio of the square of RM to the rectangle under LRK, the given rectangle under KML (by prop. 22, l. 7, Papp.) will be to the rectangle under LVK, in the same given proportion, whence the points V and R will be given. Thus in both cases the points T and V will be found by applying to the given line KL a rectangle exceeding by a square, to which the given rectangle under KML shall be in the given ratio of the square of MO to the rectangle under KOL, or of the square of MR to the rectangle under KRL; MO being to OT, and MR to RV, in that given ratio.

But in the last place, if this given ratio be that of equality, so that the square of RM be equal to the rectangle under KRL, fig. 5, by adding to both the rectangle under MRL, that under RML will be equal to that under KM, LR, and MR to RL as KM to ML, and the vertex R of the diameter RI will be given, the conic section being here a parabola, this diameter having thus but one vertex.

Hitherto the point E, when the line EFG falls between AC and BD, is without the quadrilateral, and within the lines AB, CD, when EFG is without the quadrilateral. But when E is within the lines AC, BD in the first case, and without in the second, the locus of the point E will be opposite sections, each passing through two angles of the quadrilateral.

When one section passes through A and C, and the other through B and D, then if the diameter MI be drawn, as before, and to KL be applied a rectangle deficient by a square, to which the given rectangle under KML, fig. 6, shall be in the given ratio of the square of MO to the rectangle under KOL, or of the square of MR to the rectangle under KRL, the points T and V, constituting the

* As the square of OM shall be greater or less than the rectangle under KOL, the square of NM will be respectively greater or less than the rectangle under ANB; therefore the ratio of the square of NO to the rectangle under ANB, that is, of the rectangle under FEG to that under AGB, will be accordingly greater or less than the ratio of the square of NO to the square of NM, which is the same with that of the square of the difference between AK, BL to the square of AB.—Orig.

rectangles under KTL and under KVL, being thus found, MO will be to OT, and MR to RV, in this given ratio (by prop. 30 l. 7 Papp.) O and T being the vertexes of the diameter MI. But the rectangles under KTL, KVL cannot be assigned, as here required, unless the ratio given for that of the square of OM to the rectangle under KOL, or that of the square of RM to the rectangle under KRL, be not less than that of the rectangle under KML to the square of half KL; that is, when the ratio of the square of ON to the rectangle under ANB, and that of the square of RQ to the rectangle under AQB, or that of the given ratio of the rectangle under FEG to that under AGB, is not less than that of the rectangle under AK, BL to the square of half AB, or of the rectangle under AC, BD to the square of AB.

But if one of the opposite sections pass through A and B, and the other through C and D, the ratio of the rectangle under FEG to that under AGB, will be less than that of the rectangle under AC, BD to the square of AB, fig. 7. For CL being drawn parallel to AB, and AD joined and continued to M, the line DM falls wholly within the section passing through C and D: therefore KM is less than KL, and the ratio of KD to KL less than that of KD to KM, that is, of BD to AB; whence BK being equal to AC, and CK to AB, the ratio of the rectangle under BKD to that under CKL, being the ratio of the rectangle under EGH, or FEG, to that under AGB, will be less than the ratio of the rectangle under AC, BD to the square of AB. And here the point L is given; for the given rectangle under BKD is to that under CKL, in the given ratio of the rectangle under HGE, or that under FEG, to the rectangle under AGB; hence CK, equal to AB, being given, KL is given, and consequently the point L.

Again, BL being joined, and NEOP drawn parallel to AB, also GEF continued to Q, as AG, equal to CQ, is to FQ so will CK be to DK, and OP to EG, equal to OB, as KL to BK; consequently the rectangle under OP, AG will be to that under EG, FQ, as that under KL, CK to that under KB, DK, that is, as the rectangle under AGB to that under FEG; and, by combining the antecedents and consequents, the rectangle under PEN will be to that under QEG in the same given ratio.

Moreover DK being to AC as KM to CM, the ratio of DK to AC, that is, the ratio of the rectangle under BKD to the square of AC, will be less than the ratio of KL to CL, or the ratio of the rectangle under CKL to that under AB, CL; therefore, by permutation and inversion, the ratio of the rectangle under CKL to the rectangle under BKD, that is, the given ratio of the rectangle under NEP to that under ANC, equal to that under GEQ, is greater than the ratio of that under AB, CL to the square of AC. And hence, the opposite sections passing through the angles of the quadrilateral ABCL, whose sides AB, CL are parallel, will be given as before.

When the given ratio of the square of OM to the rectangle under LOK shall be that of the rectangle under KML to the square of half KL, fig. 6, by which the given ratio of the rectangle under FEG to that under AGB shall be that of the rectangle under AC, BD to the square of AB, the points T and V shall unite in one, bisecting KL, and the points O and R shall also unite in one, dividing the line KLM harmonically; and then the locus of the point E will be each of the diagonals of the quadrilateral.

In the last place, if the diagonals AD, BC of the quadrilateral were drawn, cutting GE in I and K, fig. 8, and the ratio of the rectangle under KEI to that under AID were given, and not that of the rectangle under GEF to that under AGB; then the intersection of these diagonals, as L, will be in the line drawn from M bisecting AC, and BD, and the point L will fall within the quadrilateral, by which the locus, when an ellipsis or single hyperbola, will be assigned by the 36th prop. of the aforesaid book of Pappus; and when opposite sections, by the 30th prop. or be reduced to the preceding cases thus: since KG will be to GB as CA to AB, and IG to GA as BD to AB, the rectangle under KGI will be to that under AGB, in the given ratio of the rectangle under AC, BD to the square of AB. Therefore when the ratio of the rectangle under KEI to that under AID is given, the rectangle under AID also bearing a given ratio to that under AGB, the ratio of the rectangle under KEI to that under AGB will be given; and in the last place the ratio of the rectangle under GEF to that under AGB will be given, this rectangle under GEF being the excess of that under KGI above that under KEI, by prop. 193, lib. 7, Papp. And thence the sections will be determined, as before.

And thus may the locus of the point sought be assigned in all the cases of this ancient problem, which Sir Isaac Newton has distinctly explained. The other cases, he has alluded to, may be treated as follows. When 3 of the given lines shall be parallel, as AC, BD, and HI, the 4th line being AB, fig. 9, and KELM being parallel to AB, the ratio of the rectangle under KEL to the rectangle under EG and EM shall be given; that is, 3 points A, B, and H being given in the line AB, with the line GE insisting on AB in a given angle, that the rectangle under AGB shall be to that under GH and GE in a given ratio: then take AN equal to BH, and draw NO parallel to AC, BD, and HI.—Then if NP be drawn, so that PO be to ON in the giveratio, NP will be given in position, and PO will be to ON, that is, EG, as the rectangle under KEL to that under MEG; so that the rectangle under KEL will be equal to that under PO, EM. But the rectangle under OKM is equal to the excess of that under OEM above that under KEL, by prop. 194, lib. 7, Papp.; therefore the rectangle under OKM, or that under NAH, or under NBH, is equal to that under EM and the excess of OE above OP, that is, to the rectangle under PEM; the point E therefore is in an hyperbola described to the given asymptotes PN, MH, and passing through A and B.

Again if two of the given lines only are parallel, but the rectangles otherwise related to them, than as above. Suppose the ratio of the rectangle under AG, EF to that under BG, GE is given. Let CD meet AB in L, and let HEI, MFN be drawn parallel to AB, and LK parallel to AC and BD, fig. 10. Then the parallelogram EM will be to the parallelogram EB in the given ratio. Take AO to OB in that ratio, and draw OP parallel to AC and BD. Here the point O will be given, and the parallelogram PA will be in the given ratio to the parallelogram PB; whence AB will be to BO as the parallelogram BH to the parallelogram BP, and as the difference between the parallelogram EM and EB to the parallelogram EB, consequently as the parallelogram GM to the parallelogram PG; therefore the ratio of the rectangle under AG, FG to the rectangle under EG, EP or OG will be given; and in the last place the ratio of FG to GL being given, the ratio of the rectangle under AG and GL to that under EG, OG will be given. And thus 3 points A, L, O, will be given, with GE insisting on AB in a given angle, as in the preceding case.

Moreover, AC and BD being parallel, AB and CD may be also parallel, fig. 11. And then, when the ratio of the rectangle under AGB to that under GEF is given, the determination of the locus is so obvious as not to have required a distinct explanation. But when the rectangle under AG, EF bears a given ratio to that under BG, GE; let the diagonals AD, BC be drawn, and HELK drawn parallel to AD. Then the rectangle under HEL will be to that under KEI in the same given ratio; and if CM be taken to MB in the same ratio, the lines MNP, MOQ drawn, the first parallel to AC, BD, and the other parallel to AB, CD, will be given in position, and the diagonal BM will bisect both IK, NO, and HL; therefore the rectangle under HEL being to that under KEI as MC to MB, that is, as NH to NK, here by division the rectangle under HBL will be to that under IHK, by the prop. of Papp. before cited, as NH to HK; therefore equal to that under NH and IH or KL. But the rectangle under NEO is equal to the sum of the rectangles under HNL and under HEL, by the same; therefore the rectangle under NEO is equal to that under NH, NK, equal to that under APD, that is, equal to that under PAQ, or that under PDQ, the diagonal BM bisecting both PQ and AD. But thus the point E is in an hyperbola described to the asymptotes MN, MO, and passing through A and D.

The determination of this locus for 3 lines is solved almost explicitly by Apollonius in the last 3 propositions of his 3d book of Conics. For if the 3 lines proposed were AB, AC, BC, fig. 12, 13, 14, and the point sought D, so that the ratio of the rectangle under EDF (the line EF being drawn parallel to BC) should be in a given ratio to the square of a line drawn from D to BC in a given angle, the square of which line will be in a given ratio to the rectangle under BE, CF; then if BH, CI are drawn parallel to AC and AB respectively, also BDL, CDK drawn through D, the square of BC will be to the rectangle under BK, CL, as the rectangle under DF, DE, to that under CF, BE. Hence, if the ratio of the rectangle under DF, DE to the square of a line drawn from D on BC in a given angle, be given; the square of this line being in a given ratio to the rectangle under CF, BE, the ratio of the rectangle under BK, CL to the square of BC, will be given; whence a conic section passing through D will in all cases be given.

In the first place let the point D be within the angle BAC, fig. 12. Then if BC be bisected by the line AM, this will be a diameter to the conic section, which shall touch BA, AC in the points B, C, and BC will be ordinately applied to that diameter; the vertex of this diameter being N, the given ratio of the rectangle under BK, CL to the square of BC, will be compounded of the ratio of the square of MN to the square of NA, and of the ratio of the rectangle under BAC to the 4th part of the square of BC; and thus the line AM will be divided in N in a given ratio, and the point N, one vertex of the diameter, to which BC is ordinately applied, will be given.—If AN be equal to NM, the point N will be the only vertex of this diameter, and the section will be a parabola.—Otherwise by taking the point O in AM extended, so that the ratio of AO to OM be the same with that of AN to NM, the point O will be the other vertex of the diameter. And here, if the ratio of AN to NM be that of a greater to a less, the point O will fall beyond M from A within the angle BAC, the conic section being an ellipsis. But if the ratio of AN to NM be that of a less to a greater, the point O will fall on the other side of A, and the section will be an hyperbola, fig. 13. And in this case if the opposite section be drawn, that also will be the locus of the point D within the angle vertical to the angle BAC.

In the last place, if D be in either of the collateral angles, AM drawn as before will contain a secondary diameter in opposite sections, one of which shall touch BA in B, and the other CA in C, fig. 14. Then if one of these sections pass through D, the sections will be given. For here PAQ being drawn through A parallel to BC, the given ratio of the rectangle under CL, BK to the square of BC will be the same with that of the given rectangle under BAC to the square of AP: therefore AP is given, and thence the sections. For let RS be the secondary diameter, to which BC is ordinately applied, and T the centre of the opposite sections. Then the square of BM will be to the rectangle under AMT, as the square of the transverse diameter conjugate to the secondary diameter RS, to the square of this secondary diameter; and if a line were drawn from M to P, this would touch the hyperbola BP in P, Apoll. conic. l. 2, prop. 40. and the square of AP will be to the rectangle under MAT in the same ratio; therefore the given ratio of the square of MB to the square of AP, will be that of the rectangle under AMT to the rectangle under MAT, or the ratio of MT to AT; consequently the ratio of MT to AT is given, and thence the point T. But also the diameter RS is given in magnitude, the square of RT or of ST being equal to the rectangle under MTA; whence in the last place the transverse diameter conjugate to this is also given; for the square of this diameter is to the square of RT, as the given square of BM to the rectangle under AMT now also given.

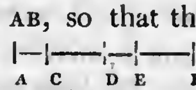
But a more simple case may also be proposed in 3 lines, when the ratio of the rectangle under EDF should be equal to the rectangle under a given line, and that drawn from D to BC in a given angle, fig. 15. This line will bear, both to BE and FC, a given ratio, and the rectangle under EDF will be in a given ratio to the rectangle under the given line and EB or CF.—Let the line given be H, and take MB and NC, so that the rectangle under MBC, and that under BCN be to that under BA and H, in the given ratio of the rectangle under EDF to that under BE and H, BM and CN being equal. Then draw from M and N lines parallel to BA, CA, which shall intersect EF in K and L, by which, MK cutting CA in I, the rectangle under MBC will be to that under BA and H, as the rectangle under BMC to that under MI and H, and also as the rectangle under EKF to that under KI and H, that is, as the rectangle under EDF to that under H and BE or MK; whence, by adding the antecedents and consequents, the rectangle under KDL will be to the rectangle under H and MI, in the same given ratio, which is also that of the rectangle under BMC to the same rectangle under H and MI: the point D therefore is in an hyperbola passing through B and C having for asymptotes the lines MK and NL given in position, the rectangle under KDL being equal to that under BMC, or that under MBN.

If the two lines AB and AC are parallel, the locus may be known to be a parabola by the last proposition of the 4th book of Pappus. But if BC were parallel to one of the other, the locus will be an hyperbola, as the preceding, but assigned by a shorter process. Suppose the given lines to be AE, AF, fig. 16, and BC parallel to AF. And let the rectangle under EDF be equal to that under DG and the given line H, the line EG making given angles with AE, AF. Here take EI equal to H, and deduct from both the rectangles that under EI or H, and DF, by which will be left the rectangle under IDF equal to that under H and FG, both whose sides are given. Draw therefore IK parallel to AE, and the rectangle under IDF will be equal to this given rectangle, the given lines KI, AF being the asymptotes to the hyperbola passing through D.

Coroll. If LM be drawn through B parallel to EF, LB shall be equal to FG, and BM equal to EI or H, by which the hyperbola opposite to that passing through D will pass through B.

SCHOLIUM.—The propositions of Pappus, which have been here referred to, are given by him, among others, for Lemmas subservient to the lost treatise of Apollonius De sectione determinata; and the four here cited respect and comprehend all the cases of the problem, where 3 points are given in any line, and a 4th is required such, that the rectangle under the segments of the proposed line intercepted between the point sought, and two of the given points shall bear a given ratio to the square of the segment terminated by the third point. The cases indeed of the problem, from the diversity of situation in the points given to the points sought, and to one another, are in number 6. The given extreme of the segment to constitute the square, may either be without the other 2 given points, or between them. And when it is without, the point sought may be required to be taken without them all, either on the side opposite to the given extreme of the segment to constitute the square, which will be one case, or it may be required to fall on the same side, which will be a second case. If it be required to fall between this point and the other two, this will be a 3d case. A 4th case will be, when the point sought shall be required to fall between the other two points. Also when the given extreme of the segment to constitute the square lies between the other two given points, the point sought may be required to fall, either there also, or without, composing the 5th and 6th cases.

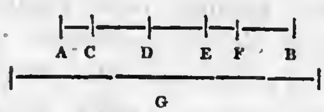
The propositions in Pappus referring to these cases, though but 4 in number, suffice for them all, each proposition being applicable to the problem 2 ways. For instance, the 35th prop. as expressed by Pappus, is this, being the first above cited. Three points C, D, E being taken in the line AB, so that the rectangle under ABE be equal to that under CBD, AB is to BE as



the rectangle under DAC to that under CED. Now AB is to BE, both as the square of AB to the rectangle under ABE, and as the rectangle under ABE to the square of BE. Therefore, if the ratio of AB to BE be given, the ratio of the square of AB to the rectangle under CBD will be given, which is the first of the cases above described, and also the ratio of the rectangle under CBD to the square of BE given, which is the 2d case. In both cases, the rectangle under DAC will be to that under CED, in the given ratio of AB to BE. But in the first the rectangle under DAC will be given, and the point E in the rectangle under CED to be found by applying a rectangle, which shall bear a given ratio to the given rectangle under DAC, to the given line CD exceeding by a square; and in the 2d case the rectangle under CED is given, and A in the rectangle under DAC to be found, by applying to the given line CD a rectangle exceeding by a square, which shall bear a given ratio to the rectangle under CED now given; whence, by the ratio of AB to BE given, the point B will be found in both cases. The 22d prop. either way applied refers to the 3d case only; the 30th relates both to the 4th and 5th; and the 36th prop. to the remaining 6th.

The 45th, and other following propositions, are accommodated to the solution of Apollonius's problem, when 4 points are given, and a fifth required, which with the given points shall form 4 segments such, that the rectangle under two shall bear a given proportion to the rectangle under the other two. The various cases of this problem appear to have been the subject of the 2d book of the mentioned treatise of Apollonius; and according to the character given by Pappus of those propositions, these lemmas serve to reduce them to problems in the first book, not those above mentioned, but those where 3 points being given, the rectangle under the segments included by two, and a 4th point, shall bear a given ratio to the rectangle under the segment formed by the 3d point, and a given line.

For instance, the 46th prop. is this: in the line AB, 4 points A, C, E, B, being given; and the point F assumed between E and B; also D taken, according to the 41st prop., so that the rectangle under ADC be equal to that under BDE; if G be equal to the sum of AE, CB, the rectangle under AFC, together with that under EFB, will be equal to the rectangle under G and DF. Here if it were proposed to find the point F, so that the ratio of the rectangle under AFC to that under EFB should be given, the ratio of the rectangle under AFC to that under DF and the given line G would be given.



But this analysis may be carried on to a compleat solution of the problem thus. If CN be taken to G in the given ratio of the rectangle under AFC to that under DF and G, the point N will be given, and the rectangle under AF, CN will be to that under AF, G, in this ratio of CN to G; consequently the excess of the rectangle under AF, CN above that under AFC, that is, the rectangle under AFN, will be to the excess of the rectangle under AF and G above that under DF and G, or the given rectangle under AD, G, in the same given ratio; and in the last place the rectangle under AFN will equal the given rectangle under AD and CN.

Here I have chosen this prop. in particular, because the case of the problem to which it is subservient, is subject to a determination, when FN shall be equal to AF. And then the rectangle under AFN being equal to that under AD and CN, as CN to FN so is AF to AD, and by division as CF to FN so DF to AD; therefore when AF is equal to FN, CF will be to AF as FD to AD: consequently CD to FD as FD to AD, and the square of DF equal to the rectangle under ADC, when the problem admits of a single solution only, where the rectangle under AFC will bear to that under EFB a less ratio than in any other situation of the point F between E and B.

Moreover CN is to G as the rectangle under AFC to the sum of the rectangles under AFC and EFB; therefore FN being equal to AF, when the problem is limited to this single solution, the rectangle under AFC shall be to the rectangles under

AFC and EFB together, as the sum of AF and FC to G, which is equal to the sum of AB and CB; whence by division the ratio of the rectangle under AFC to that under EFB, when the problem is limited to this single solution, will be that of the sum of AF and CF to the excess of FB above EF.

Thus directly do these lemmas correspond with Apollonius's first mode of solution, and lead to the general principle of applying to a given line a rectangle exceeding or deficient by a square, which shall be equal to a space given. This being a simple case of the 28th and 29th propositions of the 6th book of Euclid's elements, admits of a compendious solution. Such a one is exhibited by Snellius in his treatise on these problems (in Apollon. Batav.) and Des Cartes has exhibited another, more contracted in its terms, but not therefore more useful. It may also be performed thus. If upon a given line AB any triangle ACB be erected at pleasure; then if the legs CA, CB, fig. 17, whether equal or unequal, be continued to D and E, so that the rectangles under CAD and CBE be each equal to the given space, and a circle be described through C, D, E, cutting AB extended in F and G, the rectangle under BFA and BGA will each be equal to the space given. Also if in the legs CA, CB, fig. 18, the rectangles under CAD and CBE be each taken equal to the space given, and a circle in like manner be described through C, D, E, cutting AB in F and G, the rectangles under AFB and AGB will each be equal to the given space. Here it is evident, that the space given must not exceed the square of half AB; when equal, the circle will touch AB in its middle point.

POSTSCRIPT.

As this application to a given line of a rectangle exceeding or deficient by a square, or the more general problem treated of in the 6th book of the elements, of applying a space to a line so as to exceed or be deficient by a parallelogram given in species, is the most obvious result, to which the analysis of plane problems, not too simple to require this construction, leads; so the descriptions of the conic sections here treated of, stand in the like stead in regard to the higher order of problems, styled solid from the use of the conic sections deemed necessary for their genuine solution. And these are the only modes of solution, the modern algebra, which grounds its operations on one or two elementary propositions only, naturally leads to. But as the form of analysis among the ancients, by expatiating through a larger field, often was found to arrive at conclusions much more concise and elegant, than could offer themselves in a more confined track; the ancient sages in geometry, that the solid order of problems might not want this advantage, sought out that copious and judicious collection of properties attending the conic sections, which, with some useful additions from later writers, have come down to us.

And as the advantages of this ancient system of analysis cannot be too much inculcated, in an age in which it has been so little known, and almost totally neglected, Dr. P. closes this paper with an example in each species of problems. Thus, were it proposed to draw a triangle given in species, that two of its angles might touch each a right line given in position, and the 3d angle a given point. It is obvious how difficult it would be to adapt a commodious algebraic calculation to this problem; yet it admits of more than one very concise solution, as follows. Let the lines given in position be AB, AC, and the given point D, the triangle given in species being EDF, fig. 19, 20, 21.

In the first place suppose a circle to pass through the 3 points A, E, D, which shall intersect AC in G, fig. 19. Then EG, DG being joined, the angle DEG will be equal to the given angle DAC, both insisting on the same arch DG: also the angle EDG is the complement to two right ones of the given angle BAC: these angles therefore are given, and the whole figure EFGD given in species. Consequently the angle EGF, and its equal ADE, will be given, together with the side DE of the triangle in position.

Again, suppose a circle to pass through the 3 points A, E, F, cutting AD in H, and EH, FH joined, fig. 20. Here the angle EFH will be equal to the given angle EAH, and the angle FEH equal to the given angle FAH. Therefore the whole figure EHFD is given in species, and consequently the angle ADE, as before.

In the last place, suppose a circle to circumscribe the triangle, and intersect one of the lines, as AC, in I, fig. 21. Here DI being drawn, the angle DIF will be equal to the given angle DEF in the triangle; consequently DI is inclined to AC in a given angle, and is given in position, as also the point I given; whence, IE being drawn, the angle FIE will be the complement of the angle EDF in the triangle to two right ones. Therefore IE is given in position, and by its intersection with the line AB gives the point E, with the position of DE, and thence the whole triangle, as before. Here it may be observed, that the angle D of the triangle EDF given in species touching a given point D, and another of its angles touching AC, the line IE here found is the locus of the 3d angle E.

Again, in the astronomical lectures of Dr. Keil, it is proposed to find the place of the earth in the ecliptic, whence a planet in any given point of its orbit shall appear stationary in longitude, and a solution is given from the late eminent astronomer Dr. Halley, on the assumption, that the orbit of the earth be considered as a circle concentric to the sun. But for a complete solution of this problem let the following lemma be premised. The velocity of a planet in longitude bears to the velocity of the earth, the ratio which is compounded of the subduplicate ratio of the latus rectum of the greater axis of the planet's orbit, to the latus rectum of the greater axis of the earth's orbit, of the ratio of the cosine of the angle which the orbit of the planet makes with the plane of the ecliptic, to the radius, and of the ratio of a line drawn in any angle from the centre of the sun to the tangent of the orbit of the earth, at the point where the earth is, to a line drawn in the same angle from the sun to the tangent of the orbit of the planet projected on the plane of the ecliptic, at the place of the planet in the ecliptic.

Let A be the sun, BC the orbit of any planet, DE the same projected on the plane of the ecliptic, FG being the line of the nodes, B the place of the planet in its orbit, D its projected place: then the plane through B and D, which shall be perpendicular to both the planes BC and DE, intersecting those planes in BH, DH, the lines BH, DH will be both perpendicular to the line of the nodes, and the angle BHD the inclination of the orbit to the plane of the ecliptic. But tangents drawn to BC and DE, at the points B and D respectively, will meet the line of the nodes, and each other in the same point I, and the velocity of the planet in longitude will be to its velocity in the orbit BC, as DI to BI.

Now from the point A let AK fall perpendicular on BI, and AL be perpendicular to DI: then the ratio of DI to IB will be compounded of the ratio of DI to DH, or of AI to AL, of the ratio of DH to BH, and of that of BH to BI, that is of AK to AI. But DH is to BH as the cosine of the inclination of the orbit to the radius; and the two ratios, that of AI to AL, and that of AK to AI, compound the ratio of AK to AL: therefore the velocity of the planet in longitude, is to the velocity in its orbit, in the ratio compounded of that of the cosine of the inclination of the planet's orbit to the radius, and that of AK to AL.

Moreover the ratio of the velocity of the planet in B to the velocity of the earth in any point of its orbit, is compounded of the subduplicate of the ratio of the latus rectum of the greater axis of the planet's orbit to the latus rectum of the greater axis of the earth's orbit, and of the ratio of the perpendicular let fall from the sun on the tangent of the earth's orbit at the earth to AK, the perpendicular let fall on the tangent of the planet's orbit at B. Therefore the velocity of the planet in longitude, when in B, to the velocity of the earth in any point of its orbit, is compounded of the subduplicate ratio of the latus rectum of the greater axis of the planet's orbit, to the latus rectum of the greater axis of the earth's orbit, of the ratio of the cosine of the inclination of the planet's orbit to the radius, and of the ratio of the foresaid perpendicular on the tangent of the earth's orbit to AL, the perpendicular on DI: these perpendiculars being in the same ratio with any lines drawn in equal angles to the respective tangents.

This being premised, the place of a planet in the ecliptic being given, the place of the earth, whence the planet would appear stationary in longitude, may be assigned thus. A denoting the sun, fig. 23, let B be a given place of any planet in its orbit projected orthographically on the plane of the ecliptic, CB the tangent to the planet's projected orbit at the point B, which will therefore be given in position. Also let DE be the orbit of the earth, and the point D the place of the earth, whence the planet would appear stationary in longitude at B.

Join AB, and draw a tangent to the earth's orbit at the point D, which may meet CB in F, and the line AB in G; draw also AH making with DF the angle AHD equal to that under ABC. Then the point D being the place whence the planet appears stationary in longitude, as FB to FD so will the velocity of the planet in longitude in B, be to the velocity of the earth in D; this velocity of the planet in B being also to the velocity of the earth in D, in the ratio compounded of the subduplicate of the ratio of the latus rectum of the greater axis of the planet's orbit to the latus rectum of the greater axis of the orbit of the earth, of the ratio of the co-sine of the inclination of the planet's orbit to the plane of the ecliptic to the radius, and of the ratio of AH to AB; therefore the ratio of FB to FD will be compounded of the same ratios; and if I be taken so that the ratio of AB to I be compounded of the first two of these, I will be given in magnitude, and the ratio of FB to FD will be compounded of the ratio of AB to I, and of AH to AB. Whence FB will be to FD as AH to I; and the angles CBA, or FBG, and AHG being equal, by which FG will be to FB as AG to AH, by equality FG will be to FD as AG to I, and DK being drawn parallel to FB, BG will be to BK as FG to FD, and therefore as AG to I.

But now as this problem may be distributed into various cases, in the first place consider the earth as moving in a circle concentric to the sun, and likewise CB, the tangent to the planet's orbit, perpendicular to AB. But here DK also will be perpendicular to AB, and AB meeting the earth's orbit in L and M, the rectangle under KAG will be equal to the square of AM, fig. 24. But BG being to BK as AG to I, if BN be taken equal to I, BG will be to BK as AG to BN, and AB to KN also as AG to BN, and the rectangle under NK, AG equal to that under AB and I; therefore the rectangle under KAG being equal to the square of AM, NK will be to KA as the rectangle under AB and I to the square of AM, that is, in a given ratio, and KD with the point D will be given in position.

Again, when CB is not perpendicular to LM, let DO be perpendicular to LM. Then the rectangle under OAG will be equal to the square of AM, fig. 25. But BN being taken equal to I as before, the rectangle under NK, AG will be equal to that under AB, I, whence NK will be to AO in the given ratio of the rectangle under AB, I to the square of AM. Therefore NP being taken to PA in that ratio, the point P will be given, and KP, the excess of NP above NK, will be to PO, the excess of AP above AO, in the same ratio. Hence, as DK is parallel to CB, and DO perpendicular to LM, the triangle KOD is given in species; and if PD be drawn, the angle OPD will be given; for the co-tangent of the angle OKD will be the co-tangent of the angle OPD, as KO to OP, that is, as the rectangle under AB, I together with the square of AM, to the square of AM: and hence the point D is given by the line PD drawn from a given point P in a given angle APD; and if AD be drawn, AD will be to AP as the sine of the angle APD to the sine of the angle PDA; this angle therefore is given, and the angles APD, PDA being given, the angle PAD is given.

Coroll. Here where the orbit of the earth is supposed a circle, the ratio of I to AB, that is, of the rectangle under AB, I to the square of AB, will be compounded of the subduplicate ratio of AM, the semidiameter of the earth's orbit, to half the latus rectum to the greater axis of the planet's orbit, and of the ratio of radius to the co-sine of the inclination of the planet's orbit to the plane of the ecliptic; and, adding on both sides the ratio of the square of AB to the square of AM, the ratio of the rectangle under AB, I, to the square of AM will be compounded of the ratio of the square of AB to the rectangle under AM and the mean proportional between AM and the half of this latus rectum of the planet's orbit, and of the ratio of the radius to the co-sine of the inclination of the planet's orbit.

In the next place, though the earth's orbit is not a circle concentric to the sun; yet if the projection of the planet falls on the line perpendicular to the axis of the earth's orbit, the point A will still bisect LM. In this case draw, to the points L and M, tangents to the ellipses meeting in P, whence through D draw PD meeting the ellipses again in Q, and intersecting LM in O. Here if a tangent be drawn to the ellipses in Q, it will meet the tangent at D on the line LM in the point G, fig. 26. Now LG will be to GM as LO to OM, and the point A bisecting LM, the rectangle under GAO will be equal to the square of AM. But BG is to BK as AG to I. Therefore BN being taken equal to I, AB will be to KN as AG to I, and the rectangle under AB, I equal to that under AG, KN; whence AO being to KN as the rectangle under GAO to that under AG and KN, AO will be to KN as the given square of AM to the rectangle under AB and I also given.

Draw RP parallel to CB, and take PS to AP, also NT to AR, in this given ratio inverted. Then will the points T and S be both given, also AO will be to KN, and RO to KT, as AR to NT, that is, as AP to PS. Therefore if TV be drawn parallel to CB, that is, to KD, and VS parallel to LM, these lines will be both given in position; and WDXY being also drawn parallel to LM, WD will be equal to KT, and RO being to KT as AP to PS, DY will be to WD as XP to PS, and by composition YW to WD as XS to PS, and the given rectangle under YW, or SV, and PS equal to that under WD and XS. Whence SV being parallel to LM, the point D will be in an hyperbola through P, and having for asymptotes the lines VS, VT, given in position.

But if the projection of the planet fall on the axis of the earth's orbit, or the same continued, AB extended to the earth's orbit in L and M will be the axis of that orbit. If also CB should be perpendicular to AB, KD would be ordinately applied to LM, fig. 27; and the point R being taken, so that Q being the centre of the orbit, the rectangle under AQR be equal to the square of QM, the same will be equal also to the rectangle under GQK; whence as GQ to AQ so RQ to QK, and AG to AQ as KR to QK. But, as above, BG being to BK as AG to I, and BN taken equal to I, BG will be to BK, as AG to BN, and AB to KN also as AG to BN or I. Therefore if NS be taken to AB as I to AQ, by equality NS will be to NK as AG to AQ, that is, as KR to QK; and in the last place NS to KS as KR to QR, that is, the rectangle under SKR equal to the given rectangle NS, QR, whence the point K, the position of KD, and thence the point D will be given.

But if DK be not ordinately applied to LM, let DO be ordinately applied to LM. Then here the rectangle under AQR, equal to the square of QM, will be equal to that under OQG, and GQ to AQ as QR to OQ, fig. 28; whence by composition AG to AQ as OR to OQ. But BN being now also taken equal to I, and NS to AB as I to AQ, AB will be here in like manner to KN as AG to I, and NS to KN as AG to AQ; therefore NS will be to KN as OR to OQ, and by conversion NS to KS as OR to QR. But NS and QR being both given in magnitude, if SP be taken to NS as QR to PR, the point P will be given, and also by equality SP will be to KS as OR to PR: whence if RV be drawn parallel to DO, and ST to KD, both RV and ST will be given in position, one passing through the given point R, parallel to the ordinates applied to the axis LM, and the other through the point S also given, and parallel to KD or CB: also DTV being drawn parallel to ML, DT will be equal to KS, and DV equal to OR; therefore as SP to DT so DV to PR, and the rectangle under SPR equal to that under TDV, consequently the point D in an hyperbola passing through P, and having for asymptotes the lines ST, RV, given in position.

In the last place, when the line LM drawn through the sun in A, and the projected place of the planet in B, is neither the axis of the earth's orbit, nor bisected in A, fig. 29, the tangents to the points L, M, being drawn to meet in P, let LM be bisected in Q, and the point R taken, so that the rectangle under AQR be equal to the square of QM, which by PDO being drawn, the rectangle under AQR shall be equal to that under OQG, and QG to AQ as QR to QO, or by composition AG to AQ as OR to QO. Therefore if NB be here also taken equal to I, and NS to AB as I to AQ, AB being, as before, to NK as AG to I: by equality NS will be to NK as AG to AQ, that is, as OR to QO. Whence by conversion NS will be to KS as OR to QR; and if PT be drawn parallel to CB, and SV be here taken to NS as QR to TR, by equality SV will be to KS as OR to TR, and also by conversion SV to KV as OR to OT. Also SV will be given in magnitude, and the point V given; therefore VW drawn parallel to CB, or KD, will here be given in position. But WDXY being also drawn parallel to RV, SV will be to KV, or DW, as YD to XD; and YZ being taken equal to the given line SV, YZ will be to DW as ZD to XW, equal to TV, and the given rectangle under YZ, TV equal to that under WDZ. Therefore ΓZ being drawn parallel to RP, RΓ, and its equal YZ, being given, the line ΓZ is given in position, and the point D in an hyperbola having for asymptotes VW, ΓZ, and passing through P.

Thus in this problem in all cases solved either by a right line, or an hyperbola given in position, which shall intersect the projected orbit in the point sought. For though in each case the projection of the planet has here been considered as within the orbit of the earth, the form of argumentation will be altogether similar, were the projection of the planet without. And this is agreeable to the method pursued throughout this discourse, where I have always accommodated the expression to one situation only of the terms given and sought in each article; the variation necessary for the other cases, when one has been duly explained, being sufficiently obvious.

In the 5th vol. of the Commentaries of the Royal Academy at Petersburg, is given an algebraical computation for a general solution of this problem, in the orbits of any two planets projected on the plane of the ecliptic; but with this oversight of applying to the projected orbits a proposition from Dr. Keil's Astronomical Lectures, which relates to the real orbits.*

However, from the geometrical solution now given, a calculation for assigning the point D may be formed without difficulty. LDM being the orbit of the earth, A is the focus, and RP perpendicular to the axis. Let this axis be *ab* meeting RP in *c*, ΓZ in *d*, PT in *e* and WV in *f*. Then the angle *a*AM is given, being the distance between the heliocentric place of the planet in the ecliptic from the earth's aphelion. Also PT being parallel to CB, the angle AT*e*, and consequently the angle A*e*T, will in like manner be given, whence the points Γ, R, T, V being given, as in the solution above, the points *d*, *c*, *e*, and *f* will be given, the triangles AR*c*, AT*e*, being given in species, and similar respectively to the triangles AΓ*d*, and AV*f*. Also the rectangle under WDZ being equal to that under RΓ VT, if DK be continued to the axis in *g*, and D*h* be drawn parallel to PR, the rectangle under *fg*, *hd*, is equal to that under *fe*, *dc*, and both being deducted from the rectangle under *fhd*, the excess of the rectangle under *fhd* above that under *fe*, *dc*, will be equal to that under *ghd*, so that this difference will be a mean proportional between the square of *hd* and the square of *hg*, which is in a given ratio to the square of *h*D, and therefore is a given ratio to the rectangle under *ahb*, D*h* being ordinately applied to the axis *ab*.

Thus a biquadratic equation may be formed, by which the point *h* shall be found, and thence the point D, whose distance from A is to *he* as the excentricity of the earth's orbit to half its axis. Therefore I shall only observe further, that here occurs an obvious question, what, in so extended a search for principles leading to the solution of any problem, as the ancient analysis admits of, can conduct to the most genuine on each several occasion. But for this end, where commodious principles do not offer themselves, the most general means is to consider first

* The demonstration of Dr. Keil's proposition proceeds on the known property in the planets of having their periodic times in the sesquiplicate ratio of the axis of their orbits, which confines the proportion to the real orbits; for in each planet the periodic time through the projected orbit, is the same as through the real, though the axis in one be not equal to the axis of the other.—Orig.

simple cases of the problem in question, and thence to proceed gradually to the more complex, as has been here done in the present problem, where the several preceding cases lead one after another to the points and lines required for the last case, in which the problem is stated in its most extensive form.

END OF THE FIFTY-THIRD VOLUME OF THE ORIGINAL.

I. Of a Mummy, inspected at London 1763. *By John Hadley, M.D. F.R.S.* p. 1. *Vol. LIV.*

The mummy, which is the subject of this paper, is the first article in Dr. Grew's catalogue of the rarities of the R. S. He informs us, that it was a present from Henry Duke of Norfolk; and was an entire one, taken out of the Royal Pyramids. He then proceeds to describe the manner in which the several parts were wrapped up: but this he has not done exactly; as most of these very parts had evidently never been opened, till we examined them: and were then found in a very different state from that in which they are represented by him.

This mummy had been greatly injured, before it came into their hands; the head had been taken off from the body; and the wrappers with which they had been united, having been destroyed, the cavity of the thorax was found open towards the neck: and part of the upper crust, with the clavicles, having been also broken away, the heads of the ossa humeri presented themselves, covered with a thin coat of pitch. The feet also had been broken off from the legs; and were fixed by wires to the end of the wooden case in which the mummy lay. The outer painted covering, which reached from the upper part of the chest nearly to the bottom of the legs, had been removed, and fastened on again by a great number of ordinary nails, driven up to the head into the substance of the mummy. This had most probably been done by those who had orders some years since to repair it; and by this, and by the manner in which they had fastened on the feet, they seem to have done their work in a most clumsy manner. This whole external covering of the fore part of the mummy consisted of several folds of broad pieces of linen cloth; made to adhere together by some viscous matter, which had not yet lost its property; and the whole had received an additional degree of strength and substance from the coat of paint laid on. The figures, which were not entirely defaced, were of the usual kind, and were all so much injured, as to render a particular description of them very difficult, if not impossible.

There were not the least remains of hair or integuments on any part of the head; some parts of the skull were quite bare; particularly about the temporal bones; which had the natural polish, and appeared in every respect like the bones of an ordinary skull. To other parts of the skull adhered several folds of pitched linen; which together were near half an inch in thickness: on removing them they were found to have been in actual contact with the bone; so that the integuments must have been taken away, before the wrappers were at first applied. The under jaw was lost; and the superior maxillary, sphenoïdal and ethmoïdal bones were broken away; the foramen occipitale was stopped up with pitch, with which also the inner part of the skull was lined; this seemed to have been poured in at the foramen, and made to apply to the several parts of the inside of the skull, by turning the head in different directions; the wave of the melted pitch from such motion appearing very plain. The inside of the skull was in many places covered very thinly; and in some few, which the fluid pitch had missed, it was quite bare. The pitch which stopped up the foramen occipitale, had on it the impression of one of the vertebræ of the neck; and externally about the foramen adhered a considerable quantity of pitch.

The outer painted covering being removed, nothing but linen fillets were to be seen, which enclosed the whole mummy. These fillets were of different breadths; the greater part about an inch and a half; those about the feet much broader: they were torn longitudinally; those few that had a selvage, having it on one side only; the uppermost fillets were of a degree of fineness nearly equal to what is now sold in the shops for 2s. 4d. per yard, under the name of long lawn; and were woven something after the manner of Russia-sheeting: the fillets were of a brown colour, and in some measure rotten. These outer fillets seemed to owe their colour to having been steeped in some gummy solution; as the inner ones were in pitch. The fillets immediately under the painted covering lay in a transverse direction; under these, which were many double, they lay oblique, diagonally from the shoulders to the ilia. Under these the fillets were broader, some nearly 3 inches; and lay longitudinally from the neck to the feet, and also from the shoulders down the sides; on which there was a remarkable thickness of these longitudinal fillets: under these they were again transverse, and under these again oblique. The fillets in general externally did not adhere to each other; but though pieces of a considerable length could be taken off entire, yet, from the great age, so tender was the texture of the cloth, that it was impossible regularly to unroll them. As the outer fillets were removed, those that next presented themselves had been evidently steeped in pitch, and were in general coarser, in folds, and more irregularly laid on; as they were more distant from the surface. The inner filleting of all was so impregnated with pitch, as to form with it one hard black brittle mass; and had been burned nearly to a coal. On breaking this, it appeared in many places as if filled with a white efflorescence; like that observable on the outside of pyrites which have been exposed to the air. This efflorescence however had nothing saline to the taste, and did not dissolve in water; but instantly disappeared, on bringing it near enough to the fire to be slightly heated; and was soluble in spirit of wine.

In the cavity of the abdomen were found several small pieces of bone, which had the appearance of dry oak, mixed with crumbled pitch; under this was found more solid pitch, which adhered to the spine. After cutting away the mass of cloth and pitch which covered the thorax; it was found that the arms had been laid straight down by the sides of the chest; and the ulna and radius bent upwards, and laid with the hands across upon the breast, the right hand being uppermost. The bones of the fingers were lost; but the metacarpal bones were found broken off, and fallen into the thorax. The filleting, which went round the upper part of the body, included the arms also; but they had evidently been first wrapped separately, then laid up in the position in which they were found, and the hollows which they formed filled up with pieces of pitched cloth. In the cavity of the thorax there was also a considerable quantity of crumbled pitch and splinters of dry bone; and, as in the progress of this examination Dr. H. continually found that some of the bones did, as he laid them bare, separate into such splinters; it is very probable that this appearance was owing to the mummy's having been handled in a rough manner, and much shaken, by the persons who had driven it full of nails, when they were employed to repair the outside of it. On first opening a way into the thorax, he imagined the ribs were destroyed; but, on a more accurate examination, they were found entire; but so bedded in the pitch, and so black and burned into the mass, as to make it difficult to distinguish these very different substances from each other. The bones of the spine and of the pelvis were in the same state with the ribs; only rather more burned.

There was a considerable thickness of hard solid pitch lining the cavity of the thorax; this had been evidently liquified and poured in; and retained that glossy appearance on its surface which is observable on pitch that is suffered to cool without being disturbed. On breaking through this hard crust of pitch to examine the vertebræ and the ribs, the pitch which was under this crust and nearest to the bones, was crumbly and soft; and, on being exposed to the air, grew perfectly moist in a very short time. The lower extremities were wrapped separately in fillets to nearly their natural size, and then bound together; the interstices being rammed full of pitched rags. On cutting through the fillets on the thighs, the bones were found invested with a thin coat of pitch; and the filleting was bound immediately on this. The tibia and fibula of each leg were found also wrapped in the same manner, and the bones in actual contact with the pitch; excepting in one or two places, where the pitch was so very thin, that the cloth appeared to adhere to the bone itself.

The feet were filletted in the same manner; being first bound separately, and then wrapped together. The filleting had been by some accident rubbed off the toes of the right foot; and the nail of the great toe was found perfect: the last joints of the bones of the lesser toes had been broken away; by which it appeared that these bones had been penetrated, and their cavities quite filled with pitch. The filleting about the heel had also been broken away, and the bones of the tarsus, and some of the metatarsal bones, had fallen out and were lost; leaving the remaining filleting like a kind of case. The fillets on the left foot were perfect; except on the heel, and where they had been divided from those of the leg; a small portion of the tendo Achillis adhered to the os calcis; and some of the ligaments to the astragalus. On cutting into the fillets on the sole of this foot they were found to enclose a bulbous root. The appearance of this was very fresh; and part of the thin shining skin came off with a flake of the dry brittle filleting, with which it had been bound down; it seemed to have been in contact with the flesh: the base of the root lay towards the heel. This discovery immediately brought to mind a passage in Prosper Alpinus,* and gave some appearance of probability to a relation which, as he himself insinuates, might give great reason to doubt his veracity. Speaking of the stone image of a scarabæus, which was found in the breast of a mummy, he adds, "Incredibile dictu, rami rorismarini qui una cum idolo inventi fuerunt, folia usque adeo viridia et recentia visa fuerunt, ut eâ die a plantâ decerpti et positi apparuerint."

The fillets were removed from this foot with great care; they were much impregnated with pitch, excepting about the toes; where the several folds united into one mass, being cut through, yielded to the knife like a very tough wax. The toes being carefully laid bare, the nails were found perfect on them all; some of them retaining a reddish hue, as if they had been painted: the skin also, and even the fine spiral lines on it, were still very visible on the under part of the great toe, and of the three next adjoining toes. Where the skin of the toes was destroyed, there appeared a pitchy mass, resembling in form the fleshy substance; though somewhat shrunk from its original bulk. The natural form of the flesh was preserved also on the under part of the foot; near the bases of the toes. On the back of the toes appeared several of the extensor tendons.

The root just mentioned was bound to the foot by the filleting that invested the metatarsal bones; no more of this filleting was cut away than was just sufficient to show, without removing from its place, a substance which had been preserved in so extraordinary a manner. On cutting away the fillets which covered the tarsus, the bones adhered strongly together, and were covered with hard pitch; with which they seemed thoroughly impregnated. On cutting

* Prosper Alpinus Rerum Ægyptiarum, &c.; cum notis Veslingii, 1735, pag. 36.—Orig.

away this outer pitch, there appeared very distinctly the tendons of the peroneus anticus and posticus, the tendons of the extensor digitorum longus, and the tendon of the tibialis anticus; and besides these a considerable portion of the ligaments of the tarsus. On examining the case formed by the pitch and fillets, which had covered the right foot, and out of which the bones had been taken; there was a very plain mould left, in which there had been enclosed another root similar to that discovered in the left foot; and in which some of the external shining skin of the root still remained.

During this whole examination, excepting what was discovered in the feet, there were not found the least remains of any of the soft parts. All the bones of the trunk were bedded in a mass of pitch; and those of the limbs were covered with a thin coat of it, and then swathed in the fillets; which (as has been mentioned) in some places, where the pitch was very thin, seemed to adhere to the bone itself. The cavities of many of the bones, on being broken, were found quite full of this substance: the metacarpal bones were so; as were the radii, and many others: the ribs, as before mentioned, were impregnated with it; and so burned, as to be with difficulty distinguished from it: in which state also were the vertebræ and the bones of the pelvis. The pitch had also penetrated into the cellular part of the head of the thigh bone; the small bones of the toes were quite full: but it had not entered into all the metatarsal bones. From experiment it has been found that bones and flesh being boiled in common pitch, it will pervade the substance, and fill the cavities of the former: and the latter will be so impregnated with it, as to be reduced to a uniform black brittle mass; not in the least resembling flesh. This treatment however will not account for the state in which this mummy was found; for if the flesh had not been previously removed, though its appearance would have been entirely changed, yet the filleting could never have been found in contact with the bones. From this last circumstance it is most likely that the body, excepting the feet, had been reduced to a skeleton before it was laid up; it is also pretty certain that it must have been kept some time in boiling pitch; both before and after some of the layers of the innermost filleting were laid on. The feet seem to have been swathed, at least in part, before they were committed to the hot pitch: and this seems to have pervaded the bandages, the flesh and the bones.

It has been imagined, that the principal matter used by the Egyptians for embalming, was the asphaltus; but what Dr. H. found, was certainly a vegetable production. The smell in burning was very unlike that of asphaltus; nor did it resemble that of the common pitch of the fir-tree; being rather aromatic. It was compared with a variety of resins and gum-resins; but it seemed not to resemble any of them, excepting myrrh; and that but very slightly. In all probability it was not a simple substance; but might be a mixture of the resinous productions of the country, with the pitch of that tree which they had in greatest plenty.

The Αλειφαρ τυ Κεδρυ of Herodotus,* and the Κεδρια of Diodorus Siculus,† was most probably the tar of the cedar; it is the substance said by these authors to be used for embalming; Galen‡ mentions its power of preserving bodies; and §Dioscorides calls it Νεκρυ ζωη. Pliny, speaking of the cedar, says that the tar was forced out of it by fire; and that in Syria it was called cedrium: cujus tanta vis est, ut in Egypto|| corpora hominum defunctorum eo perfusa serventur. Some branches of the cedar were procured from the physic garden at Chelsea; and, being treated in the manner described by Pliny, yielded tar and pitch, which had no aromatic smell, and seemed in many respects similar to the produce of the fir-tree. There must undoubtedly therefore have been some other resinous matter mixed with the cedrium.

The pitch of this mummy was carefully distilled, but it gave no other produce than what might be expected from a resinous body; the caput mortuum, when burned and elixated, yielded a fixed alkali; to this may be attributed the moisture which the pitch, that was in contact with the spine, and those other parts which were most burned, contracted on being broken and exposed to the air; for this pitch had an alkaline taste, and had been more than melted, having been burned to a caput mortuum. A great variety of experiments were made on this pitchy matter; the result of them all tended to prove, that it had not the least resemblance to asphaltus, but was certainly a vegetable resinous substance.

Mons. Rouelle, in the Memoirs of the Royal Academy of Sciences for 1750, has given a very elaborate and ingenious treatise on embalming; in which he has chemically analysed the pitch of 6 different mummies. From his observations, and from what Pietro della Valle,¶ and Joannes Nardius** at the end of his edition of Lucretius, have written on this head; from what Dr. Middleton†† observed in the mummy which was opened at Cambridge; from the Memoires of Count Caylus, in the 23d vol. of Acad. des Inscript. et Belles Lettres; and from this present examination; it appears that various methods of embalming were practised among the Egyptians; and that they used different materials for this

* Herodot. Euterpe, pag. 119. ed. Gronov.—Orig.

† Diodor. Sicul. lib. i. p. 82. ed. Rhodomanni.—Orig.

‡ Galen. de Simpl. Med. Facult. lib. vii. cap. 16.—Orig.

§ Dioscorides de Mat. Medic. lib. i. cap. 105. pag. 56. Francof. 1598.—Orig.

|| Plin. Histor. lib. xvi. cap. 11. pag. 322. ed. Dalecamp.—Orig.

¶ Viaggi di Pietro della Valle, Tom. 4.—Orig.

** Lucretii Joannis Nardii de Funeribus Ægyptiorum Animadversio 50, p. 627. These accounts of della Valle and Nardius are also to be met with in the 3d vol. of Athanas. Kircher's Oedipus Ægypt.—Orig.

†† Middleton's works, vol. 4, Germana quædam Antiquitatis Monumenta.—Orig.

purpose; and though Herodotus and Diodorus Siculus have given reason to expect to find the bodies in a much more perfect state than we ever meet with them; yet on the other hand it is evident, from the foot of this mummy which Dr. H. examined, and from the account Mons. Rouelle and Count Caylus have given in the above-mentioned Memoires, that all the fleshy parts were not always previously destroyed.

II. The Sequel of the Case of Mr. Butler, of Moscow, printed in Phil. Trans., Vol. L. By Dr. Mounsey. Communicated by Mr. H. Baker, F.R.S.* p. 15.

In my former account of Mr. Butler's case, says Dr. Mounsey, it is said, that he had recovered his perfect health and strength: yet after that he was often subject to ailments of the nervous kind, and became sensibly affected not only by the smell of paints, but even the handling of some kinds of metallic inodorous bodies gave him anxiety, tremor, faintings, and many other uneasy symptoms. The handling of verdigris, vitriol, and the like, threw him into these disorders; and he asserted that the handling of copper or iron had the same effect on him. I often heard his complaints; but as I deemed them imaginary, or sensations raised by the apprehension, I only strove to undeceive his fancy. However, I began to see, by some accidents, that there was more reality than I had believed, and that his first accident had left a disposition of the body susceptible of such impressions.

One day having got home a box of cerussa, he took out some lumps to examine the quality, and handled them without the least suspicion of harm; but in a few hours after, he was taken with anxiety, palpitation of the heart, and a sense of trembling and weakness of the whole body. He was obliged to go to bed: he took some spirit of hartshorn, sweated most plentifully, and next day was recovered. Many things of this sort happened to him: but I shall only give you an account of the most extraordinary attack which happened to him June 26th, 1758.

Mr. Butler still wanting to make experiments, but not daring to meddle with the operations himself, directed his wife to make some compositions of blue vitriol, alum, quick-lime, burnt alabaster, and things of this kind. They were boiled in 6 several pots, then let stand some time, and the thin or watery part poured off. She brought these pots to her husband to look at; he was fond to try the colours himself, and without any apprehension he took some of those precipitations out of each pot, with the middle finger of his right hand, and rubbed them on grey paper to try the colours. He then put them away, and thought nothing more of the matter, drank tea, and was very well till about 3 hours after. He then began to be uneasy, and found pain in his arms, and espe-

* Vol. x, p. 66 of these Abridgments.

cially in his right hand, he became sick at stomach, and felt a trembling over his whole body. He strove to get the better of this attack, and walked slowly about for some time, but turned pale, faint, and fell down. He soon recovered again, and still thinking to master the illness, drank 2 or 3 glasses of wine, which he vomited up again. This began at noon, and at 6 in the evening I found him in bed frighted and sweating. His pulse was then regular, but quick; he was sick at stomach, with anxiety. I ordered him some saline draughts, and plenty of thin warm liquors. In the night he slept but indifferently: his complaints were not continual, but returned by fits, with stretchings of the limbs, tremor, and starting of the tendons over the whole body; and when he began to slumber, he was disturbed with frightful dreams of fire.

27th, Early in the morning he observed many small purple spots on his hands. I found them just like purple petechiæ: the most on his right arm, and perceptible through the thick skin of the palm of that hand. There were also some on the other arm and legs, and some of a deeper colour on the thighs, but very few on the rest of the body. As his pulse was now grown quicker, I suspected this to be a petechial fever: but there being no fevers of that kind then in town, and besides, as I could not reconcile the other circumstances, I remained undetermined, and much perplexed by all these appearances. About 4 in the afternoon he was again seized with great anxiety, and pricking burning pains in the feet: the toes were extraordinarily red, and he had frequent stretchings. These went almost off in a few hours. 28th, He was not so much troubled with the frequent returns of his complaints: his pulse was quick, and the spots kept out with itching. 29th, He was much the same as yesterday, only more cheerful in the intervals, and the spots appeared fewer. He got frequently out of bed and walked in the room. 30th, The attacks returned much seldomer, and he would not keep in bed, but walked a great deal about the room, though his pulse was still feverish. Many of the spots disappeared, most were become pale, and some of a dun hue: those on the palm of the right hand were almost gone. He said the spots were always fairer every time the fits returned, and then he felt pricking pain with great heat, especially on the inside of his arms and legs, and in his feet and toes.

July 1st, I found him walking about the room, his pulse still quick. Last night he had been pretty easy and free. The spots were pale and disappearing. He took a laxative, which operated very well. 2d, Last night he got pretty good rest, but this morning the prickling and tremorous sensation over the whole body returned, but did not last long. He afterwards got up, walked about, and looked after his business. The spots were mostly gone. He observed, that the pricking pains in his arms and legs, and in a large spot on his back, which troubled him in all his former accidents, came now only in the forenoons, and then almost ceased for the rest of the day. 3d, Every thing much the same, but the attacks were lighter. 4th, Very little difference; only now and then he was troubled with a glowing painful sensation immediately under the skin, sometimes in one part of the body, sometimes in another, a spot about the size of a crown piece. 5th, Things much the same. 6th, The attacks slighter, with the same feelings. 7th, Very little change, but rather better.

Hitherto I was mostly an observator; and, not being forced by an absolute necessity, I did not chuse to load him at an uncertainty with many drugs. I had given him little more than absorbent nitrous powders; but now, as he had no fever, but was rather lax and weak, and his nervous system affected, I thought I might begin to give him things more powerful, and therefore ordered some pills composed of extr. cort. Per. myrrh. g. ammon. and sal. martis.

Here, in prescribing, I had attention to the antipathy nature had shown to iron, therefore took care the quantity in each dose should be very small, the sal martis scarcely making 2 gr. The 8th, he took a dose this night, was very restless, and greatly affected with all the former symptoms. 9th, He said he felt this medicine struggling with the distemper within him; so he swallowed, though with great reluctance, another dose early in the morning. In less than 3 hours he was again taken very ill, with anxiety, a sense of trembling over the whole body, and as if prickling sparks were flying out every where. He begged me to change this medicine, and said it was like to have killed him. Having heard all his complaints, I made the pills be put away, and promised he should have no more of them: but his fear and aversion were so great, that the moment I was gone he ordered the box to be taken out of the house and thrown quite away. 10th, He passed this night tolerably, and found himself much better in the morning: but the complaints came by turns as before.

From this till the 20th, I gave him sundry medicines, but with little more effect than to ease him now and then; for the complaints always returned again in different manners and at uncertain times: but nothing extraordinary happened. On the 20th, I gave him a dose of Epsom salt, which he had been used to take: it purged very well; but immediately on its leaving off to work, his body struck out with great numbers of small red spots, without other inconvenience than a little extraordinary heat in the skin. 21st, The spots were almost gone, and he found himself more cool and easy than before. 22d, He took another dose, and the spots returned in the same way more than the first day; he found also the same relief. After this he took more doses of the same salt, always intermitting a day or two. The spots returned, but every time fewer appeared; and at last none appeared on taking these salts. This sal cathartic. amar. came from England, and whether some vitriolic acid had been used in making it, I do not know; but it is likely there had.

From the first of August he took no medicines; the attacks were much less frequent and slighter; only he often felt in the night-time like the stroke of an electrified body. August 13, he was awaked this night by pain, as if burning irons had been clapped to the insides of his legs, with anxiety and a sense of tremor over the whole body. I found his pulse very quick, irregular and small; but I could find no new cause for the return of his complaints. He had after this some smaller attacks: but in the night of the 23d he was seized with a violent fit of the same sort, with stretchings, and as if prickling sparks were flying continually out of the skin. He had palpitation of the heart, and complained of the want of breath: his left side turned cold, and his right side became hotter. 24th, He was again attacked in the same manner in the night-time, and it also went off in the same manner; but he now became feverish, and kept his bed some days.

By the word stretchings, I mean the stretching of his body and limbs by a slow and gentle convulsion of the extensor muscles; for in all the attacks I never observed the flexores any way affected. His feelings were frequently so odd, that he said he could not describe them. He often felt as if his left side, from his head to his waist, was empty; and that millions of small bodies were driven up and down with great velocity: which he likened to the shaking of peas in a bladder. I tried many kinds of remedies to rid him of this disorder. He often found relief from them, but the complaints returned again. The remedy I found the most effectual, was putting him on a milk diet, and making him drive hard on a cart every day, forenoon and afternoon, which he continued to do for several weeks. His complaints all decreased; and when he was threatened with an attack, a few drops of spirits of hartshorn and lavender, or the like, were now of service to him, which formerly had no effect. In short, I gave him again animal food, and he kept his health pretty well.

The first year after this he was always fearful; and often complaining, of what appeared to me small things; but by little and little he got the better of these also. Though he always continued to avoid handling metals, minerals, or things painted with these bodies. When I left Russia, he was very well; and I have lately heard by a letter that he continues so; and I believe he observes the same circumspection about paints and metals as before.

III. The Description of a New and Safe Crane, which has Four Different Powers; invented by Mr. James Ferguson, F.R.S. p. 24.

This paper may be more advantageously consulted in the supplement to the author's book of Lectures, p. 3.

IV. Of the Moon's Distance and Parallax. By P. Murdoch, D. D., and F. R. S. p. 29.

The following contains an easy rule for determining the moon's distance, from the received theory of central forces.

Sect. I. Sir Isaac Newton investigated the law of gravitation, in the duplicate ratio of the distance of the central body inversely, from the following data. 1. The length of a simple pendulum which vibrates in one second of time, gave him, by Huygens' theorem, a determinate measure of the force of gravity, at the place of observation. And, by his own theory, he could thence infer the like measure for any other place, of a given latitude. 2. The earth's semidiameter was computed from the Abbé Picard's measure of a degree of the terrestrial meridian. 3. The moon's parallax as determined by the most skilful astronomers, gave him the moon's distance in semidiameters of the earth. 4. The time of a periodical month gave him the ratio of the versed sine of the arc of the moon's orbit which she describes in one second, to the radius.

And from these his conclusion was: that the gravitation at the earth's surface, being diminished as the square of the distance from the earth's centre increases, would, at the distance of the moon, produce a fall from rest, in one second, precisely equal to that versed sine. Or, that the gravitation of the moon toward the earth, being increased as the square of that distance is diminished, would at the earth's surface, be of the same quantity as that of falling bodies is (by the experiment of the pendulum) actually found to be.

II. But the law of gravitation, thus deduced, being found to hold universally, and reciprocally, among all the great bodies of our system, so that even the minute anomalies of their motions are explained from it; we may now assume it as given, and make the moon's distance the quantity sought. Thus, writing F for the number of feet which a body falling from rest describes, in vacuo, at the equator, in one second; V for the versed sine of the arc of the moon's orbit described in the same time, to the radius unity; D for the semidiameter of the equator in feet: and the ratio of the distance of the centres of the earth and moon, to the semidiameter of the earth, that of X to 1: we shall have, by the general law, the moon's fall in 1^s, equal to $\frac{F}{X^2}$; but the same fall is equal to $V \times D \times X$; whence $X^3 = \frac{F}{V \times D}$, and $X = \sqrt[3]{\frac{F}{V \times D}}$ is the distance sought, in semidiameters of the equator.

Now a simple pendulum which beats seconds, measuring at London 39.126 inches; if the usual allowance is made for the weight of the air, and for the Newtonian figure of the earth, the weight ($\frac{1}{289}$) taken off by the centrifugal force being likewise restored, a second pendulum at the equator would be 39.154 inches long. And, by Huygens' rule, half this length is to the initial fall in one second, in the duplicate ratio of the diameter of a circle to its circumference; that fall therefore at the equator, and in vacuo, is 16.10185 feet; the logarithm of which number is 1.2068645 = log. F.

The toises in a degree of the equator, or, which is the same, in a degree of the meridian at lat. 54$\frac{3}{4}$, being nearly 57200, the logarithm of the number of feet English in the semidiameter of the equator,

that is log. D will be nearly 7.3211900,
And the log. versed sine of the moon's arc in 1 being . . . . —12.5492882,
Their sum . . . . — 5.8704782

taken from log. F, leaves + 5.3363863, a 3d of which is 1.7787954, the logarithm of X = 60.08906 semidiameters of the equator. And the arithmetical complement of this last logarithm, which is — 2.2212046, is the log. tangent of the moon's mean horizontal parallax at the equator; which therefore is 57′ 12″.34.

III. Such would be the distance of the earth's and moon's centres, were the earth immovable; but it is somewhat increased by their revolution round their common centre of gravity. Writing $x + 1$ for that distance, divided by the centre of gravity in the ratio of x to 1; imagine a sphere of the same dimensions as our earth, placed at that centre, to exert the same attractive force on the moon as our earth actually does, the periodic time remaining unaltered: then must the density of this sphere be diminished in the ratio of x^2 to $(x + 1)^2$, that its nearer distance from the moon may be compensated by the defect of density and attractive force. If now an inhabitant of the fictitious earth were supposed to compute its distance from the moon, in the manner just now shown; the quantities V and D would be the same as in the former calculation; but his f would be to our F, as x^2 to $(x + 1)^2$, and thence his x would be to our X as $x^{\frac{2}{3}}$ to $(x + 1)^{\frac{2}{3}}$, that is, $X = (\frac{x}{x+1})^{\frac{2}{3}} \times X$.

This is the distance from the fictitious earth, or from the common centre of gravity; but (T) the distance from our earth, is $\frac{x+1}{x} \times (\frac{x}{x+1})^{\frac{2}{3}} \times X$, greater, as was supposed, in the ratio of $x + 1$ to x; that is, $T = \sqrt[3]{\frac{x+1}{x}} \times X$.

Sir Isaac Newton, from the phenomena of the tides, estimated the ratio of $x + 1$ to x to be that of 40.788 to 39.788. In that case, the cubic root of $\frac{x+1}{x}$ will have for its logarithm 0.0035934; which added to 1.7787954, the logarithm of X computed from an immovable earth, gives 1.7823888, the logarithm of 60.5883 semidiameters of the equator. And the moon's horizontal parallax for this distance, is 56′ 44″.07.

IV. On the other hand, if we had observations of the moon's parallax, and distance, which could be reckoned exact enough for the purpose, we might thence determine the ratio of x to 1, that is, the ratio of the quantities of matter in the earth and moon. For having $\frac{T}{X} = \sqrt[3]{\frac{x+1}{x}}$, and $\frac{T^3}{X^3} = \frac{x+1}{x}$; also T being given from observation, and X computed as above; it is manifest that the ratio of $x + 1$ to x, and, by division, that of x to 1, or of the mass of the earth to that of the moon, is given. For example, if it should be concluded from good observations, that T, the moon's mean distance, is $60\frac{1}{3}$ semidiameters of the equator; from the logarithm of this distance, which is 1.7817554, take the logarithm of X, or 1.7787954, thrice the remainder will be 0.00888, the logarithm of $\frac{x+1}{x}$, = 1.02066: and the masses of the earth and moon would, on this supposition, be as 48.4027 to 1.

In all this, a small variation from the law of attraction, arising from the spheroid figure of the earth, is neglected as inconsiderable; which it will be found to be by whoever takes the trouble to compute its quantity and effects.

Remarks.—1. If F and D were taken of their just quantities, the moon's horizontal parallax for an immovable earth being, at the equator, 57′ $12\frac{1}{3}$″, is a limit which the true mean parallax cannot exceed: and the correspondent distance 60.08906 is a limit which the distance cannot fall short of: both being computed on the supposition that $x + 1 = x$, or that the matter of the moon is as nothing in comparison of the earth. Nor can the parallax and distance be supposed to lie very near these limits, without leaving too little attractive force in the moon to raise the tides.

2. If the moon's mean apparent semidiameter be 15′ $38\frac{1}{4}$″, and the distance of the centres 60.5883 semidiameters of the equator, according to Sir Isaac's estimate of the masses; the semidiameter of the moon will be 0.275601 parts of the semidiameter of the equator, or .2763 of a mean semidiameter of the earth. And the magnitudes of the moon and earth being as the cubes of their semidiameters, if the inverse ratio of their magnitudes be joined to the direct ratio of their masses (1 to 39.788) the sum will be the ratio of their densities, that of 1.19143 to 1, a little less than 6 to 5.

3. Supposing still the same semidiameter of the orbit as before, the force of gravity will be to the earth's attractive force on the moon, as 3670.94 to 1, and to the moon's force on the earth as 40.788 times that number, or 149730.4, to 1. Again, the force of the moon on that surface of the ocean to which she is vertical, being to her force on the earth's centre, as the square of 60.5883 to that of 59.5883; and the difference of these squares being to the latter as 1 to 29.54623; this difference of the forces will support the weight of $\frac{1}{4423968}$ part of the water at the vertex. And because the earth's semidiameter is small in comparison to the moon's distance, the like differences of force will decrease from the surface to the centre, nearly in an arithmetical progression, as the

weight of the water does; making the case analogous to the diminution of gravity by centrifugal force.

But it is likewise easily shown, that half this quantity of lunar force exerts itself to depress the waters all around at the distance of 90 degrees from the vertex; $\frac{2}{3}$ therefore of the former fraction, that is $\frac{1}{2949312}$ part of the force of gravity, will be the total cause of the difference in height of the flood and ebb, in an open and boundless ocean. Say therefore, if (in determining the figure of the earth) $\frac{1}{289}$ of gravity, suspended by the centrifugal force, gave, for the difference of diameters $\frac{1}{231}$, what will $\frac{1}{2949312}$ part give? and the answer, in feet, will be 8.887.

4. In like manner, if we take $8\frac{3}{4}''$ for the sun's parallax, and thence his distance from the earth 23468.6 semidiameters of the equator; we shall find that his whole force to produce a difference of flood and ebb, is to his force at the earth's centre, as 1 to $7823\frac{2}{3}$. But the sun's distance being to the radius of the moon's orbit as 387.34535 to 1, this last force will be to that of the earth on the moon, as 387.34535 to 178.7234 (by cor. 2, prop. princip. 1). And the earth's force on the moon is to gravity as 1 to the square of 60.5883; whence, adding these ratios, the sun's force to move the sea, will be to the force of gravity, as the fraction whose logarithm is — 8.1778026 to 1; or gravity is to that force as 13249445 to 1. And therefore, by the same analogy as before, we find the difference of flow and ebb, from the sun alone, to be 1.97824; 1 foot $11\frac{3}{4}$ inches.

The solar force therefore, in raising the tides, is to the lunar, as 1 to 4.4924, in a ratio somewhat less than that computed by Sir Isaac. The ratio likewise of the sum of the forces to their difference, is but 7.869 to 5, instead of 9 to 5, which he assumes from comparing the spring and neap tides at Bristol. And it is indeed surprising how he could, from that datum, arrive at conclusions so near the truth, as his very probably are. He tells us he used the ratio of 9 to 5, only till a more certain could be procured. And therefore the foreign mathematicians, who have censured him on that head, and on some other articles of this doctrine, might have spared their reflections; at least till they could show that their own deductions were more agreeable to nature and observation.

6. Unity representing the force of gravity, d the sun's distance, the earth's force on the sun will be $\frac{1}{d^2}$, or the fraction whose logarithm is — 9.2590218. And the solar force on the earth is (from the numbers in remark 4) to the force of gravity, as 1 to 1673.1: whence the attractive forces (and masses) of the sun and earth will be as 325172.3 to 1. Add to this the inverse ratio of their magnitudes, collected from the sun's mean apparent semidiameter 16′ 6″, and the parallax $8\frac{3}{4}''$: and the density of the sun will be to that of the earth, as 1 to 4.068.

All this on the supposition that the masses of the earth and moon are as 39.788 and 1. Hereafter, when the moon's distance shall be more certainly known, that element may be corrected, and the operations repeated. As to the sun's parallax $8\frac{1}{5}''$, it cannot be much affected by any future determination of the moon's distance. Nor is it here assumed of that quantity, at random; but from a theorem deduced from the established principles. I am, however, too diffident of myself to communicate it at present: because, though it agrees very well with Mr. Short's conclusion from the transit of Venus, it differs considerably from that which a very learned and justly celebrated author has lately published. —Note, The periods, as assumed in this paper, are: a sidereal year of 365.2563923 days: the periodical month 27.32165835 days.

V. An Attempt to Account for the Origin and the Formation of the Extraneous Fossil commonly called the Belemnite. By Mr. Joshua Platt. p. 38.

The public has of late been agreeably entertained with descriptions of many curious fossils discovered in different parts of this kingdom; but very little has been offered with a view to ascertain their origin and formation; a point of much greater importance to a curious mind, than the most accurate descriptions, or the neatest delineations. It may indeed be thought unnecessary at this time, to say any thing of the origin of extraneous fossils in general; all our modern naturalists being fully convinced, that they are the exuviæ or remains of animals and vegetables, and the greater part of them of marine production. But as to their particular origin and formation, in what manner they were produced in the recent, and how and with what matter they afterwards became impregnated in their fossil state, all this is a field of natural inquiry, that has been very much neglected, though it is the most fertile and productive of useful and entertaining knowledge. Besides, considering it in this view, the recent and fossil remains would be found to throw a mutual light on each other, and the naturalist would not be so often at a loss to class every new fossil acquisition, of which the recent specimen is not to be found, especially whenever the fossil has any thing seemingly equivocal in its formation, so as on a superficial inspection to render the matter doubtful whether the body belongs to the animal or vegetable kingdoms, or indeed to either of them.

One of the first note is the belemnite, which has not until very lately been even ranked among the marine productions; but whose origin and formation have never yet been fully explained. Not to enter into a minute detail of the several species of the belemnite, the history of this extraneous fossil, or an attempt to account for the origin and formation of it, so far as they can be discovered and confirmed by reasonings drawn from facts and experience, is the object of the present inquiry. Mr. P. confines himself to two species of the belemnite; the one common in most counties of this kingdom, and vulgarly known by the name of thunder-bolt (pl. 3, fig. 1): the other that of the fusiform or spindlekind, (fig. 2) found in slate-stone at Stons-field, but in far greater plenty in the clay near Piddington (fig. 3) Oxfordshire, and in the chalk-pits of Kent and Surrey, (fig. 4). Those in chalk have been often mistaken for spines of the sea-hedgehog, or echinus ovarius; but the characteristics of these two bodies are widely different. The belemnite breaks in a direction perpendicular to its axis, (fig. 5): the spine obliquely (fig. 6). The belemnite, when broken, exhibits central rays; the spine a smooth resplendent surface. This distinction is invariable, if the trial be repeated a thousand times. These different appearances are probably the effects of different formations: and therefore the belemnite seems to be formed by apposition, and the aculeus or spine by protrusion, or, as Mr. Reaumur calls it, by intus-susception. The radii in the belemnite are owing to the fine laminæ of which it is composed; they are so very thin, and break so nearly alike, that they have ever an horizontal surface when broken, which is common to all the shells of the trichite kind.* The spine being formed by protrusion, its component parts are adjusted in a different manner, and the pores, like the cancelli in bones, (though not so distinct) are irregular, which is the reason of its breaking obliquely in any direction, but it is generally smooth by being saturated with a plated kind of spar.†

Mr. Brander, in a dissertation on the belemnite, presented to the R. S.,‡ justly observes, "that the belemnite belongs to the testaceous part of the animal kingdom, and to the family of the nautili." And it may be further added, that this gentleman's sentiments are greatly strengthened by the surprising analogy which the belemnite bears to the little pearly concamerated shell, or cornu ammonis; and the orthoceratites, to the large nautilus; the former having its siphunculus on the verge, as the latter has it in the centre of the diaphragm, or partition of each cell or chamber. "It has indeed been truly matter of speculation, continues Mr. Brander, how this huge solid substance called the belemnite, exclusive of the nucleus, could be formed; and how it happens, that some belemnites 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."—But Mr. P. thinks it will be found on inquiry, that these are only circumstances which are common to other testaceous bodies, that have been ac-

* Fig. 7. A piec of the penna marina, perforated by the pholades.—Orig.

† Spar seems to be nothing but chrystal debased by a calcareous earth; the more debased sort breaks in a hairy trichite manner, the more pellucid kind with a smooth surface; and always in an oblique rhomboidal direction; which perhaps may in some measure enable us to account for its double refraction.—Orig.

‡ Philosophical Transactions, vol. xlviii. for 1754, page 803.—Orig.

cidentally broken or decayed by time, when forsaken by their inhabitants. For no testaceous body can be formed without an inhabitant; nor does it appear that any belemnite was ever formed without an alveolus, or concamerated shell.

The conical cavity and its nucleus are always proportioned to the bulk of the belemnite, but not to its length: some are 4 times longer in proportion to the alveolus than others. The apex of the conical cavity, where the alveolus is first formed, in some runs up about half the length of the whole belemnite; in others not a 6th part of the whole, (fig. 8): but the aperture, or upper chamber (fig. 13, *b*, *c*,) is equally proportionable to the bulk or circumference of the belemnite, of whatever size or shape; and is the seat* or dwelling-place of the animal that forms the belemnite. In what manner this work is executed, we shall now endeavour to explain. A considerable part of marine bodies, especially those of the testaceous tribe, are generally buried in mud or sand, except some few which stick to rocks, &c. as the limpets and periwincles; by which means we are prevented from making those remarks on the several stages of their growth, which an accurate inquirer would desire. We must therefore have recourse to the different steps or periods of their life and growth, as they are marked out by the indented lips or foldings of the shell; till they arrive at their full size; when they begin to fortify themselves by bulwarks and strong holds, against the injuries and incidents which attend old age. This is most conspicuous in the cowree, or concha veneris of Lister, book iv. sect. 9.

Mr. Reaumur † found, by repeated experiments, that land snails form their shells by juxtaposition: as the animal grows in bulk, the shell is increased by a mucous matter emitted from the body of the animal, which hardens by degrees into a testaceous substance: and from the experiments on land shells that great naturalist concludes by analogy, that all testaceous bodies are formed in like manner, particularly those of the turbinated kind. To this general rule an objection is made by Mr. Poupart, from the formation of the cowree, or concha veneris before mentioned: but this learned gentleman was not aware that this shell is first a buccinum, forming many convolutions before it draws in the verge to form the indented lip.

It was this very objection of Mr. Poupart which led Mr. Platt to examine into the growth of the cowree; and by sawing one of them through the middle, he found a turbinated shell within the outer wall, consisting of 6 or 7 convolutions, but no stages, or periods, of the indented lip appeared in any of the convolutions, as we find in the helmet shell, and several of the buccinæ. He then began to consider how this animal enlarged its dwelling; and was fully convinced

* We never find a belemnite with part of the alveolus, but the vestigia or marks of the remainder appear in the cavity, and are continued to the verge of it.—Orig.

† See his book of insects.—Orig.

that no more convolutions could be carried on; the indented lips being a full stop to its inward dimensions; and that here was the period of its growth. His sentiments were just as to its inner dimension; but observing that the lips of some were much larger than others, and that the curved part of the outer lip appeared thicker, when sawed open, than the other parts of the shell; he began to think that the animal, instead of enlarging the inner dimensions, was employed in thickening the outer wall, to guard against injuries and accidents, so common to the inhabitants of that turbulent element the sea. He was the more confirmed in these sentiments by seeing the beautiful spots with which this animal decorates its house, covered by other spots of different colour and size, as new laminæ were added to strengthen the last-formed convolution. It is really matter of admiration to see how these shells are adorned and variegated; the exquisite polish which covers the whole infinitely surpassing the skill even of the most accomplished human artist. These new coverings or laminæ, which are carried from the lips, terminate in the middle of the back part of the shell; and there form a list, or seam, of a quite different colour from that of the other part of the shell, and of an unequal surface.

This circumstance gave birth to Mr. P.'s sentiments concerning the formation of the belemnite: for whoever considers the seam or fulcus in the belemnite, will be apt to conclude that the outward lamina is formed latest, as in the cowree, and that the seam or fulcus is caused by the several additional coverings or laminæ terminating there. But as the anatomist makes fresh discoveries by dissecting the subject, so he received further information by luckily meeting with a belemnite, whose laminæ were in a manner dissected and laid open by the vague acid, or some other corroding menstruum, which every where pervades the earth, destroying some bodies,* and forming others.† The laminæ of this truly wonderful body are here exposed to view, fig. 9, and plainly show us, that nature in this, as in all her works, pursues the most simple, easy, and shortest methods, though they appear ever so intricate and interwoven. This specimen will serve to explain a matter which has so long puzzled the curious in natural history; and convince us that there is nothing more wonderful in the formation of the belemnite than in that of a cockle, oyster, or any other testaceous substance; with this difference only, the oyster strengthens its shell, and excludes its first habitation by additional laminæ formed within; the belemnite incloses its dwelling by adding new laminæ without. Fig. 10, represents the belemnite split up the middle, with the siphunculus in the front: a, b exhibit the first formed cell, or seat of the animal ab ovo. As the animal grows

* All calcareous substances.—Orig.

† Such as selenites, pyrites, marcasites, talc, gypsum, &c.—Orig.

larger, it forms a second cell or chamber b to c, at the same time covers the first cell by forming the appendage or guard c, i, which is the first stage of the belemnite. In forming the third cell c, d, fresh laminæ or coverings are carried on from d to k; and so of the rest, e, f, g, h; or l, m, n, o. When we have duly considered the manner in which the shell is thus formed; it will be no difficult task to account for the different sections and broken parts of the belemnite, and in what manner they were reduced to the several forms or appearances in which we commonly find them.

The better to illustrate his conjecture, Mr. P. first exhibits some drawings, which show the several specimens broken and imperfect; and then proposes his sentiments concerning them before they were deserted by their inhabitants. Fig. 6 shows the spine of the echinus ovarius broken obliquely, as is common to all of them. Fig. 5 exhibits the inner structure of the belemnite, when broken horizontally, with the central rays. Fig. 11 is the same belemnite split through its axis. Fig. 12 and 13, are broken in the same direction as fig. 5 and 11, and show how the several laminæ* are placed one over another, in the manner in which it is formed. Fig. 14 shows the belemnite in the most perfect state we ever find it. Fig. 4 is the fusiform belemnite found in chalk, which has been often taken for a spine: a, which is the termination of the conical cavity, has been thought to be the socket of the spine, which receives the papilla, when growing to the echinus; but, when compared with the socket of the true spine (fig. 6, *b*) we find it widely different. The pricked lines b, c, b, c, show what the fusiform belemnite was, when perfect, with the alveolus d, e. Many of those found in chalk seem to be somewhat injured at the end a, where they are deficient, and are rounded, but have an uneven surface, as if they had been gnawed or eaten by the pholas. Those found in clay near Piddington, (fig. 3,) Oxfordshire, approach nearer to the fusiform kind, and have a different appearance at the smaller end f, where the laminæ are reduced to a white impalpable powder, by corrosive juices in the earth, so as to stain the fingers when first taken out; and they afterwards retain a white chalky appearance: but, among a great number, he never found one that was 3 inches long. These have suffered in the same manner as fig. 4. Fig. 3, f, shows where the alveolus terminates: g, h, g, h, how much has been destroyed by vitriolic acids.† At

* These distinctions of the laminæ appear to be owing to the mineral steams insinuating themselves into the belemnite, when the spar pervaded the pores, and destroyed the texture, but retained the true form by substituting itself, and filling the plasm or mould of the belemnite.—Orig.

† It may be asked, why one part suffers more than another, as all parts are homogeneous, and free from extraneous mixtures? My answer is; because those parts, where the concamerated shell is lodged, are much thinner than the other parts of the belemnite; and consequently the walls are more easily broken down, and the alveolus, being still less solid, is sooner destroyed, and reduced to an impalpable powder, by vitriolic and other acids, which the water takes up as it passes through

Stons-field they are found much longer than at Piddington, and are enclosed in stone, which is split by the workmen to make slates. Here they are often found in a much more perfect state, (fig. 2,) than the former, with the alveolus in many of them; but that part is commonly crushed (ibid at y) by the incumbent matter.

The siphunculus of the belemnite is always on the verge of the chamber, or cell; and in the siphunculus is a little gut or duct, proceeding from the body of the animal, by dilating or contracting of which, the animal, it should seem, may go out or into its cell at pleasure. This is the only stay which the animal has to secure its retreat: but he cannot agree with the learned doctor Hooke,* that the gut or duct passes through all the cells to the end of the spiral cone, either in this shell or the nautilus. His discovering of a spiramentum in the centre of the latter was merely conjectural; for the ends of the spiral cone of concamerated shells† are shut up in the same manner with those of the turbinated kind; and it is common for all turbinated shell-fish as they increase in bulk, and enlarge their shells, to leave their bottom or first-formed convolutions. Therefore Mr. P. makes no doubt but the same is done by the concamerated tribe; for if the gut go through only one or two valves, it will be a sufficient stay to the animal, and, being contracted or dilated, will serve all the purposes above mentioned. How far this is practicable by our little inhabitant, cannot absolutely be determined; but if it be constantly fixed by the gut to the siphunculus, it has a surprising power of contracting and dilating its body, to extend so far as the bottom or point of the belemnite, which in some is more than 30 times the length of the cell into which it returns (fig. 8). He thinks that this gut or duct, as well as the body of the creature, is capable of being extended very considerably to serve all the uses of forming the belemnite, without leaving the siphunculus; and that the gut serves for the same purposes with the tendons of the oyster; the latter to open and shut the shell; the former to allow the animal to go out and in at pleasure. And as the oyster feeds altogether in the shell, by opening the verge, the belemnite (whose residence is in the great deep, which is seldom disturbed) very likely goes out in quest of food, but travels only on the guard or rampart, leaving a trail behind, as all land snails do; which hardening into a testaceous substance, increases the dimensions of the outer

different strata, abounding more or less with pyritical matter. Where no spar follows the acid, the parts are carried away and lost in the interstices of the earth, and a mould or plasm is left, which Steno calls an aerial shell. See his Prodromus, page 84. But where the spar abounds, it pervades the whole substance, fills up the cavity, and assumes the true form of the shell; and sometimes, by bursting the pores, is so far substituted in the place of the original particles, that the several diaphragms, with the siphunculus of the alveolus, are accurately and nicely preserved.—Orig.

* Hooke's posthumous works published by Derham, 8°· p. 306.

† See the little pearly cornu-ammonis shell.—Orig.

walls, both in length and thickness, from the cell or chamber, to the bottom or point of the whole belemnite. The animal, in its progress and return, clasps the whole guard, as a snail does a small branch of a tree in the gardens; and where the two sides meet, there the sulcus is formed, as is evident from the laminæ in fig. 9.

The belemnites, like all other testaceous bodies, have the vermicular tribe attached to them, and are perforated by the pholades. Other marine bodies also affix themselves to the belemnites, oysters in particular: but this never happens while the animal inhabits the shell, because the new additional laminæ would so cover the affixed body, and also the cells of the pholades and vermiculi, that they could have no communication with the water, and must consequently perish. These bodies, thus attached, are the strongest proof we can desire, that the belemnite is of marine production. Indeed it may be objected, that the bones of quadrupeds, wood, and stone have these bodies adhering to them, and therefore may be said to be marine as well as the belemnite. But when we bring them to chemical trial the objection vanishes; for the bones either come out of the furnace with a black core, or they are reduced to ashes; whereas the belemnite is changed into a fine calx, after the manner of all testaceous bodies, and is converted into a species of phosphorus.* The oysters, having no locomotion, frequently affix themselves to other bodies, that they may be better able to stem the tides and currents which might otherwise carry them from their proper beds and places of feeding. This attachment to other bodies no way incommodes them, because they increase the dimensions of their shells by adding fresh laminæ inwardly: the first formed laminæ being as it were excluded, lie in the manner of tiles upon the roof of a house, and exhibit the several steps or stages of their growth.

Mr. P. believes a belemnite is very rarely found perfect in the fossil state: those in gravel-beds (fig. 15) have suffered very much by being rubbed against stones, &c. by the fluctuating waters: those which we find in rubble at Garsington pits (fig. 16) have many adventitious bodies adhering to them, and consequently were deserted by their inhabitants before they rested there. In the clay at Shotover (fig. 1) near Oxford, they have a curious smooth surface, but are otherwise imperfect: at Stonsfield, in the slate-stone, they are generally crushed (fig. 2, y): those approaching nearest to perfection, which he had seen, came out of the sand (fig. 13) under the bed of stone at Garsington-pits near Oxford: the outer part is quite perfect, and the verge of the conical cavity is as

* The belemnite after calcination has all the properties of the Bolognian stone. If it be exposed a few minutes to the sun, and immediately taken into a dark room, it will shine like phosphorus for some time; and when the light diminishes, if again exposed to the sun, its splendor will be renewed.—Orig.

thin as paper, but the alveolus is destroyed, except the apex or point. At Thame, in digging for stone, several small ones were found in a stratum of blue clay of a more cylindrical form (fig. 17); some of which have the pearly substance still remaining; an incontestable proof of their being marine productions.

How much of the cavity is occupied by the alveolus cannot be truly ascertained, until a perfect one can be found, which it will be hard to do in the fossil state; but if we may judge from the nautilus, the walls are carried to a distance from the last-formed valve, much greater than that at which the valves are placed from each other: as in fig. 13, from a to b, which gives the animal all the convenience of forming a new valve or diaphragm, c. This circumstance has been very ingeniously cleared up by a learned physician in the Gent. Magazine for Jan. 1752, p. 8.

VI. On a Singular Species of Wasp and Locust. By Samuel Felton, Esq., F. R. S. p. 53.

These species of wasp and locust, Mr. F. found in the Island of Jamaica, and which he thinks have never been described.

I. Crinita.*—Vespa setis colli thoracis abdominisque radiantibus corpore longioribus, fig. 8, pl. 1.

It is as large as a common wasp, but rather narrower. The head is brownish, the vertex black, in a triangular form. The antennæ are shorter than the thorax, a little thicker towards the end, of a yellow brownish colour; but black in the middle. The thorax is light brownish on the back, but on the sides and underneath black: before the insertion of the wings, there are 2 yellow lines running transversely downwards; just over the insertion of the wings two hairs go out on each side of equal length, and very near twice as long as the whole body; from the upper part of the neck likewise go out two hairs as long as the body.

The abdomen is divided into 6 segments, of which the first is very narrow at its basis, quite black, only the hind margins yellow; from this segment there only grow out two hairs twice as long as the abdomen, at the base, but no where else; the other 5 segments are between brown and yellow coloured, their hind margins a little paler, and the second has a black girth near the fore margin; hairs go out near the fore segment as rays; in the 2d only 3, and they are shorter than the abdomen, especially the side ones; in the 3d, 4th, and 5th segments, there are 4 or 5 long hairs longer than the body, and several shorter ones, especially underneath where there are no longer ones; the 6th segment is

* The singular appearance of this insect, which seems to be a species of sphex, is supposed to be owing to a particular kind of filiform fungus springing from different parts of the animal: it is however highly remarkable that they should proceed with such regularity from the opposite sides of the insect.

terminated with a long hair. All these hairs are of a light brown colour, seem to be stiff, but their ends are quite soft like papillæ, and from thence thicker. The wings are shorter than the abdomen; the upper ones folded. The legs are black, except the thighs, which are yellow; at their joints there are short hair-like rays, whose ends are likewise short and thickened.

II. Rhombea cicada thorace compresso membranaceo foliaceo subrhombeo postico latiore,* fig. 9, pl. 1. The thorax is like a leaf that is raised perpendicularly from the body; it is 3 times as broad as the body, but the same length. This leaf is very near of a rhomboid figure, a little broader, or rather higher over the back; it is membranaceous, probably brownish, (when alive) half pellucid, with two spots that are more pellucid, or transparent; the larger one is very near the middle, but the smaller lower; the margins are waved, especially towards the hind angle; over the forepart of the body the leaf is double.

The abdomen is a little longer projected backwards than the leaf of the thorax. The insect had not yet got its coleoptera and wings. The hind thighs, that are thicker, have on the upper side an additional narrow membrane added to them. The head and maxillæ are very like those of the grylluses, but there is such an affinity between this and the cicada foliata, Linn. Syst. Nat. 435—6, that he should think it the same species, if the thorax of this was not broader behind towards the end. The antennæ are broken off; else from their length one might learn to what genus the tribe Linnæus calls cicadæ foliaceæ (Syst. Nat. p. 435) should be referred; for he doubts whether Linnæus ever had seen perfect specimens of them.

VII. Of an American Armadilla.† *By William Watson, M. D., F. R. S.* p. 57.

This animal was alive, in the possession of Lord Southwell. It is called by Linnæus, in his Syst. Nat., Dasypus cingulis novem, palmis tetradactylis, plantis pentadactylis. It is called by naturalists the American armadilla, was brought to England from the country near what is usually called the Mosquito-shore, on the American continent. Its weight was 7lb. avoirdupois, and its size that of a common cat. It was a male, and had improved greatly both in appearance and colour, since it had been brought to this country. It was fed with raw beef and milk; but it refused our grain and fruits. In its own country, according to the accounts of those who treat of it, it burrows in the ground.

VIII. Of the Quantity of Rain fallen at Mount's Bay in Cornwall, and of the Weather in that Place. By the Rev. Wm. Borlase, M. A., F. R. S. p. 59.

At Carlisle Mr. B. was informed there fell $6\frac{1}{4}$ inches of rain in the months of

* This is the *Cicada rhombea* of Linnæus.

† This species is the *Dasypus novemcinctus* of Linnæus.

June and July last, 1763. In Mount's-bay, Cornwall, according to his ombrometer, there fell

	Inches.	Tenths.	Parts.	Inches.	Tenths.	Parts.
In June	2	6	$\frac{1}{2}$	6	9	$\frac{1}{4}$
In July	4	3	0			

So that the rain in this part of Cornwall exceeded that at Carlisle almost half an inch.

It is some amusement to compare the journal of the weather in one part with the accounts in the papers of storms, heats, and drought, and their contraries, in another. On the 11th of August, there was at Brussels a most dreadful storm of thunder, lightning, and hail: at Ludgvan only misty rain and showers. On the 19th of the same month, when one of the most violent hurricanes ever known scourged some parts of Kent, from the W. and S.W., it was calm, hazy, and sunshine, and the wind at N.E. in Mount's Bay, in the morning; in the evening S.S.E. On the 2d of this month of October, there was a most violent storm on the eastern coasts of Britain, from Yarmouth to Edinburgh; wind from the N.E. and E.N.E.; many ships distressed, many wrecked. What is remarkable, at the same time a like violent storm blew in the western channel, along the coasts of Cumberland, Lancashire, and Wales, but the wind from the west. In Mount's bay the wind was somewhat stormy and showery in the morning, the wind at west half north; in the afternoon windy and showery and sun-shine, west half south. Thus we see how different, nay opposite, the winds, even in their extremest violence, are on the eastern and western coasts, where they have nothing between them but a narrow ridge of land. The cause of this remarkable opposition, Mr. B. would be glad to see well explained. It must certainly, he thinks, have lain in the middle between the two forces; and it might contribute somewhat to the discovery, to know whence, and to what degree, the wind blew on the mountains in Scotland, and as far south as Derbyshire, from Sunday morning to Monday noon.

IX. On a Hernia of the Urinary Bladder including a Stone. By Mr. Percival Pott, Surgeon, and F.R.S. p. 61.

May be consulted in this author's works, of which a complete collection was published after his death by Sir James Earle.

X. Some Observations on the Cicada of North America. Collected by Mr. P. Collinson, F.R.S.* p. 65.

In Pennsylvania the cicada is seen annually, but not in such numbers as to be

* This species is the *Cicada septendecim* of Linnæus.

remarkable; but at certain periods, of 14 or 15 years distance, they come forth in such great swarms, that the people have given them the name of locusts. About the latter end of April these cicadæ come near the surface: this is known by the hogs routing after them. They creep out of the ground, near the roots of trees, in such numbers, that in some places the earth is full of holes, like a honey-comb. Their first appearance is a hexapode (an ill-shapen grub) with six feet. This is their middle or nymph state: they creep up every thing near them, and fix their claws fast on the shrubs, and bark of trees: then the skin on its back bursts open, and the fly comes forth, disengaging itself by degrees, leaving the case or exuviæ behind, in the exact shape in which it was before occupied. At first coming out the cicadæ are all white, with red eyes, and seem weak and tender; but next day they attain to their full strength and perfection, being of a dark brown colour, with 4 finely veined transparent wings, as will be better seen than described, by the specimens, fig. A, pl. 4.

They come forth out of the ground in the night; being then secure from being disturbed by so many creatures that prey on them, while they are under the operation of exchanging one state for another. From the 10th of May to the 15th they are observed to be spread all over the country. As soon as the dew is exhaled, the cicadæ are very active, flying about from tree to tree. The male making a singing noise, calling the female, which he effects by a tremulous motion he gives to two bladders, filled with air, under his wings. From their nnmbers the noise is so loud and troublesome, that it interrupts conversation with a continual din, from morning to evening. They continue coupling to the 16th of May: soon after, the males disappear, and the females lay their eggs. They are much larger than the males. They never could be perceived eating any thing; yet as they are furnished with a long proboscis, which they frequently extend, they may suck the dews, or the farina of flowers. The male, in coupling, has at the end of his tail two hooks, with which he enters between the rings that surround the body of the female. These, spreading internally, confine them long together; which may be requisite, as there is a great number of eggs to impregnate, some say 6 or 7 hundred. Soon after this work is over the female begins laying her eggs. To assist her in this operation, she is armed with a dart near half an inch long, fixed between her breast and belly, and which extends to the end of her tail. This she sheaths up, when it is not in use: with this dart she pierces the small twigs of trees, and at the same time injects an egg.

It is surprizing to see how quick they penetrate into hard wood, and croud it full of eggs, the length of 2 or 3 inches, ranged in a line close together, from 12 to 18 in each partition. How she deposits the eggs in this direction it was difficult to discover, they are so very shy while about this work: but John Bartram, observing her, in the beginning of this operation, took a strong woody stalk of a plant, and presenting it to her, she directly fell to work upon it, as he held it in his hand. It was very wonderful to see how dextrously she worked her dart into the stalk, at every puncture dropping an egg. This was seen very distinctly, as she did not touch the stalk with any other part of her body. The cicadæ fix on most sort of trees, but like best the oak and chestnut; also the sassafras, and all orchard trees. They always dart to the pith of the branch, that when the egg hatches, the little insect may find soft food in its infant state. When mature they creep forth, go down the tree, or drop off, and soon make their way into the ground, where they have been found 2 feet deep. Here they find a secure repose, till they have passed through their changes, from a maggot to a hexapode, and lastly to a fly.

July 15th and 16th they were perceived coming forth: several darted twigs were perceived, and carefully examined, and opened: some eggs were hatched, others not mature, of a dull brown colour. These were taken out, and spread on a table; in about an hour the eggs cracked. It was very entertaining to observe how the little insect contrived to disengage itself from the shell. When it was got clear from its incumbrances, it ran about very briskly, seeking a repository in the earth.

Some General Remarks.

These cicadæ are spread all over the country in a few days; but being the prey of so many animals, their numbers soon decrease, and their duration by the order of nature being short, they quickly disappear. They are the food of most kinds of domestic and wild fowl, and many beasts: even the squirrels grow fat with feeding on them. And one of the repasts of the Indians, after having first plucked off their wings, is to boil and eat them. There are two distinct species of cicadæ in North America; the one here described being much larger than the other. The lesser species has a black body, with golden eyes, and remarkable yellow veined wings.

XI. Of the Plague at Constantinople. By Mordach Mackenzie, M. D. Dated Constantinople, St. George's Day, 1763. p. 69.

So many great men have written on the plague, such as Prosper Alpinus, Sydenham, Hodges, Diemerbroeck, Muratori, Mead, &c. that Dr. M. remarks it might be justly thought presumptuous in him to touch upon that subject after them. But as he finds that they differ in some circumstances, and that some of them have had an opportunity of seeing only one year's plague; he may (he thinks) be allowed to write such remarks, as he had made for almost 30 years, that he had lived in Turkey, without any quotations or confirmations from other authors; which he hoped would help to reconcile the different opinions of the above-mentioned authors.

It is beyond dispute, that the plague appears in a different manner in different countries; and that it appears differently in the same country in different years: for it is found that most other diseases alter more or less, according to the constitution and disposition of the air in the same climate: for, some years fevers are epidemic, and very mortal: other years they are epidemic, but not mortal; the small pox the same; &c. And so the plague is some years more violent, and has some symptoms different from what it has in other years; which he takes for granted, must be the reason of any difference that may appear in the remarks of the celebrated authors already mentioned. There is one extraordinary symptom, which the most of these authors mention, though none of them prove it, or pretend to have seen it; which seemed to him inconsistent and incompatible with the animal economy. What he means is, that a person cannot die of the plague (such as it appears at Constantinople) instantaneously, or in a few hours, or even the same day, that he receives the infection. For all that have the plague there conceal it as long as they can, and walk about as long as possible. And he presumes it must be the same in all countries, for the same reason, which is the fear of being abandoned and left alone; so that when they struggle for many days against it, and at last tumble down in the street, and die suddenly, people imagine that they were then only infected, and that they died instantly of the infection; though it may be supposed, according to the rules of the animal economy, that the noxious effluvia must have been for some time mixing with the blood, before they could produce a fever, and afterwards that corruption and putrefaction in the blood and other fluids, as at last stops their circulation, and the patients die. This was the case of the Greek, who spoke with the master of horse Knightkin, at the window, anno 1752, and went and died in an hour afterwards in the vineyards of Buiuk deré; and it was said he died suddenly, though it was very well known to many that he had the plague upon him for many days before this accident happened.

Mrs. Chapouis found herself indisposed for many days, anno 1758, and complained pretty much, before she was suspected to have the plague. Captain Hills was infected in Candia 1736; was a fortnight in his passage to Smyrna, as the Captain swore to Dr. M.; yet he was 5 days in the hospital there before he died. Mr. Lisle's gardener was indisposed 12 days before he took to his bed, and he laid in bed 8 days before he died, in July 1745.

It is true that Thucydides, in his account of the plague at Athens, relates that some were said to die suddenly of it; which may have led others into the same way of thinking: but Thucydides (with all due regard to him) must be allowed to have known very little of the animal economy, for he was no physician, though a very famous historian; and he owns moreover, that when the plague first attacked the Piræus, they were so much strangers to it at Athens, that they imagined the Lacedæmonians, who then besieged them, had poisoned their wells, and that such was the cause of their death. Besides, he pretends to affirm, from the little experience he had of the plague, that the same person cannot have it twice, which is absolutely false. The Greek Padré, who took care of the Greek hospital at Smyrna for 50 years, assured Dr. M. that he had had the plague 12 different times in that interval; and it is very certain that he died of it in 1736. M. Brossard had it in the year 1745, when he returned from France; and it is very well known that he and all his family died of it in April 1762. The Abbé, who takes care of the Frank-hospital at Pera, swore to Dr. M. that he had had it already at Constantinople and at Smyrna, 4 different times. But what was still more extraordinary was, that a young woman who had it in Sept. last, with its most pathognomonic symptoms, as buboes and carbuncles, after a fever, had it again on the 11th of April, and died of it some days after, while there was not the least surmise of any accident in or about Constantinople since December, this only one excepted: but there died 4 persons in the same small house in September; and as the house was never well cleaned, and this young woman always lived in it, she was at last attacked a 2d time, and died.

The only antecedents that Dr. M. could observe to this malady, was a great murrain among the black cattle in May 1745, and in the beginning of June, the same year, swarms of butterflies flew about, and there were great numbers of caterpillars creeping every where, and afterwards a violent plague: and after observing the same anno 1752 and 1758, he foretold to Sir James Porter, that they should have a hot plague in those years; which accordingly happened, especially in the months of August and Sept. 1758, when many of Marsellini's family, Spathari, Skwackhim's cook, Charlacci Rimbeault, Jackino's son, &c. died of it.

The plague was then more frequent in the Levant than it was when Dr. M. came first into that country, about 30 years before; for then they were almost strangers to it in Aleppo and in Tripoli of Syria, and they had it but seldom at Smyrna; whereas they then had it frequently at Aleppo, and summer and winter in Smyrna, though never so violently in the winter; which must be owing to the great communication by commerce over all the Levant, and more extended into the country villages than it used to be. He takes the plague to be an infection communicated by contact from one body to another; that is to a sound body from an infected one, whose poisonous effluvia, subtile miasmata, and volatile steams, enter the cutaneous pores of sound persons within their reach, or mix with the air, which they draw in respiration, and so advancing by the vasa inhalantia, mix with the blood and animal fluids; in which, by their noxious and active qualities, they increase their motion and velocity, and in some days produce a fever; so that the nearer and the more frequent the contact is, the greater is the danger, as the noxious particles, exhaling from the infected person must be more numerous, and consequently have greater force and activity in proportion to their distance.

Some persons are of opinion, that the air must be infected, and that it is the principal cause of these plagues; whereas he presumes that the ambient air is not otherwise concerned, than as the vehicle, which conveys the venomous particles from one body into another, at least in such plagues, as he had seen at Smyrna and Constantinople; allowing always, that the different constitution of the air contributes very much to propagate the plague; for the hot air dilates and renders more volatile and active the venomous steams, whereas cold air contracts and mortifies them. The person having the plague may be said to have a contagious and poisonous air in his room and about him, while at the same time the open air is free from any dangerous exhalations; so that he never was afraid to go into any large house, wherein a plaguy person lived, provided he was confined to one room.

The pestilential fever shows itself first by a chilliness and shiverings, even in the months of July and August, so very like the first approaches of an ague, that it is impossible to distinguish the one from the other at first sight. This cold fit is soon accompanied with a loathing nausea and desire of vomiting, which obliges the patient at last to discharge a vast quantity of bilious matter, with great uneasiness and oppression in the thorax and mouth of the stomach, attended sometimes with a dry cough, as in an intermitting fever; and even in this stage it is very difficult to distinguish the one from the other. Next the patient has a violent headach and giddiness, with some slight convulsive motions: he breathes hard; his breath and sweat stink; his eyes are ruddy, he looks frighted, sad, and pale; he has an insatiable thirst; his tongue is yellowish with a red border; he has a total loss of appetite, with restlessness, great inward heat, and more than could be expected from the fever, which is sometimes pretty moderate, but grows stronger frequently towards night: the patient very often bleeds at the nose. He continues in that dismal condition for some days, till the venomous matter begins to be separated in some measure from the blood, and discharge itself critically on the surface by the cutaneous eruptions of buboes, carbuncles, blains, petechial spots, and some small vesicles or blisters: but all these symptoms are not to be looked for in the same person. When the cutaneous eruptions appear and increase sensibly, the patient finds himself better, and somewhat relieved from the great oppression he laboured under before. Some persons in the above state have a very violent fever, sometimes attended with a delirium and phrenzy; others are stupid, sleepy, and complain of nothing: as one of Captain Hill's men mentioned before; and the young fellow who died of the plague last year. Such as are furious and delirious seldom live so long as those who are sleepy and stupid; but if they live long enough to have the cutaneous eruptions push plentifully, and their phrenzy begin to abate afterwards, they may recover more probably than such as are sleepy and have a moderate fever.

Dr. M. is of opinion that nothing at Constantinople, either in the air or diet, produces the plague, though both contribute very much to its progress and violence, after it is brought there, or to any part of Turkey from any other infected place; for it is known by long experience, that it rages most in the hot months of July, August, and Sept. when the diet of most of the poor inhabitants, who are the greatest sufferers by the plague, consists of unripe fruits, cucumbers, melons, gourds, grapes, &c. The plague breaks out at Constantinople and at Smyrna some years, when it is not possible to trace whence it is conveyed; for some houses, which were infected, and not well cleaned after the infected person is removed, lodge some of the venomous moleculæ in wool, cotton, hair, leather, or skins, &c. all winter long; which, put in motion by the heat in April or May, breathe out of their nidus, where they resided, and recover so much life and action, as to enter into the cutaneous pores of any person who comes within their reach, and so infect him; as it happened at the French palace, at Mr. Hubsch's and at Caraja's house, for 2 or 3 years running. But plagues of this kind seldom spread, and are never so fatal as those that come from abroad.

Many are of opinion, that the heat kills the plague, as they term it; which is owing to a foolish superstition among the Greeks, who pretend that it must cease the 24th of June, being St. John's day, though they may observe the contrary happen every year; and the strongest plague that was at Smyrna in Dr. M.'s time, anno 1736, was hottest about that time, and continued with great violence till the latter end of Sept. when it began to abate; but was not entirely over till the 12th of Nov. when Te Deum was sung in the Capuchins convent. This mistaken notion may be in some measure owing to a wrong sense put upon Prosper Alpinus, who allows that the plague at Cairo begins to cease in the months of June and July, when the strong northerly winds (called Embats or Etesian winds) begin to blow, which make the country much cooler than in the months of May, April, and March, when the plague rages most; which he very justly imputes to the great suffocating heats and southerly winds, which reign during those months in that country: and it is then that the ships which load rice, flax, and other goods and merchandise for Constantinople receive the infection, and bring it with them thither; and on these goods being delivered to persons in different parts of the city, the plague breaks out at once with great violence among the trading people of the Greeks, Armenians, and Jews; for, both here and at Smyrna, the Turks are commonly the last of the

4 nations who are infected; but when the plague gets once among them, they suffer most by it, because they take the least care and precaution, and their families are much more numerous. The plague, as well as all other epidemical diseases, has its rise, progress, state, and declension, when it begins to lose its virulence, and many of the sick recover. Some years it is felt sporadically all the winter; and some accidents were then heard of in the Phanar, among the Greeks, among the Jews, Turks, and Armenians; and even among the Franks; it is remembered that Pera was not clean all the winter 1762. Some years it lodges in the villages on the Bosphorus; but during the winter it is never of any great consequence.

As to the cure of this disease, some are for bleeding plentifully, as Leonardus Botallus and Doctor Dover, &c. But in Turkey it is reckoned infallible death to open a vein, and therefore bleeding is never used: but Dr. M. was of opinion that a medium between these 2 extremes might prove more to the purpose; for, as it is an inflammatory disease, bleeding and emetics might be of use in the beginning, as soon as the patient is taken with the fever, especially if the fever is very hot, and attended with a delirium or any violent head-ach; but after there begins a separation of the morbific matter, which the strength of nature, and the agitation of the fever, drive upon the surface of the body in buboes or carbuncles, bleeding or purging must prove very prejudicial; but gentle vomits might be of service even then, as they might drive out those cutaneous eruptions more powerfully than nature could do it without any help. The vomits likewise might prevent the return of the morbific matter into the blood, which frequently happens, when the buboes, &c. disappear, and the patient infallibly dies in a very short time. As the pestilential fever has many remissions, he is of opinion that the use of the bark in the remissions might be of great service; as it proved anno 1752, when the French ambassador's servant was saved at Buiukderé, by means of some bark and ipecacuanha; and he was the only person that recovered of all those who were then taken ill in the village.

The practice in the hospital is after this manner: when any person is suspected, they give him a large dose of brandy with a dram of Venice treacle; and afterwards they cover him very well, that he may sweat: for the first 3 days, he eats nothing but vermicelli boiled in water, with a little lemon juice. On the 4th day they give him rice and water; which diet they observe strictly till the 15th or 20th day, when they begin to allow him very thin chicken broth, commonly called brodo longo, and they give him from first to last nothing but warm water to drink. They apply first to the buboes and parotides a cataplasm of mallows and hog's lard, to advance maturation; and, after they are ripe and open, they dress them with basilicon ointment. They apply caimack and sugar to the carbuncles for some days, to cool them; and when they begin to separate, they apply a digestive of Chio turpentine with the yolk of an egg. They apply nothing to the blains and petechial spots, which appear and disappear again on any part of the body every 3 or 4 days. All this time they give the sick no medicines, besides Venice treacle for the poor, and some doses of bezoar for such as can afford to pay for it; and they never can be persuaded to change their method; for when Sir J. Porter gave them Doctor James's powder, they never tried what effect it might have.

Dr. M. was of opinion that all antiphlogistics should be used before the eruptions; and all alexipharmics and antiseptics after them; more particularly camphire, and some doses of bark always in the remissions of the fever; and blisters ought to be of great use in the sleepy and stupid plague, for rousing the animal spirits, and for giving them some motion: but they are never used at Constantinople; and, as they live by custom, it is impossible to prevail upon them to change it. As to preservatives, he thinks the best is to remove from the infected persons and houses, and to keep at a proper distance for many days from them. Some are of opinion, that fire preserves from the plague, and purges the air; from whom he begs leave to differ; for he had remarked that cooks and cooks' mates, who were always near the fire, suffered more by the plague than any other set of people in proportion to their number. Besides the fire enlivens and gives energy to the poisonous effluvia lodged about them, which otherwise might die and disperse in the open air, if exposed sufficiently to it. Fire also opens the pores, relaxes the fibres; and, as the hot weather propagates the plague, fire should do the same more or less; and for the same reason he imagines that all perfumes must be of very little service. The next best preservative he takes to be moderation, and a diet of such meats as are of easy digestion, of a rich balsamic quality, and capable of producing a rich and generous blood. It is also a great preservative to be under no apprehension, and to guard as much as possible against dismal thoughts and imaginations on such occasions.

1748. The plague began the 10th of May and ended in November.
1749. It began the 16th of March, and ended the 20th of October.
1750. It began April 21, and ended September 17.
1751. It began May 15, and continued till the end of September 1752.
1753. It began May 31, and continued till Sept. 17, 1754.
1755. It began in June, but there was very little plague all this year.
1756. It began March the 6th, and ended the 12th of December.
1758. Then there was none till April 23, 1758, which ended in October.
1759. It began April 4th, and ended about the 10th of September.
1760. It began April the 24th and ended the 10th of November.

1761. It began the 10th of March and continued till the 19th of December 1762. Since which day there had not, at the date of Dr. M.'s communication, been one accident, besides that of the young woman on the 11th of April already mentioned.

In 1751, the 20th of October o s. a vast quantity of snow fell, that cut off the distemper; and there was little plague in 1752. The former year was the most considerable, and more universally mortal at Constantinople than any in the space of 15 years.

XII. Of a Remarkable Tide at Bristol. By the Rev. Josiah Tucker, D.D. Dean of Gloucester.* p. 83.

On Saturday the 11th instant, [Feb. 1764,] when the tide had hardly begun to flow according to its regular course, it was observed by the water-bailiff of the city, and by several others, both on the back and at the key, to rise very suddenly to almost high-water mark; and it so continued for near half an hour; it then sunk, almost instantaneously, 3 feet perpendicular: after that it began to flow in again, and kept flowing on till 1 o'clock, and rose to the height it was expected to do. At Rownham Passage, a mile below the city, the ferry-men observed the tide to ebb almost instantaneously, and to sink at least 4 feet perpendicular. It then flowed in again, as it should have regularly done. At King Road, which is about 3 miles below the city, the officers observed the king's boat to float suddenly, which they attributed to a great fresh coming. But they found afterwards the boat presently aground. He could get no intelligence of any thing observable that happened in the river Severn, excepting that at Gloucester, and at Worcester, the inundation sunk very fast on that day. But most undoubtedly the strong rapid tide of the Severn must have been affected in a very remarkable manner, had there been any curious persons to take notice of it.

XIII. A Letter containing some Experiments in Electricity, to Mr. Benjamin Wilson, F. R. S. from Mr. Torbern Bergman, of Upsal, in Sweden, p. 84.

This letter consists chiefly of notices and inquiries concerning positive and

* This celebrated divine was born at Langhorn, in Caermarthenshire, in 1711; and died in 1799; consequently at 88 years of age. He was of St. John's College, Oxford, where he took his degree of D.D. in 1759. From the University he removed to Bristol, where he became rector of St. Stephen's, and had a stall in the Cathedral. In 1758 he was preferred to the deanery of Gloucester. He distinguished himself by his theological and political tracts, (among which last those in favour of the independence of the American colonies show that Dr. T. possessed great strength of judgment, with a mind in the highest degree liberal and enlightened,) and by his writings on commercial subjects; but it is his Treatise on Civil Government, in opposition to Locke, which is considered as his principal work.

negative states of electricity, and some other controversial matters; but is not of importance to be reprinted.

XIV. Of a Fish from Batavia, called Jaculator. By John Albert Schlosser, M. D., F. R. S.* p. 89.

Governor Hommel gives the following account of the jaculator or shooting fish, a name alluding to its nature. It frequents the shores and sides of the sea and rivers in search of food. When it spies a fly sitting on the plants that grow in shallow water, it swims on to the distance of 4, 5, or 6 feet, and then, with a surprizing dexterity, it ejects out of its tubular-mouth a single drop of water, which never fails striking the fly into the sea, where it soon becomes its prey. The account of this uncommon action of this cunning fish raised the governor's curiosity; though it came well attested, yet he was determined, if possible, to be convinced of the truth, by ocular demonstration. For that purpose, he ordered a large wide tun to be filled with sea water; then had some of these fish caught, and put into it, which was changed every other day. In a while they seemed reconciled to their confinement; then he determined to try the experiment. A slender stick, with a fly pinned on at its end, was placed in such a direction, on the side of the vessel, as the fish could strike it. It was with inexpressible delight, that he daily saw these fish exercising their skill in shooting at the fly, with an amazing velocity, and they never missed the mark.

XV. Of the Polish Cochineal. By Dr. Wolfe, of Warsaw. p. 91.

The several kinds of potentillas† are here very rare, and it was only on the polygonum minus, or scleranthus perennis Linnæi, that Dr. W. found the cochinille. He gathered about 300 of the coccusses, and put them with the plants and some sand in large pots. They are of different sizes. The insects creep out of their coccusses from the beginning of June till the middle of August: about 50 got out under his eyes. They are all exactly of the same shape: but some are 3 times smaller than others, according to their coccusses. The coccus is a thin round white skin. The insects are all hairy more or less; some are of a darker colour, some more crimson; some seem somewhat longer, others broader. But these differences seem to depend on their voluntary exten-

* This fish is the *Chætodon rostratus* of Linnæus. C. cauda integra, spinis pinnæ dorsalis novem, maculâque ocellari, rostro cylindrico. Lin. Syst. Nat. p. 462.

† The *Coccus Polonicus* of Linnæus, or Polish Cochineal, is usually about the size of a coriander seed, and of a purplish brown colour on the outside, and blood-red within. Since the introduction of the American cochineal, or coccus cacti, it has been considered as of but small consequence, even the Kermes or Coccus Ilicis having been long superseded by the American species.

sion, and on their age, because they grow from day to day darker and more hairy. No mouth is to be seen, but a deep plait or furrow at the upper part of the breast. Two extremely small dark points seem to be the eyes. The two antennæ are thick, twisted like a screw, of the length of the breast; they end in an obtuse point. The 2 fore legs are twice the size of the 4 hinder legs, they have all sharp black incurvated claws. It is impossible to find marks of the sex; and though they join sometimes their anusses, yet they do it so loosely, that it cannot be accounted for a copulation. They seem to eat nothing at all. They creep about the plant a week or two, going often under ground, and getting up again. Then they make themselves a deep cylindrical hole in the sand, down to the hard bottom of the pot, the end of which they cover with a fine white silk growing on their bodies. There they lay their eggs and die. Others, who are disturbed in their work, grow weary and white, as if they were powdered all over with a white meal, which through a glass appears to be very fine white silky hairs, coming out all over the body. At last they lay them down on their backs.—The silky hairs grow very fast, to the length of one inch and a half, and the insect twists with its claws the hairs all round its body, so as to resemble a small heap of cotton; but the hairs are so tender, that a small wind will tear and destroy it. In this heap of cotton they lay their eggs, from 50 to 100, and then they die. Thus they remain till the middle of July. Afterwards, though they make their holes, or their cotton heaps, yet they die without laying eggs. The eggs are crimson, transparent, scarcely visible, long, and round-pointed at both ends. In a week's time the young insects creep out: they are like their parents, but smooth, transparent, and crimson. He presented them every day fresh roots of the polygonum, but he could not say they eat any of them. In a week or two they disappeared, going under ground. The insects seemed then all dead, and so did the young ones, buried up in sand: but he hoped next spring to see them alive, and to prosecute their further change. He killed about 100 of the insects in hot vinegar, as it is done in Mexico; and meant to attempt to dye some woollen threads in the common way of the scarlet dyers. In the microscopical observations of Ledermuller at Nuremberg, there are tolerable drawings belonging to this subject. In the beginning of August he found an extremely small white fly, somewhat like to what is supposed to be the male insect. It is a third part of the size of what is represented by Ledermuller. It has a body like a gnat, snow-white, powdered below, but shining grey on its back, 6 tender snow-white legs without claws, a thick bulky head, two very small prominent eyes, two hair-like attennæ, two wings, large enough in comparison to the body, snow-white below, and shining grey above. The belly to the tail is taper, and at the tail are 3 white hairs, very tender, and 4 or 5 times the length of the whole fly. But as this was the single one among 300, and totally unlike in every part to the other insects, he doubted very much of its being of this genus.

XVI. Observations on Two Ancient Etruscan Coins. By the Rev. John Swinton, B. D., F. R. S. p. 99.

These two small Etruscan coins or weights, are each of them an uncia, or 12th part of an as, and in pretty good conservation. The weight or value of each piece appears from a single globule on the reverse. The first of these medals presents a diademated head, somewhat deformed by the injuries of time. The workmanship is rude, such as we find it to be in many of the more ancient Etruscan coins. The slip of metal projecting from the round of the weight demonstrates the piece to have been cast, and may therefore be considered as a certain indication of its high antiquity. From the globule and two letters, T V, on the reverse, we may infer this coin to have been a stips uncialis of the Tudertes, or people of Tuder, ΤΟΥΔΕΡ, as this ancient city of Italy seems to have been called by Strabo. This small Etruscan uncia weighs precisely 3 dwts. 1 gr.

The second piece exhibits on one side the head of Hercules, adorned with a lion's skin; behind which a fish resembling the turso, or tyrso, appears, with 3 Etruscan letters well enough preserved. On the reverse are a dolphin, or tyrso, part of an anchor, and another fish under the former. A single globule, or uncial mark, determining the weight or value of the piece, is also visible here. The workmanship is somewhat rude, and different from that of the Romans. The tyrso seems to allude to the origin and most ancient name of the Etruscans, who were called Tyrsenians by the Greek writers that flourished before Polybius. The forms of the letters on this medal are exactly the same with those of the correspondent elements used in Umbria and the Proper Etruria, and apparently answer to the Roman letters FAI, probably referring to the town of Fæsulæ in the Proper Etruria.

XVII. Observations on the Solar Eclipse, April 1, 1764, made in Surrey-street, Strand, London. By James Short, M. A., F. R. S. p. 107.

Apparent time.

March 31, $21^h\ 4^m\ 33^s$ the beginning of the eclipse by Mr. S.

4 36 by Lord Morton.

The end could not be seen for clouds. The sun's diameter parallel to the horizon, about an hour before noon on the day of the eclipse, was 31′ 59″.4, air hazy. The next day at the same hour it was 31′ 58″.6.

XVIII. Observation of the Solar Eclipse, April 1, 1764. By Dr. John Bevis. p. 105.

Dr. B. observed the beginning at $9^h\ 5^m\ 8^s$. But from the unfavourable state of thesky, he thinks it must have really happened sooner, by 10 or 15 seconds, as he judged from the first perceivable distance of the cusps; so that if he states it at 9^h 4^m 53^s, he presumes he shall err but a very few seconds. About the middle of the eclipse, the air was very clear, and the cusps well defined, which wanted about 60 degrees of joining. He could not then discern any thing on the sun about the moon's limb, which in the least indicated a lunar atmosphere. A full digit of the sun, or more, remained uneclipsed. The day-light was but inconsiderably diminished; so that neither Jupiter nor Venus could be seen, though both in a favourable position, to the east of the sun. Fahrenheit's thermometer, placed without door to the north, stood at 50 when the eclipse began, and fell but one division while it lasted. The end of the eclipse could not be observed for thick clouds.

The Moon's Eclipse of March 17th, 1764, observed in Surrey-street, in the Strand, London. By Dr. Bevis. p. 107.

Apparent time.
10^h 32^m 0^s the penumbra just sensible to the naked eye.
10 39 0 the beginning, viewed with an opera glass.
13 16 30 the end of the eclipse, with an opera glass.
24 0 the moon clear of the penumbra.

The shadow was ill defined, though the air was clear.

XIX. Observations on the Lunar Eclipse March 17, and the Solar Eclipse, April 1, 1764, made at Liverpool. By Mr. James Ferguson, F. R. S. p. 108.

The clock being duly adjusted by our meridian line, at noon, and the time being found by observations of several stars in the evening of March 17, the apparent time of the beginning of the moon's eclipse was observed to be at 10^h 27^m p. m. and the end at 13^h 11^m.

The Sun's Eclipse observed April 1.

8^h 59^m 0^s eclipse begun, the sun's altitude 28° 37′.
10 21 0 — 10¼ digits, the greatest observation.
11 50 45 eclipse ended, the sky quite clear. Sun's altitude 41° 27′ 0″.

Between the beginning and the middle of the eclipse, he could plainly perceive inequalities in the moon's eastern limb on the sun, by means of the reflecting telescope; and he often observed little tremulous bright specks of the sun's lower edge in the otherwise dark place just before, or west of the lower cusp; but they vanished in an instant, except one which was considerably larger than any of the rest, and was visible for about 2 seconds of time by estimation. This undoubtedly was owing to a dent or valley in that part of the limb of the moon, which no hill beyond it took off from the sight. But as the eclipse was drawing toward the end, he could perceive no inequalities of the moon's western limb on the sun, nor any such specks in the sun's edge about either of the cusps.

Below are the times of the beginning, middle, and ending of the eclipse, as predetermined by a projection of it for Liverpool, from Meyer's tables, which were the apparent times; reducing the observed equal times to the apparent, by subtracting 3 minutes 48 seconds (which he supposed was the equation of time) from the equal times as observed by the clock and two watches which kept equally going together.

	Apparent times. By projection.				Apparent times. By observation.		
Beginning	8^h	56^m	0^s	..	8^h	55^m	12^s
Middle..............	10	21	45	..	not certain.		
End	11	48	0	..	11	46	57
Duration	2	52	0	..	2	51	45
Digits eclipsed	$10\frac{25}{60}$	0	0	..	$10\frac{1}{2}$ exactly.		

XX. Observations on the Eclipse of the Sun, April 1, 1764, *at Brompton-Park. By Mr. Samuel Dunn.* p. 114.

At $9^h\ 4^m\ 29^s$ per watch, thought he saw a little dull tremulous vibration obtrude itself on the limb of the sun; and,
At 9 4 30 it became a little more sensible; and,
At 9 4 31 a little more sensible; but it was
At 9 4 32 per watch before he was certain the sun's limb was touched by the limb of the moon; and,
At 9 4 33 he plainly saw, through a telescope, and the thin vapours of the atmosphere, the least visible dent, perfectly well defined in the sun's limb.

The watch was but one second of time before the sun at the time of observation, for Brompton park, which is exactly one mile from Hyde-park corner, in the way towards Kensington. Clouds prevented the end of the eclipse from being observed.

Observations on the Eclipse of the Moon, March 17, 1764, *made at Brompton-Park, near London,* 10^m *of Time West of Paris, and* 43^s *of Time West of the Royal Observatory at Greenwich. By Mr. Samuel Dunn.* p. 117.

At $10^h\ 39^m\ 30^s$ the eclipse began in that part of the moon's limb between Tycho and Grimaldus.
— 13 22 10 the eclipse ended.

XXI. On the Degree of Cold observed in Bedfordshire. By John Howard, Esq. F. R. S. p. 118.

Mr. H. observed at Cardington, in Bedfordshire, Nov. 22, 1763, just before sun-rise, Fahrenheit's scale, by one of Bird's thermometers, being so low as $10\frac{1}{4}$.

XXII. Remarks on the first Part of M. l'Abbé Bathelemy's Memoir on the Phœnician Letters, relative to a Phœnician Inscription in the Island of Malta. By the Rev. John Swinton, B.D., F.R.S. p. 119.

M. l'Abbé Barthelemy having lately communicated to the learned world, a copy of one of the Phœnician inscriptions long since discovered in the Island of Malta, more accurately taken (as he pretends) than any of those that had ever before appeared, and attempted to explain it in a manner perfectly new; Mr. S. makes a few cursory remarks on what he has been pleased to advance on this occasion. But the dispute is too little interesting to merit any further consideration in these Abridgments.

XXIII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1763, *pursuant to the Direction of Sir Hans Sloane. By John Wilmer, M.D.* p. 137.

This is the 42d presentation of this kind, completing to the number of 2100 different plants.

XXIV. Observations on the Eclipse of the Sun, April 1, 1764. *By the Rev. Nathaniel Bliss, M. A., Savilian Prof. of Mathem., Oxford. &c.* p. 141.

On account of cloudy weather, only the following observations were taken.

The beginning of the eclipse at .... 9^h 5^m 3^s morn.
The digits eclipsed at least $10\frac{9}{10}$.
The moon's equatorial diameter .... 29′ 55″$\frac{1}{4}$.
The sun's horizontal diameter .... 31 56$\frac{1}{4}$.

XXV. Observations on the Eclipse of the Sun, April 1, 1764. *By the Rev. Thomas Hornsby, M.A, and Savilian Prof. of Astron. at Oxford.* p. 145.

Beginning of the eclipse at .... 8^h 59^m 33^s apparent time.
Moon's horizontal diam. by the micrometer 29′ 45.″1.

It was formerly a dispute among the astronomers, whether the moon's diameter did not appear less when viewed on the sun than when seen on a darker ground. The observations of M. le Monnier in Scotland, in 1748, seemed to leave little room for doubt. At the time of the middle, the moon's centre was about 39° high, and therefore the moon appeared under a greater angle to the eye of the observer than if seen from the earth's centre, by about 18 seconds. The true horizontal diameter from the above observations was therefore 29′ 27″; which is but 7″ less than according to the latest and best tables; which tables may perhaps give the moon's diameter too large, because constructed from observations made with refracting telescopes, through which the diameters, both of the sun and moon, must necessarily appear under an angle somewhat enlarged.

At $11^h\,54^m\,20\frac{1}{2}^s$ apparent time, Mr. H. observed the end of the eclipse, which he believes may be depended on to 3 or 4 seconds. About 20 minutes after the sun had passed the meridian, he measured the sun's horizontal diameter with Dollond's micrometer, and found it = 32′ 0″.8.

XXVI. Observations on the Moon's Eclipse, March 17, and the Sun's Eclipse, April 1, 1764. By Matthew Raper, Esq., F.R.S. p. 150.

Thorley hall, lat. 51° 50′ 45″ N. Long. 38^s east of Greenwich.

1764, Mar. 17, ☾ immerged into the true shadow at 10^h 41^m ^{s} ap. time.
Emerged out of the same at 13 25
Mar. 31, ☉ eclipse was begun above a minute at .. 21 8 0
Ended April 1 at .... 0 1 45 or 48.

Observed with an 8-foot refractor.

XXVII. A Table of the Places of the Comet of 1764 discovered at the Observatory of the Marine at Paris, the 3d of January, about 8 o'clock in the Evening, in the Constellation of the Dragon, concluded from its Situation observed with regard to the Stars. By Charles Messier, Astronomer at the Depot of the Plans of the Marine of France, at Paris. p. 151.

1764	True time.			Mean time.			R. ascension observed.			Northern declination observed.			Longitude observed.			Northern latitude observed.		
Jan. 3	9^h	24^m	33^s	9	29^m	9^s	236°	29′	16″	58°	32′	58″	♎ 11°	37′	16″	72°	53′	48″
	15	5	4	15	9	47	239	45	31	58	51	29	14	49	7	74	22	13
	18	48	55	18	53	42	241	56	1	59	2	54	17	9	14	75	19	50
10	16	10	39	16	18	28	305	57	17	44	23	10	♒ 29	0	38	60	41	41
	18	33	53	18	41	44	306	22	2	44	6	14	29	15	54	60	18	20
11	6	43	27	6	51	31	308	14	21	42	45	45	♓ 0	21	59	58	28	49
	7	30	54	7	38	59	308	20	17	42	42	35	0	26	19	58	23	58
	18	28	33	18	36	48	309	50	42	41	22	54	1	7	31	56	42	7
14	17	7	58	17	17	22	316	59	44	34	37	35	4	23	15	48	8	16
	18	32	53	18	42	18	317	5	44	34	30	37	4	25	22	47	59	50
15	5	43	14	5	52	48	317	53	29	33	39	23	5	8	47	47	6	48
	7	13	15	7	22	51	317	59	44	33	31	50	5	20	11	47	1	47
16	6	35	19	6	45	15	319	28	52	31	51	34	5	22	51	44	44	33
	7	26	11	7	35	58	319	32	37	31	48	41	5	24	58	44	40	44
18	5	40	32	5	51	8	321	52	36	28	45	54	6	9	39	41	4	3
	7	4	45	7	15	22	321	56	21	28	40	52	6	10	49	40	58	8
19	7	42	8	7	53	4	322	57	8	27	17	25	6	29	47	39	19	57
20	6	50	20	7	1	34	323	46	38	26	5	33	6	43	22	37	56	17
22	5	49	50	6	1	38	325	9	52	23	49	1	7	0	56	35	20	54
	6	11	16	6	23	4	325	10	22	23	47	57	7	0	57	35	19	45
29	5	50	59	6	4	23	328	9	59	17	28	48	7	7	46	28	25	8
30	6	2	52	6	16	28	328	23	44	16	42	10	7	1	12	27	36	59
Feb. 4	5	57	53	6	12	8	329	17	41	13	11	38	6	26	10	24	2	24
7	6	30	23	6	44	54	329	33	51	11	6	15	5	50	20	22	0	9
8	6	16	20	6	30	54	329	39	35	10	26	47	5	41	16	21	21	25
11	6	14	49	6	29	28	329	41	27	9	2	46	5	10	5	20	2	34

Below are the elements of the theory of this comet, which M. Pingré deduced from M. Messier's first observations.

The ascending node ☊	3ˢ 29° 20′ 6″
Inclination	53 54 19
Place of perihelium	16 11 48
Logarithm of the perihelium distance	9.751415.

Passage by the perihelium Feb. 12, at 10^h 29^m mean time in the meridian of Paris. The motion retrograde.

XXVIII. A Supplement to Mons. Pingré's Memoir on the Sun's Parallax. Translated from the French by M. Maty, M.D., F.R.S.* p. 152.

Mr. Pingré was anxious to determine which of the observations, viz. that of Messrs. Mason and Dixon made at the Cape of Good Hope, or that which was made at the Island Rodriguez by Mr. Thuillier and him, deserved the preference. The first reduces the sun's parallax to 8″½ at most, whereas the latter increases it to near 10½: the difference is too considerable not to deserve an inquiry into its causes. In the Phil. Trans. for 1761† are two observations, which would be decisive, if time and other circumstances had permitted them to be made with sufficient accuracy. Mr. Maskelyne observed at the island of St. Helena, situated at 15° 55′ south latitude, and according to Dr. Halley at 33^m 17^s of time west of the Observatory at Paris. But this determination of the longitude does not seem sufficiently exact. On comparing many observations of Jupiter's satellites immersions and emersions, made at the island of St. Helena by Mr. Maskelyne, with the corresponding ones made at Paris at the Marine Observatory by Mr. Messier, he has only found 31^m 56^s for the difference of longitude between the two places; and as the Marine Observatory is 2^s east of the Royal Observatory, we may conclude that the place where Mr. Maskelyne observed is only at 31^m 54^s west of the Royal Observatory. Mr. Maskelyne's observation being compared with that of Tobolsk, would give 11″ for the horizontal parallax, which is a little too much. Let the distance determined by Mr. Maskelyne be only diminished by 2^s, and his observation will perfectly agree

* Alexander Guy Pingré was born at Paris in 1711; and he died in 1796; consequently at 85 years of age. He applied with great assiduity to scientific pursuits, and became librarian of St. Genevieve at Paris. In 1760 he was sent to the South Sea to observe the approaching transit of Venus over the sun's disk. He was afterwards employed in proving the going of the time-pieces of M. Leroy. He was first admitted a member of the Academy of Sciences; and afterwards of the National Institute. M. Pingré's works chiefly are: 1. State of the Heavens from 1754 to 1757. 2. Memoirs of Discoveries made in the South Seas, 4to. 3. Historical and Theoretical Treatise on Comets, 2 vols. 4to. 4. Translation of Manilius's Astronomics, 8vo. 5. History of Astronomy in the 17th Century.

† See p. 557 and 596 of vol. xi.

with mine; but in order to make it tally with that of the Cape, it would be necessary to diminish this distance by 10 or 11^s, and it is not very likely that Mr. Maskelyne should have committed such an error. His observation might likewise be brought to coincide with mine, by making a diminution of 40 or 45^s in the western longitude of St. Helena; whereas it would be necessary to take several minutes from that longitude in order to make the observation agree with that of the Cape, which does not seem possible. Hence, though he does not take Mr. Maskelyne's observation to be in itself absolutely decisive, yet he is persuaded that it adds great weight to the exactness of his own.

Fort St. George at Madras is, according to Mr. Hirst, 13° 8′ north latitude, and 3^m 4^s of time eastward of Pondicherry, and consequently at least 5^h 12^m 54^s eastward of our observatory's meridian. The interval observed by him between the two internal contacts, was 5^h 51^m 43^s, greater by 2^m 49$^s\frac{3}{4}$ than that which was observed at Tobolsk. This would give 9″.56 for the sun's horizontal parallax, a quantity which is about a medium between the Cape observation and Mr. P's.

It is to be observed, that by increasing or diminishing by 10^s the duration observed at Madras, the question of the parallax will be decided conformably either to the observation of Rodriguez, or that of the Cape.

In the same vol.* are some observations of the same transit made at Abo and at Hernosand; the total duration was observed in both places; it may have been lengthened somewhat beyond its limits; but these observations agree at least in this point with all the others that were made in the north, viz. that being compared with the Tobolsk observation, with regard to the duration of the transit, they give above 10″ for the horizontal parallax of the sun.

Mr. P. had the communication of Mr. Rumowski's observation made at Selenginsk in Siberia. The latitude of Selenginsk is 51° 6′ 6″. And he settled the longitude to be 6^h 57^m 50^s from the Paris meridian, from many observations. This being supposed, the observation of Mr. Rumowski compared with Mr. P.'s would give 10″.1 for the horizontal parallax of the sun in the month of June, and 10″.26 for that parallax when the sun is at the mean distance. It is true, the same observation, compared with that of Messrs Mason and Dixon, would much reduce this parallax; and therefore it can be of no use to decide the question, unless we had some from Africa or from the adjacent seas.

Mr. P. observes, you will undoubtedly have observed a pretty remarkable difference between my observations of Venus, as I had the honour to send them to you from Lisbon March 6, 1762, and the same observations as I have related them in my memoir. I imagined I had sent the correction to some one of the

* Page 561 of vol. xi.

members of your celebrated society. When I made the reduction of these observations at Rodriguez, I found myself under disagreeable circumstances; and it was probably my uneasiness at that time that occasioned a want of due attention, which I was the longer in finding out as I could not easily suspect it. My clock went too slow that day at noon by $1^m\ 2^s$, so that I ought to have added $1^m\ 2^s$ to the time of the clock, whereas I subtracted as much from it. This is the reason of the difference in the times. The small variation in the distances of the limbs was owing to a stricter verification of the parts of my micrometer.

XXIX. On the Transit of Venus. By Christian Mayer, S.J. Translated from the Latin by James Parsons, M.D. p. 161.

These observations were made at Schwezinga, belonging to the elector palatine; but neither the latitude nor the longitude of the place were known.

	True time.
The interior contact of the western limb of Venus, with the western limb of the sun, observed with Dollond's telescope . .	$20^h\ 53^m\ 8^s$
The moment of the egress, when the same limb of the sun after the interior contact first appeared corniculated, most accurately observed with the same telescope, was	20 53 35
Whence he concludes that the interior contact happened	20 53 $33\frac{1}{2}$

As to the instant of the exterior contact, he sent only two observations made by him with certainty, because of the intervening clouds: the first shows the time when he distinctly saw through the clouds the certain emersion of Venus, $\frac{1}{16}$ part of the diameter of Venus, as nearly as could be judged, excavating the limb of the sun; the other when, from the unlucky clouds, he could no more observe the least vestige either of the emersion, or exterior contact, or of Venus.

The first outer contact	$21^h\ 9^m\ 4^s$
The other time of the certain emersion	24 17 27

XXX. Astronomical Observations. By Christian Mayer, S. J. Astronomer to the Elector Palatine. From the Latin. Dated Heidelberg, April 17, 1764. p. 165.

The following observations of a lunar eclipse were made March 17, 1764, at Schwezinga, near Heidelberg, in the Palatinate, with a Dollond's 10-feet tube. —The penumbra began at $11^h\ 3^m\ 24^s$ true time; the penumbra denser at $11^h\ 8^m\ 11^s$; the beginning of the eclipse, doubtful, $11^h\ 13^m\ 8^s$; the same was visibly begun $11^h\ 14^m\ 9^s$; end of the eclipse, doubtful, $14^h\ 0^m\ 11^s$; the same certain $14^h\ 1^m$; end of the penumbra $14^h\ 4^m$. The greatest obscuration was $8^d\ 39'$. And the moon's diameter measured $33'\ 49''$.

Observation of the solar eclipse made at the same place, April 1, 1764.—The eclipse began at $9^h\ 40^m\ 41^s$ in the morning; and ended at 43^m afternoon; the greatest obscuration 10 dig. 25¼′, was at $11^h\ 10^m\ 56^s$. The sun's diameter measured 32′ 4″.8.

XXXI. Observations on the Eclipse of the Sun at Chatham, April 1, 1764. *By Mr. Mungo Murray. Communicated in a Letter from Dr J. Bevis.* p. 171.

I fancy I can now satisfy your curiosity as to a place in the northern limit of the path of the moon's shadow in the eclipse; that is, where the lower limbs of the sun and moon coincided, by the following abstract of a letter from Mr. Mungo Murray of Chatham, a good mathematician, and author of an excellent work on ship-building.

"I am infinitely obliged to you for your kind present of the telescope glasses. I got them most curiously mounted, and, as you said, they make a 12-foot telescope, which takes in the whole sun nearly. I set my watch by a very good vertical sun-dial, precisely at 9 o'clock, and at 8 minutes after I perceived the moon just enter on the sun. About half an hour after 10, the eclipse was barely annular, the light of the sun below the moon being but just visible, and less than a hair in the telescope. At 55 minutes past 11 the eclipse ended, and left the sun quite round."

By this, sir, I think you may safely conclude, that Chatham was not much more than a mile (perhaps less) south-east of the limit; which therefore passed over Rochester-bridge, or very near it.

XXXII. Observations and Experiments on different Extracts of Hemlock. By Michael Morris, M. D., F. R. S. p. 172.

Dr. M. here states that Dr. Wade, an eminent physician at Lisbon, having communicated to the London Medical Society, a number of cases, in which the extract of hemlock, prepared at Coimbra in Portugal, had been given with extraordinary success, and having sent him at the same time specimens of the successful extract, and also of the extracts of hemlock prepared at Lisbon, and by Dr. Storck's apothecary at Vienna, which 2 last-mentioned extracts he had prescribed for the space of 3 years, in various disorders, to little or no effect; he thought an experimental inquiry into the component parts of these extracts and that used in London might be attended with some useful or curious consequences: more especially as this medicine was near losing its credit entirely, from its little success here, in those disorders in which it had been most strongly recommended by Dr. Storck. He thinks it not unnecessary to premise further, that the extract prepared at Coimbra is not so moist as the other extracts, and that it has been given for a considerable time at the dose of ½ scr. twice a day without producing the least disagreeable symptom.

Exper. 1.—24 gr. of the extract of hemlock prepared at Coimbra, digested with 1 oz. of highly rectified spirit of wine for 36 hours in a warm room, gave a brownish yellow tincture; the clear liquor being poured off, a fresh quantity of spirit was added as before, and exposed to digestion for the same space of time; the 2d tincture was considerably less coloured; this, added to the former tincture, was filtered, and exposed to the air in a warm room till the spirit was entirely evaporated: the dry residuum weighed 5 gr.; on exposing it to the air it became softer, and even moist at the surface. On pouring some water on the residuum now moist, it was soon tinged of a brownish yellow; which being poured off, and a fresh quantity added at different times, till 1¼ oz. of water had been used, there remained some blackish matter not soluble in water, which when dry weighed 1 gr., did not attract the moisture of the air, melted and burned with a bright flame when exposed to the fire, was soluble in spirit of wine, and had every characteristic of a resin. The tinged water, which had been separated from this resin and filtered, was evaporated slowly, till a brown dry matter remained, weighing 3 gr., which in a few hours attracted the moisture of the air, and relented into a dark brown thick liquor, of a saline taste, and the smell peculiar to the extract of hemlock. One drop of this liquor, diluted with a little water, destroyed the colour of 10 times the quantity of syrup of violets, without giving it the least red tint; reducing it, on adding some drops of oleum tartari per deliq. it suffered no remarkable change. Spirit of salt did not occasion any alteration in it. But with oil of vitriol there was a strong effervescence, without any sensible fume.

It appears from the above experiment, that the Coimbra extract of hemlock contains ⅕ soluble in spirit of wine, ⅘ of which consist of an oily essential salt, the remainder being a resin.

Exper. 2.—The extract of hemlock from Vienna was softer than that from Coimbra; on breaking it, there appeared small whitish streaks on each surface. 24 gr. of it, treated as in the former experiments with spirit of wine, gave a fine deep green tincture, which on evaporation gave a residuum of a dark green towards the edges of the cup, and a dark brown towards the middle; the whole residuum when dry weighed 2¼ gr.; on leaving it exposed to the air, the brown matter attracted moisture from it, and relented into a thick brown liquor; on adding water to it, as in the experiments on the Coimbra extract, the solution was of a light green colour; on evaporation it gave ¾ of a grain of dark brown residuum, which ran per deliquium into a brown liquor, differing only in colour from that obtained by a similar process from the Coimbra extract. The undissolved resinous matter weighed ½ gr., was of a greenish colour, but in other respects like the resin of the Coimbra extract. It appears from the green tincture communicated both to water and rectified spirit by the Vienna extract, that the hemlock had been gathered too soon, and before the plant was in vigour.

Exper. 3.—The spirituous tincture of the Lisbon extract was not so green, nor was the green so durable as that of the Vienna extract: the phenomena, in consequence of the other experiments, did not differ materially from those of the Vienna extract.

Exper. 4.—The spirituous tincture of the extract of hemlock prepared at the Apothecaries' Hall, was like in colour to that of Coimbra, but the residuum did not differ considerably from that of Vienna and Lisbon. This extract has been used with some success at the Westminster Hospital.

Exper. 5.—The spirituous tincture of the powdered leaves of hemlock was like in colour to the last; the residuum differed materially from that of the former extracts only in its resin being considerably more fluid.

These experiments show, that the extract of hemlock prepared at Coimbra contains a far greater quantity of an essential oily salt and resin, than the other extract. As the oils, salts and resins are the most active parts of vegetables, may not the well-attested salutary effects of the Coimbra extract be owing to its greater quantity of these active principles, particularly if we consider the large dose it has been prescribed in? As these active oily salts and resins are soluble in spirit of wine, we have the means of obtaining them from the extract of our own hemlock in sufficient quantities for use, and without fatiguing the stomach with the nauseous inactive parts of the extract. But as experience alone can show whether the virtues of the hemlock reside in the whole extract, or in the saponaceous parts soluble in spirit of wine, he contents himself with proposing these few hints, until experiments shall enable him to lay the other consequences of these assays with proper weight before the Society.

XXXIII. On the Use of the Ganglions of the Nerves. By James Johnstone, M.D. p. 177.

This original and ingenious essay may be consulted in an enlarged form, in this author's collected works, published under the title of Medical Essays and Observations, 1795.

XXXIV. Of several Fiery Meteors seen in North America. By John Winthorp, Esq. at Cambridge, in New England. p. 185.

On June 3, 1739, as Mr. W. was walking over the common in this town (Cambridge) about 10 o'clock in the evening, the moon, which was newly past the first quarter, shining bright, and but few clouds to be seen, he was on a sudden surprised with 4 or 5 flashes of light, succeeding each other as quick as possible. This he at first took to be lightning; but, looking up, presently discovered the cause of it, which was a large meteor moving almost in the meridian from south to north. The body of it was very bright, and left behind it several sparks or lesser balls of light. When he first saw it, it was not far from the zenith; whence it moved, not very swiftly, till at about the height of 30° above the horizon it expired. In about 2^m, it was followed by a hollow rumbling noise, pretty loud, and so much like remote thunder, that several persons in their houses, who did not see the meteor, took it to be thunder, as others within doors, who saw only the flash, and not the body of the meteor, thought it lightned. But as there was no thunder nor lightning before or after, nor any clouds likely to produce them, he questions not but this report was occasioned by the explosion of the meteor. And this is confirmed by the great extent of this sound, which was heard in several places above 80 miles distant from each other. And hence, as well as from the length of time between the light and the noise, it may be collected that the meteor must have been very high in the atmosphere.

2. A meteor was seen on the 24th of November 1742, in the southern parts of New England. In New Haven, in Connecticut, one man saw a ball of fire about 4 or 6 inches in diameter, passing along from the south-west to the north-east, and a stream of white, bright, and clear fire followed it, of nearly the same size, and of considerable length. Then the ball broke into sundry small pieces, and vanished with a kind of flash; and a full minute after he heard a noise, much like that of rumbling thunder, and about as long again as a clap of thunder usually is.—Sundry people at Rehoboth, in this province (Massachusets) saw a ball of about a foot diameter, toward the west from them, and it fell to the ground.—At New London, in Connecticut, the stream of fire appeared in the north or north-west; and some who were off at sea, near New London, took the noise to be from great guns at New London battery.—Mr. Clap observes, that though the informations he had received differ as to particular circumstances, thus much in general seems to be certain, that people in most, if not all the towns between Norwalk, near the west end of Connecticut, and Braintree near Boston, which is at least 200 miles, heard an unusual noise in the air, like thunder or the discharge of a cannon; and sundry people, in most places, about a minute or more before the noise, saw a ball or stream of fire in the air, moving in some form or other.

3. A meteor was seen on the 4th of May 1760, at Newfoundland. The deposition of James Cawley, master of the sloop Content, taken before Michael Gill, Esq. one of his majesty's justices of the peace for the district of St. John's, Newfoundland, says, that coming from the banks of Newfoundland for this harbour of St. John's, being Sunday the 4th instant, about a quarter before 12

o'clock at night, being calm and the weather very clear and fair, then, near the mouth of this harbour, a sudden light shined, at which time we saw a fiery comet or meteor in the air, at first appearing in the shape of a flask or Florence bottle, which as it came nearer to us still increased in magnitude, making the air very hot, shooting from the northward to the south-west; sparks of fire darting from it, the size of a man's fist. It came very near us before it disappeared, when it seemed to be of the size of a ship's boat, with a long tail extending from it, attended with a noise like thunder. As it came near the water, the body appeared as black as pitch, and then vanished; the tail remaining some minutes before it disappeared.

The testimony of Richard King, saith, that coming from Kitty-Vitty to St. John's on Sunday the 4th of May, between the King's Bridge and the garrison, he saw towards the garrison as if there was a star shooting, falling rapidly, but only larger. It was as large as a man's head, and just before it came to the ground it broke all to pieces, which made like large sparks of fire flying from it; and in that time it was as light as ever he saw all day, and in less than 2 or 3 minutes there was a rumbling noise in the air, something like thunder. Several other persons in St. John's were prodigiously surprised at the same light.

XXXV. Some New Properties in Conic Sections, discovered by Edward Waring, M.A., Lucasian Prof. Math. Cambridge, and F.R.S. From the Latin. p. 193.

THEOR. 1.—Let the ellipsis APBQCRDSET &c. (fig. 1, pl. 4) have described about it the two polygons *abcdef* &c, *pqrstv* &c, having the same number of sides, and which bisected by the points of contact, APBQCRDS &c. i. e. making $aA = Ab$, $bB = BC$, $cC = Cd$, &c. $pP = Pq$, $qQ = Qr$, $rR = Rs$, &c: then will the sum of the squares of every side of the one polygon, be equal to the sum of the squares of every side of the other polygon; i. e. $ab^2 + bc^2 + cd^2 + de^2 + ef^2 +$ &c. $= pq^2 + qr^2 + rs^2 + st^2 + tv^2 +$ &c.

Corol. Draw the lines AB, BC, CD, DE, EF, &c. PQ, QR, RS, ST, TV, &c. then will $AB^2 + BC^2 + CD^2 + DE^2 + EF^2 +$ &c. $= PQ^2 + QR^2 + RS^2 + ST^2 + TV^2 +$ &c.

THEOR. 2. The same being supposed, let O be the centre of the ellipse, and draw the lines OA, OP, OB, OQ, OC, OR, OD, OS, &c. then will $OA^2 + OB^2 + OC^2 + OD^2 +$ &c. $= OP^2 + OQ^2 + OR^2 + OS^2 +$ &c.

Corol. Also draw the lines *oa*, *op*, *ob*, *oq*, *oc*, *or*, *od*, *os*, &c. then will $oa^2 + ob^2 + oc^2 + od^2 +$ &c. $= op^2 + oq^2 + or^2 + os^2 +$ &c.

These are also true of polygons in like manner described between conjugate hyperbolas.

THEOR. 3.—Let MPQRST &c. (fig. 2) be a conic section, whose diameter is AL, and its ordinate ML; and let $Mp = Mv$, and consequently $Lp = Lv$. Draw the lines pq, qr, rs, st, tv, &c. touching the conic section in the points P, Q, R, S, T, &c: then will the content $pP \times qQ \times rR \times sS \times$ &c. $= Pq \times Qr \times Rs \times St \times Tv \times$ &c; or, which is the same, the sum of all these ratios, $Pp : Pq$, $Qq : Qr$, $Rr : Rs$, $Ss : St$, &c, will be equal to nothing.

Corol. Let the ellipse PQRSTV &c. (fig. 3) be circumscribed by any polygon $pqrstuw$ &c, whose sides touch the ellipse in the points P, Q, R, S, T, V, &c: then will the content $pP \times qQ \times rR \times sS \times tT \times vV$ &c. $= Pq \times Qr \times Rs \times St \times Tv \times Vw \times$ &c.

Corol. Draw the lines PQ, QR, RS, ST, &c; and for the sines of the angles wPp, QPq, RQr, QRr, SRs, TSt, &c. write respectively a, p, b, q, c, r, d, s, &c: then will $abcd$ &c. $= pqrs$ &c.

And the same of polygons inscribed between the conjugate hyperbolas.

The same is true of a polygon, of which the sum of the sides or the area is a minimum, described about any oval always concave in itself, as appears by the Miscell. Anal.

THEOR. 4.—Let the ellipsis PAQBRCSDTEVF &c, (fig. 4) be circumscribed by the two polygons $abcdef$ &c, $pqrstu$ &c. having the same number of sides; their sides ab, bc, cd, de, ef, &c, pq, qr, rs, st, tv, &c, respectively touching the ellipsis n the points A, B, C, D, E, F, &c, and P, Q, R, S, T, U, &c; and let $aA : Ab :: pP : Pq$, and $bB : Bc :: qQ : Qr$, and $cC : Cd :: rR : Rs$, and $dD : De :: sS : St$, and so on: then will the area of the polygon $abcdef$ &c, be equal to the area of the polygon $pqrstv$, &c.

Corol. Two parallelograms, $abcd$ and $pqrs$, described about the conjugate diameters (AC and BD, PR and QS) (fig. 5) will be equal to each other. For in this case $aA = Ab$, $bB = Bc$, $cC = Cd$, $dD = Da$, and $pP = Pq$, $qQ = Qr$, $rR = Rs$, $sS = Sp$; consequently $aA : Ab :: pP : Pq$, and $bB : Bc :: qQ : Qr$, and so on: therefore by the theorem these two parallelograms are equal; which is the known property of the ellipse.

The same may be said of polygons in like manner described between conjugate hyperbolas.

THEOR. 5.—Let a conic section revolve about its diameter AL, (fig. 6) and let MAM′ &c. be the solid thus generated; let pq, qr, rs, st, tv, vw, wp, &c, be lines touching the solid in the respective points P, Q, R, S, T, V, W, &c: then will the content $pP \times qQ \times rR \times sS \times tT \times vV \times wW \times$ &c. $= Pq \times Qr \times Rs \times St \times Tv \times Vw \times$ &c.

THEOR. 6.—Let the ellipsis APBQCR &c. revolve about its diameter BD, (fig. 7) and about its conjugate diameters (AC and BD, PR and QS) be described the circumscribing cylinders $pqrs$, and $acbd$; these will be equal to each other.

Let there be two solids composed of truncated cones, circumscribing the gene rated solids, the sides of which are continually divided in the same ratio by the points of contact; these two solids will be equal.

And the same of solids in like manner described between conjugate hyperbolas.

Many other similar properties of conic sections will easily appear.

And such properties may be affirmed of infinite other curves, as may easily be deduced from the Micell. Anal.

XXXVI. On the Effects of Lightning at South Weald in Essex. By W. Heberden, M.D., F.R.S. p. 198.

On Monday June 18th, 1764, between 12 and 1, about 3 hours before the time when the thunder and lightning happened in London, by which St. Bride's steeple and Essex-street were damaged, there was a storm at South Weald, attended with uncommonly loud thunder. The lightning struck the weather-cock, and passing along the iron bars, on which it stands, rushed against the walls of the turret, and broke a space from the top of the turret to the leads of the tower, about 4 feet wide, being about one-third of the circumference of the turret and facing the north. The weather-cock and irons that support it were unhurt. The walls of the turret were made of rough stones and mortar: and part of what was beaten down fell on the leads of the tower underneath, and part on the roof of the church, which was greatly damaged. The stair-case also, which leads up to the turret, was so full of the stones and mortar, that it was with great difficulty and some hazard that any one could go up it. From a leaden spout at this west end of the church, which only comes down to near the top of the west window, the plaster was beaten off the wall for some inches in breadth quite to the window; and at the bottom of the upright iron bars of this window several of the stones were cracked, and the wall chipped here and there from thence to the ground. The same is observable in the stones at the bottom of the upright iron bars in the east window, which was also near a leaden spout that comes down from the roof over the chancel, the end of which rests on a buttress, and does not reach the ground by several feet; which buttress is cracked, as well as the adjoining wall. On the inside of this wall, within the church, there is a large wooden frame, which holds the commandments. This frame at the left hand corner is supported by an iron holdfast driven into the wall, which was mentioned above as being cracked on the outside under the leaden spout. The plaster of the wall, for 3 or 4 inches all round this holdfast, within the church, is beaten off; and to the left hand there is a space, slanting from the holdfast toward the ground, 5 or 6 inches wide and 3 or 4 feet long, from which all the mortar is forced away. That part of the wooden frame where the holdfast is fixed is shattered. The canvas, on which the commandments are painted, which was in this wooden frame, is torn from the frame on the two sides of it next the holdfast, and is rent besides in several places.

The whole appearance of the damage done to this church very much favours the conjecture of Dr. Franklin, who thinks it probable, that by means of metallic rods or wires, reaching from the roofs to the ground, any buildings may be secured from the terrible effects of lightning.

XL. Observations on the Effects of Lightning, with an Account of the Apparatus proposed to prevent its Mischiefs to Buildings, more particularly to Powder-Magazines; being Answers to certain Questions proposed by M. Calandrini, of Geneva, to Wm. Watson, M.D., F.R.S. p. 201.

M. Calandrini's questions are as follow: 1. What sort of apparatus is used at Philadelphia? 2. Whether there is not some improvement to be made to their methods; 3. In what manner this apparatus may be adapted to powder magazines? 4. Into what place the thunder may be conducted, where there is no river near, to answer the purpose of the sea about ships? 5. Whether the apparatus might not electrify the air, so as to occasion lightning, which was, he believes, the cause of the death of professor Richmann of Petersburg? This apparatus may not be dangerous to dwelling houses, where the fire may slip without any manner of risk; but may be attended with the most dreadful consequences to a powder magazine, where the smallest spark may occasion the explosion of the whole. 6. Whether the square, or the circular form of building, will be easiest adapted to the apparatus? 7. Whether an iron bar fixed on the top of the building, to support a weather-cock, may not attract the thunder-bolt, and be consequently dangerous to all buildings; but more especially to powder-magazines? Whether there is not some particular manner of buildings, invented of late, adapted to powder magazines; either to diminish the shock of the explosion, or to secure them against any accident, by the methods used at Philadelphia.

M. Calandrini says further, that he himself has been eye witness of the effects of lightning coming into a room, which had received much damage from it. That he looked for the place it went out at, and after long search perceived that it had followed the wire of the bell, which had conducted it through a very inconsiderable hole into the next room; whence it had opened itself a passage into a back yard. This accident was at that time thought very extraordinary, being anterior to Dr. Franklin's experiment.

To M. Calandrini's questions Dr. W. sent the following answers:

1. The apparatus used at Philadelphia consists either of a long iron rod, placed on the highest part of a house or other building; or of a shorter rod, inserted into a long wooden pole, placed in the same manner. The iron rod, mentioned

by Mr. Kinnersley in the Phil. Trans. Abridged, vol. xi. p. 702, and which probably preserved the house in Philadelphia on which it was placed, extended in height about 9½ feet above a stack of chimneys, to which it was fixed; but he supposes that 3 or 4 would have been sufficient. These rods are pointed at their upper extremity. It is indifferent which of these two is used, provided they are high enough to reach above the chimnies, or any other part of the edifice. Connected to, or suspended from, the metal of these, a metallic wire, generally of iron, is conducted in the most convenient manner to the nearest water, viz. to the well of the house, or any other water in the neighbourhood.

2. This method, wherever it has been employed, has hitherto perfectly answered the intention; no house in Philadelphia, or in any other place I have heard of, having suffered from the effects of lightning, where this apparatus has been erected. The improvements I should recommend would be, first, that as iron wire soon becomes rusty, and when rusty to the centre is unfit for the present purpose; and as brass wire is, when long exposed to the weather, exceedingly brittle and liable to snap asunder, the wire should be of copper; and of a size not less than that of a large goose-quill. Secondly, I prefer its being conducted, from the rod at the top to the water below, on the outside of the building, and thus prevent the lightning from coming within the building. On houses where there are gutters and spouts of lead to carry off the rain, the wire need only be conducted to the lead of the gutters; and attention be had that the gutters and the spouts coming from them are in their whole length in contact, or very nearly so, one with the other. If the leaden spouts do not reach to the bottom of the building, a slip of lead, such as is employed for the gutters, and about an inch wide, should be fastened to the bottom of one or two of the spouts, and conducted to the water. If such a slip of lead was to be conducted from the rod at top to the gutters, it might with equal advantage be substituted for the copper wire: or further, a slip of lead of this kind may be connected with the rod at the top of the house; and where there are no leaden gutters or spouts, may be conducted on the outside of the house down to the water, as before mentioned. I would recommend likewise an increase of their number; as the effects of one apparatus of this kind can extend only to a certain distance, and that to no great one; and the security, where mischiefs from lightning are frequent, must arise from their number. In countries and places so circumstanced no house or other building should be without one at least; large edifices ought to have several. The number should be in proportion to the size of the building.

3. In powder magazines I should recommend the apparatus to be detached from the building itself; and to be only placed as near it as might be. Powder magazines should never be constructed so as to cover a large quantity of ground. If security from lightning was considered in their construction as a considerable

object, I should recommend a circular building; in the periphery of which should be placed storehouses sufficient in their number and extent to contain the quantity of powder proposed. In the centre of this circle should be a well, very near which should be erected a pole or mast, high enough to reach some feet above the buildings of the powder magazine, or the buildings in its neighbourhood. From this mast there should rise a brass rod, 5 or 6 feet in length, an inch in thickness, and ending in a point; and from this rod a wire of copper of a size not less than that of a large goose quill, should be conveyed down the mast, and terminate in the water of the well. If there is no well, the wire should be laid into the nearest water; as the expence even of some hundred yards of a wire of this sort can hardly be considered as an object in an affair of this importance. For though there is reason to believe that the wire communicating with the ground would prevent the mischiefs of a thunder-cloud, which came near an apparatus of this sort; yet as water is a more ready conductor than the ground, it should, if possible, be insisted on in this particular case, and employed. Mr. West's apparatus, described by the before-mentioned Mr. Kinnersley, terminated in an iron stake, driven 4 or 5 feet into the ground; yet the earth did not conduct the lightning so fast but that, in a thunder-storm, the lightning was seen to be diffused near the stake 2 or 3 yards over the pavement, though at that time very wet with rain. It is presumed, that had this iron stake been placed in water instead of earth, the lightning had not been visible, on account of the water's receiving the electric matter more readily than earth. Where this apparatus therefore is applied to powder magazines, it should certainly terminate in water. At Mr. Hamilton's at Cobham, about 20 miles from hence, where an apparatus of this sort was erected on a high and greatly exposed building, as there was no water but at a great distance, the bottom of the wire was placed deep in a hill of moist sand. If instead of one wire, two, three, or more, were adapted to the brass rod in this manner, and conducted to the water, or if the brass rod itself was continued to the water, I should consider it, in extraordinary cases, as an additional security. This will explain my sentiments on the 3d, 4th, and 6th questions.

5. As the expectation of the utility of this apparatus is presumed to be the preventing of the accumulation of electricity in its neighbourhood, by affording a constant and easy passage to the electricity of the clouds surcharged with it, nothing in my opinion need be apprehended from the apparatus electrifying the air; as its principal operation is conceived to be the reverse of that, viz. divesting the air of its electricity. I am well apprized from experiments made here, that the earth is frequently electrified plus, and the clouds minus; and that this change of plus and minus between the clouds and earth are sometimes seen to vary several times in a quarter of an hour: but in that case it is presumed that

the clouds, within the sphere of action of the apparatus, have by its operation their electricity brought to the same standard with that of the earth in its neighbourhood, and vice versâ; and consequently that the mischiefs which might arise from the difference of the densities of the electricity in the earth and clouds are prevented, by the equilibrium between them being maintained. This subject, in regard to the electricity's being plus or minus, I many years ago considered, and laid my thoughts on it before the public, as may be seen in the Phil. Trans., vol. xlv. (Abridgement, vol. ix.)

That the atmosphere at times is very strongly electrified is evident, to say nothing of lightning, not only from our apparatus, but from the masts of ships, being beset with St. Elmo's fires, which I believe would scarcely, if ever happen, were the masts provided with an apparatus of this sort; unless the cause might be so great, and come on so fast, that the metal employed between the tops of the masts and the water might not, on account of the vastness of the cause, be large enough for the purpose. If it should so happen, St. Elmo's fires might still appear at the tops of the masts, and thunder clouds might burst near them, and exert their dreadful effects.* That even artificial electricity, when in too great a quantity, and hurried on too fast through a fine iron wire, has a remarkable effect on the wire, appears from a very curious experiment of Mr. Kinnersley of Pennsylvania. This gentleman, in the presence of Dr. Franklin, by his case of bottles being electrified fully, and made to explode at once, after the manner of the experiment of Leyden, through a fine iron wire, the wire appeared at first red-hot, and then fell into drops, which burned themselves into the surface of his table or floor. These drops cooled in a spherical figure, like very small shot, of which Dr. Franklin transmitted some hither to Mr. Canton,† who had repeated this experiment. This proves the fusion to have been very complete, as nothing less than the most perfect fluidity could give this figure to melted iron. These effects from artificial lightning, are exactly similar to those of the natural; as we have several times known iron wires, nails, and other metallic substances to have been melted, and parts of them, while hot, bedding themselves in wood by a thunder storm. Of this we had some instances here in a thunder storm, which happened in July 1759, of which the effects

* See more upon this subject, Phil. Trans., Abridgment, vol. x. p. 372.

† The diameter of a piece of Mr. Kinnersley's wire, which I received from Dr. Franklin, was one part in 182 of an inch. Artificial lightning from a case of 35 bottles, I find will entirely destroy brass wire of one part in 330 of an inch. At the time of the stroke, a great number of sparks, like those from a flint and steel, fly upwards, and laterally from the place where the wire was laid, and lose their light in the day-time at the distance of about 2 or 3 inches. After the explosion, a mark appears on the table the whole length of the wire; and some very small round particles of brass may be discovered, by a magnifier, near the mark; but no part of the wire itself can be found. J. CANTON.—Orig.

were communicated to the public in the Phil. Trans. vol. li. (Abridgment, vol. xi.) As metal has been made red hot, and melted, by artificial lightning, how much greater must be presumed to be the effects of the natural; and how much larger ought to be the metallic part of the apparatus, to avert its mischief? This requires particular attention.

7. I was of opinion, that iron bars to support weather cocks, if they were placed on the tops of buildings made of brick or stone, and in contact with either of these materials, were not dangerous to ordinary buildings on the account you mention, except in very particular and extraordinary cases; as these substances, when not much heated, conduct the electric matter in a very considerable degree. But what lately happened to St. Bride's steeple, as well as the mischief to South-Weald church on the same day, evinces that the apparatus, usually applied to weather-cocks, should never be trusted in any building, without a metallic communication from them to some water, or at least very moist ground. St. Bride's steeple, one of the most beautiful in London, was on Monday, June 18, about 10 minutes before 3 in the afternoon, very greatly injured, in one of the most severe thunder storms which ever happened here. From as attentive an examination, as the steeple at the present will admit of without scaffolding, it appears to me, that the weather-cock and its apparatus had the principal share in occasioning the great mischief done to the upper part of the steeple. I am of opinion, that the lightning first took the weather-cock, and was conducted, without injuring the metal or any thing else, as low as where the large iron-bar or spindle, which is inserted into the top of the steeple, and comes down several feet of its length, terminates. There the metallic communication ceasing, part of the lightning exploded, cracked and shattered the obelisk, which terminates the spire of the steeple, in its whole diameter, and threw off at this place several large pieces of Portland stone, of which this steeple is built. Here it likewise removed a stone from its place, but not far enough to be thrown down. From hence the lightning seems to have rushed upon two horizontal iron bars, which are placed within the building, cross each other, to give additional strength to the obelisk, almost at its base, and not much above the upper story: here, on the north-east and east side, it exploded again at the end of the iron bar, and threw off a considerable quantity of stone. And here, for the sake of explanation, I must observe, that the spire of this steeple, where it rises above the bell tower, is composed of 4 stories, besides the obelisk placed over them. The lowest and 2d are of the Tuscan order; the 3d is Ionic; and the 4th or uppermost composite or Roman. The stone piers of these stories are connected together and strengthened by iron bars placed horizontally near the height of the capitals of the pilasters, and each story has only one set of these bars. From the cross bars near the base of the obelisk just mentioned, the lightning broke

through the roof above the composite story; at the ends of another set of iron bars placed lower than the former, from which it tore out a large portion of the stone. It then struck the iron bars of this story, which are placed immediately under, and in contact with the stones, broke one of the iron bars directly across, and bent the larger part of it from its horizontal direction to near an angle of 45°. Its rapid progress being here in some measure prevented, at the end of one of the iron bars, it threw off the upper part of one of the composite pillars just above its capital and a large portion of the cornice projecting over it, and that with such a force, that part of a stone which was placed here and formed a portion of the cornice, and weighed 72 pounds, was projected, not only the whole length of the body of the church, but beyond it, across St. Bride's lane; where it fell on the top of a house, and broke through the roof, and lodged in the garret. The horizontal distance from the steeple to the place where it fell, was at least 150 feet; the height from which it fell somewhat more than 200. This piece of stone was of a very irregular figure, and must have required an amazing force to rend it, detach it from the building, and throw it to such a distance. The shaft of the pillar, the next to the east of that whose upper part had suffered so much, was likewise violently struck; and a large portion of its diameter broken out and thrown down. The Ionic story has suffered considerably, more particularly the pilaster fronting the north-east, and placed directly under the composite column, whose top was thrown off. This pilaster is much injured, but the story in general has suffered less than the composite, and that chiefly where the irons are inserted; the upper Tuscan less than that, and the lower Tuscan but little, except in the north-east pier, which is considerably cracked and shaken; as if in its passage part of the force of the lightning was spent in these explosions, and part absorbed and conducted by the masses of stone. The damage done to the steeple is, except near the top, confined almost to the east and north east side, and most generally where the ends of the iron bars have been inserted into the stone or placed under it; and in some places, by its violence in the stone, its passage may be traced from one iron bar to another. And it is very remarkable, that to lessen the quantity of stone in this beautiful steeple, in several parts cramps of iron have been employed; and on these, stones of no great thickness have been placed, both by way of ornament and to cover the cramped joint. In several places these square stones have, on account of their covering the iron, been quite blown off, and thrown away. A great number of stones, some of them large ones, were thrown from the steeple, 3 of which fell on the roof of the church, and did great damage to it; and one of these broke through the large timbers which form it, and lodged in the gallery.

In the tower of the steeple, in the room where the bells are placed, the lightning took the south-west window above the bells and close to the window, not far from an iron bar, which goes round, and rent out several large stones; some of which fell into the bell, which was very near this part of the steeple, and was the largest in the steeple; and passing below the bell, tore out at another place, in a line with the former, a great number more. One of the stones, torn out above the bell, was thrown to the north-east side of the tower. Between the two places, in which the lightning had here exerted its fury, the wooden block, which confined the axis of the frame of the great bell, and was fastened down with two iron staples, was thrown off, and the staples tore out. No damage at present seems done to the bell.

It is remarkable, that less than 20 years ago, one of the stones of the obelisk of this steeple was observed to be moved from its place, and project some inches over those under it. This stone was about 7 feet from the top of the obelisk. Danger being apprehended from this state of the spire, it was taken down to the place where the stone was removed, and rebuilt with new stone. This accident at that time was supposed to be owing to the ringing of the bells; but it is highly probable, from what has lately happened, that as that stone was removed from its place, very near to that part of the spire where it is now cracked and shivered quite across, and several pieces of stone thrown down, it was owing to the same cause as the present damage, viz. lightning, though not at that time adverted to.

The lightning on June 18 came from the west and south west; the damage done both to St. Bride's church and South Weald, was on the east and north east sides, except that in the bell-loft at St. Bride's. The stones, both from the steeple of St. Bride's, and in its tower, were thrown to the east and north-east.

Since the communication of this paper to the Royal Society, the steeple of St. Bride's has been surveyed, and found so very much damaged in several of its parts, that 85 feet have been taken down, in order to restore it substantially. Within these 85 feet are comprehended the obelisk, placed at the top of the steeple, the small dome immediately under it, the space between that and the uppermost or composite story, the composite story, and the Ionic story. This last, on the east and north sides, was taken down to its bottom; but on the other sides, as they were not injured, some parts were permitted to stand. Three piers were likewise taken down of the second Tuscan story, and one pier of the first. The scaffolding to take this down and rebuild it enabled me minutely to examine, not only the damages occasioned by the lightning, but the manner of its progress. This examination confirmed the opinion of the cause and manner of this accident, which I communicated to the Royal Society soon after it happened; and before a near inspection could be obtained. It completely indicated the great danger of insulated masses of metal to buildings from lightning; and on the contrary evinced the utility and importance of masses of metal continued, and properly conducted, in defending them from its direful effects. The iron and lead employed in this steeple, in order to strengthen and preserve it, did almost occasion its destruction: though after it was struck by the lightning, had it not been for these materials keeping the remaining parts together, a great part of the steeple must have fallen.

The operation and progress of the lightning, in the obelisk and upper parts of the steeple, deserve more particular attention. To form a more perfect idea of these, the following measures will in some degree contribute.

	Feet	Inches.
The height of the octagonal obelisk	22	3
Length of the iron spindle	19	9
Thickness of the spindle, where inserted into the stone	0	2 square
Its length inserted into the stone	9	10
From the bottom of the spindle to the first cramped joints	5	10
Three courses of stones without cramps	5	7
From the bottom of the spindle to the first concealed chain	11	5
From the first concealed chain one foot above the base of the obelisk to the first cross chain	2	0
From the first cross chain to the 2d, placed in the dome	8	10

The vane, the cross above it, the ball and its socket, which covered so much of the spindle as arose above the stone, to near 10 feet of its length, were of copper gilt. This length of the spindle was cylindrical, but the other part was made square, where it began to be inserted into the stone. To fasten this spindle more securely in the courses of stone, melted lead had been poured. This lead, in the two lower courses of stone through which the spindle had passed, not only filled all the space left between the spindle and the stones; but had, as it were, ramified itself not only between the joints of the stones, but had insinuated itself in its melted state into all their small clefts and interstices. The spindle terminated in one stone, which occupied the whole area of the obelisk, and was 3 feet and near 2 inches in diameter, and one foot in thickness. Into this stone the spindle was inserted 5 inches of its depth, and fastened by melted lead. Under this stone the obelisk was hollow; but above it was solid, excepting the space left for the spindle.

On examining these several particulars, no injury had been done by the lightning to the vane, its cross, copper ball, or spindle. Of the 7 courses of stone at the upper part of the obelisk, and which were above the whole stone into which the spindle was inserted, the 5 upper courses, though connected together at top and bottom with iron collars soldered with lead, were not damaged; but the two stones which formed the 6th course were cracked, shivered, and fragments thrown from them. The 7th course consisted likewise of two solid stones. These were burst from the spindle, which was, by the intervention of the lead, connected with them, broken into many parts; each was moved from its place; some pieces were thrown down, and one large one projected 5 inches over the stone, immediately under it. The whole stone, into which the spindle was inserted, and on which it rested, was burst from the centre into a great many pieces, and every piece removed from its place. Some of these were thrown from the steeple. Several of the large masses of this stone, which still cohered, were very much shivered. The centre of the stone, on and near which the spindle rested, was beaten to powder, and a hole made through the under part of the stone. That this stone in this condition should still support the 7 courses above it, which weighed 4 tons, exclusive of the spindle, vane, and their appurtenances; and that the whole did not fall when struck with the lightning, is in no small degree surprizing. From the bottom of the spindle to the first course of stone, where the workmen had used iron cramps, the distance was 5 feet 7 inches. These cramps were bedded in the stone. Part of the lightning, from the bottom of the spindle through the hole just now mentioned, seized these cramps, and threw off large scales of stones at their ends. From these there were 3 courses of stone, in which there were no cramps; these suffered nothing. In edifices of this kind, for additional strength, the builders employ bars of iron, connected together in such a manner as their exigencies require; and these, though they have no links, are denominated chains. These are sometimes so adapted to the courses of stone as not to be visible, and are perfectly concealed: at other times, they are in part visible, and in part concealed. The first metal that occurred after the cramps before mentioned, was a concealed chain, one foot above the base of the obelisk, and 2 feet above the first cross chain. Here two stones were burst and shattered. In the course of stone where the first cross chain was inserted, and the several stones connected by iron cramps, many of the stones were much shattered. At the base of the dome, near 9 feet below the first cross chain, was a 2d. This chain was a double cross connected at its ends with a circle of iron, which was bedded into the whole course, and fastened by melted lead. Here the lightning made great ravage, burst and threw off the stones in which the iron circle was bedded, and tore out part of the roof of the dome, threw off two pieces of the cornice and one of the vases, which was contiguous to it. These 2 pieces of cornice weighed 1200 pounds. The courses of stone between the two chains, except those just now mentioned, were not injured.

To what is here said, I shall only add, that in no part the steeple was injured, except where the stones were in contact or very near the iron and lead employed in its building; and the quantity of stone burst, spoiled, or so much damaged as not fit to be used again, amounts, as I am informed by Mr. Stanes, a very honest and ingenious mason, who has contracted to repair the damage done by the lightning, to not less than 25 tons. An amazing quantity!

The abovementioned Mr. Stanes was employed a few years since, in the repair of the steeple of St. Mary le Bow in Cheapside, which was injured by a very rare and uncommon accident. At its erection, the builders had employed, near the top of the spire, for additional security, several iron cramps; the ends of which, by being exposed to the weather, became rusty, swelled, and so much enlarged, as to raise the stones above them, and to deflect the top of the spire 6 inches from the perpendicular. Danger being apprehended from this situation, the spire was taken down several feet of its length, and properly repaired. This ought to be a caution to succeeding builders, that if, in edifices of this kind, they find it expedient to employ cramps, they should be either of copper, which is not liable to swell by moisture; or, if iron be used, so much space should be left in the under bed of the stones, which immediately cover the cramps, that they may have room to extend themselves without danger to the building. This remark, though not immediately relating to our present purpose, will not, I hope, be thought impertinent in this place.

But to return: this thunder-storm had been preceded by several very warm days. The nights had scarcely furnished any dew: the air was quite dry, and in a state perfectly unfit to part with its highly-accumulated electricity without violent efforts. This great dryness made the stones of St. Bride's steeple, and all other buildings under the like circumstances, far less fit than if they had been in a moist state, to conduct the lightning, and prevent the mischief. For though this thunder-storm ended in a heavy shower of rain, none except a very few large drops fell till after the church was struck; and I have no doubt, but that the succeeding rain prevented many accidents of a similar kind, by bringing down with every drop of it part of the electric matter; and thereby restoring the equilibrium between the earth and clouds. It is frequently taken notice of in attending to the apparatus for observing the electricity of the clouds, that though the sky is much darkened, and there have been several claps of thunder at no great distance, yet the apparatus will be scarcely affected by it; but as soon as the rain begins, and falls on so much of the apparatus as is placed in the open air, the bells of the apparatus in the house ring, and the electrical snaps succeed each other in a very extraordinary manner. This demonstrates, that every drop of rain brings down part of the electric matter of a thunder cloud, and dissipates it in the earth and water; and prevents the mischiefs of its violent and sudden explosion. Hence, when the heavens have a menacing appearance, a shower of rain is much to be wished for.

From these considerations, I have no doubt, but that the mischief done to

St. Bride's steeple was owing to the efforts of the lightning, after it had possessed the apparatus of the weathercock, endeavouring to force itself a passage from thence to the iron work, employed in the steeple. As this must be done per saltum, there being no regular metallic communication, it is no wonder, when its force is vehement, that it rends every thing which is not metallic, that obstructs its easy passage; and in this particular instance, the ravages increased as the lightning to a certain distance came down the steeple. To procure this easy passage, and avert the ravage occasioned by the want of it, in future, as much as our present knowledge in these matters will enable us to do, I cannot sufficiently recommend metallic communications between the metal at the top and water, either as has been before mentioned, or in any other convenient manner, taking care not to be too frugal of the metal employed. This was first suggested by Dr. Franklin; and since much used in Philadelphia, and other parts of North America.

Near the same time that the mischief was done to St. Bride's church, the mast of his Majesty's ship Ramillies, lying at Chatham, was split and torn to pieces by the lightning. This is the less extraordinary, as, from its height, figure, and constituent parts, the mast of a ship stops the progress of lightning much more than edifices of the same height, made of brick or stone. This therefore seems to require particular attention; but on this head I fully explained myself in my letter to the late Lord Anson,* and shall therefore decline saying any thing further of it in this place.

I flatter myself, that what has here lately happened will tend to occasion the applying of an apparatus of this sort to all buildings, at least of value and extent. No steeple should certainly be without it; and in most, if the iron work of the weather-cock can be easily got at, it may be adapted with very little trouble or expence. It is only necessary to make a metallic communication between this iron work and the lead, which carries off the water. This frequently reaches to the ground or very near it. From the bottom of this, the metallic communication should continue to the nearest water, or at least to very moist ground; though where it can be procured, water should be preferred. Care must be taken likewise, that metallic communications be added to those parts of the lead which serve to convey the water from the top, as do not already touch or come near each other. And these may be either of lead or of copper wire, such as before mentioned. In thunder-storms attended with rain, sufficient in quantity to run off in streams, a great portion of the electric matter runs off in, and is dissipated by, these streams; and buildings are thus preserved from damage.

* Philos. Trans. Abridged, vol. xi. page 660.

What happened to St. Bride's cannot but give us some apprehensions for that noble edifice St. Paul's. This is above 100 feet higher than St. Bride's, and therefore more in the way of accident from thunder-storms. On its magnificent lantern is placed a cross of metal, which is inserted into the stone of the lantern; and this is supported by a truncated cone of brickwork, which arises from the arches of stone below. The cupola is covered with lead, which is continued to the spouts of the same material. These bring down the water to the stone gallery under the cupola, and end within about a foot of the stone. From hence the water is conveyed a considerable distance, in a stone trough or channel, to the leaden spouts; and these are carried down the building, and terminate, as I was informed on inquiry, in the common sewer. By this arrangement the metallic communication is interrupted. In thunder-storms during rain, the water carries off in its streams the electricity, as perfectly, as the most complete metallic communication would; but when there is no rain, it is otherwise; and these interruptions are the great cause of danger. To lessen which, as far as we can conclude at present, it would be expedient to make, by the means of several copper wires, small rods, or pieces of lead, a metallic communication between the gilded cross, and the lead of the cupola: and again, from the leaden spouts of the stone gallery to those which bring the water thence; care being taken that from the bottom of these last there should be a metallic communication, if there should be found to be none at present, with the water in the common sewer. Thus, without much expence, a complete metallic communication may be made between the top of St. Paul's church and the water; which had it been done at St. Bride's, the ravages so lately experienced had in all probability been prevented.

From considering the circumstances of this thunder-storm, I cannot but be of opinion, that the injury done to St. Bride's prevented mischief to St. Paul's. St. Bride's is a very high building, and within a small distance nearly west of St. Paul's. When this distance is considered, and that the lightning came in the direction of St. Bride's to St. Paul's, and that when the thunder-cloud came near the former, it exploded there, and parted with much of its force; what was left did no damage to the latter, though the much higher and more exposed building, and having a metallic cross at its top.

I have recommended as metallic conductors copper wires of the size of a goose quill; as, when of that thickness, they may easily be bent to any direction; and, where thought necessary, any number may be employed. I consider this as a kind of standard, from what Dr. Franklin wrote to Mons. Dalibard of Paris on this subject.* He observes, in a church which suffered greatly by lightning

* See Phil. Trans. abridged, vol. x. p. 632.

at Newbury in New-England, that though a small wire was beaten to pieces by lightning, and dissipated by its force, the rod of a pendulum conducted the whole without being melted or otherwise injured by it; and that, great as the quantity was in this instance, and which utterly destroyed the small wire, no damage was done to the building, as far as the small wire and the pendulum of the clock extended: and in the remarkable instance mentioned by Mr. Kinnersley in his* letter to Dr. Franklin, where a brass wire of about 2 lines thick, 10 inches long, and terminating in a very acute point, was inserted into the iron rod, about 2 inches and half only of its top were melted by the lightning; the remaining part of it transmitting the lightning without being fused by it.

You will observe in this disquisition, that I have no where mentioned the apparatus attracting the lightning. I have avoided introducing the term attraction here, operating as an active principle; as I consider the apparatus purely passive, and only affording, from the aptness of its parts to that purpose, an easy and uninterrupted passage to the lightning, and thereby preventing its violent efforts.

You will pardon, Sir, this long digression in relation to St. Bride's church; as it gives so positive and explicit an answer to part of your 7th question; such a one as could not, without the late thunder-storm, have been furnished, at least from hence: to wit, that without a proper apparatus, weather-cocks placed at the tops of any buildings are dangerous to them in thunder-storms; but more especially to powder magazines. The accidents which have lately happened to St. Bride's and South Weald churches, if considered as great electrical experiments, furnish very important, and, I flatter myself, useful conclusions. They are too hazardous and expensive however, to wish to see often repeated.

If the erecting of an apparatus of this sort should become general in countries where thunder-storms are frequent, and often attended with mischief, though damage should be really averted by it, the operation of the apparatus would be unseen, and therefore unknown, unless in such rare instances as that mentioned by Mr. Kinnersley. To make its effects apparent, as has been hinted to me by Dr. Heberden, a very deservedly eminent physician here, if chains are employed as metallic communications, instead of wires or rods, whenever the lightning comes near enough to affect the apparatus in a considerable degree, it will, without mischief, be visible in the dark, by its sparkling and snapping in its passage, at the links of the chain. The effects of the apparatus may be observed in another manner. If the metallic communications are by the means of a wire

* Phil. Trans. abridged, vol. xi. p. 702.

or single rod, there may be, in some parts of its length, in any place convenient for observation, a space left where the metal is discontinued; but this space should not exceed 2 inches. The two extremities of the metal at this interruption should be furnished with brass knobs not less than an inch in diameter. By this method, though the effects of the apparatus would not be considerably lessened, they might be observed. For at times when no lightning was visible, but when clouds replete with it came near the apparatus, or rain from them fell upon it, there would be a snapping from one of the brass knobs to the other. When indeed the lightning was near, there would not only be this snapping, but, if the cause was great, a stream of fire would be seen, as in M. Romas's kite,* to pass from one of these to the other, as the best and nearest conductor. If danger however is apprehended, a piece of chain may be always at hand to be hung occasionally on the upper knob, so as readily to fall in contact with the lower. Otherwise, if while the metallic communication is divided, though when entire it is apprehended it may be touched with safety, a person should touch the rod above the division, and at the same time touch or come very near the rod below the division with any part of his body; and at the same instant if a smart stroke of lightning affected the apparatus, he would certainly be destroyed, as happened to professor Richmann at Petersburgh; the lightning going through his body from one part of the apparatus to the other, which it is believed it will not do while the metallic communication is compleat.

8. I have not heard that there has been here of late any particular mode of buildings, adapted to powder magazines, to diminish the shock of the explosion in case of accident: nor do I believe that any attention has been here given, in constructing these buildings, to prevent, by an apparatus of this kind, the effects of lightning.

XLI. An Account of the Effects of Lightning on St. Bride's Church, Fleet street, on the 18th of June, 1764. By Edward Delaval, Esq. F. R. S. p. 227.

The construction of this spire is somewhat similar to that of an apparatus purposely contrived to draw the lightning from the clouds, as it runs up towards a point, and ends in a metal vane and cross, the figure of which, as well as the materials they consist of, seem calculated to admit the lightning with the least resistance. The first marks of it are seen at the top of the copper cross, which is the highest part of the building, the gilding is by the explosion partly torn off and partly discoloured, so as to differ remarkably from the rest of the cross where the gilding is very well preserved. Some small pieces of solder are

* Phil. Trans. abridged, vol. xi. p. 580.

melted; and all this part appears as if it had been exposed to the fire. The lightning seems to have entered here, and to have been conducted thence by an iron spindle 20 feet in length, and 2 inches in diameter; of which 10 feet were surrounded by the copper ball, vane, and cross; and the lower half was inclosed in a groove cut through the middle of the solid stones which composed the upper part of the spire, and rested on the bottom of that groove, which was sunk 5 inches deep into the lowest of those solid stones: this last-mentioned stone being 3 feet broad and 1 deep. The interval between the sides of the spindle and the groove made to receive it was filled up by melted lead poured in between them.

The lightning accumulated in the metal, having its passage towards the earth strongly resisted at this place, has in expanding itself formed a hole, by bursting off from the lower part of the spindle the stones contiguous to it on that side. At each of the angles of the metal, the stone on which it rested is cracked, which probably was occasioned by the lightning issuing with greater freedom from those parts, than from the flat surface. No part of the spindle is in the least injured by the lightning, notwithstanding the great quantity which, from its effects, appears to have been accumulated in it.* From hence, as low as to the corniche, it seems to have been conducted along the surface of the spire, which was wetted by the rain that had fallen in the morning, before the lightning: and having been accumulated in the iron bars, in discharging itself from them, it has made the greatest explosion at this place.

Under this part the freedom of its passage seems to have been hindered by all the dry stonework underneath, which was defended from the rain by the corniches: and it appears from some experiments which I formerly made,† that dry freestone, when warmed to a certain degree (which probably does not exceed the heat which the stones of buildings acquire in hot weather) resists the passage of the electric fluid or lightning so strongly, that with plates of that stone, instead of glass, I performed the Leyden experiment. Under the corniche, the lightning descended only by leaping from one iron to another; and at every leap its force seems to have been weakened, and at last to have been quite dissipated.

On examining the inside of the steeple, beginning from the top, the first effect of the lightning that appears is a hole in the stone work, beginning immediately above an iron bar which served to support the top of the window or opening, and running upwards towards the two cross iron bars: this, when

* In the year 1750 the stones surrounding this spindle were so much damaged, that there was a necessity of taking them down and rebuilding that part of the spire. The cause of this was not known at that time: it is probable that it was occasioned in the same manner as the present accident.—Orig.

† Phil. Trans. abridged, vol. xi. p. 334.

viewed from the outside of the church, is seen to have spread round most of the lower part of the spire, so that it seems in great danger of falling. The next stroke is about 4 feet below: at this place 4 iron bars lie horizontally across the spire, and are tied together by chain bars which are inclosed in the stonework: where the end of one of the cross bars is inserted in the stone, the lightning has burst open a hole, and when the same is viewed at the outside, a great part of the corniche appears to be broken off. Where the two iron bars serving to support the top of the windows meet and are joined together, the lightning accumulated in them has broken off the pier by which they were inclosed. A bar of iron, which served to support the top of the window in the same manner as those last mentioned, 21 inches long clear of the stonework, and half an inch thick, is broken; and the stones immediately above it are shattered and disjointed. The sills of two windows of this story are torn off from iron bars which lay beneath them.

An iron bar, N° 1, about 25 inches long, was inclosed 9 inches deep in the stone-work of the pier, separating the east arch from the arch next it towards the north: the end of this bar joins at a right angle another bar, N° 2, which is laid across the arch. The lightning accumulated in the iron N° 1, which was inclosed in the stone-work, has burst off all the stone that surrounded it, and part of the pier adjoining. The flaw is continued downwards, meeting with smaller iron cramps in its way. At the next arch, lying immediately under the last mentioned one, an iron was inclosed in the stone in the same manner as the bar at N° 1: the stone is torn off from this iron exactly in the same manner as at N° 1: but the damage has not reached much farther than the stone which was contiguous to, and covered this bar. At the bottom of this arch the sill stone, which covered some cramps of iron, is torn off from its place. At the next arch under this, the force of the lightning seems to have been much diminished, a small part of one stone only being broken.

From the wall at the west side of the south window of the belfry some stones are thrown down: one chalky stone in particular is reduced into an impalpable powder, and the wall under the west window is almost covered with the powder: this stroke seems to have been directed towards the bells, one of which is very near the place damaged: the bells have not been examined; nor can they, it is said, without danger of shaking the spire by their motion. This is the lowest mark which is left of the effects of the lightning.

In every part that is damaged, the lightning has acted as an elastic fluid, endeavouring to expand itself where it was accumulated in the metal: and the effects are exactly similar to those which would have been produced by gunpowder pent up in the same places, and exploded. Among many other stones thrown to a considerable distance by these explosions, one weighing above 70 pounds was removed 50 yards eastward from the steeple, where it fell through the roof of a house. It is evident that these effects would have been prevented, if a sufficiently large metallic conductor had been extended from the metal at the top of the spire down to the earth, communicating with the other metallic parts of the building that lay in its way. Such a communication seems very necessary in buildings of this form. The iron bars which were fixed in the stone-work of the east arches, were struck by the lightning, while those in the arches fronting them on the west side of the same story remained untouched by it. So that probably a conductor communicating with the west arches only, would not have preserved the opposite ones from the damage they have suffered.

When such buildings are exposed to very large clouds replete with lightning, there is no reason to imagine that they will not convey some of their contents to other metallic parts of the building at the same time as to the metal at the top: for though the conductor may be large enough to convey to the ground, from the top, all the lightning that enters that part; yet one such small conductor cannot be supposed to exhaust those immense bodies so quickly, as to disable them from striking at the same time other buildings, or other parts of the same building. A wire, or very small rod of metal, does not seem to be a canal sufficiently large to conduct so great a quantity of lightning to the earth; especially when any part of it, or of the metal communicating with it, is inclosed in the stone-work: in which case, the application of it would tend to increase its bad effects, by conducting it to parts of the building which it might otherwise not have reached.

Dr. Franklin, from observing that the filleting of gold leaf on the cover of a book conducted the charge of 5 large jars, reasons that a wire will be sufficient to conduct the lightning from the highest buildings to the earth. But it appears from an experiment of his own, that a much larger body of metal, when inclosed between small plates of thick looking-glass, is not sufficient to conduct a 5th part of such a charge, without being melted, and bursting to pieces the plates of glass. And it is remarkable, that in those parts of the church where the effects of the lightning are most conspicuous, the iron was inclosed in a resisting substance similar to the glass surrounding the gold leaf in that experiment. Wires, instead of conducting the lightning, have often been melted by the explosion. So that, it seems a conductor of metal less than 6 or 8 inches in breadth, and a quarter of an inch in thickness (or an equal quantity of metal in any other form that may be found more convenient) cannot with safety be depended on, where buildings are exposed to the reception of so great a quantity of lightning. These are the only points in which I have ventured to differ from Dr. Franklin.

XLII. On the Effects of Lightning, in Essex-street, on the 18th of June, 1764. By Thomas Lawrence, M. D. p. 235.

The storm, which came from the south-east, broke first on the two houses at the bottom of Essex-street (which look from their south windows on the river) and beat down several feet of the east-flue of the chimney on the west side, and separated the remainder down to the roof of the house from the western flue by a wide crack. Hence the lightning went higher up the street, and at the distance of about 18 yards from the chimney just mentioned, went through the eves of a house, in a direction from the north-east to the south-west, as appeared by the breach, and forced the ceiling of the garret inward by a kind of pointed bulge, without breaking the laths. It continued up the street, perhaps along the leaden gutter, over the eves of the houses for about 30 yards, and it seems turned downward by the side of a leaden pipe made to convey the water from the top of the house, and tore a wooden case at the lower end of that pipe, cracked the wall near that place, and broke several panes of glass in the kitchen window next it. The wall that was cracked was blackened, and there was a strong smell of sulphur in the street.

On the east side of the street the lightning broke the south garret window of the bottom house, threw down the eastern flue of the chimney down to the roof of the house, and took away part of the western flue. The lightning seemed to have passed between the garret window and the chimney, as the window was damaged on the west side; but the chimney, which stood west of the window, on the east side. The tiles on the roof of both houses were broken, both on the south and north side in a deep furrow, as if a heavy plough had passed over them.

The house last mentioned has a door on the east side, which opens into a garden looking into the Temple; from this door there are several stone steps down to the garden. On the left hand of the steps is an iron rail. The lightning, conducted it seems by the rail, and from thence by the baluster at the top of the steps, struck off the corner of the stone step at that place, without any discolouring of the step; the piece struck off might be 3 or 4 pounds weight. Part of the lightning, conducted farther along the iron rail was carried by the descending baluster, and a large piece was struck off from the corner of the stone step at the bottom of the stairs. This iron rail is within 3 feet of a leaden pipe, which comes down from the top of the house, and is not continued to the ground.

The lightning went up the east side of the street without any effect, till, at about the distance of 70 yards from the bottom house, it struck the flag pavement near the iron rails of the adjoining house, and broke off a piece of the flag stone, weighing about 2 pounds; there was no discolouring here, but, as in the stone steps before mentioned, the appearance was as if the stone had been broken by the blow of a sledge hammer. One continued leaden gutter runs over the eves of these houses on the east side as well as on the west side.

XLIII. An Account of what appeared on opening the Body of an Asthmatic Person. By W. Watson, M.D., F.R.S. p. 239.

The appearances in the body of Mr. W., aged 28, who died of an asthma, were as follow:

On lifting up the sternum, the lungs were enormously distended with air, which no pressure could force back through the windpipe. This air was extravasate, had burst through the extremities of the bronchia and vesicular substance, and had insinuated itself throughout the whole substance of the lungs, in which it was detained by the membrane investing them. Indeed the whole substance of the lungs was in a state truly emphysematous. In several parts this air had formed large bladders, which, though no pressure on the surface of the lungs could force back, a slight incision into them permitted to escape, and caused the whole lobe to collapse. Besides this emphysematous affection of the whole substance of the lungs, the pulmonary vein was in all its parts distended into numberless varices, many of which were of the size of the small, or Lucca olive, and were distended with grumous blood. Besides these, there was a larger cyst in the right lobe of the lungs, which was filled with deep coloured ichor; this lobe adhered to the pleura in great part of its surface. The lungs in general were of a deep red colour, and here and there on their surface beginning to sphacelate.

The figure of the human heart is that of a cone, divided through its axis; but in this case, the heart's figure was altered, and was more compressed than usual; and its ventricles distended with grumous blood. Every other part of the body was in its natural state.

From this examination we find that in this instance respiration was greatly disturbed, to say nothing of the cyst in the right lobe, nor of the adhesion of that lobe to the pleura, from 2 manifest and potent causes, viz. the varicose state of the pulmonary vein, and the emphysema throughout the whole substance of the lungs. The varices of the pulmonary vein not only retarded the blood in its passage to the left auricle of the heart, but, occupying a much larger space in the lungs than they naturally should, they left less room for the minute ramifications of the bronchia to extend themselves; and consequently a less quantity of air was taken in at every inspiration than was necessary for the ordinary purposes of life. But the disorder of the lungs from the varices was made infinitely worse by the emphysema. For by the extravasated air possessing so large a portion of the lungs, and which the patient could by no means part with in expiration, very little room was left for fresh air in inspiration; the lungs, from the emphysema, and from the diseased state of the pulmonary vein, filling almost the whole cavity of the thorax. This not only occasioned an enormous defect in the quantity of air in inspiration necessary to the purposes of life, but, by the preternatural compression, the motion of the blood was retarded in the lungs, more especially in their smaller vessels. This affected not only the serous extravasation in the cyst before-mentioned, but occasioned those general obstructions in the blood vessels of the lungs which brought on the sphacelated appearance; and finally, by the increase of the complaint, was the cause of death. This extraordinary distension of the lungs also accounted for the heart being of a more compressed figure than is usually seen.

In the present instance, an asthma was occasioned by 2 causes, either of which has hitherto been scarcely considered as conducing to it; the one an emphysema, and the other a varicose affection of the pulmonary vein. Had the causes of this disease been as perfectly known during the life of the patient, as since his death, the case would not have admitted of a cure; as there was no method of discharging the extravasated air from the lungs, neither could any medical process alter or amend the varicose state of the pulmonary vein.

Such a state of lungs, as that just now described, in an otherwise healthy young man, could not, Dr. W. was persuaded, happen but from some very powerful cause; and, on inquiry, he was informed, that about the beginning of October, not 2 months before his death, from something which had greatly offended his stomach, he was seized with violent and long continued vomitings. These, though at length they were quieted, left his chest very sore. From this time his cough became troublesome, as did remarkably his shortness of breath on the least motion, attended with the several circumstances above described. From considering the history of this disease, and comparing it with the appearance of the lungs after death, he could not but be of opinion, that the violent efforts to vomit occasioned primarily both the emphysema, and the varices of the pulmonary vein. This opinion, he flattered himself, would not, to persons well versed in the animal economy, seem ill founded, when they reflected how forcibly the lungs are pressed in violent efforts to vomit, both by the muscles subservient to respiration and the abdominal muscles, as well as by the contents of the abdomen itself. And it is wonderful, when the texture of the lungs is considered, that accidents of this kind do not much oftener happen, not only in vehement retchings to vomit, but in violent coughs, pains of childbirth, lifting great weights, and other preternatural exertions of strength.

When once the extremities of the bronchia and the vesicular substance have given way, the mischiefs are easily foreseen. The air getting loose into the substance of the lungs cannot be parted with in expiration; it consequently is retained there, and the space it occupies prevents as much of the external air being received into the lungs as its own quantity. As, from their incessant motion, injuries to the lungs are not easily removed, when once a rupture is made, every fit of coughing or other violent exertion extravasates more air. Hence the rupture still continuing, and probably increasing, more and more air becomes extravasated, till, as in the present case, the quantity becomes so great, as not only to impede the course of the blood through the lungs, but the internal pressure of the extravasated air prevents the ingress of a quantity of fresh air, sufficient to cool and attenuate the blood. In fact, a small part only of the lungs is employed; as the extravasated air, though still in an elastic state, by no means answers the purposes of fresh air in respiration; as the former, by its confinement in the lungs, is very soon divested of its vivifying spirit, that principle which is soon destroyed in animal bodies, and which some chemical physiologists have supposed to be an acid nitrous gas, and is most essential to human life. Hence, in a very short time, the effects are too obvious to be mentioned; and death must soon follow, as in this instance.

XLIV. Considerations to prevent Lightning from doing Mischief to great Works, High Buildings, and Large Magazines. By Mr. Wilson, F.R.S. p. 247.

Long experience since the discovery by Dr. Franklin, has now established a truth among philosophers, that lightning, like the electric fluid, passes more freely through iron, copper, and other metals, than through dry wood, stone, or marble. Instances of this truth are innumerable: and to be convinced of it, we need only trace the late violent effects of lightning on St. Bride's church, and the houses in Essex-street, &c. For, on examining these buildings, it appears that there are certain thick bars of iron, through which the lightning has passed, without producing any visible effects; and, on the contrary, in certain parts where the junctions of those bars with the stone, or wood, are made, there the lightning, rushing from the iron, has broke the stone to pieces, and shivered the wood. From the like experience we also learn, that if the iron is too slender for conducting the lightning, it is either dashed into pieces, or exploded like gunpowder; just in the same manner as we are able, by the electric power, to break and dissipate in vapour a very slender wire. Bars of metal, of a proper thickness, and conveniently disposed, seem therefore necessary for the security of such buildings.

It is to be noted, that the mischiefs caused by lightning are not always owing to its direction from the clouds to the buildings or other eminences, and thence to the earth; but sometimes, on the contrary, from the earth, buildings, and other eminences, to the clouds. For the principle on which its direction depends, appears to arise from the restoration of a certain equilibrium, in a subtile and elastic fluid, previously disturbed by various causes. Now, according to the laws of elastic fluids, the endeavour to restore the equilibrium of such a fluid, will be in that direction where the resistance to its passage happens to be the least. On this principle we therefore see a necessity, either to open a passage for it to go freely through, by placing certain bars of metal properly, or, to stop the passage of the fluid through such buildings entirely. The last method would be dangerous to put in practice; because, if high buildings were so secured, the lightning would then attack the lower buildings, which are far more numerous, and probably would destroy a greater number of people, cattle, &c. Whereas, if the first method is preferred, the high buildings will then tend to protect the lower ones more effectually; and may with propriety be considered as so many pipes to carry off the lightning quietly, either from the earth to the clouds, or from the clouds to the earth. And that several proper conductors are necessary to carry off the lightning more readily, than some of the accidental or partial conductors in a large town are capable of, appears from this; that we are able to collect small quantities of the electric fluid, with a slender apparatus in our hands only; whilst it is exposed in the street, garden, or other open place, during the hovering of such clouds as occasion violent lightning.

From repeated observations of this kind, there is reason to believe that the quantity of lightning at particular times, is so very great, that it would be dangerous to invite it to any buildings, and that unnecessarily, in the most powerful manner we are able; by suffering the several conductors to end in a point at the top. On which account, it is apprehended that pointed bars, or rods of metal, ought always to be avoided. And as the lightning must visit us some way or other, from necessity, to restore the equilibrium, there can be no reason to invite it at all; but, on the contrary, when it happens to attack our buildings, we ought only so to contrive our apparatus, as to be able to carry the lightning away again by such suitable conductors, properly fixed, as will very little, if at all, promote any increase of its quantity.

To attain which desirable end, in some degree at least, it is proposed that the several buildings remain as they are at the top; that is, without having any metal above them, either pointed or not, by way of a conductor. On the inside of the highest part of such building, and within a foot or two of the top, it may be proper to fix a rounded bar of metal, and to continue it down along the side of the wall to any kind of moisture in the ground. But if the building happens to be mounted with an iron spindle, for supporting a vane, or other ornament, and it should not be convenient to have it taken away, then the bar of metal ought to communicate with that spindle. And as to the diameter of such a metal bar, it will probably depend on the height of the building; for it is apprehended the great church of St. Paul's, to complete the partial conductors (which are the metallic cross, ball, gallery, dome, &c.) and secure it effectually, would require a bar of metal 2 inches diameter, if not more, and a building like the British Museum, one considerably less. But it appears there is no occasion for any at that repository, as it is already provided, though from accident, like many other buildings, with very effectual conductors. The copings of the roof, and the several spouts, which are continued from it into the ground, being all of lead.

That conductors ought to be thicker than is generally imagined, seems to appear from a late instance taken notice of in St. Bride's church by Mr. Delaval and Dr. Watson, where an iron bar $2\frac{1}{2}$ inches broad, and $\frac{1}{4}$ an inch thick, or more, was bent and broken asunder by the violence of the lightning. The Eddystone Lighthouse, which stands on a rock surrounded by the sea, the work of Mr. Smeaton, was thought to be an object very likely to suffer by lightning; and the more so, as the top of it consisted of a copper ball 2 feet in diameter, with a chimney of the same metal, passing through it down to the 2d floor, but no farther. Directions were therefore given to make a communication of metal from the lowest part of the copper chimney down to the sea; which was executed accordingly about the year 1760, or soon after the building was finished. Now if, instead of the copper ball, a pointed bar of metal had been put in its place, or above it, and communicated with the conducting matter below, there is no saying what might be the consequence of so powerful an invitation, to an edifice thus particularly situated.

Since the former part of this paper was communicated to the R. S., that is, on the 5th of August, 1764, I received the following account from Capt. Dibden, commander of a merchant ship, who says, that in the year 1759, he was taken by the French, and carried prisoner to Fort Royal in Martinico. That in removing him thence some time after, and on foot to St. Pierre, which is about 20 miles, his conductor, or guard, stopped at a small chapel 5 miles from the last place, to shelter themselves from the heavy rain which fell during a violent thunder storm. That the chapel had no steeple or tower belonging to it, but stood on an eminence with 3 or 4 poor low houses near it. That soon after they were thus sheltered, a violent flash of lightning struck 2 soldiers dead, who had been leaning against the wall of the chapel between 2 buttresses, and not far from the rest of the company, being all on the leeward side of the chapel. That it made an opening in the wall about 4 feet high, and about 3 feet broad, and in that part only against which they rested.

That Capt. Dibden, along with other persons, entered at this hole immediately after, to see if any other damage had been done to the chapel. That they observed a square bar of iron near the hole, and on the ground, about 4 feet long, and $1\frac{1}{4}$ inch thick, making an angle with the wall, as they suppose, to support the upper part of an inclined tombstone, which was also thrown down and broken to pieces. That this bar was joined in the middle to one end of another bar, about 1 foot long, and 1 inch thick, which laid horizontally, and, passing to the wall, had been there fastened with lead. That the lightning in rushing along the inclined bar, had wasted or reduced its thickness in some places very considerably, insomuch that it looked like a burnt poker which had been long used; and broke the bar into 2 pieces, about an inch above the joining of the lesser bar, the ends of which had a burnt flaky appearance. That the other parts of the bar were changed in colour to a grey, or whitish hue: resembling iron after it has been exposed to a violent heat and then suffered to cool. That the horizontal bar had also undergone an extraordinary change by the lightning, but particularly at that end next the wall of the chapel, it being reduced from 1 inch in diameter to the size of a slender wire, but tapering towards the wall. That when the soldiers rested against the wall, their heads were about the same height with the shortest bar; and, from what he can recollect, were very near being opposite to that end which was inserted in the wall. That the 2 soldiers were forced from the wall at the same instant by the lightning: so that their feet, which were 1 yard or more from it, were nearest to the wall, and their heads the farthest off. That their flesh appeared very black. That their clothes were burnt and scorched in many parts, and their belts shrivelled up, as if they had been exposed to a large fire. That Capt. Dibden, and other people, felt a disagreeable kind of an electric shock, at the same instant that the soldiers were killed.

Capt. Dibden gave an account also, that he was lately at Virginia, 1763: that the inhabitants of Norfolk had changed their opinions in respect to fixing of wires and small rods of iron on the tops of their houses; from the frequent instances they have lately had of their being melted, or destroyed, by the violence of the lightning: and that now they adopted in their stead, rods of iron from $\frac{1}{2}$ an inch thick to $\frac{3}{4}$ of an inch thick, or more. That those rods ended in a point at the top, and extended from 3 feet above their houses down to the ground; and that many houses had one of these conducting irons at each end. The Captain added, that though the pine trees are considerably higher than the oaks in the American woods, yet the oaks are the oftenest attacked by the lightning: and that he does not remember any oaks growing among the pine trees, when the latter have suffered by lightning: which must be owing to the greater resistance arising from the unctuous nature of the pine trees.

XLV. A Solar Eclipse observed at the Roman College by the Jesuits, April 1, 1764; *True Time after Midnight. From the Latin.* p. 254.

With a tube of 10 palms, the beginning of the eclipse was observed at $9^h\ 49^m\ 8^s$, and the end at $12^h\ 52^m\ 49^s$.

XLVI. The Description of a New Hygrometer. Invented by James Ferguson, F. R. S. p. 259.

This machine is a frame of wainscot or mahogany, grooved in the innermost edges of the two longest sides, for holding a pannel of white deal board, without pinching it. The pannel is about the thickness of a crown-piece, and 15 inches in length, crosswise to the grain of the wood. The middle part projects outward from the upper and lower edges, where it is fastened into the frame by 2 screws, to keep the middle part always in the same place, while the rest of the pannel expands by moist air toward both ends of the frame, and contracts toward the middle when the air is dry.

In 3 or 4 years at most, a new pannel should be put into the frame; because, when the old one has been so long exposed to the air, it will almost cease to be affected by it. And therefore, a large thick piece of deal should be kept in reserve for that purpose; and about the thickness of a card always planed off that side from which the new pannel is to be taken.

XLVII. Experiments and Observations on the Compressibility of Water and some other Fluids. By John Canton, M. A., F. R. S. p. 261.

In a paper lately laid before the R. S., Philos. Trans. vol. 52, p. 640, Mr. C. not only related the experiments by which he found water to be compressible, but also those by which he discovered how much a given weight would compress it when in a temperate degree of heat. By similar experiments made since, it appears that water has the remarkable property of being more compressible in winter than in summer; which is contrary to what he had observed both in spirit of wine and oil of olives: these fluids are (as one would expect water to be) more compressible when expanded by heat, and less so when contracted by cold. Water and spirit of wine he several times examined, both by the air-pump and condenser, in opposite seasons of the year: and, when Fahrenheit's thermometer has been at 34°, he has found the water to be compressed by the mean weight of the atmosphere 49 parts in a million of its whole bulk, and the spirit of wine 60 parts; but when the thermometer has been at 64°, the same weight would compress the water no more than 44 parts in a million, and the spirit of wine no less than 71 of the same parts. In making these experiments, the glass ball containing the fluid to be compressed must be kept under water, that its heat may not be altered during the operation.

The compression by the weight of the atmosphere, and the specific gravity of each of the following fluids, which are all that he had tried, were found when the barometer was at 29½ inches, and the thermometer at 50°.

	Millionth parts.		Specific gravity.
Compression of spirit of wine....	66	—	846
Oil of olives....	48	—	918
Rain-water......	46	—	1000
Sea-water.......	40	—	1028
Mercury........	3	—	13595

These fluids are not only compressible, but also elastic: for if the weight by which they are naturally compressed be diminished, they expand; and if that by which they are compressed in the condenser be removed, they take up the same room as at first. That this does not arise from the elasticity of any air the fluids contain, is evident; because their expansion, by removing the weight of the atmosphere, is not greater than their compression by an equal additional weight: whereas air will expand twice as much by removing half the weight of the atmosphere, as it will be compressed by adding the whole weight of the atmosphere. It may also be worth observing, that the compressions of these fluids, by the same weight, are not in the inverse ratio of their densities or specific gravities, as might be supposed. The compression of spirit of wine, for instance, being compared with that of rain-water, is greater than in this proportion, and the compression of sea-water is less. The weight of $32\frac{1}{2}$ feet of sea-water is equal to the mean weight of the atmosphere: and as far as trial has yet been made, every additional weight equal to that of the atmosphere, compresses a quantity of sea-water 40 millionth parts; now if this constantly holds, the sea, where it is two miles deep, is compressed by its own weight 69 feet 2 inches; and the water at the bottom is compressed 13 parts in 1000.

XLVIII. Concise Rules for Computing the Effects of Refraction and Parallax in Varying the Apparent Distance of the Moon from the Sun or a Star; also an Easy Rule of Approximation for Computing the Distance of the Moon from a Star, the Longitudes and Latitudes of both being given. By the Rev. Nevil Maskelyne, A. M., F. R. S. p. 263.

The following rules, excepting one, are the same which Mr. M. before communicated to the R. S., but without demonstration, in a letter from St. Helena, containing the results of his observations of the distance of the moon from the sun and fixed stars, taken in his voyage thither, for finding the longitude of the ship from time to time; since printed in vol. lii. of the Phil. Trans. The two rules for the correction of refraction and parallax, he had also communicated to the public in his British Mariner's Guide to the discovery of longitude from like observations of the moon; and added in the preface a rule for computing a second but smaller correction of parallax, necessary on account of a small imperfection lying in the first rule derived from the fluxions of a spherical triangle. To the rules he has here subjoined their demonstrations.

With respect to the usefulness of these rules, he entertains hopes that they will appear more simple and easy than any yet proposed for the same purpose: the last rule, for computing the distance of the moon from a star, though only an approximation, being so very exact, seems particularly adapted for the construction of a nautical Ephemeris, containing the distances of the moon from the sun and proper fixed stars, ready calculated for the purpose of finding the longitude from observations of the moon at sea; an assistance which, in an age abounding with so many able computers, mariners need not doubt they will be provided with, as soon as they manifest a proper disposition to make use of it.

A RULE. *To compute the contraction of the apparent distance of any two heavenly bodies by refraction; the zenith distances of both, and their distance from each other being given nearly.*

Add together the tangents of half the sum, and half the difference of the zenith distances; their sum, abating 10 from the index, is the tangent of arc the first. To the tangent of arc the first, just found, add the co-tangent of half the distance of the stars; the sum, abating 10 from the index, is the tangent of arc the second. Then add together the tangent of double the first arc, the co-secant of double the second arch, and the constant logarithm of 114″ or 2.0569: the sum, abating 20 from the index, is the logarithm of the number of seconds required, by which the distance of the stars is contracted by refraction: which therefore added to the observed distance gives the true distance cleared from the effect of refraction.

This rule is founded on an hypothesis, that the refraction in altitude is as the tangent of the zenith distance: and the refraction at the altitude of 45 degrees being 57″, according to Dr. Bradley's observations, therefore the refraction at any altitude, calling the radius unity, is = 57″ × tangent of the zenith distance. This rule is exact enough for the purpose of the calculation of the longitude from observations of the distance of the moon from stars at sea as low down as the altitude of 10°, for there the error is only 10″ from the truth. But if the altitude of the moon or star be less than 10°, the rule may be still made to answer sufficiently, by only first correcting the observed zenith distances by subtracting from them 3 times the refraction corresponding to them, taken out of any common table of refraction, and making the computation with the zenith distances thus corrected. This correction depends on Dr. Bradley's rule for refraction, which he found to answer, in a manner exactly, from the zenith quite down to the horizon, namely that the refraction is = 57″ × tangent of the apparent zenith distance lessened by 3 times the corresponding refraction taken out of any common table.

A RULE. *To compute the contraction or augmentation of the apparent distance of the moon from a star, on account of the moon's parallax; the zenith distances of the moon and star, and their distance from each other being given nearly.*

Add together the tangents of half the sum, and half the difference of the zenith distances of the moon and star, and the co-tangent of half the distance of the moon from the star; the sum, abating 20 from the index, is the tangent of an arch, which call A. Then, if the zenith distance of the moon be greater than that of the star, take the sum of the arch A, just found, and half the distance of the moon from the star; but if the zenith distance of the moon be less than that of the star, take the difference of the said arch A and half the distance of the moon from the star; and the sum or difference call B. To the tangent of B, thus found, add the cosine of the moon's zenith distance, and the logarithm of the moon's horizontal parallax, expressed in minutes and decimals; the sum, abating 20 from the index, is the logarithm of the effect of parallax, tending always to augment the apparent distance of the moon from the star; except the zenith distance of the moon be less than that of the star, and at the same time the arch A be greater than half the distance of the moon from the star, in which case the effect of parallax diminishes the apparent distance of the moon from the star.

Remarks on the use of the two foregoing rules.

It has been remarked, after the rule for refraction above, that if the altitudes of the moon or star are under 10°, the zenith distances must be first lessened by 3 times the refractions corresponding to their respective altitudes, before the effect of refraction be computed. But in order to compute the effect of parallax from the 2d rule, the observed distance of the moon from the star must be first corrected by adding the effect of refraction to it, found by rule the first: as must the observed altitudes of the moon and star be also corrected by taking from them their respective refraction in altitude, and the corrected arches thus found may be made use of in computing the parallax. Only, if the altitudes of the moon and star are both 10° or more, part of the calculation of rule the 2d may be saved, and arch the 2d, found by rule the first, taken for arch A in the 2d rule without any sensible error. In this case it will be most convenient to observe the following order of computation, instead of that before prescribed to be used when the altitudes are under 10°.

1st. Making use of the apparent altitudes of the moon and star uncorrected, compute arches the 1st and 2d by the directions contained in the rule of refraction. 2dly, Taking arch the 2d for arch A in the rule of parallax, compute the effect of parallax according to rule the 2d. 3dly, With arches the 1st and 2d compute the effect of refraction by rule the 1st. 4thly, and lastly, Applying the two corrections of parallax and refraction duly, according to the rules, to the observed distance of the moon from the star, you will have the true and correct distance of the moon from the star, cleared both of refraction and parallax.

A RULE. *For computing a 2d, but smaller correction than the first, necessary to be applied to the observations of the distance of the moon from a star on account of parallax.*

Call the principal effect of parallax, found by the preceding rule, the parallax in distance; and find the parallax answering to the moon's altitude. Then to the constant logarithm 0.941 add the logarithm of the sum of the parallax in altitude and the parallax in distance, the logarithm of the difference of the same parallaxes, and the co-tangent of the observed distance of the moon from the star (corrected for refraction, and the principal effect of parallax), the sum, abating 13 from the index, is the logarithm of the number of seconds required, being the 2d correction of parallax; and is always to be added to the distance of the moon from the star, first corrected for refraction, and the principal effect of parallax found above, in order to obtain the true distance; unless the distance exceed 90°, in which case it is to be subtracted.

A concise rule to find the distance of the moon from a zodiacal star, very nearly; the difference of the longitudes of the moon and star, and the latitudes of both being given.

To the cosine of the difference of the longitudes add the cosine of the difference of the latitudes, if both of the same denomination, or sum, if of contrary denomination; the sum of the two logarithms, abating 10 from the index, is the cosine of the approximate distance. This gives the true distance of the moon from the sun, being then nothing more than the common rule for finding the hypothenuse of a right angled spherical triangle from the two sides given. But in the case of a zodiacal star, apply the following correction to the approximate distance thus found.

To the constant logarithm 5.3144 add the sine of the moon's latitude, the sine of the star's latitude, the versed sine of the difference of longitude, and the cosecant of the approximate distance; the sum of these 5 logarithms, abating 40 from the index, is the logarithm of a number of seconds, which subtracted from the approximate distance before found, if the latitudes of the moon and star are of the same denomination, or added to it, if they are of different denominations, gives the true distance of the moon from the star.

This rule, though only an approximation, is so very exact, that even if the latitude of the moon was 5°, and that of the star 15°, the error would be only 10″; and if the latitude of the moon be 5°, and that of the star 10°, the error is only $4''\frac{1}{4}$; and if the latitudes be less, it will be less in proportion as the squares of the sines of the latitudes decrease.

XLIX. and L. Observation of the Transit of Venus, June 6, 1761, at St. John's, Newfoundland. By Mr. John Winthrop, Professor of Mathematics and Philosophy at Cambridge, New England. In a Letter to J. Short, F.R.S. p. 279.

Having properly prepared the clock and the astronomical instruments, they waited for the critical hour, which proved favourable to their wishes. The morning was serene and calm. The sun rose behind a cloud that lay along the horizon, but soon got above it; and at 4ʰ 18ᵐ they had the pleasure of seeing Venus on the sun; though dimly indeed at first. But the planet presently became distinct, and her limb well defined. Upon this, Mr. W. applied himself to observe the passage of the sun's and Venus's preceding limbs, by the vertical, and of their lower limbs by the horizontal wires, in the reflector, and made the following observations; one of his assistants counting the clock, and the other writing down the observations as he made them; which, having made the proper correction of the time for the change of the sun's declination, stand as follow:

True time.				diff. long. ☉ & ♀		♀ lat. s.	
				′	″		
At 4ʰ	21ᵐ	20ˢ	Sun at the vertical			10′	47″
	21	31	Venus at the same	9	2		
	23	6	Venus at the horizontal	9	4	10	50
	24	23	Sun at the same				
	27	29	Venus at the horizontal	9	25	10	52
	28	47	Sun at the same				
	35	15	Sun at the vertical			10	55
	35	21	Venus at the same	9	56		
	37	49	Venus at the horizontal	10	8	11	00
	39	9	Sun at the same				

As Venus began now to draw near the sun's limb, Mr. W. prepared to observe her egress. The interior contact did not appear so perfectly instantaneous, as Dr. Halley's papers led him to expect. He was not certain of it till 4ʰ 47ᵐ 21ˢ, though he doubted of it at 17ˢ. The exterior contact he judged to be at 5ʰ 5ᵐ 49ˢ, doubtful also 3 or 4ˢ; and so the passage of Venus's diameter, 18ᵐ 28ˢ.

The above observations gave him several altitudes and azimuths of Venus, whence he deduced her right ascensions and declinations; and from them, her longitudes and latitudes. The result of the whole, or the planet's difference in longitude from the sun's centre and her latitude, is set down above, against each observation of Venus. Hence he concluded that at the central emersion, which he put at 4ʰ 46ᵐ 38ˢ, the difference of longitude was 11′ 19″, and the latitude 11′ 6″. Also, that the conjunction in longitude happened at 2ʰ 4ᵐ 36ˢ, the planet's latitude then being 9′ 28″.

In these calculations, he supposed the semidiameter of the sun to be 15′ 50″, and of Venus 29″.

By several observations, he found the latitude of the place 47° 31′ N; which falls within the latitudes laid down in several books and maps, which make it from 47° 25′ to 48° 0′. He could make no use of Jupiter's satellites in finding the longitude, as they were not risen high enough to be observed above an hour before day-light came on. There were but two of their eclipses that could have been visible there while he was on the island; and though he watched for both of them, he was disappointed of both by unfavourable weather. Neither was he fortunate enough to get so much as one occultation of a fixed star by the moon.

The longitude of St. John's is variously set down by different authors, though none mention the observations by which it was determined. According to Sir Jonas Moore, it is 42^h 50^m west from Greenwich; and as his authority may be as good as any, Mr. W. keeps to this longitude till it can be ascertained by further observations.

Mr. W. viewed the sun with great attention in the reflector, both on the 5th and 6th of June, in hopes to find a satellite of Venus; but in vain. There were several spots then on the sun; but none that he saw could be a satellite. The variation of the needle there he found 19° W.

REMARK.—Mr. Short has computed the parallaxes at the egress for this observation at St. John's, and by comparing this observation with that at the Cape of Good Hope (on the above longitude and latitude of St. John's as set down by Mr. Winthrop) he finds the parallax of the sun, thence resulting, $=8''.25$.

LI. On the Effects of Lightning on Three Ships in the East Indies. By Mr. Robert Veicht. p. 284.

August the 1st, 1750. Lat. 1° 56′ N. Malacca bearing about N.E. After some clear serene weather, a thunder cloud arose, and soon increased very fast. The whole heavens were covered with it, and the flashes of lightning happened at times on different sides of the ship, which had all the sails furled before it came upon her. The wind, which reached the ship before the thunder, brought with it a violent and heavy rain, which sufficiently soaked the ship and every thing about her. The ship was all this time, which was in about half an hour after its first appearance above the western horizon, in the midst of repeated flashes of lightning, which were just upon the ship by her trembling and shaking on every explosion, and the flash and clap coming in the same instant, the officers and people were apprehensive of damage to the mast.

2¼ A.M. At this time a clap burst, as was judged by the report, about midway between the head of the mast and the body of the ship, or it might be higher, and in descending might cause that appearance, and just over it. This made the ship tremble and shake as if she was going to burst in pieces, and great pieces and splinters of the mast fell on different places of the ship; but it was so very dark, we could not see from which of the masts they were forced. Immediately after this 1st came a 2d, which burst just above, and on the quarter deck of the ship, which by the report was as great, and being close on the deck was more terrifying than the former.

At day light we found that the foremast and mizenmast had escaped, and the mainmast had suffered as follows: All the main top gallant-mast, from the rigging at the top of it, to the cap at the head of the maintop-mast, was entirely carried away, part falling overboard, and part into the ship in different places. The maintopmast had great pieces carried from it, from the hunes down to the cap, at the head of the mainmast, so that it could but just stand, being hardly strong enough to bear its own weight, and that of its rigging. The mainmast being composed of three pieces, towards the top of it, those of the sides being of oak, called the cheeks, were not hurt; but the middlemost part, being of fir, was shivered in several places, and pieces were carried out of it 6 or 7 inches in diameter, and from 10 to 12 feet long, and this in a circular descending manner from the parrel of the main-yard down to the upper deck of the ship, the pieces being taken out crooked, or circular, or straight, according as the grain of the wood ran.

No part of the top-gallantmast or topmast, that was covered with the lamp black, were touched with the thunder, the greasy part only being carried away. The head of the top-gallantmast, from the rigging upwards to the spindle, was entire, as was also its heel, for the lightning did not touch the heel, but missed the whole both of top-gallantmast and topmast, that lay between the cap and upper end of the greasy part of the mast. Of the topmast great pieces were carried out, of many feet in length, and 9 or 10 inches in thickness, and this on different sides of the mast, for the whole length of the greasy part. From the top of the mainmast to the upper end of that which is covered with turpentine, there was no damage; but thence downwards, the cheeks were started off from the middle part, and pieces taken out winding aslant down the mast, and out of the fir part many feet in length, and 6 and 7 inches deep, and near the upper deck a piece as large as the body of a man, and 11 or 12 feet in length.

Neither the yards nor any part of the rigging was hurt; for though the middle part of the top-gallantmast, which was 18 feet long, and 9 inches diameter, was entirely burst to pieces, and carried away; yet the rigging, which surrounded the upper part was neither burnt, scorched, nor broken. Neither did it touch the caps on the mast heads, nor the top, or round scaffolding on the mast, which in this ship was 18 feet broad; and these as well as the yards were covered with tar and lamp black, and made of three inch deal.

At the time of the first clap there might be more than 60 men upon deck, and some of them very near the mast at the very time of the clap. Some of these were stunned and beaten down; and in their arms, where they thought themselves hurt, they had a numbness, which continued some time, but not any of them otherwise hurt. Luckily before the 2d, the men who were on the quarter-deck, in number about 20, had time to retire under the awning, which is a projection of the deck of the cabin to shelter from the sun or rain; so all escaped unhurt, though sufficiently frighted. And indeed the second flash was most terrible, as it was an explosion like a great number of balls, which went off after each other, cracking like shells, which continued for the space of half a minute; and from which there was no retiring, as the door of the cabin was shut; and they might have set the ship on fire, but for the great rain which had fallen immediately before this.

Anno 1746, a Dutch ship, lying in the road of Batavia, having taken leave of the governor, was ready to depart for Bengal. The afternoon was calm, and towards evening they had loosed their sails, and lay ready to take up their anchor on the coming off of the wind from the land, which is common every night. A black cloud was gathering over the hills, and the wind brought it towards the ship: by the time the cloud and the wind reached the ship, a clap of thunder burst from it just over the ship, and set fire to the main-top-sail, which being very dry, burnt with great fury; and this set fire to the rigging and mast. They immediately attempted to cut away the mast, but were hindered by the falling of the rigging, which was burnt, from the head of the mast. By degrees the fire communicated to the other masts, and obliged the people to desert the ship; and afterwards it took hold of the body of the ship, and burning down to the powder, the upper part of the hull blew up, and the bottom part sunk in the place where she was at anchor.

Anno 1741, Bencoolen road on the s. w. side of the island of Sumatra, lat. 4° 0′ south. There lay here two ships, one an European, the other a country trading ship, both belonging to the East-India Company. Here, as well as in the strait of Malacca, you have periodical winds, which blow for 6 months of the year from the same quarter of the horizon, and the other 6 months from the opposite quarter; and it is observable that these thunder showers and squalls of wind usually come contrary to these stated winds, which are calmed during the thunder, but return to their constant quarter as soon as the thunder and rain are past. In the above year 1741, in June, the weather was very hot and sultry, and the constant wind but very faint. The wind came after this from the land, and almost opposite to the usual point a very faint air; and the thunder was frequent and close to the ships, which lay near each other, but the fog and rain prevented their seeing each other; they often trembled and shook by the explosion of the thunder. One of these claps burst on the country ship, which by this time had her topmasts struck; that is, lowered down along the lower masts. This clap carried away and burst to pieces all the part of the lower mast from where the yard is carried aloft to within 6 or 7 feet of the upper deck. The mast was woolded with ropes of 2¼ size in different places, which were burst asunder at every turn of it; and the mast all shivered into small splinters, and mostly carried overboard. Here also the mainmast was made of fir, and the part which was split and shivered to pieces, was the part usually coated with turpentine mixed as before said with tallow or oil: and the main topmast, which was made of a wood of the country called teak, and is of a texture like oak, but stronger, was untouched, notwithstanding it lay parallel and touched the mast for the whole length of the part carried away.

*LII. A Demonstration of the Second Rule in the Essay towards the Solution of a Problem in the Doctrine of Chances, published in the Phil. Trans. Vol. LIII. Communicated by the Rev. Richard Price.** p. 296.

This is a supplement to the essay on a problem in the doctrine of chances, noticed at p. 41, of this vol. of these Abridgments. This paper consists of a demonstration of a rule in that former paper. But both the rule and the demonstration have been omitted, as much too intricate and operose to be of any real use in the practice of calculations relating to the doctrine of chances.

LIII. Of a Remarkable Meteor seen at Oxford, March 5, 1764. *By the Rev. John Swinton, B. D., F. R. S.* p. 326.

Coming out of Christ-Church common-room into the great quadrangle, on Monday, March 5th, 1764, about $7^h\ 30^m$ P. M. Mr. S. observed a general brightness in the air, much superior to that of the full moon; though the heavens were then in some measure overcast, and the moon only 3 days old. This unusual and very remarkable illustration of the atmosphere continued the whole evening. Throwing up his bed-chamber sash, a little before eleven o'clock, he

* Dr. Price was born in Glamorganshire in 1723, and he died in 1791, consequently at 68 years of age. He received his education in a private academy, after which he became minister to a congregation at Newington in Middlesex; whence he removed to that of Hackney. He was also lecturer of the meeting-house in the Old Jewry, in London. In 1764 he became F. R. S., and D. D. by diploma from a Scotch university. At the time of the American war he made himself conspicuous by his zeal in the cause of liberty, which he also displayed on several other occasions: and for the publication of his Observations on Liberty and Civil Government he had the thanks of the city of London. Among many other learned accomplishments, Dr. P. was no mean mathematician, which enabled him to treat with peculiar precision the calculations relating to political arithmetic, population, annuities, &c. It is said even that he had the honour of suggesting to the late prime minister, Mr. Pitt, the measure of the present sinking-fund, to extinguish the national debt, by the allotment of an annual million to accumulate at compound interest. Dr. Price's principal works are: 1. Four Dissertations on Providence and Prayer; on the Importance of Christianity, &c. 2. A Review of the Principal Questions and Difficulties in Morals. 3. Observations on Reversionary Payments, Annuities, &c. 2 vols. 8vo. 4. Discussion of the Doctrines of Materialism and Necessity, in a correspondence with Dr. Priestley. 5. Essay on the Population of England and Wales. 6. A volume of Sermons.

unexpectedly discovered a most glorious and exceedingly resplendent white column, in the southern part of the hemisphere, which in lustre surpassed every thing of the same kind that he had ever seen before. The base of this column seemed to be between 20 and 30 degrees distant from the horizon, and was many degrees broad. The meteor ascended gradually near 30 degrees, passing to the south of the zenith. It was much narrower at the vertex than the base, and consequently approached somewhat towards a pyramidal form. It remained a few minutes in a fixed and permanent state, after it had arrived at its greatest altitude, and was completely formed. About 11^h 15^m it became fainter, and much less vivid; and there then darted from it towards the west several whitish rays and coruscations. At 11^h 20^m the lucid column was barely visible, declining apparently southward, and soon after it totally disappeared. He went to bed at 11^h 30^m, when the atmosphere was covered with the same kind of luminous vapour, that before the formation of the bright Colossean pillar had appeared; and, in the southern part of the hemisphere, diversified by undulations of shining matter, that exhibited a most beautiful and agreeable scene.

It may not be improper here to remark, that a meteor called an aurora borealis, was seen at Lisbon, according to one of the public papers, the very same night. It is said to have lasted about 4 hours, and to have engaged the attention of the philosophers there. From the similarity of certain circumstances it might have been denominated an aurora borealis, though appearing in the southern part of the heavens, as that Mr. S. observed actually did. Instances of auroræ australes, at least in our part of the world, are very rare. At present that observed by Mr. John Martyn, only occurs. The account of this appearance, transmitted by that gentleman to the R. S., and published in the Philos. Trans., vol. 46, p. 319, highly merits the attention of the curious meteorologist. This phenomenon, seen by him, January 23, 1749-50, and that now sent in several respects pretty well agree; but in others they almost totally differed. Some of the public papers informed us, that an extraordinary phenomenon was observed in the air at London, the preceding night, viz. March 4, 1764; which in a few particulars resembled this now communicated, but in the rest those two meteors were dissimilar enough.

LIV. Extract of a Letter from Mr. John Horsley, Fourth Mate on board the Glatton East India Ship, to the Rev. Mr. Nevil Maskelyne, F.R.S. Dated Batavia, Nov. 16, 1763, giving an Account of his Observations at Sea, for finding out the Longitude by the Moon. p. 329.

Mr. H. sailed from Spithead March the 8th, 1763; the 19th he had 4 observations of the distance of the moon from the sun; by taking the medium, the longitude agreed exactly with that by account. The 21st he had another observation, and the same day saw the island of Madeira, the body of which, according to this and the former observations (which agreed exactly) he made to lie 17° 18′ west of London, which differs only 18′ from what it is laid down in the chart. The success he met with in this first attempt gave him great satisfaction, and made him continue his observations regularly to the island of St. Paul's, which they made July 5. The day before he had 3 observations of the distance of the moon from the sun. July the 5th, the body of the island bearing by the azimuth compass s. 27° w. distance 6 leagues, the sky remarkably clear and fine, and the ship having hardly any motion, circumstances all in his favour, he took 9 observations of the distance of the moon from the sun, the captain and chief mate assisting him in taking the altitudes. He divided them into 3 sets, and worked from the medium of every 3; by which he made the longitude of the ship as follows, 75° 15′, 75° 25′, 74° 40′. The 3 observations, he took the day before, made the longitude of the ship 74° 38′ and 73° 32′, which brought forward to the noon of July the 5th made 75° 45′ and 74° 39′. Taking the medium of the whole 5 sets, he made the longitude of the ship at noon 75° 8′ 48″ east of London. Subtracting from this the difference of longitude, the bearings and distance of the island gave = 8′ 37″ west, he made the longitude of St. Paul's 75° 0′ 11″ east of London, and 58° 0′ 11″ from the Cape of Good Hope. By his account kept from an observation taken June the 18th, he made it 73° 35′ east of London, and 56° 35′ from the Cape, which differs 1° 25′ from what he makes the true longitude: most of the accounts on board were between 2 and 3 degrees to the westward of his.

LV. Of a Remarkable Meteor seen at Oxford, April 23, 1764. *By the Rev. John Swinton, B. D., F. R. S.* p. 332.

Returning home from a walk, about 8^h 10^m P. M. looking over the houses opposite to Alban-Hall, Mr. S. observed a very remarkable kind of light, forming the representation of an exceedingly bright crepusculum, or expanded body of vapour, which diffused itself all over the northern part of the hemisphere. About 8^h 55^m, not thinking of what he had seen, he threw up his sash, and accidentally cast his eye towards the N. W. where he discovered a luminous arch, extending itself to the opposite part of the heavens, somewhat resembling an iris, but of a bright white colour. He then went out into the street, traversed part of the town, and found the arch both in the N. W. and S. E. to be nearly terminated by the horizon; so that it seemed to be almost perfectly semicircular, and consequently in a manner to bisect the hemisphere, when completely formed. The meteor was not exactly erect, but ascended obliquely, declining a little to the N. of the zenith. It was extremely narrow, in breadth scarcely exceeding 2 degrees. Its edges towards the S. E. were not so well defined, but somewhat jagged and unequal. From 9^h to 9^h 15^m it exhibited a most vivid resplendent whiteness, such as he believes was hardly ever observed before. During that term, the phenomenon seemed altogether fixed and permanent, without increase or diminution, without any apparent motion of the whole, and indeed almost without the least external variation. An internal undulating motion of the particles, constituting the white luminous matter of the arch, was however discernible, from the first to the last moment of its existence. No stars were visible through the vapour itself, but 2 or 3 appeared at a small distance from it. Not the faintest traces of a proper aurora borealis, either before the first appearance, or during the continuance, or after the extinction of the meteor, were to be seen. A little past 9 o'clock the extremities of the arch became faint, as did soon after the whole body of the luminous vapour itself. About 9^h 20^m the summit, or highest part of the arch, a few degrees to the N. of the zenith, only remained; which continued gradually decreasing till 9^h 27^m, when the whole totally disappeared.

Mr. S. has not been able to meet with an instance of a similar phenomenon in any physiological papers, published before the year 1750. But accounts of 2 or 3 meteors somewhat resembling that above described in our Philos. Trans.* then occurred. However, this of the 23d of April, 1764, differed from one of these in its extent, as well as the inconsiderable breadth of the zone forming the arch, and the bisection of the hemisphere. From the others it was sufficiently distinguished by its most vivid resplendent whiteness, without any short, white, vibrating columns attached to it; especially, as it was neither preceded, attended, nor followed by any streaming luminous rays or coruscations.

LVI. On the Equation of Time and the True Manner of Computing it. By Nevil Maskelyne, A. M., F. R. S. p. 336.

M. Delalande says, in the Connoissance for 1760, which he repeats in the publications of other years, that, " to calculate exactly the difference between a mean and true time (that is to say the equation of time) at the instant of apparent noon, the sum of the equation of the sun's centre, the difference between his longitude and right ascension, the lunar equation, the equations of Jupiter and Venus, and that of the precession of the equinoxes, with their proper signs, must be converted into mean solar time. He adds, that it was impossible, before this time, to obtain the equation of time exactly; 1st, because hitherto no account has been made of the four little equations, the sum of which may produce above 3 seconds of time; 2dly, because it has been the practice to convert the equation of the sun's centre, and the difference between his right ascension

* Philos. Trans. vol. xlvi, p. 345, 346, 347, 648, 649.

and longitude into time of the primum mobile, instead of converting them into mean solar time, which, says he, may produce an error of 2 seconds and a half; 3dly, because the equation of the sun's centre was not known exactly before, every minute of which answers to 4 seconds in the equation of time."

I readily agree with M. Delalande, that the equation of time could not be had so exactly formerly, as it may now, when we have a much more exact theory of the sun, and are lately made acquainted with new equations of his motion. I cannot, however, assent to his position, that the equation of the equinoctial points is to be taken into this account, together with the other equations, since this is not an inequality in the sun's motion, but arises from a motion of the equator itself; yet of such a kind as cannot accelerate or retard the coming of the sun, or any star lying within the tropics, to the meridian, by above a quarter of a second of time. This will, perhaps, appear in a good measure plain, if it be considered that the diurnal motion of the earth round its axis is neither accelerated nor retarded by the action of the sun and moon in producing the precession of the equinoxes, and variations of the inclination of the earth's axis to the ecliptic. The effect of these actions is, that the terrestrial pole, each day, describes a small arc of a circle about the center of the earth, in the plane of a celestial meridian passing through the sun or moon, or rather one between both; and consequently the equator of the earth has its motion in its own plane neither accelerated nor retarded, but obtains a new motion, whose axis is one of its own diameters. This is the true origin, as well of the minuter and periodical nutations, as of the regular and perpetual motion of the earth's axis about the pole of the ecliptic, observed in all ages, on which the continual precession of the equinoxes depends.

But, to illustrate more fully the point in question, let P, see fig. 1, pl. 5, represent the north pole of the celestial equator, which suppose to be translated, in any certain time from P to Q, through the small space PQ, on the meridian PD, by the actions of the sun and moon; let A be the equinoctial point of Aries, and S the sun or star. It is evident that as the rotation of the earth round its axis is no way affected, the translation of the celestial pole from P to Q along the arch PQ, of the celestial meridian PD, will occasion no alteration in the time of any given meridian of the earth coming to the fixed celestial meridian PD, nor consequently in the time of the sun or stars, when lying in this meridian, appearing to pass the meridian of the given place, contrary to what should follow from the method of computing the equation of time, used in the Connoissance des Mouvements Celestes; according to which, as long as the equation of the equinoxes is any thing, the equation of time must be affected by it, and consequently the absolute time of the sun's passing the meridian.

But if the sun or star lie not in the celestial meridian PD, but in some other meridian PS, at S, then the spherical angle SPD is the distance of the sun from the meridian PD, when the pole is at P, and SQD is his distance from the same meridian, when the pole is translated to Q. Let PT, QT, meeting in T, be tangents of the meridians PS, QS, in P and Q; TQD being the external angle of the rectilineal triangle TPQ, the angle PTQ is = TQD — TPD = SQD — SPD, and therefore is a measure of the alteration of the time of any meridian of the earth's coming to the sun at S, produced by the translation of the pole from P to Q. Now the sine of PTQ is to the sine of TPQ, as PQ to TQ; whence, calling the radius unity, and taking PQ, on account of its smallness, = the sine of PQ, and the angle PTQ = the sine of PTQ, we have $PTQ = \frac{PQ \times \text{sine } TPQ}{TQ}$ = the translation of the pole × the sine of the right ascension of the sun or star reckoned from the meridian in which the pole moves, divided by the tangent of the polar distance, or, which is the same thing, multiplied by the tangent of the declination. Therefore, as PQ, arising from the nutation of the earth's axis, never exceeds $9\frac{1}{4}''$, the greatest value of PTQ, for the sun can never exceed $9\frac{1}{4}'' \times$ tangent of $23\frac{1}{2}^\circ$ the sun's greatest declination, = $4''.1$, which answers to about $\frac{1}{4}$ of a second of time: and so much, and no more, may the sun come sooner or later to the meridian, on account of the nutation of the earth's axis: whereas, if the equation of the equinoxes was to be applied directly in the computation, according to M. Delalande's method, it would sometimes, namely when at its maximum of $18''$, produce nearly $1\frac{1}{4}$ second of time.

But, though this demonstration may be admitted to be just, yet it may perhaps be asked, wherein lay the fault of the method of computation here censured, and whether the time of the sun's coming to the meridian is not regulated by his right ascension? It may also be thought requisite that the true manner of computing the equation of time, from the sun's right ascension, should be shown. First, let it be observed, that when the pole is at P, A is the equinoctial point, and when the pole is translated to Q, some other point B is the equinoctial point: therefore the sun's mean right ascension UPA is reckoned from A, and his apparent right ascension BQS, computed from his longitude, corrected by the equation of the equinoxes AB, or BS, is reckoned from another point B. Now the equation of time is proportional to the difference between the sun's mean and true right ascension, both reckoned from the same point; so that if the sun's mean right ascension be reckoned from A, his apparent right ascension, in this case, should be reckoned from A too; or if the apparent right ascension be reckoned, more properly, from the apparent right equinox B, his mean right ascension, for this purpose, should be reckoned from B also. For it is plain, from what has been said above, that no small motion of the pole P can at all affect the absolute time of a star in the equator's coming to the meridian of any place; for, the tangent QT then becoming infinite, the angle PTQ vanishes; therefore the mean equinox A will come to the meridian at the same instant of absolute time, as if the pole had not been translated from P to Q; and the difference of time between the sun S coming to the meridian, and a fictitious sun U, supposed to move uniformly in the equator, with a motion equal to the sun's mean motion in longitude, or the equation of time will be therefore measured by AQS ∽ APU, the difference of their right ascensions reckoned from the same point A. It will also, by the like reasoning, be measured by BQS ∽ BPU, the difference of their right ascensions reckoned from the same point B; for B being the equinox, when the pole is at Q, the absolute time of the point B passing the meridian of any place will remain the same as if the pole had continued at P; whence the proposition easily follows, in like manner as above.

It may be now proper to show how the equation of time, as affected by the nutation of the earth's axis, ought to be computed. This may be done two ways. The "first follows from what has been just laid down: correct the mean right ascension of the sun UPA, by the precession of the equinoxes in right ascension APB (which is always to the precession in longitude BA, as cosine of the obliquity of the ecliptic, to the radius, or as 12 to 13 nearly) the difference of the sun's mean right ascension thus corrected BPU, and the sun's apparent ascension BQS, turned into time, is the true equation of time."

Otherwise, the effect of the nutation of the earth's axis on the equation of time, if thought deserving notice, for it can never exceed $\frac{1}{4}$ of a second of time, might be computed from the angle PTQ = PQ × sine of TPD ÷ TQ, which, supposing the nutation of the pole to be performed in a circle, whose radius is 8″, or a mean between the two conjugate semi-axes of the ellipsis, in which it really moves, is = 8″ × tangent of the sun's declination × cosine of the difference of sun's right ascension and the longitude of the moon's ascending node.

But this is not the only mistake in the computation of the equation of time in the Connoissance des Mouvements Celestes, though it may exceed one second of time. M. Delalande says that the sum of the equation of the sun's centre, the difference between his longitude and right ascension, and the sum of the 4 little equations, must be converted into mean solar time, in order to find the equation of time; and adds, that no exact equation table could be had, before this time, for 3 reasons, one of which is, that it has always been the practice to convert the equation of the sun's centre and the difference between his longitude and right ascension into time of the primum mobile, instead of mean solar time, which, says he, may produce an error of $2\frac{1}{8}$ seconds.

Now I must here freely own, that as I could not, without some reluctance, and only from the fullest proof, allow all the mathematicians and astronomers, before this time, to have been mistaken in the manner of converting the quantities above mentioned into time, so I can find no reason to conclude so from what has been cited above: on the contrary, from a full consideration of the subject, I apprehend the method hitherto used by the mathematicians to be just, and that the author has himself fallen into an equal mistake with that of which he accuses them. But, in order to set this matter in a clearer light, it will be first necessary to consider motion and time, relatively to each other; for unless this be done, it will be impossible to understand any thing precise from converting a certain number of minutes and seconds into mean solar time, or time of the primum mobile.

There are 3 different kinds of time used by astronomers, sidereal time, apparent solar time, and mean solar time. The interval between the transit of the first of Aries across the meridian one day, and its return to it the next day, is called a sidereal day, which is divided into 24 equal parts or hours, and the hours into minutes, &c. This time is shown by a clock regulated to agree with the transit of the stars across the meridian. The interval between the transit of the sun across the meridian one day, and his transit the next day, is called an apparent solar day, which is divided into hours, minutes, &c. of apparent time. The solar day, it is manifest, and its hours, minutes, &c. are of different lengths, at different times of the year: on account of which inequality, a good clock, which keeps equal time, cannot long agree with the sun's motion, which is unequal. Therefore astronomers have devised an imaginary time, called mean solar time; which is what would be pointed out by the sun, if his motion in right ascension from day to day was uniform, or, in other words, it is what would be pointed out by a fictitious sun or planet supposed to move uniformly in the equator, with a motion equal to the mean motion of the sun in longitude, its distance from the first point of Aries (meaning hereby the mean equinox) being always equal to the mean longitude of the sun: and as apparent noon is the instant of the true sun's coming to the meridian, so mean noon is the instant at which this fictitious planet would come to the meridian. The interval between its coming to the meridian on any two successive days is a mean solar day, which is divided into hours, minutes, &c. of mean solar time; all which it is manifest will preserve the same length at all times of the year.

The equation of time, at the instant of apparent noon, or of the sun's passing the meridian, being equal to the difference between mean time and 12 hours, is also equal to the interval between the mean and true sun's passing the meridian expressed in mean solar time: to find which, we have the distance of the mean sun from the meridian, at the instant of apparent noon, equal to the difference between the sun's apparent and mean right ascension (both reckoned either from the mean or apparent equinox) which may be called the equation of right ascension. The question therefore comes to this, How many minutes and seconds of mean solar time does the mean sun take to move this distance up to or from the meridian? Astronomers hitherto have allowed 1 minute of time to every 15 minutes of right ascension, and so in proportion; and I apprehend justly too; for does not the mean sun, in returning to the meridian, describe 360° about the pole in 24 hours of mean solar time; whence it is plain that his departure from the meridian is at the rate of 15° to 1 hour, and 15′ to 1 minute of mean solar time. Therefore astronomers have not converted the equation of right ascension into time according to the motion of the primum mobile; for the equation of time being mean solar time, and the motion of the primum mobile being compleated in $23^h\ 56^m\ 4^s$ of mean solar time, therefore 15° motion of the primum mobile do not answer to 1 hour of mean solar time (though it does to 1 hour of sidereal time) but to the 24th part of $23^h\ 56^m\ 4^s$ or $59^m\ 50\frac{1}{6}^s$. And it appears that the equation of time in the Connoissance des Mouvements Celestes has been computed in this manner, and the table in the 79th page of the Connoissance for 1761 has been made use of, entitled, "A table to convert into degrees the time of a clock regulated according to the mean motion of the sun." The degrees of this table are evidently degrees of the primum mobile, 1 hour of mean solar time giving 15° 2′ 27.8″, which answers to the motion of the stars from the meridian, but not to the mean motion of the sun from it, which is 15° to 1 hour of mean solar time: whence it appears, that this writer has evidently fallen into the mistake of taking motion or space of the primum mobile, instead of the mean motion of the sun from the meridian; a mistake equal to that of which he erroneously supposes former mathematicians to have been guilty, in computing the equation of time. So that the equation of time in this ephemeris, besides the mistake arising from taking the equation of the equinoctial points into the account, is constantly too small in the proportion of 24 hours to $23^h\ 56^m\ 4^s$, or of 366 to 365, or too small by 1 second on every 6 minutes of the equation of time: and the mistake of $2\frac{1}{4}$ seconds, which was supposed to be found in the old manner of reducing the equation of right ascension into time, really takes place in this new method; which, added to 1 second of time, arising from the mistake in taking the precession of the equinoxes into the account, produces $3\frac{1}{4}$ seconds, an error which I apprehend, the astronomical equation tables used since Mr. Flamsteed's time have but rarely exceeded.

To some, who are not well acquainted with the present improved state of astronomy, the difference in question may seem a matter of indifference, and too trifling for notice. But if truth be the object of all our inquiries, why should we wilfully go beside it in the smallest matters? And is it not a justice due to past astronomers, to whom we owe the foundations of all our knowledge, to vindicate them even from the smallest censure, which they do not appear to deserve? At the same time I flatter myself, that the learned editor of the Connoissance des Mouvements Celestes, and also the friends of the late illustrious Abbé de la Caille, who I believe was inadvertently the first author of this mistake, will take no offence at my endeavouring to clear up a point, which they, doubtless for want of having given sufficient attention to, seem to have mistaken; since, truth being the common object of all our pursuits, we ought candidly to accept as well the assistance we receive from each other for bringing us into the right road, when we happen to have strayed from it, as for helping us forward on our journey.

LVII. Astronomical Observations made at the Island of St. Helena. By Nevil Maskelyne, M. A., F.R.S. p. 348.

These are observations chiefly of the moon and stars transiting the meridian and wires of the telescope, &c. also of the setting sun. Not of material use now at this time.

In the conclusion Mr. M. takes occasion to notice a former remark of Mr. Short's on some of his preceding observations. " I must not pass by this occasion, says Mr. M. without taking notice of some remarks, which Mr. Short passes on my method of examining the going of the clock, by observing stars setting behind a hill, with the telescope of the equal altitude instrument; (vide Mr. Short's account of Mr. Mason's paper concerning the going of Mr. Ellicott's clock at St. Helena, Phil. Trans. vol. lii. part 2. p. 540). Mr. Short's represents Mr. Mason, as saying in his paper, that I proposed making use of the equal altitude instrument to determine the regularity of the motion of Mr. Ellicott's clock, by observing the vanishing of the stars out of the field of the telescope, an expression not contained in Mr. Mason's paper, who is only speaking of our observing stars setting behind a hill, at the distance of a quarter of a mile, in the same part of the field of the equal altitude instrument. Had we proceeded in the method supposed in the remarks, no doubt the observations would have been liable to considerable inaccuracy; but as we used the telescope of the equal altitude instrument, only to assist the sight in observing the stars setting behind the hill, we were liable to no other error than what might arise from the small alterations of the instrument, arising from the changes of heat and cold, moisture and dryness, seen from the distance of the top of the hill, which will easily be allowed to be quite insensible. And indeed how otherwise could the observations, contained in Mr. Mason's paper, agree so well together as they do? A circumstance alone sufficient to create a suspicion of the objection being ill grounded. The reason of Mr. Mason and myself always observing the stars to vanish behind the hill, in the same part of the field of the telescope, that is, very near its centre, was in order to keep the object glass at the same height; though this being less than an inch in diameter, and consequently subtending less than 13″ from the top of the hill, there could not have been a second of time difference, whether the stars had been observed to vanish behind the hill, either in the upper or lower part of the field of view.

Mr. Short also remarks, that no inference can be formed with respect to the different forces of gravity, in different latitudes, from experiments made with clocks, because the same clock, set up on different sides of the same room, will be found to differ considerably from itself. I readily allow, that if clocks are fixed up in a slight manner, or against common wainscots, the experiments made with them cannot be depended on. Yet it does not appear, but that when they are fixed in a firmer manner, they may be depended on near enough to be of considerable use in physical inquiries; which I have reason to think from the many experiments I have tried with the Royal Society's clock, made by Mr. John Shelton, which I propose to give a particular account of at some other opportunity."

LVIII. Of an Extraordinary Disease among the Indians, in the Islands of Nantucket and Martha's Vineyard, in New England. By Andrew Oliver, Esq., Secretary at Massachusett's Bay. Dated Boston, Oct. 26, 1764. p. 386.

The uncommon sickness, hereafter described, which prevailed in 1763, at the islands of Nantucket and Martha's Vineyard, which lie about 6 or 7 leagues from each other, and the latter about 4 or 5 leagues distant from the Indian plantation at Mashpee on the continent, where it did not make its appearance at all. As Mr. O. had his account from the English minister, and from the physician at Nantucket, and from the society's missionary at the Vineyard, of each of whom he made the most scrupulous inquiry, the truth of it might be depended on.

About the beginning of August, 1763, when the sickness began at Nantucket, the whole number of Indians belonging to that island was 358; of these, 258 had the distemper between that time and the 20th of February following, 36 only of whom recovered; of the 100 who escaped the distemper, 34 were conversant with the sick, 8 dwelt separate, 18 were at sea, and 40 lived in English families. The physician stated, that the blood and juices appeared to be highly putrid, and that the disease was attended with a violent inflammatory fever, which carried them off in about 5 days. The season was uncommonly moist and cold, and the distemper began originally among them; but having once made its appearance, it seems to have been propagated by contagion; though some escaped it, who were exposed to the infection.

The distemper made its appearance at Martha's Vineyard the beginning of December, 1763. It went through every family into which it came, not one escaping it: 52 Indians had it, 39 of whom died: those who recovered were chiefly of the younger sort. The appearance of the distemper was much the same in both these islands; it carried them off in each in 5 or 6 days. What is still more remarkable than even the great mortality of the distemper is, that not one English person had it in either of the islands, though the English greatly exceed in numbers; and that some persons in one family, who were of a mixed breed, half Dutch and half Indian, and one in another family, half Indian and half Negro, had the distemper, and all recovered; and that no person at all died of it, but such as was entirely of Indian blood. Hence it was called the Indian sickness.

There had been a great scarcity of corn among the Indians the preceding winter: this, together with the cold moist season, have been assigned by some as the causes of the distemper among them. These circumstances, it is true, may have disposed them to a morbid habit, but do not account for its peculiarity to the Indians: the English breathed the same air, and suffered in some measure in the scarcity, with the Indians; yet they escaped the sickness.

LIX. Astronomical Observations made at the Island of Barbadoes; at Willoughby Fort; and at the Observatory on Constitution Hill; both adjoining to Bridge Town. By Nevil Maskelyne, A. M., F. R. S. p. 389.

The object of Mr. M.'s voyage to Barbadoes was, on the part of the Board of Longitude, to observe the going of Mr. Harrison's timekeeper, in keeping the longitude at sea; though that object is not noticed in this paper, nor the result of it. The observations here stated being chiefly of eclipses of Jupiter's satellites, and occultations of stars, &c. are not now of importance to be reprinted in these Abridgments.

Besides the above observations, says Mr. M., I have taken a great many of the difference of right ascension between the ☽'s enlightened limb and proper stars (which I have not yet reduced) by means of parallactic wires in the focus of my 18 inch reflecting telescope; from which, after making the requisite calculations, I make no doubt of being able to deduce the moon's horizontal parallax in that latitude, and thence, by proportion, the equatorial parallax of the moon with great exactness, which has never yet been done in so direct a manner.

LX. Further Remarks on M. l'Abbé Barthelemy's Memoir on the Phœnician Letters, containing his Reflections on certain Phœnician Monuments, and the Alphabets resulting from them. By the Rev. John Swinton, B. D., F. R. S. p. 393.

M. Barthelemy's memoir on the Phœnician letters has again, with very large additions been communicated to the learned. Some at least of those additions have been made, as, Mr. S. says, there is good reason to believe, if not very lately, several years after the memoir itself was read. This, as the Abbé is said to be the first antiquary in France, and must undoubtedly have a great influence over the members of that illustrious body which he has so long adorned, cannot well fail of being considered by many people as a confirmation of the suspicion for some time entertained in several parts of Europe, and hinted at by Mr. S. in a former paper. It will therefore enable us to account for the late publication of a piece, which seems to have been applauded by the Abbé's admirers as one of the most valuable literary productions of the present age. What degree of attention to this performance from the lovers of antiquity is really due, Mr. S. shall not at present take upon him to decide. His sentiments of it, however, if not yet sufficiently known, from the following short additional remarks, submitted with the utmost deference to the superior judgment of the R. S., will very clearly appear.

It is needless to add that Mr. S. controverts many of the explanations given by M. B. relative to the Maltese Phœnician inscription, before noticed by Mr. S. in the Philos. Trans. At the conclusion of this rather unprofitable dispute, Mr. S. sums up the whole in the following deductions.

" Hence it seems to appear, that the names of two Amathusians, probably of the first distinction, one of whom was unfortunate enough, have been handed down to us, and perhaps to all succeeding ages, by this sepulchral inscription. It must be further observed, that this curious monument consists of 4 short periods, every one of which may, in some respect, be taken for a complete inscription. But this is a property it has in common with other similar remains of antiquity. Thus the Sigean inscription is composed of 4 such periods, and 3 are exhibited by the Punic inscription in a former paper explained.

" I have hinted above, that the inscription is come down to us perfect and incorrupt; not so much as one of its letters having been either lost, or greatly damaged, by the injuries of time. To which I shall now beg leave to add, that the words formed of these letters are, for the most part, distinguished from one another by points, placed between them; which must, in a good measure at least, ascertain the lection here, and of course greatly facilitate the explication. The Etruscans sometimes separated their words from one another by two points, and sometimes by a single one only, as we learn from the Etruscan inscriptions on the celebrated tables of Gubbio, and others published by Sig. Gori, in the learned work referred to, which may be considered as a noble repository of all kinds of Etruscan antiquities. The earlier Greeks also used the first kind of interpunction, as we learn from the Sigean, Teian, and other ancient inscriptions. That they likewise applied 3 points for the separation of their words, on some occasions, though more rarely, as well as the Etruscans, is not unknown to those who have been conversant with the antiquities of these nations. I must further observe, that this minute kind of mark, though generally termed a point, was originally of a triangular form, as may be inferred both from our Citiean inscription, in which some of the minute black triangles plainly appear, and one at least of those preserved by the tables of Gubbio, of which so accurate a transcript has been communicated by Sig. Gori to the learned world. That these points are a certain indication of a pretty remote antiquity, is by the most competent judges of such matters readily allowed. How far therefore this interpunction and ancient history may conspire, in order to settle the age of the monument under consideration here, I am next to inquire.

"Abdemon, the Citiean, one of the Persian monarch's friends, having been expelled Salamine by Euagoras, that prince meditated the reduction of the whole island of Cyprus; in which, within the course of a few years, he made a very considerable progress. This alarming the Amathusians, Citieans, and Solians, governed then, as it should seem, by their own princes, they made the proper dispositions for opposing his ambitious designs. But not believing themselves able alone to cope with him, they applied to the Persian court for assistance. Artaxerxes Mnemon, who then sat on the Persian throne, was also himself become jealous of the growing power of Euagoras, and therefore readily entered into an alliance with the three confederated cities against him. To this he was further excited by the murder of Agyrus, king of Amathus, and one of his most faithful allies, of which Euagoras was accused; and by the engagement the three Cyprian states had entered into, to put the whole island, if possible, into his hands. In order therefore to crush Euagoras at once, Artaxerxes sent an army of 300,000 men, under the command of Orontes, one of his sons-in-law, to invade Cyprus, in the 3d year of the 98th Olympiad, or the year before Christ 386. This formidable army was attended by a fleet of above 300 sail, of which Gaus, the son of Tamus, or, as the Phœnicians wrote and pronounced the word, Tam, probably the TAM of our inscription, was admiral. This Tamus is said to have been born at Memphis, and consequently by birth to have been an Egyptian, though he was probably of Phœnician extraction. Being a person of great valour, and uncommon skill in maritime affairs, he first served Tissaphernes as a naval officer; but was afterwards employed by Cyrus, who rebelled against his brother Artaxerxes, and was killed in the battle of Cunaxa, as chief commander of his fleet. He had also been appointed governor of Ionia by that prince. Tamus was treacherously cut off, with all his family, except his son Gaus, now the Persian admiral, who staid behind in Asia, by Psammitichus, king of Egypt, about 14 years before. Euagoras's fleet of 200 sail was defeated near Citium by Gaus, the son of Tamus, or Tam, with the loss of most of his ships; though Euagoras had, before this naval engagement, gained a considerable advantage over a part of the combined army of Persians, Amathusians, Citieans, and Solians, almost immediately after the descent had been made. From this short narrative, extracted from writers of the best reputation and authority, are naturally deducible the following observations.

"Tamus, or Tam, probably the TAM of our inscription, admiral of a Persian fleet, and governor of Ioania, was cut off by Psammitichus, king of Egypt, together with his whole family, except his son Gaus, about 14 years before the commencement of the Cyprian war. 2. Gaus, the son of Tamus, or Tam, admiral to Cyrus, who was killed on the plains of Cunaxa, actually commanded the Persian fleet, and defeated that of Euagoras near Citium, in the beginning of that war. 3. Part of the combined army of Persians, Amathusians, Citieans, and Solians, was routed by Euagoras, a little before the naval engagement. 4. From what has been intimated by Diodorus Siculus we may infer, that this action certainly happened at no great distance from Citium; as the battle by sea was fought near that place, and as the fleet and army must have acted in concert, both at the debarkation of the troops, and for some time after that event. 5. It must therefore be allowed probable, that the two Amathusians mentioned in our inscription, who seem to have been persons of distinction, were killed either in the aforesaid action, or in the naval engagement that immediately followed, or in some other affair that happened much about the same time. 6. The monument recorded by our inscription was probably erected by some of Gaus's family, who might call themselves the house of Tamus, his father, several instances of such an appellation occurring in ancient history. This might have happened after Gaus's death, which was about 2 years posterior to the commencement of the Cyprian expedition. The erection of it certainly ought not to be attributed to Tamus's daughter, as some may perhaps pretend: all that admiral's family, except Gaus, having been cut off with him, by Psammitichus, king of Egypt, 14 years before. 7. From the preceding narrative we may infer, that ancient history, particularly that of Diodorus Siculus, from whence it is chiefly extracted, and our inscription mutually strengthen and support each other. 8. Hence it seems pretty clearly to appear, that the death of Abdasar and Lemb, or Lemeb, the event commemorated by our inscription, preceded the commencement of the Christian æra 386 years; and consequently that this inscription is coeval with those, found likewise in the ruins of Citium, by me some years since explained."

END OF THE FIFTY-FOURTH VOLUME OF THE ORIGINAL.

I. Of the Pholas Conoïdes. By J. Parsons, M. D., F. R. S. p. 1. *Vol. LV.*

This shell is pictured by Rumphius, and called pholas lignorum; in Dutch hout-mossel, wood muscle, because it is found burrowed in timber. This specimen is one of infinite numbers that were thus bedded in the keel of a Spanish ship, brought from the West Indies, a piece of which accompanies the shell, to show how they lie in wood, stone, or any other hard bodies, that contain them. But this name is altogether too vague and uncertain, unless it could be asserted that this is the only kind that inhabits pieces of wood; for every species of pholas penetrates that and other solid substances likewise, and so do various other shell-fish. Therefore, as all subjects in natural history should have some precision in the appellations which distinguish them from each other, the best and most clear method of giving names to them, is certainly to call them after some striking character proper to them specifically: he therefore gives this species the above title, viz. pholas conoïdes, being very different in its form from the oblong, the broad, the pointed, the cylindrical, and every other pholas he has seen. And as the figure given by Rumphius is so imperfect, and this name so general, it was necessary to give an accurate account, as well as an exact representation of so curious a species; and therefore he drew it in 4 views, and describes it as follows:

If we observe this specimen, as it is entire, it will appear to consist of 2 great valves, an anterior long piece, a posterior long piece, and an orbicular detached piece at the end of this, at that extremity which may be called the base of the cone. These make but 5 pieces to complete the whole, unless the white smooth parts, at the broad ends of the great valves, be accounted separate pieces, which they really are not, but absolute portions of the same valves; and, as to the circular piece on the back, it appears to be entire, and not divided into 2; if it may be accounted 2, then the whole would consist of 6 pieces, according to Mons. de la Faille's opinion, who seems fond of that number in the pholades.

It is $1\frac{1}{4}$ inch long, and $\frac{3}{4}$ of an inch thick at the base; and this appears to be its utmost size, because the others, which are in this piece of wood, seem nearly of the same magnitude. The wood in which they lie was said to be cedar; but it rather seems to be of fir, having a fissile grain like common deal wood, and is as easily split as that; it is also extremely light, and its fibres are very loose, nor has it the least smell like cedar; they have the same colour, which is of a yellowish cast; but the cedar has a close smooth grain, though it is a light wood, and soft in itself: whereas this does not seem to be as hard as what we call white deal, which is esteemed the lightest and tenderest of all the class of firs.

The great valves are of a dusky white, inclining, towards the base end, to a purplish cast; where the striæ are very fine and minute, running upwards to meet those which are larger on the main body, in a wavy curve direction; and the smooth parts of these, as well as the other pieces, are perfectly white, and without any striæ at all. The base end of this fish seems covered with 3 plates that are white and smooth; but these are only the 2 smooth pieces of the great valves, and the circular posterior plate, mentioned before: and where these meet, that are 2 depressions, which terminate in 2 holes in the bases of the great valves, which are half covered by the posterior circular piece. This apex is round and flattish, and forms almost a sharp edge by the concurrence of the 2 great valves, and the fore and back edges are united by the long pieces before-mentioned.

The texture of the shell is very thin and brittle; so that it is wonderful to see the holes they lie in so smooth and uniform, as if bored with a hard sharp instrument. The base end is always inward, and the hole, which opens from them outwards, very small; and this is the case of every kind which are thus lodged, whether in wood or stone; so that we must conclude that they are deposited there in a very minute state, and not in a state of maturity; for then they must bore their way inwards, and the hole would be as wide outwards as inwards, and consequently be of equal diameter. But how these animals maintain and increase the cavity, as they grow larger, is a question which it will be very difficult to resolve, and has puzzled several ingenious naturalists in the inquiry.

It is said they have a power of turning themselves about with a swift motion, and so gradually make themselves room; but this will be hard to conceive, if we consider that a fish closely shut up within its valves, and compressed on all sides, can have no power of motion. We cannot imagine any animal can move itself, when thus confined, without some fulcrum or point of effort, from which to begin such motion; and if they had such tentacula as were capable of seizing on the wood, in order to exert themselves, there can be no room for it, for it is in close contact with themselves in every point. That this is the case is very clear, from considering the state of toads, frogs, and other animals, inclosed in blocks of marble, trunks of trees, &c. which have no communication with the atmosphere at all. These are soft animals, and their shape not at all fit for turning about and boring their cavities: and yet they are found in moulds as exactly fitted to their bodies, as those are to melted matter cast into them by a founder. It may however be supposed that the stone and wood does actually give way to the growth of the animal within, because the facts are well attested; but how this comes to pass, in these pholades and cylindrical muscles, and by what means toads, &c. can receive aliment to cause their growth, without any external communication, must yet remain among those secrets of nature, which we cannot but admire, without knowing how they are brought about.

Fig. 3, pl. 5, is a view of the surface of one of the great valves, with the edges of the two longitudinal pieces, and with portions of the smooth parts at the round extremity or base. Fig. 4, shows the anterior edge of the pholas covered by the long smooth white piece, and at the base having part of the smooth portions of the great valves in view. Fig. 5 represents the posterior edge of the pholas, with the round white piece on the base end, and the long ditto, which is larger than that of the fore edge, running towards the apex. Fig. 6 is a view of the base extremity, which is round like a hemisphere, showing the 2 holes, one at the end of each great valve, just where the processes of their smooth portions, and the edge of the round piece, meet.

The apices of some of these conoïde pholades are a little curved; but that of this subject described is straight. Besides these, there were great numbers of cossi, or worms, in the bottom of the Spanish ship; the vestige of one or two of them, as visible in this piece of wood, and the channels they make, which are in all directions, are lined with a thin white incrustation, and are of equal dimensions all along.

II. The Case of a Young Lady who drank the Sea Water for an Inflammation and Tumour in the Upper Lip. By Dr. Lavington, of Tavistock, in Devon. p. 6.

This young lady, aged 16, had a strumous swelling of the upper lip, for which she was advised to drink the sea water, which she did every morning, to the quantity of a pint, for 10 days successively; during which she was as well as usual, till on a sudden she was seized with a profuse discharge of the catamenia. She had also a bleeding from the gums, with innumerable petechial spots on the neck and breast, and many livid spots on the arms and legs. Her pulse was very quick, though pretty full; her face pale and bloated, and her flesh soft and tender. To stop the bleeding from the gums, her apothecary took a little blood from the arm; from the orifice there oozed blood continually for several days, notwithstanding all endeavours to staunch it. At last blood issued from her nose perpetually, attended with frequent faintings, in which she at length expired, choaked as it were with her own blood. But before she died, it was very remarkable, that her right arm was quite mortified from the elbow to the wrist: and it is to be further noted, that though blood drawn from her some weeks before she began the use of the sea water for an inflammation in her lip, was found sufficiently dense, and in a pretty good state; yet that drawn off in her last sickness was mere putrid dissolved gore.

To this account Dr. L. subjoined the following queries:—Whether or no, a scorbutic state of the animal juices may not be produced by salt water, as well as by salt provisions; especially if, as in the present case, it does not pass off freely by the usual evacuations, which often happens when drank for a considerable time, and the body is accustomed to it?—Whether the thin tender delicate fibre is not a morbid disposition, somewhat different from the too viscid or too lax? and whether to such a constitution, attended with a loose texture of the blood, or a hectic habit, a salt water course may not be likely to increase the acrimony of the blood, rupture the vessels, and bring on a dangerous hæmorrhage? and whether, even to strumous patients thus circumstanced, the cortex Peruvianus is not more adapted?

To which Dr. Huxham replied nearly as follows: "In many cases I have known very good effects from a course of sea water, when drank in pretty large quantities, and long continued; but it was when it purged gently, and now and then puked somewhat. With the thin, tender, and hectical, it seldom agrees. The gross, heavy, and phlegmatic, commonly bear it with advantage. I have known it bring on colic pains, diarrhœa, dysentery, and bloody stools, cough, hectic heats, wasting of the flesh, and an hæmoptoë. It generally renders the body liable to very great constipation, after it has been drank for a considerable time."

III. Experiments and Observations on the Agreement between the Specific Gravities of the several Metals, and their Colours when united to Glass, as well as those of their other Preparations. Ey Edward Delaval, F. R. S. p. 10.

Sir Isaac Newton, in his optics, has showed, by a series of experiments, that the several differences of colours, exhibited by thin transparent plates, are occasioned by their several thicknesses; and that therefore the transparent parts of bodies do, according to their different sizes, reflect rays of one colour, and transmit those of another; and consequently that the size of the component particles of natural bodies may be conjectured from their colours; since the particles of those bodies most probably exhibit the same colours as a plate of equal thickness, provided they have the same density. He concludes this whole doctrine in these words: "I have hitherto explained the powers of bodies to reflect and refract, and showed that thin transparent plates, fibres, and particles, do, according to their several thicknesses and densities, reflect several sorts of rays, and thereby appear of several colours; and by consequence, that nothing more is requisite, for producing all the colours of natural bodies, than the several sizes and densities of their transparent particles."

Though he has accurately showed what colours arise from the several changes of thickness, I do not find, says Mr. D., that any one has attempted to explain in what manner the differences of density, in the component particles of bodies, contribute to produce the several differences of colours: and therefore I thought that if instances could be produced, of bodies whose several differences of colour appear to be proportioned to their several degrees of density, it would tend to illustrate this part of optics. To this purpose however are conducive all those experiments and observations, from which Sir Isaac Newton has inferred that bodies have their refractive and reflective powers nearly proportional to their densities; and that the least refrangible rays require the greatest power to reflect them: which is deducible from hence, 1. That the red rays are reflected at the greatest obliquity of incidence, and the violet at the least. 2. That the violet is reflected, in like circumstances, at the least thickness of any thin plate or bubble the red at the greatest thickness, and the intermediate colours at intermediate thicknesses. 3. The same appears from the table (p. 206) in which the thicknesses of air, water, and glass, and the colours produced by them, are set down. These experiments are applied by him to transparent bodies, and the colours exhibited by them; but they are equally applicable to permanently coloured bodies: and it appears from them, that denser substances ought, by their greater reflective power, in like circumstances, to reflect the less refrangible rays, and that substances of less density should reflect rays proportionably more refrangible, and thus appear of several colours in the order of their density.

In confirmation of this reasoning, I shall give instances of natural bodies, which differ from each other in density, though circumstanced alike in other respects; and shall show that they differ in colour in the same order as they do in density, the densest being red, the next in density orange, yellow, &c. In such an inquiry, metallic bodies seem to deserve our first and principal attention, as their specific gravities have been ascertained by well known and repeated experiments. Without entering into a minute chemical theory of the principles of metals, it is sufficient to observe that they are universally allowed to consist of 1. An inflammable or sulphureous matter, which is of the same kind in all the metals; 2. Of a fixed matter or calx, which appears in each of the metals to be specifically different in weight, as well as in other properties.

As the sulphureous matter, in the entire metals, acts strongly on the rays of light, it is necessary to calcine, or to divide them into extremely minute particles, in order to examine separately the action of the calx, or fixed matter, on the rays of light. In order to examine all the metals in like circumstances, by reducing them into the smallest particles, and depriving them of their sulphur as far as was practicable, I exposed each of them, united with a proper quantity of the purest glass, without any additional ingredient, to the greatest degree of fire they are capable of bearing, without having all colour whatever destroyed. In this state it appears, from a variety of experiments and facts, that they actually do, without any exception, exhibit colours in the order of their densities, as annexed.

Gold	—	Red.
Lead	—	Orange.
Silver	—	Yellow.
Copper	—	Green.
Iron	—	Blue.

GOLD,

Which is the densest of all the metals, imparts a red colour to glass, whenever it is divided into particles so minute, that it can be intimately mixed with the ingredients of which the glass is made; and it seems indifferent in what manner it is reduced to this state. Thus

1. From the powder obtained by rubbing gold with a pumice stone, used by the goldsmiths in polishing it, mixed with nitre, borax, and potash, a beautiful

red glass is produced. 2. When a small quantity of a solution of gold in aqua regia is evaporated on a glass plate, with a gentle heat, that part of the glass on which it lay thinnest, is tinged red, by the entrance of the particles of gold into its surface. 3. Artificial rubies are made by mixing with glass, gold dissolved in aqua regia, and afterwards calcined in the furnace. 4. Kunkel prepared a powder for the same purpose, by precipitating the gold from the solution by an alkaline liquor. 5. Gold precipitated by tin from aqua regia, and melted with glass in a proper proportion, tinges it with a beautiful ruby colour: this method was discovered by Cassius, and further improved by Kunkel. 6. The same colour is produced by fusing gold with a large proportion of tin, and two thirds of lead, or by mixing it with regulus of antimony, or tin by calcination, and adding to glass the powders of gold obtained from these processes. 7. Gold amalgamated with mercury, and digested with it for a considerable time, may be reduced to a subtile powder by expelling the mercury: this powder, melted into the glass, tinges it of a beautiful red. 8. Gold leaf melted into the surface of glass, by the electric force, imparts a red colour to it: this was first observed by Dr. Franklin, and has been often repeated.

There are many other ways of communicating this colour to glass by gold; and I find no method by which it can be made to produce any other colour. If it be mixed in larger masses, without being minutely divided, it imparts no colour to the glass, but remains in its metallic form. Grummet attributes this colour to the manganese used in making some sorts of glass, the colour of which he supposes revivified by the nitre used in the preparation of the gold: it is necessary therefore to mention, that I have given a red by gold to several glasses, in the composition of which there was no manganese, and often by gold in the preparation of which there was no nitre. Several preparations of gold will impart a fine red to the frit or materials of which glass is made, in a small degree of heat; though not minutely enough divided, or in too large quantity, to remain mixed with the glass, when exposed to a degree of heat sufficient to vitrify them perfectly.

LEAD,

which is the metal whose density is next in order to that of gold, affords a glass of the colour of the hyacinth, a gem whose distinguishing character is, that it is red with an admixture of yellow, the same colour which by writers on optics is called orange.

1. Lead, kept in fusion for a considerable time, in a strong crucible and a very violent heat, is reduced to a glass of the colour of that gem. 2. Lead reduced to litharge, and melted with one 3d or 4th part of its weight of sand, in a covered crucible, in a strong fire for 2 or 3 hours, unites with the sand into an orange-coloured glass like the former. 3. Glass of lead is mentioned by

several authors, as a composition proper, without the addition of any other ingredient, for imitating the hyacinth.

SILVER.

Yellow is the only colour which silver, the metal next to lead in density, can by any preparation be made to impart to glass.

1. Without insisting on what some chymists affirm, that silver, on being calcined and exposed to a violent fire for a long time, was partly reduced to a yellow glass. 2. I have often given that colour by moistening the surface of the glass with a solution of silver, and afterwards making it red hot. 3. If silver be calcined with sulphur, it readily communicates a yellow colour to glass. 4. Having carefully purified an ounce of silver, I kept it in fusion some hours, with a small quantity of glass, and found that the glass, when cold, had formed a beautiful yellow enamel on the surface of the silver. 5. Leaf silver, laid on red hot glass, tinges it yellow.

When we meet with authors, who mention a blue or greenish colour communicated by silver, the cause must have been, that the silver used in such processes was mixed with copper, as it generally is, when it is not carefully purified. I have always found, that silver purified by the test retained so much copper, that when melted several times with nitre and borax, it imparted a green tinge at the 1st and 2d melting, though afterwards no such colour was obtainable from it.

COPPER.

Green is the only colour which copper, the metal next to silver in density, communicates to glass, when melted with it in a sufficient heat, without any additional ingredient: Thus 1. By grinding crystal glass in a copper mortar, and afterwards melting it, it becomes green. 2. Copper calcined per se in a furnace. 3. Copper calcined with sulphur; and 4. Scales beaten off from red hot copper plates, mixed with glass, equally impart a green colour to it.

It is indifferent in what manner the copper is prepared, in order to tinge the glass green, provided it be exposed, without any other ingredient, to a sufficient degree of heat. I have frequently produced a fine green from copper filings unprepared. If a quantity of salts be added in the preparation, they will, by attenuating the mixture, and consequently lessening its specific gravity, make the glass incline to blue, the colour next in order; but this happens only when the fire is moderate; for in a greater degree of heat, the redundant salts, even those of the most fixed nature, are expelled.

It is true, that copper is mentioned by some writers, as an ingredient in red glass and enamel: but the red, which is the colour of the metal not dissolved or mixed with the glass, remains only while the composition is exposed to such a degree of heat as is too small to melt and incorporate it; for, if it be suffered to remain in the furnace a few minutes after the copper is added, the mass will turn out green instead of red: in effect, the preparation of copper recommended on this occasion, is exactly the same as that used in tinging glass green.

IRON,

being of all metals the most imperfect, is subject by various means, to be calcined or reduced to a ruddy crocus, similar to the rust that arises from its being corroded by the acid in the air. In this state it requires a considerable degree of heat to dissolve and incorporate it with glass: till that heat is applied, it retains its ruddy colour; by increasing the heat, it passes through the intermediate colours till it arrives at its permanent one, which is blue: this being effected in the same degree of heat in which we have examined the other metals, that is, the greatest that the glass will bear without losing all colour whatever. The green, with which the glass used for bottles and chemical vessels is tinged, is occasioned by the iron contained in the vegetable ashes and sand of which that glass is composed. When the pots, in which the matter has been kept in fusion, are nearly emptied, the glass remaining at the bottom is always blue; this is caused by its continuing longest exposed to the fire, and in so small a quantity that the fire has a greater effect on it. The whole mass acquires the same colour, if too much sand be added in proportion to the ashes; for in that case the materials being more difficult of fusion, the workmen are obliged to apply a greater heat, and to continue it longer. It is known, from the experiments of Lemery and others, that the vegetable ashes contain iron. To examine whether that metal be also contained in the sand used in making this glass, and how far the colour of the glass depends on it, I made the following experiments:

Exper. 1. Having procured some of the sand used in making green glass, I melted 2 parts of it with one part of borax, and one part of nitre; and found that it produced a glass similar in colour to that which is made with the same sand fluxed with pot ashes. Hence it appears, that the colouring matter was contained in the sand.—*Exper.* 2. I mixed 3 parts of this sand with one of powdered charcoal, and exposed it for some hours to a red heat. When this mixture was cold, I separated from it, by a magnet, small grains of iron, weighing about $\frac{1}{80}$ part of the sand.—*Exper.* 3. I melted sand thus deprived of its iron, with half its weight of borax, and the same quantity of nitre; and found that it produced a perfectly colourless and transparent glass.—*Exper.* 4. To 2 parts of the white sand used in making crystal glass, and one of borax and nitre, I added a 20th part in weight, of the grains of iron which I had extracted from the sand by exp. 2; and having vitrified this composition, I found that it was become exactly similar, in colour, to that commonly used in making green glass.—*Exper.* 5. I exposed several pieces of green bottle glass made at different glass-houses, under a muffle, to a strong fire, for the space of half an hour; and found that they were all become blue.

If the crocus of iron be added in too great a proportion, it continues to adhere together, and remains unmixed, or at least imperfectly mixed, with the glass, retaining for that reason the colour natural to it when undissolved; or if it be in a smaller quantity, though yet in too great a proportion to be dissolved, it will make some intermediate colour between the ruddy and the blue, which last it always imparts when in a sufficient degree of fire, and a proper proportion. The necessity of a due proportion of metal to the glass has been already instanced in gold, which, if in too large a proportion to be dissolved by the glass, instead of imparting a red colour to it, runs together in its metallic form.

Henckel has given us a method of making a beautiful blue glass by this means. It consists merely in mixing iron with the matter of which the purest glass is composed, and exposing it to a violent fire. Gellert observed also, that iron imparts to glass this colour. Mr. Lehman obtained the same colour from emery, which is a kind of iron ore, or ferruginous stone, by mixing it with a vitrifiable earth; which colour he attributes to the iron contained in it. Neri mentions a sky colour imparted to glass by Bohemian granates, which he constantly practised at a manufactory in Flanders. It is well known that iron is the metal contained in those stones; that they obey the loadstone; and that, being calcined with a proper heat, they yield a considerable quantity of iron.

I exposed in a crucible to a glass-house fire, for the space of 30 hours, part of a flint-glass retort, in which a native green vitriol of iron had been distilled, and which had been corroded and tinged by it; by this means it became coloured of a fine transparent blue, not distinguishable from that which cobalt imparts to glass. Iron vitrified per se is converted into a blue glass. In short, it is indubitable that iron is the only metal, which will, without any addition, impart to the matter of glass a blue colour; for copper will not communicate that colour, without the addition of a considerable quantity of salts, or some other matter that attenuates it; and the other metals cannot by any means be made to produce it at all.

Having showed that the metals exhibit colours, invariably in the order of their densities, when melted with glass in a due proportion, without any other ingredient, and exposed to a sufficient heat; I proceed to show that the other preparations of the metals, viz. their solutions, precipitates, crystals, &c. do for the most part exhibit the same colours, in the order of their densities, though not so invariably as their glasses; some small variation of colour happening in the more imperfect metals, probably from a change of density in their different preparations.

GOLD.

1. Gold precipitated from aqua regia, and washed with hot water, or boiled

in a solution of alkaline salt, becomes red on being exposed to a slight heat. Lewis, history of gold, p. 108.—2. The same colour is produced when this precipitate of gold is ground with oil of vitriol, or spirit of sulphur; or if it be mixed with sulphur, and the sulphur burnt away. Junker, tab. 33, p. 859.—3. The smoking spirit of Libavius, mixed with gold, and afterwards drawn off from it by distillation, changes its colour to a blood red. Sol sine veste, exp. 19. Junker, tab. 33, p. 861.—4. Gold is reduced into a red powder, by amalgamation with mercury, and exposing it for a considerable time to a slow heat. Boyle's Abridg. vol. 2, p. 77. Junker, tab. 39, p. 987.—5. If 6 parts of antimony be fused with one of gold, and the antimony driven off by the blast, a red powder of gold is left behind. Cassius de Auro, cap. 10.—6. If gold leaf be cemented and ground with decrepitated salt, hartshorn, pumice, or chalk, and exposed to a proper heat, the metal becomes red, and may be precipitated from a solution of those substances in a red powder. Junker, tab. 33, p. 854. Lewis's history of gold, p. 74. Sol sine veste, cap. 6.—7. A red tincture may be prepared from gold by several methods mentioned by Libavius, Alchem. lib. 2, p. 130. Junker, tab. 33, p. 868.—8. A solution of gold in aqua regia, prepared from sal ammoniac, may be sublimed of a blood red colour. The same is effected by dissolving the calx, or crocus of gold, in other menstrua. Lewis's history of gold, p. 100. Junker, tab. 33, p. 857.—9. A solution of gold in aqua regia, evaporated properly, affords crystals of a bright red colour. Cassius de Auro, p. 109. Junker, tab. 33, p. 862, 868. Lewis's history of gold, p. 99.—10. Aurum fulminans moistened with water, has been found to tinge gems deeply of a fine red. Phil. Trans. N° 179.—11. A solution of gold tinges ivory, cotton, the skin, and other substances red.

Rubies being frequently found in gold mines, it is very probable that they receive their colour from that metal; and from this circumstance, before the experiment had been made, Libavius rightly conjectured that a solution of gold would communicate a ruby colour to glass. Libavii Alch. p. 88.—It does not appear that, excepting the colour natural to gold in its entire state, any other than red can be obtained from preparations of this metal: it is from this colour which gold assumes whenever its metallic brightness is destroyed, that writers in chemistry call it leo ruber.

LEAD.

The only coloured preparation of lead, is that produced by calcination in the furnace. The first of the primary colours produced by this process is yellow, the calx passing from that colour through orange into red. It is remarkable, that though in the calcination previous to the reverberatory heat in which these colours are produced, the lead is diminished in weight; yet in the reverberatory fire it gains considerably, and in proportion to that increase of gravity, it passed from the more refrangible to the less refrangible colours; so that while the calx remains of a less weight than that of the lead originally, its colour is yellow; with the next increase of weight it passes to orange, which is the colour of glass of lead; and when the calx is increased more in weight, so that its gravity is become greater than that of the lead originally, it passes into red, the next colour in order. These three colours, succeeding each other in proportion as the gravity of the metal increases, seems to prove that, in this case, the greater density produces the less refrangible colours: and as orange is the colour of this calx, when in a middle degree of weight, between that which is lighter and that which is heavier than the original metal, it appears that orange is the colour natural to lead when its weight is neither much increased nor diminished.

SILVER.

The only preparation of silver, which is of any primary colour (except the yellow it imparts to glass, and other vitreous substances, as earths or salts) is luna cornea, which Mr. Boyle says is of a fair yellow, Shaw's Boyle, vol. i, p. 255. Physical Essays, Edinburgh, 1754, vol. i, p. 310.

COPPER and IRON.

It appears then that all the preparations of gold, lead, and silver, invariably retain the colours peculiar to the order of their densities, and that they are the same with those which they communicate to glass. The two most imperfect metals, copper and iron, being very easily acted on by almost all menstrua, the colours of their solutions, &c. viz. green and blue, are apt to change into each other's order; the copper in some solvents becoming blue, and the iron green; and in other solvents vice versa; this probably depending on the increase or diminution of their densities. The solutions of copper, in the acids of nitre and sea salt, and in the vegetable acids, are green. But if copper be attenuated by solution in volatile alkalies, it becomes blue. Theophrastus and others have observed, that emeralds are frequently found in copper mines; and it is probable that they obtain their tinge from that metal.

I melted some emeralds with twice their weight of salts, and found that they had formed a fine green glass, such as would have been produced from the same quantity of a vitrifiable earth, and about a hundredth part of its weight of copper.—Iron dissolved by the vitriolic acid, is green; but if further attenuated by a chemical process, it produces that beautiful colour called Prussian blue. Phil. Trans. N° 38. Henckel, Dissert. 6.—A similar blue may be obtained from the iron contained in the ashes of all plants. Henckel, Flor. Sat. chap. 8, parag. 55.—Having exposed a pound of wood ashes in a luted crucible, to a pretty strong fire, for 30 hours, the greatest part of them became tinged blue by the iron contained in them.—A blue may be also extracted from a martial vitriol, by spirit of wine. Henckel, de Appropriatione, chap. 2, parag. 257.

An instance of a mineral substance changing its colour from green to blue on its specific gravity being diminished, appears in a stone described by Dr. Grew in the Museum of the Royal Society: this gem is a kind of emerald, which, when expanded by heat, becomes blue, and remains of that colour till cold, in which state it returns to its usual colour, which is green.

Tin is not capable of being vitrified, or imparting any colour to glass; nor are any preparations of it of any primary colour.

MERCURY.

1. There is no body of an intermediate weight between gold and mercury; and it is probable that a great part of the difference between their specific gravities depends on the fluidity of the one and the solidity of the other. Mercury is not capable of communicating any colour to glass, being so volatile that it will not bear the degree of heat necessary to incorporate it with the glass in fusion. But it is well known that its calx, either prepared per se, or by dissolving it in an acid, and evaporating the menstruum, is red.—2. A solution of mercury tinges the skin, &c. red, as gold does.

PLATINA.

The specific gravity of platina being nearly equal to that of gold, it seems necessary to examine whether the colour of its preparations correspond with those of gold. On looking into a dissertation written by Dr. Lewis on that metal, in the Philos. Trans., I find that the precipitates and crystals obtained from solutions of platina, are red: and that a solution of that metal in aqua regia, to perfect saturation, is of a dark red, though, when diluted, yellow; in the same manner that "a red liquor (as Sir Isaac Newton observes) in a conical glass, looks of a pale and dilute yellow, at the bottom, where it is thin; and a little higher, where it is thicker, orange; where it is thicker still, it becomes red; and where it is thickest the liquor is deepest and darkest." Newton's Opt. p. 160.

Having gone through these experiments and facts, which seem to show that the metals invariably exhibit colours in the order of their densities, when melted with glass, under the circumstances above mentioned; and that the other preparations of the same metals, for the most part, assume the same colours; it seemed probable that the cause, on which the colour of natural bodies depend, may sometimes be conjectured from the chemical analysis of such substances. This I have attempted with regard to the colour of plants. It is known, from the experiments of Lemery and others, that all earth is impregnated with iron; that the ferruginous matter is received into the roots of plants in their growth, and makes part of their substance, and is universally disseminated through them; and that iron may be separated by a magnet from the ashes of all vegetables.

It has been already observed, that the green colour of the glass used in making bottles, is caused by the iron contained in the materials of which it is made; and I have cited Becher's opinion, that the green or blue colour in glass is an indelible mark of its vegetable origin.

This observation of the constancy of that colour in glass made of vegetable ashes, and its being caused by iron, led me to conjecture, that the colour of the entire vegetables arises also from the iron, so universally diffused throughout their substance in their growth. Green is the colour which iron assumes constantly, when dissolved by the acid in the air; that metal thus dissolved being a true green vitriol of iron: and as this ferruginous or vitriolic matter is universally disseminated through the leaves and branches of plants, those parts of it which are at the surface will, by their contact with the air, assume the colour peculiar to its salt or vitriol.

Most vegetables, when they grow in such a manner as to be defended from the contact of the air, are prevented from becoming green. This happens to the root of trees, and as much of their stem as is covered with earth: grass growing under stones, or other bodies, that accidentally lie on it, is white; not having the least green, but as the air has access to it: and it is a method commonly used by gardeners, to cover with earth those parts of plants which they would preserve white: by that means hindering them from being tinged green by the contact of the air, as the parts exposed to it are: though it appears from experiment that the presence of light, as well as of air, is necessary to the production of the colour of plants.

Besides the iron dissolved at the surface of plants by the air, that which is contained in the inside of them, may be kept in a state of solution, when it meets with a proper quantity of acid; and it is remarkable that the inside of most fruits, and other parts of plants, remain green no longer than they continue in an acid state. The quantity of iron contained in plants will not appear too small to produce their colour, when it is known that one grain of vitriol, of which only a small part is iron, the rest being acid and water, is able sensibly to communicate a green colour to ten thousand grains of water. Lemery mentions this great divisibility of iron as an argument of its being able to pass into the smallest parts of plants. Mem. Acad. anno 1706.

A circumstance which strongly confirms that the colouring matter of vegetables, and a ferruginous vitriolic substance, are of one and the same kind, is, that the vitriol of iron, which is green, passes through the same colours, while its moisture is evaporating, which vegetables do, when by withering they undergo the same sort of change: the vitriol deprived of its water by calcination grows first yellow and then red; and Sir Isaac Newton has observed that, "when vegetables wither, some of them turn to a greenish yellow, and others to a more perfect yellow or orange, or perhaps to red, passing first through the aforesaid intermediate colours: which changes seem to be effected by the exhaling of the moisture, which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy parts of that moisture." Newton's Optics, lib. 2, prop. 7. This is the only passage in Newton, in which he instances any permanent colour of a natural body as arising from a change of density: and though he has not any where at large delivered his opinion on this subject; it appears that, in this case, he considered the less refrangible colours in withered vegetables as arising from their increase of density; which is what I endeavoured in the beginning of this paper to prove deducible from his doctrines.

IV. The Case of an Extraneous Body forced into the Lungs. By William Martin, Esq., of Shadwell. p. 39.

On Tuesday Oct. 23, 1764, about 6 in the evening, as one of Mr. M.'s maid servants was drinking coffee, and eating toast and butter, having a child in her lap, who had like to have scalded itself, and she was apprehensive would have broke the cup, being surprized, and attempting to speak hastily, in the very act of deglutition, she unfortunately forced a piece of the crusty part under the epiglottis, which made its way into the larynx, or upper part of the wind-pipe; and notwithstanding the many efforts of nature by a violent and incessant cough, to discharge it, yet it fixed like a wedge; and in a few hours she was deprived of her senses and her speech, and lay in great agonies, with violent agitations of body, resembling strong convulsions, fetched her breath very short, and it was expected she would expire that night. As Mr. M. was in the house when the accident happened, every method that he could possibly think of was made use of for her relief, but without effect; and as respiration was attended with the greatest difficulty, he ordered about 10 oz. of blood to be taken from her arm, which seemed in some measure to relieve her breath; this was 2 hours after the accident. The cough continued about 3 hours, till her strength was in a manner quite exhausted, and she seemed to be entirely senseless, except at intervals, pointing to her breast; and whenever Mr. Martin examined her pulse, forcibly, and in great agonies pressing his hand to the scrobiculus cordis, as the seat of her disorder. He left her about 12 o'clock that night; and desired his servants who attended her, to call him, if they observed any sudden alteration, which they accordingly did about 2 in the morning, when he was informed she was expiring as they thought, and indeed he was of the same opinion. He found her somewhat more sensible, but in great agonies; and making motions to be blooded again; as she had a full pulse, and the greatest difficulty in respiration, he took away about the same quantity as before, which seemed to give her some relief, and she continued much more quiet. At this time her cough had left her, and he was convinced by her complaints, that the extraneous body had made its way into one of the lobes of the lungs. It was with the greatest difficulty they could get her to swallow any liquid, which for some time threw her into violent commotions. In this languid state she continued for several days, begging of God to release her by death; and desiring Mr. M. to open her body, which he promised. But what was very remarkable in this case, notwithstanding the great agonies she was in while awake, yet the 2d night after the accident happened, she fell asleep about 12 o'clock, while insensible, slept sound for several hours; and while in that state of relaxation fetched her breath quite easy, as she did every night after, when asleep, with a serene pulse, but always waked in exquisite pain, and in great agitations.

In a few days Mr. M. observed her breath to smell very strong, and he made a prognostic, that nature would form an abscess or imposthume in the lungs, as the only chance for her life, and would bring it up, by the same channel it went down; though he was not without apprehensions, as there was a prominence pointing outwards, and attended with great soreness, that a collection of matter would be formed in the cavity of the breast, which, if it had made its way outwards, would in all probability have proved fatal. However, on the 11th day from the accident, she was of a sudden seized in the morning in bed, with a nausea, violent sickness, and a cough, when the imposthume in her lungs broke, and discharged itself by the mouth, with a large quantity of bloody matter, in which the portion of crust was happily entangled, about the size of a large hazel nut or filbert, being rather oblong than round, with a great quantity of slimy substance, in which it was enveloped. After which, for an hour or more, she complained of violent pain and soreness in the whole cavity of the breast, with great tremors; but they soon went off, and her speech returned immediately, and as perfect as before the accident, though languid; for, during the greater part of her illness, she could express herself no otherwise than by applying one's ear close to her mouth, and by giving her time, she could then, by a slow whisper, make them sensible of her wants; the extraneous body pressing, or in some measure obstructing, the fine threads or ramifications of the par vagum, or 8th pair of nerves, that come out of the brain, and are dispersed on the larynx, and accompany the bronchia to their minutest branches. She was then in a fair way of recovery, though weak, and afflicted with a dry cough, attended with an uncommon hollow sound, as if her lungs were impaired.

Mr. M. adds, that he never heard of any person recovering in a case of the like nature; though he had known several instances of this accident.

V. Of an Earthquake felt at Lisbon, December 26, 1764. In a Letter to the Rev. Sam. Chandler, D. D., F. R. S. p. 43.

Dec. 26, 1764, the writer was awakened about 3 o'clock in the morning by violent squalls of wind, and a deluge of rain, accompanied with almost uninterrupted flashes of lightning, many of which ended in a bright purple. About 8, were some distant claps of thunder, at 10, some more. Before 11, the rain ceased, and the sun shone forth. In less than half an hour after, was a sudden shock, preceded by a rumbling noise, and succeeded by a dead calm of at least half an hour. Most persons maintained that it was the severest shock they had felt; but as it lasted no longer than you could clap your hands twice, it did little or no damage. They unanimously declare however, that they never had any thing of the same kind, this being a sudden perpendicular heaving up; whereas all their former shocks were undulatory.

To know the strength and direction of earthquakes.—Take a vessel, being the portion of a sphere of 3 or 4 feet diameter. Place it on a ground-floor. Dust it all over on the inside with a barber's puff, and then pour some water gently into it. On the smallest tremor, the water will wash the flour from the parts of the vesssel on which it rises; and will thus mark the direction and height of the shock.

VI. Of the White Negro shown before the R. S. By James Parsons, M. D., F. R. S. p. 45.

It appeared that the parents of this boy were brought, among many others, above 300 miles from an inland country, to the gold coast in Africa, and put on board a ship bound to Virginia, where they arrived in the year 1755. They became the property of Colonel Benj. Chambers, of Cumberland county in Pennsylvania; and were then employed on an estate of the Colonel's in Virginia; but the Colonel lived with his family in Pennsylvania, where he sold the boy to his then master; of which fact, Dr. P. saw the bill of sale that passed between the Colonel and him.

The parents were perfectly black, and both very young when landed; and the woman being asked, how far she was gone with child? answered, so as to be understood to mean, that she was with child something more than 6 moons, and that this was her first pregnancy. They also declared, that they had never seen a white person before they came to the shore where Europeans where employed in buying black slaves.

The owner of this boy was Mr. James Hill Clark, whom Dr. P. informed of what had passed between Dr. Franklin and himself, on this subject; for he paid him a visit, and in the course of conversation he informed him, that while he was in England before, he received a letter from his lady, in which was some of the wool of a white negro child's head, by way of curiosity; and when he mentioned it to Mr. Clark, he assured him that this very boy was shown in Pennsylvania as a great rarity: and that, to his knowledge, the wool sent in the letter was taken from this child's head. He was born about 6 or 7 weeks after his parents landed in Virginia, in the year 1755; and was purchased by Mr. Hill Clark of Col. Chambers in 1764, so that he appeared not to be quite 10 years old; and his mother had had 2 children after, who were both as black as the parents.

Now, though this deviation of colour in the child, from the contrary hue of both parents, was very singular, and something preternatural, yet instances of the same kind had happened before. There was one about 4 years before in London, which was a white girl, something younger than this boy, but exactly similar in colour, wool, &c. and was said, by the person who made a show of her, to have been the offspring of a black father and mother. Dr. P. did not go to see her; but he read an advertisement concerning her, several times in the public papers, wherein she was called a white negro girl; and was informed by those that saw her, that she answered the description in the advertisement very truly. She was shown in town for some months every day.

To this case he subjoins 2 others, one of which he saw himself, and the other was given him by a gentleman of undoubted veracity; which, though they differ in some circumstances from the above, yet have so much relation to each other, as will prevent their being censured as digressions from the subject. The first was of a black man who married a white woman in York several years ago; of which he had an account from an eye-witness. She soon proved with child, and in due time brought forth one entirely black, and in every particular of colour and features resembling the father, without the least participation from the mother. This was thought a very singular case, because people naturally expect the issue of such a marriage would be tawny: which indeed is the usual effect produced by the congress of black and white persons. The 2d case was of a black man, servant to a gentleman who lived in the neighbourhood of Gray's-Inn. He married a white woman, who lived in the same family; and, when she proved with child, took a lodging for her in Gray's-inn-lane; when she was at her full time, the master had business out of town, and took his man with him, and did not return till 10 or 12 days after this woman was delivered of a girl, which was as fair a child to look at as any born of white parents, and her features exactly like the mother's. The black at his return was much disturbed at the appearance of the child, and swore it was not his: but the nurse who attended the lying-in woman soon satisfied him: for she undressed the infant, and showed him the right buttock and thigh, which were as black as the father, and reconciled him immediately to both mother and child. Dr. P. was informed of the fact, and went to the place, where he examined the child, and found it true; this was in the spring of the year 1747, as his notes specified which he took on the spot.

Wishing to add as much as possible to this account, he took an opportunity of inquiring about matters of this sort, in a family who came to live in Red-lion-square not many months before; and had lived in Virginia several years in a conspicuous light; and was informed by the lady of the family of the 2 following curious particulars: About 19 years previous in a small plantation near to that of this family, which belonged to a widow, 2 of her slaves, both black, were married; and the woman brought forth a white girl, which this lady saw very often; and as the circumstances of this case were very particular, he makes mention of them here, both for the entertainment of the Society, and to show that this is exactly similar to the case of this boy. When the poor woman was told the child was like the children of white people, she was in great dread of her husband, declaring, at the same time, that she never had any thing to do with a white man in her life; and therefore begged they would keep the place dark that he might not see it. When he came to ask her how she did, he wanted to see the child, and wondered why the room was shut up, as it was not usual; the woman's fears increased when he had it brought into the light; but while he looked at it he seemed highly pleased, returned the child, and behaved with extraordinary tenderness. She imagined he dissembled his resentment till she should be able to go about, and that then he would leave her; but in a few days he said to her, "You are afraid of me, and therefore keep the room dark, because my child is white; but I love it the better for that: for my own father was a white man, though my grandfather and grandmother were as black as you and myself; and although we came from a place where no white people ever were seen, yet there was always a white child in every family that was related to us."

The other account was, that Admiral Franklin had taken a Spanish ship in war time, and brought her into Carolina; and, on searching, found a picture of a boy who was as beautifully mottled all over with black and white spots as any dog that ever was seen; it is uncertain which was the ground, or which colour the spots were of; but this lady says, that several copies of the picture were taken in Carolina; and that they said it was the portrait of a child born of negro parents on the Spanish main; the ship was bound to Old Spain; and this lady does not doubt but the admiral might still have the picture in his custody. If these facts are ascertained by these 2 gentlemen, they will be worth recording with the present subject, which Dr. P. would take the trouble of inquiring into further.

These deviations of colour are indeed very extraordinary among the African negroes, but they are not peculiar to them; some parts of America have also similar variations from the common colour of the inhabitants; and as Dr. P. esteemed it a great happiness when he could contribute to the entertainment of the R. S., he could not excuse himself from adding to the above, what Mr. Wafer's Account of the Isthmus of America gives on the like objects in that country. See p. 134 of his Description, &c. London, 1699; where, after having described the natural copper-coloured complexion of the people, he says, "There is one complexion so singular among a sort of people of this country, that I never saw nor heard of any like them in any part of the world. They are white, and there are of them of both sexes; yet there are but few of them in comparison of the copper-coloured, possibly but 1 to 2 or 300. They differ from the other Indians chiefly in respect of colour, though not in that only. Their skins are not of such a white, as those of fair people among Europeans, with some tincture of a blush or sanguine complexion; yet neither is it like that of our paler people, but it is rather a milk-white, lighter than the colour of any Europeans, and much like that of a white horse. For there is this further remarkable in them, that their bodies are beset all over, more or less, with a fine short milk-white down; for they are not so thick set with this down, especially on the cheeks and forehead, but that the skin appears distinct from it. Their eye-brows are milk-white also, and so is the hair of their heads, and very fine too, about the length of 6 or 8 inches, and inclining to a curl. They are not so large as the other Indians: and their eye-lids bend and open in an oblong figure, pointing downwards at the corners, and forming an arch or figure of a crescent with the points downwards. Hence, and from their seeing so clear as they do in a moon-shiny night, we used to call them moon-eyed. For they see not well in the sun, poring in the clearest day; their eyes being but weak, and running with water if the sun shine towards them; so that in the day-time they care not to go abroad, unless it be a cloudy dark day. Besides, they are a weak people in comparison of the others, and not very fit for hunting, or other laborious exercises, nor do they delight in any such. But, notwithstanding their being thus sluggish and dull in the day-time, yet, when moon-shiny nights come, they are all life and activity, running abroad into the woods, and skipping about like wild bucks, and running as fast by moon-light, even in the gloom and shade of the woods, as the other Indians by day, being as nimble as they, though not so strong and lusty. The copper-coloured Indians seem not to respect them so much as those of their own complexion, looking on them as something monstrous. They are not a distinct race by themselves; but now and then one is bred of a copper-coloured father and mother; and I have seen a child of less than a year old of this sort.

"Some would be apt to suspect they might be the offspring of some European father; but besides that the Europeans come little here, and have little commerce with the Indian women when they do come; these white people are as different from the Europeans, in some respects, as from the copper-coloured Indians in others. And besides, where an European lies with an Indian woman, the child is always a mostese, or tawny, as is well known to all who have been in the West Indies, where there are Mostesas, Mulattoes, &c. of several gradations between the white and the black or copper-coloured, according as the parents are, even to decompounds, as a Mulatto-fina, the child of a Mulatto man and Mostesa woman, &c. But neither is the child of a man and woman of these white Indians white like the parents, but copper-coloured as their parents were. For so Lacenta told me, and gave me this as his conjecture how these came to be white, that it was through the force of the mother's imagination looking on the moon at the time of conception; but this I leave others to judge of. He told me also that they were but short-lived."

N. B. Lacenta was the king of the Indians among whom Mr. Wafer lived.

VII. Of an Improvement made by Mr. Peter Dollond in his New Telescopes: in a Letter to James Short, M. A., F. R. S., with a Letter of Mr. Short's to the Rev. T. Birch, D. D., Secret. R. S. Dated Surry-street, Feb. 7, 1765. p. 54.

"Inclosed is a letter (says Mr. S.) which I received this morning from Mr. Dollond, concerning an improvement which he has made in his new telescopes. He, some months ago, sent me a telescope, in this new way, of 3½ feet focal length, with an aperture of 3¾ inches; I examined it, and I approved of it; I have tried it with a magnifying power of 150 times, and I found the image distinct, bright, and free from colours."

I take the liberty, says Mr. Dollond, of sending you the following short account of an improvement I have lately made in the compound object glasses of refracting telescopes. The dissipation of the rays of light may be perfectly corrected in object glasses, by combining mediums of different refractive qualities; and the errors or aberrations of the spherical surfaces may be corrected by the contrary refractions of two lenses, made of the different mediums; yet as the excess of refraction is in the convex lens, and though the surfaces of the concave lens may be so proportioned as to aberrate exactly equal to the convex lens, near the axis; yet as the refractions of the two lenses are not equal, the equality of the aberrations cannot be continued to any great distance from the axis.

In the year 1758, when my father had constructed some object glasses for telescopes in this manner, viz. with one convex lens of crown glass, and one concave lens of white flint glass; he attempted to make short object glasses to be used with concave eye-glasses, in the same manner; but as the field of view, in using a concave eye-glass depends on the aperture of the object glass, the limits of the aperture were found to be too small; this led my father to consider, that if the refraction of the crown glass, in which the excess was, should be divided, by means of having two lenses made of crown glass instead of one, the aberration would thereby be decreased, and the apertures might then be larger: this was tried with success in those object glasses, when concave eye-glasses were used, and these have been ever since made in this manner: some trials were likewise made, at the same time, to enlarge the apertures of longer object glasses, where convex eye-glasses were used, by the same method; but these not succeeding in the same manner, the method of making them with one lens of crown glass, and one of white flint glass, was continued.

As I could not see any good reason why the method, which was practised with so much success, when concave eye-glasses were used, should not do with convex ones, I determined to try some further experiments in that way. After a few trials, I found it might be done; and in a short time I finished an object glass of 5 feet focal length, with an aperture of 3¾ inches, composed of two convex lenses of crown glass, and one concave of white flint glass. Thinking that the apertures might be yet admitted larger; I attempted to make one of 3½ feet focal length, with the same aperture of 3¾ inches, which I have now completed, and am ready to show the same to the R. S., if desired. The difficulty of procuring good glass of so large a diameter, and of the thickness required, added to the great exactness of the surfaces, in order to correct the aberration in such large apertures, has prevented me from attempting to extend them any farther in that length.

VIII. Of a Salt found on the Peak of Teneriffe. By W. Heberden, M. D., F. R. S. p. 57.

In the account of a journey up the Peak of Teneriffe, by Dr. Tho. Heberden, printed in the Philos. Trans., vol. 47, N° 57, mention is made of a sort of salt, as well as of brimstone, with which some parts of the peak are covered. There is no difficulty in conceiving how brimstone may be forced up by subterraneous fires; and it is no uncommon thing to find it in other places: but it is not so easy to understand how a salt, of so fixed a nature as this is, should be sublimed to such a height, without being cooled and fixed long before it arrives at the surface of the earth, where no sensible heat is perceived. Neither could Dr. W. H. explain how it happens, that a substance so easily melted in water, is not dissolved and washed away, as fast as it can be produced, by the dews, and rains, and melted snow.

By means of his brother Dr. Tho. Heberden, Dr. W. H. had procured a parcel of this salt collected from the peak, a specimen of which, together with some of the sulphur, he presented to the Society, both which, though so very pure, were just as they were taken up. His brother informed him, that the salt is found not far from the verge of the crater, and that it is called, by the Spanish inhabitants of the island, salitron; which is the name given by them to salt-petre; and that it is sold for about 5 pence a pound. It appears to be the natron or nitrum of the ancients, or, as it is sometimes called, the fossil alkali, which is the basis of sea-salts: the same which is procured from the Spanish barilla, and from our own kelp.

The mineral alkali differs from the vegetable in its ready crystallizing without any addition of fixed air, which is necessary to make the latter take the form of crystals;* and in its not melting in a moist air; and on this last account it is a much more commodious ingredient in medicinal powders, than the vegetable alkali, as it is not like this apt to run per deliquium; but on the contrary, instead of attracting moisture from the air, it is robbed by the air of its own moisture, so that its crystals soon lose their transparence, and are turned to powder. The natron liquifies in a very gentle heat: it resembles the vegetable alkali in taste and fixedness, and like that is used in making soap and glass: and they are both applicable to most of the same purposes. Of the crystals of natron, when very dry, but yet with scarcely any white powder on them, 100 gr. may be dissolved in 384 gr. of water, when Fahrenheit's thermometer is at 37. Such crystals quite dry, and just inclining to grow white, will lose $\frac{625}{1000}$ of their weight if dried, with a heat sufficient to scorch paper.

"The vegetable alkali has a stronger affinity to the acid spirits of vitriol, nitre, and marine salt, than the fossil;" for 1, if the common alkali be added to a saturated solution of Glauber's salt in water, the spirit of vitriol will leave the natron, and, uniting itself with the vegetable fixed salt, will form vitriolated tartar; which being of difficult solution, much of it will crystallize and fall to the bottom; while the natron, robbed of the vitriolic acid, remains dissolved together with a small portion of the vitriolated tartar. 2. Gr. 166 of quadrangular nitre were dissolved by heat, in a solution containing gr. 138 of pearl-ashes. On cooling there shot some crystals of common nitre, the nitrous acid having left the fossil alkali, which is the base of quadrangular nitre, to join itself with the pearl-ashes. 3. Gr. 500 of sal-gem, which seemed quite free from sal catharticus amarus, were dissolved by heat in a solution of gr. 654 of pearl-ashes. There shot a considerable quantity of sal sylvii mixed with fossil alkali, which had been expelled by the pearl-ashes from the marine acid. These experiments were made and communicated to Dr. W. H. by the Hon. Henry Cavendish.

Besides the properties which have been mentioned, the natives of the Canary islands have found out, that they can make matches by dipping paper or tow in a strong solution of natron, which will then burn, except that they do not sparkle, almost as well as if they had been dipped in a solution of nitre, though on trial no nitre appears to be mixed with it. The salt of barilla and kelp he finds by experience, to have this property, but in a less degree, which may be owing to their not being perfectly free from other salts.

* See Dr. Black's experiments in the Edinburgh Essays, vol. 2, p. 218.—Orig.

It may be doubted whether the mineral alkali be not generated on the peak, where it is found, by the fitness of that sort of earth to attract out of the air some of the principles of which it is made: for there is often seen on walls a saline efflorescence, which proves to be this very salt: and some earth, as that at the bottom of a lake in Egypt, is said to produce it, so as to make a constant supply of a great quantity, which is every year dug up and carried away. The natron must be in great abundance in the air or earth, as it is the base of that salt which is the commonest of all, in almost every part of the world; but though it be every where found when united to the acid of sea-salt, yet there are but very few places where we have been able to procure it by itself.

IX. Short and Easy Methods for finding, 1. *The Quantity of Time contained in any given Number of Mean Lunations;* 2. *The Number of Mean Lunations contained in any given Quantity of Time;* 3. *The Number of Troy Pounds contained in any given Number of Avoirdupois Pounds, and vice versâ;* 4. *The Quantity and Weight of Water contained in a full Pipe of any given Height, and Diameter of Bore; and consequently, to find what Degree of Power would be required to work a common Pump, or any other Hydraulic Engine, when the Diameter of the Pump bore, and the Height to which the Water is to be raised in it, are given. By Mr. James Ferguson, F.R.S.* p. 61.

These methods may all be found reprinted and arranged, among many other useful rules, in the author's volume of Tables and Tracts, published in 1767.

X. A Recommendation of Hadley's Quadrant for Surveying, especially the Surveying of Harbours; with a particular Aplication of it in some Cases of Pilotage. By the Rev. John Mitchell, B. D., F. R. S. p. 70.

The use of Hadley's quadrant, as an instrument to take altitudes at sea, is already so well established, that it wants no further recommendation. But there are several other purposes to which it may be applied, with great advantage, which, though obvious enough, seem yet to be hardly sufficiently attended to. There is no instrument so well adapted to many kinds of surveying, either for exactness or conveniency, and particularly the last; but the surveying of harbours, or such sands as lie within sight of land, may often be performed by it, not only with vastly more ease, but also with a much greater degree of precision, than can be hoped for by any other means, as it is the only instrument in use, in which neither the exactness of the observations, nor the ease with which they may be taken, are sensibly affected by the motion of a vessel: and hence a single observer in a boat, may generally determine the situation of any place he pleases, with a sufficient degree of accuracy, if, with this instrument, he takes the angles subtended by 2 or 3 pairs of objects, properly chosen on the shores round about him; but it will be still better to have two observers, one of whom being in a boat must, at the time he takes the angle subtended by some 2 objects on the shore, make a signal to the other observer, who, being placed at one of the objects as a station, must at the same time observe the angle subtended by the boat and the other object. By this means, 2 angles in a plain triangle being given, with the distance between the 2 objects, as a base, the whole triangle, and the situation of every part of it, will be given likewise. By such observations as these, provided the boat be at rest during the time of making them, and they be made carefully with good quadrants, though without the assistance of telescopic sights, the situation of places may be easily determined to 20 or 30 feet on every 3 or 4 miles.

Besides the use of Hadley's quadrant in surveying, it may on some occasions be very advantageously employed in piloting ships into harbours, the great readiness with which it may be used, making it a very convenient instrument for this purpose: but that this may be done to the greatest advantage, it will be necessary to have a proper provision made for it on the charts, by expressing on them the angles subtended by given objects, by means of which, with the bearings, a ship may be enabled to know her situation with great exactness. The well known property of the circle, that angles in the same segment are equal to each other, may be often very conveniently applied on this occasion: for if, through any 2 given objects, we describe several segments of circles, in which those objects shall subtend the angles of* 120°, 90°, 80°, 70°, &c. respectively, we shall then know immediately, on finding the 2 objects subtend any one of these angles, that we are situated somewhere in the circumference of the corresponding segment; and the bearing also from one of the objects being known, our precise situation will be determined with great accuracy.

Mr. M. makes an application of this method, to the case of the mouth of the Humber, where the Spurn Lights are placed, to direct ships in entering the harbour. The present inconvenient and indeed dangerous situation of the two lights at the mouth of the Humber, says he, commonly called the Spurn Lights, must probably soon make it necessary to remove them; for the ground, on which they formerly stood, is now so far washed away, as not to leave sufficient room to erect them at a proper distance from each other; and fresh ground being grown up to the southward, so as to make the point above a mile distant from them, ships are frequently thereby liable to be deceived. In case therefore these

* The number and frequency of these segments, as well as the magnitude of their respective angles, must be determined, according to the particular circumstances of the occasion, on which they are applied: I have mentioned no greater angle than 120°, as there are few cases in which this will not be sufficient; and indeed it is the greatest that Hadley's quadrant, the only instrument fit for this purpose, will easily admit, according to the present construction of it.—Orig.

lights should, at any time hereafter, be removed nearer to the point, the foregoing principle might be very conveniently and advantageously applied, so as to enable entire strangers to enter the Humber, with the greatest security; even in the darkest nights, provided only they could see the lights; and this is the more material, as in dangerous weather it is often the best and most secure retreat between the Thames and the Tyne; but the difficulty of entering it, for want of proper helps, often obliges ships to keep the sea, not without great difficulty and hazard, when they might lie here in perfect safety.

XI. An Uncommon Anatomical Observation, addressed to the R. S. By John Baptist Paitoni, Physician at Venice. Translated from the Italian. p. 79.

The subject of this observation was a woman of about 25, of a swarthy complexion, who had from her infancy been subject to a troublesome convulsive cough, and shortness of breath on any extraordinary exercise; yet she was to all outward appearances of a hale and strong constitution, having a regular and plentiful discharge of her menstrua, which used to relieve her a few days from her disorder. At the above age, after having been singing and dancing with her friends in the carnival time, being taken suddenly with a cough more violent than usual, and a shortness of breath, she dropped into the arms of one of her acquaintance, and vomiting at the same time a little serous and frothy liquor, died immediately.

The body being opened the next morning in the presence of some of the college of physicians, the viscera of the lower cavity were first examined, and found quite sound; the stomach only was a little distended, owing to what the deceased had eaten a few moments before her death; but the cause of her death was found in the thorax, the right lobe of her lungs being wanting. The external membrane, which ought to cover the right lobe of the lungs, was of a livid colour, and adhered to the pleura. This membrane being cut through, instead of the true lobe, there was found in its place a membranous bag of a milky colour, without any visible outlet, and much of the same size as that part of the lungs which was wanting. This bag being opened, there came out into the thorax a serous fluid, void of smell, which being spread on the table, was found, as to figure, colour, and substance, much to resemble a cuttle fish.

It being evident from this, that there was in the thorax a receptacle of serosity contiguous to the sound lobe, it will not appear strange that, from the violent agitation caused by the singing and dancing, the bag burst, and that the sound lobe, which alone used to perform the office of breathing, being hindered, by the serous matter which came out, from exerting its influence, a suffocation ensued. The causes of her habitual disorder are equally obvious. In a perfect state of health there should be two lobes, which together receive the blood from the right side of the heart, and transmit it to the left, reducing it in this journey, by their joint action, to a state of perfection. One of the lobes therefore being wanting, the other must have endured considerable pain in receiving the whole quantity of blood from the heart, and whenever violent exercise made a quicker circulation necessary, the blood must have been stopped, and hence arose the shortness of breath. Nor is it less clear how the cough came to be so frequently troublesome, the motion of the fluids being disturbed, and the delicate fibres of the trachea perpetually irritated by this defect. And this is the true reason why her menstrua, being very plentiful, were for some time very beneficial to her, by causing a great diminution of blood, and leaving a quantity of fluids in the veins more proportioned to the canals of a single lobe.

These things, from the dissection of the body, are evident enough, but it still must seem wonderful to have found a young woman without one of the lobes, that bag of serous matter containing nothing in it which could deserve the name of even the most imperfect one. Dr. P. knew very well that the lungs are subject to many defects; and that there are often found in them tumours, callousnesses, adhesions, stones, ulcers, worms, tubercles, wastings, and the like; but none of these causes could hardly have made the same viscus lose every sign of its former state. The young woman being deficient in this lobe, it is suprising how the other can have performed its office in maintaining life, and have appeared of the common and natural size, and its vessels not at all dilated by the quantity of blood which they received. And as nature has made nothing in vain, we cannot but be astonished to find that single which ought to be double, as if a man was to have one kidney instead of two; much more wonderful is it that, in so delicate and important an action as respiration is, one lobe only of the usual size and structure should have performed the functions of both. He knew very well that those whose misfortune it has been to have part of their lungs only slightly disordered, must have the other considerably affected; but besides that there may have been occasionally relief given, they cannot for a long time have supported the want of a lobe, without giving visible signs of so great a defect. In this case we have reason to believe that this young woman was born with this monstrous deficiency; what is strange is, that she should have come to her full growth with it, and have been apparently healthy, except the convulsive coughs and shortness of breath. These are phenomena, which might be accounted for from obstructions in the lower cavity; at least, the brown yellowish tinge of the face, is a common symptom in these obstructions; but in this case it appears to have been owing to the want of one of the pulmonary lobes, the other not being alone sufficient to work the blood, and give it the necessary redness.

XII. An Account of a New Improvement of the Portable Barometer. By Edward Spry, M.D. of Totness, Devon. p. 83.

This barometer, Dr. S. presumes, will answer every intention of the usual, and more complex portable one, and in a much more simple and durable manner, viz. The double round at the bottom makes it difficult to cause an ascent of air, or a fall of mercury into the bowl; which, if the latter circumstance were to attend it, the quicksilver, from the bowl's construction, must remain in it, thence of no inconvenience. The small bowl at the top, with beads in it, render it far less liable to break by the mercury's ascent, the bowl giving it an immediate expanse from the colon, and the beads counteracting its force as so many springs, which has such an effect, that from many experiments it was found no easy matter to break it by the mercury's ascent, which is very easy in the common one. It is so well evacuated by boiling the quicksilver in the tube, that he depends on its being luminous after being carried so far. The tube may be as large as you please; but, if so, it should not be continued further than the tube's curve, which should have its colon small, by the tube's being drawn so, or, what is better, one of the smallest bore being joined to it. This barometer may be conveniently carried, inverted, in a walking stick, with a scale contained in a large tube covering the other.

XIII. Of a Locked Jaw. By Mr. Woollcombe, Surg. at Plymouth Dock. p. 85.

On Saturday June 2, in the afternoon, Mr. W. was sent for to a poor woman, who an hour or 2 before had been taken with an oppression at her breast, attended with a slight pain in her side, and at the same time complained of a soreness in her jaws, and a little difficulty in swallowing; as he then took it to be only a common cold, she had 14 oz. of blood drawn off, and some nitrous medicines sent her. On visiting her the next morning, he found her relieved as to her breast and side, but her jaw was fixed, and almost closed, with a very great difficulty of swallowing. On a further inquiry, and short reflections, he was soon convinced she had that terrible symptom, a locked jaw. As this disorder is more frequently the consequence of external injuries than from internal causes, he inquired whether she had any kind of wound or cut; and was told that about 8 days before a rusty nail had run into the bottom of her foot; and though the wound was painful for 2 or 3 days, yet it was cured by their own applications, and had been well 4 days before she was taken with the above complaints. He therefore examined the foot, and found it quite whole, though on pressing the tendons of the foot she expressed a little uneasiness. He now endeavoured to relieve this terrible malady; as the blood drawn the preceding day was of a firm texture, and her pulse full and tense, he took away 14 oz. which proved sizy; and having procured some stools, gave her an anodyne of 40 drops of T. Thebaica, in a very small vehicle, which she swallowed with great difficulty. He then applied a large blister to her back, but without any relief. Soon after she was seized with frequent convulsions, which for the time deprived her of her senses; and though in the intervals they were quite perfect, and her jaw not quite so shut, but a little might be put into her mouth by a tea-spoon; yet so great were the spasms, that she never after could swallow any thing; and in this manner she continued, with short remission of the spasms till 2 o'clock the next day, Monday the 4th, when death put an end to her misery. He was afterwards told, that an hour before she died, she could open her jaw, at which she seemed to be greatly rejoiced; but it was of a short duration, the convulsions again returning, and a universal one carried her off.

That a locked jaw should often be the consequence of an external wound, is nothing new; several cases having happened that put it beyond doubt; but that symptoms should come on after a slight contused wound that had been cured for 4 or 5 days, and make such a rapid progress, as to carry off the patient, in little more than 48 hours after the first appearance of the symptoms, is very remarkable. We are certainly much in the dark in regard to the nervous system; but Mr. W. thinks it a strong presumption, that from the first impression of the nail, the nerves were so peculiarly affected, that though the irritation was not sufficient to hinder the external wound from healing, yet it might be sufficient to dispose them to suffer those violent agitations, which ended so fatally.

XIV. Of a Beautiful Chinese Pheasant; the Feathers and Drawing of which were sent from Canton to John Fothergill, M. D., F. R. S. By Mr. George Edwards, F.R.S. p. 88.

The Argus is a species of the pheasant, the largest of that genus yet known, being equal in size to a full grown turkey-cock, from one of the most northern provinces of China. It is probably a male bird, from the beautiful red skin on the forepart of the head, and its fine blue changeable crest and neck; the females of all the different species of pheasants yet discovered having little or no gaudy colours about their heads.

The beak is made like that of our pheasant, of a yellowish-white colour: the forepart of the head, and the beginning of the throat, is covered with a fine scarlet skin, seemingly void of feathers, but is rough with a kind of grain. The irides of the eyes are orange-coloured, more yellow next the pupil, and redder in their outer circumference; the skin round the eye is dusky, or black: it has also blackish marks proceeding from the corners of the mouth; the top and hinder part of the head and neck are of a fine blue changeable colour: it has a crest of long loose feathers, which it can probably raise or lower at pleasure: the lower part of the neck, the back, and covert feathers of the wings, are covered with black or dusky feathers, having a small broken transverse mixture of reddish brown; the wings, when closed, measured about 17 inches, though the prime quills fall short of the length of those above them: the wing has about 20 quills, the outermost shortest, which gradually lengthens to the fifth; the 9 outermost quills are of a lightish yellow brown, spotted with dusky spots, of the size of tares, except on their inner webs, next the shafts, where they are of a dusky brown, with white spots as small as mustard seeds; the shafts of these feathers are of a lead colour; the 11 remaining quills, which characterise this bird, are of a darker brown than the foregoing, marked with round and longish dusky spots on both the inner and outer webs. What is most extraordinary in these feathers is, that each of them has on the outer web, close adjoining to the shaft, a row of very distinct spots like eyes, so shaded as to appear imbossed: they are larger and smaller as the feathers are to the outer quills; they are from 12 to 15 on each feather; the largest eyes are an inch in diameter, they are incircled first with black, and without that with light brown, their shafts are white; the eyes in the two or three innermost quills are not so regularly marked, they lose their roundness, and become confused; these beautiful eyes are not seen, unless the wings are a little spread; the inner coverts of the wings are brown with black spots; the under sides of the quills are marked like the upper, but fainter coloured, the inner webs edged with light ash colour, which forms a whitish bed within side of the wing. The throat, breast, rump, and covert feathers, on the upper side of the tail, are of a dull orange colour, with round dusky spots; the tail has 14 feathers, of very unequal lengths, the middlemost being each of them 3 feet long, the next, on each side, 18 inches, which gradually shorten to the outermost on each side, which are each 12 inches, their colour is dusky, with a tincture of bright brown, the outer feathers are dotted with white as small as mustard seeds; the next within these have larger spots, less regularly formed; the two long middle feathers have round white spots, surrounded with black, on their outer webs, and larger irregular brown spots, surrounded with dusky on their inner webs, which are ash coloured; the lower belly, and covert feathers beneath the tail, are dusky with a confused mixture of brown; the legs and feet are like those of turkeys, with 3 toes forward and one backward; the legs, feet, and claws, are of a greenish ash colour.

XV. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1764, pursuant to the Direction of Sir Hans Sloane, Bart. By John Wilmer, M. D. p. 91.

This is the 43d presentation of this kind, completing to the number of 2150 different plants.

XVI. A Course of Experiments to ascertain the Specific Buoyancy of Cork in different Waters: also, The respective Weights and Buoyancy of Salt Water and Fresh Water: and for determining the exact Weight of Human and other Bodies in Fluids. By John Wilkinson, M. D., F. R. S. p. 95.

Exper. 1. Six cubes of good cork, each measuring an inch square, as nearly as they could possibly be ascertained, with a graduated gauge of cork fastened to a point of each, severally weighed as in the statement annexed; the medium weight being the exact weight of a cubic inch of cork, is nearly $46\frac{4}{7}$.

N° 1	49	Grains
2	$47\frac{1}{2}$	
3	47	
4	$46\frac{1}{2}$	
5	46	
6	$43\frac{1}{2}$	
Sum	$279\frac{1}{2}$	

Exper. 2. N° 1, 2, 3, 4, formed a float named A, being 190 grains of cork, which was fastened to a medallion of lead, weighing 2 ounces, by a wire that weighed 8 grains; these being put into a deep jar of Thames water, taken above the new bridge, the lead weighed exactly 44 grains; so that 190 grains of cork supported precisely 916 grains of lead in river water.

Exper. 3. The float was left in the same water, immersed by the medallion for the space of 48 hours, with a view of obtaining a precise knowledge of the quantity of water the cork might imbibe; also how its buoyancy might be affected; and how much extension it might acquire from a certain continuance under the water. It was somewhat surprising to find, that after this period of immersion the cork had not, as might have been expected, lost any force of buoyancy; but on the contrary, it had actually gained 2 grains, and was found to support 918 grains of lead: which must be attributed, it may be presumed, to an expansion of the air contained in the cork, acquired from its immersion in the fresh water.

To be further informed how cork would be affected by a continued immersion, the same float, after weighing, was again immersed in the same water, for the space of 48 hours longer. And now the water was found to have penetrated its recesses and cavities, insomuch that its buoyancy had decreased 11 grains, and it then supported only 905 grains. This decrease may possibly be attributed to a greater imbibition in the cork, and a greater penetration given to the water, by its having been now 96 hours in a chamber, with a fire in it, by which it might be warmed. This float A being taken out of the water, wiped, and laid in a dry place for 24 hours, weighed 211 grains, having gained by absorption 21 grains of additional weight.

Exper. 4. The float, in this state, having been 24 hours out of the fresh water, was now put into salt water brought from the North Foreland, in which it was found to support the weight of 954 grains, of a leaden medallion, having the power of buoying up 38 grains more in the sea water, than it did in the river water, with which it was nearly saturated at the time of its immersion in the sea water.—After an immersion of 48 hours in the salt water, this float (A) lost 16 grains of its buoyant power; and having been 72 hours in the salt water, on weighing it again out of the water, being wiped, but full of water, it was found to weigh 241 grains, having gained 30 grains. After being 9 hours out of the water, in a dry room, it weighed 213 grains, having lost 28 grains. This float (A) having been 76 hours in the salt water, in which it then sustained 949 grains of lead, was removed, thus saturated with the sea water, into a vessel of fresh river water, and was found to sustain 923 grains only, being 26 grains less than it bore up in the salt water; and yet it must be observed, that this float had been saturated with fresh water before it was immersed in the salt water.

Exper. 5. Not content merely to know the buoyant power of cork in fresh water, it was next tried in sea water. Five cubes, being each an inch square on every side, made of the best sound compact cork, with distinct gauges made of cork, and adjoining, as in the preceding experiments, were prepared, each weighing as annexed; these being united, formed one float (marked B) that they might the nearer resemble the texture of the cork jacket. A vessel of sea water taken up near the North Foreland being then provided, the float was found to support in it a leaden medallion weighing 1048 grains. When it had continued in the sea water for 48 hours, it exactly supported 1024 grains, having, by being in the salt water, for that space of time lost 24 grains of its buoyant power.

N° 1	42	Grains
2	42½	
3	54	
4	53	
5	43	
Sum	234½	

From this experiment we learn, that the salt water insinuates more into the substance of cork than fresh water does, which may perhaps be attributed to its oiliness, oil being more subtil and penetrating, than water; it could not therefore be expected that the cork should retain its force so vigorously, for so long a time as in the fresh river water; and, on weighing, it was found to lose $\frac{1}{39}$ of its buoyancy in 48 hours. But then we are further to observe that the sea water supports a greater weight, and gives more buoyancy to the cork float than fresh water, in the proportion of 353 to 359; for a float weighing 44 grains, supporting 353 grains of lead in fresh water, buoyed up 359 grains of lead in the sea water, being more by 6 grains. And it is to be noted, that the sea water taken up at the North Foreland is not so much impregnated with the marine principles, as that which is taken up at a greater distance from fresh water rivers, which decreases in its buoyancy in proportion to its vicinity to or mixture with rivers; for on evaporating, it yielded only 2 drachms and 1 grain of salt to a pint, which is less by 7 grains than is yielded by that taken up at the Isle of Wight.

Exper. 6. The float B being continued in immersion in salt water for 72 hours, it had lost 26 grains more of its buoyant force, losing in the first 48 hours 24, and in the 24 succeeding hours 2 grains only: from which it appears, that after the cork has been in the water for 48 hours, its absorption becomes less than before, in proportion as 12 is to 1. This float having continued in the water for 78 hours, each piece of cork, being wiped very dry on its surface, but not squeezed, weighed as follows:

N° 1 weighed	55½	Grains having gained	12 grains
2	47		4½
3	66		12
4	59½		5½
5	51½		8½
All 5 weigh.	279½	gained	42½

Being then desirous to know if any quantity of salt had insinuated into the pores of the cork, and if so, how much; the above float was hung up to dry in a warm airy room, where it was not far from the fire, for the space of 48 hours: when it seemed to be quite dry, and each piece being weighed, was found as follows;

N° 1 weighed	45 grains,	the salt supposed to be contained was	3 grains.
2	46		3½
3	59		6
4	55		2
5	44		1
All 5 weigh.	249	salt gained	15½

But on their being hung near a fire for 72 hours, until perfectly dry, no salt appeared to be imbibed, for the above numbers weighed as follows; having lost by extraordinary dryness about $\frac{1}{20}$ of their first weight.

N° 1 weighed	40 grains,	having lost	2 grains
2	42		4
3	54		3
4	53		3
5	43		3
	232		15

Exper. 7. For the sake of greater exactness, another float (marked c) was made of two cubes of cork, amounting to 164 grains; the larger 118, and the smaller 46 grains; this was put into the same vessel of river water, after it had been 96 hours in a warm chamber, and supported a medallion of lead weighing 766 grains; it was left in the water for 36 hours longer, and weighed again, when it was found to support 754 grains, having lost 12 grains of its buoyant force in that time of immersion. From this 2d experiment does it not appear, that this loss of buoyancy was principally owing to the warmth the water had got by standing in a warm room, as was hinted at before, which was further proved by the thermometer?

Exper. 8. A medal of lead, weighing one drachm when out of the water, was found, on being weighed in river water, to be 54 grains and a half; being less by 5 grains and a half than out of the water. The same medal being weighed in the salt water was found to weigh 53 grains and a half. The difference between its hydrostatical weights being precisely one grain, or $\frac{1}{54}$ more in river water than in sea water.

Exper. 9. A phial of river water weighed exactly 82 grains; the same vessel of sea water weighed exactly 84; the difference was two grains, or as 41 to 42.

Exper. 10. After having made the foregoing experiments, with a view of ascertaining the specific buoyancy of cork, and also the alteration it might undergo from being immersed in river or sea water, we proceeded to the more important examen, to discover the precise quantity of cork necessary to sustain a man in the water. For this end, 22 pieces of the best cork, such as is directed to be used for the jackets, were provided; these were of different weights, from one ounce down to one scruple; so that by means of a string to which they were occasionally fastened, one might easily learn what weight they would suspend in the water, and what weight they would not suspend. This being done, the next business was to find a proper person to make the experiment on; for the accuracy of which it was judged proper to select a man of the smaller size (as our seamen are seldom large) that was not very fat, because fat people are more buoyant than lean or bony persons; one that could swim, that he might go through the experiment with propriety, and without fear; one therefore was fixed on who was plump and muscular, not very bony, but moderately so; his height was 5 feet 2 inches; his waist measured 2 feet 10 inches; his weight was 104 pounds. Thus fitted out, he stripped naked, and 10 ounces of cork being tied about his neck and breast, he committed himself to the river Thames, near Chelsea, in a place where the water was about a foot below his depth; but he could not keep his head above the surface without employing his art, though he found not much art required to do it: another ounce of cork was therefore added, and he perceived himself raised, so as now to be more able to keep himself above the water, but yet he went slowly to the bottom, unless he took care by swimming to prevent it. For this reason another ounce of cork was applied, but still he sunk, though more slowly than before; and it was plain to be observed, that the point between that manner of sinking and swimming was balanced with such extreme nicety, that the smallest addition of cork would determine in favour of buoyancy: accordingly half an ounce was added, and this weight supported him in a vibrating state; but by the superaddition of one drachm and two scruples more of cork, he found himself very able to keep above water, in a living position, without any help from his art of swimming, and that it required some small pains to immerse himself. It therefore results from this experiment, that 12 ounces, 5 drachms, and 2 scruples, or 6100 grains of cork, supported this man in river water; 6100 grains of cork are equal to $163\frac{30}{37}$ cubic inches; which appears, by a calculation drawn from the 2d of the foregoing experiments, to be able to support 63 ounces, 5 drachms, and 8 grains of lead, which must be therefore the exact weight of this man in river water. By comparing this calculation with the above experiment 4, made in the sea water, we shall find that this man weighed 60 ounces, 3 drachms, and 21 grains; or 4 pounds, 12 ounces, 3 drachms, and 21 grains, requiring 12 ounces and 21 grains of cork to support him in sea water. It must however be observed, as already intimated, that the same quantity of cork which supports a fat, or very plump person, in the water, will not suffice to buoy up a lean person, though their weights out of the water be equal.

XVII. Of the Disease called Ergot, in French, from its supposed Cause, viz. Vitiated Rye. In a Letter from Dr. Tissot, of Lausanne, to G. Baker,† M. D., F. R. S.* p. 106.

In 1762, Dr. Wollaston having presented to the R. S. the history of an extraordinary case of a mortification of limbs in a poor family at Wattisham, about 16 miles from Bury, in Suffolk, where Dr. Wollaston at that time resided; and about the same time the Rev. Mr. Bones, then minister of the parish of Wattisham (whose humanity led him to show a particular attention to the sufferings of that unhappy family) having transmitted to Dr. Baker, every circumstance which he could observe or collect, relating to the disease; these letters were communicated to the R. S., and both accounts were published in the 2d part of the 52d vol. of the Phil. Trans. [Abridgment, vol. 11, page 626, 628, 646.]

Some time in the winter of 1763, as Dr. Baker was perusing a work, intitled, Avis au Peuple sur sa Santé par M. Tissot, (printed at Paris in 1762) he observed a disease mentioned under the appellation of Ergot, a name borrowed from its supposed cause, viz. vitiated rye. As the phenomena of the disease described (which is said to be frequently epidemic in several provinces of France) are very similar to those of the disease, of which an account was received from Wattisham, Dr. B. wrote a letter to M. Tissot, and requested him to commu-

* Dr. Tissot practised for many years with great celebrity at Lausanne. He died in 1797. Among the physicians on the continent he was one of the earliest and warmest advocates for the inoculation of the small-pox. His principal works are his Avis au Peuple, his Maladies des Gens du Monde et des Gens des Lettres, and his Traité de l'Epilepsie.

† Now Sir George Baker, Bart.

nicate to him whatever had fallen under his own observation, with respect to the disease called Ergot.

In answer to this letter, Dr. B. was favoured with the following collection of curious facts and observations on the subject of his inquiry: which, at his request, he sent to the R. S.

After thanking Dr. B. for the present of his treatise de Catarrho et Dysenteria, Dr. Tissot states that the observations on spurred rye (secale cornutum) inserted in the first edition of his Avis au Peuple sur sa Santé, were not his own, but those of an anonymous Parisian author, and that he had omitted them in the 2d edition of that work; but in compliance with Dr. B.'s request, he had sent him all the information he had formerly collected on this subject, at a time when his inquiries were particularly directed to Dietetics. The result of these inquiries Dr. B. was at liberty to lay before the R. S.

Dr. T. then states that there are 3 principal diseases of wheat and rye, viz. the rubigo, ustilago, and secale cornutum.

The rubigo (mildew in English, rouille in French, ruggine in Italian) is a reddish-yellow, glutinous powder which adheres to the stem and glumes of many graminaceous plants, preventing their growth and depriving the grain of its nourishment, whence it wastes away and becomes shrivelled, and yields little or no farina.*

The ustilago (nielle or brulure in French, fuligine in Italian) is a generic name, denoting a black degeneration of wheat and other bread-corn, of which there are 2 species, the carbunculus (charbon) and caries (la carie.)†

The charbon is a disease of corn which is scarcely to be distinguished by its outward appearance, except that the grain appears rounder, where the internal substance is converted into a black, viscid, fœtid powder. Such grain is sometimes much swelled; hence Mons. Duhamel has called this disease la bosse. The nature of the charbon, according to the observations of Mons. Bonnet, is best seen in the grain of Indian corn (maize).(a) He had seen some of these

* This disease, the mildew, blight, or rust, according to Sir Jos. Banks, is occasioned by a minute parasitic fungus which attaches itself to the stems, leaves and glumes. At first it is of an orange colour, and afterwards changes to a deep chocolate. See an Account of the Cause of the Disease in Corn called by Farmers the Blight, the Mildew, and the Rust, by Sir Jos. Banks, Bart. P.R.S. illustrated by engravings, showing the blight as it appears when viewed through glasses of very high magnifying powers.

† The carbunculus or charbon corresponds with what is called by the English farmers rotten wheat; the caries, as here described, with the smut, which others term ustilago.

(a) Recherches sur l'Usages des Feuilles dans les Plantes, p. 327. Dr. Tissot's description is not sufficiently explicit. Mons. Bonnet's words are "Ils (les grains) étoient composés interieurement de plusieurs feuillets posés les uns sur les autres, et qui laissoient entreux des vuides. Ces vuides étoient remplis par une poussiére noirâtre, &c."

grains as large as a hen's egg, (quæ ovum gallinaceum æquabant) full of a black, fœtid, sanious powder, interposed between various lamellæ.

The caries (which by the French is commonly called nielle) infects wheat, barley, rye, and other plants, attacking not only the grain or seeds, but also the flowers and leaves, under the form of a black, viscid powder, completely enveloping the grain or flowers, and destroying whatsoever it touches. This disease seems to attack the corn at the time when it is in flower, thereby preventing the grain from ever coming to maturity. Dr. T. had by him several ears of corn infected with the caries; they were completely covered with this black powder, and were destitute of seed or grain, exhibiting nothing but whitish glumes, with a fibrous substance in the middle of the glume, which appeared to have been the fibrous part of the grain. This black powder had very little smell or taste.

According to Ginanni, the caries has been known from time immemorial; but the charbon is a disease of modern times, and was never observed in Lombardy before the year 1730, nor at Cesena before the year 1738.

The secale cornutum (ergot) is a very different disease from the preceding. It attacks rye, and 2 or 3 other graminaceous Alpine plants, according to the observations of Haller. It is an irregular vegetation of the rye-grain, which puts on a middle nature between the grain and the leaf, becoming of a brownish-green colour, irregularly compressed, and, according to Marchand and Vaillant, frequently 14 or 15 lines long, and 2 lines broad. Langius, in particular, has given a very accurate description of such vitiated rye, together with some experiments concerning its nature. He found that, when put into the ground, such seed never germinated; that it abounded most in rainy years, and when a very hot summer followed a wet spring. (b)

Here Dr. T. takes occasion to remark, after Ginanni (c) that many mistakes have occurred among authors, owing to their confounding the rubigo with the ustilago, and the ustilago with the rubigo. What Ramazzini has described as the rubigo in the epidemic constitution of 1692, he believes to have been the true ustilago.

A confusion of less moment has occurred in the names given to the secale cornutum (spurred rye); which by some is called secale luxurians, by others mater secalis, (the muttercorn of the Germans) by others orga, and by Lan-

(b) Langius's treatise was published in the German tongue, at Lucerne, in 1717. The import of the title is, An Account of the Disorders occasioned by eating spurred Rye (ex esu clavorum secalinorum) in Bread. A good abstract of this work may be seen in the Acta Eruditor. for the year 1718.

(c) Count Ginanni, author of a learned treatise entitled Delle Malattie del Grano in Herba 4to. Pesaro. 1759.

gius clavus secalinus; but notwithstanding this diversity of names, it cannot easily be confounded with other diseases. Perhaps Moneta is the only writer who has introduced an error in this respect, affirming that the secale cornutum is nothing more than the rye-grain increased to a larger bulk than natural (gigantea grana) from a luxuriancy of growth in fruitful years; that such rye-grain is not unwholesome; and that, contrary to the observation of all other writers, barley and wheat are also liable to the ergot; (d) but Dr. T. is persuaded that Moneta never saw the secale cornutum, but merely some over-grown rye-grain, samples of which are every year to be met with. The lady who wrote the letter published by Salerne, (e) mentions that the spurred rye is sometimes larger, sometimes smaller than the common rye-grain; and Mr. Hanovius (f) seems inclined to believe that the secale cornutum is a disease of the nature of a marasmus; but these opinions are contrary to those of all other writers on this subject.

As often, therefore, as there is any mention of the rubigo, ustilago, or secale cornutum, it should be carefully remembered, that the rubigo, ærugo, ruggine, robbiga, mildew, suc miellè and perhaps bled vanté, are one and the same disease; that the ustilago, uredo, fuligo, nigella, volpe, nielle, brulure, is another and a very different disease, of which there are 2 species, viz. the caries (la carie) and the carbunculus (le charbon;) and that the secale cornutum (called also secale luxurians, mater secalis, mutterkorn, orga, and clavus secalinus,) is a third kind of disease, totally distinct from those before mentioned.

Dr. T. then states that the ancients were not ignorant of the mischief occasioned by the use of vitiated grain for food, and that Galen has delivered some excellent precepts on the subject. (g)

Bread made of wheat or other grain infected with the nigella or charbon, is viscid, heavy, and nauseous, to those who are unaccustomed to it. And in 1758, when this sort of bread was very common, it appeared to be the cause of many chronic diseases of the skin and bowels. Langolius mentions that a person, out of curiosity, swallowed some grains of wheat infected with the ustilago, and was afterwards seized with pains of the limbs, which, however, went off after a few evacuations by stool.

But the mischief which follows the use of the secale cornutum in food, is of the most serious kind; and as it is probable that this vitiation of rye has existed from time immemorial, Dr. T. is induced to believe that mankind have in all ages suffered from the disorders which it occasions, although none of the an-

(d) Comment. de Reb. in Hist. Nat. et Med. gestis Tom. III. p. 520.

(e) Memoire sur les Maladies que cause la Seigle Ergotté in the Memoires de Mathematique et de Physique presentés à l'Academie Royale des Sciences, Tom. II. p. 161.

(f) Comment. de Reb. in Hist. &c. as before quoted.

(g) De Alimentor. Facultatibus, l. i. c. 37.

cient physicians have described those disorders. The first accurate account of them was published in 1596. F. Hoffman (h) notices both forms of the disease. Dr. T. describes first the spasmodic, and afterwards the gangrænous form.

In 1596 a disease, accompanied with spasms and convulsions, was epidemic in Hesse and the neighbouring district. It was attributed to the use of spurred rye by the medical faculty of Marpug, who in 1597 published a tract on this subject in the German language, describing the symptoms, causes, and method of cure. From this tract Sennertus (i) seems to have derived his information on this subject. As the works of this author are so well known, Dr. T. contents himself with extracting from his ample account of this disorder, the following particulars:

1. Those who were seized with epilepsy scarcely ever recovered.

2. Those who were disordered in their intellects, remained so until the time of their death.

3. Although some lived 15 years after being attacked with this disorder; yet every year, in the months of January and February, they found themselves ill.

4. This disorder was not free from contagion; a circumstance (Dr. T. remarks,) which is not noticed by any other writer.

According to Hoffman this disorder was very prevalent in Voigtland in 1648, 1649, and 1675. In 1702 it spread through the whole country of Friburg. In 1716 it was epidemic in Saxony and Lusatia, and has been described by G. V. Vedelius. (k) At the same time Goelike (l) published a treatise on this disorder, referring to every author who had then written upon it, and showing the variation in its symptoms, as it prevailed epidemically in different places.

The same disorder afflicted various parts of Germany in 1717. It appeared in Silesia in 1722. Vater (m) gave an account of it. It appeared at Saboth in Silesia, and at Wartenburg in Bohemia in 1736. The epidemic of Saboth is described by G. H. Burghart; (n) that of Wartenburg by J. A. Scrinc (o) who visited as many as 500 patients. The last mentioned author states that the disorder began with a disagreeable titillation of the feet, as if ants had been creeping up them; this was soon followed by a violent pain of the stomach (cardialgia); the hands were next affected, and then the head. This titillating sen-

(h) Patholog. Generalis, Part ii. cap. ix. sect. 16 in Scholio.

(i) De Febribus lib. iv. cap. xiv. De Febre Maligna cum Spasmo.

(k) Dissertatio de Morbo Spasmodico Epidemico Maligno in Saxonia, Lusatia, vicinisque Locis grassato. Ienæ 1717.

(l) A. O. Gœlike Horæ Subscesivæ Tom. II.

(m) Chr. Vater Dissertatio de Morbo Spasmodico Populari Silesiaco. Witemberga 1723.

(n) Satyræ Medicor. Silesiacor. Specim. iii. Obs. 4.

(o) Satyræ Medicor. Siles. Specim. iv. Obs. 5.

sation was succeeded by a violent contraction not only of the hands and feet, but also of the fingers and toes. The patients exclaimed that their hands and feet were on fire, while their bodies were bedewed with copious sweats. After much pain, the head became heavy and vertigo came on, with dimness of sight. Some either became totally blind or saw objects double. They staggered like drunken people, and lost their recollection. Some became maniacal, others melancholic, others comatose. Such as were above 15 years of age were liable to become epileptic; and to the greater number of these, the disorder proved fatal. The epileptic affection was accompanied with opisthotonos, and the foam from the mouth was either tinged with blood, or was of a yellow or green colour. During the convulsions the tongue was often lacerated, and in some cases it was swoln to such a degree as to interrupt the voice; at the same time there was a copious discharge of saliva from the mouth. If epilepsy came on after cardialgia and vomiting, it was fatal. Those who were seized with chilliness and shivering after the titillation, had their hands and feet less convulsed. In addition to this complication of sufferings, the patients were afflicted with a voracious appetite, which in many instances it was impossible to satisfy; in a very few instances there was a loathing of food. One patient had glandular abscesses in the neck; but they were by no means of a pestilential nature. They discharged a yellow pus, and were excessively inflamed and painful. Another person had petechiæ on the feet, which lasted for 8 weeks. In some instances the face was disfigured with blotches (maculis). The pulse, in every instance, was like the pulse of a person in health. In some the disorder lasted a fortnight; in others a month; in others 6 or 8 weeks; and in some even for as long a period as 12 weeks; but with intermissions. There died of this disorder 100 chiefly infants. Of 500 patients, 300 were infants, reckoning as infants, all who were under 15 years of age. Two entire families were destroyed by this disorder, not a single individual of those 2 houses escaping. The disorder however, was not infectious.

Burghard, after mentioning the convulsions of the extremities and other parts of the body, with loss of intellect, adds that there was seldom any abatement of the disorder before the 3d week (especially in those who had no medical assistance, or who did not observe a proper regimen) and that in many it lasted upwards of 1 or 2 months. Those who had a continued fever, and who sweated profusely recovered soonest. Those to whom the disorder proved fatal, seemed to be seized with palsy and apoplexy a short time before they expired. In women, after intervals of remission, the disorder was aggravated at the period of menstruation; when this was over, they complained of little else for a week or two except debility; but at the next monthly period, a renewal or exacerbation of the disorder took place. Such as recovered were for a long time afflicted with a weakness of the limbs, or a stiffness and immobility of one joint or other, together with a dulness of intellect.

In 1741 this disorder appeared in Neumarck, where it continued until May 1742. It has been well described by Muller.(p) His description accords with that of Scrinc, except that in Neumarck the disorder was constantly attended with fever, from which the sick in Bohemia were exempt.

Dr. T. next gives an account of the other form of disease produced by spurred rye, viz. the spontaneous gangrene.* This disorder was known in some provinces of France as far back as the year 1630, according to the testimony of Dr. Thuilier, physician to the Duke of Sully.(q) In 1650, 1670, and 1674, it raged in some parts of Aquitania, in Sologne, and in the district of Gatinois; and in 1764, in the neighbourhood of Montargis, according to Mons. Perrault.(r)

The first symptom was a numbness of the legs, then a pain with a slight swelling, but no inflammation; and in quick succession, coldness, lividness, mortification, and dropping off of the limb. At Sologne there was no fever, and the pains were slight. No remedies were applied; but the nose, fingers, hands, arm, feet, leg, thighs, sphacelated spontaneously and dropped off.

In 1695, Dr. J. C. Brunn saw at Augsburg, a woman labouring under a spasmodic disorder and a mortification of the hands, in consequence of eating some spurred rye; and he was told by the surgeon who attended her, and who a very short time before had amputated her foot, which had mortified from the same cause, that it was owing to such vitiated bread-corn, that so many of the inhabitants of the Black Forest were afflicted not only with convulsions, but also with mortifications of the extremities.(s)

In 1709, Sologne was again visited by the same disorder, a fourth part of the rye being that year infected with the spur (ergot) Mons. Noel, surgeon to the Hotel Dieu at Orleans, had under his care, in that hospital, above 50 ergotted patients, chiefly men and boys.(t) The disorder commonly began in the toes, and often spread to the upper part of the thigh. The first symptom after eating the poisoned bread, was a sort of intoxication. Four patients died after amputation, the mortification having spread to the trunk of the body; whence Dr. T. infers that it is dangerous to amputate before the mortification

(p) C. A. a Bergen et J. M. F. Mulleri Disputatio de Morb. Epid. Spasm. Convuls. Contagii experte. Francofurti ad Viadr. 1742. Vide Halleri Dissert. Med. Pract.

* Gangræna spontanea. The epithet spontaneous is not well applied to a disease supposed to be excited by the use of vitiated rye in bread.

(q) Lettre de M. Dodart au Journalist des Sçavans ann. 1676. Tom IV. p. 79.

(r) Journ. des Sçavans, ibid.

(s) Act. Curiosor. Natur. Dec. III. Ann. 2. Obs. 224.

(t) Hist. de l'Acad. Royale des Sc. Ann. 1710, p. 80.

has stopped. At Blois the following melancholy case occurred as related by Fontenelle; Un paisan fut attaqué de la maniere la plus cruelle: la gangrene lui fit tomber d'abord tous les doits d'un pied, ensuite ceux de l'autre, aprés cela le reste des deux pieds, et enfin les chairs des deux jambes et celle des deux cuisses se détacherent successivement et ne laisserent que les os. Dans le temps qu'on en ecrivit la relation, les cavités des os des hanches commençoient à se remplir de bonnes chairs qui renaissoient. (u)

The same year (1709) memorable for a hard frost, this disorder appeared in the canton of Lucern, and again in 1715 and 1716; at which time also it was epidemic in the cantons of Zurich and Bern. Langius has given an account of it. (x)

Since that time the disorder had not again occurred in Switzerland, as far as Dr. T. could learn; but from the year 1709 it had been epidemic at Orleans 3 or 4 times, within the space of 30 years, according to Mons. Noel. (y)

It seems to be endemic in that part of France.* Mons. Duhamel has given a description which was communicated to him by Mons. Mulcaille, of a very malignant form of this disorder, which prevailed in Sologne, and which destroyed most of those who were seized with it. The disorder was first felt by a weariness and pain in the feet and legs, of which after they had become livid, there was a mortification, rather dry than moist; worms were often engendered in the mortified parts; the toes separated from their articulations and fell off with the metatarsus; afterwards the foot, the leg, and even the thigh, which last dropped from the cotyloid cavity. It was the same with the upper extremities; and instances had occurred in the hospital of persons living several weeks, after their legs and arms had rotted off, and nothing remained but the bare trunk; for this dropping off of the limbs was never followed by hemorrhage. They had not succeeded in curing any of these patients; upwards of 60 had died. (z)

Mons. Salerne (aa) has described another epidemic of this kind, of which the following were the principal phenomena:

1° It attacked persons of both sexes and all ages. 2° It did not spread above the knee-joint; whereas in the preceding year (probably the same of which Mons. Mulcaille gave an account) a boy 10 years old lost both thighs, and his brother who was 14 years old, lost the leg and thigh of one side, and the leg of the other; they both died after 28 days. 3° Some few recovered from the

(u) Hist. de l'Acad. Royale des Sc. Ann. 1710, p. 80.

(x) Acta Erudilor. Ann. 1718, p. 309. In this place Dr. Tissot introduced an extract from Langius; but as his description of this disorder coincides with the descriptions before given, that extract eemed to be superfluous, and has accordingly been omitted in this Abridgment.

(y) Quesnay Traité de la Gangrene, p. 408.

* Namely, in the district of Orleans, Sologne, &c.

(z) Mem. de l'Ac. R. des Sciences ann. 1748. p. 528.

(aa) Mem. de Mathemat. et Phys. presentès à l'Acad. Roy. des Sciences. Tom. II. p. 155.

sphacelation of the limbs; but they seldom lived long afterwards. 4° Amputation only served to accelerate the patient's destruction. 5° Out of 120 patients, scarcely 4 or 5 recovered; all the rest died within six months. 6° The blood was so viscid that it would hardly flow from a vein. 7° An inflammation of the skin, denoted a suppuration in that part. 8° There was no occasion for the tourniquet or ligature after amputation. 9° In Sologne, which is a marshy country, the disorder commonly attacks the feet. 10° As, from the beginning the intellects, in all instances, are more or less impaired, the patients are incapable of giving any account of their illness; their countenances are yellow, and they become so much emaciated as to resemble corpses. 11° The disorder is by no means contagious.

Mons. Puy, first surgeon to the hospital at Lyons, informed Dr. T. that he had often seen there, and always in rainy years, some patients labouring under this disorder, brought from the neighbourhood. Among those patients was a woman from whom both the lower extremities dropped off. The most distressing symptom to such patients, is the sensation of a burning fire in the affected part. He added that he had heard of some instances of this disorder occurring in Dauphiny.

Dr. T. next quotes various authors (bb, cc) to show that spurred rye is poisonous to quadrupeds, poultry, and other brute animals, as well as to man.

He then puts the following questions:

1° What is the cause of this degeneration of rye? He remarks that this question is involved in the greatest obscurity; but that Mons. Aimen, (dd) who had shown that the caries was owing to the seeds being contaminated by situation (cariem oriri ex seminibus situ fœdatis)* had promised to inquire into the causes of the spurring of rye.

2° In what manner does the spurred rye produce its deleterious effects? In answer to this question, he remarks, that there are many vegetable poisons, whose mode of operation we do not understand, and that the secale cornutum is one of these. It has an acrid, nauseous taste, in common with many other deadly poisons. This vitiated rye seems to infect the humors with a poisonous taint, which either irritates the nerves so as to excite spasms, or corrupts the blood, and thereby produces gangrene.

3° In what way does the nigella prove hurtful? It is an acrid, viscid poison; and if a person walks barefooted in fields covered with the nigella, his feet and legs will be ulcerated. (ee)

(bb, cc) Sat. Med. Siles. & Muller as before quoted.

(dd) Mem. presentés, &c. Tom. III. p. 68.

* i. e. contaminated by mucor (moisissure) in the soil, in which the seeds are planted, as Mons. Aimen represents.

(ee) Langius as before quoted.

How comes it that this poison (the secale cornutum) at one time produces spasms, at another gangrene, sometimes with fever, but generally without? These are questions which are involved in much difficulty, and of which a solution is only to be expected after numberless observations and experiments. Dr. T. remarks that the whole of this subject is highly deserving the attention of physicians, presenting many phenomena, which if well understood, might throw much light on some obscure points in physic.

In regard to the treatment of this disorder; the medical practitioners at Marpurg give purgative medicines at first, and afterwards sudorific bitters. Langolius recommended acids. Langius prescribed an emetic in the beginning and afterwards sudorific bitters, directing his patients to abstain from all sort of food that was viscid, fat, and otherwise difficultly digestible. The use of hot or new bread was especially forbidden, being found much more hurtful than stale bread. The vitiated rye loses its poisonous quality by keeping. Hence this disorder is most prevalent immediately after the harvest, becoming gradually less frequent, till at length it entirely ceases, although there still remains a supply of the secale cornutum.

The only part of Muller's practice (which consisted chiefly of antispasmodics) which Dr. T. approves, was the application of blisters.

In Sologne the pains were relieved by bloodletting. The mortification was sometimes stopped by [using externally] a decoction of vitriol, alum, and common salt.(ff) In the case of a boy, whose leg had mortified, Mons. Puy made a large incision down to the bone, after which he perforated the bone in several places with a trephine; nearly the whole of the bone came away by exfoliation; but the loss was gradually supplied by a callus, and new granulations of flesh taking place, the patient at length got entirely well.

Having himself had no experience in these cases, Dr. T. only suggests that after premising venesection according to circumstances, it might be proper to prescribe an ipecacuanha emetic, and perhaps to repeat the emetic; to purge with the bitter salts; and then to give large doses of elixir of vitriol and Peruvian bark with the decoction of chamomile.* He further suggests the application of large blisters to the neck and os sacrum, and after making large incisions into the affected parts, to foment them constantly with a vinous decoction of Peruvian bark.

Is the term gangræna ustilaginea, which has been applied to this disorder, a proper one? He thinks it is not.

Is this disorder the morbus ardentium? The disease so called (he observes) appears to have been an erysipelas frequently terminating in gangrene. According-

(ff) Memoir. present. Tom. II. p. 162.

* Why not also wine and opium? The curative indications seem to be, first to cleanse the intestinal canal by cathartics, and then to administer cordial and anodyne medicines.

ing to Mons. Puy such was in some instances the form of the disorder in Dauphiny,

Was the mortification which made such havoc in the family of J. Downing, of Wattisham, in Jan. 1762, and which has been described by Mr. Bones,(gg) Dr. Wollaston,(hh) and Dr. Parsons,(ii) the same disease? Dr. T. thinks it was.

But the same cause, viz. spurred rye, did not operate; there was, however, damaged wheat, of which bad bread was made, and which produced a slighter degree of the same disorder in another person. The gangrene which attacked Downing's family, is therefore to be imputed to the wheat, which, according to Dr. Wollaston, was black and corrupted.

But why should this family have been disordered more than others?

1. In Silesia 2 whole families were destroyed, in consequence (it may be inferred) of some predisposition. In Sologne 2 brothers had the disorder more severely than the rest. At Blois only a single individual appears to have been ill of it. Other observations show that some persons are very readily affected with gangrene.(kk) 2. In Silesia the disorder particularly attacked children; the English patients were young persons, and a mother weakened by giving suck. 3. They were all lean and unhealthy, a proof of a vapid blood. 4. It has been observed in other places that the disease was aggravated by a damp and confined air, by pork and bacon, and by a milk diet; all which circumstances concurred in Downing's family. 5. This poor family lived not only upon bad bread, but also upon bad mutton, bad bacon and bad pease; each of which must have contributed its share towards exciting the gangrene. 6. The disorder was not contagious.

Lausanne, June 28th, 1764.

XVIII. *Observations for Settling the Proportion which the Decrease of Heat bears to the Height of Situation. By Thomas Heberden, M.D., F.R.S.* p. 126.

The remarkable transition from heat to cold in all seasons, in proportion as we ascend the mountains here, [Teneriffe,] induced Dr. H. to make the following observations, with intention to discover if there subsists any regularity between the difference of heat and the elevation of situation. For which purpose, besides several observations made at different times, without any remarkable variation, he took the opportunity of a journey of some English gentlemen

(gg) Phil. Trans. Vol. 52. Vol. xi. p. 626 & 628, of these Abridgments.

(hh) Ibid.

(ii) Medical Museum, Vol. I. p. 442. Vol. II. p. 499.

(kk) Quesnay de Gangræna, p. 413.

in October 1764, whose curiosity led them to ascend the mountain, called here Pico Ruivo, being the highest land on the island, the perpendicular height of which above the surface of the sea is, according to Mr. de Luc's method of mensuration by the barometer and thermometer, $5141\frac{8}{10}$ English feet. Being supplied by him with the proper instruments, and their watch adjusted by his regulator, they carefully remarked the hour and minute each observation was made, which on their return was compared with the height of the thermometer and barometer in his study at the time of the observation, of which he had kept an exact account during the journey. From which observations he formed the following table, supposing the descent of the barometer $\frac{1}{10}$ of an inch for every 90 feet.

Descent of Barometer.		Elevat. at 90 feet for one tenth of in.	Descent of Thermom.	Elevat. corresponding to each Degree of Thermometer.
In.	Dec.	Feet.	Degrees.	Feet.
0.	4	360	2	180
1.	2	1080	$5\frac{1}{2}$	196+
1.	3	1170	6	195
1.	5	1350	9	150
1.	65	1485	10	148.5
3.	75	3375	17	198
4.	2	3780	19	199
5.	1	4590	18	255

Dr. H. suspected the justness of the last observation, it being made at noon on the summit of the mountain, the sun shining very hot, and no proper shade for the instrument. The thermometers were Fahrenheit's.

Though the different degrees of heat in different places must depend greatly on the accident of situation, with regard to mountains, valleys, and to the different soils, &c. yet there is so much regularity in the above observations, that perhaps we shall not err much in computing, where the soil and surface are tolerably uniform, "the decrease of heat, by Fahrenheit's thermometer, in the proportion of one degree for near 190 feet of elevation on this island."

XIX. Of a Stone voided without Help from the Bladder of a Woman at Bury. Communicated by Wm. Heberden, M.D., F.R.S. p. 128.

The wife of Charles Coe, a poor labouring man, of Lawshall, Suffolk, aged about 67, having been afflicted with symptoms of the stone between 11 and 12 years; her urine continually draining away with great uneasiness, and sometimes excruciating pains; and for some years unable to sit on a seat; on Monday, Feb. 11, 1765, voided a stone of a great size. For 2 or 3 days before the stone came away, blood was discharged from the meatus urinarius, particularly a large quantity of blood, without mucus, at the time the stone was voided; at which time she was not in great pain; but after its exclusion remarkably easy. Her urine then passed involuntarily without pain; and she could sit upon a seat without uneasiness. Her poverty was so great, that during this long and painful scene of suffering, she had no assistance from medicine or art, in any shape whatever; so that the exclusion of the stone was wholly the work of nature.

This stone is represented by fig. 2, pl. 5, being a side view of it: a shows the stone as indented by the pressure of the neck of the bladder, where appears the nucleus marked b; cc, several small striæ leading to those larger canals marked ddd, being the only passages by which the urine could get off, which was continually draining away; the lower canal appears corroded by the acrimony of the urine; ee, two appendices of fresh calculous matter. The weight of the stone was 2 oz. 2 dr. 24 grs. Its length was 3¾ inches, and its compass at the thickest part at a, 4 inches ¾.

XX. Astronomical Observations, made at Vienna. By the Rev. Father Jos. Liesganig. p. 130.

These observations of occultations and eclipses, are preceded by some remarks by Dr. Bevis, to whom they were sent, to the following purport. By Fa. L.'s own account, he was appointed to the Observatory of the Jesuit's College at Vienna towards the end of 1754; where he found, indeed, a large stock of instruments, but mostly unfinished and imperfect; that, after spending a whole year in getting them fit for use, he had disagreeable and unavoidable avocations, which kept him some years from resuming the care of his beloved Observatory; but that he now finds himself in possession of the following complete apparatus. 1. Two mural quadrants, each of 9 feet radius, placed north and south in the meridian (the Vienna to the London foot, as 10000 to 9646.) 2. A 6-foot quadrant, supported by a vertical axis, and convertible to any azimuth. 3. A 10-foot sector, constructed in P. Boschowick's manner. 4. A 4-foot quadrant placed on the azimuth circle which Tycho Brahe used at Prague. 5. A moveable quadrant of 2½ feet radius, which he used in the mensuration of 3 degrees on their meridian, by order of the Empress Queen, by means of a series of triangles, the result of which he is calculating at this time. 6. A transit instrument of 6½ feet.

Together with several fixed telescopes, a gnomon 14 feet high, micrometers, &c. of all which he intends to publish a particular description, with his observations taken at Vienna reduced and compared with astronomical tables. He makes the latitude of his observatory, at the Jesuit's College, 48° 12′ 35″.

April 4, 1764. J. BEVIS.

An Occultation of Spica ♍ by the Moon, Feb. 20, 1764.

True time.			
At 14^h	51^m	0^s	Immersion of the star in the ☾'s lucid limb.
15	14	16	Emersion from the dark limb.

A Lunar Eclipse, March 17th, 1764.

True time.	
At 11h 14m 45s	Beginning of the shadow, doubtful.
12 59 30	Greatest obscuration 8 dig. 23′.
14 29 30	End of the dense shadow.

A Solar Eclipse April 1, 1764.

True time.	
10h 21m 50s A. M.	Beginning of the eclipse.
11 11 46	Digits eclipsed 6.
11 43 6	Greatest obscuration 9 dig.
1 23 13 P. M.	End of the eclipse.

Occultation of Spica Virginis by the Moon, April 15, 1764.

11h 21m 42s Immersion in the moon's bright limb.

XXI. On the Case of a Supposed Hydrophobia. By James, Earl of Morton, President of the Royal Society. p. 139.

Having read in the Public Advertiser of June 22, 1764, that a person who, in consequence of the bite of a mad dog, was affected with the hydrophobia, had been cured at Padua by draughts of vinegar, I was willing to get the best information of the true state of the fact. Accordingly I wrote to my acquaintance general Græme, respecting it; and received from him the inclosed account. The account in the Public Advertiser, being so very circumstantial, induced me at first to believe it might be well founded; in which case, so valuable a discovery ought to have been published every where: but, as it turns out to be altogether a fallacy, the public ought equally to be undeceived. MORTON.

Venice, December 8, 1764.

The history of the hydrophobia cured by vinegar is equivocal, or perhaps altogether a mistake; and the process was what follows. Dr. Bertossi, physician, of Padua, came to Venice, in the last spring, and brought an account to Dr. Reghellini, that 3 hydrophobous persons, all bitten by the same mad dog, had been treated in the hospital at Padua, 2 of whom died, and only one escaped, and that the person who survived was cured bv Dr. Leonissa with vinegar, which he was made to swallow every 3 hours in doses of about 4 oz. at a time. This cure, performed by Dr. Leonissa, was suggested to him by a student of physic at Udine, who observed, in the Friuli, a hydrophobous person, who was cured by means of a mistake, that happened in the family, by giving him vinegar to drink instead of water. Dr. Reghellini, willing to be thoroughly satisfied, whether this acceptable discovery was strictly true, immediately wrote to a friend of his, a physician at Padua, stating all the circumstances, which had been related to him by Dr. Bertossi, and desiring to know if the fact really was as it had been stated. His friend, the physician, gave him for answer, that the thing was true.

Dr. Reghellini thereupon communicated the case to the physicians of the hospital of Florence and Pisa, and desired them to make trial of it, the first opportunity that should offer, and acquaint him with the success. He likewise communicated it to his other friends, among which was Dr. Turton, an English physician then at Venice; and to Dr. de la Fontaine, a physician, who attended Lord Spencer. Dr. Reghellini judged, that so uncommon an event ought to be published with all its circumstances; and having in his possession the history of 28 hydrophobous persons, though in different manners, and treated by different physicians, 15 of whom were afterwards opened, and the bodies carefully examined, he thought he might thence compose a rational and useful tract. He therefore went to Padua, to have a personal interview with Count Leonissa, the physician; but in this conference he discovered, that the man who was said to have been cured with the use of vinegar, really never had the hydrophobia, though he had been assured that Dr. Bertossi saw him in the hydrophobous state. That man, it was true, did receive a very slight and superficial scratch on his cheek from the same dog, who bit the other 2 persons, who became hydrophobous, and afterwards died; but the person of whom the account was published, about the useful discovery of a cure by vinegar, was in reality never arrived to the state of the hydrophobia; that is, to such a degree of the malady as most frequently follows the bite of a mad dog, and which, after some weeks, discovers itself by an uneasiness in attempting to drink; and after drinking, by a fever, delirium, convulsions, vomiting, sweating, and death, within the 5th, and sometimes within the 4th day.

Dr. Reghellini, having thus found, that the account first given him, and the confirmation of it from his friend at Padua, were doubtful, or rather a misapprehension, wrote again to Florence and Pisa, retracting his former account, and relating the fact, as on a more strict examination he had found it truly to be, and which was exactly agreeable to the preceding account.

XXII. Two Theorems, by Edward Waring, M.A., F.R.S., &c. From the Latin. p. 143.

THEOR. 1.—In a given ellipse inscribe 2 polygons of n sides, *abcde* &c, and *pqrst* &c; at the respective points a, b, c, d, e, &c, p, q, r, s, t, &c, draw the tangents AB, BC, CD, DE, &c, and PQ, QR, RS, ST, &c, fig. 7, pl. 5; and let the $\angle ab\mathrm{B} = \angle cb\mathrm{C}$; $\angle bc\mathrm{C} = \angle dc\mathrm{D}$; $\angle cd\mathrm{D} = \angle ed\mathrm{E}$; &c; and $\angle pq\mathrm{Q} = \angle rq\mathrm{R}$; $\angle qr\mathrm{R} = \angle sr\mathrm{S}$; $\angle rs\mathrm{S} = \angle ts\mathrm{T}$; and so on. Then will the sum of the sides $ab + bc + ed + de +$ &c $= pq + qr + rs + st +$ &c.

Corol. In the ellipse draw the polygon *abcde* &c, fig. 8, of n sides, in the above method; inscribe also another polygon *ahklm* &c, of *n* sides, in any other manner, but having one angle at the point *a*. Then the sum *ab* + *bc* + *cd* + *de* + &c, will be greater than the sum *ah* + *hk* + *kl* + *lm* + &c.

THEOR. 2.—In fig. 7, describe about the given ellipse two polygons ABCDE &c, and PQRST &c, of *n* sides, of which the points of contact are respectively *a, b, c, d, e,* &c, and *p, q, r, s, t,* &c. And let the

(tang. + sec.) comp. ∠ *a*B*b* : (tan. + sec.) comp. ∠ *c*C*b* :: *b*C : *b*B, and
(tang. + sec.) comp. ∠ *c*C*b* : (tan. + sec.) comp. ∠ C*D*d* :: CD : *c*C, and
(tang. + sec.) comp. ∠ *c*D*d* : (tan. + sec.) comp. ∠ *e*E*d* :: *d*E : *d*D, &c. also
(tang. + sec.) comp. ∠ *p*Q*q* : (tan. + sec.) comp. ∠ *q*R*r* :: *q*R : *q*Q, and
(tang. + sec.) comp. ∠ *q*R*r* : (tan. + sec.) comp. ∠ *s*S*r* :: S*s* : *r*R, and
(tang. + sec.) comp. ∠ *s*S*r* : (tan. + sec.) comp. ∠ *t*T*s* :: T*r* : *s*S, and so on.

Then will the sum of the sides AB + BC + CD + DE + &c. = PQ + QR + RS + ST + &c.

Corol. Let there be described about an ellipse, as before, a polygon of *n* sides ABCDE &c, fig. 9; describe also about the ellipse another polygon GHKLM &c, of *n* sides, in any other manner, but having one point of contact *a* the same as the former. Then will the sum AB + BC + CD + DE + &c. be less than the sum GH + HK + KL + LM + &c.

Similar properties may also be affirmed of the polygons described in the hyperbolas.

XXIII. A Dissertation on the Nature of Evaporation and several Phenomena of Air, Water, and boiling Liquors. By the Rev. Hugh Hamilton, D. D., F.R.S. p. 146.

This dissertation may be consulted with advantage in a much improved edition of it, in the author's volume of Philosophical Essays.

*XXIV. Physical and Meteorological Observations, Conjectures, and Suppositions. By Benjamin Franklin, LL.D., and F.R.S.** p. 182.

The particles of air are kept at a distance from each other by their mutual repulsion. Every 3 particles mutually and equally repelling each other, must form an equilateral triangle. All the particles of air gravitate towards the earth; which gravitation compresses them, and shortens the sides of the triangles, otherwise their mutual repellency would force them to greater distances from each other. Whatever particles of other matter, not endued with that repellency, are supported in air, must adhere to the particles of air, and be supported by them;

* On reading the preceding paper in the Society, it was recollected that this paper, similar in some particulars, had been communicated to the Society about nine years before, though not till now printed.—Orig.

for in the vacancies there is nothing they can rest on. Air and water mutually attract each other. Hence water will dissolve in air, as salt in water.

The specific gravity of matter is not altered by dividing the matter, though the superficies be increased. Sixteen leaden bullets, of an ounce each, weigh as much in water as one of a pound, whose superficies is less. Therefore the supporting of salt in water is not owing to its superficies being increased. A lump of salt, though laid at rest at the bottom of a vessel of water, will dissolve in it, and its parts move every way till equally diffused in the water; therefore there is a mutual attraction between water and salt. Every particle of water assumes as many of salt as can adhere to it; when more is added, it precipitates, and will not remain suspended. Water, in the same manner, will dissolve in air, every particle of air assuming one or more particles of water; when too much is added, it precipitates in rain. But there not being the same contiguity between the particles of air as of water, the solution of water in air is not carried on without a motion of the air, so as to cause a fresh accession of dry particles.

Part of a fluid, having more of what it dissolves, will communicate to other parts that have less. Thus very salt water coming in contact with fresh, communicates its saltness till all is equal, and the sooner if there is a little motion of the water. Even earth will dissolve, or mix with air. A stroke of a horse's hoof on the ground in a hot dusty road, will raise a cloud of dust, that shall, if there be a light breeze, expand every way till perhaps near as large as a common house. It is not by mechanical motion communicated to the particles of dust by the hoof that they fly so far, nor by the wind that they spread so wide. But the air near the ground, more heated by the hot dust struck into it, is rarefied and rises, and in rising mixes with the cooler air, and communicates of its dust to it, and it is at length so diffused as to become invisible. Quantities of dust are thus carried up in dry seasons. Showers wash it from the air and bring it down again. For water attracting it stronger, it quits the air and adheres to the water.

Air suffering continual changes in the degrees of its heat, from various causes and circumstances, and consequently changes in its specific gravity, must therefore be in continual motion. A small quantity of fire mixed with water, or degree of heat in it, so weakens the cohesion of its particles, that those on the surface easily quit it, and adhere to the particles of air. A greater degree of heat is required to break the cohesion between water and air. Air moderately heated will support a greater quantity of water invisibly than cold air; for its particles being by heat repelled to a greater distance from each other, thereby more easily keep the particles of water, that are annexed to them, from running into cohesions, that would obstruct, refract, or reflect the light. Hence, when we breathe in warm air, though the same quantity of moisture may be taken up from the lungs as when we breathe in cold air, yet that moisture is not so visible.

Water being extremely heated, i. e. to the degree of boiling, its particles, in quitting it, so repel each other, as to take up vastly more space than before, and by that repellency support themselves, expelling the air from the space they occupy. That degree of heat being lessened, they again mutually attract, and having no air-particles mixed, to adhere to, by which they might be supported and kept at a distance, they instantly fall, coalesce, and become water again. The water commonly diffused in our atmosphere never receives such a degree of heat from the sun, or other cause, as water has when boiling; it is not, therefore, supported by such heat, but by adhering to air. Water being dissolved and adhering to air, that air will not readily take up oil, because of the natural repellency between water and oil. Hence cold oils evaporate but slowly, the air having generally a quantity of dissolved water. Oil being heated extremely, the air that approaches its surface will be also heated extremely; the water then quitting it, it will attract and carry off oil, which can now adhere to it. Hence the quick evaporation of oil heated to a great degree. Oil being dissolved in air, the particles to which it adheres will not take up water. Hence the suffocating nature of air impregnated with burnt grease, as from snuffs of candles, and the like. A certain quantity of moisture should be every moment discharged and taken away from the lungs. Air that has been frequently breathed is already overloaded, and for that reason can take no more, so will not answer the end. Greasy air refuses to touch it. In both cases suffocation ensues for want of the discharge.

Air will attract and support many other substances. A particle of air loaded with adhering water, or any other matter, is heavier than before, and would descend. The atmosphere supposed at rest, a loaded descending particle must act with a force on the particles it passes between, or meets with, sufficient to overcome in some degree their mutual repellency, and push them nearer to each other. Thus, supposing the particles ABCD, and the others near them to be at the distance caused by their mutual repellency (confined by their common gravity) if A would descend to E, it must pass between B and C. When it comes between B and C, it will be nearer to them than before, and must either have pushed them nearer to F and G, contrary to their mutual repellency, or pass through by a force exceeding its repellency with them. It then approaches D, and, to move it out of the way, must act on it with a force sufficient to overcome its repellency with the two next lower particles, by which it is kept in its present situation. Every particle of air, there-

A
O O O
FO BO CO GO
O DO O
O O O O
E

fore, will bear any load inferior to the force of these repulsions. Hence the support of fogs, mists, clouds.

Very warm air, clear, though supporting a very great quantity of moisture, will grow turbid and cloudy on the mixture of a colder air, as foggy turbid air will grow clear by warming. Thus the sun shining on a morning fog, dissipates it. Clouds are seen to waste in a sunshiny day. But cold condenses and renders visible the vapour. A tankard, or decanter, filled with cold water, will condense the moisture of warm clear air, on its outside, where it becomes visible as dew, coalesces into drops, descends in little streams. The sun heats the air of our atmosphere most near the surface of the earth; for there, besides the direct rays, there are many reflections. Moreover, the earth itself being heated, communicates of its heat to the neighbouring air. The higher regions having only the direct rays of the sun passing through them, are comparatively very cold. Hence the cold air on the tops of mountains, and snow on some of them all the year, even in the torrid zone. Hence hail in summer. If the atmosphere were, all of it, both above and below, always of the same temper, as to cold or heat: then the upper air would always be rarer than the lower, because the pressure on it is less, consequently lighter, and therefore would keep its place. But the upper air may be more condensed by cold, than the lower air by pressure. The lower more expanded by heat, than the upper for want of pressure; in such cases the upper air will become the heavier, the lower the lighter. The lower region of air being heated and expanded, heaves up and supports, for some time, the colder heavier air above, and will continue to support it while the equilibrium is kept. Thus water is supported in an inverted open glass, while the equilibrum is maintained by the equal pressure upwards of the air below; but the equilibrium by any means breaking, the water descends on the heavier side, and the air rises into its place. The lifted cold heavy air over a heated country, becoming by any means unequally supported, or unequal in its weights, the heaviest part descends first, and the rest follows impetuously. Hence gusts after heats, and hurricanes in hot climates. Hence the air of gusts and hurricanes, cold, though in hot climes and seasons; it coming from above.

The cold air descending from above, as it penetrates our warm region full of watry particles, condenses them, renders them visible, forms a cloud, thick and dark, overcasting sometimes at once, large and extensive, sometimes, when seen at a distance, small at first, gradually increasing; the cold edge, or surface of the cloud, condensing the vapours next it, which form smaller clouds, that join it, increase its bulk, it descends with the wind and its acquired weight, draws nearer the earth, becomes denser with continual additions of water, and discharges heavy showers. Small black clouds thus appearing in a clear sky, in hot climates, portend storms, and warn seamen to hand their sails.

The earth turning on its axis in about 24 hours, the equatorial parts must move about 15 miles in each minute. In northern and southern latitudes this motion is gradually less to the poles, and there nothing. If there was a general calm over the face of the globe, it must be by the air's moving in every part as fast as the earth, or sea, it covers.

He that sails, or rides, has insensibly the same degree of motion as the ship, or coach, with which he is connected. If the ship strikes the shore, or the coach stops suddenly, the motion continuing in the man, he is thrown forward. If a man were to jump from the land into a swift sailing ship, he would be thrown backward, or towards the stern, not having at first the motion of the ship. He that travels, by sea or land, towards the equinoctial, gradually acquires motion; from it, loses. But if a man were taken up from latitude 40 (where suppose the earth's surface to move 12 miles per minute) and immediately set down at the equinoctial, without changing the motion he had, his heels would be struck up, he would fall westward. If taken up from the equinoctial, and set down in latitude 40, he would fall eastward.

The air under the equator, and between the tropics, being constantly heated and rarefied by the sun, rises. Its place is supplied by air from northern and southern latitudes, which coming from parts where the earth and air had less motion, and not suddenly acquiring the quicker motion of the equatorial earth, appears an east wind blowing westward, the earth moving from west to east, and slipping under the air. Thus, when we ride in a calm, it seems a wind against us. If we ride with the wind, and faster, even that will seem a small wind against us. The air rarefied between the tropics, and rising, must flow in the higher region north and south. Before it rose, it had acquired the greatest motion the earth's rotation could give it. It retains some degree of this motion, and descending in higher latitudes, where the earth's motion is less, will appear a westerly wind, yet tending towards the equatorial parts, to supply the vacancy occasioned by the air of the lower regions flowing thitherwards. Hence our general cold winds are about north-west, our summer cold gusts the same.

The air in sultry weather, though not cloudy, has a kind of haziness in it, which makes objects at a distance appear dull and indistinct. This haziness is occasioned by the great quantity of moisture equally diffused in that air. When, by the cold wind blowing down among it, it is condensed into clouds, and falls in rain, the air becomes purer and clearer. Hence, after gusts, distant objects appear distinct, their figures sharply terminated.

Extreme cold winds congeal the surface of the earth, by carrying off its fire. Warm winds, afterwards blowing over that frozen surface, will be chilled by it. Could that frozen surface be turned under, and a warmer turned up from beneath it, those warm winds would not be chilled so much. The surface of the earth is also sometimes much heated by the sun; and such heated surface not being changed, heats the air that moves over it. Seas, lakes, and great bodies of water, agitated by the winds, continually change surfaces; the cold surface in winter is turned under, by the rolling of the waves, and a warmer turned up; in summer, the warm is turned under, and colder turned up. Hence the more equal temper of sea-water, and the air over it. Hence in winter, winds from the sea seem warm, winds from the land cold. In summer the contrary. Therefore the lakes north-west of us,* as they are not so much frozen, nor so apt to freeze as the earth, rather moderate than increase the coldness of our winter winds. The air over the sea being warmer, and therefore lighter in winter than the air over the frozen land, may be another cause of our general north-west winds, which blow off to sea at right angles from our North American coast; the warm light sea air rising, the heavy cold land air pressing into its place.

Heavy fluids descending frequently form eddies, or whirlpools, as is seen in a funnel; where the water acquires a circular motion, receding every way from a centre, and leaving a vacancy in the middle, greatest above, and lessening downwards, like a speaking trumpet, its large end upwards. Air descending, or ascending, may form the same kind of eddies, or whirlings, the parts of air acquiring a circular motion, and receding from the middle of the circle by a centrifugal force; and leaving there a vacancy, if descending, greatest above, and lessening downwards; if ascending, greatest below, and lessening upwards; like a speaking trumpet, standing its large end on the ground. When the air descends with violence in some places, it may rise with equal violence in others, and form both kinds of whirlwinds. The air in its whirling motion receding every way from the centre or axis of the trumpet, leaves there a vacuum, which cannot be filled through the sides, the whirling air as an arch preventing; it must then press in at the open ends. The greatest pressure inwards must be at the lower end, the greatest weight of the surrounding atmosphere being there. The air entering rises within, and carries up dust, leaves, and even heavier bodies that happen in its way, as the eddy or whirlpool passes over land. If it passes over water, the weight of the surrounding atmosphere forces up the water into the vacuity; part of which, by degrees joins with the whirling air, and adding weight, and receiving accelerated motion, recedes still farther from the centre or axis of the trump, as the pressure lessens; and at last, as the trump widens, is broken into small particles, and so united with air as to be supported by it, and become black clouds at the top of the trump. Thus these eddies may be whirlwinds at land, waterspouts at sea. A body of water so raised may be suddenly let fall, when the motion, &c. has not strength to sup-

* In Pennsylvania. —Orig.

port it, or the whirling arch is broken so as to let in the air; falling in the sea, it is harmless, unless ships happen under it. But if in the progressive motion of the whirl it has moved from the sea over the land, and there breaks, sudden, violent, and mischievous torrents are the consequence.

XXIV. Historical Memoirs, relating to the Practice of Inoculation for the Small Pox, in the British American Provinces, particularly in New England. By Benjamin Gale, A. M. p. 193.

The small-pox, by the vigilant execution of the laws subsisting in the several New England colonies, had never generally prevailed among the inhabitants, excepting in Boston, the capital town, in the province of the Massachusett's Bay, where it had been epidemical, A. D. 1649, 1666, 1678, 1689, 1702, 1721, 1730, 1752, and at the above date, 1764, and where the success attending inoculation, after much opposition, and endeavours used to bring the same into disrepute, became incontestably evident. In the provinces of New York, New Jersey, and Pennsylvania, the like precautions had not been taken; and the small-pox had prevailed in those provinces, but more especially in the capital towns, and places adjacent, once in about 6 or 7 years, where inoculation had been practised with surprizing success, to the preservation of many lives.

A. D. 1702, the inhabitants of the town of Boston were 6750 souls, at this time there died of the whites 300. A. D. 1721, the number of the inhabitants were 10,567, besides those moved out to avoid the disease; the discumbents were 5,989, of which 844 died, i. e. near 1 in 7. At this time, in and about Boston, 286 were inoculated, of which 6 died, i. e. about 1 in 48. This was the beginning of inoculation in New England, soon after it was first practised in London.* A. D. 1730, the discumbents were estimated at 4000, of which about 500 died; of about 400 inoculated, 12 died, i. e. 1 of 33.

A. D. 1752, there was an exact account taken, by order of the magistrates of the town of Boston, and rendered upon oath, in order to remove the prejudices and objections made against inoculation, of all who had the small-pox, either in the natural way or by inoculation, and of the precise number of those who died of the small-pox in either way; by which account it appears that the number of those who had the distemper in the natural way, including blacks, amounted to 5,544, of which number died, including blacks, 514; the whole number inoculated, including blacks, was 2,113, of which died, including blacks, 30. At this time, all present had the small-pox, except about 174; the total of residenters, including 1544 negroes, being 15,734; those who fled from the small-pox estimated at 1,800. Hitherto mercury had not been made use of in inoculation in Boston.

* Dr. Douglas's Summary Hist. Vol. 2, p. 395.—Orig.

A. D. 1764, at the above date, the small-pox was prevalent in the town of Boston; by the last accounts, 3000 had recovered from inoculation, in the new method, by the use of mercury, and 5 only had died, viz. children under 5 years; so that it appears that death without inoculation is 1 in 7 or 8; by inoculation without mercury, 1 in 80 or 100; by inoculation with mercury, 1 in 800 or 1000. The use of mercury, in the small-pox, was first hinted by the learned Boerhaave, who died in 1738; this intimation was improved, and mercury introduced into practice by physicians in the English American colonies, about 1745. Several American physicians claim the 2d glory to Boerhaave. Dr. Thomas of Virginia, and Dr. Murison of Long Island, in the province of New York, may justly have merited that honour, who have successfully practised by the use of mercury, perhaps before any other, either in Europe or America.

During the late war, the small-pox was brought into divers towns, in this and the other colonies, by the return of our soldiers (employed in his majesty's service, in the pay of the New England colonies) for winter quarters, and by seamen employed in our navigation to the British islands in the West Indies, where the small-pox was universally prevalent, which produced a universal concern among the inhabitants, lest the same should become general, and spread through this and the other colonies in New England. On which, application was made to the legislature of this colony, for liberty to inoculate for the small-pox, by the officers of our provincial troops and others, which was accordingly granted; as also that hospitals for that purpose might be erected in such towns of the colony as should see cause to permit the same. However, instead of regulating such hospitals as should be erected for that purpose, by well-adapted laws, to prevent any communication with these hospitals from abroad, or the subjects of inoculation leaving the same without licence from the attending physician, unhappily that matter was left to be regulated at the discretion of the overseers of the several towns where inoculation should be practised, which required the strictest laws, enforced by severe penalties, without which it would be impossible for the attending physician to restrain his patients, when grown impatient with confinement and a recluse life. From this defect, some persons left the hospitals, not being duly cleansed, and unhappily communicated the small-pox to divers persons, of which some died; on which, the law permitting inoculation was repealed, notwithstanding three hospitals had been erected in this colony, at no inconsiderable expence, and no further attempts were made to regulate the practice of inoculation, by measures well adapted for that purpose.

On which, persons engaged in trade, seamen, and youth, living in sea-port towns, and places more exposed to frequent invasions of the small-pox, resorted in great numbers to New York, to obtain inoculation. On this emigration of the inhabitants, and partly to prevent, but principally to secure against the spreading of the contagion in the colony, the assembly prohibited inoculation within the limits of this colony, on very severe penalties; and in case people went into any other government to obtain it, ordered them not to return again to the colony, without first having remained out at least 20 days after leaving the hospital, or place of infection, on the penalty of £20; and if after remaining out of the colony 20 days, they should unfortunately happen, either by their clothes or otherways, to communicate the infection, they were made liable to pay to the party injured, treble damages, and costs of suit. Thus the practice of inoculation for the small-pox stands wholly interdicted within the colony, and laid under such disadvantages and discouragements, when persons go abroad to procure it, that we are in a great measure deprived of the only method, ever discovered to the world, to escape the hazards attending that disease which has made such havock of the human species.

Was inoculation, on some of our small islands on the sea coast, or on some point of land at a proper distance from inhabitants, impracticable with safety to the inhabitants of this colony, he should not think it unreasonable wholly to suppress it; but doubtless it may be so regulated as to be quite safe, and without danger of communication; and therefore he thinks he may justly say, to deny liberty of inoculation to persons in trade, seamen, and such as are more immediately exposed to the disease, or to lay those who would go out of the colony to obtain it, under so great disadvantages, is an invasion of the natural rights of mankind, and an obstruction to their pursuing the first law of human nature.

The well-peopling the colonies, and securing our new acquisitions, are matters of great importance to our mother country, as well as to ourselves; and the more it is effected from the colonies themselves, without transporting settlers from the kingdoms of Great-Britain and Ireland, the greater advantages must accrue to the manufacturers of the mother country, as colonizing from the plantations will keep the price of labour at so high a rate, as will effectually prevent our engaging in manufactures, and greatly increase the sale of British manufactures in America. The number of the inhabitants in our old American settlements double once in 20 or 25 years, and in our new made settlements, once in 15 or 20 years. The New England colonies are better peopled than the other provinces and colonies in America; which he principally attributes to the tenure of their lands, which are held in fee-simple, according to the tenure of the manor of East Greenwich in Kent; and he conceives nothing would so much facilitate the settlement of crown lands, obtained by new acquisitions in America, as their being granted in like manner: paying quit-rents to monopolizers of large tracts of land, is not well relished by Americans, and has in itself a natural tendency to render the defence of the country against foreign invaders, and their savage enemies despicably infamous. A signal instance of this happened during the French war, A. D. 1745. The colony of Connecticut having just before finished the settlement of their new lands, adjoining to the Manor of Livingston, in the province of New York, being on the north-west frontiers of this colony, some skulking parties of Indians being seen in the manor aforesaid, the tenants left their settlements, which had been made almost a century before, and fled over into this country to our new-made settlements, which then had not been made more than 7 years, where they thought themselves safe and secure; a convincing proof that no men will face an enemy like those who fight pro aris et focis. The southern colonies in particular, have been driven before a despicable enemy like sheep; this never was the case, even in the infant state of these colonies.

The census of the inhabitants of this colony, transmitted by governor Fitch, A. D. 1756, by order of the Lords of Trade, was 128,218 souls whites, and 3587 blacks; that of the year 1762, 141,000 souls whites, 4590 blacks, of which 930 were Indians. The levies of our fencible men diminished the increase, so that the last 7 years the colony only increased 13,000. On the peace, doubtless the rapidity of population will recover; and in how short a space of time the well settling the new acquisitions may be effected, from all the American colonies collectively, he leaves every one to determine; and he cannot but think, that whenever the state of public affairs will permit the parliament of Great Britain to advert to the peopling and securing the acquisitions made in America, they will judge it best effected, as much as may be, from her colonies in America; and that the law prohibiting inoculation in America will be accordingly annulled, by their superintending authority, as prejudicial to the population of the colonies.

It appears from Dr. Douglas's account of the small-pox in the town of Boston, where he lived, and made critical observations, the last 3 times that it was epidemic there, viz. A. D. 1721, 1730, and 1752, that the number of persons visited with the small-pox, in the natural way, was 16,047, of which 1,858 died; and that in 1752, the number of those who received the infection by inoculation, before mercury was made use of in Boston in inoculation, amounted to 2,113 persons, of which 30 died (blacks in both being included;) granting that those who had the disease in the natural way stood an equal chance for life with those who were inoculated, it appears that, in those 3 years, there died 1,831, in the town of Boston only, for want of inoculation; by which deaths, according to the longest term of doubling the number of inhabitants in America, in one century from those periods, the number will be diminished by 29,296, which is, from the best calculation, a number far superior to those who came from Europe, into the New England colonies, from their first settlement, to this present day.

Various sea-port towns, as well as some inland places, have been visited with the small-pox, since the first settlement of the New England colonies, by which many have died; perhaps, taken collectively, not more than 1 in 4 have recovered; partly owing to removing the sick, in order to prevent communicating the infection; and partly to the want of skilful physicians, and faithful experienced nurses to attend the sick, which often obliged to make use of some of the most abandoned wretches, for want of suitable persons who had had the small-pox, to perform that service. It is much regretted by many, that the practice of inoculation may not be tolerated, in the New England colonies, and regulated by laws, well adapted to prevent the spreading the contagion, among such as do not chuse, and those whose circumstances will not permit them, to comply with the expence attending it.

The increase of mankind has been more impeded by the small-pox, than is usually imagined; it is not the loss of one in 6 or 8, who die with the disease, that is chiefly to be attended to, but the accumulated loss of all the posterity, which might have descended from them, multiplied through all succeeding generations. Therefore perhaps it might be thought wisdom to address the throne for liberty to erect one hospital in each of the New England colonies for that purpose; that those, at least, who are engaged in trade and navigation, might have the benefit of inoculation, and be exempt from the hazard of the disease, while necessarily engaged in business abroad, and not endanger their friends on their return home.

XXV. A Balance of a New Construction, supposed to be of Use in the Woollen Manufacture. By W. Ludlam, B. D. p. 205.

It is of consequence in some branches of the woollen manufacture, that the thread of which any piece is woven should be all of the same fineness. After it is spun, it is made into skains of the same length, and these are sorted according to the fineness of the spinning. The manufacturers usually distinguish and denominate the fineness, by the number of skains which go to the pound; the coarsest being about 12 to the pound, and the finest near 60. There is no other method of sorting in use, except by the eye; but it requires great nicety to distinguish the size of threads so small, and long experience to know by the look only, how many skains of any particular sort will make a pound. A method of weighing them readily would save much time: the machine here delineated is for that purpose. It resembles the beam of a common pair of scales, fig. 10, pl. 5. At one end of it is a fixed weight, which Mr. L. calls the counterpoise, at the other a hook: in sorting, the skain to be examined is put upon the hook, and sinks down more or less, according to its weight, till the counterpoise by rising balances it, and then the index or cock of the beam points out, on a graduated arch, the number of skains of that sort which go to the pound. A scale, instead of the hook, might be used for weighing money, if the arch were properly divided for that purpose.

Mr. Rouse, of Harborough, many years ago, made a machine for sorting woollen thread on the same principle with this; but as what he did was mostly tentative, he was not aware of some considerable advantages which the theory points out. For the machine will not distinguish with equal nicety the skains of every size. In Mr. Rouse's machine, the divisions were too small, and the largest chanced to fall at 18 to the pound; but it would have been better if the finer sorts had been more accurately distinguished, being of greater consequence to be well sorted, and more difficult to be sorted by the eye only, than the coarser ones. This machine distinguishes best the yarn of 36 to the pound, one of the finest sorts in common use, the largest division lying between 36 and 37; the other divisions are as large, and the whole range of the index as much as can be allowed without other inconveniencies. The theory contains the necessary rules for finding the angle of the beam, for calculating the divisions on the arch, and for placing their largest interval in any part of them.

Directions for making the Balance.

It consists of a mahogany stand, a steel beam and brass ring for the divisions. FGH is the triangular base of the stand, having a screw in each angle to set it level: into this is dove-tailed the upright back KK; the standard board LLL is put into a dove-tailed groove in the back, and tenanted into the triangular board at the bottom. The two cocks CC, between which the arbor of the beam plays, and the ring RR, are screwed to the standard board LL. The beam AB, with its cock or index E, is broad in the middle to gain strength, and pierced to make it lighter. It is rivetted on a collet soldered to the arbor, as clock wheels are The pivots run in hard brass, and have plates of hardened steel for their points to bear against.

At one end of the beam is the counterpoise A, consisting of 2 round pieces of brass, screwed together through a hole in the beam. The other end of the beam is made thicker for about half an inch in length, and slit to receive a loop of hardened steel, which hangs on the steel pin B; on the lower part of the loop hangs the hook, which holds the skain. The block of wood N, is screwed to the standard board; the upper part of which is lined with velvet for the counterpoise to rest upon, when the skain is taken off the hook; the brass pin P is for the other arm of the beam to bank against. The angle of the beam ACB is 168° 6′, the radius CA or CB, 6 inches; the breadth of the beam in the middle $\frac{3}{4}$ of an inch, at the ends $\frac{1}{4}$; the thickness at the end B where it is slit $\frac{1}{6}$; every where else $\frac{1}{18}$. The length of the arbor $1\frac{3}{4}$; diameter of the pivots $\frac{1}{30}$. The weight of the counterpoise one ounce avoirdupois. The hook, with the loop and steel pin included, 0.68 avoirdupois. The division of 50 is at the top of the ring.

In making the beam, the point of the index E must be equally distant from the centres A and B; the whole beam and index made so as to poise itself, and remain at rest in any position, before the counterpoise steel pin and loop are put on. The counterpoise being at first made too heavy, to adjust it, take off the beam, but let the two cocks and ring remain screwed on; hang then a fine wire and plummet on the top division, and with a watch glass look through one pivot hole till you see the wire against the other; turn the screw G till the wire bisects the hole, and the top division is then perpendicularly over it. Take away the plumb line, and put on the beam and counterpoise; on the loop hang a weight, which together with the steel pin and loop makes one ounce avoirdupois; turn now, by degrees, some metal off the counterpoise, till the index points at the top division, and the counterpoise will be truly adjusted, whether the arms of the beam, CA, CB, are precisely equal or not: for though the arms should be nearly equal, it is not necessary they should be exactly so, as in scales.

The divisions are set on the ring by an instrument made on purpose, which will very readily cut them, though unequal, with great exactness, on any circle, large or small. To prove the beam, put a weight into the scale (that of 28 to the pound is the best in this case) and see if it brings the index down to the proper division on the ring: if it carries it too far, the angle of the beam ACB is too great; if the contrary, too little; and the arms CA, CB, must be set a little in or out, till the angle is right: or the angle of the beam may be first found experimentally, and the divisions calculated to it, which is not much trouble; for having a table ready made for the intended angle, the alterations in that table occasioned by a small variation from that angle, will be easily found. When the balance is to be used, a weight of $\frac{1}{50}$ of a pound avoirdupois is to be put on the hook: the screw G must then be turned, till the index E points at the division of 50: the machine is then properly adjusted, and the weight may be taken off, and a skain put on.

XXVI. An Experimental Enquiry into the Mineral Elastic Spirit, or Air, contained in Spa Water; as well as into the Mephitic Qualities of this Spirit. By William Brownrigg, M.D., F.R.S. p. 218.

Dr. B. here states that 24 years before, he had presented to the R. S. several Essays on Damps, which were read at the meetings, and were preserved among the papers of the R. S. Those essays contained a few of the observations and experiments which he had collected, for a larger work on that subject; and for which, as opportunity offered, he had afterwards collected materials. And being desirous that some of his observations should be further verified, and more fully ascertained before they were made public, he had then declined the honour of having those Essays published in the Phil. Trans., esteeming it sufficient, at that time, to have excited several of his friends, and among these, Sir Hans Sloane, and the Rev. Doctor Hales, to communicate to him their sentiments on this subject.

In one of those essays he had occasion to remark, that a more intimate acquaintance with damps and other mineral exhalations might lead to a discovery of the nature and origin of those subtile and volatile principles, which enter the composition of various mineral waters, and are stiled their spirit; on which their chief virtues are found to depend; and that some of those waters seem to be impregnated with exhalations that nearly resemble the fulminating-damp; which, by its explosions, is found so destructive in coal-mines: while other waters are more evidently saturated with that most subtile and active exhalation, which, in many places perspires from springs and lakes, and other openings of the earth; or arises in pits and mines, where it is discovered by extinguishing flame; and, from its pernicious effects, in killing all animals that breathe therein, is known to our miners by the name of choak-damp. This mephitic exhalation he long ago discovered to be a particular kind of air, or permanently elastic fluid; and, from various observations and experiments related in the above-mentioned Essays, had reason to conclude, that it enters into the composition of the waters of Pyrmont and Spa, and of all others which, from their sharp and pungent taste, are stiled acidulæ; and that it constitutes the volatile principle of those waters, called their spirit, on which their prime virtues chiefly depend; although it has hitherto evaded the inquiries of the most skilful chemists, who have not been able to retain it in their vessels; neither have they discovered any method of imitating it by their art.

In order the more fully to ascertain a fact of such importance, and to obtain a clearer knowledge of the nature of this spirit, he took the opportunity while at Spa, to make a few experiments on those celebrated waters, as follow:

Exper. 1. Having filled a common quart bottle with Spa water at the fountain Pouhon, Dr. B. took a dried calf's bladder, made limber with oil, from which he carefully pressed out the air, by twisting it round, then drawing its orifice over the neck of the bottle, there tied it close, so as to leave an open communication between the water in the bottle and the empty cavity of the bladder, while the external air was excluded from both. He filled 2 other quart bottles with the Pouhon water, and fitted them to bladders in like manner. These bottles stood 14 days during the month of July, in a warm room; where he often examined them, expecting to have seen the bladders distended with air arising from the water. But in this was disappointed. For, during those 14 days, he never could discover any air-bubbles adhering to the insides of the bottles; neither, from other signs, did it appear that any elastic substance was separated from the water; the bladders, all that time, remaining as empty as when first tied on. Some of the water, being then poured from each of the bottles, was found to sparkle more than when taken fresh from the fountain, and sooner to emit bubbles on the sides of the glasses into which it was poured; it was perfectly clear, and still retained its brisk and pungent taste, to which was added a taste from the bladders; but, in all other respects, it seemed as little altered as it would have been, had the bottles, which contained it, been all the while well closed with corks.

In order more fully to ascertain the facts above-mentioned, he filled 2 of those long phials in which Frontiniac wine is usually kept, and 2 common quart bottles, with the Pouhon water, and fitted to them bladders in manner before related; excepting that the necks of the bladders were soaked in water, in order that they might better adhere to the glass than they did when moistened with oil. The water, thus excluded from any communication with the external air, stood 7 days in a room, where it was continually kept lukewarm; the weather at that time being excessively hot, the mercury then usually standing from 80 to 85 degrees in Fahrenheit's thermometer. During all that time, the bladders were not distended by any elastic substance arising from the water thus heated, but remained as empty as when tied to the vessels. The water in the 2 quart bottles, being examined after it had thus stood 7 days, was clear, retained its brisk and sharp taste, and seemed in nowise decompounded; but, when poured into a glass, sparkled much, like wine on the fret. After a 3d part of the water was poured from one of those bottles, it was immediately closed with the hand, and shaken briskly about for $\frac{1}{2}$ a minute; and being then suddenly opened, the air rushed out of it with an explosive noise, and more than ordinary violence, driving the water with great force, and dispersing it over the floor in a shower of 7 yards in diameter.

It has been shown by Mons. Mariotte, that air is imbibed, in considerable quantities, by common water, and may again be separated from it, either by heat, or by cold, as in congelation; or by removing from it the pressure of the atmosphere, as in the exhausted receiver of an air-pump. And Dr. Shaw* relates that, without any of these aids, air separates spontaneously from the mineral water of Scarborough, and that in a few minutes, he collected into bladders large quantities of air, from bottles of that water, after the manner attempted in the

* See his Inquiry into the Contents, Virtues, and Uses of the Scarborough Spa waters. Part 2, sect. 4.—Orig.

foregoing experiments: he also relates, that the air seemed to arise from the water with some degree of force, and continued long in an elastic state. The same experiment was since tried, by Dr. Home, on the chalybeate waters of Dunse in Scotland;* who collected considerable quantities of true permanent air from those waters; which he conjectures in no respect to differ from the common air of the atmosphere. And as Dr. B. had no doubt of the accuracy and fidelity of those gentlemen in their experiments; and had entertained an opinion that the most spirituous acidulæ contained the most air, which, it was reasonable to suppose, might be separated from them in the same manner that it was separable from those that were less spirituous; he therefore was greatly disappointed in his expectations, when he could not obtain any air from the Pouhon water after the manner above related. He did not, however, conclude, till after repeated trials, that air does not spontaneously separate from the Pouhon water, in such manner, and in so short a time, as it is said to separate from the Dunse and Scarborough waters; and that, when excluded from the common air, it will, for several days, bear the greatest heats of the summer, usually found in the shade, at Spa, without parting with any of the air, or other principles of which it is composed. It is, however, manifest, that, by the heat applied in the above experiments, the Pouhon water was disposed more readily to part with its air, or some other elastic substance, than it is when taken cool from the spring; seeing that, when poured into a glass, after it had been thus heated, it sparkled more than when fresh drawn from the fountain, and flew with more violence when shaken in a bottle.

We may also hence learn, that when bottles filled with the acidulæ are broken in hot, sultry weather, accompanied with thunder, as Hoffman and others have observed them, this accident must rather proceed from other causes, than from the expansion of their subtile mineral spirit, to which it is usually attributed. That flasks filled with the Pouhon waters, are liable to such accidents when unskilfully closed up, those who fill them for exportation, have learned from experience. To prevent which, they suffer the flasks, after they are filled, to remain several hours in a warm air before they cork them up. And he found, that one of those flasks being filled to the neck, at the Pouhon spring, when Fahrenheit's thermometer stood therein at 53 degrees, the water had arisen near $\frac{4}{5}$ of an inch in the slender neck of the flask, after it had stood, thus filled, for 3 hours, in a heat of 76 degrees; which rarefaction of the water, by the common heat of the air in summer, was more than sufficient to have broken the flask, had it been filled quite full with the water, and immediately closed at the fountain.

Exper. 2. Dr. B. took one of the long Frontiniac phials, which might contain about 21 oz. avoirdupois weight of Spa water, and which had stood 7 days in ex-

* Essay on the Contents and Virtues of Dunse Spa, Edinburgh, 1751, p. 99, &c.—Orig.

cessive hot weather, filled with Pouhon water, with a bladder fitted to it, as related in the foregoing experiments. The phial thus filled was placed in a copper vessel, so as to be immersed in water to $\frac{2}{3}$ of its height; and to the neck of the phial was fitted a kind of truncated cone of spiral wire, to keep the bladder in an erect position. The copper vessel was set over a slow fire, and the water therein heated to as great a degree as he could well endure with his hand; which he found to be about 110 degrees, according to Fahrenheit's scale. In this degree of heat, large bubbles of air soon began to arise to the top of the phial, forcing up with them small particles of water, which bedewed the sides of the bladder 2 inches or more in height. The phial being taken for a few moments out of the bath, the more exactly to view this kind of ebullition, very minute bubbles were observed to be formed, in the middle of the water, near the bottom of the phial; and from thence ascending with a rapid motion, and in continued streams, gradually to enlarge, until they flew off, with a boiling motion, and considerable force, from the surface of the water, which by its dilatation, arose from the bladder. The phial was kept in this moderate heat 2 hours, during which time the bladder became more and more distended with the air, or other elastic substance expelled from the water; which was observed to acquire a muddy whiteness in proportion as the elastic substance was expelled from it. The bubbles, after about 1$\frac{1}{4}$ hour, gradually lessened in number and size, and at the end of 2 hours almost disappeared. The heat was then gradually increased for another hour; at the end of which time the water in the copper vessel began to boil. And in this boiling heat the phial remained another hour; when it being judged that all the elastic substance contained in the water was expelled from it, the phial was removed from the fire, after it had continued in the water bath 4 hours. From the time that the heat of the bath was increased, the water in the phial grew more and more turbid; the earthy particles were formed into small masses, which were driven about by the heat. Towards the end of the operation, these earthy masses cohered into larger flocculi: and from white became of a yellowish colour. These flocculi grew larger as the water cooled, and slowly subsided to the bottom of the phial. While the water in the phial was yet warm, the elastic substance that had been expelled from it was tied up close in the bladder, and then removed from the phial. The water in the phial being corked up, was suffered to stand till perfectly cool. Being then examined, it was found to have got a taste from the bladder; but was quite vapid, having entirely lost its distinguishing brisk, sharp, ferruginous taste, as also its power of striking a purple colour with galls. The elastic substance contained in the bladder, when taken from the fire, appeared equal in bulk to $\frac{1}{4}$ a pint of water. It lessened considerably in the cool air, for some days after it had been thus extracted, but did not afterwards seem to diminish much in bulk, though kept for a month in the bladder.

Dr. B. had several times occasion to repeat the above operation: and found that all the elastic substance could scarcely be expelled from fresh Pouhon water, by the above degrees of heat, in a much shorter time than that employed in the foregoing experiment. For a common quart flask of this water, having been kept 3 hours in a water bath, heated to as great a degree as he could well endure with his hand, which might be about 130 degrees, according to Fahrenheit's scale, and having afterwards stood 24 hours before the bladder was removed from it; when opened, was found turbid, but had not deposited all its earthy particles, and still retained a little of its brisk taste. Another flask of the same water, fitted in like manner with a bladder, was kept 2 hours in a scalding heat, of about 160 or 170 degrees of the same thermometer, so that most of its elastic substance seemed forced from it; yet when the flask was opened the next day, the water struck a purple colour with galls, and had not quite lost its sharp subastringent taste; and after about $\frac{1}{3}$ part of this water was poured out, and the rest shaken briskly in the close flask, and then suddenly opened, an elastic substance was still discharged from it, with a considerable explosion.

From these experiments we learn, "that the Pouhon water contains a large quantity of a very subtle, light, and permanently elastic fluid, or of a true mineral air; and that this aërial fluid is closely united to the other principles of which this water is composed." For, from exper. 1, it appears, that when this water is excluded from all communication with common air, and at the same time liberty is given to the aërial fluid contained therein to expand, and to fly from it, with the same facility as from the water in an open vessel; yet under these circumstances, this elastic fluid does not exert its power of expansion, but remains so firmly united to the other principles of this water, that it does not separate from them when agitated, for several days, with a heat of 80 degrees of Fahrenheit's thermometer. While therefore this water is in its natural state, and is not acted on by any other body, the aërial principle remains quiet: and, with the other principles, seems equally dissolved in the watery element. It is not, therefore, then confined by any external force, like the air of beer, cyder, champaign, and other huffy liquors, which, while they are closed in bottles or other vessels, by their fermentatory motion, generate more air than they can imbibe and keep dissolved; so that much of the air so generated is pent up in a confined state, and continually presses on every side, until a vent is given it, and then it rushes out with violence. But it appears, that this subtle elastic fluid, while it is associated with the other principles of the Pouhon water, and is kept from contact of common air, and of such other bodies as are found to decompose this water, it remains in a quiet dissolved state, intimately mixed with the other principles of which this water is composed, and so closely joined to them, that it is not readily separated from them by a less heat than that of 100 degrees of Fahrenheit's thermometer; which heat it endures for several hours, before it is entirely detached from its union with them. It also appears, from the above experiment, that in proportion as this mineral air is separated by heat, in the same proportion the more gross earthy parts of the water seem also to separate from it; and that as the mineral air is thus entirely expelled, the water is then wholly decomposed, its metalline and earthy particles having subsided, its more volatile and elastic principles being exhaled, and nothing remaining in the water, save only the small portion of alkaline and neutral salt, which is found dissolved therein.

It may here be noted, that the numerous analyses of these waters, which have been attempted in retorts and receivers, where the water was exposed with a large surface to the common air, the elastic substance seems to have been extracted from the water more readily, and with less heat than in the preceding experiment; and pellicles have sometimes been observed on the water in the retort, as on the same water exposed to the open air; the causes of which phenomena will be explained hereafter.

Exper. 3. In order to ascertain the quantity of air contained in the Pouhon water, a Frontiniac phial was therewith filled at the fountain, on a clear morning, when the wind was easterly, and a strong swine's bladder, well freed from air, was immediately fitted thereto; all the air was then carefully expelled from this water by the heat of the bath, after the manner related in the foregoing experiment. The phial, with its contents, as soon as cold, was placed in an inverted position, over a cistern of common water, so that the air, which had been expelled from the water, ascended to the upper part of the phial, while an equal bulk of the water contained in the phial descended into the bladder. When all the air had ascended into the phial, the height at which the water stood therein was marked with a diamond. The bladder being then removed, the phial was carefully closed with a cork, and then taken from the cistern; and the air which it contained was kept therein, until it was wanted for the use which will be mentioned hereafter.

As soon as the phial was emptied of the air and water which it contained, and had been exactly weighed, it was filled a second time with the Pouhon water, which was found to weigh 20 oz. 7 dr. and 14 grs. apothecaries' weight. The phial was then emptied to the marks at which the water had stood therein, when in an inverted position; and the water remaining in the phial (which now filled the space that had before been occupied by the air extracted in the above-mentioned process) was found to weigh 8 oz. 2 dr. 50 grs. So that the bulk of the air extracted from the Pouhon water was to the bulk of the water from which it had been extracted, nearly as $8\frac{3}{8}$ to $20\frac{7}{8}$. Or, if we choose to reduce the above quantities into cubic inches, and allow a cubic inch of water to weigh 265 grains, we shall find, that in this process $15\frac{40}{265}$ cubic inches of air were extracted from $37\frac{229}{265}$ cubic inches of the Pouhon water. From this experiment it appears, that a very large quantity of air is contained in the Pouhon water: and a way is pointed out whereby the proportion of the bulk of air to that of a certain quantity of water, from which it had been extracted, may be exactly determined. Although this proportion must vary considerably in many waters, according to the weight and heat of the atmosphere, or the dryness or moisture of the season, at the time that they are taken from the fountain; and will also vary with the different degrees of expansion of the atmosphere, at the times that the different quantities of air are measured; Dr. B. does not, therefore, give the above proportions for such as always hold; neither does he assert that they most commonly take place. He rather suspects the contrary; and, from other trials, is apt to believe, that a larger proportion of air is usually contained in the Pouhon water, than that which he obtained in the above experiment. For, when a boiling heat was used, the necks of the bladders were apt to shrivel, and became liable to crack on the least motion; so that it was difficult to confine the air in them. Had he had an opportunity at Spa of prosecuting this experiment further, he should have attempted to have extracted all the air by a milder heat of about 110 degrees, in phials inverted in the water bath; so as that, while the air arose into the phials, an equal portion in bulk of the water might have been received into bladders fitted to them; or should have made use of other methods, which he shall have occasion hereafter to describe.

Exper. 4. He took a glass receiver, of a cylindric form, near $5\frac{1}{2}$ inches high, and $3\frac{2}{3}$ inches in diameter, which contained 25 oz. 150 grs. of water, Troy weight; or in measure $45\frac{225}{265}$ cubic inches, according to the above calculation of 265 grains of water to the cubic inch. This receiver being immersed in a cistern of water, was there turned, with its mouth downwards, and then raised, through a hole in a board fitted to the top of the cistern. The receiver was there firmly secured with wedges, in such manner as to continue full of water, the lower part of it remaining immersed in the water of the cistern. He then took a Frontiniac phial, which contained 19 oz. 5 drs. of water: this was filled with fresh pure air, by emptying it of the water in a cool open place, and then stopping it with a cork. The phial, thus filled with air, was placed in an erect position under the receiver; and the cork being taken out, the air ascended into the receiver, as it was forced out of the phial by the water rushing into it. Into this air a mouse was conveyed, by placing it on a round piece of cork, and plunging it quickly through the water into the receiver, in which it floated on the cork. In this air it lived for an hour in great ease, breathing with freedom, and diverting itself, while wet, with drying its face and head with its paws, like a cat. After having thus lived an hour in this close prison, it was taken out of it, through the water, by the same way that it had been conveyed in, and did not seem to have received any harm by this confinement, and by passing through the water; as after 24 hours, by the help of proper food, it was found as lively and active as it was before it had been thus treated.

A small bird, viz. a green wagtail, being treated in the same manner, continued for an hour in the receiver in the same quantity of fresh air. It breathed $\frac{3}{4}$ of that time with great ease, but, towards the end of the hour, quicker, and with some struggle. Soon after it was taken out, it grew lively, and again breathed with the same freedom as before the experiment. Our countryman Mayow asserts, that $\frac{1}{14}$ part of the air, in which these small animals are confined, is consumed by them before they expire. His experiments merit to be further verified. The experiments here related, which differ in several respects from those of Dr. Mayow, seemed necessary, in order to show that these small animals can live commodiously, for a considerable time, closely confined in the above-mentioned quantity of pure air; and that they suffer no harm in passing into and out of the receiver.

Dr. B. here remarks that by filling phials with dry sand, instead of water, here used, and emptying them in the Grotto di Cani, or other deadly caverns, those mephitic exhalations (of which so many of the ancients, as well as the moderns, have so much and so variously written) may be collected, and conveyed to a great distance; and, when duly examined, will, doubtless, be found true, permanent, mineral air, of that kind known to our miners by the name of choak damp. Of which sort the air of the mineral water of Spa appears also to be, from the following experiment.

Exper. 5. He took the air which, 2 days before, he had extracted from the Pouhon water, as related in exper. 3. This air he emptied out of the phial into the receiver fixed in the cistern, in the manner described in the foregoing experiment. Into the same receiver he also emptied a bladder of air, extracted from the Pouhon water 4 days before. Which 2 parcels of mineral air filled as much of the receiver as had before been filled with common air, in the last-mentioned experiment. Into this mineral air, thus included in the receiver, the mouse was conveyed, which had been employed on the preceding day in the foregoing experiment. On passing into this air, it immediately held up its head very high, and turned it on every side; and in 4 or 5 seconds, without any difficulty of breathing, or other struggle, fell down on one side, and remained without motion. Half a minute after, it was taken out of the receiver, and placed on a table before a window in the open air, where it lay four hours without showing any sign of life, being quite stiff and dead. Two other lively mice were successively treated in the same manner. The appearances in these were exactly the same as in the first. Both of them, after they had been in the mephitic air a few moments, fell down motionless, and were taken out dead. And though the last of the three was taken out of the receiver as soon as possible after it ceased to move, yet it never after showed the least sign of life. The same experiment was, some days after, tried in air fresh extracted from the Pouhon water, on 2 of the small birds mentioned in the foregoing experiment; in which air they, in like manner, also soon expired.

[Then follows an extract from the Essay, the title of which is immediately subjoined, which extract, it was not thought necessary to reprint in this Abridgment, the nature of subterraneous exhalations and the gaseous principles of mineral waters, being now so well understood and so fully explained in various treatises on chemistry.]

Extract from an Essay, intituled, On the Uses of a Knowledge of Mineral Exhalations when applied to Discover the Principles and Properties of Mineral Waters, the Nature of Burning Fountains, and of those Poisonous Lakes, which the Ancients called Averni; which was read before the Royal Society in April 1741. p. 236.

XXVII. Extract of a Letter from Benj. Gale, a Physician in New England, concerning the Successful Application of Salt to Wounds made by the Biting of Rattle Snakes; dated at Killingworth in Connecticut, August 20, 1764. p. 244.

I have been disappointed in procuring a rattle snake, to make experiments in expelling the poison, particularly the efficacy of sea salt; but have now the satisfaction to acquaint you, that having desired Mr. Porter, an eminent surgeon, to make inquiry, whether the efficacy of sea salt could be properly attested, he this day informs me, that a person was wounded by that serpent, about the beginning of this month, just above his shoe. The teeth of the snake, on examination by the probe, he found to have entered near half an inch. The person bitten immediately made a strong ligature above the wound, and in less than 2 hours came to Mr. Porter's. The leg and foot below the ligature were much swelled, and the patient grievously affected with a nausea. Mr. Porter made immediately a deep scarification, rubbed it well with salt, applied a dossil of lint moistened over the salt and scarification, and dismissed his patient, who the next morning returned. The ligature was continued, yet the tumefaction was greatly abated; the dressing before applied was renewed, and the person recovered without any further application. This perhaps, together with the former instance,* may serve to establish the truth of its efficacy."

* This was a person, under the care of Mr. Strong, a surgeon in New England, who in the year 1761, was bitten by a rattle-snake in the left foot, between the great toe and the next. He immediately perceived a sickness at the stomach, which continued some time. Scarifications were directly made, by cutting the skin, pulled up by an awl, formed into a hook for that purpose. The first ap-

XXVIII. Extracts of three Letters of Sir F. H. Eyles Stiles, F. R. S. concerning some new Microscopes made at Naples, by J. M. di Torre, and their Use in viewing the Smallest Objects. Dated Naples, 1761. p. 246.

The diameters of the glasses, sent to the R. S. and their magnifying powers, are as follow.

Glass.	Diameter.	Magnifying powers.
1st,	Near two Paris points,	640 times, and upwards, in diameter.
2d,	One Paris point,	1280
3d,	One Paris point,	1280
4th,	Half a Paris point,	2560

These glasses are so small, that the diameter of the highest magnifier among them is but half a Paris point, which is no more than $\frac{1}{144}$ of an inch, the point being $\frac{1}{6}$ of a line, and the line $\frac{1}{12}$ of an inch. The great difficulty with respect to these glasses, consists in the handling them, and getting them into the little brass sockets prepared for their reception. The making them in his method is soon done; but they must be socketed before he can examine to see whether the flame of his lamp, which produces them, has succeeded to the perfection he wished; that is, whether they be perfectly spherical, and without flaw, speck, or other accident, which they are so liable to, that he makes many before he produces one to his mind: and indeed he sets so little value on them, till he has proved them by the vision of some object, that if one of them, when he has made it, slips out of his little pincers before it has been proved, if he cannot recover it at once, he often makes a new one, rather than give himself the trouble to search for it. This high magnifier increases the object 2560 times in diameter; it is the first and only one he has ever succeeded to make of that high power; so that this present is a curious one.

An Account of some Microscopic Observations on the Human Blood.

The first observation was made July 2d, 1761, with Wilson's single microscope, constructed for a perpendicular inspection, with a mirror beneath it for reflecting the light. The instrument was placed on a table near a south window, but the sun's rays were not reflected on the object; it being Father di Torre's opinion, that the ordinary day-light would show the globules in a more natural

plication was fine sea salt, which was plentifully sprinkled and rubbed in and about the wound and scarification. These were done in the space of about 2 minutes after the wound was made. Then a poultice made of burdock-root pounded, and mixed with a large portion of sea salt, was applied to the wound, and another of blood-root was bound about his leg a little below the knee. In the mean time, the patient took inwardly saffron and water, in which was steeped the bark of white ash, which caused him to vomit. The consequence of the wound was a tumefaction, which was greatest in the foot, but extended to the knee, where it ended. After these applications, nothing remarkable was observed in the wound. They were continued for 2 days, and the patient perfectly cured. Mr. Strong supposed the salt to be the principal ingredient, which effected the cure.—Orig.

state. A small drop of blood was included between two talks, and was changed during the observation, in order to give the appearance all the advantage that might arise from the accidental situation of the globules between the talks, and also to renew their motion; for though the blood viewed in this manner is not in a state of circulation, yet, either from the vacuum formed between the talks, the attraction of their surfaces, or some other cause, the serum is seldom in a quiet state on its being first included between them, and the globules that float in it move for some time in various directions as the current of the serum inclines them, or as they themselves are more or less attracted by each other or by the talks, till the whole has found a state of rest. The magnifier applied was a spherical glass, which he computed to magnify the object 512 times in diameter. In these views, the globules, though they varied a little from each other in their outlines and dimensions, appeared in general to be circular or elliptic, and of the size of swan-shot. Their figure as solids was not easily to be determined in such an exposition of them, but they had the appearance of oblate spheroids much compressed, though, from their free motion with the serum, it was manifest they suffered no pressure from the talks; where the focus was perfect, or nearly so, the middle part of each globule was darker than the margin; and this difference in shade gave them an appearance, as if a dent or concave impression had been made on their surfaces, which resembled those of young peas that have dimpled in the boiling. What this darkness next the centre was owing to, will appear from the 2d observation; but in this I could not judge it to arise from any thing but a sinking in of the surface in that part, or some accident of the light that furnished such an appearance.

July 9th, a 2d observation was made with the same instrument, to which was applied a sphere which magnified the object 1280 times in diameter. In this view, the variation of shade on the middle part of each globule from that of the margin, was such as carried with it strongly the appearance of a perforation. I had indeed my doubts of this on first examining them, because, on a very slight alteration of the delicate focus of so high a magnifier, the light and dark parts would interchange, so that the refraction or reflection of the light might still be suspected to occasion the appearance in question: but continuing my view stedfastly, and taking notice of all the diversities which a numerous collection of globules presented, I remained at last thoroughly convinced that they were perforated; for, when any of the globules happened to move with the serum in the most perfect focus, which could not happen to all, in a medium of some little depth, I could with great clearness distinguish the exterior and interior circumference of the ring, of which each globule consisted; the interior one being bounded by a black line or shade next the perforation, exactly resembling that which bounded the exterior one, and distinguished it from the serum without. In such globules I could also easily observe the ring to be articulated, the transverse lines at the joints being very distinguishable. The figures of the articulations were various; in some they were roundish, so that the ring appeared like a bead necklace; in others cylindrical, and of some length. The number of which the whole was composed, seemed uncertain, varying from 2 or 3 to 6 or 7; many of the rings were broken, either by some confinement of the talks, or by beating against each other, which I saw them continually doing; and by these accidents the joints of the rings were detached, and wandered about separately in great numbers; and indeed they appeared separable with as much ease as if they had been united by mere contact only. Some of the rings were broken into semicircles, others into greater or less portions, and others again divided into their constituent articulations, which in some places floated about single, and in others formed by their mutual attraction a lateral union, like the pipes of an organ. These separated parts seemed to be hollow and transparent, and, like inflated bladders, would easily yield, and change their figure, stretching or contracting themselves from round to oval and cylindrical, and vice versa, as any lateral pressure in crowding along with the serum brought a constraint upon them. As they floated at different levels, many of them passed over or under each other without interruption, and the same would happen also to the whole rings and larger portions. I remained therefore, after repeated examinations of the globules in this state, without the least doubt either of their perforation or articulation; for though the articulation was not distinguishable in every globule, it was so in the greater part of them; and it is natural to imagine that the rest were articulated likewise, though they might not pass at the proper distance for its being distinguished. I omitted to speak of the size of the globules in this observation, nor indeed can I, from so various an appearance, form any judgment of it, further than to say that they appeared in general much augmented beyond their appearance on the 2d of July.

August 27th, a 3d observation was made with the same instrument of some blood dropped upon a single talk, and viewed as it lay without any cover, so that there could be no compression. It was viewed while the globules were still in motion, with a spherule that magnified the diameter 1920 times; and in this view the globules appeared so clearly to be hollow rings, that there was not room for the least suspicion of the reality of the fact from any circumstance. The diameter of the perforation appeared much larger in proportion to the thickness of the circumference, than it had done in the former views. The figure of the rings, where they were free, and in their natural state, was circular; but where they were so crowded together as to compress one another in their passage, they assumed a variety of different figures, though they generally restored themselves to a circular figure again, unless broken by the compression, which frequently happened, and then the broken parts floated separately; or, if they opened at a single joint only, the whole of the ring would float along, varying its figure occasionally from that of a portion of a circle, which it would first assume, to a straight line, an undulated one, or some other accidental incurvature. The articulation was visible in several of the perfect rings, but for the most part it was not to be distinguished, though even in these, from their breaking so easily, it was not to be doubted but that they consisted of the same detached members or joints as those in which the transverse divisions were visible.

On applying afterwards magnifiers of less power to the same blood, the greater advantage of light made the rings appear still more perfect and distinct; but as these were not applied, till the globules had lost their motion, and the whole drop had become dry upon the talk, the divisions at the joints were none of them visible; almost all the globules or rings, had, on drying, formed themselves into perfect circles. The most complete and satisfactory view of them in this dried state, was with a magnifier that increased the diameter 640 times; though the perforation was distinguishable even with one which increased it only 129 times. In many places the globules had, by the drying of the whole drop united into a closer body, and seemed as if cemented together by a grumous substance of a blackish or deep red colour, which possessed and filled up all the exterior spaces formed by the union of so many circular bodies; but the interior spaces or perforations of the rings were still free, and for the most part distinctly visible, some few places excepted, where the globules seemed to have united over one another, and not only lay in too much confusion to give room for any proper observation of them, but formed also a body too dense to transmit the light. The grumous substance above-mentioned, as it extended itself along the exterior spaces, had the appearance of a ramification; and it was perhaps in some such state that the globules were viewed by Dr. Adams, whose glasses induced him to suspect the truth of the common opinion, that the blood consisted of globular particles, and to describe them rather as imitating the branches of a tree.

On the Sexes of Plants. By the same. p. 258.

Mr. E. S. supposes, that not only in the Diœcious plants, but in the Monœcious and Polygamious also, and, to speak more generally, in all cases where the male and female organs are found separate, the defect is not in the flower, which he supposed to be originally constructed with the rudiments of the organs of both sexes, but that it arises from some circumstance in the plant that determines it to blow the one organ and not the other. That the absence of the rudiments is not to be inferred from the want of their expansion, appears plainly from the following circumstances that fall under every one's observation, viz.

That plants do not produce their flowers all the year, but only at particular seasons. That many plants are some years before they produce their flowers, and hardly any, except annuals, blow the first year after they are sown. That soil, climate, pruning, and many other circumstances, will bring plants to blow sooner or later than they would otherwise do. That culture will increase the quantity of bloom, and so occasion the expansion of flowers, which would otherwise have remained within the wood.

Now if these circumstances, which are similar to those of which the explanation is sought, be so common, why may we not in like manner suppose, "that, whenever either the male or female organs are absent, it is owing to some circumstance that determines the sap into other channels, and so prevents the expansion of the part." This will perhaps be thought to amount to more than a conjecture; because, besides its probability from the circumstances stated above, it will perfectly explain another well attested phenomenon in the class Diœcia, that is scarcely to be accounted for on any other supposition, viz. that a male plant has, at a certain age, been found to change to a female one, and vice versa, and also to bear flowers of both sexes, to which he may add another which he had observed in the Monœcious plants, Zea and Ricinus, where he often found spikes of fruit breaking out among the male flowers, though they commonly come out separate from them in another part of the plant. If there be any weight in these arguments, the general conclusion will be this, that the flowers of all vegetables whatever are hermaphrodite in their original construction, though both the organs do not appear in all cases.

Remarks on the Impregnation of Vegetables. By the same. p. 261.

Each grain of pollen is a vessel filled with pulpy matter, in which are lodged a considerable number of smaller grains, which may be called the impregnating corpuscles; see pl. 6, fig. 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13. These are not visible with the naked eye, but may be distinguished with glasses of moderate power, if the grain be transparent, or if the pulp be forced out by compressing the grain between the talks. They are round, transparent, and nearly of the same size in all plants. They are conveyed to the germen through the style, which is furnished with internal ducts for that purpose; and in the class syngenesia, and in the small plants of other classes, where the style is slender and transparent, they may be distinguished in their passage, as in fig. 19. The manner of their reception into the style depends on the disposition of its surface: our observation fell chiefly on those plants that have hairy styles or stigmas; and in these the corpuscles enter by means of the hairs, which are often found on the style itself, so that the stigma must not always be considered as the only recipient part, though it may perhaps be so in most instances. The hairs are so many tubes open at the extremity for the reception of the corpuscles; they are usually shaped like a thorn, or prickle, widening towards the base. They are each of them furnished with a canal, or tube, which divides itself at the broader part of the hair, and enters the pistillum in two branches (fig. 17) which run on till they join the longitudinal ducts that lead to the germen (fig. 21.) These canals, after they enter the pistillum, are less regular, branching out frequently into smaller ones, which, instead of running directly to the longitudinal ducts, vary their direction, and fall into the canals that run from the hairs next adjoining, furnishing the appearance of an irregular reticulation (fig. 22,) though nevertheless there are commonly principal canals observable that run more directly towards the longitudinal ducts, and fall into them (fig. 22.) The corpuscles are admitted into the hairs in the following manner; the grains of the pollen having dispersed themselves about the style and stigma, great numbers of them find a lodgement among the hairs; those which fall between the hairs, or cling to their sides, may be supposed to lose their effect; which will not be thought improbable, if it be considered what an abundant provision there is of the pollen, and how large a part of it must necessarily be wasted by being carried away by the flower, or at least not falling on the female organs, but there are many of the grains that fall on the points of the hairs (fig. 23,) and these furnish the impregnation. The grains being arrived at a state of maturity before they issued from the antheræ, are prepared to burst and discharge their contents when they fall on the hairs, and the female organ assists likewise in producing this effect; for soon after a grain has lodged itself, the point of the hair begins to open, and the mouth extends itself by degrees over the surface of the grain, till almost the whole body of the grain is drawn within the tube (fig. 23;) in this situation, the grain soon yields to the compression of the tube, and discharges its corpuscles, which, with the assistance of the fluid parts of the pulp that enter with them, or of the juices with which the tube itself is furnished, float on till they enter the longitudinal ducts which convey them to the germen. The grains, after thus emptying themselves of their contents, wither and contract, and, falling off from the mouth of the tube, remain in a perished state about the sides of the pistillum (fig. 19.) The figure of the hair, while the grain is lodged in the mouth of the tube, is remarkable; for the tube is then widest at the extremity, and lessens gradually as far as the bifurcation, where it forms a narrow neck, which gives a bell-shaped figure to the superior parts, while the lower part widens again towards the base (fig. 23.) In transparent styles, the ducts that lead to the germen may be seen filled with corpuscles, which, being supplied in great quantities from the hairs, pass on through these ducts in regular lines so close as to touch one another (fig. 19.) In some inspections, the corpuscles were seen to move both in the hairs and in the principal ducts of the style; which showed them to be detached substances, that could pass freely with the current of the juices in which they floated; but their regular progress towards the germen was doubtless interrupted by the gathering of the flower; so that the motion observed could only be ascribed to accidental attractions, which put the juices in motion between the talks; and this was evident also from the direction of their motion, which was casual, and not always leading towards the germen. The number of the principal ducts that lead to the germen cannot be ascertained; they probably vary according to the number of loculaments to be supplied; more than one was commonly observable with the corpuscles passing in close files through them, as has already been described. In the pistilla of flowers in bud, no corpuscles could be discovered; which is a strong proof that they are received from the pollen, and destined for the impregnation.

On examining the pappus or down that crowns the seeds in the class syngenesia, the hairs of the pappus were found to be hollow, and filled with the same corpuscles (fig. 24). How the corpuscles are admitted into them, or for what purpose they are lodged there, must be left to further inquiry; in the mean time it may be observed, that the situation of the pappus makes it improbable that the corpuscles should be received for the purpose of conveying them to the germen; and that therefore it is more natural to suppose, that the corpuscles arrive there after their passage through the germen, and that the hairs of the pappus serve as excretory vessels for taking off those that were useless to the impregnation. This is the more probable, as the great quantity of them brought by the ducts must doubtless occasion such a superfluity.

On examining various plants of the order filices, of the class cryptogamia, no male organs could be discovered. If the flowers of these plants be hermaphroditic, the staminiferous part doubtless falls off as soon as the impregnation is over, as it does in other cases; so that if the male organs are not sought for at the precise time when the plant is in bloom, the search must be a vain one. The fructification in these plants is for the most part covered with a thin membrane, which Mr. Miles calls a sort of fungus or tubercle (Phil. Trans., Abridg. vol. viii, p. 505) and which, at its first appearance, and for some time afterwards, seems to have its margin closely adhering to the leaf. If the antheræ lie under this cover, it is probable that the flowers do not blow till the margin has detached itself from the leaf, and admits the air to come under it, for the maturation and dispersion of the pollen. This may perhaps point out the critical time for searching for the antheræ. However this may be, the antheræ and pollen are probably very minute; and as it is no easy task to make the examination of what is concealed under these membranes with a single microscope, to which the glasess we have used are commonly applied, we have not yet found the means of discovering them. The seed vessels and seeds have been already well described and figured by Mr. Miles; however, as some delineations were made of them as they appeared to us, they will accompany the other drawings (figures 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 35, 36,) and may serve to confirm Mr. Miles's account of them.

In the male flowers of Marchantia polymorpha, Linn. the grains of pollen were observed in a thick tuft of hairs, where they seemed rather to have lodged themselves accidentally after their dispersion, than to be affixed to them as they are described by Linnæus, (farina crinulo affixa. Gen. Plant.) These hairs, viewed with the microscope, had a remarkable appearance, each hair consisting of a double chain, and each chain being composed of round bodies, placed at regular distances from each other, and connected by a thread. The two chains are so close together, that the bodies touch each other in pairs, the whole making a regular figure. After compressing one of these hairs between the talks, the 2 chains seemed to have twisted round each other, and to exhibit an appearance somewhat resembling the cable of a ship. Of the use of these hairs in the male flowers, we could form no conjectures. The figures of them will be found among the drawings (figures 37, 38).

Explanation of the Figures in Plate 6.

The generic and specific names here used are the Linnæan. The numbers following the description, show how many times the object was magnified in diameter.

Fig. 1, A grain of the pollen of Hibiscus Syriacus: magnified 512 times.—Fig. 2, The same, with the impregnating corpuscles forced out by compressing the talks: magnified 512 times.—Fig. 3, Some of the same corpuscles separate from the grain, and more extended in length, which was supposed to arise from their adhesion to each other, though the division was not perceptible: magnified 512 times.—Fig. 4, Grains of the pollen of Mirabilis Ialapa, with the impregnating corpuscles seen within them: magnified 72 times.—Fig. 5, A grain of the same: magnified 512 times.—Fig. 6, 3 grains of the same sticking together: magnified 1280 times.—Fig. 7, Corpuscles of the same, viewed separate: magnified 1280 times.—Fig. 8, A grain of the pollen of Cucumis sativus, with the corpuscles within it, and some on the outside, that had been forced out: magnified 192 times.—Fig. 9, Grains of the pollen of Bignonia radicans, with the corpuscles within them.—Fig. 10, A grain of the same: magnified 1280 times.—Fig. 11, Corpuscles of the same, viewed separate: magnified 1280 times.—Fig. 12, A grain of the pollen of Gomphræna globosa, compressed: magnified 1280 times.—Fig. 13, Grains of the pollen of Bryum, with the corpuscles within them: magnified 1280 times.

The same corpuscles were seen in the pollen of Atropa, Hyoscyamus, Scilla, Scabiosa, Valeriana, Verbascum, and a great number of other genera; but the delineations were omitted, as the appearances were nearly alike in all.

Fig. 14, The style and stigma of Leontodon Taraxacum: magnified 72 times. —Fig. 15, Two hairs of the same, in which the perforation was distinguishable: magnified 1280 times.—Fig. 16, The stigma and part of the style of Carduus crispus: magnified 72 times.—Fig. 17, Two of the longer hairs of the same, in which the canals or tubes, with their bifurcation, are shown, and the corpuscles passing through them: magnified 1280 times.—Fig. 18, The stigma and part of the style of Conyza squamosa: magnified 72 times.—Fig. 19, A portion of the style of the same, with the corpuscles passing through its longitudinal ducts, and the emptied grains of the pollen adhering to its sides, after having dropped from the hairs of the stigma: magnified 859 times.—Fig. 20, A perfect grain of the pollen of the same: magnified 859 times.—Fig. 21, Part of the side of a style of Solanum officinarum, with its hairs, and the continuation of their canals through the body of the style: magnified 128 times.—Fig. 22, Part of the same, in which the transverse communication between the canals is shown: magnified 384 times.—Fig. 23, A hair of the same, with a grain of pollen lodged within the extremity of the tube: magnified 1280 times.—Fig. 24, Part of a hair of the pappus that adheres to the rudiments of the seeds in Sonchus oleraceus, with the corpuscles within them: magnified 1280 times.—Fig. 25, A leaf of Asplenium ruta muraria, with its seed vessels, which in this plant have no membrane that covers them.—Fig. 26, The seed vessels of the same, with their elastic rings: magnified 42 times.—Fig. 27, A seed vessel of the same, broken by compression, with the seeds falling out: magnified 128 times.—Fig. 28, Seeds of the same: magnified 859 times.—Fig. 29, A seed vessel of the same, in which the parallel streaks on its exterior surface are shown: magnified 128 times.—Fig. 30, A small portion of the capsular part of the seed vessels of the same, on the surface of which there appeared smaller streaks, which subdivided the parallel ones shown in fig. 31: magnified 859 times.—Fig. 31, A portion of the ring of a seed vessel of the same: magnified 512 times.—Fig. 32, A smaller portion of the same ring, in which was observed a plane side at the under part, where it adhered to the capsule; on examining the broken part at the end, the ring appeared to be solid.—Fig. 33, A membrane that covered the fructification of Polypodium filix was separated from the leaf, and shown as it appeared on viewing its under side with some of the seed vessels adhering to it; (a) is the broken part where it had been joined to the leaf.—Fig. 34, Some of the seed vessels of the same viewed separate: magnified 48 times.—Fig. 35, The peduncle of a seed vessel of the same supporting the ring: magnified 1280 times.—Fig. 36, Part of a leaf of Adiantum capillus veneris, showing the membrane which grows from its margin, and which folds over it to cover the fructification, but had been un-folded for observing the under side and seed vessels, which are here shown.—Fig. 37, Part of one of the hairs of the male flowers of Marchantia polymorpha, with some of the pollen: magnified 1280 times.—Fig. 38, The same compressed: magnified 1280 times.

XXIX. Sequel of the Case of Anne James, who had taken the Green Hemlock. By Mr. Josiah Colebrooke, F.R.S.* p. 271.

From this communication it appears, that the use of the green hemlock, though attended with advantageous circumstances for near 2 years, proved to be merely palliative. For in July, 1764, Mr. C. was informed, that the schirrus in the right breast (which was very small when she began to take hemlock) was increased, and extended itself under the axilla, looked vivid, oosed a little, and was painful. He desired a little blood might be taken away, that she might take rhubarb or magnesia to go through her, and then pursue the same regimen with the hemlock, she had done before. For external application, Mr. C. desired 1 oz. of common lead might be scraped as thin as possible, and infused in 2 oz. of salad oil, to be shaken daily for 2 or 3 days; when settled, to moisten a linen rag with the oil, and apply it to the part; to renew the dressing every day, and to shake the bottle every time after they had used it, that the oil might be the better impregnated with the drying quality of the lead. That, if the pain increased, they might use the following cataplasm, viz. green hemlock 1 oz., boiled in a small quantity of milk till tender, and then thickened with linseed meal, or oatmeal, and a little oil, and applied barely warm, over the other dressing, once or twice in a day.

In Sept. 1764, he saw her: she complained that her pain was rather increased; and on examining her breasts, he found the left, of which she first complained, quite easy: in the right breast the nipple was drawn in, and the schirrus of a purple colour, but the discharge was very small. He advised her to persist in taking the hemlock, and pursuing the former regimen. In May, 1765, he was informed that her pains were so much increased, that she was obliged to have recourse to opiates; her breast discharged a great quantity of fœtid matter, as is usual in cancers; and she ended a miserable life in September.

XXX. Of the Effects of a Storm of Thunder and Lightning in Pembroke Coll., Oxford, on June 3, 1765. By Mr. Griffith. p. 273.

In the afternoon of June 3, a very black cloud appeared in the wind, which was nearly N. E. A little before 4, a gentle rain, attended with slight thunder, fell, just sufficient to wet the surface of the streets, &c. But the wind blowing

* See p. 37 of this volume of these Abridgments.

from the northward, the wall of the college fronting the south remained perfectly dry. About half past 4 came a terrible flash of lightning, attended at the same instant by a violent clap of thunder. The lightning was of a remarkably red colour, and at the instant of the flash, every body, for a considerable distance round the spot where the damage was done, felt and complained of an intense heat; several people were either forcibly beaten down, or fell through fear and surprise. Some thought themselves in the middle of fire. The lightning entered into the south side of Pembroke college in 4 different places at the same instant. A chimney which fronted the s. was beaten down, and looked exactly as if it had been cut off in the shaft, about 12 feet from the top. In the garret to which the chimney belonged, there is a lath-and-plaster wall running on each side of the fire-place, for the convenience of the room, supporting a kind of dormer roof. At the end of this wall was a strong oak post, which was full of nails. This post, roof, &c. was thrown into the room, to a considerable distance, and shattered to pieces; and the window fronting the quadrangle to the N. was blown outwards. It was at first apprehended that this was the only part of the college which was struck, and that the mischief done in the other rooms was only the effects of the same ball, conducted from this garret to the other parts. But this is very doubtful: for had that been the case, it should seem that the direction of the electrical matter must have been altered, whereas in every room its course was from S. W. to N. E. The lightning entered a room below at a window on the W. side of the fire-place; the casement (an iron one) was open, and was little or not at all damaged. The window-curtain, with the frame it hung upon, was thrown at least 20 feet to the opposite corner of the room; the window-seat, and all the wainscot about it, were shattered to pieces, and carried away in the same direction with the curtain. The door of the bedchamber, near the window, was extremely scorched, and at the distance of a few feet was a beaufet which was likewise much scorched, and the brass escutcheons were all forced off. There was in this beaufet some valuable china, and a quantity of glasses, which suffered much. Some of the china had Mr. Collins's arms fixed on it, and was gilt round the edges; two cups of this kind had each two little triangular notches cut in their rims, the gilding in those parts being defective. A number of china plates, glasses, &c. were broken. On the lowest shelf was a quart drinking glass, which had long stood there, inverted. It was probably in some degree fastened to the shelf by the paint. This glass was almost reduced to dust, a great deal of which was found on the uppermost shelf of all. This was probably owing to the sudden expansion of the air within; and to the same cause it must be attributed, that the tops of the canisters were taken off. The tea-spoons were found discoloured and black; but Mr. Collins recollected, that a small drop of mercury from a broken barometer was left in the beaufet, which no doubt discoloured the silver. The window fronting the quadrangle to the N. had every pane of glass in it forced outwards, and broken to pieces; the casement, which was open, escaped unhurt. The lead belonging to each pane was bent outwards exactly in the middle, but there were no signs of fire here; and indeed it is probable that the lightning reached no farther than the beaufet. A young gentleman, who stood in his window, was almost blown down by this sudden gust of wind. A painter was at work in this room when the accident happened. He was at the window, on the left side of the fire-place from that which the lightning came in at. His account is, that he felt an intense heat; saw, as he thought, fire running all round him in circles; that he had a stroke on the shoulder, which beat him down, and made him senseless for some time; when he recovered, the room was full of smoke, and smelt strongly of brimstone. Near the window, where the lightning entered this room, it made a round hole through the floor into the ground room, inhabited by Mr. Williams, a young gentleman of this college, who had gone out but a very little time before. Near one corner of the iron window frame, a round hole, about an inch in diameter, was struck through the stone work, as if made with a bullet. A strong iron bar in the window was forced into the room, and carried to some distance. The hinges of the window shutters, and the wall they touched, were discoloured, just as if gunpowder had been fired on them. A nail happening to be in the stone, on the side of the chimney, the lightning drove it with great force into the solid free-stone, making a round hole to a considerable depth. The window-curtain here was thrown to the same distance, and in the same direction, as in the room above; and pretty nearly the same effects appeared; only the wainscot and window-seat were not shivered into small pieces, as in the room above, but were thrown in large splinters, and with great force, about the room; some of them broke the window and a large looking-glass on the opposite side, and more than one flew endwise like an arrow, with such force as to pierce through a very strong lath-and-plaster wall, the ends of them appearing several inches through the wall in the adjoining stair-case. Close to the window where the lightning entered, was a strong piece of oak timber, being the corner of the partition to a closet. This post, 9 feet long, and 6 by $6\frac{1}{4}$ inches in the clear, was thrown in a different direction from any thing else, from E. to W. into the closet. It was carried near 8 feet, and then struck a clothes press with such force, as to do it very considerable damage. At the bottom of this, a hole was made through the floor into the ground; the window of this room was not blown out. For some time, it was supposed that these 3 rooms only had suffered; but, going to view the ruins on the outside, there were observed some traces of mischief in the roof of the garret opposite to that first mentioned, and near 40 feet W. of the chimney which was beaten down. And though all had been over for more than an hour, the stench of sulphur remained so strong in this garret, as almost to endanger suffocation. This garret had been for some time used as a lumber-room by the bursar, and, within a few feet of the place where the lightning entered, lay a heap of old iron casements; it came in here with an amazing force, and shivered the side beam of the roof into ten thousand little splinters, scarcely larger than common needles. As there were many boards, shavings, &c. in the room, it is more than probable something might have taken fire, but most fortunately under a large bow window on the s. side of the room were laid carelessly a number of long iron window bars, almost from one end of the window to the other. These saved the room from further damage; the electrical matter was by them conducted to the corner of the window, and there made a large round hole, and went out of the room to an iron cramp on the outside, about 11 or 12 feet long, at the lower end of which a stone was cut out of the wall by the lightning, and thence it no doubt descended to the street, then quite wet, without further damage.

XXXI. On the Nature and Formation of Sponges. By John Ellis, Esq., F. R. S. p. 280.

Among those animals commonly called zoophytes, we may plainly discover an evident approximation, from the rudest irregularly formed sponge, which is the lowest being yet observed to have the appearance of animal life, to the most beautiful and elegant red coral. The nature and formation of sponges having never yet been thoroughly investigated, every attempt to explain this dark part of nature must give satisfaction to the curious. The intent then of this letter is to convey to the R. S. what we have seen in the experiments made on them at the sea-side; the substance of what has been said on the subject by moderns as well as ancients; and lastly, to show how nearly they approach to the alcyoniums, a class of beings next above them in the scale of nature, as being one step nearer to the appearance of animal.

If we consult the ancients we shall find, that in the days of Aristotle, the persons who made it their business to collect these substances, perceived a particular sensation, like shrinking, when they tore them off the rocks; and in the time of Pliny the same opinion continued of their having a kind of feeling or animal life in them; but after his time no attention was paid to this kind of knowledge, and it still remained a doubt, till Count Marsigli pronounced them vegetable, as he did all the corals, keratophytons, and alcyoniums, &c. After him, it fell to the lot of Dr. Peyssonel, in his inquiries, to discover them to be animals, or rather, as he calls it, the fabric of animals, formed by a species of urtica marina, (see his manuscript which he sent to the R. S. in the year 1752); but finding on re-examining these intricate bodies in sea water at Guadaloupe, he favours the R. S. with a letter dated from thence, March 1, 1757. Vid. Phil. Trans. vol. 50, p. 592,* where he has given a particular account of the animal which he assures us forms the sponges. There is something so remarkable in his description of the animal, and its manner of fabricating the sponge, that it is necessary to quote the most striking parts, in order to submit the probability of it to the R. S.

He takes notice, "that the same kind of animal forms the 4 principal species of sponges described by Father Plumier, as the tube sponge, the cord-like sponge, the digitated sponge, and the honeycomb sponge. These sponges, he says, consist of hard firm fibres, twisted about in doubles, and the interstices filled with a mucilaginous gluey matter, having large hollows, with cylindrical tubes, dispersed through their substance, forming a kind of labyrinth filled with these worms." He says, he has observed, "that the sponges begin to be formed on a nodule of petrified sand or other like matter, round which the worms begin to work, and round which they retire as to their last seat of refuge." He then proceeds to give a description of them, which is, "that they are $\frac{1}{4}$ of a line thick, 2 or 3 lines long, of a conic figure, with a small black head furnished with 2 pincers; the other extremity is square, and much larger than the head; their motion begins at the tail, and ends at the head; they are so transparent, that the circulation of the blood may be perceived; and where the viscera should be, there is a kind of circular motion of a blackish matter moving to and fro in the animal. He says, he has kept them alive more than an hour out of the sponge, and, which is very singular, when he put them near a piece of fresh sponge, where the nests were moist, and from which he had before pulled them, he saw them enter and disappear. He goes on to tell us, that these worms have no particular lodge; that they walk indifferently into the tubular labyrinth; so that, he says, without offence to Pliny and other nauralists, he does not see that it is in their power to dilate and contract the bodies of sponges, which always remain in the same state of magnitude, without being sensible to the touch, being an inanimate body, all the sensitive life belonging to the worms. He then tells us, that with the slaver or juice they deposit, they make the sponge increase or grow, as bees and wasps, and especially the woodlice of America, increase their nests and cells."

This account appearing so contrary to the proceeding of nature in the formation of the other kindred marine bodies, called zoophytes, such as corals, keratophytons, and alcyoniums, particularly the last; Mr. E. was determined to find out the truth of this extraordinary discovery, which he found had been thought worthy of a place in the Philos. Trans. Accordingly in the year 1762, when at

* Page 227, vol. xi, of these Abridgments.

the sea side at Brighthelmstone, he dissected carefully the spongia medullam panis referens, or crumb of bread sponge, in hopes of discovering the small animal that was supposed to fabricate them; and was surprised to find a great number of small worms in them, particularly a very small kind of nereis, or sea-scolopendra; but these worms appeared evidently, instead of being the fabricators of it, to have pierced their way into its soft substance, and made it only their place of retreat and security. After this, he proceeded along the sea coast to Little Hampton, near Arundel, on the coast of Sussex, where he took up out of the sea several specimens of the same sort of sponge full of an orange-coloured gelatinous matter; and, while they were just fresh from the sea, examined them, after they had rested for some time, in glasses of sea water; and to his great surprise, instead of seeing any of the polype-like suckers, or any minute animal figure, come out of the papillæ, or small holes with which they are surrounded, he only observed these holes to contract and dilate themselves. And as a further confirmation of this motion, being at Hastings in Sussex, in August 1764, he collected from the rocks at ebb-tide, just under water, a variety of the same kind of sponge, but of a pale yellow colour, and in the form of several cock's combs united together, the tops of which were full of tubular cavities, or papillæ; when he examined these in glasses of sea water, he could plainly observe these little tubes to receive and pass the water to and fro; so that the sponge is an animal sui generis, whose mouths are so many holes or ends of branched tubes opening on its surface; with these it receives its nourishment, and by these it discharges, like the polypes, its excrements.

But to give a further proof of sponges sucking in and throwing out the sea water, he quotes a passage from that fair investigator of nature, the celebrated Count Marsigli, in his Histoire Physique de la Mer, p. 53, who, notwithstanding he took them for plants, as well as he did corals, &c. has in his chapter of sponges this curious observation, which proves quite the contrary. "J'ai un fond suffisant de ces plantes pour en faire une botanique entiere, et plusieurs reflexions curieuses sur la systole et diastole, que j'ai observées, dans certains petits trous ronds de ces plantes, lors qu'elles sortent de la mer, mouvement qui dure jusqu'à ce que l'eau soit entierement consumée." In English thus: "I have a sufficient stock of these plants (sponges) to make a complete botanical collection, with many curious remarks, which I have made on the systole and diastole, which I have observed in certain small round holes, when they are first taken out of the sea; this motion continues in them till the water they contain is entirely wasted away." Nothing can more clearly describe what Mr. E. saw in his sponges; so that, making an allowance for the then prevailing opinion that they were vegetables, he thinks the Count comes nearer the truth than Dr. Peyssonel's account of the formation of sponges by little animals, that walk to and fro in the labyrinth of the tubes to construct his extraordinary animal fabric.

In order to explain how near they approach to the alcyoniums in their internal form and manner of growth, Mr. E. has here given the perpendicular and horizontal sections of the common officinal sponge; because this is in the power of most gentlemen to examine. And at p. 44 of these Abridgments, he has given the perpendicular and horizontal sections of the alcyonium manus marina, both magnified and in the natural size; because specimens of this kind are likewise easily obtained, being found in plenty on rocks and shells near the isle of Sheppey, at the entrance of the river Thames. It is observed that the connected tubes of both arise from the part to which they adhere to the rocks, &c. Hence both kinds branch out and swell into irregular lobes; with this difference, that the surface of the sponge is covered with holes guarded with minute points like little spines; the surface of the alcyonium with starry openings of 8 rays, whence the polype-like suckers are protruded, to find out proper nourishment: and these starry openings in one, and the holes in the other, so far correspond, that in both kinds they are found of different sizes; but this is in proportion to the age of the branching tubes that come to the surface.

In the sections of the alcyonium may plainly be distinguished the reticulated elastic fibres, that enclose the transparent stiff gelatinous part, as in the sponges; but as this gluey substance is of a firmer texture than what is found in sponges, it requires more pains to separate it from the elastic fibres; however, with a little trouble it may be done sufficiently to evince what he has endeavoured to prove, viz. the great proximity there is between the animal life of sponges and alcyoniums, and consequently that both are animals.

Before concluding, he endeavours to remove some doubts, which seem to have istressed the generality of curious persons to account for; the one is, what occasions those very large holes that appear here and there irregularly on the surface of most sponges? the other is, how came those extraneous bodies, such as small shells, stones, and even parts of fucuses, in the middle of these animal bodies? in answer to the first, on cutting open and examining these bodies while recent in sea water, as before shown, we frequently find a variety of different worms, who bore their way into them, and make their nests and retreats there, or perhaps to live on the gelatinous part of the sponge. This the celebrated Donati confirms, in his History of the Adriatic sea; who endeavouring to find out, like Peyssonel, the animal fabricator of the alcyonium primum Dioscoridis, which approaches very near to the sponges, he met with many irregular cavities in it, and also different kinds of inhabitants; one of them he has particularly described and figured. Vide Donat. Hist. M. Adriat. p. 58. t. 8. fig. G. But he very judiciously says, these are not the fabricators, but the inhabitants; and allows the alcyonium to be of animal origin, in which he says he has discovered evident marks of sensation. As to the 2d doubt, it may be observed by the curious inquirer into nature, that the same property of inclosing extraneous substances is common to the whole class of zoophytes, as they grow up, from the sponge to the red coral. In order to prove this, Mr. E. had various specimens, as well of sponges as keratophytons and corals, with different bodies inclosed in them, both animal and vegetable. He had specimens where even the red coral encloses the white coral, and the white the red: with many keratophytons, that have enclosed small roundish shells of the barnacle tribe, thought by some superficial inquirers into nature (who would have them to be vegetables) to be the fruit of the keratophytons.

Explanations of the Figures, which are in plate 7.—A is an irregular piece of the crumb of bread sponge, found at Pagham on the sea coast of Sussex; aa represent the papillæ, through which the sponge receives and discharges the water: this, when recent, is of a fine orange colour.—Fig. B is the branched English sponge; at bb, along the edges, and on the surface of the branches, are rows of small papillary holes, through which the animal receives its nourishment.—Fig. C represents the downy branched English sponge found on the Sussex coast; this is covered over with a fine down so close, that it hides the many small holes that are on its surface.—Fig. D and E, the perpendicular and horizontal sections of the common officinal sponge.—Fig. F is the cock's comb sponge, taken off the rocks at Hastings in Sussex, and viewed while alive in sea water. The other sponges represented here, are introduced to show the variety of forms these animals appear under in different parts of the world.—Fig. G is a branched tuberculated sponge from Cape Coast Castle in Africa. Fig. g represents the appearance of the tubercles in their dried state, when magnified. This sponge approaches very near to the figure of the corallium album porosum maximum of Sir Hans Sloane, see the Hist. of Jam. vol. i, tab. 18, fig. 3, and of the porus albus erectior ramosus tuberculis crebris sursum spectantibus of Morison. See Hist. Ox. p. 3, sect. 15, tab. 10, fig. 3.—Fig. H is a sponge from Stavanger on the coast of Norway; this may be called the sea-fan sponge, from its great likeness to the keratophyton of that name; all its pores are surrounded with small spiculæ, which, from their minuteness, could not be well represented in the drawing. The dichotomous branched sponge at fig. I, is of a firm but elastic texture, very full of small holes, guarded by minute spines; this was found on the coast of Norway, and presented by Peter Collinson, Esq., F.R.S.

XXXII. Extract of a Letter from Dr. John Hope, Prof. of Medicine and Botany, Edinburgh, to Dr. Pringle. Dated Edinb. Sept. 24, 1765. p. 290.

In autumn 1763, Dr. H. received from Dr. Mounsey the seeds of the rheum palmatum, which he assured him were the seeds of the true rhubarb. Dr. H. sowed them immediately in the open ground in the botanic garden. In the beginning of May, 1764, one of the plants from these seeds pushed up a flowering stem, and about the middle of the month, the flowers began to open, and continued in great beauty till the 8th or 9th of June: during this period, the wind was from the east, and extremely cold, and both the air and ground very dry. These circumstances had a great effect on the flowers; for, at their first appearance one cold day, many of them turned black, and Dr. H. imagined they would have been totally destroyed; they recovered however, and opened very well, and he had the pleasure of collecting near 30 seeds, some of which, he hoped would prove fertile.

Dr. H. was so much afraid the severity of the cold would destroy the flowers, that he caused the drawings of the plant to be taken when it was 4 feet high; but in less than 14 days it grew to 8, and at that time was most beautiful, with numerous and lofty panicles of flesh-coloured flowers, and large elegant leaves at its base. It is proper to take notice, that the foliage at the base of the plant did not all belong to one plant, but to 2 or 3, which accidentally grew so close together, that it was impossible to make a drawing of the flowering plant singly, without destroying the rest, which seemed unnecessary, and he could by no means consent to: further, the figure of the root was not taken from the root of the flowering plant, but from another sprung from the same seeds. On cutting this root across, he found it very succulent, the juice a little mucilaginous and of a sweetish taste. Though the root was taken up a great deal too young, and at an improper season, viz. in July, yet it had most perfectly the smell of the true rhubarb; and when chewed, though it was at first soft and mucilaginous, it soon discovered exactly the taste of the best foreign rhubarb. He had made trials of the powder of the root in the same doses in which the foreign rhubarb is given, and found no difference in its effects; its operation being equally easy and powerful.

From the perfect similarity of this root with the best foreign rhubarb in taste, smell, colour, and purgative qualities, Dr. H. had no doubt of our being at last possessed of the plant which produces the true rhubarb, and reasonably entertained the agreeable expectation of its proving a very important acquisition to Britain.*

[Then follows a botanical description, which it was judged unnecessary to reprint, as well as the print of the figures, 10, 15, 16; this plant being now so well known to gardeners and botanists.]

* Dr. Hope's expectations were well founded, large quantities of the rheum palmatum having of late years been cultivated in this country, the roots of which have been found to be equal in medicinal virtue to the rhubarb imported from Russia, Turkey, or China. See Transactions of the Society for the Encouragement of Arts, &c. Vols. 7, 8, 10, 11, 12.

XXXIII. On the History of the Return of the famous Comet of 1682, *with Observations of the same, made at Paris, at the Marine Observatory, in Jan. Feb., March, April, May, and the beginning of June,* 1759. *By M. Messier, Astronomer, Keeper of the Journals, Plans, and Maps, belonging to the Marine of France, and F. R. S. &c. Translated from the French by M. Maty, M. D. Sec. R.S.* p. 294.

Dr. Halley, who was first aware of the unequal returns of this comet in its former appearances, which he found to have been alternately of 75 and 76 years, was likewise the first who assigned their true cause. He ascribed it to the nearer or more distant approaches of the planets of our system; and having observed that this comet came very near Jupiter in the summer of 1681, above a year before its last appearance, and remained several months in the neighbourhood of that planet, he judged that circumstance alone sufficient to have considerably retarded its motion, and prolonged the duration of its revolution. Hence he concluded that its return was not to be expected till the latter end of 1758, or the beginning of the next year. Dr. Halley observes, in confirmation of this opinion, that the action of Jupiter on Saturn is alone sufficient to alter the duration of Saturn's period one whole month; and he adds, how much greater irregularities must not a comet be liable to, which at its remotest distance gets near 4 times farther from the sun than Saturn, and whose velocity in drawing near the sun needs but a very small increase to change its elliptic into a parabolic curve.

Dr. Halley does not determine more exactly the time of the return of the comet of 1682; neither could he do it but by determining exactly the effect of the neighbourhood of Jupiter, which must very sensibly affect the velocity with which the comet was moving towards the sun. Besides, regard must be had, not only to this approach to Jupiter in 1681, but also to the other approaches to this and all the other planets, which act more or less on the comet, as they do on each other. In short, it was necessary to consider all the different situations and distances of all the planets with regard to the comet, during the whole of its last revolution, and even during the former ones, when the returns had been found to be unequal.

M. Clairaut undertook this calculation, and his results differed but one month from the observation. No small degree of exactness this, considering the immensity of the object. In November 1758, he published his conclusion, which allowed about 618 days more for the period that was to end in 1759 than for the former; whence he inferred that the comet must be in its perihelion towards the middle of April. The comet however reached its perihelion on the 13th of March in the morning. M. Clairaut has since published the methods and calculations, by which he has arrived at this conclusion.

The impatience of astronomers, and their desire to prepare for verifying this prediction of Dr. Halley, had put them on inquiring for several years in what part of the heavens this comet was likely to appear; but, being ignorant of the exact time of its return, they could not determine the spot where it might be expected to be seen, but by making various suppositions as to the time of its perihelium. This Mr. Dirck of Klinkenberg, a famous astronomer, had attempted 7 or 8 years before, having taken the pains to calculate the principal points of 14 different tracks, which the comet was to take, on as many different suppositions relating to its passage through its perihelium, almost from month to month, from the 19th of June 1757 to the 15th of May 1758. Messrs. Pingré and de la Lande proceeded much in the same manner, in the calculations they published in the Memoirs of Trevoux for April 1759, 1st and 2d parts, with this difference, that the latter in their suppositions had taken narrower limits, and nearer to Mr. Clairaut's determination, who had fixed the return of this comet to the middle of April.

Mr. de L'Isle, being curious of seeing the comet on its first return, as soon as it could be discovered by means of telescopes, before it was visible to the naked eye, thought he must proceed in a different manner from what other astronomers had done, to find out in what part of the heavens it must be looked for. He considered that it was not necessary to know its place throughout its whole course, but only at the first moment of its appearance, because, having once found it out, it would be an easy matter afterwards to trace it through its whole progress by observation and calculation. A full description of this method is found in an ample memoir concerning this comet, which Mr. M. laid before the Royal Academy of Sciences at Paris, with a northern hemisphere, by means of which he was enabled to look for this comet in the very place of the sky, where it ought to appear, and it was by the help of this planisphere that he discovered the comet from the Marine Observatory at Paris on the 21st of January in the evening, after searching for it 2 years successively whenever the sky would permit. After this first discovery of the comet on the 21st of January 1759, Mr. M. continued to watch it daily, and made observations of it every day, either night or morning, that it could be seen for the weather or its own situation; till the 3d of June following, when he saw it for the last time.

The following are the elements of the comet, as computed by Messrs. de la Caille, Maraldi, and De Lalande.

	Mr. de la Caille.	M. Maraldi.	M. de la Lande.
Place of the ascending node	1s 23° 49′ 0″	1s 23° 49′ 41″	1s 23° 45′ 35″
Inclination of the orbit	17 39 0	17 35 20	17 40 14
Place of the Perihelion	10 3 16 0	10 3 16 20	10 3 8 10
Logarithm of the distance at the perihelion	9.766039	9.766115	9.7670848
Time of the perihelion	March 12. at 13h 41′	at 12h 57′ 36″	at 13h 59′ 24″

Mean time, at the meridian of Paris. The motion of the comet was retrograde.

XXXIV. On the Transit of Venus in 1769. By Tho. Hornsby, Professor of Astronomy at Oxford, and F.R.S. p. 326.

The observations of the late transit of Venus, though made with all possible care and accuracy, have not enabled us to determine with certainty the real quantity of the sun's parallax; since, by a comparison of the observations made in several parts of the globe, the sun's parallax is not less than 8¼″, nor does it seem to exceed 10″. From the labours of those gentlemen who have attempted to deduce this quantity from the theory of gravity, it should seem that the earth performs its annual revolution round the sun at a greater distance than is generally imagined; since Mr. Professor Stewart has determined the sun's parallax to be only 6″.9, and Mr. Mayer, the late celebrated professor at Gottingen, who has brought the lunar tables to a degree of perfection almost unexpected, is of opinion that it cannot exceed 8″.* In this uncertainty, the astronomers of the present age are peculiarly fortunate in being able so soon to have recourse to another transit of Venus in 1769, when, on account of that planet's north latitude, a difference in the total duration may conveniently be observed, greater than could possibly be obtained, or was even expected by Dr. Halley, from the last transit.

The experience which we gained in the year 1761, the knowledge of the errors, from whatever cause they may arise, which must unavoidably be committed in observations of this kind, will enable us to put in practice every method of solving this problem, and to determine with what degree of accuracy, and within what limits, the true quantity of the sun's parallax may be obtained, and consequently the dimensions of the whole solar system.

But, before giving a computation of the effect of parallax at the several places where this transit ought to be observed, it will be necessary to premise the principles on which the general calculus was formed. Having found, by computing the observations made on the transit in 1761, that when the Abbé de la Caille's solar tables are used, the epoch of the mean motion of Venus for 1761, as given in Dr. Halley's tables, requires a correction of 4′ 52¼″; and that the place of the ascending node was, at the beginning of the same year, in 2^s 14° 31′ 10″: having collected also, by computing the observations of Mr. Horrox in 1639, with the assistance of Dr. Halley's tables of Venus, and the solar tables above-mentioned, that the motion of the planet's mean longitude is 6^s 19° 12′ 22″, and of the node 52′ 18″, in 100 Julian years. Mr. H. has supposed the mean longitude of Venus, in the beginning of the year 1769, to be 5° 23′ 48″, and the place of the node to be in 2^s 14° 35′ 21″ —; and has assumed the rest of the planet's ele-

* Mr. Machin, professor of astronomy at Gresham college, deduced the same quantity many years ago. See a tract entitled, The Laws of the Moon's Motion according to Gravity, p. 24. —Orig.

ments as given in Dr. Halley's tables. According to these numbers, and the Abbé de la Caille's solar tables, the ecliptic conjunction will happen on June 3, 1769, at $9^h\ 59^m\ 24^s$ mean time at Greenwich, the planet's geocentric latitude being 10′ 13″.5 N. The log. of the earth's distance from the sun = 5.0065166; the log. of Venus's distance from the sun = 4.8610947; and of the planet's distance from the earth = 4.4606784; and the equation of the præcession of the equinoctial points = + 17″.7. By computing the geocentric longitude and latitude of the planet 3 hours before and 3 hours after the ecliptic conjunction, Mr. H. finds the planet's hourly motion from the sun in the ecliptic = 3′ 57″.7; the hourly motion in the relative orbit = 4′ 0″.3; the hourly motion in latitude = 0′ 35″.45; the angle of the planet's path with the ecliptic = 8° 29′ 2″; the angle of the ecliptic with the equator = 7° 2′ 54″; and therefore the angle made by the planet's visible path with the equator = 15° 31′ 56″. The geocentric latitude is, as has been observed, = 10′ 13″.5; and hence it is easily determined that the least distance of the centres will be 10′ 6″.8, and the interval between the time of the ecliptic conjunction and the middle of the transit = $22^m\ 21^s$ of time. As the planet has not yet passed its node, the middle of the transit will therefore be at $10^h\ 21^m\ 45^s$ mean time at Greenwich. In every inferior conjunction the motion of Venus is retrograde, and therefore the effect of the aberration of light in long. = 3″.7, when reduced to time = 55^s, is to accelerate the several phases of the transit; the equated mean time of the middle therefore will be at $10^h\ 20^m\ 50^s$. But the place of the planet is also affected by the aberration of light in latitude, and as Venus's latitude is decreasing, the least distance of the centres will be increased by 1″.35. The equated least distance of the centres therefore will be 10′ 8″.15. Now, supposing the semidiameter of the sun = 15′ 45″.6, and of Venus = 29″, the semidurations, or intervals between the middle and the external and internal contacts, will be found = $3^h\ 10^m\ 8^s.5$, and $2^h\ 51^m\ 13^s.2$. The equation of time is about $2^m\ 14^s$ at the middle of the transit, by which quantity the apparent time is before the mean, and decreases at the rate of about 2^s in 6 hours. Therefore the apparent times of the several phases of this transit for the meridian of Greenwich are as follow:

	Apparent time.		
First external contact, June 3	7^h	12^m	56^s
Total ingress	7	31	52
Middle	10	23	04
Beginning of egress	13	14	16
Last contact	13	33	11

Hitherto we have had no regard to parallax, and the above times are such as would be observed from the earth's centre. To the British isles, and to the neighbouring parts of the continent, the effect of parallax is nearly at a maximum, and will considerably accelerate the times of external contact and ingress. If we suppose the sun's parallax on the day of the transit = 8″.7, the horizontal parallax of Venus from the sun will be 21″.87; and the times of the external and internal contacts visible in England will be accelerated by the joint effects of parallax, both in the direction of the planet's path and perpendicular to it; the former by $7^m\ 9^s$, the latter by $7^m\ 12^s$. And therefore Venus will be seen to touch the sun's limb at $7^h\ 5^m\ 47^s$, more than an hour before the time of sun-set; when the apparent altitude of the planet above the horizon will be about 8 deg.; and the total ingress will happen at $7^h\ 24^m\ 40^s$, when the planet's altitude will exceed 5 deg. If the sun's parallax should be one second larger than we have supposed, or 9″.7, the time of ingress will happen at $7^h\ 23^m\ 51^s$. These times are not here given with any great degree of confidence: but as the errors in the planet's orbit will, in June 1769, be nearly the same with those which were observed 8 years before, it may be presumed that the foregoing computation will be found not to differ very widely from the truth.

Having rectified the globe to the declination of the sun at the middle of the transit = 22° 26′ 40″, and also at the times of the two internal contacts, Mr. H. finds that the whole transit will be visible to a considerable part of Swedish Lapland, the northernmost parts of Asia, and the northern and N. W. parts of North America; for the circle of illumination at the first internal contact passes along the western coast of Africa from Cape Verd, through the straits of Gibraltar, to Clermont in France, leaving Paris about a degree and a half to the west; thence it passes through Germany and along the Baltic Gulf, through Wibourg and Archangel, along the northern coast of Asia; and then traversing the N. E. parts of Siberia, it passes over Japan, enters the great Atlantic ocean, leaves the Marian isles and New Zealand on the west, and running round Cape Horn, and near Falkland isle, passes on to Cape Verd through the Ethiopic ocean, in a direction nearly parallel to the eastern coast of South America. All places situated under the first part of this line from Nova Zembla towards Cape Horn will see Venus enter on the sun at the time of sun-setting, and at sun-rising under the other half of it. The circle of illumination at the beginning of egress enters Europe to the north of Drontheim in Norway, and crossing the Bothnic and Finland gulfs, passes over Muscovy and the Caspian sea; and running through Persia traverses the Arabian gulf, going southward near the isles of Maldivia, and taking a large circuit towards the south pole, returns through Mexico, Louisiana, Canada, and the southern parts of Greenland, to Drontheim. So that almost all Africa is deprived of a view of this transit, and a very considerable part of Europe.

If we examine the observations of the transit in 1761, in places where there were more observers than one, and where the contacts were observed when the sun was near the horizon, and at higher altitudes, we may safely conclude that the observations will be made with sufficient accuracy when the sun is at such altitudes above the horizon, as not to be greatly affected by the vapours. At the observatory at Upsal, when the sun's altitude at the ingress was 3½ degrees, three observers differed 22ˢ; whereas at the egress, when the sun was 44¼° high, the difference amounted only to 6ˢ. It should seem therefore, that observers ought not to be sent to places where the sun will be much less than 5° high at the time of either of the contacts.

It appears by computation, that the joint effect of the parallaxes, both in longitude and latitude, to lengthen the total duration, will be the greatest to those places which are about 24° or 25° to the east of Greenwich, in the 66th or 67th degree of N. latitude, when the sun's altitude is about 5° at each contact; or if the sun's altitude at each contact be required = 10°, the latitude of places under the same meridian must be 73° or 74° N. In the former case, this transit may be very advantageously observed at Tornea°, Kittis, and the adjoining parts of Swedish Lapland; in the latter at Wardhus, and in the neighbourhood of the North Cape; for an error of one or two degrees either in longitude or latitude will make but a very inconsiderable difference in the parallactic time; as will sufficiently appear from an inspection of the following table, which contains the joint effect of the parallaxes of longitude and latitude in accelerating the times of the two internal contacts at Tornea°, Kittis, and Wardhus, for each of which places the parallactic angle, or the angle made by a vertical circle with the orbit of Venus, was carefully computed.

	First internal contact.	Second internal contact.	Total effect of par. = 8″.7.	Total effect of par. = 9″.7.	Difference for 1″ of parallax.
Tornea°	6ᵐ 53ˢ	4ᵐ 47ˢ	+ 11ᵐ 40ˢ	+ 12ᵐ 58ˢ	1ᵐ 18ˢ
Kittis	6 51	4 43	+ 11 34	+ 12 51	1 17
Wardhus	6 38	4 41	+ 11 19	+ 12 37	1 18

Having determined the greatest effect of parallax in lengthening the total duration at such places to which observers may conveniently be sent, let us examine how far we may be enabled to obtain observations in such part of the earth's surface where the effect of parallax will be contrary; and consequently where the total duration will be as short as possible. By the assistance of calculation it may be found, that in the latitude of about 54° south, and in 155° of west longitude nearly, the total duration will be the shortest, when the sun's altitude is 5°; or in about 47° of south latitude under the same meridian, when the sun is 10° high. And accordingly, by computing the parallactic angle for the latitude of 55° south, and the meridian opposite to that of Tornea°, Mr. H. finds that the total duration will be shortened by parallax no less than 12ᵐ 53ˢ, supposing the sun's parallax = 8″.7, and consequently that there might be observed a difference in the total duration between this place and Tornea° of $24^m 33^s$; a difference considerably greater than was expected by Dr. Halley in 1761; and, supposing with that astronomer, that the observations at each contact may be taken true to a single second, which indeed experience will not warrant, sufficient to determine the sun's parallax within $\frac{1}{750}$ part of the whole.

But as this and the other point fall in the great South sea, where it does not certainly appear that there is any land, let us inquire in what parts of the South sea we may reasonably expect to find land. From the accounts of some of the circumnavigators, it should seem that there are islands scattered here and there about the tropic of Capricorn, particularly the island or islands of St. Peter, in about 150° of w. longitude from Greenwich, and in about 21° of s. latitude. Mr. H. therefore computed the parallactic angle for a place $10^h 22^m 50^s$ to the west of Greenwich, and in 21° of s. latitude, and found that the ingress would happen $6^m 10^s$ later, and the beginning of egress $6^m 6^s$ sooner, than if seen without parallax; that the total duration is therefore shortened $12^m 16^s$ by parallax; and consequently that there is a difference of $23^m 56^s$ in the total duration between Tornea° and this island, supposing the sun's parallax = 8″.7 on the day of the transit; or of $26^m 39$, if that parallax be supposed = 9″.7.

About the latter end of the 16th century, Don Pedro Fernandez de Quiros made two voyages for the discovery of the southern continent and islands, under the patronage of the Viceroy of Peru. From several memorials which he presented to the court of Spain in the year 1609, with a view to procure a settlement of the countries he had actually discovered, it appears that he had found many islands, and particularly a large tract of land lying in or near the 15° of s. latitude, well peopled and well cultivated, the inhabitants generally of a peaceable disposition. The produce of this country is represented to be such as to render it a fit object to any commercial nation: consisting of gold, silver, pearl, spices of many sorts, and sugar-canes. He describes several safe and commodious harbours, particularly Puerto de la Vera Cruz in lat. 15° 40′ s. capable of holding 1000 ships, with a safe anchorage in every part; and where he himself actually staid 36 days with 3 ships. From the wholesomeness of the air, the fertility of the soil, and many other circumstances peculiar to this continent, he makes no scruple to prefer it to every country which the Spaniards had conquered, whether in the E. or W. Indies.* Mr. H. however observes, that, if this country be 195° to the w. of London, the whole of the transit, in all probability, will not be visible, as Venus will enter wholly on the sun's disk at, or a few minutes before, the time of sun rising.

The Spaniards gave the name of the islands of Solomon to certain countries in

* Harris's Voyages, 2d edit. vol. 1, p. 63.—Orig.

the South seas, reported to be very rich in gold. They were first discovered by Alvarez de Mendoza in 1527, and are supposed by some to be the very lands which were afterwards found by Fernandez de Quiros. The Spaniards are said to have had very clear and satisfactory accounts of these islands; but to have destroyed them for political reasons, by express orders from Old Spain, when Sir Francis Drake sailed into the South seas.* Their situation is not known; and from some fruitless attempts to find them, it has been, and is still perhaps questioned, whether there be any such islands. Some time after the year 1720, while Capt. Betagh, commander of the marines on board of Capt Shelvocke's ship, was in Peru, the discovery of these islands was again attempted, on some fresh information, by command of the Viceroy,† but without success: for the latitude of these islands is not even nearly known. They are however supposed to lie between the 10th and 20th degree of s. latitude, in about 175° of w. longitude from London, according to the best English and French maps: or, according to some geographers, these islands are only 120° to the w. of London.

Soon after the government of the Dutch in the East Indies was settled at Batavia, it was thought proper, by the Dutch East India company, that an exact survey of their countries already discovered should be made and preserved. For this purpose Capt. Abel Jansen Tasman sailed from Batavia in 1642. In this voyage several lands were discovered, particularly the two islands of Amsterdam and Rotterdam, lying in 21° and 20° of s. latitude, and in 173° or 174° of w. longitude. The islanders are represented to be of a civil and peaceable disposition, and to all appearance unacquainted with the use of arms; the lands well cultivated, and planted with all kinds of fruit-trees.‡ Not far from these two islands are 19 or 20 more, in 17° or 18° of s. latitude, and 4° or 5° to the w. of the former.

Mr. de Chabert, in the Memoirs of the Academy of Sciences for 1757, has given an account of 4 islands in the South sea, lying in about 10° of s. latitude, and 134° or 135° to the w. of London, discovered in July 1595, by Alvaro Bendano de Neyra, commander of a Spanish squadron of 4 ships, in his 2d voyage for the discovery of the Solomon isles. "The first and easternmost he named the island of Magdalene. It is about 6 leagues in circuit, with high coasts and mountains in the middle: and is extremely well peopled. More than 40 Indians came on board the ship. To the N. W. at the distance of about 10 leagues, lies the island of St. Peter, near 3 leagues in circuit, and presenting an agreeable prospect to the eye. About 5 leagues to the s. w. of St. Peter is another and larger island, named Dominica, about 15 leagues in circumference, well peopled, and affording beautiful prospects. To the s. of this island is St. Christine, near

* Harris's Voyages, 2d edit. vol. 1, p. 63.–Orig. † Id. ib. p. 245.–Orig. ‡ Id. ib. p. 327.–Orig.

8 leagues in circumference. The whole squadron passed between this island and Dominica, and anchored in a very good haven, to the w. of St. Christine, in the latitude of 9° 30′; near which they found a rivulet of very fine and fresh water. The coasts of all the islands seemed in general very safe and commodious for shipping. In this harbour they found all kinds of refreshment: as fowls, hogs, sugar-canes, plantanes, cocoa nuts, and many sorts of other fruits. They conversed with the natives of the country, and erected 3 crosses."

Besides the countries already mentioned, it should seem, from Mr. de Lisle's map of the southern hemisphere, that there are many islands situated between the parallels of 5° and 23° of s. latitude, in the great Pacific Ocean; the most remarkable of which, together with those already mentioned, are given in the following table.

	w. lon.	s. lat.		w. lon.	s. lat.
Island of St. Peter, according to some maps	130	18	Isle de la Belle Nation	160	12
Islands of Mendoza	135	9½	Island of Jesus	162	7
Islands discovered by Quiros, 1605	138	21	Rotterdam isle	173	20½
Isles des Tiburons, or Dog island	141	16	Amsterdam isle	174	21
Isle Habitée	144	17	Solomon isles	175	10
Islands of St. Barnard	150	11	Island of Taumaco	177	13
Water island	151	15	Prince William islands	178	17
Fly island	153	15½	Terra Australis	190	15
Land discovered by Mendana	157	4			

It appears then not only possible, but highly probable, that observers may be stationed in the South seas. Mr. H. therefore computed the parallactic angle for different longitudes and latitudes, answering to such places whose positions seem to be most certainly known; the result of which calculations is given at one view in the following table.

w. lon.	s. lat.	Places names.	First internal contact.	Second internal contact.	Tot. eff. of par. = 8″.7.	Diff. in tot. dur. fr. Tor.
155 42	21 0	Island of St. Peter	6m 10s	6m 6s	12m 16s	23m 56s
135 0	9 30	Mendoza isles	4 31	6 49	11 20	23 0
173 0	20 30	Amsterdam and Rotterdam isle	6 19	4 42	11 1	22 41
190 0	15 0	Terra Australis	5 43	3 16	8 59	20 39

By the last column it appears, that if an observer be stationed in any of the above places, perhaps in any part of the South seas where the whole transit is visible, and the total duration observed there be compared with that at Tornea°, we may obtain a difference in time from 20 to 24 minutes; which is indeed so considerable, that the sun's true distance must be ascertained more exactly than can possibly be expected from any other method.

But, if it should be found impracticable to station an observer in the South seas, the loss may in a great measure be repaired, if the transit be observed in such parts of North America where the whole is visible. At Mexico the total ingress will happen when the sun is very near the meridian; and if the longitude of the place and the general computation may be depended on, the beginning of egress will happen towards the time of sunset, when the apparent altitude of the sun will not much exceed 4 degrees. Observers therefore should be stationed farther to the west, about Cape Corientes; at which place and at Mexico, though in about 20° of N. latitude, the effect of parallax will still be considerable, particularly at the egress, as appears from the following table.

Places.	First internal contact.	Second internal contact.	Total effect of par. = 8″.7.	Difference in total duration from Tornea°.
Mexico.	1^m 7^s	5^m 3^s	6^m 10^s	17^m 50^s
Cape Corientes	0 26	5 0	5 26	17 6

By comparing the observations to be made at either of the above places, or in any of the neighbouring parts, with those of Tornea°, a difference of more than 17 minutes in the total duration may commodiously be obtained; by which the quantity of the sun's parallax may be determined agreeably to the method proposed by Dr. Halley in the case of the last transit, and in which no error but that of the observation can take place, supposing the situation of the two places to be nearly known.

When Dr. Halley's computation was examined, and it was found that so great a difference in the total duration of the transit at any two places as had been expected, could not conveniently be obtained; another method was proposed, and was accordingly carried into execution, viz. to station two observers in such a manner that one of the internal contacts might be observed with the greatest difference possible arising from a contrary effect of parallax at the two places. This method, though necessarily inadequate, because the longitude of the two stations must be rigorously known, may be practised at both contacts in 1769.

It appears by computation that the time of the first internal contact in the evening, is accelerated as much as possible by parallax in 48° 42′ of N. latitude, and 6° to the east of Greenwich, at or near Nancy in France. But this computation is framed on a supposition that the sun's centre is in the very horizon: in which circumstances no observation can be taken. If the sun's altitude at the time of the contact should be required equal to 5 or 10 degrees, then it will appear that Greenwich and Dublin are stations very advantageous; and we have already seen that the time of ingress at the former place will happen 7^m 12^s sooner than if seen from the earth's centre, on account of parallax. Indeed the effect of parallax will be nearly the same to every part of Great Britain. The part of the earth's surface, where the effect of parallax on the planet at the same contact will be as great as possible in a contrary direction, when the sun's altitude is about 5°, is in 46° of S. latitude nearly, and in 168° or 169° of W. longitude from London, in the great Pacific Ocean, where it does not at present appear that there is any land. If however an observer should be stationed in any of the islands in the South sea, for which a computation has already been made, even in the islands of Mendoza, the morning ingress will be found to be retarded by parallax 4^m 31^s; and consequently a difference at this contact of 11^m 43^s may be obtained by comparing the observation of the first internal contact with the observations at Greenwich; or 11^m 24^s, if the same observation be compared with that at Tornea°.

In order to see the beginning of egress accelerated by the greatest effect of parallax possible, when the altitude of the sun is 5°, an observer must be stationed in about 123° of W. longitude from London, and in about 19° of S. latitude; or, as it does not appear at present that there is land there, the observer may be stationed with considerable advantage either in the islands of Mendoza, or in the island of St. Peter. The same contact will be as much retarded on account of parallax to an observer placed under the tropic of Cancer, in about 67° of E. longitude from London. This point indeed falls into the gulf of Sindi; but as a difference of many degrees, either in longitude or latitude, will occasion but a very inconsiderable difference in parallactic time, this contact may be very advantageously observed on any part of the coast from the mouth of the Indus to Cape Comorin, and thence along the coast of Coromandel and Golconda as far as the mouth of the Ganges. It may naturally be expected that the end of the transit will, if the weather be favourable, be observed at many of our own settlements in these parts; Mr. H. therefore computed the effect of parallax at the egress for Madras and Calcutta, at which places the last transit was observed; and found that the time of the 2d internal contact will happen 6^m 41^s later on account of parallax at the former, and 6^m 44^s at the latter, than if seen from the earth's centre. By comparing the observations made at either of the above places with the corresponding observation at Mexico, we may obtain a difference of 11^m 44^s; a difference greater than could be obtained by any observations that could conveniently be made at the egress in the transit of 1761.

On the whole, the necessity of sending an observer into the South seas sufficiently appears, whether it be proposed to determine the sun's parallax by the difference in the total duration of the transit, or by the observations of the internal contacts either at the ingress or egress. For if there should happen to be no land in the meridian opposite to Tornea°, and in about 21° of S. latitude; yet if an observer can be stationed either in the islands of Mendoza, or in the islands of Amsterdam and Rotterdam, a difference in parallactic time will be obtained as in the following table.

Places compared.	Difference in total duration.	Difference at ingress.	Difference at egress.
Tornea° and Mendoza isles	23^m 0^s	11^m 24^s	11^m 36^s
Tornea° and Amsterdam or Rotterdam	22 41	13 12	9 29
Tornea° and the opp. merid. in 21 S. lat.	23 56	13 3	10 53

If the sky should prove favourable, the observations made at Tornea°, and in any of the above places, will enable us to determine the sun's parallax with great precision, and independent of the exact knowledge of the longitude of either place. But as the situation of Tornea° is perhaps very exactly known, if it should be convenient to the southern observer to continue long enough on his station to determine its exact longitude, to which the situation of Jupiter at that time will greatly contribute, both methods might be practised at the same time, and they would mutually confirm and illustrate each other.

An opportunity of observing another transit of Venus will not again offer itself till the year 1874. It behoves us therefore to profit as much as possible by the favourable situation of Venus in 1769, when we may be assured the several powers of Europe will again contend which of them shall be most instrumental in contributing to the solution of this grand problem. Posterity must reflect with infinite regret on their negligence or remissness; because the loss cannot be repaired by the united efforts of industry, genius, or power. How far it may be an object of attention to a commercial nation to make a settlement in the great Pacific Ocean, or to send out some ships of force with the glorious and honourable view of discovering lands towards the south pole, is not his business to inquire. Such enterprizes, if speedily undertaken, might fortunately give an advantageous position to the astronomer, and add a lustre to this nation, already so eminently distinguished both in arts and arms.

END OF THE FIFTY-FIFTH VOLUME OF THE ORIGINAL.

I. Observations of the Solar Eclipse, Aug. 16, 1765, made at Colombes, near Paris; at the Observatory of the Marquis of Courtenvaux, 5′ 13.8″ North of the Royal Observatory, and 20½s in Time to the East. By M. Messier, Astronomer, F. R. S., &c. Translated from the French. Vol. LVI. p. 1.

At 3^h 58^m 13^s true time, beginning of the eclipse.
4 28 0 thick clouds covering the sun.
5 20 0 clear again, but the eclipse was over.
31′ 42½″ the sun's diameter measured.

II. Remarks on the Palmyrene Inscription at Teive. By the Rev. John Swinton, B. D., F. R. S. p. 4.

The palmyrene inscription at Teive having been inaccurately taken by Signor Pietro della Valle, the transcript published in the Philos. Transact. must be incorrect, and consequently the explication of that inscription by Mr. Swinton cannot in all points be entirely depended on. Having therefore been informed, that the stone itself, brought a few years since out of the east, was in the possession of the Earl of Besborough, Mr. S. procured a view of the stone, examined the inscription with all the attention he was capable of, and took the exact transcript of it on the spot, and communicates the explanation of it in Hebrew characters, with Latin and English versions.

The inscription in Hebrew or Chaldee characters.

JOVI FVLMINATORI IN AETERNVM sit REVERENTIA—OPERIMENTVM ET LECTVM ei DEDICAVIT AGATHANGELVS.	לבעל שמצ מרא עלמא קרב כסתא וערשא אגתגלמ

TO JUPITER THE THUNDERER FOR EVER be REVERENCE—AGATHANGELVS DEDICATED to him this COVERED BED.

III. Of the Somersham Water, in the County of Huntingdon. By Daniel Peter Layard, M. D. p. 10.

From his experiments on this mineral water, Dr. L. infers that it "is a chalybeate water, strongly impregnated with the vitriol of iron and alum, and containing some calcareous earth, selenites, and salt."

On the Experiments made on the same Somersham Water. By Michael Morris, M. D., F. R. S. p. 22.

Dr. M. infers from his experiments, "that the contents of the Somersham water are; 1° iron, 2° selenite, 3° alum, 4° some marine salt, with a little alum and vitriol in the state of an aqua magistra aluminis et vitrioli, incapable of crystallization."

IV. Of an Inedited Coin of the Empress Crispina. By the Rev. John Swinton, B. D., F. R. S. p. 27.

An inedited Greek coin of the empress Crispina, which seems to have had a place formerly assigned it in the cabinet of the celebrated professor Ott, and thence afterwards to have passed into that of his son, the late Reverend Mr. Ott, some years since fell into Mr. S.'s hands. The medal is nearly of the size of the middle Roman brass, and tolerably well preserved. The workmanship is somewhat rude, and savours sufficiently both of the age and the remote province in which it first appeared. On one side is exhibited the head of Crispina, wife of the emperor Commodus, attended by the Greek legend ΚΡΙCΠЄΙΝΑ CΕΒΑCΤΗ, CRISPINA AVGVSTA; and on the reverse are two human figures, one sitting in a chair, with a lance in its left hand, and the other standing by its side. They are both surrounded by the inscription ΔΑΡΔΑΝΟΣΣΗΝΩΝ, DARDANOSSENORVM, or DARDANOSSENSIVM, which evidently points at the inhabitants of some ancient town. Who the Dardanossenians were, or in what part of the world situated, Mr. S. will not take upon him to decide; the word ΔΑΡΔΑΝΟΣΣΑ, DARDANOSSA not appearing, as the name of a city, in any ancient writer. But that this word occurred, in such a sense, in the original text of Ptolemy, (Geograph. lib. v. c. 13.) and was afterwards converted by some ignorant transcriber into DARANISSA, which still remains in all the printed and manuscript copies of that author, will, he thinks, not be contested. The coin therefore was struck at Dardanossa, or Daranissa, which seems to have been a town seated in Sophene, a province of the Greater Armenia, in the reign of the emperor Commodus, where the Roman power at that time prevailed.

V. Observation of the Eclipse of the Sun, of August 16, 1765, made at Leyden. By Professor Lulofs. F.R.S. p. 30.

At 4^h 29^m 1^s Beginning of the eclipse.
5 18 58 End of the eclipse.
Quantity eclipsed, 2 dig. 41′.

VI. On the Double Horns of the Rhinoceros. By James Parsons, M. D., F. R. S. p. 32.

When Dr. P. laid his natural history of the rhinoceros before the R. S. in 1743, which is printed in number 470 of the Transactions, (Abridgment, vol. viii. p. 692,) he had not an opportunity of showing a double horn to the members; he has therefore taken this occasion to present them with a sight of a specimen of the horns of an African rhinoceros, brought from the Cape of Good Hope, by William Maguire, Esq. What renders this subject the more worthy of observation, is that, by means of knowing there is a species of this animal, having always a double horn on the nose, in Africa, Martial's reading is supported against the criticism of Bochart, who changed the true text of that poet, in an epigram on the strength of this animal; for when Domitian ordered an exhibition of wild beasts, as was the custom of several emperors, the poet says, the rhinoceros tossed up a heavy bear with his double horn:

Namque gravem gemino cornu sic extulit urum.

and as Bochart knew nothing of a double horn, he changed this line both in reading and sense, thus:

Namque gravi geminum cornu sic extulit urum.

as if two wild bulls were tossed up into the air, by the strong horn of the rhinoceros.

The dimensions are as follow, viz. The length of the anterior horn, measuring with a string along the convex fore part, is 20 inches; perpendicular height 18; circumference 21½ at the base; the posterior horn is in perpendicular height 19¼; circumference round the base 18; length of both bases together on the nasal bones 14; and the weight of both together is 14 pounds 10 ounces.

The rhinoceros of the year 1739, described in the Transactions, was three years old; and the horn not three inches high; and hence by comparing that with this, one may imagine this to be many years old, perhaps above twenty; and that this animal lives to a great age. It is also plain that the horns are perpetual as are those of oxen.

VII. Extract of Two Letters, dated Dec. 7, and 12, 1765, from the Rev. William Borlase, of Ludgvan, Cornwall, F. R. S. to Emanuel Mendes da Costa, Librarian, &c. to the R. S. p. 35.

As the existence of native tin is absolutely denied by all mineralists both ancient and modern; and at the time Mr. B. wrote his Natural History of Cornwall, having no evidences to prove the contrary, he contented himself with suggesting that its existence was far from being improbable, and in that manner he left the dispute undecided. But a fortunate discovery, which had furnished him with 3 specimens of this metal, native, or pure, would exclude all further doubt. The account of it is as follows. In the month of May 1765 was found near St. Austle, a large cake, or nodule, of tin ore, weighing about 6lb, irregular in shape, cracked or jagged at the edges, lying about 5 feet under the surface, and in the middle of that stratum of tin ore, so remarkably spread in the moor adjoining to the forementioned town. When the lump was broken, it appeared to consist of two coats or incrustations, surrounding the whole, and of a nucleus or central substance of a quartz, intermixed with the purest malleable tin.

Of the first specimen, the outer crust was about $\frac{1}{8}$ of an inch thick at a medium, and of a brownish straw colour; the 2d or inner coat was blacker, closer grained, with some faint appearances of whitish specks interspersed, and about $\frac{1}{8}$ of an inch thick; these two coats inclosed a third substance, consisting of laminated crystals, rising side by side out of an edging, which shines like melted tin, and lies as it were at their roots, coherent to the 2d coat. These crystalline laminæ are thin almost as the flakes or scales of talc, and being shot in a great variety of directions, intersect each other, and leave a vast number of cells, within which are plainly seen, and may be cut freely with a knife, many specks and granules of pure native tin.

The 2d specimen, which he sent to the Museum of the R. S. was of the same structure, and part of the above-described lump, but much richer in quality. Besides all the appearances of native tin taken notice of in the former specimen, in this N° 2, was seen the malleable tin, in colour equal to the finest tin of the furnace, more liberally and distinctly dispersed. The metal is not only found in granules, but in a foliaceous manner issuing out of the quartz, and formed like a thick, jagged, or scalloped lace or edging, of which the specimen itself only can give the justest idea. The lump, or nodule, of which these two specimens are fragments, was so richly impregnated with tin, that though the best tin ore, in general, will not melt without flux, nor do 20lb. of black tin usually produce more than 14lb. of white, this melted without flux, and 20 oz. produced 18 oz. of the purest tin.

The 3d specimen, found the 17th of July last, was found in a stream work near the borough of Granpont; its weight between 11 and 12lb; the native tin was inclosed so securely, that, but for the extraordinary weight, it had passed unnoticed. Within the crust, the metal was not in granules, as in the first specimen, nor thin as a leaf, as in the 2d; but much more abundant, and in some places more than an inch thick. The crust, inclosing this 3d specimen, was certain stone of the quartz kind, very hard to break, and exactly the same, to all appearance, with that of the first mass.

Thus far is the relation Mr. Borlase gives; but as the existence of native tin is so universally doubted, Mr. Da Costa thought it necessary, that other proofs than a mere historical account, and the exhibition of only two specimens, and both from the same hand, should be produced to prove it. Mineralists might then doubt whether what Mr. Borlase calls tin, was really that metal, or rather an arsenical marcasite, or other mineral, which might appear like tin, or be mistaken for it. He thought it very necessary to remove all doubts, by making proper experiments to try if it was tin; it being so extraordinary a discovery. The experiments he made, and which he hoped would prove satisfactory, to convince every one that it is really tin, are as follow; 1. It was perfectly ductile and malleable; and, bent between the teeth, gave the same crackling noise as tin always does. 2. In an open fire it melted easily, calcined on the surface, and smoked somewhat; forced in a stronger fire, with borax, it detonated with small phosphorescent sparks, which is a property of pure tin. 3. It was only corroded to a white calx in spirit of nitre, and oil of tartar per deliquium being added to the solution, not any thing was precipitated. It was therefore pure tin.

VIII. Abstract of a Letter from Edward Wortley Montagu, Esq. F. R. S. to William Watson, M. D., F. R. S. containing an Account of his Journey from Cairo, in Egypt, to the Written Mountains, in the Desart of Sinai. Dated Pisa, Dec. 2, 1765.* p. 40.

Mr. M. set out from Cairo, by the road known by the name of Tauriche

* Edw. Wortley Montagu, or Mountagu as he himself writes it, was a most singular and eccentric character. He was the son of the famous Lady Mary W. M. by her husband Edw. W. M. and was born at Warncliffe-lodge, in Yorkshire, about 1714; and he died in Italy in 1776, consequently at 62 years of age. His father going ambassador to Constantinople in 1716, Lady Mary accompanied him, with her young son Edward; where observing that the country people practised inocula-

Beni Israel, Road of the Children of Israel. After 20 hours travelling, at about 3 miles an hour, he passed, by an opening in the mountains on the right hand, the mountains Maxatree. There are two more roads, one to the northward of

tion on their children for the small-pox, Lady Mary had the operation performed on her son; from which circumstance, and that of her letters on this subject, the practice first came to be adopted in this country. From Westminster school, where he had been placed for his education, he ran away three several times. Once, when he exchanged clothes with a chimney-sweeper, and for some time followed that occupation. From which perhaps might arise the custom of his relatives, the family of the Montagu's in Portman-square, London, of annually treating all the chimney-sweepers of London with a feast on the 1st of May. Young Montagu next attached himself to a fisherman, and cried flounders in Rotherhithe. He afterwards sailed as a cabin-boy to Spain; where he soon deserted the vessel, and hired himself to a driver of mules. After thus acting the vagabond for some time, he was discovered by the English consul, and returned to his friends in England, who received him with the joy of the father of the prodigal son. A private tutor was soon employed to endeavour to recover those rudiments of learning which must have been greatly obliterated by such a course of dissipation. He was afterwards sent to the West Indies, and after remaining there some time, he returned to England; and acting more conformably to his rank, he was elected and served as a member in two successive parliaments. But his profuse expences involving him in embarrassments, he quitted his native country, and commenced that wandering traveller he continued to the time of his death. Having visited most of the eastern countries, he contracted a partiality for their manners. He drank little wine; but a great deal of coffee; wore a long beard; smoked much; wore the Turkish habit, even when in Italy; and sat cross-legged in their manner through choice, and embraced the Mahometan religion. It would be endless to attempt the particulars of all the characters he acted in the different countries he passed through: how he conversed with the nobles in Germany, and served an apprenticeship in the science of horsemanship at their country-seats: how he had been a labourer in the fields of Switzerland and Holland, and had not disdained the humble professions of postillion and plowman; how he assumed at Paris the ridiculous character of a petit-maitre: how he was an abbot at Rome: put on at Hamburgh the Lutheran ruff, and with a triple chin and a formal countenance, he dealt about him the word of God, so as to excite the envy of the clergy; acting, as he said, all the parts that Fielding has described in his Julian.—With the Hebrew, the Arabic, the Chaldaic, and the Persian languages, he was as well acquainted as with his native tongue. Besides two papers in the Philos. Trans. he published some other pieces: one on the "Rise and Fall of the Roman Empire:" another on "The Causes of Earthquakes."

Mr. M. had been early married to a person of no higher character than that of an industrious washerwoman. As the marriage was solemnized in a frolic, he never deemed her sufficiently the wife of his bosom to cohabit with her. Yet he was a perfect patriarch in his manners. He had wives of all nations wherever he came. When in Egypt, he had his household of Egyptian females. At Constantinople, the Grecian maids had charms to captivate this unsettled wanderer. In Spain a Spanish brunette; in Italy, the olive-complexioned female shared his embraces. At last, however, hearing of the death of the original Mrs. M. the washerwoman, by whom there was no issue; and without male issue, a large estate would revert to the second son of Lord Bute; Mr. M. thinking he owed the family no obligations, was determined if possible, to defeat their expectations. He therefore resolved to return to England and marry. He acquainted a friend with his intentions; and commissioned him to advertize for any decent young woman who might be in a pregnant state. Several ladies answered it; and one was selected, as the most eligible object. She waited with eagerness for the arrival of her expected bridegroom, who was however arrested on his journey by the hand of death.

this, which the Mecca pilgrims go, and one to the south, between the mountains, but never travelled, as it does not lead to Suez, to which it is 30 hours march from Cairo. Through this breach the children of Israel are said to have entered the mountains, and not to have taken the most southern road: which he thinks most probable: for those valleys, to judge by what now appears, could not be passable for Pharaoh's chariots. At Suez he found an opportunity of going to Tor by sea, which he gladly embraced, that by going nearer the place at which the Israelites are supposed to have entered the gulf, and having a view from the sea, as well of that as of the opposite shore, he might be a little better able to form a judgment about it. Here it is high water always when the moon is at her meridian height, and it ebbs 6 hours. At Suez, it flows 6 feet; the spring tides are 9; and in the variable months, from the beginning of November to the end of April, sometimes 12. From the beginning of May to the beginning of October, a northerly wind generally rises, and goes down with the sun; it is often very strong. This wind never fails in these months, unless there be some violent storm; the rest of the year the winds are variable, and when they blow hard at s. and s. s. e. these winds set up the sea through the narrow strait of Babel Mandel, and up this gulf through its mouth, between Gebel El Zait, on the west side of this sea, and the southernmost point of the bay of Tor, on the east side of this western branch of this sea, where it is not above 12 or 14 miles over. Probably such a wind, hindering the water from going out, causes this extraordinary increase in the spring tides. The same thing happens with the same winds at Venice, both gulfs running nearly in the same direction.

The Egyptian, western, or Thebaic shore, from Badeah southward to opposite Tor, on the eastern shore, is all mountainous, and steep; and at Elim, the northernmost point of the bay of Tor, ends the ridge of mountains, which begin on the eastern shore of this western branch at Karondel. The garden of the Monks of Mount Sinai at Elim renders in dates, &c. 20,000 piastres per ann. or £2,500. Thence they crossed the plain, in about 8 hours, and entered the mountains of Sinai. They are of granite of different colours. At the entrance of the narrow breach, through which they passed, he saw, on a large loose granite stone, an inscription in unknown characters, given he thinks by Dr. Pocock, bishop of Ossory; however, as the Israelites had no writing, that we know of, when they passed here, he did not think it of consequence enough to stop for: they arrived at the convent of Mount Sinai, after the usual difficulties mentioned by other travellers, were received as usual, and saw the usual places. The monks were far from owning that they had ever meddled with the print of the foot of Mahomet's camel. He examined it narrowly, and no chissel has absolutely ever touched it, for the coat of the granite is entire and unbroken in every part; and every body knows, that if the coat of less hard stones than granite be once destroyed, it never returns. It is a most curious lusus naturæ, and the Mahometans turn it to their account. Meribah is indeed surprizingly striking. He examined the lips of its mouths, and found that no chissel had ever worked there; the channel is plainly worn by only the course of water, and the bare inspection of it is sufficient to convince any one it is not the work of man. Among the innumerable cracks in rocks, which he had seen in this, as well as other parts of the world, he never met with any like this, except that at Jerusalem, and the two in the rock which Moses struck twice.

He inquired of the Monks, as well as Arabs, about certain places, as well as about some ruins, supposed by the bishop of Ossory, to be Kadesh Barnea: the former could only tell him they had not received any fish from thence in many years; that it was two easy days journey off, but the road was mountainous; so one may suppose the distance less than 40 miles. The Arabs agreed as to the road; but they said, it was once a large place, where their prince lived, whose daughter Moses married; that Moses was afterwards their prince, and the greatest of all prophets. These Arabs place Moses the first, Solomon the second, Mahomet the third, Christ the fourth, and then the prophets of the Bible. As to Dzahab, the Monks only knew the distance to be 4 days journey, and that there was a road from it to Jerusalem: the Arabs told him the same, so the distance is about 80 miles. He inquired of them all about the ruins; they told him there were very considerable ones about half way to Dzahab, about 40 miles from Sinai; but he thinks Kadesh must have been much nearer to Jerusalem. He would willingly have gone to these places; but as the 4 clans of Arabs, which inhabit this promontory, were then at war one with the other, he could get no conductor. However, combining the whole together, and comparing it with what we collect from Scripture, he thinks we may well conclude, Sharme to be Midian, and Meenah El Dzahab to be Eziongeber: what the interjacent ruins are he cannot conjecture; but he believes he had found Kadesh Barnea to be elsewhere. He thinks it cannot be here, for the Israelites were on the borders of the Holy Land, or Land of Promise, when they were ordered back; and when they were stopped by the Moabites, they are said to have been brought up from Kadesh Barnea.

There are two roads from Mount Sinai to Jerusalem; the one through Pharan, the other by the way of Dzahab: that through Pharan is 11 days journey; 2 to Pharan, 3 to a station of the Mecca pilgrims called Scheich Ali, one and a half to some considerable ruins; all this to the northward: thence rather more than 4 to Jerusalem, by way of Hebron, leaving the Asphaltic Lake on the right hand to the south-eastward. The other way is longer, on account of the road being more mountainous; that too passes the same ruins, and also Scheich Ali. He inquired about this at Jerusalem, and received the very same account, with this addition, that such Mahometans as went from Jerusalem to Mecca, went that way to join the Cairo caravan at Scheich Ali. This seems to be a situation opposite to Kadesh Barnea; at the line drawn by all the geographers; it is without Mount Sinai (taken for this whole tract); and just before the Moabites, as the children of Israel passed by Mount Hor, now Acaba, leaving the Asphaltic lake on their left hand, to the north-west. The tradition too of the Arabs is, that they passed this way; therefore he thinks Kadesh Barnea must be near this spot. There are here considerable ruins; and he knew of no city that ever was here; for Petra lay more to the east, between the Asphaltic lake and the Elanitic gulf. To leave no inquiry wanting, he asked the Rabbins of Jerusalem where they placed Kadesh Barnea; and they said these ruins.

Mr. M. set out from Mount Sinai by the way of Scheich Salem; and, after passing Mahomet's stone, came to a beautiful valley. He lay there, and hoped he discovered the manna, and did not set out before day light, that he might not pass the rock which Moses struck twice. He searched, and inquired of his Arabs, but could neither hear nor see any any thing of it. He saw several short inscriptions stained on some parts of the mountains, the characters being the same with those on mount Sinai, Meribah, &c. given by the bishop of Ossory. About 4 miles before arriving at Pharan, they passed through a remarkable breach in a rock; each side of it is perpendicular as a wall, about 80 feet high, and the breach is about 40 broad. It is at this breach, he imagines, the Horites were smitten, 4 miles beyond the present ruins of Pharan; for having passed this breach they could make a stand, nor could they well be pursued. Here, on the tops of the mountains on the right hand, were ruins of buildings, and one seemed a castle. From Meribah to near this place, they had always rather descended; in most places there is the bed of a stream, and after rain the water runs; but a little before they came to this breach, it winded off towards the west, for the waters fall into that part of the desert which they crossed from Tor. Between this breach and Pharan, there are several springs, and one at Pharan where they encamped; there is the bed of a river, the traditional account of which agrees with what is said by St. Paul. Waters seem to have run from Meribah to within about 6 miles of this place; the bed of a stream is here again very plain, and a spring at the upper end of it, which does not yield water enough to make a stream, the bed then is dry; 4 valleys terminate here, and form a large area. They travelled in the bed of the river through the valley to the north; and in about half an hour, the sight and appearance of a large stone, not unlike Meribah, which lay at some distance from the mountain on the right hand, struck him; and he also observed it had many small stones upon it. The Arabs, when they have any stone or spot in veneration, as Mahomet's stone, and the like, after their devotion, lay some smooth stone upon it.

He asked what it was, and they told him Hagar Mousa, the stone of Moses. He told them that could not be, for that lay in Rephidim; they said that was true, but this was Hagar il Chotatain, the stone of the two strokes; that he struck it twice, and more water came from it than from Meribah; witness the river. The bed of the river winds to the eastward, about E. S. E. He asked how far it went; they said this bed ran by Sheich Ali to those ruins, and quite away to the sea; so the river must have begun here, and not at Pharan, and the bed from Pharan here is only formed probably by winter torrents. If this is the bed of the river mentioned by St. Paul, as he thinks it is, we have the 2d rock: if it runs to the ruins, as is said, and there is no reason to doubt it, they will be pretty plainly those of Kadesh Barnea; and if this bed continues in the same course to the sea, as it probably does, this must be the river at Rinocolura, supposed, by Eratosthenes, to be formed by the Arabian lakes; because he did not know its miraculous head.

They went down a large valley to the west, towards the sea, and passed the head of a valley, a part of the desart of sin, which separates the mountains of Pharan from those which run along the coast, and the same plain which they had passed from Tor. They had scarcely entered these mountains, and travelled an hour, when after passing a mountain, where there were visible marks of an extinguished subterraneous fire, they saw, on their left hand, a small rock, with some unknown characters cut out on it, not stained as those hitherto met with; and, in ten minutes, they entered a valley 6 miles broad, running nearly north and south, with all the rocks, which enclose it on the west side, covered with characters. These are what are called Gebel El Macaatab, the written mountains. On examining these characters, he was greatly disappointed, in finding them every where interspersed with figures of men and beasts, which convinced him they were not written by the Israelites; for if they had been after the publication of the law, Moses would not have permitted them to engrave images, so immediately after he had received the second commandment; if they went this way, and not along the coast, they had then no characters, that we know of, unless some of them were skilled in hieroglyphics, and these have no connection with them. It will be difficult to guess what these inscriptions are; and probably, if ever it is discovered, they will be found scarcely worth the pains. If conjecture be permitted, he gives thoughts thus. They cannot have been written by Israelites, or Mahometans, for the above reason; and if by Mahometans, they would have some resemblance to some sorts of Cuphic characters, which were the characters used in the Arabic language, before the introduction of the present Arabic letters. The first MSS. of the Alcoran were in Cuphic: there is a very fine one at Cairo, which he could not purchase; for it is in the principal Mosque; and the Iman would not steal it for him, under

400 sequins, £200. These have not the least resemblance to them: Saracen characters are very unlike: besides, he places them higher than the Hegira. He thinks it then not unprobable, that they were written in the first ages of Christianity, and perhaps the very first; when probably pilgrimages from Jerusalem to Mount Sinai were fashionable, consequently frequent and numerous, by the new Christian Jews, who believed in Christ; therefore he believes them Hebrew characters, used vulgarly by the Jews about the time of Christ. He showed them when at Jerusalem to the Rabins; and they were of the same opinion. Here are on other parts of this rock, some Greek, and Arabic, as well as some Saracen inscriptions, and a Hebrew one, which is ושמו אחר. The Saracens and Arabic only say, "such a one was here at such a time;" the same say the Greek ones, except one, which says, "The evil genius of the army wrote this," which can only prove, that some body of Greeks was worsted here, after the characters were written, and that they attributed their defeat to some magic power in these characters. The characters seem to be of the very same kind with those stained on different parts of Mount Sinai, Meribah, &c. which the Bishop of Ossory has given.

The third day from this place, travelling westward, they encamped at Sarondou, or Korondel, where are the bitter waters, Marah. He tried if the branches of any of the trees had any effect on the waters; but found none: so the effect mentioned in scripture must have been miraculous. These waters at the spring are somewhat bitter and brackish, but as every foot they run over the sand is covered with bituminous salts, grown up by the excessive heat of the sun, they acquire much saltness, and bitterness, and very soon become not potable. This place, off which the ships cast anchor, is below a shoal sand, near the Birque Korondel. After 9 hours and a half march they arrived and encamped at the Desert of Shur, or Sour. The constant tradition is, that the Israelites ascended from the sea here; this is opposite to the plain Badeah, to which the above-mentioned pass in the mountains leads. From this place the openings in the mountains appear a wide crack, and may be called a mouth, taking Hiroth for an appellative. It would hardly have been necessary for the Israelites to pass the sea, if they were within 2 or 3 miles of the northern extremity of the gulf; the space of at most 2 miles, the breadth of the gulf at Suez, and at most 3 feet deep at low water, for it is then constantly waded over, could not have contained so many people, or drowned Pharaoh's army. There would have been little necessity for his cavalry and chariots to precipitate themselves after a number of people on foot; incumbered with their wives, children, and baggage; when they could soon have overtaken them with going so little about. These reasons, added to the significant names of the places, Tauriche Beni Israel, road of the children of Israel; Attacah, Deliverance, Pihahiroth, whether an appellative or significative; Badeah, new thing, or miracle; Bachorel Polsum, sea of destruction; convince him that the Israelites entered the sea at Badeah, and no where else. Besides, all the rest of the coast from Suez, and below Badeah, is steep rocks, so there must have been another miracle for them to descend: the current too sets from this place, where he encamped, toward the opposite shore, into the pool Birque Pharaone, Pool of Pharaoh, where, the tradition is, his host was drowned; a current, formed, he supposes, by the falling and rushing of one watery wall on the other, and driving it down. The Ain Mousa, which the Israelites would have met with, if they had passed at Suez, and the coast from hence southward, about a mile to Tor, being all rock and steep too, induce him to believe that they entered the sea at Badeah, and ascended from it here, and not at any other place. I only throw out what occurs to me, from the inspection of the country, an inspection as accurate as I am capable of. If any thing I have said can in the least support that revelation, to which I dare declare myself a friend, even in this enlightened age, I shall be very happy; or if this trip of mine can be of any use whatever, as I had great pleasure in it, I may truly say with Horace—*Omne tulit punctum*, &c.

The denomination of ים סוף, he thinks, only regards the hierapolitic branch, as the marine productions, madrepores, &c. which form admirable forests in the bottom of it, are not in the elanitic branch, or the gulf; viz. the broad part below Cape Mahomet. No more than that western branch was known to the Israelites at the time of their passage, if it was to the Egyptians: but the name descended to the whole, as their knowledge of it. The Red Sea seems to regard the broad part alone; for though there are not the above-mentioned sea productions, yet there is so great a quantity of tube coral (not found in the western branch of the Hierapolitic gulf) and such rocks, as one may say of them, that the Gedda ships fasten themselves to them, instead of casting anchor. It is of a deep red; so that possibly the first navigators entering at the strait of Babel Mandel, from the red they saw, called it the Red Sea, and that name descended to the whole with their navigation. This sea is tempestuous and full of shoals; there is no harbour on the Arabian coast after Tor, except one, viz. between Suez and Gidda or Mecca, which is a day and a half from Gidda. Gidda is its port; and there is only one on the other coast, Cossire; but it is a very bad one; however, ships sometimes go thither, and caravans cross the country to Morshout. The ships are, as the Bishop of Ossory has described them; the helm is on the outside, as I suppose, with his lordship, that of St. Paul was. They make use of but four sails, and no compass, nor do they ever cast the lead. They sail only by day-light, from anchoring place to anchoring place, and are not above 2 days out of sight of land, from Cape Mahomet to the Arabian main: if a gale happen, they are often lost; about 1 in 10 every year.

The second rock struck by Moses is, he thinks, 43 feet long, 16 broad, 13 high; it has two cracks, oblique ones; in them are some mouths, like those of Meribah: it is of a hard stone, not granite or marble.

IX. A Discovery, with Observations, of Two new Comets, in the Marine Observatory at Paris. By M. Messier, of Paris, F. R. S. From the French. p. 57.

The first of these comets Mr. M. discovered the 8th of March, 1766; and, from the observations he made of it, till the 15th, Mr. Pingre computed the elements of its orbit, as follows:

Place of the ascending node ☊	8	4°	10′	50″
Inclination of the orbit..................		40	50	20
Place of the perihelium	4	23	15	25
Logarithm of the perihelion dist.		9.703570.		

The comet passed its perihelion the 17th of February, at 8^h 50^m, mean time, at the meridian of Paris.

The motion of the comet retrograde.

The 2d comet Mr. M. discovered April 8, the same year 1766; which he continued to observe in the evenings till April 12. And from his observations, M. Pingre calculated the elements of this comet as follows:

Place of the ascending node	1^s	17°	22′	12″
Inclination of the orbit		8	18	45
Place of the perihelion....................	6	26	5	13
Perihelion distance.......................		0.636825		
Logarithm of the perihelion distance.......		9.804020		

It passed the perihelion April 17, 0^h 26^m 13^s mean time, at the meridian of Paris.

The motion of the comet direct. By these elements M. Pingré judged that this comet might be seen again in the morning, after getting clear of the sun's rays.

Mr. Pingré's remarks on the two comets of this year.—The elements of the first comet I give as absolutely certain, those of the 2d I cannot be so sure of. The interval was only 4 days between the first and the last observation. The last two days, and especially the last, the twilight and the moon-light must have produced some uncertainty in the observation. The ephemeris is founded on the certainty of the elements. Supposing there were no more than 3 or 4 minutes error in the last two observations, this would not much alter the theory from what I have given; but an alteration in the elements would produce 2 or 3 degrees difference in the place of the perihelion, which might be sufficient to render the reapparition of the comet uncertain in these high latitudes. Some German observations sent to Mr. Messier, made in the beginning of April, induce me to conclude, that the place of the perihelion ought really to be placed a little more eastward; but these observations were sent in so confused a manner, that it seems impossible to obtain the least light from them. Perhaps we may hereafter receive from some southern parts, observations sufficient to make out the true orbit of this comet.

X. Account of a Comet seen by Mr. Alexander Brice, dated Kirknewton, April 11, 1766. p. 66.

This comet Mr. B. observed, April 9 and 10, in the north-west, and very near the horizon. It began to appear at half an hour after 8 o'clock, and set 25 minutes after 9. The tail was very visible to the naked eye; but the nucleus could not be seen without a telescope, through which it appeared very distinctly, like a star of the 4th or 5th magnitude. It was surrounded with a gleam of light, like what is seen round the stars in Orion's sword, commonly called Janua Cœli. The tail stretched upwards, and inclined to the west, and was about 4 degrees long: the body of the comet was also 4 degrees distant from the new moon (then 34 hours after the change) and almost perpendicular above it; and it appeared to more advantage after the moon was set. The comet, when setting, was 37° to the north of due west, and 13° more northerly than the Pleiades, below them, but in the same tract: it was descending towards the sun, at the rate of 6°, as near as he could guess, in the space of 24 hours.

XI. A Report concerning the Microscope-Glasses, sent as a Present to the Royal Society, by Father di Torre of Naples, and referred to the Examination of Mr. Baker, F. R. S.* p. 67.

These microscopes are globules of glass, formed over a lamp, by Father di Torre, and ingeniously placed in cells of brass, adapted to Wilson's microscope. Four of these cells, thus furnished, were sent as a present from the Father to this Society, under the care of our late worthy member Sir Francis Eyles Stiles: but when they came into Mr. Baker's hands, one of these minute glasses was wanting, having probably been shaken out of its cell in carriage: the loss, however, signifies little, as there remains another of the same magnifying power. These globules are wonderfully small: the largest being in diameter only 2 Paris points, and said to magnify the diameter of an object 640 times; the second is the size of one Paris point, magnifying the diameter of an object 1280 times; and the third is so extremely minute, as to be no more than one half of a Paris point, or the 144th part of an inch in diameter, and is said to magnify the diameter of an object 2560 times, and consequently it must magnify the square of such diameter 6,553,600 times.

Now as the focus of a glass globule is at the distance of ¼th of its diameter, it is with the utmost difficulty that globules so minute as these can be employed to any purpose. For instance, the focus of that globule, whose diameter is but one half of a Paris point (or the 144th part of an inch) is no farther from the object to be examined, than the 576th part of an inch. In attempting to find

* Vid. p. 245 of this vol.

this focus, it is scarcely possible to avoid touching the object with the glass, if it be not placed between laminæ of talc or isinglass; and if it be so placed, even the thinnest talc bears a considerable proportion to this 576th part of an inch, and will prove an unsurmountable obstacle to the seeing any object, unless by some very happy accident. The other globules, whose focus is not quite so near, are liable proportionally to the same inconvenience.

The very great power of magnifying glass globules is sufficiently well known: many years ago they were much used, and highly boasted of on that account. But they now, long since, have been laid aside, and convex lenses substituted in their room; and that with very good reason, from the difficulty in the application of such globules, from the deficiency of light, from the distortion of the image seen, from the painful straining of the eyes, and from the boundless latitude given to imagination and conjecture, for want of sufficient distinctness and precision. Nothing can be more injudicious than the desire of such excessive magnifying power: whenever we can see an object clearly and well defined, we ought to be contented; all beyond this there is no dependence on. In some letters, sent with these glasses, the society has been favoured with uncommon observations on the globules of the blood, described as having been viewed (it is not said by these glasses) floating in the serum, and sometimes changing their figure: and also with a long account of the impregnation of vegetables; where we are told, that the exquisitely minute corpuscles or seminal particles, emitted by the grains of the farina fœcundans, have been seen to enter into, and be conveyed along tubes exceedingly small, which at the time dilated and contracted occasionally to convey them to the ovarium.* Mr. Baker was extremely desirous to repeat these experiments: but as it was absolutely necessary to spread the blood as thin as possible, to render it very transparent, without which nothing can be seen by such small glasses, he could not possibly prevent its becoming quite dry, before he could apply it to the eye, and consequently was unable to perceive any floating globules: and though he has been many years conversant with microscopes, he has not been able to contrive any method of applying the parts of generation of plants in such manner, to these glasses, as to view this wonderful impregnation.

It is, however, proper to take notice, that in these letters an apparatus is described, to be added to Wilson's microscope, when these glasses are made use

* It must be observed here, in justice to Mr. Turberville Needham, F. R. S. that he was the person who first discovered, that, on applying water to the farina fœcundans, many of its grains emitted streams of exquisitely minute globules, as if through a small aperture: this he published in the year 1745, and from thence imagined the impregnation of plants to be carried on in a manner somewhat similar to that in the account referred to; but the same justice must allow, that before Father di Torre, nobody is supposed to have seen these several progressions towards impregnation.—Orig.

of; which apparatus Mr. Baker was not at the expence of procuring, as it would answer no other purpose: but the method he contrived, instead thereof, he imagines to be equally effectual. In truth, Mr. Baker has employed much time and his best endeavours in the examination of these glasses, as they were supposed capable of such wonderful discoveries: and that as well by candle-light, as (by what is recommended) the strongest day-light: and yet he must declare, with some concern, that through the smallest globule, viz, of one half of a Paris point in diameter, he has not been able to distinguish any thing; and even through that which magnifies the least, he could never view any object with satisfaction; though he applied the most minute, and consequently the properest objects for these glasses, viz. the globules of the blood, the farina of vegetables, the seeds of mushrooms, the feathers of butterflies, pepper-water, &c. He hopes his eyes are not injured by these examinations, as they have been much used to microscopes; but he believes there are very few who would not have been nearly blinded by them. On the whole,—Mr. Baker thinks the R. S. much obliged to the Father di Torre for these specimens of his great dexterity, ingenuity, and patience, in forming and setting glass spheres thus extremely minute; but he considers them as matters of curiosity rather than of real use.

XII. On the Transit of Venus over the Sun, June 6, 1761. By Frederick Mallet, Astron. Royal, Upsal. From the Latin. p. 72.

From these observations very few data are obtained. It hence appears that the first exterior contact was doubtful; but that the interior contact was at $3^h\ 38^m\ 2^s$; the interior egress at $9^h\ 28^m\ 0^s$; and exterior egress, or last contact, at $9^h\ 46^m\ 29^s$.

XIII. A Hepatitis, with Unfavourable Symptoms, treated by Robert Smith, Surgeon at Edinburgh, now at Leicester. p. 92.

In this patient, (Mrs. Morton, aged 26,) after an attack of hepatitis, in the summer of 1750, there arose a tumor on the anterior part of the liver, of an oblong figure, and which extended its longest diameter across the epigastrium about 7 inches. The patient, greatly debilitated by the large evacuations and fever, became so low and dispirited, that she had given over all thoughts of recovery. To Dr. J. Dundas, an eminent physician, who had occasionally attended, Mr. S. proposed making an incision into the tumor; though the event, under the present circumstances, had but an indifferent aspect. This proposal was, however, approved of by the doctor, the patient, and her relations, under the following terms, viz. to have the opinion of the principal surgeon or surgeons in that city on the expediency of the operation, in order that, should the experiment prove unsuccessful, there might be no blame imputed afterwards.

Strong suppuratives, in the form of cataplasms, were now used, whereby the tumor became more prominent in 2 days; a very deep fluctuation being felt, a large caustic was applied on the most depending posterior side, thereby to avoid hurting the stomach or its appendages by an incision, which was made several hours after, from whence issued a copious discharge, at first purulent, at last glutinous, resembling the white of an egg: no adhesion to the peritoneum could be felt, though accurately tried all round with the finger. Great care was used in the proper applications, bandage, &c. particularly in the posture of the patient; ivory and silver flat cannulas, kept in the aperture, were materially beneficial, as well for the conveyance of balsamic injections, as to facilitate the exit of the putrid contents. The night after the operation, she turned delirious; this symptom, with an increased fever and excessive cough, afforded little or no hopes of recovery, the more especially, as the discharge was now turned excessively thin, of a dusky colour, and very fœtid; for these reasons, Mr. S. dressed her twice a day, throwing in large quantities of a warm injection, composed of a decoct. ficuum, and rad. alth. wherein was dissolved bals. capiv. to which was added, when the fever abated, some calomel ppt. In the mean time medicines internally, to allay her fever and cough, were not neglected; and she afterwards took daily, as her stomach and other symptoms would admit, a light infusion of cort. peruv.

By these means strictly followed, about the 21st day from that of the incision, a laudable pus was obtained; but on the 23d, a thin sanious discharge in great quantities burst out worse than the former, and extremely fœtid. Towards the end of the month, it began once more to assume a benign aspect, but broke out a 3d and 4th time, on the 1st and 15th of Sept., every time the discharge growing more and more acrid, so as to excoriate and inflame the external parts; notwithstanding these threatenings, by a close perseverance in the forementioned method, at the end of 10 weeks, a callous cicatrix was obtained on the external wound, and the recovery compleated soon after by the use of a few alterative mercurial pills.

The woman was alive in 1766, and enjoyed a middling state of health; only had been liable to complaints of gripes and indigestion, every 3, 4, or 5 months. Her last complaint was generally relieved by a few saponaceous pills.

XIV. Experiments on the Peruvian Bark, by Arthur Lee, M. D. p. 95.

As these experiments do not lead to any result of importance, that was not before known, relative to the chemical composition of the Peruvian bark, or to its pharmaceutical preparations, it was deemed unnecessary to reprint them.

XV. Specimen of some New Electrical Experiments. By John Baptist Beccaria, F. R. S.* p. 105.

These experiments may be consulted to more advantage in the author's published works on electricity, which are very curious and valuable.

XVI. Proposal of a Method for Measuring Degrees of Longitude on Parallels of the Equator. By J. Michell, B. D., F. R. S. p. 119.

There have been already several attempts made towards discovering the figure of the earth, by measuring the length of a degree of the meridian in different latitudes: now if these measures had been sufficiently accurate and numerous, and we could also depend on the uniformity of the earth's surface, we might then immediately discover from them the form sought; but these measures, not agreeing exactly to any certain rule, leave us still in some degree at a loss. It is therefore much to be wished, that more measurements of degrees on the meridian were to be made, in order to determine with greater accuracy a question of this importance. But what would tend yet more to determine this matter, would be the measurement of degrees of longitude as well as those of latitude. Astronomers have indeed expressed their wishes that this might be done, and though no attempt has been hitherto made towards it, yet as it is probable, that such measurements may some time or other take place, it will not be amiss to suggest a method, which will admit of more exactness than any I have seen proposed for this purpose, all of which, depending on an observation of the time, are therefore liable to an error of 15 seconds of a degree for every second of time; but the method Mr. M. recommends, stands on the same foundation with the measurement of a degree of the meridian, and, the instruments being equally good, and the number of miles to be measured the same, the exactness of it, to that of a degree of the meridian, will be in the proportion of the sine of the latitude to the radius very nearly.

In pl. 5, fig. 11, let AB represent the equator; P the pole; DLEF a parallel of the equator; PEC a meridian passing through the station E; PLMN a meridian passing through another station M; and let AMEB be a great circle cutting the meridian PEC at right angles in the point E.

Then in the spherical triangle AMN, right angled at N, we shall have R : cos.

* J. B. Beccaria was successively professor of philosophy at Palermo, Rome, and Turin, where he was in great favour with the King of Sardinia, and became tutor to his sons. He was an ingenious philosopher, and published some valuable works, particularly on electricity, in which he was more remarkably distinguished. He published also an account of his measurement of a degree of the meridian near Turin, which he carefully executed between the years 1760 and 1764. Mr. B. was a native of Mondovi in Piedmont; and he died in 1781; but at what age does not appear.

AM (sin. ME) : : tan. MAN : co-tan. AMN : hence $\frac{\text{tan. MAN}}{\text{R}} \times$ sin. ME = co-tan. AMN; but tan. MAN being the tangent of the latitude of the given place E, and therefore given, the quantity $\frac{\text{tan. MAN}}{\text{R}}$ will also be given, and greater or less than unity in the proportion of the tan. of the latitude to the R. The co-tan. therefore of the angle AMN, that is the tan. of the complement of the angle AMN to 90°, will be greater or less than the sine of the arc ME, in the proportion of the tan. of the latitude of the place, to the R. And consequently, while the arc ME is small (in which case the sine, arc, and tangent differ very little from each other) the angular deviation of the intersection of the meridian PLMN with the great circle AMEB, from a right angle, will contain more or fewer degrees, &c. than the arc ME, nearly in the same proportion of the tan. of the latitude of the place to the R.

By this means then, the latitude of the place and the angle PME (contained between the meridian PMN and the great circle AMB) being given, the length of the arc ME will likewise be given, with great exactness. But as the angles PEM and PME must be taken by the observation of some star near the pole, they will be less accurate, when reduced to the plane of the horizon, than at the pole, in the proportion of the sine of the distance between the pole and zenith, that is the cos. of the latitude to the R, which with the proportion just mentioned of the tan. of the latitude to the R, makes the accuracy of this method on the whole, when compared with that of the measurement of a degree of the meridian, in the proportion of the tan. multiplied into the cos. of the latitude, to the square of the R very nearly; but the tan. of any angle into its cos. is equal to the sin. into the R. whence this proportion is the same as the sin. into the R. to the square of the R. and dividing both by the R. simply as the sin. of the latitude, to the R. as above.

Having got the length of the arc ME, of a great circle, in degrees, &c. together with the distance of the two stations M and E, it is easy to conclude from these the length of a degree of the parallel of latitude, at the place of observation, which will be the same, without sensible error, as it would be, supposing the earth was an exact sphere, to the same scale, with the degree of a great circle just found.

For, in fig. 12, let APB represent a section of the earth through its axis PCH; ACB an equatorial diameter; AD the radius of curvature at the point A; and PH the radius of curvature at the point P; DFH the evolute of the curve AEP; EF the radius of curvature at the point E; which suppose to have the same latitude with the point E in fig. 11; and let EF be produced till it cuts the axis PH in G: then with the radius EG and centre G, describe the arc IEK, which will be the least circle that can touch the curve AEP at the point E, without cutting it.

Let now the curve PEA, the line EG, and the arc IEK revolve about PH as an axis; then, PE being equivalent to PE in the former figure, the point E in the latter figure will describe the parallel DEF in the former; AEP at the same time describing the surface of the earth, and IK describing a portion of a sphere, which will be every where a tangent to the parallel DEF, and whose centre will be G. The curvature therefore of this sphere will be less than the curvature of the earth, in the direction of the meridian, at the point E, as the radius GE is greater than the radius FE; but this, in moderate distances, can cause no sensible error. The difference between AD, the radius of curvature at the point A, on the earth's surface, and the line AC, according to that hypothesis, which makes it the greatest, does not exceed $\frac{1}{60}$ part of the whole; and on the same hypothesis, the part FG of the line EG, supposing E to be in the latitude of 45°, would not exceed $\frac{1}{130}$ part of the whole. If then we take any other point on the surface of the earth, as M, at a small distance from E, the distance between that point and the sphere described by the arc IK, will be only $\frac{1}{130}$ part of the versed sine of the arc EM; and the perpendicular standing on the surface of the earth at M, will be inclined to the perpendicular standing on the sphere, in an angle, which is equal to $\frac{1}{130}$ part of the angle subtended by the arc EM. And in higher latitudes these quantities will be still less. Let us now return to fig. 11, and supposing the point E to be situated in latitude 45°, let the arc EM, cutting PE at right angles, consist of 2°, near 140 statute miles; then will the side PM, of the triangle PME, consist of 45° 2′ $5\frac{1}{2}$″, and consequently, if LM in fig. 11, be supposed to correspond to EM in fig. 12, the distance of these two points E and M, in the latter figure, will be only 2′ $5\frac{1}{2}$″, the $\frac{1}{130}$ part of the versed sine of which is a little more than $\frac{1}{3}$ of an inch, to the radius of the earth, which will therefore be the distance of the point M on the earth's surface, and the point of the imaginary sphere, described by IK, immediately over it. Hence also the inclination of the real perpendicular at M, and the imaginary one standing on the arc IK, at the same place, to each other, will be something less than a second, a quantity in itself almost too small to be regarded, unless the instruments made use of are both very large and very excellent in their kinds, and which, being wholly in the plane of the meridian, will produce an error, that must be perfectly insensible, with any instruments whatever, in an observation of the angle PME, fig. 11, which will therefore, to all intents and purposes, be the same, as if the curvature of the earth in the direction of the meridian, and in the direction of ME or LE were accurately the same.

I have supposed the arc ME to stand at right angles to the meridian PE, which passes through one of the extreme stations; the method here proposed is however liable to the least error, when the meridian cuts the arc to be measured at right angles in the middle of it; but this makes so very small a difference, that it is not worth regarding; nor is it indeed necessary that the arc should not deviate 2 or 3 degrees from right angles with the meridian, at that end where it cuts it most nearly at right angles, in case the situation and circumstances of the country should make this more convenient, the errors, that would be occasioned by such a deviation, being too small to affect the conclusion. And if this deviation was still much greater, and the length of a degree of the meridian at the same place was known, it would be very easy to make the necessary corrections.

It will perhaps be objected, that the method above proposed depends, in some measure, on time, as well as others, the finding of the meridian not being to be performed without it; but it must be observed, that the motion of the pole star, by which it is proposed to find the meridian, being slower than that of a star at the equator, nearly in the proportion of 30 to 1, this method will admit of an exactness greater in the same proportion (except the reduction of the sin. to the R. before-mentioned) than those observations, by which we endeavour to find the difference of the longitude of two places, by the difference of the time of the sun or a star's coming to their respective meridians.

The method above proposed will also require different instruments from those commonly in use; but admitting that instruments of equal radius are capable of equal exactness, this method will admit of the same exactness with the observations of a degree of the meridian, except the before-mentioned limitation. Nor would the instruments for this purpose, if well contrived, be either less portable, or more expensive, than those for measuring a degree on the meridian; the same telescope which would be necessary for finding the meridian, would serve likewise for tracing the arc of a great circle. It may further be observed that by means of the above-mentioned method, a country not too near the equator, nor attended with any other unfavourable circumstances, might be laid down with great exactness. By running a great circle nearly E. and W. through the midst of it, we should get the longitude of all the places the great circle passed over; and if, by means of the meridian telescope, we should trace meridians through a few of these places, as far N. and S. as the survey was intended to be carried, we should then have a number of stations, in several parts of the country, whose longitudes, with respect to each other, would be very accurately determined, and to which other places might easily be referred, when the length of a degree of longitude in those situations was known.

XVII. Observationes de Ascaridibus et Cucurbitinis, et potissimum de Taenia, tam humana quam leporina. p. 126.

In the present advanced state of knowledge respecting the different species of worms found in the intestines of man and other animals, there is nothing in this paper sufficiently interesting for republication.

XVIII. Of an Uncommon Large Hernia. By Dr. George Carlisle. p. 133.

John Hallowday, an out pensioner of Chelsea, aged near 80, having entered very young into the army, and undergone several hardships in the campaigns under the Duke of Marlborough, on his return to England from Flanders, at the conclusion of the war, first perceived a small tumour in the right side of the scrotum and groin. This he carefully concealed, to avoid the scoffs of his companions, and least it might be the occasion of his discharge, which he wanted to avoid; as he found no other inconvenience from it, but what its bulk occasioned, nor ever had pain, vomiting, obstructions to stools, or any other symptoms of a strangulated hernia. From that time, however, it continued to increase in bulk; and from that, and its weight, grew daily more inconvenient to him, insomuch, that about the year 1725, being unable to go through the duty of a soldier, he was admitted to the out-pension of Chelsea hospital. Its size was then such, that he was obliged to have a particular bag made in the forepart of his breeches, to enable him to carry about its weight, and always wore a leather apron to conceal its figure. For 6 or 7 years before his death, the weight and bulk of the hernia had made such an alteration, in the outward appearance of the parts about the scrotum, that the penis was entirely buried in the tumour; a small oval opening only was left, out of which the urine was discharged: this opening was sometimes excoriated, from the acrimony of the urine, as the penis could not be extracted to throw it off, nor the glans be made to appear by any endeavours. A year or two before his death, after a cold, and fretting the part by too much walking, the urine had brought on a considerable inflammation, which mortified to a large extent, one considerable eschar, formed on the anterior and most depending part of the bag, one less on the right side where it touched the thigh, and a third behind; yet all cast off and healed kindly, by the help of the bark, warm dressings, &c. Except from this accident, in the latter years of his life, he was not subject to any other complaints than are common at his years; such as dimness of sight, catarrhous coughs, shortness of breath on motion, swellings of his legs occasionally; and he wore off at last by a gentle decay, having all along as good an appetite, and digestion, as could be expected at his time of life; regular discharges, both by stool and urine; very rarely vomitings, except from overloading his stomach; purgatives, and every other medicine, operated as regularly on him as on any other person. He was a well made man, rather above the middle size; was as corpulent, and had as much strength, as most of his years, until within a very little time of his death.

The large hernial bag Dr. C. had measured as exactly as he could, about a year before his death; and found its length, from the os pubis to the most depending point, 15 inches; its greatest breadth, while it lay supported by the thighs, 17½ inches; and its greatest circumference 34 inches; but in the body, the day after death, its length, from the pubis to the most depending part, was only 13 inches; its breadth, to the part where it fell in between the thighs, 12 inches; its circumference round the thick or smallest part, where it descended from the pubis, 19 inches; and round its large circumference, 27 inches. It was covered with the common integuments of the scrotum; but at its lower and posterior parts, the cellular membrane, or dartos, was reduced to an almost cartilaginous hardness, where the weight, both in sitting, standing, and lying, had the greatest effect; the cicatrices also, where gangrenous sloughs had been cast off, were of an equal firmness and hardness under the knife. The testicle of the left side was plainly to be felt, at the prominent part above, and to one side of the opening for the penis, not far from its natural situation, the right testicle was obscurely to be felt, a little above the lowest and anterior point of the bag. Besides what appeared on the front view of the bag, a large portion of it, like a ridge, extended backwards, where the space between the thighs allowed it more room; they being rather more concave than usual inwardly towards each other; and more distant, from the constant pressure they sustained. The colour of the bag was the same as that of the other parts, except where the mortified sloughs had been cast off, where it was of a shining white. On opening the abdomen the liver appeared rather large, and farther extended over the left side than usual. The gall-bladder was small, with a little diluted bile in it. None of the intestines appeared, but a portion of the colon, towards the anterior edge of the pelvis, on the left side; where it made 2 inflections, much in the way as the lowest turns of the intestines are shown to do, below the omentum, in Eustach. tab. 9, from these it went downwards, and backwards, into the pelvis, to make its last curve, and be continued into the rectum; which, with that last curve of the colon, was in its natural place and direction. The stomach, instead of an horizontal, had a longitudinal position; its large, and here upper extremity, being placed behind the left lobe of the liver, close to the diaphragm, and its large convex side lay along the left side of the abdomen; it descended to near the crest of the os ilium, whence it turned over the inflection of the colon before-mentioned, across the pelvis, to the large hernial aperture, in the right side; within the verge of which, it ran downwards about an inch, then ascended, and made a semicircular turn to the pylorus, which mounted towards the abdomen; thence the beginning of the duodenum made another turn, to descend into the hernial bag; immediately below which, viz. just within the opening of the hernial bag, the ductus communis choledochus entered it; and seemed the cause which kept it from falling farther into the sac. From this, the remainder of the duodenum, and all the other intestines, were entirely contained in the hernial bag, to near the extremity of the colon before-mentioned. The duodenum, after entering the sac, first ran a little downwards, and backwards, then horizontally, and lastly upwards, to within the edge of the sac, towards the abdomen; thence the tegumen proceeded backwards and downwards, and then formed, with the ileum, pretty near their usual convolutions, about the middle of the tumour, as they should have done in the abdomen. The cæcum had a very small appendix, but was itself very large; as was the colon through its whole length, while contained in the sac; that part of it which returned into the pelvis again, being much smaller, even only of the dimensions of the smaller intestines: the length of the colon too seemed more than usual. The cæcum began in the lower part of the bag, and thence the colon kept pretty near the course it should have kept, if the bag had been the abdomen, for a great part of its length; running up from the cæcum along the right side of the bag, to near the pubis, and then crossing over towards the left side, before the duodenum, to the left edge of the hernial aperture; at which place, slipping behind the lower extremity of the stomach, it appeared in the pelvis, crossing over to its left side: thence to follow the course before described. The pancreas lay in a longitudinal direction, along the concave arch of the stomach, through its whole course: and was placed before the bodies of the vertebræ. The ductus choledochus, besides its great length from the liver, to within the hernial bag, was of such a width as easily to admit a middle-sized finger, being about 2¼ inches in circumference: in some parts of its course, it was little inferior in width to the gall-bladder, in this same subject. The kidneys were rather small, in general sound, except that some few hydatides were here and there fixed on their outer surface, and that 2 or 3 steatomatous tumours, of about the size of a pea, and white, were in the substance of each: but not rising above their surface: they were each in their proper situation; the left lay behind the stomach, and was less, probably from the pressure it was exposed to. The ureters and bladder were in their usual situation; the bladder was no way engaged in the hernia, and a catheter was pretty easily introduced, through the concealed penis, into it. The spleen was small, in its natural situation, and sound. The mesenteric glands were numerous, large, hardened, and surrounded with a fat of a deep yellow, as was the pancreas; no omentum appeared; its place seemed supplied by the fat interspersed among the glands and pancreas. The testes were of a natural size, but loose and flabby, and had many varicose veins on their surface: the right, which was so much out of its proper place, was the least, and laxer of the two: the spermatic vessels belonging to it were large, through the great length they ran. The sac and intestines were adherent, almost at every point of their contact; in some places so firmly, that they were with great difficulty separated, and often not without danger of tearing: the intestines also adhered in the same manner to each other; all, by means of a firm cellular membrane. The containing bag was very firm, thick, and strong, as before observed. Its aperture, at the right ring from the abdomen, was so wide, as readily to allow a middle-sized hand to pass through it, from the abdomen, for a small space, between its anterior edge and the convolutions of the lower extremity of the stomach, and the semicircular turn it made to the pylorus, with the beginning of the duodenum from thence, and the other extremity of the duodenum, before the jejunum commenced, and that part of the colon which returned into the pelvis; all of which were lodged in the very aperture: so that the space left unoccupied by these parts could not be much less than 8 inches in circumference: notwithstanding which, very little of a watery fluid was found in the sac: indeed it would not have had a very easy admittance, from the many adhesions formed between the sac and its contained parts, a little below the opening from the abdomen.

XIX. Three Papers, containing Experiments on Factitious Air. By the Hon. Henry Cavendish, F. R. S. p. 141.

By factitious air, says Mr. C., I mean in general any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art. By fixed air, I mean that particular species of factitious air, which is separated from alkaline substances by solution in acids, or by calcination; and to which Dr. Black has given that name in his treatise on quicklime. As fixed air makes a considerable part of the subject of the following papers; and as the name might incline one to think that it signified any sort of air which is contained in other bodies in an unelastic form; I thought it best to give this explanation before I went any farther.

Before proceeding to the experiments themselves, it will be proper to mention the principal methods used in making them. In order to fill a bottle with the air discharged from metals or alkaline substances by solution in acids, or from animal or vegetable substances by fermentation, I make use of the contrivance represented in pl. 5, fig. 13, where A represents the bottle, in which the materials for producing air are placed; having a bent glass tube C ground into it, in the manner of a stopper; E represents a vessel of water; D the bottle to receive the air, which is first filled with water, and then inverted into the vessel of water, over the end of the bent tube; FF represents the string by which the bottle is suspended. When I would measure the quantity of air produced by any of these substances, I commonly do it by receiving the air in a bottle, which has divisions marked on its sides with a diamond, showing the weight of water required to fill the bottle up to those divisions: but sometimes I do it by making a mark on the side of the bottle in which I have received the air, answering to the surface of the water in it; and then, setting it upright, find how much water it requires to fill it up to that mark.

In order to transfer the air out of one bottle into another, the simplest way, and that which I have oftenest made use of, is that represented fig. 14; where A is the bottle, into which the air is to be transferred; it is supposed to be filled with water and inverted into the vessel of water DEFG, and suspended there by a string; the line DG is the surface of the water: B represents a tin funnel held under the mouth of the bottle: C represents the inverted bottle, out of which the air is to be transferred; the mouth of which is lifted up till the air runs out of it into the funnel, and thence into the bottle A.

In order to transfer air out of a bottle into a bladder, the contrivance fig. 15 is made use of. A is the bottle out of which the air is to be transferred, inverted into the vessel of water FGHK: B is a bladder, whose neck is tied fast over the hollow piece of wood CC, so as to be air-tight. Into the piece of wood is run a bent pewter pipe D, and secured with lute.* The air is then pressed out of the bladder as well as possible, and a bit of wax E stuck on the other end of the pipe, so as to stop up the orifice. The pipe, with the wax on it, is then run up into the inverted bottle, and the wax torn off by rubbing it against the sides. By this means, the end of the pipe is introduced within the bottle, without suffering any water to get within it. Then, by letting the bottle descend, so as to be totally immersed in the water, the air is forced into the bladder.

The weights used in the following experiments, are Troy weights, 1 ounce containing 480 grains. By an ounce or grain measure, I mean such a measure as contains one ounce or grain Troy of water.

PART I.—*Containing Experiments on Inflammable Air.*

I know of only three metallic substances, namely, zinc, iron, and tin, that generate inflammable air by solution in acids: and those only by solution in the diluted vitriolic acid, or spirit of salt. Zinc dissolves with great rapidity in both these acids; and, unless they are very much diluted, generates a considerable heat. One ounce of zinc produces about 356 ounce measures of air, the quantity seems just the same whichever of these acids it is dissolved in. Iron dissolves readily in the diluted vitriolic acid, but not near so readily as zinc. One ounce of iron wire produces about 412 ounce measures of air: the quantity was just the same, whether the oil of vitriol was diluted with $1\frac{1}{4}$, or 7 times its weight of water: so that the quantity of air produced seems not at all to depend on the strength of the acid. Iron dissolves but slowly in spirit of salt while cold: with the assistance of heat it dissolves moderately fast. The air thus produced is inflammable; but I have not tried how much it produces.

* The lute used for this purpose, as well as in all the following experiments, is composed of almond powder, made into a paste with glue, and beat a good deal with a heavy hammer. This is the strongest and most convenient lute I know of. A tube may be cemented with it to the mouth of a bottle, so as not to suffer any air to escape at the joint; though the air within is compressed by the weight of several inches of water.—Orig.

Tin was found to dissolve scarcely at all in oil of vitriol diluted with an equal weight of water, while cold; with the assistance of a moderate heat it dissolved slowly, and generated air, which was inflammable; the quantity was not ascertained. Tin dissolves slowly in strong spirit of salt while cold: with the assistance of heat it dissolves moderately fast. One ounce of tinfoil yields 202 ounce measures of inflammable air. These experiments were made when the thermometer was at 50° and the barometer at 30 inches.

All these three metallic substances dissolve readily in the nitrous acid, and generate air, but the air is not at all inflammable. They also unite readily, with the assistance of heat, to the undiluted acid of vitriol; but very little of the salt, formed by their union with the acid, dissolves in the fluid. They all unite to the acid with a considerable effervescence, and discharge plenty of vapours, which smell strongly of the volatile sulphureous acid, and which are not at all inflammable. Iron is not sensibly acted on by this acid, without the assistance of heat, but zinc and tin are in some measure acted on by it, while cold. Hence it seems likely, that when either of the above-mentioned metallic substances are dissolved in spirit of salt, or the diluted vitriolic acid, their phlogiston flies off, without having its nature changed by the acid, and forms the inflammable air; but that when they are dissolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiston unites to part of the acid used for their solution, and flies off with it in fumes, the phlogiston losing its inflammable property by the union. The volatile sulphureous fumes, produced by uniting these metallic substances by heat to the undiluted vitriolic acid, show plainly that in this case their phlogiston unites to the acid; for it is well known, that the vitriolic sulphureous acid consists of the plain vitriolic acid united to phlogiston.* It is highly probable too, that the same thing happens in dissolving these metallic substances in the nitrous acid; as the fumes produced during the solution appear plainly to consist in great measure of the nitrous acid, and yet it appears, from their more penetrating smell and other reasons, that the acid must have undergone some change in its nature, which can hardly be attributed to any thing else than its union with the phlogiston. As to the inflammable air, produced by dissolving these substances in spirit of salt or the diluted vitriolic acid, there is great reason to think that it does not contain any of the acid in its composition; not only because it seems to be just the same whichever of these acids it is produced by; but also because there is an inflammable air, seemingly much of the same kind as this, produced

* Sulphur is allowed by chemists, to consist of the plain vitriolic acid united to phlogiston. The volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur. A circumstance which I think shows the truth of this, is that if oil of vitriol be distilled from sulphur, the liquor which comes over will be the volatile sulphureous acid.—Orig.

from animal substances in putrefaction, and from vegetable substances in distillation, as will be shown hereafter; though there can be no reason to suppose that this kind of inflammable air owes its production to any acid. I now proceed to the experiments made on inflammable air.

I cannot find that this air has any tendency to lose its elasticity by keeping, or that it is at all absorbed, either by water, or by fixed or volatile alkalies; as I have kept some by me for several weeks in a bottle inverted into a vessel of water, without any sensible decrease of bulk; and as I have also kept some for a few days, in bottles inverted into vessels of soap lees and spirit of sal ammoniac, without perceiving their bulk to be at all diminished.

It has been observed by others, that when a piece of lighted paper is applied to the mouth of a bottle, containing a mixture of inflammable and common air, the air takes fire, and goes off with an explosion. In order to observe in what manner the effect varies according to the different proportions in which they are mixed, the following experiment was made. Some of the inflammable air, produced by dissolving zinc in diluted oil of vitriol, was mixed with common air in several different proportions, and the inflammability of these mixtures tried one after the other in this manner. A quart bottle was filled with one of these mixtures, in the manner represented in fig. 14. The bottle was then taken out of the water, set upright on a table, and the flame of a lamp or piece of lighted paper applied to its mouth. But, in order to prevent the included air from mixing with the outer air, before the flame could be applied, the mouth of the bottle was covered, while under water, with a cap made of a piece of wood covered with a few folds of linen; which cap was not removed till the instant that the flame was applied. The mixtures were all tried in the same bottle; and as they were all ready prepared, before the inflammability of any of them was tried, the time elapsed between each trial was but small: by which means I was better able to compare the loudness of the sound in each trial. The result of the experiment is as follows.

With one part of inflammable air to 9 of common air, the mixture would not take fire, on applying the lighted paper to the mouth of the bottle; but on putting it down into the belly of the bottle, the air took fire, but made very little sound. With 2 parts of inflammable to 8 of common air, it took fire immediately, on applying the flame to the mouth of the bottle, and went off with a moderately loud noise. With 3 parts of inflammable air to 7 of common air, there was a very loud noise. With 4 parts of inflammable to 6 of common air, the sound seemed very little louder. With equal quantities of inflammable and common air, the sound seemed much the same. In the first of these trials, namely, that with one part of inflammable to 9 of common air, the mixture did not take fire all at once, on putting the lighted paper into the bottle; but one might perceive the flame to spread gradually through the bottle. In the next 3 trials, though they made an explosion, yet I could not perceive any light within the bottle. In all probability, the flame spread so instantly through the bottle, and was so soon over, that it had not time to make any impression on my eye. In the last mentioned trial, namely, that with equal quantities of inflammable and common air, a light was seen in the bottle, but which quickly ceased.

With 6 parts of inflammable to 4 of common air, the sound was not very loud: the mixture continued burning a short time in the bottle after the sound was over. With 7 parts of inflammable to 3 of common air, there was a very gentle bounce or rather puff: it continued burning for some seconds in the belly of the bottle. A mixture of 8 parts of inflammable to 2 of common air caught fire on applying the flame, but without any noise: it continued burning for some time in the neck of the bottle, and then went out, without the flame ever extending into the belly of the bottle. It appears from these experiments, that this air, like other inflammable substances, cannot burn without the assistance of common air. It seems too that, unless the mixture contains more common than inflammable air, the common air therein is not sufficient to consume the whole of the inflammable air; by which part of the inflammable air remains, and burns by means of the common air, which rushes into the bottle after the explosion.

In order to find whether there was any difference in point of inflammability between the air produced from different metals by different acids, 5 different sorts of air, namely, 1. Some produced from zinc by diluted oil of vitriol, and which had been kept about a fortnight; 2. Some of the same kind of air fresh made; 3. Air produced from zinc by spirit of salt; 4. Air from iron by the vitriolic acid; 5. Air from tin by spirit of salt; were each mixed separately with common air in the proportion of 2 parts of inflammable air to $7\frac{7}{10}$ of common air, and their inflammability tried in the same bottle that was used for the former experiment, and with the same precautions. They each went off with a pretty loud noise, and without any difference in the sound that I could be sure of. Some more of each of the above parcels of air were then mixed with common air, in the proportion of 7 parts of inflammable air to $3\frac{1}{2}$ of common air, and tried in the same way as before. Each of them went off with a gentle bounce, and burnt some time in the bottle, without my being able to perceive any difference between them.

To avoid being hurt, in case the bottle should burst by the explosion, I have commonly, in making these sort of experiments, made use of an apparatus contrived in such manner, that by pulling a string, I drew the flame of a lamp over the mouth of the bottle, and at the same time pulled off the cap, while I stood out of the reach of danger. I believe however that this precaution is not very necessary, as I have never known a bottle to burst in any of the trials I have made.

The specific gravity of each of the above-mentioned sorts of inflammable air, except the first, was tried in the following manner. A bladder holding about 100 ounce measures was filled with inflammable air, in the manner represented in fig. 15, and the air pressed out again as perfectly as possible. By this means the small quantity of air remaining in the bladder was almost entirely of the inflammable kind. 80 ounce measures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the same manner: after which, the pewter pipe was taken out of the wooden cap of the bladder, the orifice of the cap stopped up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air pressed out as perfectly as possible, and the bladder weighed again. It was found to have increased in weight $40\frac{3}{4}$ grains. Therefore the air pressed out of the bladder weighs $40\frac{3}{4}$ grains less than an equal quantity of common air; but the quantity of air pressed out of the bladder must be nearly the same as that which was forced into it, i. e. 80 ounce measures: consequently 80 ounce measures of this sort of inflammable air weigh $40\frac{3}{4}$ grains less than an equal bulk of common air. The three other sorts of inflammable air were then tried in the same way, in the same bladder, immediately one after the other. In the trial with the air from zinc by spirit of salt, the bladder increased $40\frac{1}{2}$ grains on forcing out the air. In the trial with the air from iron, it increased $41\frac{1}{2}$ grains, and in that with the air from tin, it increased 41 grains. The heat of the air, when this experiment was made, was 50°; the barometer stood at $29\frac{3}{4}$ inches.

There seems no reason to imagine, from these experiments, that there is any difference in point of specific gravity between these four sorts of inflammable air; as the small difference observed in these trials is in all probability less than what may arise from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce measures of inflammable air weigh 41 grains less than an equal bulk of common air. Therefore if the density of common air, at the time when this experiment was tried, was 800 times less than that of water, which, I imagine, must be near the truth,* inflammable air must

* Mr. Hauksbee, whose determination is usually followed as the most exact, makes air to be more than 850 times lighter than water; vide Hauksbee's experiments, p. 94, or Cotes's Hydrostatics, p. 159. But his method of trying the experiment must in all probability make it appear lighter than it really is. For having weighed his bottle under water, both when full of air and when exhausted, he supposes the difference of weight to be equal to the weight of the air exhausted: whereas in reality it is not so much; for the bottle, when exhausted, must necessarily be compressed, and on that account weigh heavier in water than it would otherwise do. Suppose, for example, that air is really 800 times lighter than water, and that the bottle is compressed $\frac{1}{12000}$ part of its bulk; which seems no improbable supposition: the weight of the bottle in water will thereby be increased by $\frac{1}{12000}$ of the weight of a quantity of water of the same bulk, or more than $\frac{1}{15}$ of the weight of the air exhausted: whence the difference of weight will be not so much as $\frac{14}{15}$ of the weight of the

be 5490 times lighter than water, or near 7 times lighter than common air. But if the density of common air was 850 times less than that of water, then would inflammable air be 9200 times lighter than water, or $10\frac{8}{10}$ lighter than common air. This method of finding the density of factitious air is very convenient and sufficiently accurate, where the density of the air to be tried is not much less than that of common air, but cannot be much depended on in the present case, both on account of the uncertainty in the density of common air, and because we cannot be certain but what some common air might be mixed with the inflammable air in the bladder, notwithstanding the precautions used to prevent it; both which causes may produce a considerable error, where the density of the air to be tried is many times less than that of common air. For this reason, I made the following experiments.

I endeavoured to find the weight of the air discharged from a given quantity of zinc by solution in the vitriolic acid, in the manner represented in fig. 16. A is a bottle filled nearly full with oil of vitriol diluted with about 6 times its weight of water; B is a glass tube fitted into its mouth, and secured with lute; C is a glass cylinder fastened on the end of the tube, and secured also with lute. The cylinder has a small hole at its upper end to let the inflammable air escape, and is filled with dry pearl-ashes in coarse powder. The whole apparatus, together with the zinc, which was intended to be put in, and the lute which was to be used in securing the tube to the neck of the bottle, were first weighed carefully; its weight was 11930 grains. The zinc was then put in, and the tube put in its place. By this means, the inflammable air was made to pass through the dry pearl-ashes; by which it must have been pretty effectually deprived of any acid or watery vapours that could have ascended along with it. The use of the glass tube B was to collect the minute jets of liquor, that were thrown up by the effervescence, and to prevent their touching the pearl-ashes; for which reason, a

air exhausted: and therefore the air will appear lighter than it really is in the proportion of more than 15 to 14, i. e. more than 857 times lighter than water: whereas, if the ball had been weighed in air in both circumstances, the error arising from the compression would have been very trifling.

It appears, from some experiments that have been made by weighing a ball in air, while exhausted, and also after the air was let in, that air, when the thermometer is at 50°, and the barometer at $29\frac{3}{4}$, is about 800 times lighter than water. Though the weight of the air exhausted was little more than 50 grains, no error could well arise near sufficient to make it agree with Hauksbee's experiment. Air seems to expand about $\frac{1}{500}$ part by 1° of heat, whence its density in any other state of the atmosphere is easily determined. The density here assumed agrees very well with the rule given by the gentlemen who measured the length of a degree in Peru, for finding the height of mountains barometrically, and whic his given in the Connoissance des Mouvemens Celestes, année 1762. To make that rule agree accurately with observation, the density of air, whose heat is the same as that of the places where these observations were made, and which I imagine we may estimate at about 45°, should be 798 times less than that of water, when the barometer stands at $29\frac{3}{4}$. —Orig.

small space was left between the glass-tube and the pearl-ashes in the cylinder. When the zinc was dissolved, the whole apparatus was weighed again, and was found to have lost $11\frac{3}{4}$ grains in weight* ; which loss is principally owing to the weight of the inflammable air discharged. But it must be observed, that before the effervescence, that part of the bottle and cylinder, which was not occupied by other more solid matter, was filled with common air; whereas, after the effervescence, it was filled with inflammable air; so that, on that account alone, supposing no more inflammable air to be discharged than what was sufficient to fill that space, the weight of the apparatus would have been diminished by the difference of the weight of that quantity of common air and inflammable air. The whole empty space in the bottle and cylinder was about 980 grain measures; there is no need of exactness; and the difference of the weight of that quantity of common and inflammable air, is about one grain: therefore the true weight of the inflammable air discharged, is $10\frac{3}{4}$ grains. The quantity of zinc used was 254 grains, and consequently the weight of the air discharged is $\frac{1}{23}$ or $\frac{1}{24}$ of the weight of the zinc.

It was before said, that one grain of zinc yielded 356 grain measures of air; therefore 254 grains of zinc yield 90427 grain measures of air; which we have just found to weigh $10\frac{3}{4}$ grains; therefore inflammable air is about 8410 times lighter than water, or $10\frac{1}{2}$ times lighter than common air. The quantity of moisture condensed in the pearl-ashes was found to be about $1\frac{1}{4}$ grains. By another experiment, tried exactly in the same way, the density of inflammable air came out 8300 times less than that of water.

The specific gravity of the air, produced by dissolving zinc in spirit of salt, was tried exactly in the same manner; 244 grains of zinc being dissolved in spirit of salt diluted with about 4 times its weight of water, the loss in effervescence was $10\frac{3}{4}$ grains, the empty space in the bottle and cylinder was 914 grain measures; whence the weight of the inflammable air was $9\frac{3}{4}$ grains, and consequently its density was 8910 times less than that of water. By another experiment, its specific gravity came out 9030 times lighter than water.

A like experiment was tried with iron: $250\frac{1}{2}$ grains of iron being dissolved in oil of vitriol diluted with 4 times its weight of water, the loss in effervescence was 13 grains, the empty space 1420 grain measures. Therefore the weight of the inflammable air was $11\frac{5}{8}$ grains, i.e. about $\frac{1}{22}$ of the weight of the iron, and its density was 8973 times less than that of water. The moisture condensed was $1\frac{1}{4}$ grains.

A like experiment was tried with tin: 607 grains of tinfoil being dissolved in

* As the quantity of lute used was but small, and as this kind of lute does not lose a great deal of its weight by being kept in a moderately dry room, no sensible error could arise from the drying of the lute during the experiment.—Orig.

strong spirit of salt, the loss in effervescence was 14¾ grains, the empty space 873 grain measures: therefore the weight of the inflammable air was 13¾ grains i. e. $\frac{1}{44}$ of the weight of the tin, and its density 8918 times less than that of water. The quantity of moisture condensed was about 3 grains.

It is evident, that the truth of these determinations depend son a supposition, that none of the inflammable air is absorbed by the pearl-ashes. In order to see whether this was the case or no, I dissolved 86 grains of zinc in diluted acid of vitriol, and received the air in a measuring bottle in the common way. Immediately after, I dissolved the same quantity of zinc in the same kind of acid, and made the air to pass into the same measuring bottle, through a cylinder filled with dry pearl-ashes, in the manner represented in Fig. 17. I could not perceive any difference in their bulks.

It appears from these experiments, that there is but little, if any, difference in point of density between the different sorts of inflammable air. Whether the difference of density observed between the air procured from zinc, by the vitriolic and that by the marine acid is real, or whether it is only owing to the error of the experiment, I cannot pretend to say. By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or 11 times lighter than common air.

In order to see whether inflammable air, in the state in which it is, when contained in the inverted bottles, where it is in contact with water, contains any considerable quantity of moisture dissolved in it, I forced 192 ounce measures of inflammable air, through a cylinder filled with dry pearl-ashes, by means of the same apparatus, which I used for filling the bladders with inflammable air, and which is represented in fig. 15. The cylinder was weighed carefully before and after the air was forced through; by which it was found to have increased 1 grain in weight. The empty space in the cylinder was 248 grains, the difference of weight of which quantity of common and inflammable air is ¼ of a grain. Therefore the real quantity of moisture condensed in the pearl-ashes is 1¼ grain. The weight of 192 ounce measures of inflammable air deprived of its moisture appears from the former experiments to be 10½ grains; therefore its weight when saturated with moisture would be 11¾ grains. Therefore inflammable air, in that state in which it is in, when kept under the inverted bottles, contains near $\frac{1}{9}$ its weight of moisture; and its specific gravity in that state is 7840 times less than that of water.

I made an experiment with design to see, whether copper produced any inflammable air by solution in spirit of salt. I could not procure any inflammable air by it: but the phenomena attending it seem remarkable enough to deserve mentioning. The apparatus used for this experiment was of the same kind as was represented in fig. 13. The bottle A was filled almost full of strong spirit of salt, with some fine copper wire in it. The wire seemed not at all acted on by the acid, while cold; but with the assistance of a heat almost sufficient to make the acid boil, it made a considerable effervescence, and the air passed through the bent tube, into the bottle D, pretty fast, till the air forced into it by this means seemed almost equal to the empty space in the bent tube and the bottle A: when, on a sudden, without any sensible alteration of the heat, the water rushed violently through the bent tube into the bottle A, and filled it almost entirely full.

The experiment was repeated again in the same manner, except that I took away the bottle D, and let out some of the water of the cistern: so that the end of the bent tube was out of water. As soon as the effervescence began, the vapours issued visibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A small empty phial was then inverted over the end of the bent tube, so that the mouth of the phial was immersed in the water, the end of the tube being within the body of the phial and out of water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to shake the inverted phial a little, the water suddenly rushed in and filled it almost full; thence it passed through the bent tube into the bottle A, and filled it quite full. Hence it appears likely that copper, by solution in the marine acid, produces an elastic fluid, which retains its elasticity as long as there is a barrier of common air between it and the water, but which immediately loses its elasticity, as soon as it comes in contact with the water. In the first experiment, as long as any considerable quantity of common air was left in the bottle containing the copper and acid, the vapours, which passed through the bent tube, must have contained a good deal of common air. As soon therefore as any part of these vapours came to the farther end of the bent tube, where they were in contact with the water, that part of them, which consisted of the air from copper, would be immediately condensed, leaving the common air unchanged; by which the end of the tube would be filled with common air only; by which means the vapours, contained in the rest of the tube and bottle A, seem to have been defended from the action of the water. But when almost all the common air was driven out of the bottle, then the proportion of common air contained in the vapours, which passed through the tube, seems to have been too small to defend them from the action of the water. In the 2d experiment, the narrow space left between the neck of the inverted phial and the tube would answer much the same end, in defending the vapours within the inverted phial from the action of the water, as the bent tube in the first experiment did in defending the vapours within the bottle from the action of the water.

PART II.—*Containing Experiments on Fixed Air, or that Species of Factitious*

Air, which is produced from Alkaline Substances, by Solution in Acids or by Calcination.

Exper. 1.—The air produced, by dissolving marble in spirit of salt, was caught in an inverted bottle of water, in the usual manner. In less than a day's time, much the greatest part of the air was found to be absorbed. The water contained in the inverted bottle was found to precipitate the earth from lime-water; a sure sign that it had absorbed fixed air.*

Exper. 2.—I filled a Florence flask in the same way with the same kind of fixed air. When full, I stopped up the mouth of the flask with my finger, while under water, and removed it into a vessel of quicksilver, so that the mouth of the flask was entirely immersed in it. It was kept in this situation upwards of a week. The quicksilver rose and fell in the neck of the flask, according to the alterations of heat and cold, and of the height of the barometer; as it would have done if it had been filled with common air. But it appeared, by comparing together the heights of the quicksilver at the same temper of the atmosphere, that no part of the fixed air had been absorbed or lost its elasticity. The flask was then removed, in the same manner as before, into a vessel of soap leys. The fixed air by this means, coming in contact with the soap leys, was quickly absorbed. I also filled another Florence flask with fixed air, and kept it with its mouth immersed in a vessel of quicksilver in the same manner as the other, for upwards of a year, without being able to perceive any air to be absorbed. On removing it into a vessel of soap leys, the air was quickly absorbed like the former. It appears from this experiment, that fixed air has no disposition to lose its elasticity, unless it meets with water, or some other substance proper to absorb it, and that its nature is not altered by keeping.

Exper. 3.—To find how much fixed air water would absorb, the following experiment was made. A cylindrical glass, with divisions marked on its sides with a diamond, showing the quantity of water which it required to fill it up to those marks, was filled with quicksilver, and inverted into a glass filled with the same fluid. Some fixed air was then forced into this cylindrical glass, in the same manner that it was into the inverted bottles of water in the former experiments; except that, to prevent any common air from being forced into the glass along with the fixed, I took care not to introduce the end of the bent tube within the cylindrical glass, till I was well assured that no common air to signify could re-

* Lime, as Dr. Black has shown, is no more than a calcareous earth rendered soluble in water by being deprived of its fixed air. Lime water is a solution of lime in water: therefore, on mixing lime water with any liquor containing fixed air, the lime absorbs the air, becomes insoluble in water, and is precipitated. This property of water, of absorbing fixed air, and then making a precipitate with lime water, has been taken notice of by Mr. M'Bride.—Orig.

main within the bottle. This was done by first introducing the end of the bent tube within an inverted bottle of water, and letting it remain there, till the air driven into this bottle was at least 10 times as much as would fill the empty space in the bent tube, and the bottle containing the marble and acid. By this means one might be well assured, that the quantity of common air remaining within the bent tube and bottle must be very trifling. The end of the bent tube was then introduced within the cylindrical glass, and kept there till a sufficient quantity of fixed air was let up. After letting it stand a few hours, the division answering to the surface of the quicksilver in the cylinder was observed and written down, by which it was known how much fixed air had been let up. A little rain water was then introduced into the cylindrical glass, by pouring some rain water into the vessel of quicksilver, and then lifting up the cylindrical glass so as to raise the bottom of it a little way out of the quicksilver. After having suffered it to stand a day or two, in which time the water seemed to have absorbed as much fixed air as it was able to do, the division answering to the upper surface of the water, and also that answering to the surface of the quicksilver, were observed: by which it was known how much air remained not absorbed, and also how much water had been introduced: the division answering to the surface of the water telling how much air remained not absorbed, and the difference of the two divisions telling how much water had been let up. More water was then let up in the same manner, at different times, till almost the whole of the fixed air was absorbed. As all water contains a little air, the water used in this experiment was first well purged of it by boiling, and then introduced into the cylinder while hot. The result of the experiment is given in the following table; in which the first column shows the bulk of the water let up each time; the 2d shows the bulk of air absorbed each time; the 3d, the whole bulk of water let up; the 4th, the whole bulk of air absorbed; and the 5th column shows the bulk of air remaining not absorbed. To set the result in a clearer light, the whole bulk of air introduced into the cylinder is called 1, and the other quantities are set down in decimals of it.

Bulk of air let up = 1.

Bulk of water let up each time.	Bulk of air absorbed each time.	Whole bulk of water let up.	Whole bulk of air absorbed.	Whole bulk of air remaining.
.322	.374	.322	.374	.626
.481	.485	.803	.859	.141
.082	.048	.885	.907	.093
.145	.079	1.030	.986	.014

I imagine that the quantities of water let up and of the air absorbed could be estimated to about 3 or 4 1000th parts of the whole bulk of air introduced. The height of the thermometer, during the trial of this experiment, was at a medium 55°. This experiment was tried once before. The result agreed pretty nearly with this; but, as it was not tried so carefully, the result is not set down. It hence appears, that the fixed air contained in marble, consists of substances of different natures, part of it being more soluble in water than the rest: it appears too that water, when the thermometer is about 55°, will absorb rather more than an equal bulk of the more soluble part of this air. It appears, from an experiment which will be mentioned hereafter, that water absorbs more fixed air in cold weather than in warm; and, from the following experiment, it appears, that water heated to the boiling point is so far from absorbing air, that it parts with what it has already absorbed.

Exper. 4.—Some water, which had absorbed a good deal of fixed air, and which made a considerable precipitate with lime water, was put into a phial, and kept about $\frac{1}{4}$ of an hour in boiling water. It was found when cold not to make any precipitate, or to become in the least cloudy on mixing it with lime water.

Exper. 5.—Water also parts with the fixed air, which it has absorbed, by being exposed to the open air. Some of the same parcel of water that was used for the last experiment, being exposed to the air in a saucer for a few days, was found at the end of that time to make no clouds with lime water.

Exper. 6.—In like manner it was tried how much of the same sort of fixed air was absorbed by spirits of wine. The result is as follows.

Bulk of air introduced = 1.

Spirit let up each time.	Air absorbed each time.	Whole bulk of spirit let up.	Whole bulk of air absorbed.	Bulk of air remaining.
.207	.453	.207	.453	.547
.146	.274	.353	.727	.273
.074	.103	.427	.830	.170
.046	.030	.473	.860	.140

The mean height of the thermometer, during the trial of the experiment, was 46°. Therefore spirit of wine, at the heat of 46°, absorbs near $2\frac{1}{4}$ times its bulk of the more soluble part of this air.

Exper. 7.—After the same manner it was tried how much fixed air is absorbed by oil. Some olive oil, equal in bulk to $\frac{1}{3}$ part of the fixed air in the cylindrical glass, was let up. It absorbed rather more than an equal bulk of air; the thermometer being between 40 and 50. The experiment was not carried any further. The oil was found to absorb the air very slowly.

Exper. 8.—The specific gravity of fixed air was tried by means of a bladder, in the same manner which was made use of for finding the specific gravity of inflammable air; except that the air, instead of being caught in an inverted bottle of water, and thence transferred into the bladder, was thrown into the bladder immediately from the bottle which contained the marble and spirit of salt, by fastening a glass tube to the wooden cap of the bladder, and luting that to the mouth of the bottle containing the effervescing mixture, in such manner as to be air-tight. The bladder was kept on till it was quite full of fixed air: being then taken off and weighed, it was found to lose 34 grains, by forcing out the air. The bladder was previously found to hold 100 ounce measures. Whence if the outward air, at the time when this experiment was tried, be supposed to have been 800 times lighter than water, fixed air is 511 times lighter than water, or $1\frac{57}{100}$ times heavier than common air. The heat of the air during the trial of this experiment was 45°. By another experiment of the same kind, made when the thermometer was at 65°, fixed air seemed to be about 563 times lighter than water.

Exper. 9.—Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminished by the mixture of a small quantity of fixed air; as appears thus: A small wax candle burnt 80s in a receiver, which held 190 ounce measures, when filled with common air only. The same candle burnt 51s in the same receiver, when filled with a mixture of one part of fixed air to 19 of common air, i. e. when the fixed air was $\frac{1}{20}$ of the whole mixture. When the fixed air was $\frac{3}{40}$ of the whole mixture, the candle burnt 23s. When the fixed air was $\frac{1}{10}$ of the whole, it burnt 11s. When the fixed air was $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of the whole mixture, the candle went out immediately.

Hence it should seem, that when the air contains near $\frac{1}{9}$ its bulk of fixed air, it is unfit for small candles to burn in. Perhaps indeed, if I had used a larger candle and a larger receiver, it might have burnt in a mixture containing a larger proportion of fixed air than this; as I believe that large flaming bodies will burn in a fouler air than small ones. But this is sufficient to show, that the power which common air has of keeping fire alive, is very much diminished by a small mixture of fixed air. This experiment was tried, by setting the candle in a large cistern of water, in such manner that the flame was raised but a little way above the surface; the receiver being inverted full of water into the same cistern. The proper quantity of fixed air was then let up, and the remaining space filled with common air, by raising the receiver gradually out of water; after which, it was immediately whelmed gently over the burning candle.

Experiments on the Quantity of Fixed Air, contained in Alkaline Substances.

Exper. 10.—The quantity of fixed air contained in marble, was found by dissolving some marble in spirit of salt, and finding the loss of weight, which it suffered in effervescence, in the same manner as I found the weight of the inflammable air discharged from metals by solution in acids, except that the cylinder was filled with shreds of filtering paper, instead of dry pearl ashes; for pearl ashes would have absorbed the fixed air that passed through them. The weight of the marble dissolved was $311\frac{1}{2}$ grains. The loss of weight in effervescence was $125\frac{1}{4}$ grains. The whole empty space in the bottle and cylinder was about 2700 grain measures: the excessive weight of that quantity of fixed, above an equal quantity of common air, is $1\frac{3}{4}$ grains. Therefore the weight of the fixed air discharged is $127\frac{1}{4}$ grains. The cylinder with the filtering paper was found to have increased $1\frac{3}{4}$ grains in weight during the effervescence. The empty space in the cylinder was about 1160 grain measures: the excess of weight of which quantity of fixed air, above an equal bulk of common air, is $\frac{3}{4}$ grains. Therefore the quantity of moisture condensed, in the filtering paper, is one grain, or about $\frac{1}{125}$ part of the weight of the air discharged.

As water has been already shown to absorb fixed air, it seemed not improbable but there might be some fixed air contained in the solution of marble in spirit of salt; in which case the air discharged, during the effervescence, could not be the whole of the fixed air in the marble. In order to see whether this was the case, I poured some of the solution into lime water. It made scarcely any precipitate; which, as the acid was entirely saturated with marble, it would certainly have done if the solution had contained any fixed air. It appears therefore from this experiment, first, that marble contains $\frac{127\frac{1}{4}}{311\frac{1}{2}} = \frac{407}{1000}$ of its weight of fixed air; and 2dly, that the quantity of moisture, which flies off along with the fixed air in effervescence, is but trifling; as I imagine that the greatest part of what did fly off, must have been condensed in the filtering paper. By another experiment, tried much in the same way, marble was found to contain $\frac{408}{1000}$ of its weight of fixed air.

Exper. 11.—Volatile sal ammoniac dissolves with too great rapidity in acids, and makes too violent an effervescence, to allow one to try what quantity of fixed air it contains in the foregoing manner: I therefore made use of the following method. Three small phials were weighed together in the same scale. The first contained some weak spirit of salt, the 2d contained some volatile sal ammoniac in moderate sized lumps without powder, corked up to prevent evaporation, and the 3d, intended for mixing the acid and alkali in, contained only a little water, and was covered with a paper cap, to prevent the small jets of liquor, which are thrown up during the effervescence, from escaping out of the bottle. To prevent too violent an effervescence, the acid and alkali were both added by a little at a time, care being taken that the acid should always predominate in the mixture. Care was also taken always to cover the bottle with the paper cap, as soon as any of the acid or alkali were added. As soon as the mixture was finished, the 3 phials were weighed again; by which the loss in effervescence was found to be 134 grains. The weight of the volatile salt made use of was 254 grains, and was pretty exactly sufficient to saturate the acid. The solution appeared, by pouring some of it into lime water, to contain scarcely any fixed air. Therefore 254 grains of the volatile sal ammoniac contain 134 grains of fixed air, i. e. $\frac{528}{1000}$ of their weight. It appeared from the same experiment, that 1680 grains of the volatile salt saturate as much acid as 1000 grains of marble.

By another experiment, tried with some of the same parcel of volatile salt, it was found to contain $\frac{538}{1000}$ of its weight of fixed air, and 1643 grains of it saturated as much acid as 1000 grains of marble. By a medium, the salt contained $\frac{533}{1000}$ of its weight of fixed air; and 1661 grains of it saturated as much acid as 1000 grains of marble.

One thousand grains of marble were found to contain $407\frac{1}{2}$ grains of air, and 1661 grains of volatile sal ammoniac contain 885 grains. Therefore this parcel of volatile sal ammoniac contains more fixed air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 885 to $407\frac{1}{2}$, or of 217 to 100. N. B. It is not unlikely, that the quantity of fixed air may be found to differ considerably in different parcels of volatile sal ammoniac; so that any one, who was to repeat these experiments, ought not to be surprized if he was to find the result to differ considerably from that here laid down. The same thing may be said of pearl ashes.

Exper. 12.—This serves to account for a remarkable phenomenon, which I formerly met with, on putting a solution of volatile sal ammoniac in water into a solution of chalk in spirit of salt. The earth was precipitated, as might naturally be expected: but what surprized me, was, that it was attended with a considerable effervescence; though I was well assured, that the acid in the solution of chalk, was perfectly neutralized. This is very easily accounted for, from the above-mentioned circumstance of volatile sal ammoniac containing more fixed air in proportion to the quantity of acid that it can saturate, than calcareous earths do. For the volatile alkali, by uniting to the acid, was necessarily deprived of its fixed air. Part of this air united to the calcareous earth, which was at the same time separated from the acid; but, as the earth was not able to absorb the whole of the fixed air, the remainder flew off in an elastic form, and so produced an effervescence.

Exper. 13.—The same solution of volatile sal ammoniac made no precipitate, when mixed with a solution of Epsom salt; though a mixture of it with a little spirit of sal ammoniac, made with lime, immediately precipitated the magnesia from the same solution of Epsom salt; as it ought to do according to Dr. Black's account of the affinity of magnesia and volatile alkalies to acids. This experiment is not so easily accounted for as the last; but I imagine, that the magnesia is really separated from the acid by the volatile alkali; but that it is soluble in water, when united to so great a proportion of fixed air, as is contained in a portion of volatile sal ammoniac, sufficient to saturate the same quantity of acid. The reason why the mixture of the solution of volatile sal ammoniac, with the spirits of sal ammoniac made with lime, precipitates the magnesia from the Epsom salt, is, that as the spirits made with lime contain no fixed air, the mixture of these spirits with the solution of volatile sal ammoniac contains less air, in proportion to the quantity of acid which it can saturate, than the solution of volatile sal ammoniac by itself does. Volatile sal ammoniac requires a great deal of water to dissolve it, and the solution has not near so strong a smell as the spirits of sal ammoniac made with fixed alkali; the reason of which is, that the latter contain much less fixed air. But volatile sal ammoniac dissolves in considerable quantity in weak spirits of sal ammoniac made with lime, and the solution differs in no respect from the spirits made with fixed alkali. This is a convenient way of procuring the mild spirits of sal ammoniac, as those made with fixed alkali are seldom to be met with in the shops.

Exper. 14.—The quantity of fixed air contained in pearl ashes was tried, by mixing a solution of pearl ashes with diluted oil of vitrol, in the same manner as was used for volatile sal ammoniac. As much of the solution was used as contained 328¼ grains of dry pearl ashes. The loss of effervescence was 90 grains. The mixture, which was perfectly neutralized, being then added to a sufficient quantity of lime water, in order to see whether it contained any fixed air, a precipitate was made, which being dried weighed 8½ grains. Therefore, if we suppose this precipitate to contain as much fixed air as an equal weight of marble, which I am well assured cannot differ very considerably from the truth, the fixed air in it is 3½ grains, and consequently the air in 328½ grains of the pearl ashes, is 93½ grains, i. e. $\frac{284}{1000}$ of their weight. By another experiment tried in the same way, they appeared to contain $\frac{237}{1000}$ of their weight of fixed air. 1558 grains of the pearl ashes were found to saturate as much acid as 1000 grains of marble. Therefore this parcel of pearl ashes contains more air, in proportion to the quantity of acid that it can saturate, than marble does, in the proportion of 109 to 100.

Exper. 15.—Dr. Black says that, by exposing a solution of salt of tartar for a long time to the open air, some crystals were formed in it, which seemed to be nothing else but the vegetable alkali united to more than its usual proportion of fixed air. This induced me to try whether I could not perform the same thing more expeditiously, by furnishing the alkali with fixed air artificially; which I did in the manner represented in fig. 18: where A represents a wide-mouthed bottle, containing a solution of pearl ashes; Bb represents a round wooden ring fastened over the mouth of the bottle, and secured with luting; c is a bladder bound tight over the wooden ring. This bladder, being first pressed close together, so as to drive out as much of the included air as possible, was filled with fixed air, by means of the bent tube D; one end of which is fixed into the wooden ring, and the other fastened into the mouth of the bottle E, containing marble and spirit of salt. By this means the fixed air thrown into the bladder mixed with the air in the bottle, and came in contact with the fixed alkali. The fixed air was by degrees absorbed, and crystals were formed on the surface of the fixed alkali, which were thrown to the bottom by shaking the bottle. When the alkali had absorbed as much fixed air as it would readily do, the crystals were taken out and dried on filtered paper, and the remaining solution evaporated; by which means some more crystals were procured.

N. B. It seemed as if not all the air discharged from the marble was of a nature proper to be absorbed by the alkali, but only part of it; for when the alkali had absorbed somewhat more than $\frac{1}{4}$ of the air first thrown into the bladder, it would not absorb any more: but on pressing the remaining air out of the bladder, and supplying its place with fresh fixed air, a good deal of this new air was absorbed. I cannot however speak positively as to this point; as I am not certain whether the apparatus was perfectly air-tight.*

These crystals do not in the least attract the moisture of the air; as I have kept some, during a whole winter, exposed to the air in a room without a fire, without their becoming at all moist or increasing in weight. Being held over the fire in a glass vessel, they did not melt as many salts do, but rather became white, and calcined. They dissolve in about 4 times their weight of water when the weather is temperate, and dissolve in greater quantity in hot water than cold.

It was found, by the same method that was made use of for the volatile sal ammoniac, that these crystals contain $\frac{423}{1000}$ of their weight of fixed air, and that 2035 grains of them saturate as much acid as 1000 grains of marble. Therefore these crystals contain more air, in proportion to the quantity of acid they saturate, than marble does, in the ratio of 211 to 100.

Exper. 16.—As these crystals contain about as much fixed air, in proportion to the quantity of acid that they can saturate, as volatile sal ammoniac does, it was natural to expect that they should produce the same effects with a solution of Epsom salt, or a solution of chalk in spirit of salt; as those effects seemed owing only to the great quantity of fixed air, contained in volatile sal ammoniac. This was found to be the real case; for a solution of these crystals in 5 times

* Pearl-ashes deprived of their fixed air, i. e. soap lees, will absorb the whole of the air discharged from marble: as I know by experience. But yet it is not improbable but that the same alkali, when nearly saturated with fixed air, may be able to absorb only some particular part of it. For as it has been already shown, that part of the air discharged from marble is more soluble in water than the rest; so it is not unlikely but that part of it may have a greater affinity to fixed alkali, and be absorbed by it in greater quantity than the rest.—Orig.

their weight of water, being dropt into a solution of chalk in spirit of salt, the earth was precipitated, and an effervescence was produced. No precipitate was made on dropping some of the same solution into a solution of Epsom salt, though the mixture was kept upwards of twelve hours. But, on heating this mixture over the fire, a great deal of air was discharged, and the magnesia was precipitated.

PART III.—*Containing Experiments on the Air produced by Fermentation and Putrefaction.*

Mr. M'Bride has already showed, that vegetable and animal substances yield fixed air by fermentation and putrefaction. The following experiments were made chiefly with a view of seeing whether they yield any other sort of air besides that.

Exper. 1.—The air produced from brown sugar and water, by fermentation, was caught in an inverted bottle of soap leys in the usual manner, and which is represented in fig. 13. As the weather was too cold to suffer the sugar and water to ferment freely, the bottle containing it was immersed in water, which, by means of a lamp, was kept constantly at about 80° of heat. The quantity of sugar put into the bottle was 931 grains: it was dissolved in about $6\frac{1}{4}$ times its weight of water, and mixed with 100 grains of yeast, by way of ferment. The empty space left in the fermenting bottle and tube together measured 1920 grains. The mixture fermented freely, and generated a great deal of air, which was forced up in bubbles into the inverted bottle, but was absorbed by the soap leys, as fast as it rose up. It frothed greatly; but none of the froth or liquor ran over. In about 10 days, the fermentation seeming almost over, the vessels were separated. The bottle with the fermented liquor was found to weigh 412 grains less than it did before the fermentation began. As none of the liquor ran over, and as little or no moisture condensed within the bent tube, I think one may be well assured, that the loss of weight was owing entirely to the air forced into the inverted bottle; for the matter discharged, during the fermentation, must have consisted either of air, or of some other substance changed into vapour: if this last was the case, I think it could hardly have failed, but that great part of those vapours must have condensed in the tube. The air remaining unabsorbed in the inverted bottle of soap leys was measured, and was found to be exactly equal to the empty space left in the bent tube and fermenting bottle. It appears therefore, that there is not the least air of any kind discharged from the sugar and water by fermentation, but what is absorbed by the soap leys, and which may therefore be reasonably supposed to be fixed air. It seems also, that no part of the common air left in the fermenting bottle was absorbed by the fermenting mixture, or suffered any change in its nature from it: for a small phial being filled with 1 part of this air, and 2 of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the same appearances, as far as I could perceive, as when the phial was filled with the same quantities of common and inflammable air.

The sugar used in this experiment was moist, and was found to lose $\frac{228}{1000}$ parts of its weight by drying gently before a fire. Therefore the quantity of dry sugar used was 715 grains; and the weight of the air discharged by fermentation appears to be near 412 grains, i. e. near $\frac{57}{100}$ parts of the weight of the dry sugar in the mixture. The fermented liquor was found to have entirely lost its sweetness; so that the vinous fermentation seemed to be completed; but it was not become at all sour.

Exper. 2.—The air discharged from apple-juice, by fermentation, was tried exactly in the same manner. The quantity set to ferment was 7060 grains, and was mixed with 100 grains of yeast. Some of the same parcel of apple-juice, being evaporated gently to the consistence of a moderately hard extract, was reduced to $\frac{1}{7}$ of its weight; so that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor fermented much faster than the sugar and water. The loss of weight during the fermentation was 384 grains. The air remaining unabsorbed in the inverted bottle of soap leys was lost by accident, so that it could not be measured; but, from the space it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty space in the bent tube and fermenting bottle, if it did at all. Therefore there is no reason to think that the apple-juice, any more than the sugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near $\frac{381}{1000}$ of the weight of the extract contained in the apple-juice. The fermented liquor was very sour; so that it had gone beyond the vinous fermentation, and made some progress in the acetous fermentation. In order to compare more exactly the nature of the air produced from sugar by fermentation, with that produced from marble by solution in acids, I made the three following experiments:

Exper. 3.—I first tried in what quantity the air from sugar was absorbed by water, and at the same time made a like experiment on the air discharged from marble, by solution in spirit of salt. This was done exactly in the same way as the former experiments of this kind. The result is as follows, beginning with the air from sugar and water.

Air from sugar and water let up = 1000.

Bulk of water let up each time.	Bulk of air absorbed each time.	Whole bulk of water let up.	Whole bulk of air absorbed.	Bulk of air remaining.	Height of thermometer when obs. was made.
375	517	375	517	483	40
143	164	518	681	319	45
153	164	673	845	154	45
82	103	755	948	52	46

Air from marble let up=1000

391	473	391	473	527	40
143	133	534	606	394	45
284	115	818	811	189	45
194	80	1012	891	109	46

The apparatus used in this experiment was suffered to remain in the same situation till summer, when the thermometer stood at 65°. The bulk of the air from sugar, not absorbed by the water, was then found to be 287; so that the matter had remitted 235 parts of air. The bulk of the air from marble not absorbed, was 194; so that 85 parts were remitted; which is therefore a proof that water absorbs less fixed air in warm weather, than in cold. It appears from this experiment, that the air produced from sugar by fermentation, as well as that discharged from marble by solution in acids, consists of substances of different nature: part being absorbed by water in greater quantity than the rest. But, in general, the air from sugar is absorbed in greater quantity than that from marble. In forcing the air from sugar into the cylindrical glass, no sensible quantity of moisture was found to condense on the surface of the quicksilver, or sides of the glass; which is a proof that no considerable quantity of any thing except air could fly off from the sugar and water in fermentation.

Exper. 4.—The specific gravity of the air produced from sugar, was found in the same way as that produced from marble. A bladder holding 102 ounce measures, being filled with this kind of air, lost $29\frac{1}{8}$ grains on forcing out the air, the thermometer standing at 62° and the barometer at $29\frac{1}{2}$ inches. Whence, supposing the outer air during the trial of this experiment to be 826 times lighter than water, as it should be, according to the supposition made use of in the former parts of this paper, the air from sugar should be 554 times lighter than water. Its density therefore appears to be much the same as that of the air contained in marble; as that air appeared to be 511 times lighter than water, by a trial made when the thermometer was at 45°; and 563 times lighter, by another trial when the thermometer was at 65°. This air seems also to possess the property of extinguishing flame, in much the same degree as that produced from marble; as appears from the following experiment:

Exper. 5.—A small wax candle burnt 15^s in a receiver filled with $\frac{1}{10}$ of air from sugar, the rest common air. In a mixture containing $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of air from sugar, the rest common air, the candle went out immediately. When the receiver was filled with common air only, the same candle burnt 72^s. The receiver was the same as that used in the former experiment of this kind, and the experiment tried in the same way, except that the air from sugar was first received in an empty bladder, and thence transferred into the inverted bottles of water in which it was measured; for the air is produced from the sugar so slowly, that if it had been received in the inverted bottles immediately, it would have been absorbed almost as fast as it was generated.

It appears from these experiments, that the air produced from sugar by fermentation, and in all probability that from all the other sweet juices of vegetables, is of the same kind as that produced from marble by solution in acids, or at least does not differ more from it than the different parts of that air do from each other, and may therefore justly be called fixed air. I now proceed to the air generated by putrifying animal substances.

Exper. 6.—The air produced from gravy broth by putrefaction, was forced into an inverted bottle of soap leys, in the same way as in the former experiment. The quantity of broth used, was 7640 grains, and was found, by evaporating some of the same to the consistence of a dry extract, to contain 163 grains of solid matter. The fermenting bottle was immersed in water kept constantly to the heat of about 96°. In about two days the fermentation seemed entirely over. The liquor smelt very putrid, and was found to have lost $11\frac{1}{4}$ grains of its weight. The soap leys had acquired a brownish colour from the putrid vapours, and a musty smell. The air forced into the inverted bottle, and not absorbed by the soap leys, measured 6280 grains: the air left in the bent tube and fermenting bottle was 1100 grains; almost all of which must have been forced into the inverted bottles: so that this unabsorbed air is a mixture of about 1 part of common air and $4\frac{7}{10}$ of factitious air.

The air was found to be inflammable; for a small phial being filled with 109 grain measures of it, and 301 of common air, which comes to the same thing as 90 grains of pure factitious air, and 320 of common air, it took fire on applying a piece of lighted paper, and went off with a gentle bounce, of much the same degree of loudness as when the phial was filled with the last mentioned quantities of inflammable air from zinc, and common air. When the phial was filled with 297 grains of this air, and 113 of common air, i. e. with 245 of pure factitious air, and 165 of common air, it went off with a gentle bounce on applying the lighted paper; but I think not so loud as when the phial was filled with the last-mentioned quantities of air from zinc and common air.

5500 grain measures of this air, i. e. 4540 of pure factitious air, and 960 of common air, were forced into a piece of ox-gut furnished with a small brass cock, which I find more convenient for trying the specific gravity of small quantities of air, than a bladder: the gut increased $4\frac{1}{4}$ grains in weight on forcing out the air. A mixture of 4540 grains of air from zinc, and 960 of common air being then forced into the same gut, it increased $4\frac{3}{4}$ grains on forcing out the air. So that this factitious air should seem to be rather heavier than air from zinc; but the quantity tried was too small to afford any great degree of certainty.

N. B. The weight of 4540 grain measures of inflammable air, is $\frac{68}{100}$ grains, and the weight of the same quantity of common air is $5\frac{7}{10}$ grains.

On the whole it seems that this sort of inflammable air is nearly of the same kind as that produced from metals. It should seem however, either to be not exactly the same, or else to be mixed with some air heavier than it, and which has in some degree the property of extinguishing flame, like fixed air.

The weight of the inflammable air discharged from the gravy appears to be about one grain; which is but a small part of the loss of weight which it suffered in putrefaction. Part of the remainder, according to Mr. M'Bride's experiments, must have been fixed air. But the colour and smell, communicated to the soap leys, shew that it must have discharged some other substance, besides fixed and inflammable air. Raw meat also yields inflammable air by putrefaction, but not in near so great a quantity, in proportion to the loss of weight which it suffers, as gravy does. Four ounces of raw meat mixed with water, and treated in the same manner as the gravy, lost about 100 grains in putrefaction; but it yielded hardly more inflammable air than the gravy. This air seemed of the same kind as the former; but, as the experiments were not tried so exactly, they are not set down.

Mr. C. endeavoured to collect in the same manner the air discharged from bread and water by fermentation, but he could not get it to ferment, or yield any sensible quantity of air; though he added a little putrid gravy by way of ferment.

XX. A further Account of the Polish Cochineal, from Dr. Wolfe, of Warsaw, p. 184.

In the LIV* volume of the Phil. Trans. are published two curious papers, from Dr. Wolfe, of Warsaw, describing the Polish cochineal, the plants on whose roots it is found, the manner of collecting and curing it, the method of dying with it, and also the doctor's own experiments on these curious insects.

Since that time, the doctor has been very industrious in breeding and observing these insects, and has thus discovered the male fly, about which he was before uncertain; and has sent to Mr. Baker an elegant picture of it, painted from the life in its natural colours, and also of the young female just crept from the egg, both in their natural size, and as magnified by glasses; together with a drawing and description of the polygonum minus of Casper Bauhine, or scleran thus perennis of Linnæus, which is the plant, adhering to the roots of which this insect is chiefly found in Podolia and the Ukrain.

* Page 110 of this vol. of these Abridgments.

All these Mr. Baker lays before the R. S. to complete Dr. Wolfe's account of this insect; and as this plant is common in England, as well as the Potentilla and Fragaria, at the roots of which these insects are likewise found, he is in hopes, that such gentlemen, as have opportunity, will seek them in the months of June, July, and August; the time they seek for, and collect them, in the above-mentioned countries. The curious will receive pleasure and information from comparing the male fly of this Polish cochineal, with the male fly of the cochineal of South America; communicated some time ago by Mr. John Ellis, and published in vol. lii, p. 661, [Abridg. vol. xi. p. 674.] Pl. 8. fig. 1, is the Polish Cochineal male insect, just come out of the egg, of its natural size. The body and head of this beautiful little fly have several teints of a brownish crimson: the wings are white and transparent, except the darker parts in the plate, which are of a lively crimson colour. Fig. 2. The same magnified. Fig. 3. The female insect, just crept out of the egg, of the natural size. Its colour a brownish crimson. Fig. 4. The same magnified. Fig. 5. Polygonum minus IV C. Bauh. or, Scleranthus perennis calycibus clausis, Linnæi. The root fibrous; when old, woody. The young stalks of a grey green; in the 2d year, red. They have knots at different intervals. Each knot has two sharp-pointed leafy narrow stipulæ. The stalks are dichotomous; and near the umbella there is, at every bifurcation, a flower twice as large as the others, having its seeds more ripe and perfect. The calyx grows almost woody, and is five-pointed. The petala are small, oval-pointed, white, in number 10: the 5 stamina short: the antheræ yellow: the pistilla, 2, very short. The seeds egg-shaped, one or two strongly adhering to the calyx. The whole plant, when old, has stalks 10 inches in length, procumbent by the weight of the flowers, and making a sort of convex bush round about the root.

XXI. Some further Account of the Jaculator Fish, mentioned in the Phil. Trans. for 1764, [p. 110 of this abridged vol.] *from Mr. Hommel, at Batavia, together with the Description of another Species, by Dr. Pallas, F. R. S.* p. 186.

Amsterdam, Feb. 15, 1766.

When the Jaculator fish intends to catch a fly or any other insect, which is seen at a distance, it approaches very slowly and cautiously, and comes as much as possible perpendicularly under the object: then the body being put in an oblique situation, and the mouth and eyes being near the surface of the water, the Jaculator stays a moment quite immoveable, having its eyes directly fixed on the insect, and then begins to shoot, without ever showing its mouth above the surface of the water, out of which the single drop, shot at the object, seems to rise. With the closest attention Mr. H. never could see any part of the mouth out of water, though he has very often seen the Jaculator fish shoot a great many drops one after another, without leaving its place and fixed situation. No more than two different species of this fish are found at Batavia. The first and rarest kind is that he sent before.

This second species* is Sciæna Jaculatrix, quinque maculata, pinnis ventralibus adnatis, maxilla inferiore longiore. Locus. Mare Indicum. Magnitudo cyprini rutili.

XXII. Of an Amphibious Bipes, by John Ellis, Esq. F. R. S. p. 189.

Mr. E. received in the summer of 1765, two specimens of a remarkable kind of animal† from Dr. Alex. Garden, of Charlestown, Carolina, who says, it is evidently a new genus not yet noticed by naturalists, and that it appears to come between the *Muræna* and the *Lacerta*. The natives call it by the name of *Mud-Inguana*. It is found in swampy and muddy places, by the sides of pools, under the trunks of old trees that hang over the water.

The lesser specimen, which was preserved in spirits, measured about 9 inches in length; and appeared to be a very young state of the animal, from the fin of the tail and the opercula or coverings of the gills being not extended to their full size. These opercula, in their then state, consisted each of 3 indented lobes, hiding the gills from view, and placed just above the two feet. These feet appeared like little arms and hands, each furnished with 4 fingers, and each finger with a claw.

In the large specimen, which was about 31 inches long, the head is something like an eel, but more compressed: the eyes small, and placed as those of the eel are; in this they are scarcely visible: this smallness of the eye best suits an animal that lives so much in mud. The nostrils are very plainly to be distinguished; these, with the gills and the remarkable length of the lungs, show it to be a true amphibious animal. The mouth is small in proportion to the body; but its palate and inside of the lower jaw are well provided with many rows of pointed teeth; with this provision of nature, added to the sharp exterior bony edges of both the upper and under jaw, the animal seems capable of biting and grinding the hardest kind of food. The skin, which is black, is full of small scales, resembling chagrin. These scales are of different sizes and shapes according to their situation, but all appear sunk into its gelatinous surface: those along the back and belly are of an oblong oval form, and close set together: in the other parts, they are round and more distinct. Both the sides are mottled with small white spots, and have two distinct lines composed of small white streaks, conti-

* This is the *Siœna jaculatrix* of the Gmelinian edition of the Systema Naturæ, and the *Labrus jaculator* of Shaw's General Zoology.

† This celebrated animal, the *Siren lacertina* of Linnæus, which has by many naturalists been considered as the larva of some large species of lizard, is now allowed to constitute a distinct genus, of which some other species have been discovered.

nued along from the feet to the tail. The fin of the tail has no rays, and is no more than an adipose membrane like that of the eel; this fin appears more distinctly in the dry animal than in those that have been preserved in spirits.

The opercula or coverings to the gills in dry specimens appear shrivelled up, but yet we may plainly see they have been doubly pennated. Under these coverings, are the openings to the gills, 3 on each side, agreeable to the number of the opercula. The form of these pennated coverings approach very near to what he had before observed, in the larva or aquatic state of our English lacerta, known by the name of eft or newt, which serve them for coverings to their gills, and for fins to swim with during this state; and which they lose, as well as the fin of their tails, when they change their state and become land animals.

Recollecting these observations on the changes of our lizard, and at the same time the many remarkable changes in frogs, Mr. E. began to suspect whether this animal might not be the larva state of some large kind of lizard; and therefore requested the favour of Dr. Solander, to examine with him the lacertas in the British Museum; that they might see whether any of the young ones had only 2 feet; but, after carefully going through many kinds, they could plainly discover 4 feet perfectly formed, even in those that were just coming out of their eggs. During this state of uncertainty, Mr. E. forwarded to Dr. Linnæus of Upsal, at Dr. Garden's request, his account of the largest specimen, and, at the same time, sent him one of the smaller specimens preserved in spirits; desiring his opinion, for Dr. Garden's, as well as his own satisfaction. About the latter end of January last, he was favoured with an answer from the professor, dated Upsal, Dec. 27, 1765, wherein he says, "I received Dr. Garden's very rare two-footed animal with gills and lungs. The animal is probably the larva of some kind of lacerta, which I very much desire that he will particularly enquire into. If it does not undergo a change, it belongs to the order of Nantes, which have both lungs and gills; and if so, it must be a new and very distinct genus, and should most properly have the name of Siren. I cannot possibly describe to you how much this two-footed animal has exercised my thoughts; if it is a larva, he will no doubt find some of them with 4 feet. It is not an easy matter to reconcile it to the larva of the lizard tribe, its fingers being furnished with claws; all the larvas of lizards, that I know, are without them. Then also the branchiæ or gills are not to be met with in the aquatic salamanders, which are probably the larvas of lizards. Further, the croaking noise or sound it makes does not agree with the larvas of these animals; nor does the situation of the anus. So that there is no creature that ever I saw, that I long so much to be convinced of the truth, as what this will certainly turn out to be."

P. S. In a letter lately received from Dr. Garden, he mentions one remarkable property in this animal, which is, that his servant endeavouring to kill one of them, by dashing it against the stones, it broke into 3 or 4 pieces: he further says, that he has had an opportunity of seeing many of them lately of a much larger size, and that he never saw one with more than 2 feet; so that he is fully convinced, that it is quite a new genus of the animal kingdom.

XXIII. Observations on Animals, commonly called Amphibious, by Authors. Presented by Dr. Parsons, F. R. S.* p. 193.

If we consider the words ἄμφι and βίος, from which the term amphibious is derived; we should understand that animals, having this title, should be capable of living as well by land or in the air, as by water, or of dwelling in either constantly at will; but it will be difficult to find any animal that can fulfil this definition, as being equally qualified for either; and in classing creatures of this kind, authors are much divided, and sometimes mistaken.

Now if any natural historian should deduce his distinction of this class, from the structure or characteristic of any part of the animal, Dr. P. thinks he would be a little out of the way; because the term comprehends nothing but what regards its living in both air and water at discretion; however, since the word amphibious is adopted by the writers of the history of animals, let us retain it still, and examine some of this class, and, by considering their natural economy respectively, endeavour to range them according to that standard in the following manner. They are such as: 1. Enjoy their chief functions by land, but occasionally go into the water. 2. Such as chiefly inhabit the water, but occasionally go ashore. Of the latter there are but very few species. And though none of the winged tribe are to be ranged under this class, yet as many of them remain ong on the water, in search of their proper food, we shall enumerate some peculiar advantages, which have been allowed to several of them by the bountiful wisdom of the Creator, in order to render them the more able to obtain it; and this will make one curious part of the present purpose, not generally known.

As to the class of the phocæ, which consists of a very numerous tribe of different species; he thinks that none of them can live chiefly in the waters, but that their chief enjoyment of the functions of life is on shore. These animals are really quadrupeds; but, as their chief food is fish, they are under a necessity of going out to sea to hunt their prey, and to great distances from shore; taking care that, however great the distance, rocks or small islands are at hand, as resting places when they are tired, or their bodies become too much macerated in the water; and they return to the places of their usual resort to sleep, copulate, and bring forth their young, for the following reasons, viz. It is well known

* It is to be observed that Dr. Parsons in this excellent paper, uses the term, amphibious, in a large or general sense, and does not restrict it to the *Linnæan* Amphibia.

that the only essential difference, (as to the general structure of the heart), between amphibious and mere land animals, or such as never go into the water, is that in the former the oval hole remains always open. Now, in such as are without this hole, if they were to be immersed in water for but a little time, respiration would cease, and the animal must die; because a great part of the mass of blood passes from the heart, by the pulmonary artery, through the lungs, and by the pulmonary veins returns to the heart; while the aorta is carrying the greater part of the mass to the head and extremities, &c. Now the blood passes through the lungs in a continual uninterrupted stream, while respiration is gentle and moderate; but when it is violent, then the circulation is interrupted, for inspiration and expiration are now carried to their extent; and in this state the blood cannot pass through the lungs either during the total inspiration or total expiration of the air in breathing; for in the former case the inflation compresses the returning veins, and in the latter, by the collapsion of the lungs, these veins are interrupted also; so that it is only between these two violent actions that the blood can pass: and hence it is that the lives of animals are shortened, and their health impaired, when they are subjected to frequent violent respiration; and thus it is that in animals who have once breathed, they must continue to respire ever after; for life is at an end when that ceases.

There are 3 necessary and principal uses of respiration in all land animals, and in these kinds that are counted amphibious; the first is that of promoting the circulation of the blood through the whole body and extremities; in real fishes, the force of the heart is alone capable of sending the blood to every part, as they are not furnished with limbs or extremities; but in the others mentioned, being all furnished with extremities, respiration is an assistant force to the arteries in sending blood to the extremities, which, being so remote from the heart, have need of such assistance; otherwise the circulation would be very languid in these parts; thus we see, that in persons subject to asthmatic complaints, the circulation grows languid, the legs grow cold and œdematous, and other parts suffer by the defect in respiration. A second use of breathing is that, in inspiration, the variety of particles of different qualities, which float always in the air, might be drawn into the lungs, to be insinuated into the mass of blood, being highly necessary to contemperate and cool the agitated mass, and to contribute refined pabulum to the finer parts of it, which, meeting with the daily supply of chyle, serves to assimilate and more intimately mix the mass, and render its constitution the fitter for supporting the life of the animal. Therefore it is, that valetudinarians, by changing foul or unwholesome air for a free, good, open air, often recover from lingering diseases. And a third principal use of respiration is, to promote the exhibition of a voice in animals; which all those that live on the land do according to their specific natures.

From these considerations it appears, beyond contradiction, that the phocæ of every kind are under an absolute necessity of making the land their principal residence; but there is another very convincing argument why they reside on shore the greatest part of their time, and that is, that the flesh of these creatures is analogous to that of other land animals; and therefore, by over-long maceration, added to the fatigue of chacing their prey, they would suffer such a relaxation as would destroy them. It is well known that animals, which have lain long under water, are reduced to a very lax and even putrid state; and the phoca must bask in the air on shore; for while the solids are at rest, they acquire their former degree of tension, and the vigour of the animal is restored; and while he has an uninterrupted placid respiration, his blood is refreshed by the new supply of air, as explained above, and he is rendered fit for his next cruise: for action wastes the most exalted fluids of the body, more or less, according to its duration and violence; and the restorative rest must continue a longer or shorter time, according to the quantity of the previous fatigue.

Let us now examine by what power these animals are capable of remaining longer under water than land animals. All these have the oval hole open, between the right and left auricles of the heart, and in many the canalis arteriosus also: and while the phoca remains under water, which he may continue an hour or two more or less, his respiration is stopped, and the blood, not finding the passage through the pulmonary artery free, rushes through the hole from the right to the left auricle, and partly through the arterial canal, being a short passage to the aorta, and thence to every part of the body, maintaining the circulation: but, on rising to come ashore, the blood finds its passage again through the lungs the moment he respires.

Thus the fœtus in utero, during his confinement, having the lungs compressed, and consequently the pulmonary arteries and veins impervious, has the circulation of the blood carried on through the oval hole and the arterial canal; now so far the phoca in the water and the fœtus in utero are analogous; but they differ in other material circumstances: one is, that the fœtus, having never respired, remains sufficiently nourished by the maternal blood circulating through him, and continues to grow till the time of his birth, without any want of respiration during 9 months confinement; the phoca, having respired the moment of his birth, cannot live very long without it, for the reasons given before; and this hole and canal would be closed in them, as it is in land animals, if the dam did not, very soon after the birth of the cub, carry him into the water to teach him, so very frequently; by which practice these passages are kept open during life; otherwise they would not be capable of attaining the food designed for them by Providence.

Another difference is, that the phoca, as before said, would be relaxed by maceration by remaining too long in the water; whereas the fœtus in utero suffers no injury from continuing its full number of months in the fluid he swims in: the reason is, that water is a powerful solvent, and penetrates the pores of the skins of land animals, and in time can dissolve them; whereas the liquor amnii is an insipid soft fluid, impregnated with particles more or less mucilaginous, and utterly incapable of making the least alteration in the cutis of the fœtus.

Otters, beavers, and some kinds of rats, go occasionally into the water for their prey, but cannot remain very long under water. Dr. P. has often gone to shoot otters, and watched all their motions; he has seen one of them go softly from a bank into the river, and dive down, and in about 2 minutes rise, at 10 or 15 yards from the place he went in, with a middling salmon in his mouth, which he brought on shore; Dr. P. shot him, and saved the fish whole. Now, as all fœtuses have these passages open, if a whelp of a true water spaniel was immediately after its birth served as the phoca does her cubs, immersed in water, to stop respiration for a little time every day, Dr. P. makes no doubt but the hole and canal would be kept open, and the dog be made capable of remaining as long under water as the phoca.

Frogs, how capable soever of remaining in the water, yet cannot avoid living on land, for they respire; and if a frog be thrown into a river, he makes to the shore as fast as he can. The lizard kind, such as may be called water lizards, or lacertæ aquaticæ, are all obliged to come to land and deposit their eggs, to rest, and sleep; even the crocodiles, who dwell much in rivers, sleep and lay their eggs on shore; and, while in the water, are compelled to rise to the surface to breathe; yet, from the texture of his scaley covering, he is capable of remaining in the water longer by far than any species of the phocæ, whose skin is analogous to that of a horse or cow.

The hippopotamus, who wades into the lakes or rivers, is a quadruped, and remains under the water a considerable time; yet his chief residence is on land, and he must come on shore for respiration. The testudo, or sea tortoise, though he goes out to sea, and is often found far from land; yet, being a respiring animal, cannot remain long under water. He has indeed a power of rendering himself specifically heavier or lighter than the water, and therefore can let himself down to avoid an enemy or a storm; yet he is under a necessity of rising frequently to breathe, for reasons given before: and his most usual situation, while at sea, is on the surface of the water, feeding on the various substances that float in great abundance every where about him; these animals sleep securely on the surface, but not under water, and can remain longer at sea than any others of this class, except the crocodile, because, as it is with the latter, his covering is not in danger of being too much macerated; yet they must go on shore to copulate and lay their eggs.

The consideration of these is sufficient to inform us of the nature of the first order of the class of amphibious animals: let us now see what is to be said of the 2d in our division of them, which are such as chiefly inhabit the waters, but occasionally go on shore. These are but of 2 kinds, the eels and water serpents, or snakes of every kind. It is their form that qualifies them for loco-motion on land, and they know their way back to the water at will; for by their structure they have a strong peristaltic motion, by which they can go forward at a pretty good rate, whereas, all other kinds of fish, whether vertical or horizontal, are incapable of a voluntary loco-motion on shore; and therefore, as soon as such fish are brought out of the water, after having flounced a while, they lie motionless, and soon die.

Let us now examine into the reason why these vermicular fish, the eel and serpent kinds, can live a considerable time on land, and the vertical and horizontal kinds die almost immediately when taken out of the water; and, in this research, we shall come to know what analogy there is between land animals and those of the waters. All land animals have lungs, and can live no longer than while these are inflated by the ambient air, and alternately compressed for its expulsion; that is, while respiration is duly carried on, by a regular inspiration and expiration of air. In like manner, the fish in general have, instead of lungs, gills, or branchiæ; and, as in land animals, the lungs have a large portion of the mass of blood circulating through them, which must be stopped if the air nas not a free ingress and egress into and from them; so in fish, there is a great share of blood vessels that pass through the branchiæ, and a great portion of their blood circulates through them, which must in like manner be totally stopped, if the branchiæ are not kept perpetually wet with water; so that, as the air is to the lungs in land animals, a constant assistant to the circulation, so is the water to the branchiæ of those of the rivers and seas; for when these are out of the water, the branchiæ very soon grow crisp and dry, the blood vessels are shrunk, and the blood is obstructed in its passage; so, when the former are immersed in water, or otherwise prevented having respiration, the circulation ceases, and the animal dies. Again, as land animals would be destroyed by too much maceration in water, so fishes would, on the other hand, be ruined by too much exsiccation: the latter being, from their general structure and constitution, made fit to bear, and live in the water; the former, by their constitution and forms, to breathe, and dwell in the air.

But it may be asked, why eels and water snakes are capable of living longer in the air than the other kinds of fish? this is answered by considering the providential care of the great Creator for these and every one of his creatures; for since they were capable of loco-motion by their form, which they need not be if they were never to go on shore, it seemed necessary that they should be rendered capable of living a considerable time on shore, otherwise their loco-motion would be vain. How is this provided for? why in a most convenient manner; for this order of fishes have their branchiæ well covered from the external drying air, and are also furnished with a slimy mucus, which hinders their becoming crisp and dry for many hours, and their very skins always emit a mucus liquor, which keeps them supple and moist for a long time; whereas the branchiæ of other kinds of fish are much exposed to the air, and want the slimy matter to keep them moist. Now if, when any of these is brought out of the water, it was laid in a vessel without water, he might be kept alive a considerable time, by only keeping the gills and surface of the skin constantly wet, even without any water to swim in.

Dr. P. mentions something that relates to a family among the fish kinds, which is of a middle nature between the phocæ, and the real fishes of the sea, in one peculiar respect. This is the class of the phocenæ, or porpoises, of which there are several species; and these have lungs, and therefore are forced to come up to the surface to breathe at very short intervals; but, when brought on shore, have no progressive loco-motion. So that, having lungs, they resemble the phocæ, and in every other respect, the real fishes of the sea. Blasius, in his Anatome Animalium, p. 288, gives an account of one of these taken and brought on shore alive; the people let him lie, to see how long he could live out of the water; and he continued alive only about 7 or 8 hours, and exhibited a kind of hissing voice.

From what has been said, Dr. P. hopes it will appear rational, that these are the only two orders that can properly be deduced from the class of amphibious animals; and that the genuses of either order are very few in the animal world.

XXIV. An Account of some peculiar Advantages in the Structure of the Asperæ Arteriæ, or Wind Pipes, of several Birds, and in the Land Tortoise. By Dr. Parsons, F. R. S. p. 204.

Dr. P. having in the former discourse given an account of some particular phenomena in amphibious animals, which rendered them more happy and perfect in their animal economy towards their preservation; he now lays before the R. S. certain advantages in some birds, towards assisting them in the acquisition of their food, which they seek for in the water; and some of these swim on the water and dive down occasionally; others only wade into the water, in shallow places, as far as their long legs will carry them, without touching the water with their feathers, in search of their nourishment.

The natural history of 4 of them is very well set forth by authors; the other 2 are not mentioned, but barely by their names; and though the author has not described them, yet he knew the structures of their asperæ arteriæ, and was a person who made many observations in natural history, of whom he will speak in his turn. These birds are: the wild swan, colum, seras, crane, Indian cock, demoiselle. The structure of the wind pipe of all these is so singular and so little known, that he thought a proper notice of the subject would be agreeable to the R. S.

The wild swan is somewhat smaller than the tame one; this inhabits fresh rivers in land, while the wild one always resorts to great lakes and arms of the sea. These are two distinct species, the river swan, and the sea or wild swan; and yet it has been suggested, that the latter might become as tame and familiar as the others, if they were brought up young: and hence they were supposed to be the same. But this wonderful structure of the aspera arteria shows that they are different, for the river swan has it not; though a very modern author, who is certainly as well versed in natural history as any one whatever, has these remarkable words: "All the writers on birds, says he,* have described the swan: they have called it cygnus domesticus, and cygnus ferus, distinguishing it in its wild and familiar state into two species, but this is idle and unnecessary, the bird is wholly the same in both." It will be seen, however, by the description of the part mentioned, that they cannot be the same species; for, besides this formation of the pipe, Mr. Edwards shows their heads to be very different also.

In the general run of birds, the aspera arteria is nearly straight; that is, having no plications, but descending directly from the epiglottis into the cavity of the body, to lie on the sternum, and terminating in the lungs; whereas, in these birds, which are the subjects of this discourse, they have certain turnings within the sternum or breast-bone, and run back again to double up into the thorax; which elongates them to double the length of those in other birds of equal, nay of greater magnitude, than the birds that have them.

In the wild swan, the wind pipe runs down from its upper extremity under the epiglottis, in company with the œsophagus, till it comes within about 4 or 5 inches of the last vertebra of the neck; here the pipe quits the œsophagus, which keeps its course to the intestines, and makes a convex curve forward between the ossa jugalia, in a circular sweep, till it enters into a hole formed through a strong membrane in the centre between the insertions of the ossa jugalia into the sternum under the breast; and in that circular sweep is covered closely by the skin, so that, in that place, a very slight blow would destroy the bird. This hole is the beginning of a theca or cavity in the keel of the sternum, in which the pipe passes on to the end, and then returns back, forming a loop which is circular; and, passing out by another hole through the same strong membrane, makes another circular sweep within, and parallel to the exterior one,

* The author here alluded to is Sir John Hill.

and then rises in that round direction, till it enters the cavity of the thorax, and is divided into two branchiæ, which terminate in the lungs.

When one views this structure, it is impossible to avoid being surprized at the wonderful formation of this part, especially too if we attend to the noble contrivance for securing these circular volutions of the pipe, from compressing one another, or from bending into angles; for, if this was the case, their long and free respiration could not be maintained, and the end, for which the pipe is so formed, would not be answered. An explanation of this contrivance will be necessary in this place: there is a strong membrane, which arises from all the clavicle, and is inserted all along the jugal bone on each side, very stiff like a drum; and as the aspera arteria makes its anterior volutions between the latter, (for the posterior turning is that loop within the keel of the sternum described,) it was necessary that the pipe should be supported by a stay in each circular sweep, to prevent the impediments just mentioned. Accordingly there are three strong transverse ligamentous membranes, running from one jugal drum to the other; over the outer of which, the pipe goes into the keel of the sternum through the under hole, and, in its return, rides over two others in a circular direction, in its way to the thorax. These are the stays, which prevent its doubling back in an angle, in these two volutions; and in that within the theca there was no need of such a fulcrum, being secured in its bed from any external pressure.

This wild swan was brought alive from Philadelphia, but died soon after its arrival; and Dr. P. assisted in the dissection, and made these drawings from the prepared parts. He found no mention of this structure of the aspera arteria in the wild swan*, but originally in Bartholin, who took delight in comparative anatomy, from whom Blasius has taken it.

It is difficult to say what may be the real use of this kind of wind-pipe, in the several birds that have it, if it be not to procure them a longer retention of inspired air, (while they seek their food where they are obliged to remain some time immersed in water,) than if the pipe was straight, as in geese, ducks, and such like; for these and the river swan often dive down to feed, yet it is always in shallow places, and their continuance under water is very short; whereas the wild swan dwells upon and seeks his food in great lakes, and arms of the sea, and dives into deeper waters, and consequently requires a power of continuing longer without respiration than the others.

Poets and natural historians in great numbers have asserted that these birds sing very harmoniously; and this gave occasion to a friend of Dr. P's., to whom he showed these drawings, to surmise that this structure might be of use to them in singing; but Dr. P. never found any one who would say they ever

* Mr. Edwards found it in the swan he describes. See his History of Birds.—Orig.

heard either wild or river swans sing: and therefore he doubts it much. But if they do sing, the length of the pipe contributes nothing towards it; it is the glottis, which forms the voice, and modulates it, whether the pipe be long or short: besides, none of the song, or speaking-birds, have any flection in their pipes, that we are acquainted with.

The crane is the next that Dr. P. mentions, which has such a turning of the asperia arteria in the keel of the sternum; but the volution of this bird is round within the bone, and may be compared to that of a French-horn; whereas that of the wild swan is straight within the bone, and may be compared to a trumpet; yet the entrance of this into the sternum, and its exit, and its passage into the cavity of the thorax, are similar to those of the swan. This is a bird which cannot go upon the water, being no more capable of swimming than a common cock or hen. His feathers will not admit of it; and, having no webs to his toes, he is unable to swim. It is somewhat surprising that not one of the tribes which are similar to the crane, such as the herons, storks, bitterns, &c. has any such structure of the asperia arteria; and yet they all feed upon fish or water insects. We are however, to consider that the heron chiefly haunts brooks, springs, and the narrow heads of rivers, where he seeks his food, and finds it with ease: but the crane is under a necessity of immersing its head, and remains a considerable time in that situation upon strands and marshes: it is also a bird of congress; for at certain seasons, a multitude of cranes flock together, and rise upon the wing to a great height in the air, being birds of passage, and they are by many authors said to travel from most parts of Scythia to Egypt, where, for a certain season, they remain about the Nile, and the great lakes of that country. Perhaps this elongation of the windpipe in them may be also of use, in their great flight through various degrees of rarefied or condensed air, in the variety of climates through which they pass.

The Indian cock, Gallus Indicus of Aldrovand and Longalius, and Gallus Persicus of Johnston, the Mutu Poranga of Margrave, is not the Coq d'Inde, or Turkey cock, but by the last author ranged among the pheasant tribe; this bird has a plication of the aspera arteria, but not so considerable as either of the forementioned swan, or crane: for it descends in a straight line, along with the œsophagus, to the middle of the jugal bone, without and above the thorax, where it is spread and fastened on each side. Then, turning backwards, being somewhat flat, it makes a fold upward to about an inch and half high, and there being made fast again, by a strong membrane, it doubles down and passes into the thorax, terminating by two bronchia in the lungs: and where it is fastened and folded, that is, in the flat parts, it is triple the circumference of any other part of the pipe. This bird, and another of the same species, were dissected by the Royal Academy of Sciences, and this structure of the pipe appeared in both;

for which it is difficult to assign a reason in any of the pheasant kinds, if it be not to retain inspired air longer than ordinary on some occasions, though they are not frequenters of rivers or marshy grounds; which one might reasonably suggest from the great capacity in the plicated parts of the windpipe.

The next he mentions is the Grus Numidica, Numidian crane, or Demoiselle. This bird has likewise a plication in the windpipe, which was also dissected by the Academy of Sciences, in whose account the natural history of it may be seen; and a true description and figure of it from the life, by Mr. Edwards, in his Natural History of Birds. Dr. P. confines himself only to the configuration of this point, in as many animals as he can find endowed with such a structure, in order to collect them, and lay them here in view: and will hereafter make further researches and dissections, in such as he may reasonably suppose to have a different formation from the common standard of a straight windpipe. In this bird, being of the crane kind, the pipe runs down in company with the œsophagus, to about a foot in length, and then turns outwards and forwards, as it does in the swan and crane, and enters into the keel of the sternum, which, like the others, forms a bony box for its reception, through a ligamentous hole for about 3 inches: then it returns upwards, and a round turn into the thorax terminating in bronchia and lungs. Now, in the Indian cock mentioned, the plication is made above the sternum, in a roomy part between the jugal bones; whereas, in the others mentioned, the plication is within the keel of the sternum.

The other birds he finds any account of, having the asperia arteria folded, are only 2, and of these our information is very short. In Dr. Fryer's account of India and Persia, where he treats of his description of Surat and his journey into Duccan, p. 119, is found the following passage: "Fish, oysters, soles, and Indian mackerel, the river yields very good; and the pools and lakes store of wild fowl: particularly brand geese, colum, and serass, a species of the former; in the cold weather, they, shunning the northern rigid blasts, come yearly hither from mount Caucasus; what is worth taking notice of, is their asperia arteria wound up in a case on both sides their breast-bone, in manner of a trumpet, such as our waits use: when it is single, it is a serass; when double, it is a colum, making a greater noise than a bittern, being heard a great while before they can be seen, flying by armies in the air."

From this passage, it is plain that our author had examined the interior parts of this colum and serass, and that they are different species of the same genus. We can only however endeavour to find what this genus is; and, by what we have heard of the crane, it is not improbable that they are of that kind. The crane, by every author, is said to take long flights in vast multitudes; and to make a great noise in the air. The colum and serass are said to come to the rivers and lakes about Surat or Duccan from Caucasus, flying in armies, and making so great a noise, that they are heard a great while before they are seen. Again, by his short account we may easily learn, that they are not the crane that is described in this paper, because the colum has a volution of the wind-pipe on both sides of the keel of the sternum, the crane but one; and it seems, from the likeness of the serass to the colum, he says, that the former is a species of the latter; nor can we have room to suggest that these birds are of the wild goose kind; because he mentions the brand geese first, without taking the least notice of their aspera arteria; and confines the rest of the paragraph to the other two, calling the one a species of the other.

These are all the birds that have hitherto come to notice, having this remarkable flection in the aspera arteria; it now only remains to mention further that of the land tortoise, which Dr. P. brings in here on account of a similar formation of that organ in him. Having never dissected a land tortoise himself, he has recourse to those that have; and accordingly he finds its parts examined by a celebrated physician of his time, Velchsius, from whom Blasius has taken it, and by the Academy of Sciences. Blasius has however made the anatomical distribution of its parts and their explanation: from whom Dr. P. takes the figure; and though this last author quotes Severinus and Coiterus as dissectors of the tortoise, as well as Velchsius, yet this latter only mentions the windpipe. In Blasius's figure, this pipe, for a few inches from the epiglottis, is single, but soon divides into two; and as it descends in company with the œsophagus, it forms a folded ring outwards on each side, and turns down again to enter the lungs: so that the animal has the advantage of a double aspera arteria, with a volution in each: which shows that this provision is intended to contain a greater portion of air than ordinary while he is under ground in winter.

The tortoise dissected by the Academy of Sciences, was a large land tortoise from the coast of Coromandel; in which was found a bifid windpipe; each branch is said to be 6 inches long, but no mention is made of the volutions in this land tortoise; which, one would think, being so remarkable a variety from the common standard, ought not to escape the notice of such able anatomists. Yet it was found, by several experiments, that respiration was very slow and unequal in this animal, as well as in the camelion; the Academicians observed several tortoises for a long time together, and have taken notice that they sometimes cast forth a cold breath through the nostrils, but that is by long intervals and without order; and that the camelion is sometimes half a day without one's being able to discern in him any motion for the respiration. From this it is easily seen, that they can retain inspired air a long time: and the Academicians therefore think, that the principal use of the lungs in tortoises, is to render themselves specifically lighter or heavier in the water, by their inflation and compression at will, as fishes do by their swimming bladders; indeed such a power of long inspiration seems to be as necessary in the land tortoise as in that of the sea; because, in many countries where they breed, they are known to go into the ground and lie concealed for several months; and it is well known, that several species of land tortoises go into ponds or canals in gardens, where they are kept, and remain long under water at pleasure. Of this, Mr. Collinson had instances in his gardens at his country seat: and Dr. P. saw two land tortoises in the bottom of a circular canal, in the gardens of the palais royal in Paris several times, which were not very large ones, and remained under water many hours together.

The ingenious Academicians however, in order to verify their sentiments, that one principal use of the lungs in a tortoise is to render it capable of remaining at any depth in the water at will, made the following experiment; they locked up a living tortoise in a vessel of water entirely full; on which there was a cover exactly fastened with wax, from which there went a glass pipe: the vessel being full, so as to make the water appear at the bottom of the glass pipe; they observed that the water sometimes ascended into the pipe, and that it sometimes descended. Now this could be done only by the augmentation and diminution of the bulk of the tortoise; and it is probable that when the tortoise endeavoured to sink to the bottom, the water fell in the pipe, because the animal lessened its bulk by the contraction of its muscles; and that the water rose by the slackening of the muscles, which, ceasing to compress the lungs, permitted it to return to its first size, and rendered the whole body of the tortoise lighter. Dr. P. has, in many kinds of fish, dissected their swimming bladders, and found that in great and small these are vested with a strong muscular membrane, which they are capable of contracting and dilating at will, by which they are able to compress or expand the column of air within very considerably; this bears some analogy to the detrusor muscle of the human urinary bladder, in contracting itself for the expulsion of the urine.

Plate 9, fig. 1, represents the situation and inclosure of the aspera arteria of the wild swan, in a lateral view; *a* is the aspera arteria; *b* the œsophagus; *c* vertebræ of the neck; *d* the ossa jugalia; *e* the keel of the sternum; *h* the membranes that support the aspera arteria. Fig. 2, a view of the same, with its progress within the thorax on the sternum. Here *a* is the aspera arteria, passing in the theca, and doubling round to lie upon the sternum, ending in *b*, the branchia. Fig. 3, The aspera arteria of the Indian cock. This plication is made between the jugal bones, and not in the breast bones, as in the others, and then the end, with the bronchia, enters the thorax, and lies upon the sternum. Fig. 4, A lateral view of the aspera arteria of a crane, as situated in the cavity formed in the sternum. Fig. 5, shows the flection of the aspera arteria of the Numidian crane; having the plication made within the keel of the sternum. Fig. 6, the bifid aspera arteria of the land tortoise. The pipe on each side has a small figure under each of the white fascia, which admits air into every respective lobe: so that the lobes are not filled at once by inspiration, but successively downwards.

XXV. Observations on the Variation of the Magnetic Needle, as made on Board the Montagu Man of War, in the Years 1760, 1761, *and* 1762, *by Mr. David Ross, Surgeon. By Mr. W. Mountayne, F. R. S.* p. 216.

The following tables Mr. M. compared with the Variation Chart, published in the year 1756, and found that they agree pretty well in general, making allowance for the time elapsed *: it is true, that in some few places in the Atlantic Ocean they differ; yet this may probably arise, as is often the case, from an error in the Montagu's supposed longitude, where such observations were made. But the greatest difference, a greater than should arise, he thinks, according to common course, appears on the coast of Portugal, Cape Saint Vincent, and about Gibraltar, near and within sight of land, where the observations are ascertained to the spot. Hence if Mr. M.'s observed about the year 1756, and those of Mr. Ross's, were both near the truth, at the respective times when they were taken, he knows not how to account for this considerable increase, unless those late extraordinary convulsions, in the bowels of the earth, on those several coasts, may be found, by further experiments, to have there influenced the directions of the magnetic needle.

On the Variation of the Magnetic Needle; with a Sett of Observations made on Board His Majesty's Ship, Montagu, during the Years 1760, 1761, *and* 1762. *By Mr. David Ross, Surgeon.* p. 218.

These observations Mr. R. had an opportunity of making, with great care and accuracy on the deflection of the magnetic needle, by an azimuth compass of Dr. Gowen Knight's construction, during a passage to the West-India islands, on a cruise there, and return to Britain; and likewise a cruise about the Straits of Gibraltar: usually taking a medium of 6 or 10 azimuths, or more. Mr. R. also remarks with surprize that he always found the variation less at anchor, in the West-India islands, than at sea, though near the same spot. As for instance, June 18, 1760, 3 leagues south of Prince Rupert's Bay, Dominico, he found the variation, by a medium of 5 azimuths and an amplitude, to be 5° 27′ E: and in the bay itself, June 20, by 3 azimuths and an amplitude, it was 3° 27′; and on the 22d, by 6 azimuths and amplitude, 3° 12′ E. The medium of these is 3° 20′ E., full 2° 7′ less than in the offing, though but 9 miles off. The same phenomenon appeared on experiments repeated March 23d 1761, when in the same bay, the medium of 8 azimuths gave 3° 19′ E. Off of Antigua, and at anchor in Saint John's Road, the same thing was observed, though the difference was not so great.

* By this expression, I do not mean that the variation undergoes a regular and uniform alteration, notwithstanding such appearance, but rather suspect the contrary.—Orig.

A table of the variations of the magnetic needle, as observed on board his Majesty's ship Montagu, in the years 1760, 1761, and 1762, by Mr. David Ross; and communicated by Mr. William Mountaine, F. R. S.

Time when.	Latitude in	Long. fr. Lon.	Variation.
1760			
April 12	Off Bell-Isle and Grois.		19° 11′ w.
26	43° 23′ N.	11° 15′ w.	18 51 w.
29	40 20 N.	13 39 w.	19 11 w.
May 2	40 18 N.	18 36 w.	17 0 w.
5	34 44 N.	21 42 w.	14 30 w.
6	31 38 N.	22 25 w.	14 15 w.
7	30 7 N.	23 12 w.	13 50 w.
9	28 51 N.	25 39 w.	10 4 w.
10	27 2 N.	28 40 w.	10 30 w.
11	26 10 N.	30 30 w.	10 16 w.
12	25 9 N.	32 37 w.	7 10 w.
13	24 3 N.	34 14 w.	5 59 w.
14	22 51 N.	35 51 w.	5 0 w.
15	21 40 N.	37 33 w.	4 20 w.
17	19 8 N.	41 27 w.	1 30 w.
18	18 4 N.	43 18 w.	1 0 w.
19	16 58 N.	45 9 w.	0 20 w.
20	16 7 N.	46 50 w.	0 30 E.
21	14 52 N.	48 39 w.	1 7 E.
22	13 38 N.	50 28 w.	1 59 E.
23	13 13 N.	51 48 w.	2 12 E.
25	13 30 N.	56 5 w.	3 45 E.
26	13 10 N.	57 50 w.	3 58 E.
28, 29 & 31	In Carlisle, Barbadoes		4 3 E.
June 1	Barbadoes, S. E. 10½ leagues dist.		5 20 E.
2	15° 19′ N.	59° 0′ w.	5 12 E.
3	16 16 N.	59 18 w.	4 24 E.
4	Antigua w. N. w. dist. 6 leagues		4 51 E.
6	Redondo E ½ N. 4 leagues		4 50 E.
8	Montserrat S. by E. Redondo S. by w.		5 32 E.
15	In the pas. bet. Martin. and Domin		5 6 E.
16	Off the S. w. end of Martinique		5 41 E.
17	Off the S. end of Dominico		5 21 E.
18	S. of Rupert's Bay, Domin. dis. 3 lg.		5 27 E.
20, 22	In Prince Rupert's Bay — 3° 27 Dominico 3 12		3 20 E.
July 11	Deseada S. w. by w. dist. 3 leag.		5 3 E.
12	15° 0′ N.	59° 7′ w.	5 35 E.
Sept. 15, 16, 18	In Grand Courland's Bay At the S. w. end of Tobago, the medium is		4 32 E.
Oct. 5	N. end of Tobago E. N. E. dist. 5 lg.		4 45 E.
6	D° E. by S. dis 4 leagues		5 0 E.
10	Isl. Bequia N. w. by N. ½ w. dis. 7 lg.		4 0 E.
14	Off the N. w. end of St. Lucia		5 14 E.
15	Off Saint Pierre's Martinico		5 17 E.
22	At the wind of the Channel between Martinique and Dominico		5 19 E.
23	At the E. end of the same		5 36 E.
1761 24	Saint Lucia w. ¼ N. dist. 15 lgs.		5 34 E.
Mar. 23	In Prince Rupert's Bay, Dominico		3 19 E.
April 4	In St. John's Road, Antigua		3 9 E.
15	Off the east end of Antigua		4 31 E.
May 4	In Carlisle Bay, Barbadoes		3 47 E.
July 27	At the Dog and Prickly Pear		4 44 E.

Time when.	Latitude in	Long. fr. Lon.	Variation.
1761			
July 28	19° 35′ N.	62° 50′ w.	4° 35′ E.
29	20 46 N.	62 43 w.	4 27 E.
30	22 20 N.	62 30 w.	2 42 E.
31	23 42 N.	62 36 w.	2 15 E.
Aug. 1	25 5 N.	62 37 w.	1 38 E.
2	26 20 N.	62 57 w.	1 18 E.
3	27 26 N.	62 37 w.	0 44 E.
4	27 56 N.	62 43 w.	0 3 E.
5	28 5 N.	62 50 w.	0 23 E.
6	28 11 N.	62 35 w.	0 1 E.
7	28 40 N.	62 59 w.	0 27 w.
8	29 23 N.	63 1 w.	0 48 w.
9	30 1 N.	62 59 w.	1 33 w.
10	31 1 N.	62 21 w.	1 58 w.
11	32 41 N.	60 37 w.	2 45 w.
12	33 58 N.	59 14 w.	4 13 w.
13	35 3 N.	57 54 w.	5 0 w.
15	36 31 N.	54 57 w.	7 4 w.
16	37 33 N.	52 55 w.	8 20 w.
17	37 59 N.	51 4 w.	9 40 w.
18	38 8 N.	50 13 w.	10 20 w.
19	38 36 N.	48 41 w.	10 12 w.
20	38 23 N.	47 48 w.	10 57 w.
21	38 57 N.	46 30 w.	11 52 w.
22	39 22 N.	45 52 w.	12 0 w.
23	40 13 N.	44 29 w.	12 0 w.
24	41 8 N.	42 26 w.	12 0 w.
26	41 31 N.	41 42 w.	13 0 w.
27	42 0 N.	39 58 w.	13 35 w.
28	42 38 N.	38 11 w.	13 36 w.
29	43 15 N.	36 21 w.	14 0 w.
30	43 0 N.	35 53 w.	14 30 w.
31	43 28 N.	34 40 w.	14 30 w.
Sept. 1	44 45 N.	31 58 w.	17 11 w.
2	45 36 N.	29 49 w.	18 51 w.
4	48 14 N.	25 17 w.	18 31 w.
5	48 57 N.	23 13 w.	19 37 w.
6	49 11 N.	20 28 w.	19 30 w.
7	50 35 N.	18 17 w.	19 52 w.
10	Off Cape Clear in Ireland		19 55 w.
20	Scilly S. S. E. dist. 5 leags.		19 55 w.
21	The Start E. ½ N. dis. 16 ml.		20 16 w.
Dec. 8	36° 25′ N.	7° 45′ w.	16 53 w.
19, 25, & 26	In Gibraltar Bay		17 11 w.
1762 Jan. 23	35° 46′ N.	7° 2′ w.	18 3 w.
Jan. & Feb.	Bet. Cape St. Vincent & the Straits Mouth		17 31 w.
Feb. 11	In Cascais Road		17 32 w.
D° 12, 23, & March 2	In Lisbon River		17 32 w.
5	37° 56′ N.	9° 55′ w.	17 29 w.
	Bet. C. St. Mary's & C. Spartel, the medium is		17 15 w.
From 3 to 12 leag. E. off Gibral. Hill on the	Euro. Shore		17 20 w.
	Bar. Shore		18 30 w.

XXVI. A New Manner of Measuring the Velocity of Wind, and an Experiment to ascertain to what Quantity of Water a Fall of Snow is Equal. By Alex. Brice. Dated Kirknewton, May 13, 1766. p. 224.

Mr. B. attempted to determine the velocity of the wind by letting light downy feathers fly in it; being the method used by Dr. Derham; but in all the trials he made, though he has let 50 of these feathers fly, one after the other, at a time, he has never seen above 1 or 2 at most, on which he could have founded a calculation. The velocity of the wind near the earth is very unequal, on account of the frequent interruptions it meets with from hills, trees, and houses; and even in open plains, the surface of the earth, though much smoother than it commonly is, must reflect, and interrupt such a fluid as the air, and occasion great irregularity in the velocity of its current: this is the reason, when a feather is let fly with the wind, why it seldom, if ever, describes a straight line, but moves sometimes in a kind of spiral, now high, and then low, sometimes to the right, and then again to the left; and why two feathers let fly at once, seldom, if ever, keep together, or describe similar lines.

But, at some considerable distance from the earth, the velocity of the wind seems to be regular and steady: nothing can be more uniform than the velocity of a cloud in the sky appears to be, even in the greatest storm: it is like a ship carried away insensibly by a smooth and gentle current, passing over equal spaces in equal times. This suggested the thought, that the motion of a cloud, or its shadow, over the surface of the earth, would be a much more proper measure of the velocity of the wind. In the end of March 1763, Mr. B. had as favourable an opportunity of putting this method into practice, as he could wish; the storm was exceedingly high, and moved with vast velocity; the sun was bright, the sky clear, except where it was spotted with light floating clouds; he took his station in the north window of a room, near the clock, from which he had a free prospect of the fields; the sun was in the meridian, the wind due west intersecting his rays at right angles; he waited till the fore-part of the shadow of a cloud, that was distinct, and well defined, just touched a south and north line, which he had marked on the ground; at that instant he began his reckoning, and followed the shadow with his eye in its progress, counting seconds all the while by the clock, till he had reckoned up 15 seconds; then he observed exactly where the foresaid edge of the shadow was. This experiment he repeated 10 times in half an hour, and seldom found the difference of a second, in the time which different clouds took to move over the same space. On the 5th of May current, he repeated the trial 4 different times, the sun being also near the meridian, the wind in the west, with light clouds floating in a clear sky as formerly; and found that the shadows of different clouds took some of them 44, and others 45 seconds, to pass over the same space which they had moved over in 15 seconds, in the former trials.

	Feet
This space measures exactly	1384 = space passed over in 15 seconds,
which multiplied by	4
gives	5536 = space passed over in one minute,
which multiplied by	60
gives	332,160 = space passed over in one hour.

Which space is = 62.9 English miles per hour, the velocity of the wind in March 1763. One-third of this, or 21 miles nearly, shows the velocity of the wind on May the 6th, when it blew a fresh gale. May 12, there was a small westerly breeze, the velocity of which he measured on the same line, the sun being 10 minutes past the meridian, and found that the shadow took 95 seconds to pass over the above space; which gives the velocity of the wind at the rate of 9.9 English miles per hour.

Thus, by having several lines in different directions of a known length marked on the ground, one may easily, and with great accuracy, measure the velocity of the wind. If a person was provided with an instrument for measuring the force of the wind, it would perhaps be worth while to observe whether, when the velocities of different winds were the same, or nearly so, the forces of these winds did not vary with the seasons of the year, the points of the compass from which the wind blows, and also with the different state of the barometer and thermometer, since the momentum of the wind depends not only on its velocity, but also on its density.

From the end of March 1765, to the end of March last, in that part of Scotland, they had very little rain, and less snow in proportion; the rivers were as low, through the winter, as they used to be in the middle of summer; springs failed in most places, and brewers and maltsters were obliged, even in winter, to carry their water from a considerable distance.

In the end of March last, they had a fall of snow; and, as he did not remember to have ever read an account of such an experiment, he wished to be able to determine, to what quantity of rain this fall of snow was equal. The snow had been falling from 5 o'clock the former evening, till 10 o'clock next day; about 11 o'clock he measured the depth of the snow, and found it to be 6.2 inches; he then took a stone jug, holding about 3 English pints, and turned the mouth of it downwards on the snow measured, and where the ground below was smooth, and hard; and by this means he took up all the snow from top to bottom in the jug; this snow he melted by the side of a fire, and the 6.2 inches of snow yielded 6 tenths of an inch deep of water in the same jug. After emptying the jug, he dried, and weighed it in a balance, and took up the same quantity of snow in it as before, weighed it again, and found the weight of the snow taken up, and from this weight computed what quantity of water it should have produced, and found that it ought to have produced 6 tenths of an inch, and $\frac{1}{20}$ of an inch more; he then dissolved the snow, and found that it yielded a quantity of water in the bottom of the jug, 6 tenths of an inch deep as in the former experiment. The difference of $\frac{1}{20}$ of an inch in the depth of the water, between the weight and the melting of the snow, was probably owing to an exhalation from the jug, while the snow was melting by the fire, for he observed a steam sometimes rising from it. A greater or less degree of cold, or of wind, while the snow falls, and its lying a longer or shorter time on the ground, will occasion a difference in the weight, and in the quantity of water produced, from a certain number of cubic feet, or inches, of snow; but if he may trust to the above trials, which he endeavoured to perform with care, snow, newly fallen, with a moderate gale of wind, freezing cold, which was the case of the snow he made the trials on, the 27th of March last, will produce a quantity of water equal to $\frac{1}{10}$ part of its bulk; or the earth, when covered with snow, 10 inches deep, will be moistened by it when melted, or rivers and springs recruited, as much as if a quantity of rain had fallen that had covered the surface of the earth to the depth of one inch.

XXVII. Observations on the Country and Mines of Spain and Germany, with an Account of the Formation of the Emery Stone. By William Bowles, Esq. Director General of the Mines of Spain. p. 229.

At the extremity of Old Castile, in Spain, is situated a territory called Montana, which is divided into two parts. The Low Montana is that chain of mountains, which bounds the Cantabrian sea. The city Santander is its chief port, whence you ascend southerly, 12 long leagues, a succession of high craggy mountains, to the town of Reynosa in the upper Montana, which extent stretches 3 leagues more, and then you continually descend about 14 leagues to the city of Burgos, the capital of Old Castile. Reynosa is in the centre of an open plain, surrounded by a ridge of high mountains, at whose feet are low hills of pasture-land. To the west of Reynosa, in an hour's walk, is the source of the great river Ebro, which receives all the waters on that side, and conveys them into the Mediterranean, 7 leagues below the city Tortosa. All the spring, rain and snow waters, of the mountains to the north of Reynosa, run into the Bay of Biscay. The waters, from the south chain of the mountains, are collected in the river Pisverga, which runs into the river Duero, and from thence are carried to the Atlantic ocean at Oporto. Hence we see, that the adjacent parts of Reynosa divide the waters of the three seas, which lie north, east, and west.

Eight leagues square of this upper Montana is the highest land in Spain; the mountains rise in the atmosphere to the line of congelation; snow is seen from the window this 4th of August, while writing this letter. Some years ago there used to fall so much snow, that the people were forced to dig lanes through it, to go to church in the winter; but there has fallen little snow since the earthquake at Lisbon, and some years none at all. Mr. B. is persuaded it changed the climates of many parts of Spain; for no man living saw, nor heard his father say he saw, snow fall in or about the city of Sevil, till the year 1756. He found many plants in these mountains which he had seen in Switzerland; they abound with oak, beech, birch, holly, and hazel. The hills and plains are fine pasture; he never saw a meadow in any other part of Spain, neither did he see horses and cows feed on hay any where else.

These mountains are formed of sand-stone, lime-stone, plaster-stone or gypsum, and emery-stone. The sand-stone is at the summit of the mountains, and some hills, and the lime-stone forms the body; but the contrary is seen in others; the sand-stone abounds, and the plaster is always lowest. As for example, the high mountain of Arandilla, which is a small league off the town, is all sand-stone at the summit; its body is a mass of ash-coloured lime-stone, in which are found imprisoned petrified cornu ammonis, and scollop shells; and there are beds of plaster-stone at its foot, towards the plain; these join to a stratum of black marble veined white and yellow, which is no more than a purer kind of lime-stone, as all other marbles are.

On the hill to the east of Reynosa, and in the plain, are found blocks of emery-stone, which the looking-glass grinders of the king's fabric at St. Ildefonso say is the most biting emery they ever used; and Mr. B. never saw any other in its native matrix. That iron has been, and is now, in a fluid state, percolating through the earth, and that it subsides, crystallizes, or is precipitated, to form different bodies, is demonstrated by the black and red blood stone, by some beautiful stalactites, which are almost pure iron, by the eagle-stone, by figured pyrites, by native vitriol, and by native crocus. When this fluid iron penetrates a rock of sand-stone, and only stains the surface of each grain, of a brownish, reddish, or yellow colour, it becomes only sand and crocus; but when it is joined with the crystalline matter in a fluid state, in the very act of crystallization of each grain of sand it incorporates with it, its weight and hardness is increased, and then it becomes emery.

The earth of the mountains and hills is of the same nature as that of the rock below. If it is lime-stone, the soil cast into any acid liquor will boil up with a violent effervescence, and the acid will dissolve it. If the rock below be sand-stone, or plaster-stone, or emery, the earth of that hill or mountain will remain quiet in the acid, and there is no effervescence nor dissolution. When the rocks below are mixed, calcary and noncalcary, the soil of the surface is also of a mixed nature; and he always found the action of the acid to be weak or strong on these earths, in proportion to the sort of stone with which they abound.

Thirty-one leagues south-east of Madrid, and 5 leagues south of the source of the river Tagus, is situated the town of Molina Aragon, capital of a lordship of the crown, almost in the centre of Spain. The high hills of this little territory are covered with pine trees. Here he learned some truths, which prove that the following opinions ought to be ranked among vulgar errors.

First, that salt-springs are not found in the high primitive mountains, but in the low hills and plains only. The elevated town of Molina, and the rocky country about it, is formed of red and grey sand-stone, lime-stone, white and grey granite. These rocks contain either salt, or salt-petre; the houses built of this stone are covered with the saline efflorescences, which are drawn out by the sun after rain. The whole territory of Molina is full of salt springs; but there is a copious salt spring, rising out of a land yet higher than the source of the Tagus, and not far from it, which is one of the highest territories in all the inland parts of Spain, for it divides the waters of the ocean and Mediterranean. This spring furnishes salt to the jurisdiction and bishopric of Albarrazen. There is besides another salt spring, in another elevated ground, which supplies the 82 towns and villages of Molina-Aragon with salt: besides which, there is a salt spring, issuing out of a spot in the Montana, which is higher than the fountain of the Ebro, and about a quarter of a mile distant from it.

Secondly, that metallic vapours destroy vegetation; the following instances evince the contrary. There are many iron, copper, lead, and pure pyritous ores, in these mountains; and yet the same plants, and the same sweet grass grow there as in other parts. About 2 hours walk northwest of Molina, there is a little hill called Platilla; it extends about half a league over, from valley to valley; its body is a solid, rocky, white granite, through which run, in different directions, and without any order, an infinite number of blue, green, and yellow veins of rich copper ore, which holds a little silver, mineralized by a great quantity of arsenic and sulphur: the very surface of the rock is in many places stained blue, and green, and the veins of ore are not above a foot deep. In the fissures, and in the solid rock, is contained lead ore, which is sometimes found even on the surface; and yet the following plants grow out of the soil, which covers these arsenical sulphurous veins, and it is not more than a foot deep; true oak, flax, white-thorn, juniper, cystus, wild rose, uva ursi, phlomis, verbascum, stœchas, sage, thyme, serpillum, rosemary, and many others. The earth of this same hill is covered with the same sweet small grass as the rest of the country.

Mr. B. has also made the same observations, out of Spain, at the 3 greatest mines in Europe, viz. St. Mary of the mines in Alsatia; Clausthal, in the Hartz-mountains of Hanover; and Freyberg, in Saxony. The mines of St. Mary are at the head of a valley. Its hills are some of them covered with oak, pines, and others with apple, pear, plum, and cherry, and others, with fine grass downs. The tops of others are fields of wheat, which, in the year 1759, gave a produce of 8 for one. All these vegetables grow in a soil, a foot or two deep, which covers a rock, full of the most arsenical, sulphureous, silver, copper, lead, and cobalt ores, in Europe, and most of the veins are near the surface. The mines of Freyberg are in low hills near the city. He saw them all covered with barley in July. A stranger would not imagine that men were reaping corn over hundreds of miner's heads, who were blowing up veins of ore, arsenic, and brimstone. The mines of Clausthal are in a plain, which, in truth, is the summit of a mountain; the Dorothy and Caroline veins of silver, lead, and copper ore, stretch away 8 miles to the Wild-Man mountain; the finest meadows and sweetest grass are on these veins, and all their branches near the city: they feed 900 cows, and 200 horses; they are mowed in June, and a second crop springs up, which is mowed in August; a multitude of plants grow in these high meadows, over the mines.

It is true, he saw mines in the barren naked mountains and hills; but it is certain that their barrenness is not the effect of mineral vapours; but the air, moisture, heat, and cold, have more power over the surfaces of some rocks, than of others, to moulder the stone into earth. Such is the high mountain of Ramelsberg, above Goslar, whose inhabitants have lived by the mines found there. Mr. B. crept up this steep rock to its summit; he found it split and cracked into millions of fissures, from 1 foot to an inch wide; in other places it was shivered into small rotten stones, which became a receptacle for a few plants, grass, moss, &c. and, as this decayed stone moulders into earth, it will be more abundant in vegetable productions: this may perhaps have been the original state of those mountains, which are now covered with verdure.

XXVIII. On the Electrical Nature of the Tourmalin. By Torbern Bergman, F. R. S., &c. From the Latin. p. 236.

Mr. B. first describes several tourmalins in the possession of the R. S. of Stockholm, on which he made experiments. They are very various both as to shape and colour; and are brought from different countries, as Ceylon, Brasil, &c. As to the electric nature of the stone, he states it as a fundamental law, that of any tourmalin, the one pole by dilatation acquires a positive electricity, and by contraction a negative one; but that the other has a contrary nature, so as by contraction to be made positive, and by dilatation negative. By a state of contraction, the author means that in which the capacities of the pores are diminished, and a state of dilatation, when they are enlarged. Hence he determines various particular cases of its states of positive and negative electricity.

XXIX. Theory of the Parallaxes of Altitude for the Sphere. By Mr. F. Mallet, Professor and Astronomer at Upsal. From the French. p. 244.

Let P be = the moon's horizontal parallax, or 1 to sin. P, as the moon's distance to the radius of the terrestrial sphere, on which the spectator is supposed to be placed. Let A be the distance of the moon from the zenith, and p the parallax of altitude for the same distance. The astronomers usually compute the value of p in the following manner: let sin. p = sin. P sin. A, and p' being found by the tables of logarithmic sines, sin. p'' = sin. P. sin. (A + p') is found in like manner, p'' being assumed for the true parallax, which is not accurate.

2. In order to show this, Mr. M. has given another method of computing the parallax of altitude as exactly as may be, by means of the common tables, in the following manner: since sin. p = sin. P. sin. (A + p'), we have sin. p = sin. P sin. A cos. p + sin. P cos. A sin. p, or sin. p (1 — sin. P cos. A) = sin. P sin. A cos. p; hence tang. $p = \frac{\text{sin. P sin. A}}{1 - \text{sin. P cos. A}}$. This formula seems a little difficult to be wrought in numbers, but it is as easy as the one above: for, supposing sin. B^2 = sin. P cos. A, the tables will give the angle B, and tang. $p = \frac{\text{sin. P sin. A}}{\text{cos. B}^2}$, the computation of which can give no trouble. Hence it appears, that the calculus for finding the true parallax is not more difficult than that which gives the said parallax with an error, the value of which is unknown; for it is evident that the above computation for finding p'' is only an approximation, and that to make it accurate, it would be necessary to carry it still on by finding sin. p''' = sin. P sin. (+ p''), and afterwards sin. p'''' = P sin. (A + p''') &c.

3. Mr. M. therefore thinks himself in the right to prefer this method to that hitherto used by astronomers. To confirm his opinion, he made a trial, by putting P = 59′ and A = 30°, and found $p - p'' = 0''.43$, in which the error of the usual computation amounts to near half a second; he therefore gives the preference to the geometrical calculus.

4. Before he quits the formula tang. $p = \frac{\text{sin. P sin. A}}{1 - \text{sin. P cos. A}}$, he observes, that the computation of p may be executed by other methods to the same exactness. If we take cos. 2 C = sin. P cos. A, we shall have tang. $p = \frac{\text{sin. P sin. A}}{2\,(\text{sin. C})^2}$, and the computation of this new formula is extremely easy.

5. The formula tang. $p = \frac{\text{sin. P sin. A}}{1 - \text{sin. P cos. A}}$, gives besides, sin. $p = \frac{\text{sin. P cos. A}}{\sqrt{1 + \text{sin. P}^2 - 2\,\text{sin. P cos. A}}}$; make sin. P = 2 cos. D, D being a given angle, of which we may have tables ready made, and we shall have sin. $p = \frac{\text{sin. P sin. A}}{\sqrt{1 + 2\,\text{sin. P (cos. D} - \text{cos. A)}}} = \frac{\text{sin. P sin. A}}{\sqrt{1 + 4\,\text{sin. P sin.}\,\frac{1}{2}\text{A} + \frac{1}{2}\text{D}}}$ sin. $(\frac{1}{2}\text{A} - \frac{1}{2}\text{D})$; since cos. D — cos. A = 2 sin. $(\frac{1}{2}\text{A} + \frac{1}{2}\text{D})$ sin. $(\frac{1}{2}\text{A} - \frac{1}{2}\text{D})$. This being found without any logarithmic computation, we shall find tang. E^2 = 4 sin.

P sin. $(\frac{1}{2}A + \frac{1}{2}D)$ sin. $(\frac{1}{2}A - \frac{1}{2}D)$, if $A > D$, and hence we may easily compute sin. $p =$ sin. P sin. A cos. E; but if $A < D$, we shall find cos. $F^2 = 4$ sin. P sin. $(\frac{1}{2}A + \frac{1}{2}D)$ sin. $(\frac{1}{2}A - \frac{1}{2}D)$; and hence sin. $p = \frac{\text{sin. P sin. A}}{\text{sin. F}}$.

6. Similar formulæ may be found for cos. p. But as the angle p is pretty small, we might easily fall into some error by the usual tables of logarithms. He does not say what would be the amount of this error of p, having furnished the manner of avoiding it; but this remark has not, he thinks, as yet been made in astronomical calculations; and he has found it of great consequence in computing eclipses, where the distances to be found are very small arches.

7. It may also be observed, that if $A = D$, sin. $p =$ sin. P sin. A; hence $p' = p$ in the same case, and $p'' > p$, which seems very odd; but the moon then is below the sensible horizon.

Theory of the apparent Diameters of the Moon.—1. First the expression of horizontal diameter of the moon, or of the diameter seen at the horizon, seems to Mr. M. too vague; for we ought to understand by it the diameter seen at the centre of the terrestrial sphere, rather than the apparent diameter at the horizon, which is not affected by refraction. Without this, if the one was confounded with the other, an error would arise for the latitude of Paris from $0''.25$ to $0''.32$.

2. Let us keep the same denominations of P, p, and A, and call D the apparent semidiameter of the moon at the centre of the sphere, and $d =$ the apparent semidiameter of the moon at the zenith distance $=$ A. We shall have sin. A : sin. $(A + p)$ :: tang. D : tang. d, or if we will, sin. A : sin. $(A + p)$ :: D : d: the error not exceeding an 100th part of a second.

3. We had above sin. $p =$ sin. P sin. $(A + p)$. Hence sin. P sin. A : sin. p :: (tang. D : tang. d) :: D : d, or because sin. $p = \frac{\text{cos. } p \text{ sin. P sin. A}}{1 - \text{sin. P cos. A}}$, it is $1 -$ sin. P cos. A : cos. p :: D : d, and $d = \frac{\text{D cos. } p}{1 - \text{sin. P cos. A}}$.

4. Mr. Euler, in the Memoirs of the Academy of Berlin, 1747, p. 175, makes this same value $= \frac{V}{1 - p^2 \text{ sin. } h}$, and according to him, $V =$ D. M $=$ sin. P sin. $h =$ cos. $(A + p)$; whence it appears that the true value of the apparent diameter of the moon, is not more difficult to be computed than the approximate one of Mr. Euler, the exact and geometrical formula being tang. $d = \frac{\text{tang. D cos. } p}{1 - \text{sin. P cos. A}}$, and that of Mr. Euler $d = \frac{D}{1 - \text{sin. P cos. } (A + p)}$; for in both, the values of D, A and p must be employed.

5. It also appears to Mr. M. that since $\frac{\text{cos } p}{1 - \text{sin. P cos. A}} = \frac{\text{sin. } p}{\text{sin. P sin. A}}$, and therefore tang. $d = \frac{\text{tang. D sin. } p}{\text{sin. P sin. A}}$, astronomers ought no less to employ this last formula, than any other more troublesome, in practical computation. The simplest is tang. $d = \frac{\text{tang. D sin. } (A + p)}{\text{sin. A}}$, on the supposition of an exact table of the parallaxes of altitudes ready made; and he be-

lieves it will be as easy to compute with tangents as with arches, by means of logarithms; and therefore this simplification in putting arches instead of tangents is unnecessary.

6. To try the consequences of this theory, he made A = 30°, D = 15′, and taking the vertical of Upsal to the terrestrial axis for the radius of the sphere, he found P = 55′ 10″.3, supposing that the axis of the earth, is to the diameter of the equator, as 199 to 200, and by the formulæ tang. $d = \frac{\text{tang. D sin. } (\text{A} + p)}{\text{sin. A}} = \frac{\text{tang. D cos. } p}{1 - \text{sin. P cos. A}} = \frac{\text{tang. D sin. } p}{\text{sin. P sin. A}}$, he found $d = 15' \ 12''.664$, but by the formula $d = \frac{\text{D cos. } p}{1 - \text{sin P cos. A}}$, he had $d = 15' \ 12''.675$; and lastly by that of Euler $d = \frac{\text{D}}{1 - \text{sin. P cos. } (\text{A} + p)}$ we have $d = 15' \ 12''.635$; whence it appears that the error is very small, but that with the same trouble one may avoid any error whatever.

7. The present case did not give an error of 0″.001 in substituting 1 or the radius instead of cos. p. Hence he concludes that $d = \frac{\text{D}}{1 - \text{sin. P cos. A}}$ will be a more exact formula than that of Euler $d = \frac{\text{D}}{1 - \text{sin. P cos. } (\text{A} + p)}$.

8. By taking $d = \frac{\text{D}}{1 - \text{sin. P cos. A}}$, we have $d - \text{D} = \frac{\text{D sin. P cos. A}}{1 - \text{sin. P cos. A}} = \frac{\text{D sin. } p \text{ cos. A}}{\text{sin. A cos. } p} = \frac{\text{D tang. } p}{\text{tang. A}}$, which affords an elegant theorem, to find the increase of the apparent diameter of the moon.

9. He found others by the following methods. Since sin. A : sin. (A + p) :: tang. D : tang. d, and sin. A : sin. (A + p) − sin. A :: tang. D : tang. d − tang. D :: sin. D cos. d : sin. (d − D); but cos. D = cos. d without any sensible error, and sin. D cos. D = ½ sin. 2 D, and sin. (A + p) − sin. A = 2 sin. ½p cos. (A + ½p), we shall have sin. $(d - \text{D}) = \frac{\text{sin. 2 D sin. } \frac{1}{2}p \text{ cos. } (\text{A} + \frac{1}{2}p)}{\text{sin. A}}$. In the same manner, as he before found sin. p' = sin. P sin. A, and sin. P sin. A : sin. p :: tang. D : tang. d, hence sin. p' : sin. p − sin. p' :: sin. p' : 2 sin. (½p − ½p') cos. (½p + ½p) :: ½ sin. 2 D : sin. $(d - \text{D}) = \frac{\text{sin. 2 D } (\frac{1}{2}p - \frac{1}{2}p') \text{ cos. } (\frac{1}{2}p + \frac{1}{2}p')}{\text{sin. P sin. A}}$.

10. Lastly let L = the distance of the moon from the centre of the sphere, l its radius, that of the sphere being = 1, we have 1 : L :: sin. P : 1, and L : l :: 1 : tang. D or 1 : l :: sin. P : tang. D = l sin. P; hence $l = \frac{\text{tang. D}}{\text{sin. P}}$ being once found, since sin. A : sin. (A + p) :: tang. D : tang. d, and sin. (A + p) : sin. p :: 1 : sin. P, we shall have sin. A : sin. p :: tang. D : sin. P tang. d :: l : tang. $d = \frac{l \text{ sin. } p}{\text{sin. A}}$. He found the logarithm of l = 9.4343965 at Upsal, by putting 10 for that of the radius of the sphere determined as before.

XXX. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1765; *pursuant to the Direction of Sir Hans Sloane, Bart.* p. 250.

This is the 44th presentation of this kind, completing the catalogue to the number of 2200 different plants.

XXXI. Observations on the Eclipse of the Sun of August 5, 1766, *made at Colombes, the Observatory of the Marquis of Courtenvaux,* 20½″ *East of the Royal Observatory at Paris, and in Lat.* 48° 55′ 28″. *By M. Messier, F. R. S., &c. From the French.* p. 259.

This eclipse began at $5^h\ 43^m\ 50^s$ true time, as observed by M. Messier; but at $3\frac{1}{2}^s$ later as observed by M. Jeaurat. The end was not seen.

XXXII. Observations of the Eclipses of the Sun of Aug. 16, 1765, *and of Aug.* 5, 1766, *made at Calais. By the Prince de Croy. From the French.* p. 262.

1. *Observation of the Eclipse of the Sun, of the* 16*th of August* 1765, *at Calais, near the Steeple.*

Beginning	3^h	50^m	46^s
End	5	8	17½
The duration, of which I am very certain	1	17	31½

The greatest magnitude was of 2° 50′.

There appeared hardly any sign of an atmosphere, except against the cusps of the sun, which seemed something larger at the two sides which touched the moon. There is no indication of one any where else, unless that the disk seems not so well defined at the ingress and egress. These two remarks would induce me to believe there is a small atmosphere.

He saw very distinctly with the reflector of 4 feet 3 inches (which answers a common telescope of 70 or 80 feet) and with the acromatic telescope, several mountains, and particularly 5, besides some inequalities. He always saw them in their fixed places, whatever motion was given to the telescope. These seemed to have their summits at about an equal distance from each other. The first 3 seemed to have hollows between them, which penetrate a little into the moon; all the rest projected out of the disk.

On mature consideration, he thinks the moon's atmosphere perceptible. 1. Because the two small elevations, which he constantly saw of the sun's limb, towards the cusps, was easily distinguished in the large telescope, through which the other parts appeared very exactly defined. It is true that the sun appears equally bright at the place of this small elevation, but the atmosphere cannot affect its brightness, and can only somewhat raise the rays, which pervade or slide over a denser medium. 2. Because this elevation is gradual, rising higher towards the moon, according to the degree of the density of its atmosphere. 3. Because this elevation of the sun against the cusps seemed to be a little higher than a mountain which jetted out the most on a side view. 4. Because the penumbra, which was observed at the ingress and egress, and which certainly is much less defined than towards the middle, is very probably occasioned by the mixture of the rays, which are refracted in passing through the lunar atmosphere. This appears to be more likely than that it should be caused by the inflexion of light, from the globe of the moon; because such inflected light sliding on the globe ought rather to lessen its dark limb; whereas, on the contrary, the penumbra, which was sufficiently thick, served to enlarge that limb. He thinks he has seen enough to conclude, that the moon's atmosphere extends to at least double the height of the highest mountains in it; and that it has some dry cavities, which sink below the disk. The most surprizing circumstance, was that of the mountains seen in profile, and perfectly welldefined. The 3 summits of N°. 1, 2, and 3, were very distinct, and especially the hollows between them, surprizing on account of their depth, for they made cavities within the circle of the disk. I am no longer astonished at the luminous points, which are seen starting from the crescent; but I wonder that no notice should hitherto have been taken of these mountains seen sideways in the eclipses of the sun: a 4-foot telescope well fixed is necessary to observe them distinctly. Large refracting telescopes cannot serve for that purpose, as it is difficult to prevent entirely their shaking.

He is inclined to believe that there is no water in the moon, for the two cavities which penetrated within the disk, continued concave to the bottom, whereas if there had been there any water, the bottom ought to have been convex. The mountains in the moon must be very high and hollow; and that is the reason of their white circle. If ever Tycho could be seen edge-ways, it would make a beautiful appearance.

2. *Observation of the Eclipse of the Sun the 5th Aug.* 1766, *near the Courgain at Calais.*—Beginning $5^h\ 39^m\ 9^s$, end $7^h\ 19^m\ 13^s$.

The sun did not set in the sea till about 14 minutes after the end of the eclipse; that is, at least 10 minutes later than the almanack makes it to do at Paris. His lower limb touched the sea at $7^h\ 23^m$ in thick vapours, which made Mr. M. prefer the setting of this lower limb rather than the centre. It is easy to conclude the difference which should result from the different situations of Paris and Calais. No inequalities nor mountains were discernible, nor could any be seen on that part of the moon. The elevation of the light of the sun's cusps was but small at the middle of this eclipse (about as much as in the last eclipse) enough, however, to indicate an atmosphere, as in the former observation; but the air was charged with strong vapours, which hindered him from seeing it so distinctly as he then did.

XXXIII. On the Extraction of 3 Inches and 10 Lines of the Bone of the Upper Arm, which was followed by a Regeneration of the Bony Matter; with a Description of a Machine made use of to keep the Upper and Lower Pieces of the Bone at their proper Distances, during the Time that the Regeneration was taking Place; and which may also be of Service in Fractures happening near the Head of that Bone. By Mr. Le Cat, F. R. S. p. 270.

Francis Romain, formerly a foot-soldier, aged 41, received a gun-shot wound in the left arm, about 2 fingers breadth below the head of the bone of the upper arm, which had been considerably shattered in this place by the ball. Mr. Bousselard, his surgeon, found the situation of the wound too high to risk amputation. After 7 months attendance however, the patient appeared to be cured; he was put on the list of invalids, and stationed with one of those companies at Dieppe.

Encouraged by good health, he ventured to undertake the laborious business of a ship-wright; but the great fatigue produced abscesses in the arm which had been lately healed; and he was admitted into the hospital in the year 1755. Mr. Le C. immediately made free incisions and counter-openings in the parts which contained the matter, and extracted some splinters of bone; he then applied a proper bandage, and after the separation of several bony fragments, the cure was completed, at least a good cicatrice was formed. The man returned to his work, which was then going forward at Rouën, and was employed in carrying wood for the construction of flat-bottomed boats. His limbs in general were strong enough to support these loads; but his left arm, in which he had received the wound, was of little use to him, being shorter and weaker than the other.

March 15th, 1760, being seized with a pleurisy and peripneumony, he was brought back into the hospital. After his recovery from this disease, an abscess was formed in the injured arm, which made an opening for itself in the fore-part, larger than a bullet. The arm was deprived of all motion, strength, and connection; and the callus of the former fracture appeared to be entirely destroyed by this fresh accident. In this state of the case, the patient being brought from the infirmary into the ward designed for wounded persons, Mr. Le C. passed his probe into the wound, and found the bone of the arm bare, and carious to a very great extent: the middle of this carious part was rotten and totally destroyed throughout its whole substance. Anodyne cataplasms were applied to abate the inflammation and swelling which attended the ulcer.

April 15th, Mr. Le C. began to put in execution the plan he had fixed on for his cure; the first intention of which was, to lay bare the carious part of the bone in its whole length, which was rather more than 3 inches. The wound was then filled with pads of lint, and the 2d operation deferred to the next day. It was the opinion of the by-standers, that the arm should be taken off at the shoulder-joint; but the great danger attending this kind of amputation deterred him from performing it, and induced him more particularly to consider, whether it might not be possible to save the limb. The instance of Charles Lehee had sufficiently convinced him, that bones have the power to regenerate: it must indeed be allowed as a favourable circumstance to the vegetation of the bone, that Lehee was a child; but, though this patient was an adult, he considered that we knew not at what age nature had put a stop to this regenerative faculty, and that therefore no argument could be deduced from experience to prevent the expectation of the like success in the present case.

These considerations determined him, and on the 16th of April he performed the operation, by separating the upper and lower parts of this carious bone from their connections with the sound parts, by methods, which every operating surgeon will readily conceive. He measured the distance between the end of the bone left at the upper part, and the blade of the saw at the lower, and found it to be just 3 inches and 10 lines. The cavity was then filled with proper dressings; and the form of the arm, as well as its natural length, preserved by an instrument calculated to answer these intentions; the description of which he thought more particularly deserving the attention of the R. S., as it might be of service in many cases different from this.

In pl. 8, fig. 6, ABC is a double screw, the turns of which are in contrary directions; these screws are moved by the handle A, in such a manner that when each of them B, C, is screwed into its correspondent worm D, E, one motion of the handle A brings the 2 worms D, E nearer together; and a contrary motion sets them at a greater distance. Mr. Le C. invented this instrument about 15 years before, to compress the wounds of those who had been cut for the stone, so as to prevent the passage of the urine, and thereby hasten the closing of the incision. To apply it properly in these cases, he passed a collar, fastened to the neck of the patient, through the ring F, in the upper screw worm, and the bandage which supported the dressings, through the ring G, in the lower screw worm; and he had consequently experienced the success of this manœuvre.

In the year 1757, he made use of it to preserve an arm, fractured near the shoulder-joint, in its natural state of extension, by fastening the 2 flat rings F, G, in the 2 pieces of wood H, I; placing H under the arm-pit, and I on the fore-arm against the bend of the elbow; and keeping the fore arm bent by the sling KL. He also applied this instrument in the present case; and in order to assist in giving the proper direction, and necessary solidity to the part, he supported the arm with a vambrace, or half canal, made of one very thin piece of wood, which surrounded ⅔ of the circumference of the limb. The whole was fastened by the bandages commonly used in fractures.

On the 29th day after the operation, the wound having filled up very fast, the arm appeared to have a sort of firmness, that the muscles alone were incapable of giving it; and on the 30th of the same month it had acquired a degree of solidity, which was nearly equal to the hardness of bone; for it was strong enough to support itself, and yielded very little to the pressure that was made upon it; but the patient was still unable to make any use of it, till the 55th day after the operation, at which time he began to move it a little. It was scarcely possible that so remarkable a case should not be attended with some accidents, in the course of the cure; so that, on the 16th of June, he was not at all surprised at the appearance of a pustule on the upper part of the scar, from which there was an oozing of matter. On the next day he passed a probe into this opening, which entered with some difficulty the length of an inch; but he did not find any splinters of bone, which he expected to meet with. June 18, he made a large opening in this part, and extracted a point of bone, which seemed to have shot out in a very particular manner towards this pustule, and might probably have contributed, by its irritation, to have produced this fistula. June 19, he took away the remains of this bony substance, situated underneath the above fistula, and which was but slightly attached to the neighbouring soft parts; after which the cure of this wound was completed in 15 days.

On the 25th of July, the 100th day after the operation, he perceived another pustule breaking out near the lower part of the scar. He then began to suspect, as there had been a flow of matter from the arm for several years, that the constitution had acquired a habit of discharging itself at this part; and attributed to that cause these trifling relapses, as well as the former more considerable one, which the patient had suffered. From whatever spring these habitual discharges may be derived, and whatever may be the reasons assigned for the relapses they produce, it is well known that they are sometimes diverted or cured by issues; he therefore ordered a large caustic to be applied to the patient's left leg. The arm was at this time very solid to the touch, but still yielded a little to its own weight, as the branch of a tree, or a piece of green wood; he therefore had a bracelet of whalebone made to support and keep it steady. August 1st, the pustule, which he had perceived on the 25th of July, appeared to be much increased in size, and was spread out into fungous flesh: he passed a probe into it, and found some pieces of bone bare towards its upper part. August 10, a small scale was exfoliated, and from that time the fungous flesh began to disappear.

In Sept., 5 months after the operation, the issue afforded a very plentiful discharge, and had attracted the humour so powerfully towards this part, that the leg on which the caustic had been applied, appeared swelled and grew painful; but at the same time, the arm was entirely cured; having recovered all its actions and uses, together with its proper form and length. And on the 12th of October, Romain was discharged from the hospital, very thankful for the recovery of his health, and the perfect cure he had received. He then went to rejoin his company at Dieppe, and resume his former employments. This observation, at the same time that it furnishes a remarkable instance of animal vegetation, strongly encourages surgeons to attempt the preservation of limbs, in all cases where there is a possibility of bringing about this sort of regeneration, so useful to mankind, and so honourable to the art.

XXXIV. A New Method of Determining the Longitude of Places, from Observations of the Eclipses of Jupiter's Satellites. By Mr. Wargentin, F. R. S., and Sec. of the Royal Acad. of Sciences at Stockholm. From the Latin. p. 278.

M. Wargentin remarks, that the method heretofore practised, to find the longitude of places by means of the eclipses of Jupiter's satellites, especially of the two nearest, which are the fittest for this purpose, is to observe the same eclipse at two different places, noting the exact times, with good telescopes and well regulated clocks; then taking the difference of these times, that will be the difference in time between the meridians of the two places; or turning that time into motion, at the rate of 15° to an hour, it gives the difference of longitude, in degrees, minutes and seconds, between the two places: then, if the longitude of one place be previously known, that of the other hence becomes known also, viz. by adding or subtracting the observed difference of longitude with that of the given place, according as the case may require.

But plain and easy as this method appears, Mr. W. observes that it can but seldom be practised, to find the longitudes of unknown places, on account of the want of good correspondent observations, viz. such as are accurately made, of the very same eclipses, in two places, of one of which the longitude is well known. To remedy this defect then, Mr. W. further observes, that astronomers use to follow two ways; viz. to supply correspondent observations at some fixed observatory, or place of known longitude. Thus, some inquire whether there is noted down, at the known observatory, any near eclipse of the satellite, either next before or after the eclipse in the unknown longitude, to which the correspondent observation is wanted: which being found, then by adding or subtracting the interval of time for the known number of intervening revolutions, it gives the time at the observatory of the correspondent eclipse required. But this method is attended with great hazard: for the revolutions of a satellite are unequal, so as that hardly any two successive ones perfectly agree together. It is true that the theory teaches how to compute many inequalities, but yet with much trouble and danger of mistakes. Sometimes also correspondent observations are defective, by their interval of time being too remote from the observed eclipses, or if there be one, out of a great many that is nearer, it may be incorrect or insufficient.

But the other is a much better way of supplying the want of correspondent observations; viz. by the help of a calculation made for the given meridian, and corrected by adding or subtracting for the error of the tables, which error will be deduced from the other observations, made in the same month or near it: for in this way all the observations, made in the place to be determined, will contribute partly to confirm the required longitude. And the certainty of this method which Mr. W. means chiefly to explain, depends principally on this, the error, to which the tables are liable, may be rightly corrected for any moment. This can easily be done if the error be constant for some years, or even for months. But experience shows that observations made about the same time, though deemed almost equally good by their observers, yet differ unequally from calculation; nay even the same immersion or emersion, as noted by different astronomers, often gives an error of calculation differing by many seconds or a whole minute, not wholly to be ascribed to the fault of the tables, but partly to the observations themselves. In this case it is very often hard to find out the true error of the tables; for even expert observers often inadvertently slip into errors, and note down as true, observations that are erroneous. If therefore, as is often the case, there be only one observation selected for finding out the error of the tables, it is easy to fall into error. From all these reasons it appears how necessary it is to examine the observations themselves, before they are employed for determining the longitude, by any method whatever. And that may be done as follows:

Dispose all the useful observations of the same satellite, especially of the first, made about the same time, as well in the place to be determined as in the observatory before sufficiently known, in a table according to the order of their times. Assume for the present the required difference of meridians, as far either as the observations, if there be any, offer immediate correspondents, or as an incorrect calculation of one observation may require. Then examine exactly all the observations by the tables, and mark the errors of calculation so found. Which done, and having attentively examined the series of errors, it will sufficiently appear, 1st, which, among the selected observations made at the fixed observatory, are the best and most deserving of confidence: 2d, what may be the error of the tables at the middle time: and lastly, whether the difference of meridians sought be greater or less than that which was assumed in the calculation; and how much it must be either augmented or diminished, to produce nearly equal errors of calculation. Then it will be very probable that such immersions as appear to be the most too early, and the emersions too late, are to be esteemed the least certain, though they may have been deemed good ones by the observers themselves: for the theory of Jupiter's satellites, especially of the two nearest, is now so perfected, that it can hardly be doubted that all the immersions of the same year, or at least of the same month, will recede almost equally from calculation, if the observations are made with equal certainty; and the emersions the same. Therefore the variations, and as it were leaps of errors, are to be attributed to the unequal goodness of the observations. If the observations made in the place to be determined differ almost equally from the tables, it will be an indication that the assumed difference of meridians is the true one: but if unequally, then how much it is to be augmented or diminished. If the immersions seem to require the difference of meridians to be greater or less than the emersions, it is to be ascribed to the unequal power of the tubes, and the mean difference of the meridians may be accounted so much the more certain, as it has more observations to confirm it.

For example, to determine the longitude of a place in the island of Barbadoes, by means of 17 observations of the first satellite made there, in the months of November and December 1763, and in January and February 1764. Now there were 21 other observations of the same satellite made in Europe in the same months, in places well determined, of which 2 only are correspondentes to the former, the medium of which gives $5^h\ 10^m\ 14^s$ for the difference of meridians of Stockholm observatory and Barbadoes. To know whether that will agree with the rest of the observations also; to all the observations in these months add the last in October 1763, and the first in March 1764, arranging them in due order, and compute the true time, according to the tables, when each one should happen in its own meridian.

This calculation being made, it appears that the European immersions in some places were later, and in others sooner, than by the tables they ought to be. The former seem to be the best, but the latter, especially those which differ the most, are rejected as doubtful. By a medium, all the immersions seem to agree nearly with the calculation. But among 15 emersions observed in Europe, mostly calculated, some at the most are 31^s too early, excepting three at Tyrna, which are too late, and rejected. The medium of the good emersions were about 20^s too early.

Considering the calculation of the other observations at Barbadoes, it appears that, supposing the difference of meridians $5^h\ 10^m\ 14^s$, almost all of them are marked much more before the computation than those of Europe; but that they differ almost equally from the tables, if the difference of the meridians be increased 22^s. Mr. W. therefore takes it to be very nearly $5^h\ 10^m\ 36^s$; as will more plainly appear by the following correction of all the observations, with the corrected Barbadoes calculation.

The Paris observations were made by M. Messier, with a Gregorian telescope magnifying 104 times. Those at Vienna by Fa. Hell, with a 4¼-foot telescope, the longitude from Paris being $56^m\ 11^s$. Those of Tyrna by Fa. Weifs, with a

4-foot Newtonian telescope, the longitude from Paris being $1^h\ 1^m\ 56^s$. And Mr. Wargentin's at Stockholm, with a Dollond's 10-foot tube, the difference of longitude from Paris being $1^h\ 2^m\ 51^s$, and from Greenwich $1^h\ 12^m\ 7^s$.

Finally, Mr. W. is persuaded that this method of determining longitudes by observations of Jupiter's satellites, is to be preferred to the others; that it is more clear and general; and especially as it indicates at once which of the observations are too great or too little.

Observationes comparatæ primi Sat. Jovis.

	Tempus observat.	Tem. comp.	Err. comp.	Locus obs.		Tempus observat.	Tem. comp.	Err. comp.	Locus obs.
Oct.	$23^d\ 13^h\ 32^m\ 21^s$ im.	$13^h\ 32^m\ 9^s$	$0^m\ 12^s$ —	Paris.	Jan.	$2^d\ 11^h\ 25^m\ 59^s$	$11^h\ 26^m\ 17^s$	$0^m\ 18^s$ +	Stockh.
	23 14 27 50	14 28 18	0 28 +	Vienna.		2 11 19 10	11 19 37	0 27 +	Vienna.
	25 8 56 26	8 56 56	0 30 +	Vienna.		4 5 47 15	5 47 29	0 14 +	Vienna.
	25 9 3 48	9 3 36	0 12 —	Stockh.		7 13 39 43	13 39 33	0 13 —	Barbad.
Nov.	1 9 55 6	9 54 59	0 7 —	Paris.		9 8 7 20	8 7 29	0 19 +	Barbad.
	6 18 21 14	18 21 10	0 4 —	Tyrna.		16 9 59 38	9 59 52	0 14 +	Barbad.
	8 12 44 24	12 44 49	0 25 +	Vienna.		23 11 32 36	11 52 52	0 16 +	Barbad.
	10 7 17 36	7 17 48	0 12 +	Tyrna.		25 6 20 51	6 21 15	0 24 +	Barbad.
	13 15 5 38 im.	15 5 37	0 1 —	Barbad.		25 11 31 20	11 31 51	0 31 +	Stockh.
	15 14 37 34	14 37 44	0 10 +	Vienna.	Feb.	8 10 9 19	10 9 50	0 31 +	Barbad
	17 8 9 39	8 9 47	0 8 +	Paris.		10 8 46 6	8 46 24	0 18 +	Paris.
	20 16 57 20	16 58 10	0 50 +	Barbad.		10 9 47 41	9 47 18	0 23 —	Tyrna.
	22 11 26 6	11 26 12	0 6 +	Barbad.		17 10 41 48	10 41 51	0 3 +	Paris.
	22 16 34 10	16 34 53	0 43 +	Tyrna.		17 11 43 6	11 42 45	0 21 —	Tyrna.
	24 10 2 13	10 2 0	0 13 —	Paris.		17 11 44 22	11 44 40	0 18 +	Stockh.
Dec.	1 7 46 0 im.	7 45 58	0 2 —	Barbad.		19 6 13 22	6 13 39	0 17 +	Stockh.
	6 17 21 4 em.	17 18 44	2 20 —	Barbad.		19 6 6 34	6 6 59	0 25 +	Vienna.
	8 11 46 35	11 46 33	0 2 —	Barbad.		19 6 11 45	6 11 44	0 1 —	Tyrna.
	10 11 24 45	11 24 56	0 11 +	Stockh.		24 8 30 42 em.	8 30 13	0 29 —	Barbad
	15 13 37 9	13 37 41	0 32 +	Barbad.	March	4 10 4 51	10 4 53	0 2 +	Tyrna.
	17 8 5 46	8 5 27	0 19 —	Barbad.		4 10 6 41	10 6 48	0 7 +	Stockh.
	22 15 28 35	15 28 49	0 14 +	Barbad.		13 6 26 48	6 26 34	0 14 —	Vienna.
	24 9 56 20	9 56 33	0 13 +	Barbad.		13 6 31 45	6 32 19	0 34 —	Tyrna.

XXXV. On the Coluber Cerastes, or Horned Viper of Egypt. By John Ellis, Esq., F. R. S. p. 287.

The coluber cerastes,* or horned viper of Egypt, which Mr. E. presented a specimen of to the R. S. is very rare, and scarcely to be found in any of the cabinets of Europe. Besides, the authors who have treated on the cerastes, as Alpinus and Bellonius, have given such unsatisfactory descriptions of it, and inaccurate figures, that an exact drawing from nature, together with the best and latest systematical account of it, may be agreeable, as well to the lovers of antiquity as natural history.

The ancient Egyptians most certainly esteemed it a hieroglyphic of some im-

* The coluber cerastes is a native of the sandy desarts of Arabia and many parts of Africa; it is furnished with poisonous fangs, like the common viper. It is admirably figured in Mr. Bruce's Travels.

portance; for when we examine their monuments of the greatest antiquity, such as their obelisks, temples, statues, palaces, and even their mummies, we are almost sure to find many representations of it on them. Those two immensely large stones, lately brought from Alexandria, in Egypt, now in the court-yard of the British Museum, which appear to be part of the grand cornice of some magnificent palace, have many figures of the cerastes curiously engraved on them. Dr. Hasselquist, a pupil of the celebrated Linneus, who was in Egypt in 1750, has given a particular description of this curious animal; but neither he nor the former writers on Egypt, that mention the cerastes, say any thing about the venom of its bite. This we are informed of only by Dr. Turnbull, who lived many years in Egypt, both at Alexandria and Cairo, and who presented Mr. E. with two specimens of it. Dr. Linneus, in his System of Nature, p. 217, calls it coluber cerastes. Dr. Hasselquist, in his Iter. p. 315, coluber cornutus.

The length of this specimen here mentioned is as follows; from the nose to the anus $22\frac{1}{2}$ inches, the tail $3\frac{1}{2}$ inches; so that the whole serpent is 26 inches long. The belly is covered with 145 broad scales, or scuta; the tail with 43 pair of small scales, or squamæ. The number of squamæ and scuta have been thought by late authors to be the best method of determining the species of ser pents; but they are not ignorant that they differ a few now and then: Hasselquist reckoning 150 scuta, and 50 pair of squamæ, to his Coluber cornutus.

XXXVI. Abstract of a Journal of the Weather in Quebec, between April 1*st* 1765, *and April* 30*th* 1766. *By Capt. Alex. Rose, of the* 52*d Reg.* p. 291.

This register contains the greatest and least height of the thermometer, for many days in the year above-mentioned, commonly 8 or 10 days in each month, more or less; also the direction of the wind, with the kind of weather on the same days, as to fair or foul, rain, snow, &c. It appears that the range of the quicksilver in the thermometer is very great, and the changes sudden and various, both in the different parts of the day, and between the different seasons of the year. Thus, the greatest height of the thermometer on April 15, was 60°, and the least as low as 31, a difference of 29° in the same day. On May 27, the greatest height was 80°, and the least 58, a difference of 22°. On June 9, the thermometer rose as high as 87°, the greatest height that year, and sunk as low as 64°, a difference of 23°. On Dec. 25, the greatest height was 29°, and the least 0, or 32 below the freezing point. On Dec. 31, the greatest height was 15° below zero, and the least 29° below zero, or 61° below the freezing point, being the lowest that it fell on any day that year. On Jan. 5, it rose as high as 25° above, and fell to 25° below zero, a difference of 50°. On Jan. 21, it rose as high as 37°, and 2 days after sunk as low as 23° below zero, a difference of 60° in the space of 2 days only.

XXXVII. On two Parthian Coins, never before published. By the Rev. John Swinton, B. D., F. R. S. p. 296.

Mr. S. met with two ancient brass coins, pretty well preserved, that had formerly had a place in the valuable collection brought by Dr. Pococke out of the east. The workmanship of these pieces seems considerably different from that both of the Parithian coins, hitherto published, and those struck by the Persian princes of the house of Sassan. It somewhat however resembles that of the brass medals of one or two of the later Parthian kings. These pieces have several unknown characters on them, which can by no means be deemed the same with those preserved on the Persian coins struck by the princes of the house of Sassan.

These two medals are of the size of the smaller middle Roman brass, or nearly so. Their workmanship, as just remarked, is inelegant, or rather somewhat rude. They are so similar to each other, that they may be considered, without any great impropriety, as duplicates of the same medal. Both of them, on the anterior part, seem to have retained the effigies of the same Parthian king; and on the reverse they both exhibit a human head, with the hair formed into curls, on which is just visible a rude sort of crown. Before the face of the latter, the Greek elements Π,Ρ,Ο,Ζ,Ο,Υ, or ΠΡΟΖΟΥ, on both plainly enough appear; though one of them only presents to our view, before the face of the Parthian king, a complex character, or monogram, seemingly composed of the Greek letters Ε,Λ, and three or 4 unknown characters, that have suffered a little from the injuries of time.

With regard to the word, or rather name, ΠΡΟΖΟΥ, Mr. S. hesitates not a moment to read and pronounce it ΠΕΡΟΖΟΥ; instances of the omission of a Greek letter, having been often met with on the Parthian coins. Nor can this be matter of surprize to any one who considers, that Greek words are sometimes very inaccurately expressed on those coins. The unusual curls, on the reverse, may possibly be thought to point at Armenia, as the country where these pieces were struck; especially, as the complex character, if it is a monogram formed of the Greek elements Ε,Λ, or ΕΛ, seems to direct us to the city of Elegia in Armenia, where a whole Roman army was cut off by Vologeses II. And this will appear still more probable, after we have discovered the monarch denominated Perozes, or Peroz, and the reason of that name.

Vologeses II. having finished his preparations for a war with the Romans, in the reign of Antoninus Pius, soon after that prince's death, made an irruption into the Greater Armenia. This happened, according to Dio, in the year of Rome 915, or of Christ 161. Meeting with little or no opposition, he advanced to Elegia, a city of that kingdom, where a Roman army, under the command of Severianus, the prefect of Cappadocia, was at that time posted. This formidable body he immediately attacked, and so entirely defeated it, that scarcely a single Roman found means to escape. So complete a victory as this must of course have put Vologeses in possession of the greater part of Armenia, and particularly of the city of Elegia. After so important a conquest, the Parthian monarch may naturally enough be supposed to have caused these medals to have been struck, and that in the town of Elegia. And that this was really the case, some will perhaps allow deducible from the monogram presented to our view on one of these coins. Nay, that he derived the name, or surname, Peroz, or Perozes, itself from a successful expedition he undertook against the Romans, we learn from Moses Chorenensis, the Armenian historian. "At which time, says this writer, after the death of Titus the II. king of the Romans, called Antoninus Augustus, Peroz, or Perozes, king of the Persians, (Parthians) made an irruption into the Roman empire; whence he deduced the name Perozes, which signifies the Conqueror, or the Victor, having before been denominated Vologesus, according to the Greeks, but by what name he went among the Persians I have not yet been able to learn." Which passage seems not only to point at the defeat of Lucius Attidus in Syria, but likewise at the terrible overthrow given the Romans in Armenia, soon after Marcus Aurelius and Lucius Verus ascended the imperial throne, Mr. S. adds, that the Arabic Firûz and the Persic or Armenian Peros agree entirely in signification, if they may not be considered as absolutely the same word: that a Persian king, named Firûz by the Arabs, is called Perozes (Περόζης) by Agathias; and that Moses Chorenensis and one, at least, of the medals here described, mutually strengthen and support each other.

XXXVIII. A Successful Operation for the Hydrops Pectoris. By William Moreland, Surgeon, at Greenwich. p. 302.

As few instances are to be met with in chirurgical writers, of the successful opening of the thorax in the dropsy of the breast; the following person's case, who was preserved by it in the most imminent danger of death, may encourage others under similar circumstances to perform the operation, which has hitherto been very rarely attempted.

Anne Harmsworth of Croom's Hill, Greenwich, of a thin, hectic habit of body, and subject to defluxions on the breast, about the latter end of the year 1760, complained of a smart, shooting pain in her right side, which somewhat affected her breast. Her evacuations by stool and urine were by no means deficient, nor was there any remarkable appearance on the part affected. A blister was applied, and oily medicines given, which relieved her in a few days, yet not so entirely but that she had returns of the pain at different times, though not sufficient to make her apply for advice, till Nov. 1762, when she was seized with a much greater degree of the same kind of pain, attended with difficulty of respiration, a sense of weight on the diaphragm, and a quick pulse, with a little more heat than usual.

On the 18th of Dec. Mr. W. saw her, for the first time, with Mr. Mills, a surgeon at Greenwich, when she related to him the above complaints, now much augmented, having a sense of fulness in that side (which was ready to burst, as she termed it) and an evident fluctuation in the right cavity of the thorax. But her left side was free from complaint. She made very little urine, and that limpid. The expectorant medicines (blister and cathartic) were administered without the least relief; her symptoms gradually increasing. On the 1st of Jan. 1763, she could breathe in no other situation than that of the thorax brought forward to the knees, in which posture she continued till the 30th of Jan. when finding the ribs elevated exceedingly, and the right side of the thorax uniformly distended, with every other reason tending to confirm the notion of a fluid's being lodged there: they, in company with Mr. William Sharp (whose opinion they had this day requested) proposed the operation to her, which the present pressure of her disease, and the little probability of her living long in that state, determined her to consent to.

Mr. M. then, in presence of Mr. William Sharp, surgeon to St. Bartholomew's, and Mr. Mills, made an incision, about 4 inches long, between the 6th and 7th ribs, (reckoning upwards) and about half way between the spine and sternum, into the cavity of the thorax, and thence discharged 7 pints of limpid serum. Immediately the difficulty of breathing was removed, but a faintness succeeding seemed to endanger her for a short time, occasioned more by the sudden removal of the pressure from the lungs, than any other inconvenience from the operation, the loss of blood being very inconsiderable. From this time to the next morning, the urine was secreted and discharged to the quantity of 3 pints more than she had drank. On the first dressing, the next day, there issued about a spoonful of serum, but none afterwards; and though she remained weak and faint for several days, yet she had no other inconvenience, from the time of the operation to that of the cicatrization of the wound, which was completed in less than a month; the wound having been dressed superficially the whole time. It may be remarked, that though at the time of the operation she was 2 months gone with child, she completed her pregnancy, and is now in as good a state as she had enjoyed for many years before.

XXXIX. A Supplement to the Account of the Discovery of Native Tin, Art. VII. By the Rev. William Borlase, LL. D., F.R.S. p. 305.

" Mr. Henry Rosewarne, of Truro, says, that when he sent the first specimen (presented to the R. S. by me, and now lodged in their Museum) he mentioned as a proof of its being native tin, that between the ore and the tin there was a mixture of quartz: but, on a nearer examination and some trials with aquafortis, he and another person found it was not quartz. At last, on melting a piece, he perceived no small quantity of arsenic to be mixed with it, and therefore suspected that the white parts which had passed for quartz were nothing but arsenic. Accordingly he scraped off a little of it and put it on a red hot iron, where it immediately caught fire, and evaporated into smoke, leaving behind it the most poisonous stench they ever smelt. This confirmed some, who had hitherto doubted, in the most firm belief that it really was native tin and genuine, it being impossible for tin to be melted and the arsenic left untouched."

XL. A Supplement to the Account of an Amphibious Bipes; Art. XXII. By John Ellis, Esq. being the Anatomical Description of the said Animal, by Mr. John Hunter, F. R. S. p. 307.

The tongue is broad and has very little motion. It has a bone similar to that in birds, turtles, &c. On the posterior and lateral parts of the mouth, are 3 openings on each side; these are similar to the slits of the gills in fish, but the partitions do not resemble gills on their outer edges, for they have not the comb-like structure. Above,* and close to the extremity of each of these openings externally, so many processes arise, the anterior the smallest, the posterior the largest; their anterior and inferior edges and extremity are serrated, or formed into fimbriæ: these processes fold down and cover the slits externally, and would seem to answer the purposes of the comb-like part of the gill in fish.

At the root of the tongue, nearly as far back as these openings reach, the trachea begins, much in the same manner as in birds. It passes backwards above the heart, and there divides into two branches, one going to each lobe of the lungs. The lungs are two long bags, one on each side, which being just behind the heart, and pass back through the whole length of the abdomen, nearly as far as the anus. They are largest in the middle, and honey-combed on the internal surface through their whole length. The heart consists of one auricle and one ventricle. What answers to the inferior vena cava, passes forwards above, but in a sulcus of the liver, and opens into a bag similar to the pericardium; this bag surrounds the heart and aorta, as the pericardium does in other animals; from this there is an opening into a vein which lies above, and on the left of the auricle, which vein seems to receive the blood from the lungs, gills, and head, is analogous to the superior vena cava, and opens into the auricle, which is on the

* To avoid the confusion in our ideas, which might arise from the use of the words anterior, posterior, upper, lower, &c, in the whole of this description, the animal is considered in its natural horizontal position, so that the head is forwards, the back upwards, &c.—Orig.

left of the ventricle. The aorta goes out, passing for a little way in a loose spiral turn, then becomes straight, where it seems to be muscular; at this part the branches go off, between which there is a rising within the area of the aorta like a bird's tongue, with its tip turned towards the heart*.

The liver is principally one lobe, pretty close to the heart at the fore part, and passes back on the right of the stomach and intestines; at its anterior extremity on the left side, there is a very short lobe, ending abruptly. The gall-bladder lies in a fissure on the left side of the liver, near its middle; there is no hepatic duct; the hepato-cystic ducts, which seem to be 3 in number, enter the gall-bladder at its anterior end or fundus, and the cystic duct passes out from the posterior end of the gall-bladder, and terminates in the gut, about half an inch from the pylorus. The oesophagus, which is pretty large, passes back, and is continued into the stomach in the same line. The stomach, at the posterior end, bends a little to the right, where it terminates in the pylorus. The intestines pass back, making many turns; at the posterior end they become pretty straight, forming what may be called the colon, or rectum, where they are a little larger, and run to the anus in a straight direction. At the beginning of this larger part of the intestinal tube, there is no valvular structure. The spleen is a very small but long body; its anterior end is attached to the upper surface of the stomach, and it is continued back along the left side of the mesentery, to which it adheres. The pancreas is a small body lying above the duodenum, and is attached also to the left side of the mesentery. The kidneys are situated in the upper and posterior part of the abdomen, having the rectum passing below and between them, as in the snake, &c. Below the rectum lies a long bag, like a bladder; it adheres all along to the inside of the abdominal muscles, and its mouth opens into the rectum; but whether it is the bladder of urine, or not, he cannot tell. On each side of the rectum, close to the lungs, there is a body, the posterior end of which rests on the anterior end of the kidney; whether they are testicles or ovaria, he cannot pretend to determine; but imagines that they are either the one or the other.

* This account of the vena cava opening into the cavity of the pericardium may appear incredible; and it might be supposed, that in the natural state of the parts, there is a canal of communication going from one cava to the other, which being broken or nipt through in the act of catching or killing the animal, would give the appearance above described. I can only say, that the appearances were what have been described in three different subjects which I have dissected; and in all of them the pericardium was full of coagulated blood. But besides the smallness of the subjects, it may be observed that they had been long preserved in spirits, which made them more unfit for anatomical inquiries. They had been in my possession above 7 years.—Orig.

END OF THE FIFTY-SIXTH VOLUME OF THE ORIGINAL.

I. A Monstrous Human Fœtus; having neither Head, Heart, Lungs, Stomach, Spleen, Pancreas, Liver, nor Kidneys. By C. N. Le Cat, M. D., F. R. S., &c. From the French. Vol. LVII. p. 1.

All the lower part of this monstrous fœtus, from a finger's breadth above the navel, was a female, tolerably well formed, except that on her left foot she had but 4 toes, joined together by a membrane, like the web of a duck's foot. But all the parts of this fœtus, above the navel, composed a perfect mola, a shapeless mass.

Mr. le C. began the dissection of this monster on the hind part; the muscles of the back were well formed. He found on the right side 8 ribs, reckoning from below upwards, and 7 on the left. Immediately above these was an hydatid, in which lay the cervical nerves, destined for the upper extremities. In this place also might be seen very imperfect rudiments of the scapula and clavicle. On the right side, and near to those rudiments, was a sort of thumb, easily known to be the thumb of the right hand, whose extremity projected beyond the integuments $\frac{3}{4}$ of its natural length. Above, and on the right side of this hydatid, Mr. le C. discovered another more considerable, surrounded by a large bag, very smooth on the inside, and supported by something, which had the appearance of a beginning of a maxilla. At the extremity of the fore part of this bag were 2 orifices, almost contiguous, across a septum, which led to another bag of a much smaller size. These two hydatid bags were behind a part which Mr. le C. took for an imperfect eye, because it was transparent, and surrounded by teguments not unlike eyelids.

On tracing the spine, and divesting it of all the soft parts, he discovered that it terminated above in a bony mass, that resembled the larynx, above which was a large soft substance of the consistence of, and covered with that kind of skin common to a cow's udder; on which he saw hair like that of other children. This occupied the usual place for the head. Under this kind of parenchymatous substance, which was white and glandular, was a muscular mass, more considerable and conspicuous than one could well have expected in such a subject. It doubtless consisted of the occipital and perhaps frontal muscles, drawn towards each other. Having raised this fleshy part, he opened the upper bag of the spine, resembling a larynx. Its surface was altogether bony, as usual in the fœtus, viz. somewhat cartilaginous. This he opened in the direction of a membranous triangular line, somewhat like the lambdoidal suture of the occiput. He found this kind of larynx filled with cerebrum, or rather cerebellum; it might be about a cubical inch in proportion; and this was all the brain of the fœtus. At the extremity of this cavity, backwards, lay the proper medulla spinalis. This cavity was not separated by an elongation of the cerebellum, it had but a very small falx forwards; and on the right side was another appearance of an elongation of the cerebellum; so that this small portion of brain did most likely belong to the cerebellum.

At the basis of this kind of unformed cranium, forwards, was an opening leading to a small brown hydatid, situated on the right side, under a bone which had the appearance of a portion of the maxilla, which led towards a sort of mouth, scarcely formed and closed. There was nothing on the other side, no appearance of a mouth, nor any thing that seemed the least like it. He took this hydatid for an unformed jugular bag, or true cæcum; in the adjoining bone he found a kind of right ear. The fore part of this same superior surface of the cranium was flat, but a little hollowed, like the upper surface of the larynx; in the middle was a considerable ridge, and on its anterior part appeared a prominence: thus this bone, which should have been similar to the 2 parietals, did not resemble them at all. Underneath this prominence, the bone took a perpendicular turn, making a sharp angle with the upper surface, and forming a cavity in its descent, which terminated in a projection forwards; it was on the right side of this projection where the supposed right branch of the maxilla was attached; within that branch appeared the trace of the jugular above-mentioned, and very distinctly the nerve of the eighth pair.

In the breast, or rather under the ribs, were neither heart nor lungs, but the same white, parenchymatous and œdematous-like substance, seen in the place of the head. Below this was no diaphragm, at least no distinct one. In the belly, which extended itself just under the ribs, was a bundle of intestines, and a little red mass, which he called the liver, for want of a better name, because it seemed, that when he pulled the umbilical cord, this substance moved, which induced him to believe that the umbilical vein entered there. No stomach, spleen, pancreas, or kidneys were seen. The intestinal mass was divided into 2 portions. The first was of a reddish colour, which terminated upwards in a blind pouch, and below joined the other portion, as the ileum does where it unites with the colon and cæcum. This 2d portion was white, and seemed to include the large intestines. The cæcum was very long, or rather the cæcum and its appendix vermi-formis were of the same size. Thus, there was neither jejunum, nor duodenum, nor stomach, nor any liver properly speaking: for that which he found in the place of it, was a red viscus, and of the conglomerate kind, like the kidney in a fœtus. Having cleared it from all its adhesions, he discovered neither vessels analogous to those of the sinus of the vena portæ, nor any thing that resembled the figure of the liver, or of any of its appurtenances. He opened it, and was more and more convinced that it was rather a kidney, or knot of renal glands, than a liver, though it was one mass, and placed in the midst of the intestines; it had still less the resemblance of a heart, having no cavity, no vessels, nor any muscular fibres.

The extremity of the colon, or the rectum, passed between the bladder and the uterus as usual. He dissected all these parts, and traced the bladder up to the umbilical cord, where it lengthened into a pipe, and formed an open urachus. It had not the pyriform shape of the common bladder. It was in making these dissections of the kidney-liver, and those of the pelvis, that he divided the principal vessels, mentioned below. All these intestines, and especially the rectum, contained excrements of a light ash colour, but no meconium. The bladder, though lengthened and continued by an urachus, as high as the navel, opened in the usual place between the nymphæ. The anus was imperforate, and the rectum, immediately under the uterus, terminated in a blind pouch, attached to some membranes that went to the anus. This pouch was quite full of that sort of excrement just now described.

Mr. le C. had confined himself, in the first examination, to the singularity of a want of head, lungs, and heart, and of the existence of nerves, notwithstanding these defects. In his 2d review, the next morning, the organs of the circulation in such a production, which had lived nine months, raised his curiosity; but it was rather too late for entire satisfaction, for he had not taken care enough to preserve the internal parts, imagining before-hand that they resembled those of two headless twins, in his possession, whose inside was entirely similar to that of other fœtuses. He discovered however the following things:—In the region of the breast, the internal surface was lined with a membrane, which he took to be a diaphragm thrust back and stuck to the pleura, because it arose from lumbar muscular portions resembling the pillars of the diaphragm. Under this species of midriff was a very regular distribution of arteries and nerves.

In fig. 7, pl. 8, B is the lumbar region; C, those of the ossa ilia, and of the pelvis; D, the umbilical cord passing through an opening across the teguments of the lower belly, to be brought into view; E, the intestines; F, the aorta superior before-mentioned; GH, the integuments of all the right side, opened on the back, to preserve it whole, and also those of the belly; they are thrust to the right side to give a view within; a a kind of single kidney, which occupied the place of the liver. The principal part is covered by the intestines. It received several small vessels from the aorta, but none of them was near so large as the emulgent; b a small lobe, that might pass either for a small lobe of the liver, or for a capsula renalis; d an orifice of a vein which was also ramified, through the kidney-liver, by branches as fine as those it received from the aorta. But this vein did not terminate there; 1. It sent, upwards, branches to the muscles, to the vertebræ, and thence to the ribs; for, by blowing into the trunk, the air came out at the origin of the uppermost rib he had dissected; 2. Below, it formed two large iliac veins, g, which took the usual course; e the trunk cut, belonging to the aorta inferior, of the length of a line; f the distribution of the rest of this aorta, or right iliac, through that side of the pelvis; g the iliac veins going off to right and left as usual; h the orifice of the umbilical vein, being so exactly of the same diameter, of the same white colour and strength, and of the same consistence with the divided trunk e, that at first sight, he took this for the same vessel. The trunk d was much thinner, and of a more bluish tint.

Under the umbilical vein, was the umbilical artery on each side, which went as usual into the pelvis, and there sent off the ordinary ramifications. On the right side, its anastomosis with the iliac artery was very discernible; but on the left he could not discover the iliac, a vessel which is pretty considerable, even in the fœtus, and was so on the other side, as seen at letter i. On the left side, the umbilical artery, at its origin, or its inflection, had a kind of web of arteries subdivided, some of which doubtless communicated with the aorta, or right iliac; but neither of these branches appeared to be near the size of the right iliac, nor could he find any thing like to the trunk, which he long suspected to be the trunk of the right iliac; what vessels then are these trunks, d, e? Which of the 2 is the continuation of the umbilical, h? This is a very important point, but not easily determined. It would not have proved so, if he had injected the umbilical vein, as he generally did in all his monsters; or if he had taken more care of the parts about the kidney-liver, which he certainly should have done, had he but suspected so many singularities.

He said, that at first sight, he took the trunk e to belong to the umbilical vein, and it was still an opinion to which he was inclined, for the following reasons:— 1. At this part e, the aorta had its greatest circumference; and, in tracing it from this trunk, above as well as below, it grew less and less. This was, therefore, its trunk, or origin, and could not be a branch of it. 2. He had already said, that the mouths, h, of the umbilical vein, and e, of the aorta inferior, were exactly of the same diameter, of the same white colour, and of the same strength; and that the other vessel d was much weaker, of a laxer texture, and of a somewhat livid colour, like the coats of the veins.

The umbilical vein is, with respect to the mother, or to the placenta, which transmits the blood to the fœtus, a real artery, going from the centre to the circumference, or from the principal body, which is the mother, to an adjoining organ, which is the child; and the umbilical arteries are properly veins, which return the blood from that adjoining body to the common centre of the grand circulation. The blood from the umbilical vein then is truly an arterial blood to the fœtus. In the usual structure of the embryo, nature has shortened all the ways, to bring the arterial blood of the mother more speedily into the heart, into the very aorta inferior of her fœtus. Therefore, in a subject where there is no heart, or even liver, that vein ought to communicate immediately with the aorta inferior. In this manner one conceives how this subject could do without a heart, the umbilical blood being a continuation of that from the arteries of the placenta, the uterus, and in short of the mother; the impulsion of the maternal blood was propagated by that aorta through all its ramifications, both above and below. In short, the heart of the mother supplied that of the fœtus, and the circulation in this was a continuation of that of the mother. These are the reasons inclining him towards that first opinion; and here are those that suspended his judgment for some time in favour of the supposition of a communication of the umbilical vein with the trunk of the vena cava of the fœtus. 1. In every fœtus the umbilical vein empties itself into the vena cava in the liver; therefore nature has here followed her usual course. 2. With regard to the arterial trunk e, it is close by the division of the iliac vein; whence it is very likely that it was the left iliac vein which was divided.

This last, and most striking reason, made him employ almost a whole morning in looking over and over this left iliac region, to discover the divided vessel, which would have put the whole matter out of dispute; but he could find no trace or appearance of it. All the vessels communicating with the left umbilical vein appeared very entire, though deprived of part of their ramifications by which the air escaped, but all grew in their course less and less in diameter. Which then was the origin of this left umbilical artery? doubtless the branches of the trunk of the aorta, which were numberless in the pelvis, but had partly been spoiled the preceding night, in dissecting the rectum, uterus, and bladder of this monster. He adds, that this trunk was joined to some membranes, which he was obliged to pull about, to make it turn to the left, and this direction appeared not to be its natural position.

Be it granted for a moment, that e is the left iliac, and that the umbilical vein joins it at d; how could the blood circulate in this fœtus? How could it have lived the 9 months? d is evidently a trunk of the cava, which generally enters the right auricle of the heart, dividing, like this, into the cava superior, d, b, which rises by the vertebræ up to F, and into the cava inferior, d, g. 1. It would be absurd to place the only moving power of circulation in the vena cava, or indeed in any vein. 2. When you have placed it there, what will this supposition tend to? 3. This vein subdivides, and ramifies itself through the kidney liver, the muscles and the spine; but none of its branches communicate with the aorta. The aorta on its part sends several branches into the kidney liver, very slender, and resembling, by their transversal direction, the common emulgents, but very different in size. If then the circulating force were placed at d, it could only produce an inverse circulation, by the communication the cava might have by its capillary branches, with the like ramifications of the aorta, which supposition seems too much against nature to counterbalance the other opinion, which makes the trunk e of the aorta, a portion of the umbilical vein, and the substitute of the heart.

Another anatomical fact proves this last opinion; which is, that the aorta, and especially the superior, F, ran up as high as the cranium, and was of a pretty considerable size, while the venal trunk, d, had nothing but capillary branches in the upper parts; so that it almost appeared evident that the vessel EF was connected with the chief mover of the fluids. Therefore, supposing the trunk d to be the vena porta, or an imperfect cava going to some of the viscera, being the rudiments of an imperfect heart, or a vena cava ending in a pouch, as the intestines did which should have entered the stomach, if there had been one; the difficulty almost disappears. He says almost, because, even on this supposition, if there was a circulation in this monster, we must admit some anastomoses, between the arterial and venal system, which supplied those found in other fœtuses; since the venal blood must in some place or other re-enter the arterial torrent. Such might be the anastomosis K*, fig. 8. For, by this hypothesis, the vascular system of this subject would be represented by this fig. 8 thus:

A The umbilical cord; B the intestinal tube; D the kidney liver; E a sort of glandula renalis; a the umbilical vein: the great mover of the fluids; b the aorta, a continuation of that vein; cd aorta superior, accompanied by the vena cava; e aorta inferior; f the distribution of the iliac; g the umbilical arteries, making a part of the distribution; h the trunk of the vena cava coming either from the portæ, or from the viscus D, or forming a blind pouch in that part; some traces of the vena cava superior appear towards C; I the cava inferior going to form the iliacs; K a necessary anastomosis between the two kinds of vessels, arteries, and veins.

He repeats it again, if he had but suspected so many singularities, what he now could give only by way of conjecture, might have become demonstrable in fact. It was scarcely probable that he should ever have such another opportunity; but it was more so, that it might offer to some one among the great number of the literati in Europe, who read the Philos. Trans. This was the principal motive that determined him to present this observation, though imperfect, to the R. S.

Another motive, which engaged him to offer this observation, such as it was, was, that even the imperfection of it did not affect the useful consequences deducible from it; for whatever may have been the disposition of the blood vessels of this monster, it is a fact absolutely certain, that it had no heart, nor any other viscus in the place of it; and that the circulation of the fluids, which appears to

* Several letters of reference will be found in this paper which are not engraven on the plate: they were omitted from the plate in the original Transactions.

have taken place from the existence of the principal arteries and veins, could not have had any other moving power than the circulation of the mother itself. Hence this child, monstrous as it was, demonstrates the circulation of the blood from the mother to the fœtus, and from the fœtus to the mother again; which some moderns deny, and others endeavour at least to render doubtful. He presented to the Academy at Rouen some years ago, several observations which favoured the ancient system; the present comes to their support, to give this excellent hypothesis of Harvey all the credit it deserves.

The child he speaks of had no mouth, œsophagus, nor stomach; thus it could not, by that usual passage, be nourished from the waters that surrounded it; it could not absorb from the surrounding fluid wherewith to fill its vessels and supply its growth. It therefore follows, that it received both its arterial and alimentary fluids from the mother by the umbilical cord, and that it owed every thing to that circulation, which some would attempt to annihilate. In the intestines of new born children we find a black excrement, called meconium; this black pulp can receive its colour only from a bile thickened by retention, and poured directly from the ductus choledochus into the duodenum. Now this fœtus having no liver, nor gall bladder, &c. could have no meconium; therefore the pulp found in the intestines was of an ash-colour.

This monster had so little brain, that that viscus must have been of small import towards the functions of this animal. And yet all these brainless fœtuses are very lively. Mr. Denis, who, in his 12th conference, has given an account of one of them, and M. Vaissiere, who sent Mr. le C. one from Toulouse, the year before, both remark, "that these children are remarkably lively in the mother's belly; that they were in violent motion at the time of labour; that the moment they were in the world they seemed suffocated, and became all at once motionless." This is a matter worthy of much reflection. How can we conceive there can be sensation and motion, without hardly any brain in one of these monsters; and absolutely without any in the other? Sensibility, sensations, and passions, may exist without the brain, and have their seat in its meninges, and in the coats of the nerves formed by those meninges. If he had not endeavoured to prove that point in his physiology, and lately in his dissertation on the sensibility of the dura and pia mater, the observations of children and animals born without brain, which are pretty considerable in number, would demonstrate it by facts. That these children are more lively, that is, more sensible, he attributes to their having little of the nervous juice, though not less of the active fluid. This nervous juice he had termed fluide conservateur, the preserving fluid, in his physiology. It is long since he had observed that an abundance of this nervous juice produces the contrary of vivacity; and, as a consequence of our principles, long sleep, or rigid continency for a length of time, renders us heavy and benumbed, because in each of these circumstances, this nervous juice abounds and regurgitates, if he might so speak.

But these violent motions, whence have they their origin? there must be a nervous juice, to act in the muscles, and here we have very little. Neither of these animals was deprived of the medulla spinalis, and one of them had a small portion of brain, or cerebellum. This is one source of the nervous juice, and of the active fluid, necessary to muscular motion. This source, he grants, is weak and poor, but he had made it appear in his treatise on this subject, that there is in the blood a richer store, which the nervous fluid unites to, and makes use of, in muscular motion. By this it is explained, how it happens that an ass, which has so much less brains than a man, is yet so much stronger, because it has much more blood. Here then is a 2d spring that affords these monsters a considerable supply; but though sufficient for their motions, it is not equal to that of an ordinary fœtus, and the violent agitations of their body arise from their great sensibility, which we have just now accounted for. Now the blood in the fœtus, and especially in these, belongs to the mother; they are furnished by her, as well with air as with the nervous juice, and the animal fluid, which are essential to her. Therefore as soon as these children are separated from the mother, and deprived of that vital source, all motion must cease in them, as if they were suffocated, that is, as in any other suffocated fœtus.

Mr. le C. concludes with a word or two on the cause of these monsters. The great quantity of waters voided by the mothers of these children, proves that the principle of their monstrocity is a disease, a sort of dropsy, and even a kind of hydrocephalus, which had run off a considerable time before the labour. The 2 hydatides he found at the origin of the brachial nerves, and which had evidently been the cause of the mutilation of the upper extremities, are examples that help us to comprehend that of the other organs. On supposing a like disorder on the origin of other nerves, which have their rise from the brain, it will be obvious that the organs to which these nerves run, that is, where they convey the nervous fluid, which contain the rudiments of every part, will be wanting. It may indeed be said, there are hydrocephali that have all the organs very well formed: but there the disease has commenced after the perfect formation of these parts; whereas if you suppose it to have happened in the very time of that formation, you will see that the nervous juice, vitiated, diluted, and turned out of its natural course, can no longer be employed in the generation of those organs.

II. A Description of Three Substances mentioned by the Arabian Physicians, in a Paper sent from Aleppo, and translated from the Arabic, by Mr. J. Canning, Apothecary. p. 21.

The Tabashir, Mamithsa, and Mamiraan are used by the Arabian physicians;

by Rhazes particularly; in page 62 (note 32), page 110 (note 4), page 146 (note 6). Last week in the Bodleian Library, I met with an Arabic MS. of Dioscorides. It appears quite entire and perfect. The Greek titles are inserted in the margin by Dr. Hyde, in red ink, which is some proof of his value for this MS. It appears a real treasure, and it is likely it may be of excellent use in correcting the very corrupt text of that author: perhaps too it may be a means of ascertaining the Materia Medica of the elder Greek physicians. The Escurial MSS. contains only the first 3 books, and is imperfect at the beginning. If a transcript of this latter however could be obtained, it might likewise be of great use.

Copy of a Paper sent with the Specimens, by a Gentleman at Aleppo.

The specimens sent of the Tabasheer, though taken from different parcels, are not regarded here as different sorts. Among them will be found one or two pieces, which in their form answer to the ancient Arabic description of this drug. It is not from the sugar cane that the Tabasheer is supposed to be procured, but from that kind of cane of which the Arabs make their lances, and of which a piece is herewith sent. Different opinions concerning this substance, as also concerning the Mamithsa and Mameraan, collected from sundry Arabian writers, will be found in the paper inclosed: but as the Tabasheer is brought from the east, not prepared here, I cannot affirm any thing certain about it. From the specimens sent of the Mameraan, it will appear evidently to be a root. It is commonly believed to be a species of the Chelidonium, and, like the Tabasheer, brought from the east to Aleppo.

Mamithsa, is the common name used here for wormwood. Our mint is called Nana. The literary name, however, of wormwood is Ifsantin, absinthium. But there is a plant here known by the name of Mamitha, of which a specimen is now sent. This, from the use made of it by the natives in distempers of the eyes, as well as from other circumstances, appears to be the Glaucium of Dioscorides. An papaver corniculatum floribus cæruleis? Concerning the Isbidrowia, I have not been able to get any intelligence. The Orichalcum is called here Dgaam or Tûck. The paper made of silk husks is not to be found at present in the city. If any can be procured from the Bassora caravan lately arrived, it shall be sent.

*Translation of a paper in Arabic, sent to W——— C———, Esq. from Aleppo, with several Specimens of Tabashir.**

Tabashir.—In the Camus, an Arabic Lexicon, which the celebrated Golius translated into Latin, Tabashir is said to be a substance found in the hollow of that species of Indian cane of which lances are made: or the lower part of that

* See a further account of the Tabashir by Dr. Russell in the 80th vol. of the Philos. Trans., and Mr. Macie's experiments upon it in the 81st vol. of the Philos. Trans.

cane burnt. The Tabashir which is formed at the knots of the cane is round like a dirhem (ducat). This substance is found in the cavity of those canes which have been fired by rubbing one against another. It is frequently adulterated: the burnt bones of sheep, the skulls particularly, are sold for it. Ebn Beitar, in his Treatise of Simples, says, "Tabashir is a substance found in the hollow of the Indian cane." Ali Ebn Mohammed says, "It is the burnt part of the lower stem of the Indian cane; and is imported only from the coast of India; chiefly from that part of the coast called Sendapour, or Sendafour, whence the black pepper is brought." Avicenna, in his canon, says of Tabashir, "it is the lower part of the cane which has been burnt; it is reported that the canes are fired by being rubbed one against another by the violence of the winds. This drug is produced on the coast of India."

Mamitha.—Ebn Beitar, in his Treatise of Simples, calls it Mamitha. Abu'l Abbas the Nabathæan, the botanist, calls it Mamithsa. Both these names are sufficiently known. In the Treatise of Simples called Ma-la-Yesa, i. e. a treatise of those things which no physician ought to be ignorant of, Mamithsa, is the name of a plant like the papaver maritimum, or corniculatum. At the lower part of the Mamithsa is a moisture which sticks to the hand: it has a yellow flower like the papaver before-mentioned; its seeds are different, inclining to black, like and about the size of the seeds of sesamum. The plant is of a strong and offensive smell, and very succulent. The difference between these two plants is this; the papaver corniculatum dies to the root in the winter, and sprouts again from its root in the spring; the Mamithsa, on the contrary, sprouts again in the spring from the top of its stem." Avicenna, in his canon, says, "Mamithsa is like acorns, of a yellow colour inclining to black, easily broken. It is bitter, of a substance watery and earthy; cold, but not vehemently so; its juice is in the same degree of cold as the water of pools or lakes. It is prepared from a plant which is brought from Manbedge, (a town of note in Syria), of a very diffusive scent, a bitter taste, whose juice is yellow, of a saffron colour."

Mamiraan.—In the Liber Memorialis, it is said, Mamiraan is a plant, at the bottom of whose stem are produced knotted, crooked, hard roots. The Indian is the best; this inclines to a black colour: the Chinese to yellow: the other sorts are green. It grows in the water; its leaf is like the leaf of the convolvulus; it is hot and bitterish; its seed is like that of sesamum. It is said in the canon of Chalid and Manown, "Some say it is a root, and called Mamiraan, others say, the smaller roots are called Mamiraan, but the larger Zeradgush." In Castell's Lexicon, col. 308, and in Meninski, col. 2441, the word is Zeradgiob, which signifies yellow wood. and is the Persic name for curcuma. Avicen, in his canon says, "Mamiraan is a woody, knotted substance, inclining to a black colour, has small curvatures, and is one of the things used by dyers."

Ma-la-Yesa says, " Absinthium is a Greek word, in Persic it is called Mowi-Chowsheh. This is a plant which grows freely and largely; it rises in a stem, from which shoot out many branches, on which are many thick and tufted leaves; it bears a flower like that of a parthenium, small and white; in its middle it has a part yellow; its head is small, in which is a small seed; its taste is bitter and styptic. Some sorts of it have a leaf like the daucus, and a yellow flower. The inhabitants of Egypt call this kind of it Demsisah. It grows plentifully in the east, and in Syria, Chorasan, and Irak. The two last sorts of it are less esteemed, and of less value." Absinthium " some physicians call this Alshich Alroumi", i. e. Absinthium Ponticum, or Romanum. Look into the canon of Avicenna, under the article Absinthium, you will find there several things concerning Mamithsa.

III. A General Investigation of the Nature of the Curve, formed by the Shadow of a Prolate Spheroid, on a Plane standing at Right Angles to the Axis of the Shadow. By Mr. George Witchell, F. R. S. Dated Fleet-street, Jan. 7,* 1767. p. 28.

The following is an investigation of an irregularity in the duration of the eclipses of Jupiter's satellites, occasioned by the figure of his body. It has been long known, that Jupiter's body was not truly spherical, but a prolate spheroid, and that in a much greater degree than any of the other planets; but yet it was

* This excellent astronomer was born in 1728, and died of a paralytic stroke in 1785, consequently at 57 years of age. He was descended by the mother's side from the celebrated clock and watch maker Daniel Quare, and was himself brought up to that business. He was educated in the principles of the quakers, all his progenitors for many generations having been of that community; but he quitted them on arriving at years of maturity for those of the church of England, or rather for those professed by Sir Isaac Newton, Dr. Clarke, and Mr. Whiston, and many others; though he all his life continued to act with the same simplicity and integrity of manners and conduct, as the best of those whose community he had quitted, being a man of a most worthy and upright character. It appears that Mr. W. cultivated the study of astronomy at a very early age indeed, as he had a communication on that subject published in the Gentleman's Diary for 1741, which must have been written before he was 13 years of age. Soon after he became a pretty constant correspondent both of the Diaries and the Gent. Magazine, which he continued a long time, sometimes under his own name, but more frequently under the initials G. W. In 1764 he published a map of the passage of the moon's shadow over England in the great solar eclipse of April 1 that year, the exact correspondence of which to the observations gained him great reputation. In the following year he presented to the commissioners of longitude a plan for calculating the effects of refraction and parallax, on the distance of the moon from the sun or a star, for facilitating the discovery of the longitude at sea; and for which the commissioners gratified him with a very handsome reward. Having been elected F. R. S., and taught mathematics in London for many years, with much reputation, he was, in 1767, appointed head master of the Royal Naval Academy at Portsmouth, on the recess of Mr. Robertson. There he died, after a residence of 18 years, and was succeeded in that office by Mr. Bailey.

never suspected that it would affect the durations of the eclipses of the satellites, till Dr. Bevis first thought of it, in the latter end of the summer 1761. The Doctor, being at that time indisposed, recommended the subject to Mr. W.'s consideration; and in consequence Mr. W. not long after presented him with a solution of the problem, being in substance the same with this, as far as proposition 5; a copy of which he soon after transmitted to that excellent mathematician the late M. Clairaut.

In March 1763, M. de la Lande, an eminent French astronomer, being here, Dr. Bevis showed him Mr. W.'s paper; this occasioned a new article in the Conn. des Mouv. Celest. 1765, p. 177, under the title, Inégalité dans les demi-durées des éclipses des satellites de Jupiter, causée par l'applatissement de Jupiter: in which he mentions this circumstance in the following words; M. le Docteur Bevis me fit voir à Londres, au mois de Mars dernier, une solution rigoureuse et algebraïque de ce probléme, qui consiste à trouver la courbe qui resulte de la section de l'ombre d'un spheroïde à une distance quelconque.

A few months since, M. Bailly, a French gentleman, published at Paris an elaborate treatise on the theory of Jupiter's satellites; in which he has been pleased to give the honour of this discovery entirely to M. de la Lande, without the least mention of Dr. Bevis. Mr. W. then thought it incumbent on him to do justice to the doctor, by immediately finishing his paper in the best manner he was able, and presenting it to the Royal Society.

Lemma. If any spheroid be cut by a plane, in any direction whatever (excepting that which is perpendicular to its axis), the figure of the section will be an ellipsis. This is demonstrated in Simpson's Fluxions, vol. 2, p. 456.

Prop. 1. In pl. 8, fig. 9, Let the sphere BEGK be cut through its centre by the planes BGK, BPD, B*o*D, BOD, EAK, and LPH; it is required to determine the inclination of the planes LPH, BOD, and also the inclination of the right lines AC, BC, which is measured by the arc AB; there being given the angles of inclination EBF, FBa, together with the arc BF: the angles AFB, EAL, being right angles, and the inclination of the required plane BOD, but little exceeding that of the given plane B*o*D.

Let the sine of EBF $= a$, its cosine $= a'$, the tangent of BF $= b$, its cosine $= b'$, the sine of FBa $= p$, its cosine $= p'$, the sine of aBA $=$ z, the sine of AB $=$ z, its cosine $=$ z$'$, the sine of PAO $= \zeta$, and radius $= 1$; then will the sine of ABF $=$ the sine of (FBa $+$ aBA) $= p + p'$z, and its cosine $= p' - p$z: therefore by trigonometry, in the right-angled spherical triangle ABF, as rad. (1): cosine BF (b') :: sine ABF ($p + p'$z): cosine BAF $=$ sine of LAB, or its equal PAO; therefore $\zeta = b' \times (p + p'\text{z}) =$ the sine of the required inclination of the planes LPH, BOD. In like manner in the same triangle it will be as rad. (1): cotan BF

$(\frac{1}{b})$ :: cosine ABF $(p' - pz)$: cotan. BA $= \frac{z'}{z}$; hence $z = \frac{\beta}{\sqrt{b^2 + (p' - pz)^2}}$, and $z' = \frac{\beta}{\sqrt{b^2 + (p' - pz)^2}}$, which are the sine and cosine of the required arc AB.

Corol. 1. If, instead of a sphere, we now suppose BEGK represent a prolate spheroid, whose axis is CP; the figures of the sections LPH, BOD, &c. instead of circles, will become ellipses, by the lemma; but it is evident that the inclinations of those planes to each other, and likewise the inclination of the right lines AC, BC, or the angle ACB, will remain unaltered.

Corol. 2. If BEGK represent any primary planet revolving about the sun, in an orbit whose plane coincides with the plane BCD, it is manifest that BCD will be its ecliptic, making the angle of obliquity BIE with its equator EAK, whose pole is P; and if B be the place of the sun in this ecliptic, at any given time, the arc BI will be the distance of the sun from the nearest equinoctial point I; and the arc BF his declination at the same time.

Corol. 3. If the plane POG, which passes through P, the pole of the spheroid, be perpendicular to the plane LPH, it will also be perpendicular to any other plane BOD, which passes through A, the intersection of the equatorial plane EAK with the plane LPH; therefore, the angle ACO being a right angle, it is evident that AC will be the semi-transverse, and CO the semi-conjugate axis of the elliptic section BOD.

Corol. 4. Hence it appears, that the transverse axis of any elliptic section BOD, made by a plane passing through the centre of the spheroid, will always be equal to the equatorial diameter of the spheroid, but the conjugate axis will be longer or shorter, according as the inclination of the planes LPH, BOD, is more or less.

Prop. 2. Fig. 10. To find the length of the semi-conjugate axis CO, of the elliptic section AO*b*, formed by a plane cutting the given prolate spheroid POG through its centre C, and making the angle PCO with the axis CP.

Let the sine of the angle PCO $= \zeta$, CG $= t$, CP $= c$, CO $= x$, radius being unity; draw PO perpendicular to CP: then in the right angled plane triangle BCO, as rad. (1) : CO (x) :: sine PCO (ζ) : BO $(x\zeta)$; and rad. (1) : CO (x) :: cosine PCO $(\sqrt{1 - \zeta^2})$: BC $(x\sqrt{1 - \zeta^2})$; but, from the nature of the ellipsis, $\frac{c^2}{t^2} \times (t^2 - x^2 \zeta^2) = \text{BC}^2 = x^2 - x^2 \zeta^2$; therefore $x^2 = \frac{t^2 c^2}{t^2 - (t^2 - c^2) \times \zeta^2}$; or putting $t^2 - c^2 = f^2$, and $t^2 - k^2 = \varphi^2$, we have $x^2 = \frac{t^2 c^2}{t^2 - f^2\zeta^2}$, and $\varphi^2 = \frac{t^2 f^2 \times (1 - \zeta^2)}{t^2 - f^2\zeta^2}$.

Prop. 3. Fig. 11. Let BOD be an ellipsis, whose transverse diameter A*b* makes the angle ACB, with the right line BCD; and let T*k*G be a tangent to the ellipsis in the point F, making the angle GTC with the right line BCD: it is required to find the length of the normal C*k*, drawn from the centre of the ellipsis, to the tangent TG.

From C, the centre of the ellipsis, let CE be drawn parallel to the tangent TG, meeting the ellipsis in the point E; and CG perpendicular to the line BCD, meeting the tangent in the point G: put the sine of ACB = z, its cosine = z′, the sine of TGC = v, its cosine = v′, radius being unity, AC = t, CO = x, and $t^2 - x^2 = \varphi^2$; then will the sine of OCE (= the sine of OCD + DCE) be expressed by z′v + zv′; and by the last prop. CE = $\frac{tx}{\sqrt{t^2 - \varphi^2 \times (z'v + zv')^2}}$; but, by conics, CE × C$k$ = CO × CA, whence we obtain Ck = $\sqrt{t^2 - \varphi^2 \times (z'v + zv')^2}$.

Prop. 4. Fig. 12. In the two similar right angled plane triangles HKS, HMN, right angled at K and M, there is given the right lines KS and MS, to find the acute angles, supposing the given angle hnM to be nearly equal to the required angle HNM.

Put MS = Δ, KS = r, MN = υ, the sine of the given angle hnM = q, its cosine = q', the sine of HNM = v, its cosine = v′, the sine of (HNM — hnM) = x, and radius = 1. Let ML be drawn parallel to HK, and Ml parallel to SK: then in the right angled plane triangles NMl, SML, we have as rad. (1) : MN (υ) :: sine HNM (v) : Ml (υv), and as rad. (1) : MS (Δ) : sin. LMS (v′) : LS (Δv′); but Ml + LS + = KS; therefore υv + Δv′ = r, and by the foregoing notation v = $q + q'x$, and v′ = $q' - qx$; therefore these values of v and v′ being written in the above equation, we shall find x =

$\frac{q'\Delta + q\upsilon - r}{q\Delta - q'\upsilon}$; hence v = $\frac{\Delta - q'r}{q\Delta - q'\upsilon}$, and v′ = $\frac{qr - \upsilon}{q\Delta - q'\upsilon}$.

Prop. 5. Fig. 13. If the opaque prolate spheroid BPOD, given in species and position, be opposed to the given luminous sphere HKQI, at the given distance CS, forming the shadow Ffbc: it is proposed to determine the figure of the section aRN made by a plane, cutting the shadow perpendicularly to its axis at the given distance MS.

Let the required curve aRN be conceived to be generated by the extremity R, of the variable right line MR, revolving about the given point M as a centre, the line MR being always perpendicular to the axis of the shadow MS: let the right line RQ be a tangent to the sphere HKQI in the point Q, and in the same plane with the right lines RM, MS; it will then represent one of the rays of light, which constitute the conical superficies of the shadow, and, therefore, by the laws of optics, will be a tangent to the spheroid also; now when the generating point R has arrived at N, the ray RQ (being supposed to revolve with it) will coincide with the tangent NK, touching the sphere in K, and the spheroid in F: join K, S, and the angles NMS, and NKS, will be right angles; let the spheroid be supposed to be cut, by the quadrangular plane NMSK, forming the elliptic section BOD; draw Ck perpendicular, and Cl parallel to NK; put CA = t, MC = δ, CS = d, MS = Δ, SK = r, MN = υ, CO = x, the sine of bNM = v, its cosine = v′ the sine of ACB = z, its cosine = z′, and radius = 1: then in the right angled plane triangles cls, it will be as rad. (1) : cs (d) :: sine scl (v) : sl (dv'), and consequently ck ($=$ KS $- sl$) $= r - dv'$; but by prop. 3, $ck = \sqrt{t^2 - \phi^2 \times (z'v + zv')^2}$, hence $r - dv' = \sqrt{t^2 - \phi^2 \times (z'v + zv')^2}$: now by prop. 1, we find $\zeta = b' \times (p + p'z)$, $z = \frac{-\beta}{\sqrt{\beta^2 + (p' - pz)^2}}$, and $z' = \frac{p' - pz}{\sqrt{\beta^2 + (p' - pz)^2}}$; by prop. 2, $x^2 = \frac{t^2c^2}{t^2 - f^2\zeta^2}$, and $\phi^2 = \frac{t^2f^2 \times (1 - \zeta^2)}{t^2 - f^2\zeta^2}$; lastly by prop. 4, $v = \frac{\Delta - q'r}{q\Delta - q'\upsilon}$, and $v' = \frac{qr - \upsilon}{q\Delta - q'\upsilon}$; which values, being substituted in the above equation, will exhibit the nature of the required curve aRN, in terms of z and υ.

SCHOLIUM. If the sphere HKQI represent the sun, and the spheroid BPOD one of the primary planets, it will appear, from the preceding reasoning, that the figure of the section of its shadow received on a plane, which is perpendicular to its axis, will not be a circle, except when the axis of the planet produced passes through the sun's centre, but a curve of the oval kind, whose species will be known from the foregoing equation. If the sphere HKQI had been regarded as a spheroid in the above solution, it is easy to see that the foregoing process would have determined the nature of the required curve; but the figure of the sun is so nearly spherical, that it was not thought necessary to embarrass the solution with that consideration.

Hence the duration of an eclipse of a given satelles may be determined in the following manner: let BRC (fig. 14) be the section of the shadow, through which the satelles passes, NpN the path of the satelles, making the given angle NpM, with the circle of latitude RpM; BMC a part of the primary's orbit produced, and Mp the given latitude of the satelles at the time of the syzygia; the circle of latitude RpM is represented in fig. 9, by the primitive circle BEGD, and the angle RMN, by the spherical angle EBA; therefore the sine of RMN $=$ the sine of EBA $=$ the sine of (EBF $+$ FBa $+$ aBA) $= ap' + a'p + (a'p' - ap) \times z$, and its cosine $= a'p' - ap - (ap' + a'p) \times z$; which for the sake of brevity may be expressed by y, and y'; then putting Mp $= n$, MN $= \upsilon$, the sine of MpN $= m$, its cosine $= m'$, and radius $= 1$; we shall have the sine of MNP expressed by $my' + m'y$; and therefore we shall have in the plane triangle MpN, as the sin. MNp ($my' + my$) : Mp (n) :: sine MpN (m) : MN (υ); hence $\upsilon = \frac{mn}{my' + ym'}$; from which, and the equation of the curve (determined above) $\frac{\upsilon y}{m} = p$N, and consequently the duration of the eclipse will become known.

In prop. 1, the sine of the angle ABF is expressed by $p + p'z$, and its cosine by $p' - pz$, instead of their true values $pz' + p'z$, and $p'z' - pz$; this was done to render the following conclusions more simple than they otherwise would have been; and as the angle aBA is, by hypothesis, but small, its cosine will approach so near to the radius, as not to occasion any sensible error in the result; and the same may be observed with regard to what is advanced in prop. 4.

It remains now to apply what has been investigated above, to the eclipses of Jupiter's satellites, and to examine whether the prolateness of his figure will have any sensible effect on their durations; and this is become the more necessary, as that celebrated astronomer M. de la Lande (who candidly acknowledges that he was excited to turn his thoughts on this subject, from a cursory view of this paper, which was shown him by Dr. Bevis) does not seem to have considered the question with that degree of attention which Mr. W. thinks it demands.

But before this can be done with exactness, it will be necessary to have the inclination of Jupiter's axis, with respect to his ecliptic, and the place of his equinoxes, determined by observation, neither of which he believes has yet been done with any degree of certainty; he therefore proceeds in this inquiry on M. de la Lande's hypothesis, that Jupiter's axis is perpendicular to his orbit; and perhaps this supposition is not so far distant from the truth, as to occasion any material error in the conclusion. It may also be remarked, that in the general equation given above, v′ and v express the sine and cosine of the semiangle of the cone of Jupiter's shadow; but this angle can never exceed 3′, and consequently we may very safely use the radius instead of v wherever it occurs.

By this means the general equation will become $r - d\text{v}' = \sqrt{t^2 - \phi^2}$, or, which is the same, $r - d\text{v}' = \varkappa$, therefore $\text{v}' = \frac{r - \varkappa}{d}$; but by prop. 4, $\text{v}' = \frac{qr - \upsilon}{p\Delta - q'\upsilon}$, which, because q is nearly equal to v, and $q'\upsilon$ very small with respect to $q\Delta$, will become $\text{v}' = \frac{r - \upsilon}{\Delta}$; therefore $\frac{r - \varkappa}{d} = \frac{r - \upsilon}{\Delta}$, from which we shall find $\upsilon = \frac{\Delta\varkappa - \delta r}{d}$; and this equation is exactly the same with that which would arise from considering the sun as a circular, and Jupiter as an elliptic plane, limited by one of his meridians, and always parallel to the disk of the sun; which supposition, the immense distance of Jupiter from the sun renders very allowable.

From this equation an easy mechanical method may be derived of delineating the curve of the shadow, at any given distance from Jupiter. For as $\varkappa$ denotes any semidiameter of the elliptic section of Jupiter's body, it is manifest that the term $\frac{\Delta}{d} \times \varkappa$ will express the corresponding semidiameter of a similar ellipsis, whose axes are to those of Jupiter, in the given ratio of Δ to d, and the term $\frac{\delta r}{d}$ is wholly given; therefore if *arm* (fig. 15) be such an ellipsis, and there be drawn through its centre M any number of semidiameters M*a*, M*b*, M*c*, &c. meeting the ellipsis in *a*, *b*, *c*, &c. let *a*A, *b*B, *c*C, &c. be taken each equal to the given term $\frac{\delta r}{d}$, and the points A, B, C, &c. will be in the required curve.

It appears, from considering the nature of this curve, that it will have two cusps, one at each extremity of its lesser axis, which will approach toward each other, according as the distance δ is augmented; therefore, if the distance of the section of the shadow, from Jupiter's centre, was taken such, that $\delta =$

$\frac{dc}{r-c}$, the lesser axis of the curve would then vanish, and the cusps meet in the centre, and form two distinct shadows, as represented in fig. 16; in consequence of which, if a satelles, revolved at that distance, it might suffer a double eclipse, at the same conjunction, which remarkable phenomenon may also happen, at a less distance from Jupiter, in some circumstances. Mr. W. now shows how the duration of an eclipse of a given satelles may be determined independant of the equation of the curve; and this perhaps will be the more acceptable, as it affords a practical rule, which may be applied, in every position of Jupiter's axis, with very little trouble. This may be done by the help of the following proposition.

Prop. 6.—If a circle E D *f* G, fig. 17, be described about the conjugate axis GD, of a given ellipsis ADBG, and a right line EF be drawn, making the given angle FπD, with that axis, and passing through the given point π taken in it; it is proposed to determine the length of the segments F*f*, E*e*, intercepted between the circumference of the circle, and the perimeter of the ellipsis.

From the point F, draw the right line F*d* parallel to the transverse axis AB, meeting the conjugate GD in the point *d*, and the circle in *c*; draw the lines CF, C*f*, C*c*, and let *c*π be joined: then, by conics, as CB : CD :: tang. FπD : tang. *c*πD, and in the right lined triangle Cπ*c*, as C*c* (CD) : sin. Cπ*c* :: Cπ : sin. C*c*π, whence the angle *c*Cπ becomes known; but as CD : CB :: tang. *c*Cπ : tang. FCπ; therefore FCπ is known; from which taking away the given angle *f*Cπ, there remains the angle FC*f*; consequently all the angles, in the right lined triangle *f*CF, together with the side C*f* (CD), are known: we shall therefore have, in the right lined triangle, F*f*C, as sin. *f*FC : C*f* :: sin. *f*CF : *f*F, one of the required segments; and by a similar operation, the other segment E*e* will be found, whence as *ef* is given, EF will become known.

Corol. 1. The required segments F*f*, E*e*, will be found in the same manner, when the given point π is not taken in one of the axes, but any where between; but in that case, the point where the line EF intersects the conjugate axis, must be first determined.

Corol. 2. If a perpendicular C*n* be let fall from C on the line EF, the angle πC*n* will be given, to which adding FCπ (found above) the angle FC*n* will be known; hence we shall have the following analogy for determining F*n*; as tang. *f*C*n* : tang. FC*n* :: *fn* : F*n*. Now let K*k*H, fig. 18, represent the disk of the sun, and E D *f* G that of Jupiter, considered as a circle, whose diameter is equal to his axis DG: draw N*pn*, the path of the satelles, making the given angle N*p*R, with a right line R*g* drawn parallel to the diameter DG, and let *ab* be the duration of the eclipse, and V the apex of the shadow in this hypothesis; join V*a*, V*b*, and let the plane *a*V*b* be produced, till it meets the sun's disk in K and *k*, it will then intersect the disk of Jupiter in the line *f*π*e*, and the lines VK, V*k* will also touch the circumference of the circle EDfG, in the points e and f; draw the line SV, and it will be the axis of the shadow, and consequently will pass through C and M, the centres of Jupiter and the section of the shadow; join aM, bM, fC, eC, and the triangles abM, efC will be similar to each other, and therefore, abM being wholly given, feC will likewise be known. Let ADBG be the elliptic section of Jupiter's body, and produce eπf both ways, till it meets the periphery of the ellipsis in the points E and F; draw KF, kE, and produce them till they meet with ab, produced both ways, in N and n; then will Nn be the required duration of the eclipse in the true shadow: now the triangles KfF, KaN being similar, as are also the triangles keE, kbn, and the segments Ff, eE being given, by the preceding proposition, the required segments Na, bn will also become known, for they will be to the former segments in the given ratio of SM to SC.

It may be observed, that this method is equally applicable, whether the axis of Jupiter is perpendicular to his orbit, or not; for if it is not, we can easily find, by prop. 1 and 2, the species and position of that elliptic section of Jupiter's body, to which a right line connecting the centres of the sun and Jupiter is perpendicular; and this being obtained, every thing else will remain as before.

As it would require more time than Mr. W. had to spare, to enter into a particular inquiry concerning the alterations, which this irregularity in the shadow will occasion, in the present theory of Jupiter's satellites, he concludes with observing, that the errors in the semidurations of their eclipses, arising from this cause, may sometimes amount to 20^s in the first; 50^s in the second; 2^m 19^s in the third; and 11^m 14^s in the fourth; which errors he deems sufficiently large to merit the attention of astronomers.

IV. An Attempt to Account for the Universal Deluge. By Edward King, Esq., of Lincoln's Inn, F. R. S. p. 44.

Dr. Burnet, in his theory, has given such an account of the deluge, as Dr. Keill has shown to be very improbable, and unphilosophical. He has first described the primæval earth so as to divest it of all beauty and elegance, and then has ascribed the deluge to such causes, as are not only somewhat inconsistent with that part of his theory where he supposes the earth to be well watered and moistened with dew; but are also insufficient to account for the waters flowing over the tops of the mountains: since, on the breaking of his imaginary shell, it is impossible to suppose that the waters of the abyss, even on such a concussion, should flow up high enough on those parts that were left elevated, so as to cover the mountains that now subsist. Mr. Whiston has called in the assistance of another planetary body; and has supposed the tail of a comet to be so greatly condensed, as to afford a quantity of water sufficient for this purpose. But, besides the inconsistency of this theory with that of gravitation, it is no less difficult, according to his hypothesis, to get rid of the water with which the earth was covered, than it is, according to others, to find a sufficient quantity. Mr. Ray has accounted for this amazing event, by supposing a change to have happened in the centre of gravity of the earth. But how to find a cause for such a change in the centre of gravity, and for a restoration of it to the same place again, is more difficult, and the supposition of it more inconsistent with our philosophical ideas, than any other hypothesis whatever.

Such have been some of the principal theories hitherto advanced, and far be it from me, says Mr. K. to presume that mine may not in the end be found equally fallible; but it appears to be more plain and consistent, and at the same time is free from that great difficulty which has perplexed all the rest, and is indeed the most important difficulty in the inquiry, that is the accounting for a sufficient quantity of water.

We find in the Mosaic history of the creation, that God at the first created sea as well as land; and therefore have grounds to believe both from thence, and from the reason of things, that there was as great a quantity of sea on the antediluvian earth, as there is now on the earth in its present state. We find also the whole surface of the earth to be undermined by subterraneous fires, which make their appearance in various places, in very formidable volcanoes. This has been the case in Italy, and among the Azores, in Tartary, in Kamtschatca, in South America, in Ireland, in the islands of the East Indies, and in other parts: and we have reason to believe that these subterraneous fires have made eruptions, not unfrequently, even in the bottom of the sea; as Mr. Mitchell has made appear in his excellent paper concerning the causes of the earthquakes. We have also in the Phil. Trans. accounts of entire islands being raised in the Archipelago, and among the Azores, by such subterraneous fires; and Mr. Ray, in his travels, mentions a mountain 100 feet high, raised by the earthquake in 1538, which also threw up so much earth, stones, and ashes, as quite filled up the Lacus Lucrinus. To which may be added, that fossil shells and other marine bodies are so universally found in all parts of the present continents and islands, as to amount almost to a demonstration, that all the now dry land was once covered with sea, and that for a considerable space of time, probably much longer than the continuance of the deluge is related to have been. For though such a violent flux of waters might have thrown up some shells and marine bodies on the hills and mountains, yet it could not have flung up such vast quantities, nor so universally. The prodigious beds of shells which we now find in all parts cannot well be accounted for, but by supposing the waters, in which those shell-fish lived, to have covered the countries where they are now found, for a long time, and even for ages.

The supposition therefore, which Mr. K. advances, founded on these facts is this; that originally Almighty God created this earth with sea and land nearly in the same proportion as they now remain, and that it continued in that state for many ages, during which the bottom of the sea became covered with shells, and various heterogeneous bodies; that from the first of its creation there were also many subterraneous fires found within the bowels of the earth; and that, at the appointed time, these fires bursting forth at once with great violence, under the sea, raised up the bottom of the ocean, so as to pour out the waters over the face of what was before dry land, which by that means became sea, and has perhaps continued so ever since, as that which was before the flood the bottom of the sea, probably from that time has continued to be continent and dry land.

This hypothesis may perhaps be liable to great objections; but it is at least consistent with what Moses relates of the fountains of the great deep being broken up; and, without any perplexity or difficulty, accounts at once for a sufficient quantity of water to cover the tops of the highest antediluvian mountains, even supposing they were left standing: though it is not improbable but that they might be thrown down by means of the same earthquake. If they were left standing, some of them might, on the retreat of the waters from their tops after the first concussion, form some of the islands that now subsist. This hypothesis is also perfectly consistent with, and perhaps in some measure accounts for, that singular position of the strata of coals, ores, and various kinds of earths (mentioned in Mr. Mitchell's paper), which are found always sloping from mountainous countries, and higher grounds, towards the bottom of the sea; so that what is nearest the surface of the earth in mountains and high countries, lies deepest in low lands and under the sea. It is also somewhat confirmed by that singular observation of Dr. Hasselquist's, in his travels, (p. 33) where, speaking of Natolia and the eastern countries in general, he says, "In no place was it more evident that the continent, we call earth, was in the beginning the bottom of the sea." Ulloa also informs us, that the same thing is evident in the whole country of Valles in South America: and Norden tells us, that the rocks in Egypt bear evident marks of having been washed by the sea.

These are the reasons which induce Mr. K. to venture on this supposition. He next considers one or two objections, that appear the most material which may be made to what he has advanced. It may perhaps be said, that we read "of the waters returning from off the earth, and of their being abated at the end of the 150 days: and also, of the water decreasing continually till the 10th month; and of the tops of the mountains being then seen." And it may be objected that we ought thence to conclude, that the waters of the deluge, having covered what was before dry ground, afterwards retreated, and left the very same hills and land dry again. But this conclusion is by no means necessary; for all that can be inferred from what we find in Genesis concerning the decrease of the waters, is, that they gradually subsided from off the face of what is now continent and dry land, as of course they would do on the elevation of it, agreeable to the foregoing hypothesis. And indeed, if the deluge was effected in the way here supposed, we can then give a rational and easy account how all the water came to drain off the ground, and to leave it dry so soon as is recorded; which otherwise is a circumstance in this piece of history very perplexing. It is evident, that such a violent earthquake, or bursting forth of the subterraneous fire, as is here supposed to have raised the bottom of the then sea (the present continents), at once as high or higher than what was before dry land, must in a very short time have drowned and overwhelmed the antediluvian earth, by pouring out the waters upon it; and it is also evident, that for some time the bottom of the sea, so raised, would continue covered with the waters, which, till the vast agitation into which they were flung subsided, would continue flowing backwards and forwards. But, by degrees, and very easily within the time mentioned in scripture, the water would drain off from all the higher parts, and leave the new land quite dry, and in the state we now find it, with strata of shells, and sand, and stones, and other bodies, lying just as the sea had by accident many ages before placed them. Whereas, were the deluge occasioned only by an addition of water sufficient to raise the surface of the sea higher than the land and mountains, in that case, it is impossible to imagine any means, at all consistent with the course and laws of nature, by which such an immense body of water could be evaporated or conveyed away in so short a space of time. And besides, in that case, the shells, &c. flung upon the land by the concussion of the waters, and subsiding there within so short a space of time, would rather be found lying according to their specific gravities: a fact which Dr. Woodward supposed certain, but which is by no means true. Nor indeed, according to the conjectures here advanced, is it at all necessary that it should be so. For, as I imagine the shells and other marine bodies, which are now found on various parts of the dry land, to have been placed there gradually during a succession of ages, while it was at the bottom of the sea; it will follow, that they must be found just as the sea, by its washing and motion, laid them; which would of course first wash many of them together, and then wash gravel, or sand, or clay, or other substances over them; after which, more shells or other bodies would be deposited, and then more stones or gravel, &c. according to the nature of the soil. In short, whatever was specifically heavier than water, would, after its removal by any agitation, soon subside, and remain fixed, whether the substances underneath it were specifically heavier than itself or not; it is sufficient that they were only specifically heavier than the water.

Another objection may perhaps be made by saying, if all the antediluvian earth was at once overwhelmed, and of course all its plants with it, whence came it to pass, that the now dry land was so soon covered with vegetables and herbage of all kinds? To this I answer, in the first place, that the difficulty is just the same, whether we suppose the bottom of the antediluvian sea to be the present continents, or whether we suppose the face of the earth to have remained the very same; since, by the waters of the deluge, all plants, trees, and vegetables, must in both cases equally have been destroyed; and nothing could well remain, except some of their shoots and seeds; which might just as well take root on the new continent, on the subsiding of the waters, as on the old. And in the next place, there are not a few instances of barren rocks and plains becoming by degrees well covered with verdure, though very remote from any places that might apparently furnish seeds. They have first borne a kind of moss, and afterwards other plants of a higher order, the seeds being brought there by accident, and by the various and admirable means of conveyance, which the Creator has given them, till at last they have been covered with rich verdure. To which may be added a very extraordinary fact, now well known, namely, that if a piece of ground which has not been cultivated be turned up, and the clods loosened, it will very soon produce a variety of plants, some of which were never known to grow there before. We find that one acorn is sufficient to produce a forest, and it is by no means to be supposed, let the deluge have happened how it would, that immediately after it, the earth was as well clothed with verdure, as it has become since. Probably it was for a time in general very barren, except such parts as Noah and his sons cultivated, with seeds which they had preserved in the ark.

Another objection may perhaps arise from this circumstance, that shells are found in various parts of the earth, which are evidently not the shells peculiar to the seas adjoining, but such as belong to a different climate. This fact at first certainly seems to contradict what has been advanced; and yet, when well considered, it will perhaps rather be found to confirm this hypothesis. For let any one but look on a terrestrial globe, and he will instantly see, that the present continents are evidently not in the same climates as the present seas; and therefore, though the shells found in many places of the earth are not found in the neighbouring parts of the ocean; yet, when those parts of the earth were ocean, they might have had a very proper climate and situation there. Thus, for instance, we may observe that the Mediterranean is in a more southern climate than the neighbouring continent of Europe, and in a more northern climate than that of Africa. And the whole continent of Asia is in a climate much more northern, than the neighbouring Indian ocean.

Another thing Mr. K. takes notice of, is the horns and bones of terrestrial animals being found in the earth, together with the fossil shells; which seems to contradict the supposition of the present continents having been originally the bottom of the sea. But with regard to this, he observes, that probably some of those bones have been deposited there since the flood, and have been covered by an addition of earth, as has happened also to some of the trees and woods that were cut down in this island by the Romans. And, as to the rest, it cannot be supposed, but that on the first great eruption, which poured the waters of the ocean upon the dry land, there must have been a violent agitation for some time, by their flowing backward and forward; during which interval, the bodies of many terrestrial animals, floating on the water, would be washed to different parts of the new-raised continent, and be left there as the water subsided.

V. An Attempt to account for the Formation of Spars and Crystals. By Edward King, Esq. F. R. S. p. 58.

In the first place, it is known, that the Bristol stones grow within the hollow cavity of some other rough stone; and that the substance of the external stone is porous, and often so strongly impregnated with crystalline corpuscles, that they glitter among the earthy particles, when held up to the light. 2. In the next place, it is to be observed, that wherever there is a hollow cavity in these kind of stones, the inside is almost always lined with such shining substances, either in a perfect or imperfect state. 3. We find the Bristol stones appear in several different states; for in some places of the cavity, where the crystallization is not completed, they are of a dusky red, without any transparency; in others they appear of a dirty yellow; and in others white; and at last transparent. 4. As to the spars and crystals formed even in flints, and other hard bodies; they are generally observed in such as have evidently been at one time or other in a soft state, and lay in or near moist places strongly impregnated with saline particles; or else they are found in bodies wherein some saline and moist substances have formerly been inclosed, and prevented from evaporating; of which kind are the spars found in fossil shells, in which the bodies of the shell fish have perhaps lain and perished. 5. We observe, not only in the small cavities of stones, but also in large caverns, such as those in the Peak in Derbyshire, Okeyhole in Somersetshire, and the famous grotto in the Greek island of Antiparos, and in short wherever moisture descends through the earth to a void space, and stops on the inner surface, that it there forms crystals, or spars, or stony concretions of some sort or other; of which some are so very imperfect, as to have only the appearance of rude heaps of petrified matter, without any regular form, which chiefly happens where there is much moisture, and where it descends, or soaks through pores so large as to carry many earthy particles with it. 6. To all which he adds, that Sir Isaac Newton has made it appear, that the transparency of bodies is occasioned by the minuteness of their pores, and the opacity of them by the largeness of the pores, in which the rays of light being reflected from side to side are lost, and prevented from passing through; whence it is, that paper becomes transparent by being oiled, and the oculus mundi stone by being soaked in water.

These are the principal observations on which Mr. K. founds his conjectures; and hence he is induced to conclude, that all these above-mentioned substances, are formed by means of those crystalline, perhaps saline, corpuscles, with which the surrounding earth or porous stones abound, and which probably are diffused throughout the whole globe, and mixed in some degree with most strata. These small particles, he apprehends, are carried along gradually, by the moisture, or vapors, which soak through the pores, till they come to some cavity, and there, being stopped by the discontinuance of the earthy or stony substance from proceeding any farther, they collect together in drops, and as they dry and harden, do of course, by their mutual attraction, form themselves into crystalline figures; and as the pores are more and more filled up, by the accession of more corpuscles, or by their mutual attraction which draws them closer together, they become more and more transparent. Some of the bodies however thus formed never have any transparency at all, being mixed with too many earthy or stony particles, or other heterogeneous matter, and have sometimes so much of that as not to be able to put on any regular form, but only to petrify in a confused heap; the earthy or stony particles preventing the crystalline or saline particles from forming themselves, by their mutual attraction, into regular figures; and there being perhaps but few of the true crystalline corpuscles mixed with them. This seems to be the case with many of the stony concretions in large caverns: and perhaps, from a small mixture of these same heterogeneous particles it is, that spars are inferior to crystals, and also differ from one another. Mr. Platt, in the Philos. Trans., Vol. 54, has observed, that spar seems to be nothing but crystal debased by a calcarious earth.

Mr. K. suspects, that what he has called crystalline corpuscles, are in reality a kind of salts; he therefore calls them hereafter by that name; and now endeavours to illustrate what he has said more particularly by the instance of Bristol stones. In their first state, these are of a dirty red, or some other dusky colour; but afterwards, as more salts, or crystalline corpuscles, are added, by the descent of moisture, or the passage of more vapour, they begin to be more compact; and then, the pores becoming smaller, they approach nearer to transparency, and put on a yellow or whitish colour; and at last, receiving a further addition of salts, and having the component particles drawn still closer together by their mutual attraction, they become still harder and more transparent, till they acquire, by a length of years, their greatest degree of perfection.

In such formations he considers the largest caverns in the earth, and the smallest cavities in stone, as producing similar effects, and therefore considers them in the same light: for wherever there are cavities in the earth, or in stones, into which moisture can any way descend, we almost always find these kind of crystallizations and concretions; and the more plentiful the moisture is, and the more porous the strata of earth or stones are through which it passes, the larger the concretions are, and the more remote from a transparent state; as appears in those great caverns in the Peak, and in Somersetshire, &c. Whereas, on the contrary, the harder and less porous the substance is, through which the moisture passes, the more transparent are the stones formed by it, as in the case of Bristol stones, and of some of those beautiful spars adjoining to veins of ore.

Whether all kind of stones may not be formed in somewhat the same manner, by the water carrying the stony particles to the same place, and their collecting there together, by their mutual attraction, he leaves to others to determine; but he is much persuaded, that this may probably be the manner of the production of spars and crystals; and perhaps jewels, or precious stones, may grow just in the same way; and owe their perfection solely to their being composed of still more minute salts, and more slowly; whence we may conjecture, why it is so rare to find large diamonds. Some of the Bristol stones are observed to have a fine purple appearance, like an amethyst; and it is well known, that several sorts of spars are of various beautiful colours, by means of a mixture of mineral particles, in which they have a distant resemblance of jewels; and indeed they seem so be very analogous to them in many respects.

VI. Experiments with Camphire. By Mr. Alexander, Surg. in Edinburgh. p. 65.

As medical authors have differed so widely in their opinions concerning the nature and effects of camphire, one part of them positively affirming that it heats the body, and another asserting with the same confidence that it cools it; Mr. A. made the following experiments with it, in order, if possible, to clear up the difficulty.

If camphire was a heater, he concluded it would raise his pulse, and augment his natural heat; and therefore, previous to his taking it, he counted the number of pulsations in a minute, which were 68, and found that, in the space of 5 minutes, the mercury in Fahrenheit's thermometer rose 18 degrees by the heat of the pit of the stomach. Having thus found the state of his pulse, and of his natural heat; he took ℈j of camphire in a little of the pulp of tamarinds; and 20 minutes after he applied the thermometer to his stomach: the mercury, in the space of 5 minutes, rose exactly 18 degrees, as it had done before taking the dose, but his pulse beat only 66, which was 2 strokes less. Three quarters of an hour after he had taken the camphire, he applied the thermometer again; in the same space of time, the mercury rose exactly the same as in the last trial, but his pulse beat only 65, which was one stroke less, and 3 fewer than it had done before he took the camphire.

The next day, having found that the mercury rose 19 degrees in 5 minutes, by the heat of his stomach; and that his pulse beat 77 in a minute, he took ℈ij of camphire in a little of the syrup of pale roses; immediately after swallowing it, he felt a sensation in his mouth something similar to that occasioned by strong peppermint-water, but much more disagreeable; 10 minutes after he had taken it he applied the thermometer to his stomach; in 5 minutes the mercury rose 18 degrees, which was 1 degree less than it had done before he had taken it. His pulse now beat only 70, whereas before he took the dose it had beat 77: 25 minutes after he had taken it, he applied the thermometer again, and the mercury rose the same as at the last trial, but his pulse had increased from 70 to 77, the exact number which it had beat before he took the camphire; soon after this his head became so very giddy, that it was with great difficulty he could walk through the room. In this condition, he had an inclination to breathe the fresh air, opened the window and looked over into the street, where every thing appeared to him in the utmost tumult and confusion; feeling himself in danger of tumbling from his station, he shut the window and staggered from it to bed, threw himself down upon it, and having a book with him, endeavoured to read, but had no distinct idea of any one sentence, and far less could he connect 2 or more of them together, so as to comprehend the meaning of the author: not being able to amuse himself by reading, he rose, to see whether he could walk any better, but, to his great mortification, found that he was more giddy, and could hardly walk at all. He then returned to the bed, and feeling himself thirsty, called for some mutton broth; it being dinner time, the servant, instead of bringing the broth, covered the table as usual, not knowing that he was any way disordered. Seeing the dinner on the table, he got out of bed again, and with no small reluctance, swallowed down a plate-full of the broth, but could neither taste bread nor meat, on account of a nausea, which however was not accompanied with any inclination to vomit.

He now staggered again to the bed, and took up the book he had left there, with a design to divert the attention of his mind into some other channel than that into which the confusion of his ideas had hurried him: at this time, self-preservation suggested to him the expediency of taking a vomit; but as he felt very little pain, and was not apprehensive of much danger, he resolved not to spoil the success of his experiment by evacuating the camphire before he should discover what its effects would be. Hitherto, amid a tumult of indigested ideas, he had retained some degree of sensibility; but now there arose such a noise in his ears, the confusion and giddiness of his head increased so much, that all consciousness of what was present, as well as memory of what was past, were soon entirely obliterated, so that whether he endeavoured to read in the book he had taken up, or what else he did, he knew not.

Fortunately, at this juncture, one of his young gentlemen came into the room, who told him, after he recovered, that he desired him to shut the windows, and threw himself backward on the bed, where he lay a few minutes very quiet, after which, in a sort of frenzy, he started up and sat upon the side of it, made some efforts to vomit, but evacuated nothing; that he then threw himself back again, fell into strong convulsions, foamed at the mouth, shrieked with great violence, stared dreadfully at, and endeavoured to grasp and tear every thing around him. This outrageous fit was succeeded by a calm, something similar to fainting, during which time a relation was sent for, who came between 3 and 4 o'clock; when he spoke to him, he awaked, as he thought, from sleep, and knew him, though almost entirely insensible to every other object. Soon after, came Dr. Cullen, who had been sent for also; when he had felt his pulse, which beat 100 in a minute, he ordered him to be blooded; but as it is probable that natural antipathies will remain when every other sensation is nearly lost, Mr. H. obstinately refused to undergo this operation, on account of an insuperable aversion he had to it. All this time, no person knew any thing of his having taken the camphire; nor did he recollect any thing of it himself; and though he was recovered so much from the fit he had just described, as to know every one about him, he neither knew where he was, nor what he did.

As he felt a very uncommon sensation of heat, he got violently out of the bed, and threw himself on the floor, the coolness of which was very agreeable to him; on which some cold water was brought, and his hands and face bathed in it; this proved still more agreeable, and in some degree quieted a tremor which had seized on every part of his body. At this time, Dr. Monro, junior, who had also been sent for, came to his assistance. As he could give him no account of the cause of his illness; while he was walking through the room, he accidentally cast his eyes on a paper he had left on the table, containing the relation of his having taken the camphire, and its effects upon him, so long as he had been able to mark them. On this discovery, he immediately ordered him warm water; of which having drank pretty plentifully, he soon vomited, and, though more than 3 hours had passed, since he had taken the camphire, a great deal of it was evacuated in an undissolved state.

While he was holding his head over the basin into which he was vomiting, the smell of the camphire arose very strong, and first made him recollect that he had taken it, though he could give no distinct account of the time when, or manner how. He now, by the doctor's order, drank the juice of 2 or 3 lemons and oranges, but was not sensible of any benefit from them. He mentioned before, that he had not only lost all remembrance of his past actions, but also the knowledge of almost every present object; bu the now began, in some degree, to recover both; though in a manner so extraordinary, as he could not possibly describe, so as to give a clear idea of it. Among the first things he recollected, was, that he had that day visited several patients; but he could neither discover their diseases, names, number, nor any other circumstance relating to them. He could likewise recollect, that he had formerly known a great many things, of which he was become entirely ignorant, but could not fall on any method of recovering that knowledge which he had lost. A person who has lost his senses by liquor, as soon as he recovers, is perfectly well acquainted with every thing he knew before: but the case was very different with him, for the furniture of his room, and almost every other object on which he cast his eyes, appeared as strange, and new to him, as if he had only that moment begun his existence; and though he could remember the name of any thing when he looked at it, yet it was not without investigating its nature, that he could discover its use.

He had been put to bed when he vomited, and he knew not whether it was owing to it, or the camphire, but he had now a severe head-ach which disturbed him not a little all the evening. Between 5 and 6 o'clock he arose, and drank a bowl of tea, and the diluted juice of some more lemons and oranges. The giddiness in his head, singing in his ears, excessive heat and tremor, which he had felt so severely before, were now considerably abated, though far from being entirely gone off. About 7 o'clock, he had another visit from Dr. Monro, who, on numbering his pulsations, found they were now reduced from 100 to 80; in a minute after this, the thermometer was applied to his stomach, and in half an hour the mercury rose 2 degrees above blood-warm; it was then removed from his stomach to the doctor's, and the mercury fell more than 1 degree.

Between 8 and 9 o'clock, though he was considerably better, he still felt an uneasiness of body, and a confusion of mind, which it is impossible to describe; on account of which, he went to bed, and very soon fell into a calm and soft repose, which continued, without any interruption, till next morning. When he awaked, he found his head-ach quite gone, though a small degree of the confusion in it still remained. On going to stool that morning, he was extremely costive, though he had not been so before, nor continued to be so after. All that day he felt a great soreness, and rigidity over his whole body, as if he had caught cold, or undergone some severe exercise; the next day he was something better, and the day following quite recovered.

As the foregoing experiments had not fully satisfied him, whether camphire acted as a heater or cooler on the body, he resolved to try if it would give any additional heat or cold to fluids, in which it was dissolved; but, after repeated trials, he found that it never altered the natural heat of spirits, or oils, in whatever degree they were impregnated with it.

The first dose he took was a moderate one, and appeared to have acted as a cooler; but the next, if there is any trusting to the sensations occasioned by it, or to the increased celerity of the blood, certainly must have heated to a very great degree.

VII. Of a very Remarkable Aquatic Insect, found in a Ditch of standing Water near Norwich, in the Spring of* 1762. *By Edward King, Esq., F. R. S.* p. 72.

Plate 10, fig. A is the female, and B the male, both represented on their backs, in the posture in which they usually swim; a, a, are a number of smal transparent, fringed, fins, placed parallel, and contiguous to each other. They are almost always in a waving motion, and the animalcules seem to keep themselves suspended at different heights in the water, by means of them; for on their ceasing to move they sink to the bottom: d is one of those fins belonging to the female seen in front, and h is one of those belonging to the male, in which there is a very remarkable difference: c is the head of the female; and g the head of the male; distinguished by three projecting substances like horns or tusks, which are marked k in figure B; one of the long ones on the side is drawn separate at e, and the crooked one in the middle at f; this last probably serves as a kind of trunk, and the former may be of service to catch their prey, whatever it is; i is a very singular projecting substance in the male, and may perhaps contain the parts of generation; and b is the ovarium of the female, in which, it being quite transparent, the ova or spawn are very visible, and may be seen from time to time to change their places, and to have a kind of circulation.

C is a view of the female, placed on its back, in order to show the position of the fins, and their appearance when one looks down upon the insect; and D is the male, placed with its back uppermost, in the posture in which he sometimes lies still at the bottom of the water. Lastly, b is the tail magnified in a microscope, showing the hairs which grow on both sides out of it; but as the animalcule did not lie still long enough in the water, he could not view it with a glass so exactly as he wished to do, and therefore is not sure of the accuracy of the drawing of this part; all the other parts he has drawn as carefully as he was able, and they are about the natural size.

In these insects, besides their form, several particulars are very remarkable 1. Their bodies are entirely transparent, and mostly of a yellowish hue, except towards the tail, and part of the ovarium, where the colour is reddish; and, through a long vessel, which reaches almost the whole way from the head to the tail, somewhat of a circulation, by fits and starts, is very visible, even to the naked eye. 2. In the ovarium of the female, the ova, which are of a mixed colour in different parts, some brown, some yellow, and some red, are also in a

* The elegant animal here described is the *Cancer Stagnalis* of Linnæus. It has been amply described by Schoeffer and others. See vol. i. of the Transactions of the Linnæan Society, p. 103.

constant circular motion round the bag, or at least by a deception of sight they appear to be so. 3. They swim constantly on their backs, keeping themselves suspended by the vibrations of their numerous fins, and moving forwards, by giving a sudden spring with their tails; which latter circumstance is common to almost all aquatic insects.

In the ditch whence these were taken, there was a vast multitude of the same kind, though they have not been found in any other place that he knows of. From their being prolific in this state, he suspects it to be their only one, and that they are merely aquatic, and never turn to flies, as many insects found in water do: but then it seems very unaccountable how they came to be in such abundance in this ditch, and no where else, at least so as to be observed.

VIII. An Account of the very Tall Men, seen near the Straits of Magellan, 1764, by the Equipage of the Dolphin Man of War, under Commodore Byron. In a Letter from Mr. Charles Clarke of that Ship. p. 75.

Mr. Clarke describes these Patagonians as of a remarkably tall stature, and proportionally stout; the men from 8, to more than 9 feet high; and the women from $7\frac{1}{2}$ to 8 feet. But the description of these people may be read with more advantage in the account of Commodore Byron's voyage, afterwards published by authority.

IX. Account of a New-invented Instrument for Fractured Legs. By Mr. Wm. Sharpe, Surgeon to St. Bartholomew's Hospital. p. 80.

This account of an instrument for fractured legs, may be consulted in Mr. Sharpe's chirurgical works.

X. Of a Locked Jaw, and Paralysis, cured by Electricity. By Dr. Edward Spry, of Totness. p. 88.

Catharine Smellidge, of Ditford, a girl aged 18, of a strong healthy constitution, at the accidental death of a friend, took a great fright, and the next day (Easter-day, 1765) at his funeral, fell ill of very severe convulsive fits, which lasted, with slight intermissions, upwards of a month. From the first attack she never spoke, though otherwise sensible; soon after, her jaws became quite fixed, so that she was obliged to be fed with thin panada, and the like, strained between her teeth, being not able to have them opened but a very little way, even by a wedge made for that purpose. She became likewise paralytic from her hip downward, on the right side.

January 10, 1766, she consulted Dr. S., when he found her incapable of supporting herself without assistance, her leg and thigh of the right side very torpid with a loss of motion, and much more flaccid than the other, though not emaciated. She was incapable of uttering the least articulate sound, or even of having her teeth so far separated by the speculum oris, as to admit his little finger between them. The masseter and temporal muscles, from their contraction, felt vastly tense and rigid, being particularly painful on pressing them, or on endeavouring to open her mouth; the genio-hyoidei muscles appeared alike circumstanced, and the platysmamyoides on the right side very often greatly convulsed.

After every usual method judiciously administered by Mr. Guddrige of Brent, her surgeon, to little avail, Dr. S. had but small hopes from medicine; therefore recommended electricity; on which account, she, having no opportunity of its being done in the country, came to her lodgings, taken in town for that purpose, on January 15, when, she being somewhat inclined to be plethoric, and her menses not hitherto interrupted, he ordered 14 oz. of blood to be taken away, and the next day gave her a few slight electrical shocks on the leg of the diseased side; she immediately felt an agreeable sensation in it. This process was daily repeated, with a gradual increase of the vis electrica, sometimes plus, sometimes minus, electrifying her for 6 or 7 days, by which time she became much stronger, and capable of walking alone tolerably well.

Dr. S. now (she being, as to her jaw and speech, as at first) several times full-charged her with the electric matter, discharging it alternately from the masseters, her temples, and under the chin; immediately on her parting with which, she involuntarily shook her head, making her usual noise in endeavouring to speak. The next day he fixed the conductor round her temples and throat, and gave slight shocks, by touching sometimes her chin, othertimes her teeth or cheeks, with the communicant wire. This she disagreeably, though advantageously, felt, her jaws hereby admitting their being opened a little. The next day he increased the shocks considerably, by which, though she very discontentedly bore them, she became capable of opening her mouth to the width of an inch, and of articulating an imperfect, though with difficulty, an intelligible sound. The next day she very reluctantly received several smart shocks, and at last unexpectedly, the air being very electric, to such a degree, as to deprive her of her senses; and she remained for $\frac{1}{2}$ an hour strongly convulsed. The next day, after the first shock, she spoke so as to be tolerably well understood, telling us that the shocks were frequently vastly severe for her to bear; but that as she was fully sensible of the advantage she had already received by them, she would gladly submit, in hopes of a further advantage. She was even then incapable of bringing her tongue without her teeth, and of moving it without great difficulty, complaining it seemed very large and heavy. On inspecting her mouth, which she was able to open to almost its usual width, he discovered nothing particular, but an extraordinary turgescence, without induration, of the sublingual glands. After this she received about 20 shocks daily on her tongue, and other parts, for a fortnight, by which time all her complaints were removed, and she returned home quite well, and has remained so ever since.

N. B. In the first week's experiments, the shocks were confined between her hip and foot, of the right side; after that, on various parts, as judged requisite: her tongue at its tip became very red and tender, after the first electrization, its papillæ appearing very prominent; and its subjacent glands soon lessened their bulk, her mouth running greatly with saliva: her pulse, with a shock or two, generally quickened 12 or 14 times per minute. After she got tolerably well, immediately on having a smart electrical stroke, she frequently became, for some small time, as paralytic as ever on her right side; and sometimes had a return of her fits, the going off of which were attended with profuse sweats. Her blood appeared of a good texture, otherwise than giving off a little more than its due proportion of latex.

XI. Experiments on Rathbone-place Water. By the Hon. Henry Cavendish, F.R.S. p. 92.

Dr. Lucas has given a short examination of this water in the first part of his treatise on waters. It is the produce of a large spring at the end of Rathbone-place, and used a few years ago to be raised by an engine for supplying part of the town. The engine is now destroyed; but there is a pump, nearly in the same situation, which yields the same kind of water. It is the water of this pump which was used in these experiments.

Most waters, though ever so transparent, contain some calcarious earth; which is separated from them by boiling, and which seems to be dissolved in them without being neutralized by any acid, and may therefore not improperly be called their unneutralized earth. The following experiments were made chiefly with a view of inquiring into the cause of the suspension of this earth, for which purpose this water seemed well adapted; as it contains more unneutralized earth than most others. These experiments were made towards the latter end of September 1765, after a very dry summer; whence the water was most likely more impregnated with saline and other matters than it usually is.

The water, at the time of using it, looked rather foul to the eye. On exposing some of it for a few days to the open air, a scurf was formed on its surface, which was nothing else but some of the unneutralized earth separated from the water. On dropping into it a solution of corrosive sublimate, it became cloudy in a few seconds; it quickly became opaque, and let fall a sediment. This is a property, which he believes does not take place, in any considerable degree, in most of the London waters.

Exper. 1. 494 oz. of this water were distilled in a copper still, till about 150 oz. were drawn off. A good deal of earth was precipitated during the distillation, which being collected and dried, weighed 271 grs. It proved to be entirely a calcarious earth, except a small part, which was magnesia. This Mr. C. found in the following manner. A little of this earth, being mixed with spirit of salt, dissolved entirely; which shows it to consist solely of an absorbent earth, but does not show whether it is a calcarious earth or magnesia. The remainder was saturated with oil of vitriol: a great deal of matter remained undissolved, which, as the earth was shown to be entirely of the absorbent kind, must have been selenite, or a calcarious earth saturated with the oil of vitriol. The clear liquor strained from off the selenite, yielded on evaporation only 18 grs. of solid matter, which proved to be Epsom salt; so that all the earth, except that contained in the 18 grs. of Epsom salt, must have been of the calcarious kind. That contained in the Epsom salt is well known to be magnesia.

The water remaining after distillation, and from which the earth was separated, was evaporated, first in a silver pan, and afterwards in a glass cup, till it was reduced to about 3 oz. Not the least earth was precipitated during the evaporation, till it was reduced to a small quantity; there then fell 39 grs., which were entirely selenite: so that all the unneutralized earth in the water was separated during the distillation. The liquor thus evaporated was of a reddish colour, like an infusion of soot.

Many waters contain a good deal of neutral salt, composed of the nitrous acid united to a calcarious earth; the most convenient way of ascertaining the quantity of which, is to drop a solution of fixed alkali into the evaporated water, till all the earth is precipitated; by which this salt is changed into true nitre, and is capable of being crystallized. For this reason some fixed alkali was dropped into the evaporated water till it made no further precipitation. The earth thus precipitated weighed 36 grains, and was entirely magnesia. The liquor was then further evaporated, but no nitre could be made to shoot: being then evaporated to dryness, it weighed 256 grs. It gave not the least signs of containing any nitrous salt, either by putting some of it on lighted charcoal, or by making a match with a solution of it, but appeared to be a mixture of sea salt and vitriolated tartar, or some other salt composed of the vitriolic acid. As Mr. C. had heard of no other London water, that had been examined with this view, but what had been found to contain a considerable proportion of nitrous salt, it seemed very remarkable that this should be entirely destitute of it. He now proceeded to the experiments made on the distilled water.

The distilled water, especially that part of it which came over first, became opaque, and let fall a precipitate, on dropping into it a solution of sugar of lead. It also became opaque by the addition of corrosive sublimate, much in the same manner that the plain water did before distillation. It was found, by dropping into it a little acid of vitriol and committing it to evaporation, to contain a small quantity of volatile alkali; as it left 4 grs. of a brownish salt, which being re-dissolved in water, yielded a smell of volatile alkali on the addition of lime. It is doubtless this volatile alkali which is the cause of the precipitate, which the distilled water makes with sugar of lead and corrosive sublimate. What first suggested to Mr. C. that the distilled water contained a volatile alkali, was the distilling some of it over again in a retort; by which the first runnings were so much impregnated with volatile alkali, as to turn paper, died with the juice of blue flowers, to a green colour, and in some measure to yield a smell of volatile alkali.

In the foregoing experiment, the salt procured from the distilled water was perfectly neutral; so that the quantity of acid employed was certainly not more than sufficient to saturate the alkali, but it may very likely have been less; as in that case the superfluous volatile alkali would have flown off in the evaporation. The following experiment shows pretty nearly the quantity of volatile alkali in the distilled water.

Exper. 2. 1128 oz. of Rathbone-place water were distilled in the same manner as the former. The distilled water was divided into 2 parcels, that parcel which came over first weighing 121 oz., the other 146. A preparatory experiment was first made, in order to form a judgment of the comparative strength of each parcel, and also of the quantity of acid which it would require to saturate them. This was done by dropping sugar of lead into each parcel till it ceased to make a precipitate. It was judged from hence that the first parcel contained about 2½ times as much volatile alkali as an equal quantity of the second. Into 30 oz. of the first parcel, mixed with as much of the second, was then put 43 grs. of oil of vitriol, which was supposed to be about ⅛ more than sufficient to saturate the alkali in it. The mixture was then evaporated. When reduced to a small quantity, it was found to be rather acid: 16 grs. of volatile sal ammoniac were therefore added, which seemed nearly sufficient to neutralize it. Being then evaporated to dryness, it left 66 grs. of a brownish salt, which dissolved readily in water, leaving only a trifling quantity of brown sediment. A little of this salt was found to make no precipitate on the addition of fixed alkali, and the remainder, being boiled with lime, was converted into selenite; a sure sign that the salt was merely vitriolic ammoniacal salt. The volatile alkaline salt contained in 66 grs. of vitriolic ammoniacal salt, is 58½ grs.; from which deducting 16 grs., the weight of the volatile sal ammoniac added, it appears that the distilled water used in this experiment, contains 42½ grs. of volatile salt; and therefore the whole quantity of volatile salt driven over by distillation, seems to be about 68 grs., which, as the 2d parcel was so much weaker than the first, is probably nearly the whole volatile alkali contained in the water.

Exper. 3. Dr. Brownrig, in a paper printed in the Phil. Trans. for 1765, shows that a great deal of fixed air is contained in Spa water. This induced Mr. C. to try whether he could find any in that of Rathbone-place; which he did by means of the contrivance represented in fig. 1, pl. 10.

Here ACDE represents a tin pan, filled with Rathbone-place water as high as BG. HKL is another tin pan, within the first, in the manner of an inverted funnel, and made in such a manner, as to leave as little room as possible between that and the sides of the outer vessel. M represents a bottle, full of the same water, inverted over the mouth of the funnel. By this means, as fast as the air is disengaged by heat from the water within the funnel, it must necessarily rise up into the bottle. The Rathbone-place water put into the vessel, weighed 411 oz., the funnel held 353 oz. A bottle full of water being inverted over the mouth of the funnel, as in the figure, the water was heated, and kept boiling about $\frac{1}{4}$ of an hour. As soon as one bottle was filled with air, it was removed by putting a small ladle under its mouth, while under water, and set with its mouth immersed in the same manner in another vessel of water, taking care not to suffer any communication between the included air and the outward air during the removal. At the same time, another bottle full of water was inverted over the mouth of the funnel, in the same manner as the former. It was not easy telling how much air was discharged from the water; as the air in the bottles, when first removed, was hot and expanded; and before he could be sure it was cold, there was some of it absorbed by the water: but there seemed to be above 75 oz. measures discharged, scarcely 20 of which arose before the water began to boil. The water continued discharging air after the experiment was discontinued. In about a day's time, much the greatest part of the air was absorbed, scarcely 16 oz. measures remaining. That which was absorbed appeared to be fixed air, as the water which had absorbed it, made a precipitate with lime-water. But in order to absorb all the fixed air more perfectly, the air which remained not absorbed was transferred into another bottle of water, in the manner described in Mr. C.'s first paper on factitious air, p. 298, &c. of this volume. This bottle was then set with its mouth immersed in a bottle of soap-leys; after which, by shaking the bottle, the soap-leys was mixed with the included water; by which the air in the bottle was brought in contact with the soap-leys, which is well known to absorb fixed air very readily. By this means the air was reduced to $8\frac{3}{4}$ oz. measures. A small phial being filled with equal quantities of this and inflammable air, and a piece of lighted paper applied to its mouth, it went off with as loud a bounce, as when the same phial was filled with equal quantities of common air and inflammable air. The specific gravity of the remainder was tried by a bladder, in the manner described in the above-mentioned paper: as well as could be judged from so small a quantity, it was just the same as that of common air. From these 2 circumstances, Mr. C. thinks we may fairly conclude that this unabsorbed part was entirely common air; consequently the air discharged from the Rathbone-place water consisted of 8¾ oz. of common air, and about 66 of fixed air. The air which was discharged before the water began to boil, contained much more common air, than that which was discharged afterwards; that which was discharged towards the latter end seeming to contain scarcely any but fixed air.

As so much fixed air is discharged from this water by boiling, it seemed reasonable to suppose, that the distilled water should contain fixed air. He accordingly found it to make a precipitate with lime-water.

Exper. 4.—The following experiment shows that the fixed air was not generated during the boiling, but was contained in the water before. Into 30 oz. of Rathbone-place water was poured some lime water, which immediately made a precipitate. More lime-water was added, till it ceased to make any further precipitate. It required 20¼ oz. The precipitated earth being dried, weighed 39 grs.

The unneutralized earth contained in 30 oz. of Rathbone-place water is 16½ grs., and the earth contained in 20¼ oz. of lime-water (as was found by precipitating the earth by volatile sal ammoniac) is 21 grs. Therefore the earth precipitated from the mixture of Rathbone-place water, and lime-water, is about equal to the sum of the weights of the earth contained in the lime-water, and of the unneutralized earth in the Rathbone-place water; and consequently all the unneutralized earth seems to be precipitated from Rathbone-place water by the addition of a proper quantity of lime-water. But a more convincing proof that this is the case, is that the clear liquor, after the precipitate had subsided, did not deposit any earth on boiling, or become in the least cloudy on the addition of fixed alkali; whereas Rathbone-place water in its natural state becomes opaque by it. It might perhaps be expected, that the clear liquor should still make a precipitate on the addition of fixed alkali, though the unneutralized earth is precipitated; as in all probability there is still a good deal of earth remaining in it in a neutralized state. The reason why it does not, seems to be, that the remaining earth is most likely entirely magnesia; and Epsom salt, when dissolved in a great quantity of water, does not make any precipitate on the addition of fixed alkali.

There is great reason to suppose that the earth, precipitated on mixing the Rathbone-place water and lime-water, was very nearly saturated with fixed air, i. e. that it contained very near as much fixed air as is naturally contained in the same quantity of calcarious earth. If so, 30 oz. of Rathbone-place water contain as much fixed air as 39 grs. of calcarious earth; whereas the unneutralized earth, in that quantity of water, is only 16½ grs. so that Rathbone-place water contains near 2⅓ times as much fixed air as is sufficient to saturate the unneutralized earth in it. It seems likely from hence, that the suspension of the earth in the Rathbone-place water, is owing merely to its being united to more than its natural proportion of fixed air, as Mr. C. has showed that this earth is actually united to more than double its natural proportion of fixed air, and also that it is immediately precipitated, either by driving off the superfluous fixed air by heat, or absorbing it by the addition of a proper quantity of lime-water.

Calcarious earths, in their natural state, i. e. saturated with fixed air, are totally insoluble in water; but the same earths, entirely deprived of their fixed air, i. e. converted into lime, are in some measure soluble in it; for lime-water is nothing more than a solution of a small quantity of lime in water. It is very remarkable therefore, that calcarious earths should also be rendered soluble in water, by furnishing them with more than their natural proportion of fixed air, i. e. that they should be rendered soluble, both by depriving them of their fixed air, and by furnishing them with more than their natural quantity of it. Yet, strange as this may appear, the following experiments, he thinks, show plainly that it is the real case.

Exper. 5.—In order to see whether he could suspend a calcarious earth in water, by furnishing it with more than its natural proportion of fixed air, Mr. C. took 30 oz. of rain water, and divided it into 2 parts: into 1 part he put as much spirit of salt as would dissolve $30\frac{8}{10}$ grs. of calcarious earth, and as much of a saturated solution of chalk, in spirit of salt, as contained 20 grs. of calcarious earth: in the other part he put as much fixed alkali, as was equivalent to $46\frac{8}{10}$ grs. of calcarious earth, i. e. which would saturate as much acid. This alkali was known to contain as much fixed air as 39 grs. of calcarious earth. The whole was then mixed together, and the bottle immediately stopped. The alkali was before said to be equivalent to $46\frac{8}{10}$ grs. of calcarious earth, and was therefore sufficient to saturate all the spirit of salt, and also to decompound as much of the solution of chalk as contains $16\frac{1}{2}$ grs. of earth. This mixture therefore, supposing he made no mistake in his calculation, contained $16\frac{1}{2}$ grs. of unneutralized earth, with as much fixed air as is contained in 39 grs. of calcarious earth; which is the quantity which was found to be in the same quantity of Rathbone-place water. The mixture became turbid on first mixing, but the earth was quickly re-dissolved on shaking, so that the liquor became almost transparent. After standing some time, a slight sediment fell to the bottom, leaving the liquor perfectly transparent. The mixture was kept 3 or 4 days stopped up, during which time it remained perfectly clear, without depositing any more sediment. The clear liquor was then poured off, from the sediment, and boiled for a few minutes, in a Florence flask; it became turbid before it began to boil, and discharged a good deal of air; some earth was precipitated during boiling, which being dried weighed 13 grs. This shows that there was really, at least, 13 grs. of earth suspended in this mixture, without being neutralized by any acid; the suspicion of which could be owing only to its being united to more than its natural proportion of fixed air. But as a further proof of this, Mr. C. made the following experiment.

Exper. 6.—He took the same quantities of rain water, solution of chalk, spirit of salt, and fixed alkali, as in the last experiment, but mixed them in a different order. The fixed alkali was first dropped into the spirit of salt, and when the effervescence was over, was diluted with $\frac{1}{2}$ the rain water. The solution of chalk was then diluted with the remainder of the rain water, the whole mixed together, and the bottle immediately stopped, and shaken vehemently. A precipitate was immediately formed on mixing, which could not be re-dissolved on shaking.

It must be observed, that in the first of the 2 foregoing experiments, all the fixed air contained in the alkali was retained in the mixture, none being lost by effervescence; whereas, in the last experiment, the greatest part of the fixed air was dissipated in the effervescence; no more being retained than what was contained in that portion of the fixed alkali, which was not neutralized by the acid; and consequently the unneutralized earth, in the mixture, contained not much more fixed air than what was sufficient to saturate it. As the latter of these mixtures differed no otherwise from the former, than that it contained less fixed air, the suspension of the earth in the former must necessarily be owing to the fixed air.

In the 2 foregoing experiments the water contained, besides the unneutralized earth and fixed air, some sal sylvii, and a little solution of chalk in the marine acid; which, it may be supposed, contributed to the suspension of the earth: but the following experiment shows that a calcarious earth may be suspended in water, without the addition of any other substance than fixed air.

Exper. 7.—A bottle full of rain water was inverted into a vessel of rain water, and some fixed air forced up into the bottle, at different times, till the water had absorbed as much fixed air as it would readily do; 11 oz. of this water were mixed with $6\frac{1}{4}$ of lime water. The mixture became turbid on first mixing, but quickly recovered its transparency, on shaking, and has remained so for upwards of a year. This mixture contains 7 grs. of calcarious earth; and, from a subsequent experiment, he guessed it to contain as much fixed air as there is in 14 grs. of calcarious earth.

Exper. 8.—Lest it should be supposed, that the reason why the earth was not precipitated in the foregoing experiment, was, that it was not furnished with a sufficient quantity of fixed air, the following mixture was made, which contains the same proportion of earth as the former, but a less proportion of fixed air: $4\frac{3}{4}$ oz. of the above mentioned water, containing fixed air, were diluted with $6\frac{1}{4}$

of rain water, and then mixed with 6½ ounces of lime-water. A precipitate was immediately made on mixing, which could not be re-dissolved on shaking.

Exper. 9.—Mr. C. made some experiments to find whether the unneutralized earth could be precipitated from other London waters, by the addition of lime-water, as well as from Rathbone-place water. It is necessary for this purpose, that the quantity of lime-water should be adjusted very exactly; for, if it is too little, it does not precipitate all the unneutralized earth; if it is too great, some of the earth in the lime-water remains suspended. For this reason, as he found it almost impossible to adjust the quantity with sufficient exactness, he added such a quantity of lime-water, as he was well assured was more than sufficient to precipitate the whole of the unneutralized earth; and when the precipitate was subsided, decanted off the clear liquor, and exposed it to the open air, till all the lime remaining in the water was precipitated, by attracting fixed air from the atmosphere. The clear liquor was then decanted and evaporated, which is much the most exact way he knows, of seeing whether any unneutralized earth remains suspended in the water. The result of the experiments was as follows:—200 oz. of water, from a pump in Marlborough-street, were mixed with 38 oz. of lime-water. The earth precipitated, weighed 38 grs. The clear liquor, exposed to the air, and evaporated in a silver pan till it was reduced to 6 or 7 oz. deposited no more than 2 or 3 grs. of unneutralized earth. A like quantity of the same pump water, evaporated by itself, without the addition of lime water, deposited about 19 grs. of unneutralized earth.

200 oz. of water, from a pump in Hanover-square, being mixed with 67 oz. of lime water, the precipitate weighed 93 grs. The clear liquor, treated in the same way as the former, deposited about 2 grs. of earth. 200 oz. of the same water, evaporated by itself, deposited 28 grs. of earth.

The same quantity of water from a pump in St. Martin's church-yard, being mixed with 82 oz. of lime-water, the precipitate weighed 108 grs. The clear liquor deposited scarcely any unneutralized earth on evaporation. The same quantity of water, evaporated by itself, yielded 45 grs. of unneutralized earth.

The way by which he found the quantity of unneutralized earth deposited on evaporation, was, after having decanted the clear liquor, and washed the residuum with rain water, to pour a little spirit of salt into the silver pan; which dissolves all the calcarious earth, but does not corrode the silver. Then, having separated the solution from the insoluble matter, the earth was precipitated by fixed alkali. In this way of finding the quantity of unneutralized earth, care must be taken to add very little more acid than is necessary to dissolve the unneutralized earth, and to use as little water in washing out the solution as possible; for otherwise a good deal of the selenite, which is deposited in the evaporation of most water, will be dissolved; the earth of which will be precipitated by the fixed alkali, and by that means make the quantity of unneutralized earth appear greater than it really is.

It appears from these experiments, that the unneutralized earth is entirely precipitated from these 3 waters, by the addition of a proper quantity of lime-water; as the trifling quantity found to be deposited, on the evaporation of 2 of them, most likely proceeded only from not exposing the water to the air long enough for all the lime to be precipitated. So that he thinks it seems reasonable to conclude, that the unneutralized earth, in all waters, is suspended merely by being united to more than its natural proportion of fixed air.

To return to Rathbone-place water; it appears from the foregoing experiments, that 1 pint of it, or 7315 grs. contains, 1st, as much volatile alkali, as is equivalent to $\frac{9}{10}$ grains of volatile sal ammoniac: 2dly, $8\frac{4}{10}$ grs. of unneutralized earth, a very small part of which is magnesia, the rest a calcarious earth: 3dly, as much fixed air, including that in the unneutralized earth, as is contained in $19\frac{6}{10}$ grs. of calcarious earth: 4thly, $1\frac{8}{10}$ of selenite: 5thly, $7\frac{9}{10}$ of a mixture of sea salt and Epsom salt; and the whole solid contents of 1 pint of the water is $17\frac{1}{2}$ grs.

One pint of water, from the pump in Marlborough-street, contains $1\frac{4}{10}$ grs. of unneutralized earth, and as much fixed air as is contained in $2\frac{9}{10}$ grs. of calcarious earth.

The same quantity of water, from the pump in Hanover-square, contains $2\frac{1}{10}$ grs. of unneutralized earth, with as much fixed air as is contained in $7\frac{2}{10}$ of earth.

The same quantity of water, from St. Martin's church-yard, contains $3\frac{4}{10}$ grs. of unneutralized earth, with as much fixed air is contained in $8\frac{8}{10}$ of earth.

XII. Of a Meteor seen at Oxford, Oct. 12, 1766. By the Rev. John Swinton, B. D., F. R. S. p. 108.

The Rev. Dr. Sharp coming into Christ-church common-room out of the great quadrangle, Oct. 12, 1766, about 8^h 30^m P. M. informed the company there, that he had seen some remarkable Auroræ Boreales a few minutes before. But, as such phenomena are common enough here, they gave little attention to the information. Being on the terrace, about 8^h 45^m P. M. Mr. S. discovered a broad luminous arch, in the northern part of the hemisphere, extending from E. to W. almost terminated by the horizon, and somewhat less than a semicircle. The upper or exterior limb of this arch, with a certain portion of the lucid adjoining tract, was white and resplendent; but the brightness gradually decreased as it approached the lower or interior limb, which was fuscous and obscure, that it seemed scarcely distinguishable from the clouds that were contiguous to it.

For about five minutes the lustre remained pretty strong and vivid, and the meteor without any visible change or variation; but, after the expiration of that short term, the arch began to grow faint, and in one or two minutes more totally disappeared.

How long this meteor had been formed, when Mr. S. first observed it, he cannot say; but he believed it was then, and perhaps for some time had been, on the decline. The crepusculum, or illustration of the atmosphere, which sometimes precedes such meteors as this, and even continues long after their extinction, might perhaps have remained till 10 or 11 o'clock; which if we admit, this crepusculum may not improbably be considered as the same phenomenon with "a surprising bright luminous appearance visible at London in the hemisphere from the E. to the W. about 10 o'clock, which lasted about an hour," the same night, or at least as something similar to it.

The singularity of this meteor was fixed by the gradual and regular diminution of its resplendency between the upper and lower limbs, an instance of which he never observed before. This continued from the time he first discovered the arch almost to the very moment of its extinction. The limbs of the zone forming this arch were however very well defined; insomuch that the regularity of its figure, by the gradual decrease of brightness, was not in the least impaired.

XIII. Some Observations on Swarms of Gnats, particularly one seen at Oxford, August 20, 1766. *By the Rev. John Swinton, B. D., F. R. S.* p. 111.

The gnats have been more numerous, as well as more noxious, here, (Oxford) during the months of July, Aug. and Sept. 1766, than perhaps they were ever known to be before in the memory of man. So many miriads of them have sometimes occupied the same part of the atmosphere, in contiguous bodies, that they have resembled a very black cloud, greatly darkened the air, and almost totally intercepted the solar rays. The repeated bites likewise of these malignant insects have been so severe, that the legs, arms, heads, and other parts, affected by them, in many persons, have been swelled to an enormous size. The colour also of these parts, at the same time, was red and fiery, perfectly similar to that of some of the most alarming inflammations.

Mr. S. takes notice of one very remarkable property of these little mischievous animals, which lately presented itself to his view, and which has not yet perhaps been duly attended to by any naturalist. Being in the garden belonging to the fellows of Wadham College, August 20, 1766, about half an hour before sun-set, such an immense number of gnats filled the atmosphere, as he had never seen before. He then observed 6 columns, formed intirely of these insects, ascending from the tops of 6 boughs of an apple-tree, in another garden, separated from that he was in by a partition-wall, to the height of at least 50 or 60 feet. Two of these columns seemed perfectly erect and perpendicular, 3 of them oblique, and one approached somewhat towards a pyramidal form.

XIV. A Description of the Andrachne, with its Botanical Characters. By G. D. Ehret, F. R. S.* p. 114.

From a short and crooked stem go off irregularly several branches bending in various directions; but the younger shoots mostly pointing upwards. The height of the shrub is now about 4 feet. The stem and branches are of different colours at different seasons. In the spring, they appear of a greenish cinnamon colour: this is gradually heightened to almost a red during winter; towards the end of which, the epidermis peels off, and the new bark exhibits the like appearance as it had the spring before. On the extremities of these branches, the shoots of the preceding year, which are of a deep red colour, are many leaves of different sizes, placed irregularly; the largest leaves were in length, when the figure was drawn, about 4 inches, and 2 inches and a half in breadth, of an oval figure: they are mostly entire, though the edges of some are lightly serrated: their surface is smooth and lively, but not glossy or shining. They are supported on the branches by footstalks about an inch long, of a red colour and smooth. The young leaves, at their first appearance, are of a faintish green with a cast of yellow yet beautifully shaded with red: their footstalks and middle rib are then hoary, but they lose this appearance as they grow older.

This very rare shrub produced its flowers, for the first time in England, in the garden of Dr. John Fothergill, at Upton near Stratford in Essex, May 1766. The principal spikes of flowers in this species of arbutus are erect, producing many side ones in a horizontal direction, their extremities inclining downwards. Each of these simple ramifications contain many white globular flowers, hanging on long hoary glutinous pedunculi, which are situated alternately. These spikes of flowers, forming a kind of loose tuft with the bright bunches of leaves, have an elegant appearance.

It seems worthy of observation, that the plants raised by the gardeners by grafting or inarching the andrachne on the common arbutus, which is the method chiefly used in propagating this elegant shrub, differ considerably from the plants raised from seed, particularly in this, that the young branches, and the footstalks of the leaves, are very hairy, and the leaves themselves are all without exception deeply serrated like the arbutus. Dr. Russell also says, that the outer bark of the old stem and branches abroad, are for some months of the year of as beautiful a crimson, as the young shoots are here described to be, and doubts not but it will be so in this country, as the shrub grows older.

* *Arbutus Andrachne.* Linn.

XV. History of a Fœtus born with a very imperfect Brain; to which is subjoined a Supplement to the Essay on the Use of Ganglions, published in Philos. Trans. for 1764.* *By James Johnstone, M. D.* p. 118.

October 27, 1765, a monstrous birth was brought by a midwife. It was a female child, come to its full time, in which the whole skull, excepting its basis, was wanting: this was covered with something which had the appearance of red flesh. He found it to consist of different membranes; and in a small depression, in a back part of the basis of the skull, lay the brain, such as it was, not exceeding the size of the kernel of a filbert nut, flaccid and membranous. He could not have positively pronounced it brain, had he not traced its continuation into spinal marrow, down the channel of the vertebræ. The eyes were perfect and sound. The optic nerve of one eye he examined, though not large enough, yet in thickness was almost equal to one-third of the spinal marrow, which was too small likewise.

On opening the breast and abdomen, all the organs there seemed in structure perfect, properly situated, and full grown. The heart in particular was plump and strong. This infant had not breathed, its lungs, which were perfect, sunk in water: yet the mother and midwife felt it active and strong just before delivery. This child had tongue, nostrils, eyes, and ears, and every other part, excepting the brain, perfect and plump, as in the healthiest infants come to their full time.

Many births similar to this, in most circumstances, are recorded in the Transactions of the R. S. No. 99, 226, 228, 242. First, such of them as were born alive, died soon after birth, though lively and strong in the womb, and perfect in all parts, the brain and skull excepted. 2. In that of which an account is given by Dr. Preston (Philos. Trans. No. 226), the celebrated anatomist Mons. du Verney traced the 8th and 9th pairs, the medulla spinalis, and the intercostals. The child was well proportioned, the cranium, brain, and cerebellum were wanting; instead there remained only a substance, like congealed blood, covered with a membrane. 3. In a case related, and largely commented on, by the celebrated Wepfer, which differs in many respects from other children said to be without brains; the child was well-proportioned, its head of the usual size, but its brain had degenerated into vesicles, or hydatides, each of which had its blood vessel (might one from thence infer the natural state of the cortical substance of the brain to be cellular?) and the optic and auditory nerves took their rise from 3 portions of medullary substance lying on the sphænoid bone near the sella equina. 4. These singular existences afford inferences, and show that the irritability of the heart is capable of being sustained by very low degrees of the nervous power,

* Page 122 of this *abridged* volume.

while that irritability is kept up by the fostering heat of the mother. This feeble life is soon extinguished, when the influences of the mother's warmth and circulation cease (No. 1.) Such infants die as soon as born, or soon after. 5. Such examples, more consequentially than experiments, demonstrate that the spinal marrow is the principal origin of the intercostal nerves (No. 2); and better than ligatures illustrate their vast importance; for, 6. From the plump state of the body, and vigorous appearance of the heart, it is evident that the circulation, and the developement of the several organs, had been carried on properly in the fœtus; and that the irritability of the heart derived a sufficiency of nervous influence from the intercostal nerves, and its ganglions, and these again from the spinal marrow, for growth, and that state of existence.

Then follows the Supplement to this Author's Essay on the Use of the Ganglions, which may be seen in his collected works, published in 1795.

XVI. Thoughts on Comets. By Mr. John Winthrop, Profes. of Math. and Philos. at Cambridge, in New England. From the Latin. p. 132.

The scope of the following problems, is to find the limit of attraction between the sun and a comet, or the point between them where a corpuscle will be equally attracted by them; and also other matters depending on it.

Prob. 1.—Given the quantity of matter in two bodies, and the distance between their centres; to find the limit of attraction.

In fig. 2, pl. 10, let S and C be the centres of the bodies, the greater being S; and let their quantities of matter be called S and C respectively. Cut the line SC, produced beyond the less body C, in A and O, so that SA may be to AC, and so to OC, in the sub-duplicate ratio of S to C; and on the diameter OA, describe the semicircle OLA; then the limit of attraction will be the spherical superficies generated by the rotation of the semicircle OLA about the axis OA. This Mr. W. easily proves by the nature of the circle, since SL to LC, and SA to AC, and so to OC, are all in the same ratio.

Schol. 1. Within this limiting superficies, the force of the less body is greater: without it, less.

Corol. 1. The diameter AO of the limiting sphere, as also its two segments AC, CO, are as the distance between the centres of the bodies.—*Corol.* 2. Having given any point in the limiting superficies, and the distance of the bodies, the whole superficies will be given.—*Corol.* 3. From every point in this superficies, the direction of gravitation is to the point A, as to a centre. For because of the equality of the forces, by which the corpuscle at L is attracted towards the bodies S and C, the direction of the force composed of them bisects the angle SLC; consequently it passes through the point A by Eucl. 3, 3.—*Corol.* 4. And, drawing CB perpendicular to CL, meeting LA in B, then that compound force will be reciprocally as the rectangle CLB. For, drawing CD perpendicular to LA, the simple force towards C will be to the compound force towards A, as CL to 2LD, that is, as BL to 2CL. Hence, since the simple force is as $\frac{1}{CL^2}$, the compound force will be as $\frac{2CL}{CL^2 \times LB}$, or as $\frac{1}{CL \times LB}$.

Schol. 2. If the two bodies were equal, the limit of attraction would be an infinite plane, bisecting perpendicularly the distance of the two bodies.—*Schol.* 3. Putting the distance SC $= d$, the semidiameter of the greater body S $= b$, and that of the less C $= k$; if then it were as the distance $d : k :: \sqrt{s} + \sqrt{c} : \sqrt{c}$, the point A would touch the surface of the body C. And the same would happen to the point O if, diminishing the distance a little, it be $d : k :: \sqrt{s} - \sqrt{c} : \sqrt{c}$. But if the distance d were less, the problem would be impossible.

PROB. 2. The same things being supposed; to find the locus in which the forces of the bodies may be to each other in a given ratio.

Let the given ratio be that of h to c, viz. of the force of the greater body to that of the less. Cut SC produced in E and P, fig. 3, so that SE be to EC, and SP to PC, in the subduplicate ratio of s to h: then the required locus will be the superficies of the sphere PFE, described on the diameter PE. Which is easily demonstrated as prob. 1.

Corol. 1. If CS be cut in G, so as that CG be to GS, as $\frac{h}{c} \times$ CE to ES; then the point G will be the centre to which is directed the gravitation compounded in the superficies PFE. This is easily proved by joining FS, FG, FC, and drawing GK parallel to SF.—*Corol.* 2. And if in the diagonal FG there be taken FH = FC, and HM be drawn parallel to SF, the compound force in the point F will be reciprocally as the rectangle CFM. This is proved as cor. 4, prob. 1. And the same things are to be understood of the interior surface *pfe*, and the points *g*, *k*, *h*, *m*, in fig. 4.—*Corol.* 3. Where h is less than c, the centre g will be within the surface *pfe*, as in fig. 4. Where greater, the centre G will be without the surface PFE; and that the farther from the body C, the greater the given ratio is.

Schol. If the given ratio be the same as s to c, the spherical surface PFE will change into a plane, as in schol. 2, prob. 1, the point P going off infinitely. If the ratio were greater, the point P would fall on the contrary side of the centre S; and the surface would again be spherical, and excentric of the greater body; and its diameter would be found as above; but if the ratio should be less than $(b + d)^2 \times s$ to b^2c, or greater than k^2s to $(k + d)^2 \times c$, the problem would be impossible.

PROB. 3. To describe the motion generated by the conjunct forces of the corpuscles of the attracting bodies S and C.

If the bodies F and C be included in a fluid medium, in which there are immerged corpuscles specifically lighter or heavier than the medium, the corpuscles will accordingly ascend or descend, by the attraction of each body, as if they were attracted to one body; and they will be moved either in right lines or curves, according as their motions are direct or oblique, in respect of the compound centre of gravitation. For this centre is equivalent to the centre of one body placed in the same point. The author then considers 2 cases: the 1st case when the corpuscles situated in a right line CS between the bodies C and S, are lighter than the ambient medium; there they will tend to the point A, fig. 2. Case 2d. All the lighter corpuscles, arising from the body C, (those excepted which are posited in the line of the syzygies PS) ascend in curves, not much unlike the dotted curve lines in fig. 5.

PROB. 4. From what is said above to derive the principal phenomena of comets' tails, on the theory of Newton.

The author here extracts the account of the formation of comets' tails from Newton's Principia, p. 511, 3d edition. From which he deduces the following corollaries.

Corol. 1. The tails of comets must be in a direction opposite to the sun.—*Corol.* 2. The tails must dilate from the lower extremity to the higher.—*Corol.* 3. And the tails must be longest in the vicinity of the sun.—*Corol.* 4. Hence those comets have the longest tails that approach nearest the sun.

Schol. From the law of centrifugal force, and what is said above, the figure of a comet's tail may be determined as it were geometrically, being perhaps a kind of paraboloid, after the manner as shown in fig. 6. Part of the tail may be attracted by the planetary spheres, and become attached to them like an atmosphere, or mixing with it. But the greater part may go off to vast distances to cool other comets.

PROB. 5. To find, by observation, the limit of attraction between the sun and a given comet.

Observe the breadth of the comet, CA in fig. 6, estimated from the centre of the comet, in the part opposite to the tail; and hence, together with the comet's distance, from the sun and from the earth, which are given from the Newtonian theory of gravity, will be obtained the point A, in figs. 2, 5, 6, either accurately, or at least nearly. Hence, by cor. 2, prob. 1, the whole limit will be given.

Corol. 1. The breadth of the coma in the part next the sun, is as the comet's distance from the sun.—*Corol.* 2. Hence the quantity of matter in the comet becomes known. For since SC and CA are given, their difference SA is given; and hence the ratio SA to AC, as also the duplicate ratio are given; which, by the solution of prob. 1, is also the ratio of the matter in the sun to the matter in the comet.—*Corol.* 3. Hence, and from the observed diameter, there will be also known the density of the comet. For the density of a sphere is as its matter directly, and cube of its diameter inversely. An example of which Mr. W. gives in the comet of 1665, of which he finds the quantity of matter to be to that in the sun, as 1 to 1510724, and its density about 3½ times that of the earth. And the density of the comet of 1682, to the density of the earth, is in like manner found to be as 5 to 11 nearly.

XVII. Some Attempts to Ascertain the Utmost Extent of the Knowledge of the Ancients in the East Indies. By Mr. John Caverhill.* p. 155.

It may be proper first to observe, that Cattigara was the name of a port situated somewhere beyond the Aurea Chersonesus or Malacca; and that the ancients had never sailed farther than Cattigara: for contiguous to it was a terra incognita. But at what distance the Aurea Chersonesus was from Cattigara, Ptolemy himself was ignorant; for he says, that Marinus, who is quoted by him on this occasion, had not marked the number of the stadia. By a vague calculation on the report of seamen, Ptolemy makes Cattigara 17½ degrees of longitude from Malacca, whereas they are nearly on the same meridian. For several reasons, Mr. C. concludes, it is extremely probable that Cattigara stood somewhere on the north-east coast of the bay of Siam, and probably the same as the port Ponteamas; also that the modern city Cambodia is the ancient metropolis Sinæ. It appears too that the ancients sailed to, and were acquainted with the island of Java, and they made mention of the Manillas. Notwithstanding Ptolemy has mentioned the Philippines, yet we do not imagine that any of the persons from whom he acquired his information had ever been there: but that they had heard of these places at Java, to which they might easily have sailed, either from the Javanese themselves, or from the inhabitants of the circumjacent islands, who resorted to Java for the same advantages of commerce which they themselves came in pursuit of. However, though they must almost necessarily have been acquainted with Sumatra, yet it is evident they had never sailed quite round it; for if they had, they would certainly not have mentioned Ceylon as the largest island in the ocean. Hence it would appear that they only knew part of Sumatra and Java; and either conjectured these were islands, or depended on some informations they might probably have received from the inhabitants of these places, relative to this particular.

So that here we may venture to fix the limits of Ptolemy's knowledge; for as these islands at that time were but a late discovery, they were very imperfectly known; and unfortunately the geographers who lived after him were all so prepossessed with his superior abilities, that they imagined his accuracy would bear no correction, and that he had exhausted the subject. For no other author

* On this subject a more extended treatise was written by the late celebrated historian Dr. Robertson, of Edinburgh. See also Dr. Vincent's Periplus of the Erythræan Sea.

mentions any discoveries to the east of these, taken notice of by him; and Marcianus Heracleota had such an opinion of his great merit, as to call him by the name "of the most divine and most wise Ptolemy."

By a retrospect on such authors as have been quoted, and some others who wrote nearly at the same time, according to the order in which they lived, this subject will still appear in a clearer light. In the days of Strabo, who lived before the Christian æra, and is supposed to have survived it 28 years, few people had sailed so far as the Ganges; and being intimately acquainted with Gallus, the 3d governor of Egypt, he had undoubtedly the most favourable opportunities of the most authentic intelligence concerning naval affairs.

Pomponius Mela is supposed to have written before Pliny, in the reign of Claudius, and 30 years after Strabo. In that interval, there appears to have been made some further discoveries on the continent to the east of the Ganges; but so very imperfect, that they either imagined that country was an island, or had confounded their descriptions of it with those islands which they would necessarily meet with in this voyage. For it is very certain from Mela's own words, that his knowledge of these places we are speaking of was extremely obscure, as all he has said of them is, "ad Tabim insula est Chrysa, ad Gangem Argyra, alteri aurea soli, altera argentea; atque ut maxime videtur, aut ex re nomen, aut ex vocabulo ficta fabula est." The elder Pliny died in the 79th of the Christian æra, and was a contemporary of Mela; and seems to have referred to the above passage, in the following words: "Extra ostium Indi Chryse et Argyre, fertiles metallis, ut credo; nam quod aliqui tradidere, aureum argenteumque iis solum esse, haud facile crediderim."

Though the age in which Solinus lived is so uncertain, yet it might be imagined that it was not very long after Pliny; having copied from the other geographers which went before him, he has advanced nothing on this point that had not been already mentioned. His words are these: "Extra Indi ostium insulæ duæ, Chryse et Argyre, adeo fœcundæ copia metallorum, ut plerique eas aurea sola prodiderint habere et argentea."

Josephus was 56 years of age, in the 14th year of Domitian's reign, or 93d of the Christian æra; and he appears to have had a little more knowledge of these places than any we have yet mentioned; for, speaking of Saphira, whence king Solomon had his gold, he says, that "it was a country of India, and not an island; and that it was now called by the name of Aurea."

Dionysius is supposed to have lived after Domitian, and before Severus. He wrote a description of the world in Greek verse, which it may be supposed he had finished before the reign of Trajan; or at least that he had not heard of the increase of geographical knowledge which took place at that time, for he was as little acquainted with the country beyond the Ganges, as those who are supposed to have been his predecessors, and only mentions it as an island remarkable for the distinctness with which the sun-rising was observed.

Ptolemy flourished under Adrian, and Antoninus; and made his last astronomical observation on a Wednesday, the 2d of February, in the year 141. He has taken notice of many places not mentioned any where else, and is the first who has called Malacca a peninsula. Marinus indeed, whom he quotes as a late author, knew likewise that it was so; which still more confirms the supposition, that this was found out in Trajan's reign. Ptolemy's works evidently show, that his knowledge was superior to that of all the other ancient geographers; and his living in Egypt gave him many opportunities of a very early intelligence concerning any discoveries made by navigation, which might be a long time before they were communicated to the other learned men of that extensive empire. Accordingly we see that the author of the P. Maris Erythræi, who is supposed to have been his contemporary, but lived a little later to the time of Marcus and Verus, was less acquainted with these late discoveries.

Agathemerus, who had read Ptolemy's works, lived in the reign of Severus and Galienus, in the beginning of the 3d century, and mentions the country of the Sinæ as the most oriental he was acquainted with.

Marcianus Heracleota is the last geographical author it will be necessary to mention. He is supposed to have lived some little time before the building of Constantinople, and even at that time this nation appears to have been the most oriental; for though he copied from such authors as wrote in the interval between Ptolemy and him, yet all the improvement made during that time was only a mensuration of this particular coast, which Ptolemy himself tells us was not done in the days in which he lived.

From these circumstances it is apparent, that no mention was made of this country during the first century. Marinus, as we have seen, wrote before Ptolemy; Ptolemy was far advanced in years before the middle of the 2d century; and further, as it may be supposed that Trajan sent these ships to India at the time of his arrival in Arabia, which was in the 116th of the Christian æra; this may very well agree, in point of chronology, not only with these authors, but also with our former supposition, that this country was found out in his reign. But as he scarcely survived the expedition 2 years, such persons as were employed in this voyage, finding on their return that he was dead, might be discouraged from pursuing any discoveries they had made; especially as the voyage was attended with so much hazard and difficulty, and as the views on which they had undertaken it were in all probability frustrated by the accession of a new emperor. Admitting therefore, that this was their first attempt, may not the extent of their discovery be looked on as very considerable; and will it not in some measure account for their not having proceeded any farther than the east side of the bay of Siam?

On the whole, as nothing was exported from this kingdom of the Sinæ but what the city of Cambodia excelled in; and as the ancient and modern situations of these cities appear to be reciprocal; above all, as we have the testimony of the Periplus Maris Erythræi, that it lay somewhere in the bay of Siam, and the express declaration of two others, that it was situated on the east side of the bay; joined to the unanimous consent of all the geographers, that the country to the east and south was unknown, it may reasonably be inferred, that their ultima were on this coast; and the metropolis Sina or Thina the same as the modern city of Cambodia.

VIII. A Computation of the Distance of the Sun from the Earth. By S. Horsley, LL.B., Rector of St. Mary, Newington, in Surry, F.R.S.* p. 179.

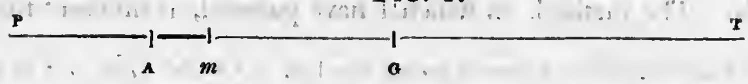
I offer the following computation, rather as a verification than an amendment of Dr. Stewart's. The method, in which I have pursued, is different from what

* This very able mathematician and learned prelate was born in October 1732, and he died Oct. 4, 1806, consequently about 74 years of age. He received the elements of his education at Westminster-school, whence he was removed to the university of Cambridge. There he chiefly applied to the study of mathematics; and not content with carefully consulting the writings of the acutest of the moderns, he recurred to the profoundest of the ancients in that line, and made himself master of their most intricate reasonings. Having taken his degree as master of arts, he accepted an invitation to accompany the present Earl of Aylesford, as private tutor, to Oxford. Here, in 1769, Dr. H. printed his edition of the Geometrical Inclinations of Apollonius, and here he first conceived the design of publishing a complete edition of the works of Sir Isaac Newton, for which he began to prepare the materials. And from this time it has been more the fashion to cultivate the mathematical sciences at that university. On leaving this place Dr. H. came to London, where he was elected F.R.S., and enriched the Phil. Trans. with many valuable essays, from vol. lvii. to vol. lxvi. He was also chosen one of the secretaries in 1773, on the resignation of Dr. Morton; an office he continued to serve, with the greatest credit to himself and benefit to science, till he thought it proper to resign along with the late president Sir John Pringle, in 1778. Soon after settling in the metropolis, Dr. H. was noticed by Bishop Louth, and was appointed his lordship's domestic chaplain, as well as was presented by that prelate successively to several different livings. In 1776, Dr. H. published proposals for a complete edition of the works of the immortal Newton, with notes, which appeared in 1779, in 5 vols. 4to, with an elegant Latin dedication to the king. In 1778 he preached and published a sermon against the principles of materialism, agitated between the doctors Price and Priestley, &c.; and on the publication, in 1783, of Dr. Priestley's History of the Corruptions of Christianity, Dr. H. again attacked the positions and principles of that writer and his adherents, with great applause and success, after the exchange of several pamphlets, &c. on both sides; which chiefly laid the foundation of Dr. H.'s elevation to the episcopal dignity in 1788, assisted probably by the very celebrated speeches he made in the course of the violent debates that took place in the Royal Society 2 or 3 years before, on occasion of the removal of Dr. Hutton from the office of foreign secretary, when Dr. Horsley, and several other members, forsook, as expressed in his own forcible language, " that temple, where Philosophy once reigned, and where Newton pre-

is used by that great and able geometrician, in his treatise on the distance of the sun, but founded entirely on the theorems established in that and the preceding tracts of the same author.

Let TA be a given line. Take A*m*, so that TA may be to A*m*, as the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take AG quintuple of A*m*. Take AP, such that twice A*m* may be to AP, as TG to TA. Now it is proved in the 25th prop. of Dr. Stewart's 4th tract, that the cube of TA is to the cube of TP, in the duplicate proportion of the periodic month to the anomalistic month. Therefore the proportion of TA^3 to TP^3, and consequently that of TA to TP, is given; and by division, that of TA to AP is given. Therefore TA being given, AP is given. Now $TG : TA = 2Am : AP$. That is $TA - 5Am : TA = 2Am : AP$. Therefore $TA \times 2Am = (TA - 5Am) \times AP$. Therefore $2Am = \frac{TA \times AP - 5Am \times AP}{TA}$. That is $2Am + \frac{5Am \times AP}{TA}$, or $\frac{(2TA + 5AP) \times Am}{TA} = AP$. That is $Am = \frac{TA \times AP}{2TA + 5AP}$.

Fig. 1.

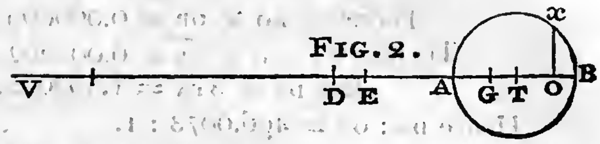


sided as her officiating minister." In 1794. Dr. H. was translated from St. David's to the see of Rochester, with the deanery of Westminster; and in 1802, to the lucrative see of St. Asaph. In 1801 he published the first, and in 1803 the last, of 3 octavo volumes of practical mathematics, for the use of students; consisting of Euclid's Elements and Data, with notes, the properties and projection of the sphere, and spherical trigonometry, Archimedes on the mensuration of the circle, a tract on the nature and use of logarithms, &c. Besides these, numerous and important were the writings and labours of Dr. H. in theology, in politics, &c.; for an ample account of which, and of various other prrticulars of his life and transactions, reference may be had to several memoirs of them, in the Monthly Magazine, anno 1806, vol. xxii. p. 401, and the Gent. Mag. of the same year, vol. lxxvi. pp. 987, 1057, 1095; but especially the former.

Dr. H. had gone down to Brighthelmstone, in the latter part of September, chiefly to visit his very respectable friend Lord Thurlow, who however died two or three days before the doctor's arrival. Dr. H. had not been at that place two weeks before he was seized with a complaint in the bowels, then frequent in the place, which soon turned to a mortification, and terminated his valuable existence on the 4th day of his illness. Though Dr. H. had reached the 74th year of his age, the powers both of his body and mind were so vigorous as to promise still a considerable length of years; indeed he was always of a very remarkably active and energetic cast of mind, and firm, compact constitution of body. Perhaps no man of the age possessed more of what is termed recondite learning, or was more profoundly versed in classical chronology. As a senator he ranked in the first class: there were few important discussions in the house of lords in which he did not participate; especially when the topics related to the hierarchical establishments of the country, or to the French revolution, or to the African slave trade, of which he was a systematic opponent. As an orator, his voice was deep, full-toned and commanding; his enunciation distinct and clear; and his delivery in other respects highly advantageous, abating only for a remarkable mixture of certain guttural sounds, such as are almost peculiar to the inhabitants of the county of Northumberland, occasioned, he used to say, by the circumstance of a nurse from that part of England being long employed in his father's family.

2. Let TA represent the moon's mean distance from the earth. Take TV, such that TA may be to TV, in the duplicate proportion of the periodic month to the sidereal year. Take TG, such that VT may be to TG in the proportion of the moon's accelerating attraction to the earth, to the sun's mean disturbance of that attraction. Take TE such that TE may be to TA, as TA to TG. Take EO, such that the rectangle EOA may be equal to 3TE × TA. On the centre T, with the interval TA, describe a circle. Draw Ox perpendicular to AB, meeting the circle in x. Take AD = AT. The proportion of TA to TV being given, and TA being given, TV is given. But the proportion of TV to TG is given. Therefore TG is given, and the proportion of TG to TA is given. TG : TA = TA : TE. Therefore the proportion of TA to TE is given. Therefore TE is given. Therefore 3TE × TA is given. Therefore EO × OA is given. And EA (= TE − TA) is given. Therefore AO is given. But AB (= 2AT) is given. Therefore OB is given. Therefore AO × OB is given. AO × OB = Ox^2 (by the circle). Therefore Ox^2, and consequently Ox is given. But DB (= 3AT) is given. Therefore the proportion of DB to Ox is given. And the proportion of DB to Ox, is that of the mean distance of the sun, to the mean distance of the moon.

FIG. 2.



This is in brief the method of my computation. The computation is as follows: The periodic month is to the anomalistic month, as 57600 to 58091.

Therefore, in fig. 1, $TA^3 : TP^3 = 57600^2 : 58091^2 = 3317760000 : 3374564281$

$$\frac{3374564281}{3317760000} = 1.0171212748963155^c864197530^c864197530, \text{ \&c.}$$

Hence, by extracting the cube root, I find TA : TP = 1 : 1.005674827053.

Therefore put TA = 1. Then TP = 1.005674827053; and AP = 0.005674827053.

Hence, $\frac{TA \times AP}{2TA + 5AP} = 0.002797722 = Am.$

The square of the periodic month is to the square of the sidereal year, as 1 to 178.725.

Therefore TA : TV = 1 : 178.725, fig. 2.

But TV : TG = 1 : 0.002797722.

Therefore TA : TG = 1 : 178.725 × 0.002797722 = 1 : 0.50002286445.

TA : TG = TE : TA. Therefore TE : TA = 1 : 0.50002286445.

Therefore put TE = 1.

Then TA = 0.50002286445

And EA = 0.49997713555

And 3TE × TA = 1.50006859335 = EOA.

Hence AO $(= \sqrt{TE \times TA \frac{1}{4}EA^2} - \frac{1}{2}EA) = 1.00003658292.$

But AB = 2TA = 1.00004572890.

Therefore OB = 0.00000914598.

Therefore AO × OB = 0.000009146314586654616

Therefore $\sqrt{\text{AO} \times \text{OB}}$ = 0.0032024287 = Ox.

But DB = 3TA = 1.500068593.

Hence DB : Ox = 496.0073 : 1.

These computations have been made with no small rigour. I was sensible that, to obtain an accurate conclusion, it was necessary to determine AO with extreme precision; and for that purpose I submitted to the laborious task of computing the foregoing numbers to the 11th or 12th decimal place, by the common operations of arithmetic. In the result I differ from Dr. Stewart, by much less than $\frac{1}{6000}$ part of the whole distance, that is, by less than 5 semidiameters of the earth; a very contemptible difference in so nice a calculation. That great mathematician indeed seems to have flattered himself, that he had determined the sun's distance within $\frac{1}{430000}$ of the truth. I suspect that when he affirmed this, he did not consider that to attain so great an accuracy in the conclusion, the line Eq in his method (vide Stewart on the sun's distance, fig. 10), or AO in mine, should be determined strictly to the 11th or 12th decimal place. And after the utmost rigour of computation, I am afraid any pretensions to such extreme nicety in the result will be but ill founded. For it is very likely that these computations represent the sun's distance less than it really is: because the whole progression of the moon's apogee (which is the basis of the calculation) is ascribed to the sun's disturbance of the moon's gravitation to the earth. Whereas part of it must be due to the disturbances of the planets. What part is due to them we cannot tell, and therefore cannot allow for it. But in giving the whole to the sun we certainly overrate his disturbing force, and by that means must obtain too small a distance. It is most likely indeed, that the motion of the apogee produced by the disturbing forces of the planets bears but a very small, perhaps insensible, proportion to the whole. But those who are masters of Dr. Stewart's theorems will easily perceive that an insensible error in the proportion of the moon's gravity, and the sun's disturbance, may produce a very sensible error in the proportion of the mean distances. And therefore the real distance is probably greater by two or three semidiameters of the earth than these computations make it. This however is much too nice a point for the approaching transit, or perhaps for any method of observation to determine. The highest expectations of astronomers will be answered, if they can come within 50 or 60 semidiameters of the earth.

It is to be hoped, that every civilized nation of the universe will give due attention to that interesting phenomenon, which we, the present possessors of these sublunary regions, shall behold no more; and that proper persons will be sent in due time, and duly equipped, to the most advantageous stations. If the decisions of observation in so nice a point should be found to agree with the previous conclusions of theory, the disciples of Newton will have no small reason to exult in a new attestation of nature to the truth of their great master's doctrine. But it is much to be wished, that they who shall be deputed to prosecute this curious search, in distant and sequestered parts, may divest themselves of all prejudice; that they may have nothing at heart, but that which the world will expect from them, the advancement of real science; that they may be diligent in their observations, and faithful in their reports; and not sacrifice the repose of their own minds, or the interests of philosophy, to the credit of an admired hypothesis, the memory of a friend, or the jealousies of rival nations. If the moon's mean distance from the earth be $60\frac{1}{3}$ semidiameters of the earth, the sun's mean distance is 30008.4416 semidiameters of the earth. The sun's semidiameter is to the semidiameter of the earth, as 139.876 to 1. The globe of the sun is to the globe of the earth, as 2736718.8 to 1; and the sun's horizontal parallax is $6'' 52'''.415$.

To satisfy myself more fully of the accuracy of my work, I have recomputed the whole, from the determination of EA, in Dr. Stewart's approximating method. I found the proportion of DB to *vs* (see Dr. Stewart on the distance of the sun, fig. 10) that of 496.00579 to 1; and the proportion of DB to *tv*, that of 496.00805 to 1. The mean of these two gives the proportion of DB to *ox*, nearly that of 496.0069 to 1. Which differs from the result of my former computation by less than $\frac{1}{1240000}$ of the whole; and the method of the former computation is undoubtedly the most accurate.

Supplement to the Foregoing Paper.

In deducing the distance of the sun in semidiameters of the earth, and his horizontal parallax, from the proportion above concluded between the sun's mean distance and that of the moon; I have supposed the latter to be $60\frac{1}{3}$ semidiameters of the earth, as it is reckoned by Sir Isaac Newton. According to the hypothesis which seems to be now generally received, that the density of the moon is very nearly equal to that of the earth, (the French reckon it rather less) the moon's mean distance should be little more than 60.23207, that is, not quite $60\frac{1}{4}$ semidiameters of the earth. But from some computations that I have formed with great care; I have reason to think, that Sir Isaac Newton's determination is much nearer to the truth; that the density of the moon is actually greater than that of the earth, in the proportion of 6 to 5 nearly; and that the moon's mean distance amounts to 60.441 semidiameters of the earth; which differs from the distance assigned by Sir I. Newton, by less than $\frac{1}{1007}$ of the whole.*

* Notwithstanding the seeming confidence of this excellent mathematician, in the calculations of the earth's distance from the sun, as given by Dr. Stewart and here repeated by himself, it appears that the result is far from being accurate, being about a 4th part too great; owing partly to inaccuracies in the data assumed and employed in the calculation, and partly to the rejection of certain terms

XIX. Description of an improved Apparatus for performing Electrical Experiments, in which the Electrical Power is Increased, the Operator Entirely Secured from receiving any Accidental Shocks, and the whole rendered more Convenient for Experiments than heretofore. By C. L'Epinasse, F.R.S. p. 186.

The first method of improvement consists in lining the inside of the glass cylinder or globe with the following composition. Take 4 lb. of Venice turpentine, 1 lb. of resin, 1 lb. of bees' wax; boil these over a gentle fire, stirring them now and then, for about 4 hours, at the end of which stir in a quarter of a pound of vermilion: then, a little of the mixture being taken out and left to cool, will be hard and brittle; a token that it is fit for use. Having well heated the globe or cylinder, pour the melted mixture into it; turn the cylinder about so as to spread it evenly over the inside surface to the thickness of a sixpence, and let it cool very gradually.

A cylinder thus lined acts with much greater force than it did before it was lined, every other circumstance alike. And as a small cylinder thus prepared is equal in power to one much larger, that is not, and requires less friction, the apparatus in which it is mounted may be much contracted, and the whole, together with the person that turns the machine, may be easily supported on one or two small stools with glass feet, when experiments require it.

As raising the greatest quantity of electrical fire was the object of the first improvement, the next thing was, to preserve it when raised, and use it without wasting any, so that it might have its full effect. Mr. L. had observed, that whenever a single wire was made use of instead of a chain in discharging the jars, the effect was much stronger; and on making further experiments, he found that when the discharging parts were not all in close contact, such as being screwed tight together, or ground into one another, the effect was considerably diminished. In constructing the discharging apparatus, he therefore contrived that all the parts should be in close contact, by screwing, grinding, or soldering, them together; and thus the electrical fire exerts its whole force on the body on which the experiment is made. Lastly, it often happened in discharging the jars when loaded very high (as they must be to kill a large animal, or to force the fire through bodies that make a great resistance), that the persons operating, notwith-

and small quantities, to render the calculation easier. It further appears that an accurate determination of that distance is not even to be expected on the same principles as here employed. On this subject, see two pamphlets, the first in 1769 by Mr. Dawson of Sedberg, but without his name, entitled "Four Propositions, &c.;" and the other by Mr. Landen, in 1771, entitled "Animadversions on Dr. Stewart's Computation of the Sun's Distance from the Earth." See also an account of Dr. Stewart's Life in the 1st vol. of the Edinburgh Phil. Trans.

standing all their skill and care, received the whole or part of the shock. To remove this inconvenience entirely, the discharging frame is contrived, which, at the same time that it prevents the wasting of the electrical fire, leaves no possibility of the operator's ever receiving any shock.

XX. Two Letters from the Hon. William Hamilton, Envoy Extraordinary at Naples, containing an Account of the last Eruption of Mount Vesuvius. Dated Naples, June* 10, 1766, *and Feb.* 3, 1767.

Sir W. H. generally observed the smoke from the mouth of the Volcano was much more considerable in bad weather than when it was fair; and he often heard (even at Naples, 6 miles from Vesuvius) in bad weather, the report of the interior explosions of the mountain. When at the top of Mount Vesuvius in fair weather, he sometimes found so little smoke that he could see far down the mouth of the volcano, the sides of which were incrusted with salts and minerals of various colours, white, green, deep and pale yellow. The smoke that issued from the mouth of the volcano in bad weather was white, very moist, and not near so offensive as the sulphureous steams from various cracks on the sides of the mountain. Towards the month of September last, the smoke was more considerable, continued even in fair weather; and in October he perceived sometimes a puff of black smoke shoot up a considerable height in the midst of the white, which symptom of an approaching eruption became daily more frequent; and soon after, these puffs of smoke appeared in the night tinged like clouds with the setting sun.

About the beginning of November, he went up the mountain; it was then covered with snow, and he perceived a little hillock of sulphur had been thrown up since his last visit there, within about 40 yards of the mouth of the volcano, it was near 6 feet high, and a light blue flame constantly issued from its top. While examining this phenomenon, he heard a violent report, and saw a column

* Sir W. Hamilton was born in Scotland, of the noble family of that name, in 1730; and he died at London the 6th of April, 1803, in the 74th year of his age. His education was liberal, and he possessed an enlightened mind. And having been appointed ambassador to the court of Naples in 1764, he there distinguished himself by his exertions in promoting the fine arts, in collecting antiquities, and advancing the interests of science, till his recal in 1800. He was indefatigable in examining the volcanic mountains of Vesuvius and Etna, his valuable observations on which have enriched many of the volumes of the Philos. Trans. from which they have also been collected, and republished with notes, in 1772, 8vo. And he greatly enriched the British Museum by his presents of antiquities and other curiosities. His Campi Phlegræi, 2 vols. folio, is considered as a most interesting and splendid performance. He was the chief promoter of the publication of that elegant and magnificent work, Antiquités Etrusques, Grecques, et Romaines, tirées du Cabinet de M. Hamilton, the editor of which was D'Hancarville. See other particulars of Sir W. H. in the Literary Journal.

of black smoke, followed by a reddish flame, shoot up with violence from the mouth of the volcano, and presently fell a shower of stones. From November to the 28th of March, the date of the beginning of this eruption, the smoke increased and was mixed with ashes, which fell, and did great damage to the vineyards in the neighbourhood of the mountain. A few days before the eruption he saw (what Pliny the younger mentions having seen, before that eruption of Vesuvius which proved fatal to his uncle) the black smoke take the form of a pine tree. The smoke that appeared black in the day-time for near 2 months before the eruption, had the appearance of flame in the night.

On Good Friday, March 28, at 7 o'clock at night, the lava began to boil over the mouth of the volcano, at first in one stream; and soon after, dividing itself into two, it took its course towards Portici. It was preceded by a violent explosion, which caused a partial earthquake in the neighbourhood of the mountain, and a shower of red hot stones and cinders were thrown up to a considerable height. The lava ran near a mile in an hour's time, when the two branches joined in a hollow on the side of the mountain, without proceeding farther. Sir W. approached the mouth of the volcano, as near as he could with prudence: the lava had the appearance of a river of red hot and liquid metal, such as is seen in the glass houses, on which were large floating cinders half lighted, and rolling one over another with great precipitation down the side of the mountain, forming a most beautiful and uncommon cascade; the colour of the fire was much paler and more bright the first night than the subsequent nights, when it became of a deep red, probably owing to its having been more impregnated with sulphur at first than afterwards. The 29th the mountain was very quiet, and the lava did not continue. The 30th it began to flow again in the same direction, while the mouth of the volcano threw up every minute a girandole of red hot stones, to an immense height. The 31st he passed the night on the mountain; the lava was not so considerable as the first night, but the red hot stones were perfectly transparent, some of which, of a ton weight, mounted at least 200 feet perpendicular, and fell in, or near, the mouth of a little mountain, that was now formed by the quantity of ashes and stones, within the great mouth of the volcano, and which made the approach much safer than it had been for some days before, when the mouth was near half a mile in circumference, and the stones took every direction. It is impossible to describe the beautiful appearance of these girandoles of red hot stones, far surpassing the most astonishing artificial fire-work. On the 10th of April at night the lava disappeared on the side of the mountain towards Naples, and broke out with much more violence on the side next the Torre dell' Annunciata. It burst out of the side of the mountain, within about half a mile of the mouth of the volcano, like a torrent, attended with violent explosions, which threw up inflamed matter to a considerable height, the adjacent ground quivering like the timbers of a water-mill. Notwithstanding the consistency of the lava, it ran with amazing velocity; the first mile with a rapidity equal to that of the river Severn, at the passage near Bristol. The stream at its source was about 10 feet wide, but soon extended itself, and divided into 3 branches, so that these rivers of fire communicating their heat to the cinders of former lavas, between one branch and the other, had the appearance at night of a continued sheet of fire, 4 miles in length, and in some parts near 2 in breadth. Your lordship may imagine the glorious appearance of this uncommon scene, such as passes all description. The lava, after having run pure for about 100 yards, began to collect cinders, stones, &c. and a scum was thus formed on its surface, which in the day-time had the appearance of the river Thames, after a hard frost and great fall of snow, when beginning to thaw, carrying down vast masses of snow and ice. In two places the liquid lava totally disappeared, and ran in a subterraneous passage for some paces, then came out again pure, having left the scum behind. In this manner it advanced to the cultivated parts of the mountain, destroying the cottages in its way. The lava, at the farthest extremity from its source, did not appear liquid, but like a heap of red hot coals forming a wall, in some places 10 or 12 feet high, which rolling from the top soon formed another wall, and so on, advancing slowly, not more than about 30 feet in an hour.

The mouth of the volcano has not thrown up any large stones since the 2d eruption of lava, on the 10th of April, but has thrown up quantities of small ashes and pumice stones, that have greatly damaged the neighbouring vineyards. In his last visit to Mount Vesuvius the 3d of June, he still found that the lava continued, but the rivers were become rivulets and had lost much of their rapidity.

Mount Etna in Sicily broke out the 27th of April, and made a lava in two branches, at least 6 miles in length, and a mile in breadth, and that of Vesuvius resembled it in every respect, except that mount Etna, at the place whence the lava flowed (which was 12 miles from the mouth of the volcano), threw up a fountain of liquid inflamed matter to a considerable height; which he was told, mount Vesuvius has done in former eruptions. Other particulars of this, and some later eruptions of these two mountains, may be seen with advantage, in Sir W.'s observations on these volcanos, as extracted from these Transactions, and published with notes in 1772.

XXI. Observations on the Heat of the Waters at Bath. By John Howard, Esq. F. R. S. p. 201.

King's bath pump	113°	Hot bath, coolest part	96°
Hot bath pump	114	Ditto, warmest part	97
Cross bath pump	108	The pump in the hot bath	113

King's bath, coolest part	99°	Pump in the Market-place, Bath	54°
Ditto, hottest part	101	Springs on Claverton, and at the late Mr. Allen's	47
Queen's bath, coolest part	97	Springs on Lansdown	45
Ditto, warmest	98	St. James's spring water	43
The pump in the bath	113	Old well house, Bristol	67
Cross bath, coolest part	89	New well, ditto	76
Ditto, warmest part	90		
Cross bath pump	107		

The temperature of the above springs taken in November, and December last 1765, by Fahrenheit's scale (Bird's Thermometer).

XXII. Observations on the Heat of the Bath and Bristol Water. By Mr. John Canton, A. M., F. R. S. p. 203.

After pumping about a quarter of an hour, a Fahrenheit's thermometer, held in the stream from the common pump of the king's bath, was raised to 112°. The stream from the common pump of the hot bath raised it to 114½°. At the pump of the cross bath, it stood at 110°. At noon, the heat of the shaded air was 66°, and of common water exposed to it 61°. The bath water, and common water, brought to the same degree of heat, cooled equally fast. The next day, Sept. 14, he was at the hot-well near Bristol, the water of which raised the thermometer to 76°. In common water exposed to the shaded air it stood at 62°.

XXIII. On some Particular Fish found in Wales. By the Hon. Daines Barrington, F. R. S.* p. 204.

The following particulars are with regard to perch † in a pool of Merionethshire; and trout, which are found in a river of Cardiganshire. The pool is si-

* Mr. D. Barrington was the 4th son of lord viscount Barrington, of the kingdom of Ireland, so well known by his "Miscellanea Sacra." Mr. B. was bred to the law; but, though esteemed a very sound lawyer, he never rose to any distinguished eminence as a pleader. He was however for some time recorder of Bristol. He was also advanced to the rank of king's counsel, and was, during several years, one of the Welsh judges. Had it been his wish, doubtless Mr. B. might have attained the English ermine. But, possessing an ample income, and having a strong bias to antiquarian knowledge, natural history, and its concomitant studies, he retired from the practice of the law, and applied his legal knowledge to the purposes of investigating curious questions of legal antiquity; which were published in a 4to volume. His inquiries into ornithology and various phenomena of nature, are well known. He was an old and most respectable member of the R. S. and an ingenious contributor to the annual volume of its Transactions, as well as to those of the Antiquarian Society, of which he was a vice-president. Mr. B. published Observations on the Statutes, in 4to.; also Tracts on the Probability of Reaching the North Pole; and a number of other pieces on miscellaneous subjects. Mr. B. had for a great number of years occupied the chambers in the King's Bench Walks, in the Inner Temple, where he died the 14th of March, 1800, after a lingering illness, at a very advanced age. See an extensive account of many particulars of Mr. B.'s life and transactions, in the Gent. Mag. for 1800, vol. 70, p. 291, &c.

† See Pennant's British Zoology, vol. 3, article *Perch*, where a figure of this distorted variety may be found.

tuated in the parish of Trawsvynnyd, and is called Llyn Raithlyn; as it does not lie near any road, a common traveller cannot hear any thing about it, but by very extraordinary accident. Having been informed that perch were frequently caught there, which were crooked near the tail, Mr. B. procured fish of this sort at 3 different times; as he intended to preserve them in spirits, he always desired that they should be of a small size. These fish were all of them most apparently crooked in that part; which appears still more in those of a larger size, and some of them have been taken of nearly 2lb. weight. These fish are not only crooked near the tail, and for about one third of the whole length of their body; there is likewise a very remarkable protuberance on each side, which he opened with a knife, but did not observe it to differ materially from other parts of the flesh. In eating, they are not to be distinguished from the common perch.

Mr. B. heard of trout, which were crooked in the same part, said to be peculiar to the river Eynion in Cardiganshire, a small brook that empties itself into the Dovey, near Egglwys Vach, and is on the road from Machentleth in Montgomeryshire, to Talypont in Cardiganshire.* Mr. B. procured at two several times specimens of these trout likewise. They are crooked in the same manner near the tail; but, as the make of a trout is more taper than that of a perch, the curve does not appear so strongly: no one, however, who looks at them with any degree of attention, can have the least doubt of their differing most materially from other fish of the same kind. These trout are caught only in a small basin of perhaps 8 or 9 feet deep, which the river Eynion forms after a fall from the rocks. By very accurate accounts from those who have caught both the perch and trout, it appears that it is not above half of these fish that are thus crooked; and that the others do not in any respect differ from the common ones of these two sorts.

As Mr. B. often observed that the existence of such fish was doubted by the Welsh themselves, till he had procured these specimens, it occasioned his inquiring with regard to monocular fish, which are said by Giraldus Cambrensis, to be found in the lakes of Snowden. This writer was archdeacon of Brecknock, and attended Baldwin Archbishop of Canterbury, in a progress which he made in the year 1188 through South and North Wales, to recommend a collection for a

* In Dalekarlia, a province of Sweden, near Fahlun, are two small lakes, famous for the singular shape of the perch, wherewith they abound. These perch grow to the common size, and are of a good taste, but they have all a hump on their back. This peculiarity is taken notice of in Linnæi. Fauna Suecica, p. 118. The country people in the neighbourhood imagine that it may be occasioned by the quality of the water in those lakes, which might probably be impregnated with some mineral salt, especially as they are situated near the largest copper mine in Europe. DAN. SOLANDER.

There is no copper mine near Llyn Raithlyn, or the river Eynion. DAINES BARRINGTON. —Orig.

crusade which was then in agitation. It will appear to any one who reads the whole of his Iter, and is at all acquainted with the geography of the country, that Giraldus (who was a native of Pembrokeshire) never was himself in these mountains of Snowden; he had therefore only picked up this account, from some of the inhabitants of the towns through which the archbishop passed, who themselves probably received it from mountaineers. There are few inhabitants of the principality, who have ever been in this tract of mountains; and Mr. B. who had been in most parts of them, had always been informed, at his setting out, that the roads were nearly impassable. On these occasions, he frequently inquired whether there was any such notion or tradition among the mountaineers, with regard to monocular fish, and found, that it is supposed there are such in a pool called Llyn y Cwn, which indeed he had never seen; but, by the best accounts, it is high up the Glyder mountain, which forms the opposite side of the vale of Lanberris to Wyddva, or the highest part of Snowden. But on further inquiry, he found no satisfactory confirmation of the matter.

XXIV. An Observation of an Eclipse of the Sun at Newfoundland, August 5, 1766. *By Mr. James Cook. Communicated by J. Bevis, M.D., F. R. S.* p. 215.

Mr. Cook, a good mathematician, having been appointed by the lords commissioners of the admiralty, to survey the sea-coasts of Newfoundland, Labrador, &c. took with him a good apparatus of instruments, and among them a brass telescopic quadrant made by Mr. John Bird. Being, on August 5, 1766, at one of the Burgeo islands near Cape Ray, latitude 47° 36′ 19″, the south-west extremity of Newfoundland, and having carefully rectified his quadrant, he waited for the eclipse of the sun; just a minute after the beginning of which, he observed the zenith distance of the sun's upper limb 31° 57′ 00″; and, allowing for refraction and his semidiameter, the true zenith distance of the sun's centre 32° 13′ 30″, whence he concluded the eclipse to have begun at $0^h\,4^m\,48^s$ apparent time, and by a like process to have ended at $3^h\,45^m\,26^s$ apparent time. There were 3 several observers, with good telescopes, who all agreed as to the moments of beginning and ending. Mr. Cook having communicated his observation to me, I showed it to Mr. George Witchell, who told me he had a very exact observation of the same eclipse, taken at Oxford by the Rev. Mr. Hornsby; and he would compute, from the comparison, the difference of longitude of the places of observation, making due allowance for the effect of parallax, and the earth's prolate spheroidal figure; and he has since given me the following result:

5^h	23^m	59^s	beginn. at Oxford.	7^h	7^m	5^s	end at Oxford.
0	46	48	beginn. at Borgeo Isles	3	39	14	end at Borgeo Isles.
4	37	11		3	27	51	
—	51	59	effect of parallax, &c.	+	17	35	effect of parallax, &c.
3	45	22	diff. of meridians.	3	45	26	diff. of meridians.

XXV. On the Heat of the Climate at Bengal. By Fleming Martin, Esq. Chief Engineer at that Place. p. 218.

The intense and uncommon heat in this climate has been for some time past almost insufferable. The thermometer was seldom under 98, and the quicksilver rose at certain times of the day to 104° by the best adjusted instrument; he was even assured by some gentlemen, that in the camp, 500 miles distant, the thermometer often stood at 120; but such a difference might be occasioned by the badness of the instrument. However, nothing could exceed the intense heat felt day and night, during the month of June. May and July were little inferior at times, but afforded some intermission; otherwise a very great mortality must have attended this settlement, though they were not without instances of fatal effects in the month of June, when some few individuals in sound health were suddenly seized, and died in the space of 4 hours.

The rains have set in since the 4th of June. This is called the unhealthy season, on account of the salt-petre impregnated in the earth, which is exhaled by the sun, when the rain admits of intervals. Great sickness is caused by it, especially when the rains subside: which generally happens about the middle of October. The air becomes afterwards rather more temperate, and till April permits of exercise, to recover the human frame, that is relaxed and worn out by the preceding season; for in the hot periods every relief is denied, except rising in the morning, and being on horse back by day break, to enjoy an hour, or little more, before the sun is elevated; it becomes too powerful by 6 o'clock to withstand its influence; nor can the same be attempted that day again till the sun retires; so that the rest of the 24 hours is passed under the most severe trials of heat. In such season it is impossible to sleep under the suffocating heat that renders respiration extremely difficult; hence people get out into the virandos and elsewhere for breath, where the dews prove cooling, but generally mortal to such as venture to sleep in that air. In short, this climate soon exhausts a person's health and strength, though ever so firm in constitution, as is visible in every countenance, after being here 12 months.

XXVI. Experiments on the Peruvian Bark. By Thomas Percival, M.D., F.R.S. p. 221.

May be consulted in this author's Medical and Experimental Essays, in 2 vols. 8vo.

XXVII. An Inquiry into the Probable Parallax and Magnitude of the Fixed Stars, from the Quantity of Light which they afford us, and the Particular Circumstances of their Situation. By the Rev. John Michell, B.D., F.R.S. p. 234.

Though no man can at present doubt, that the want of a sensible parallax in the fixed stars, is owing to their immense distance, yet it may not perhaps be disagreeable to see that this distance is further confirmed by other circumstances; for let us suppose them to be, at a medium, equal in magnitude and natural brightness to the sun, to which they seem in all respects to be analogous. And, having laid this down as a foundation to build on, let us inquire what would be the parallax of the sun, if he were to be removed so far from us, as to make the quantity of the light, which we should then receive from him, no more than equal to that of the fixed stars. To do this with accuracy, it would be proper to compare the quantity of light, at present received from him, with that of the fixed stars, by some such methods, as are made use of by M. Bouguer, in his Traité d'Optique; but as the present purpose does not require any such exactness, Mr. M. deduces it in a more gross way, from facts already well known. He assumes Saturn then in opposition, exclusively of his ring, and when the earth and he are at their mean distances from the sun, as equal, or nearly equal in light, to the most luminous fixed star. Now the mean distance of Saturn from the sun being equal to about 2082 of the sun's semidiameters, the density of the sun's light at Saturn, will consequently be less than at his own surface, in the proportion of the square of 2082 (or 4334724) to 1. If Saturn therefore was to reflect all the light that falls on him, he would be less luminous in the same proportion: but, besides this difference in his brightness, his apparent diameter, in opposition, is at most but 105th part of that of the sun, and consequently the quantity of light, which we receive from him, must again be diminished in the proportion of the square of 105 (or 11025) to 1. If we multiply these two numbers together, we shall have the whole of the light of the sun to that of Saturn (on the supposition of his reflecting all the light that falls on him) as the square of nearly 220000 (or 48,400,000,000) to 1; and removing the sun to 220000 times his present distance, he would still appear at least as bright as Saturn, and his whole parallax on the diameter of the earth's orbit would be less than 2 seconds. This must consequently be assumed for the parallax of the brightest of the fixed stars, on the supposition that their light does not exceed that of Saturn.

By a like computation we shall find, that the distance at which the sun would afford us as much light as we receive from Jupiter, is not less than 46000 times his present distance, and his whole parallax, in that case, on the diameter of the earth's orbit, would not be more than 9 seconds, the light of Jupiter and Saturn, as seen from the earth, being in the ratio of about 22 to 1, when they are both in opposition, and supposing them to reflect equally in proportion to the whole of the light that falls on them.

But if Jupiter and Saturn, instead of reflecting the whole of the light that falls on them, should in fact reflect only a part of it, as for example, only a 4th or 6th, and this may very possibly be the case, we must then increase the distances computed above, in the proportion of 2 or $2\frac{1}{2}$ to 1, to make the sun's light no more than equal to theirs; and his parallax would be less, in the same proportion, than those already mentioned.*

On the supposition then, that the fixed stars are of the same magnitude and brightness with the sun, it is no wonder that their parallax should have hitherto escaped observation; since, if this is the case, it could hardly amount to 2 seconds, and probably not more than 1 in Sirius himself; though he had been placed in the pole † of the ecliptic, and in those that appear much less luminous, such for example as γ draconis, which is only of the 3d magnitude it could hardly be expected to be sensible with such instruments as have hitherto been used in search of it.

We have assumed the magnitude of the fixed stars, as well as their brightness, to be equal to those of the sun; it is however probable that there may be a very great difference among them, in both these respects; and how much soever we may therefore be wide of the truth, in attempting to fix the distance of particular stars from this reasoning, yet there is a very great probability that their mean distances, settled by this method, will not be much out, some exceeding and some falling short of it. And perhaps the consideration that a star must be 1000 times as great, cæteris paribus, to appear equally bright, if it is placed at 10 times the distance, may serve to make it probable that the limits of the errors, which we are likely to commit, in judging by such a rule, are not so great as we might otherwise imagine them to be.

With regard to the difference there may be in the native brightness of different stars, though it is probably very considerable, yet we can hardly suppose that it is equal to their difference in magnitude, at least if we except those which are subject to certain changes, and which for that reason we may suppose to be luminous in some parts of their surfaces only. In other instances we may perhaps judge in some degree of the native brightness of different stars with respect to one another by their colour, those which afford the whitest light, being probably the most luminous.‡

* The light which we receive from the full moon, according to M. Bouguer's experiments, in the work above-mentioned, is only a 300000th part of that which we receive from the sun, whereas it ought to amount to no less than a 45000th part of it, according to the principles above made use of in computing the quantity of light derived from Jupiter and Saturn; so that the moon, as appears from these experiments, reflects no more than between a 6th and a 7th part of the light that falls on her.—Orig.

† The latitude of Sirius being only 39° 33′, his parallax will be a little less than $\frac{2}{3}$ of the whole parallax.—Orig.

‡ We have at present no means of judging of the comparative brightness of the sun and of the fixed stars, in proportion to their respective sizes, excepting from the comparison of the sun's bright-

As far then as we can guess of the parallax at the fixed stars from the principles above laid down, we may reasonably expect that it should be exceedingly small, even in those of the first magnitude; yet, besides the probability that some of them may be either less, or less luminous than the sun, it is not so all as to leave us altogether without hopes, that we may some time or other be able to discover it in some of them; for it appears not impracticable to construct instruments, capable of distinguishing even to the 20th part of a second, provided the air will admit of that degree of exactness; but such instruments must be on a plan a good deal different from those hitherto made use of, as they would otherwise be, not only vastly too expensive, but also much too large and unwieldy to be of any use.

But whatever room there may be to hope, that we may some time or other be able to discover the parallax of a few among the fixed stars, yet at the same time it seems probable that we shall never be able to discover any sensible magnitude in their apparent diameters, which in Sirius himself, if he is not of less native brightness than the sun, must be considerably less, whatever be his parallax, than the 100th, probably than the 200th part of a second; so that it

ness with that of our common fires; but the sun's light exceeds the light of our brightest fires in so very great a proportion, viz. of some thousands to one, that we want some middle terms to be able to form any analogy, which might serve to carry us further. We find however in general, that those fires which produce the whitest light, are much the brightest, and that the sun, which produces a whiter light than any fires we commonly make, vastly exceeds them all in brightness; it is not therefore improbable, from this general analogy, that those stars, which exceed the sun in the whiteness of their light, may also exceed him in their native brightness; now this is the case with regard to many of them; and, on the contrary, there are some that are of a redder colour.

If however it should hereafter be found, that any of the stars have others revolving about them, for no satellites shining by a borrowed light could possibly be visible, we should then have the means of discovering the proportion between the light of the sun, and the light of those stars, relatively to their respective quantities of matter; for in this case, the times of the revolutions, and the greatest apparent elongations of those stars, that revolved about the others as satellites, being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us; and the actual quantity of matter which they contained would be known whenever their distance was known, being greater or less in the proportion of the cube of that distance. Hence, supposing them to be of the same density with the sun, the proportion of the brightness of their surfaces, compared with that of the sun, would be known, from the comparison of the whole of the light which we receive from them, with that which we receive from the sun; but, if they should happen to be either of greater or less density than the sun, the whole of their light not being affected by these suppositions, their surfaces would indeed be more or less luminous, accordingly as they were, on this account, less or greater; but the quantity of light corresponding to the same quantity of matter, would still remain the same.

The apparent distances, at which satellites would revolve about any stars, would be equal to the semiannual parallaxes of those stars, seen from planes revolving about the sun, in the same periodical times with themselves, supposing the parallaxes to be such as they would be, if the stars were of the same size and density with the sun.—Orig.

would scarcely be distinguishable with a telescope, on the former supposition, that should magnify 6, or on the latter with one that should magnify 12000 times.

Nor can we well expect to find their apparent diameters from any occultation by the moon, since a diameter of the 100th part of a second would be covered by the moon, if it entered directly, in less than the 10th part of a second of time, and therefore a star can hardly enter so obliquely, as to appear to vanish by degrees; no star probably, which the moon can pass over, subtending an angle half so great. A star might however appear to vanish by degrees in an occultation by the planet Venus, especially if the occultation was to happen only a little before or after either station; but this is an event which can occur so very seldom, that little is to be expected from it; and if Venus should be surrounded with an atmosphere, which is probably the case, it might very possibly then be of no service at all. For the same reasons also it is probable, that nothing can be determined from occultations by any of the rest of the planets, which on other accounts are still less proper for the purpose than Venus.

There seems to be little chance therefore of discovering with certainty the real size of any of the fixed stars, and we must consequently be contented to deduce it from their parallax, if that should ever be found, and the quantity of light which they afford us, compared with that of the sun. And in the mean time, till this parallax can be found, or something else may arise to furnish us with a more general analogy, we can only suppose them, at a medium, to be equal in size to the sun, this being the best means, which we have at present, of forming some probable conjecture concerning the extent of the visible universe. That we may be the better enabled to do this, it seems to be an object worth the attention of astronomers, to inquire into the exact quantity of light which each star affords us separately, when compared with the sun; that, instead of distributing them, as has hitherto been done, into a few ill defined classes, they may be ranked with precision, both according to their respective brightness, and the exact degree of it.

A catalogue of the stars formed on this plan, would answer several good purposes; for besides giving us a more just and certain notion of their general distances, it would perhaps help us, especially if the parallax of a few among them should be discovered likewise, to trace some analogies in their situation, which might enable us to determine both their distances, and other circumstances relating to them, with still more probability; and it would be a standing register, by which future astronomers might inform themselves of many variations, of which we are now ignorant for want of an ancient register of that kind.

From what has been said above, Mr. M. thinks it is very probable that we shall not be a great way from the truth in estimating the whole parallax of Sirius at one second, supposing him to be of the same size and native brightness with the sun; this therefore he assumes as a standard, till some better experiments shall inform us more exactly of the quantity of his light. Now, according to the best judgment he was able to make from some gross experiments, the quantity of light which we receive from Sirius does not exceed the light which we receive from the least fixed stars of the 6th magnitude, in a greater proportion than that of 1000 to 1, nor in a less proportion than that of 400 to 1; and the smaller stars of the 2d magnitude seem to be about a mean proportional between the two. Hence the whole parallax of the least fixed stars of the 6th magnitude, supposing them of the same size and native brightness with the sun, should be from about 2‴ to 3‴, and their distance from about 8 to 12 million times that of the sun: and the parallax of the smaller stars of the 2d magnitude, on the same supposition, should be about 12‴, and their distance about 2 million times that of the sun.

Mr. M. has hitherto argued about the distances of the fixed stars, on the supposition of their being of the same size and brightness with the sun; and if this was really so, those which appear the brightest must be the nearest to us. That this is in general the case, will be very readily allowed; for though it is true, that a much greater degree of real magnitude may compensate for the greatness of distance, and there is no reason for assigning any one limit to them rather than another; yet when it is as likely that the largest stars should be in any one part of space as in any other, the probability in favour of this hypothesis is very great: the real motions also, which have been observed among several of the brightest of the fixed stars, is another argument to the same purpose; and we shall find it still further confirmed by very strong arguments of analogy drawn from the circumstances of the particular situation of the stars in the heavens.

It has always been usual with astronomers to dispose the fixed stars into constellations: this has been done for the sake of remembering and distinguishing them, and therefore it has in general been done merely arbitrarily, and with this view only; nature herself however seems to have distinguished them into groups. What he means is, that from the apparent situation of the stars in the heavens, there is the highest probability, that either by the original act of the Creator, or in consequence of some general law, such perhaps as gravity, they are collected together in great numbers in some parts of space, while in others there are either few or none. The argument he intends to make use of, in order to prove this, is of that kind which infers either design, or some general law, from a general analogy, and the greatness of the odds against things having been in the present situation, if it was not owing to some such cause.

Let us then examine what it is probable would have been the least apparent distance of any two or more stars, any where in the whole heavens, on the supposition that they had been scattered by mere chance, as it might happen. Now it is manifest, on this supposition, that every star being as likely to be in any one situation as another, the probability that any one particular star should happen to be within a certain distance, as for example one degree, of any other given star, would be represented, according to the common way of computing chances, by a fraction, whose numerator would be to its denominator, as a circle of one degree radius, to a circle whose radius is the diameter of a great circle, this last quantity being equal to the whole surface of the sphere, that is, by the fraction $(\frac{60'}{6875.5'})^2$, or, reducing it to a decimal form, .000076154, that is, about 1 in 13131; and the complement of this to unity, viz. .999933846, or the fraction $\frac{13130}{13131}$, will represent the probability that it would not be so. But because there is the same chance for any one star to be within the distance of one degree from any given star, as for every other, multiplying this fraction into itself as many times as shall be equivalent to the whole number of stars, of not less brightness than those in question, and putting n for this number, $.999923846^n$, or the fraction $(\frac{13130}{13131})^n$, will represent the probability, that no one of the whole number of stars n would be within one degree from the proposed given star; and the complement of this quantity to unity will represent the probability, that there would be some one star or more, out of the whole number n, within the distance of one degree from the given star. And further, because the same event is equally likely to happen to any one star as to any other, and therefore any one of the whole number of stars n might as well have been taken for the given star as any other, we must again repeat the last found chance n times, and consequently the number $.999923846^{nn}$, or the fraction $(\frac{13130n}{13131n})^n$ will represent the probability, that no where, in the whole heavens, any two stars, among those in question, would be within the distance of one degree from each other; and the complement of this quantity to unity will represent the probability of the contrary.

By a like reasoning, if we would compute the probability, on the same supposition, that no two stars should be, one within the given distance x, and the other within the given distance z, of some one particular star, we must first find the probability that no star, of the whole number of stars n, would be within the distance x from the given star, which will be represented, as before, by the fraction $(\frac{6875.5^2 - xx}{6875.5^2})^n$; and secondly, we must find the probability that no star, of the whole number of stars n, would be within the distance z from the given star, which, for the same reason, will be represented by the fraction $(\frac{6875.5^2 - zz}{6875.5^2})^n$; and the complements of these to unity will represent the respective probabilities of the contrary; but the probability that two events shall both happen, is the product of the respective probabilities of those two events multiplied together; if therefore we multiply the two last-mentioned complements together, we shall have the probability that some two stars would be, one within the distance x, and the other within the distance z from the given star; and the complement of this to unity, will be the probability that it would not be so: let us now suppose $\frac{c}{d}$ to represent this last quantity, and because the same event may as well happen in respect to any one star as any other, multiplying this quantity into itself n times, according to the number of the stars, we shall have $(\frac{c}{d})$ representing the probability that no where, in the whole heavens, would be found any two stars, one within the distance x, and the other within the distance z from the same star.

If now we compute, according to the principles above laid down, what the probability is, that no two stars, in the whole heavens, should have been within so small a distance from each other, as the two stars β Capricorni, to which we shall suppose about 230 stars only to be equal in brightness, we shall find it to be about 80 to 1. For an example, where more than 2 stars are concerned, we may take the 6 brightest of the Pleiades, and, supposing the whole number of those stars, which are equal in splendor to the faintest of these, to be about 1500, we shall find the odds to be near 500000 to 1, that no 6 stars, out of that number, scattered at random, in the whole heavens, would be within so small a distance from each other, as the Pleiades are.*

* The computation of these probabilities are as follow: The distance between the two stars β Capricorni is something less than $3'\frac{1}{3}$; according to the rule above laid down therefore, if we suppose 230 stars equal to these in brightness, the probability that no two stars of that brightness will be found, any where in the whole heavens, within the distance of $3'\frac{1}{3}$ from each other, will be represented by the fraction $\left(\frac{6875.5^2 - 3.33\,\&c.^2}{6875.5^2}\right)^{230 \times 230}$. From twice the logarithm of 6875,5 [7.6746086] then, subtract twice the log. of 3.33 &c. [1.0457496] and the remainder 6.6288590 will be the log. of the number of times, that $3.33\,\&c.^2$ is contained in 6875.5^2, viz. 4254603 times, and consequently $\left(\frac{4254602}{4254603}\right)^{230 \times 230}$ will be equivalent to the former fraction. From the log. of 4254602, subtract the log. of 4254603, and the remainder will be $-$ .00000102, the proportional part of a unit in the number 4254603: this multiplied into 230 times 230, or 52900, gives $-$ .0053958, the log. of the whole quantity, which corresponds to the proportional part of a unit between 80 and 81; this quantity is therefore equivalent to the fraction $\frac{80}{81}$ nearly, the complement of which to unity is $\frac{1}{81}$.

In the Pleiades, the five stars Taygeta, Electra, Merope, Alcyone, and Atlas are respectively at the distances of 11, $19\frac{1}{2}$, $24\frac{1}{2}$, 27, and 49 minutes from the star Maia nearly. From 7.6746086, twice the log. of 6875.5, then, as before, subtract 2.0827854, twice the log. of 11 (the number of minutes between Taygeta and Maia) and the remainder 5.5918232 will be the log. of the number of times, that 11^2 is contained in 6875.5^2, viz. 390682 times; consequently a fraction, whose denominator is this number, and whose numerator is this number less by a unit, multiplied into itself 1500 times, will represent the probability, that no star out of 1500, scattered by chance in the

If, besides these examples that are obvious to the naked eye, we extend the same argument to the smaller stars, as well those that are collected together in clusters, such for example, as the Præsepe Cancri, the nebula in the hilt of Perseus' sword, &c. as to those stars which appear double, treble, &c. when seen through telescopes, we shall find it still infinitely more conclusive, both in the particular instances, and in the general analogy, arising from the frequency of them.

We may hence therefore, with the highest probability conclude, (the odds

whole heavens, would be within the distance of 11 minutes from the star Maia. From the log. of 390681 therefore subtract the log. of 390682, as in the former example, and the remainder will be — .00000111, the proportional part for a unit in the number 390682, which multiplied by 1500 will give us — .0016650 for the log. of the probability sought. In like manner from 7.6746086 subtract 2.5800692, twice the log. of 19½ (the number of minutes between Electra and Maia) and the remainder will be 5.0945394, the proportional part for a unit corresponding to the natural number of which will be —.00000349; and 1500 times this quantity, or —.0052350, will be the log. of the quantity, representing the probability, that no star out of 1500 scattered by chance would be within the distance of 19½ minutes from Maia. If we follow the same rule for the three remaining stars Merope, Alcyone, and Atlas, we shall find the similar logs. corresponding to these to be —.0076650, —.0100395, and —.0330300 respectively. The natural numbers corresponding to these five logs. taken in the same order, are .996173, 988018, .982506, .977148, and .926766, which severally express the respective probabilities, that no stars out of 1500 scattered by chance, would be within the same distances, at which the five stars above mentioned are found to be, from Maia. The complements of these quantities to unity .003827, .011982, .017494, .022852, and .073234, which severally express the respective probabilities, on the contrary, that such stars would be found within the distances above specified from the star Maia, must all be multiplied together, to determine the probability, that these events should all take place at the same time. The sum of the logs. of these numbers is — 9 + .1279139 or — 8.8720861, which is therefore the log. of all these numbers multiplied together; and subtracting this number from 0, or, what amounts to the same thing, changing the sign, we shall have the log. of the number of times that this quantity is contained in unity, that is, about 744880000 times; a fraction therefore, whose denominator is this number, and its numerator unity, will represent the probability in favour of all these events taking place together; and a fraction, whose denominator is the same number, and its numerator the same number less by a unit, will represent the probability of the contrary. But as this event might as well have happened to any other star as to Maia, we must multiply this last fraction into itself 1500 times, according to the supposed number of stars, to find the probability that it should not have happened any where in the whole heavens. Now the proportional part for a unit on this number is — .000000000005825, which multiplied by 1500 gives us —.00000087377, the proportional part for a unit in somewhat more than 496000.

But it must be observed, that this number is smaller than it ought to be on two accounts; for, in the first place, this method of computation gives only the probability, that no 5 stars would be within the distance above specified from a 6th, if they occupied the largest space they possibly could do, under that limitation; and secondly, we have made no allowance on account of the different magnitudes, which, if it had been attended to, would have given a somewhat greater result. These considerations however would have made the reasoning a good deal more intricate; and we have no need to descend to minutiæ, a difference in the proportion of 10 to 1 not at all affecting the general conclusion.—Orig.

against the contrary opinion being many million millions to one), that the stars are really collected together in clusters in some places, where they form a kind of systems, while in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation, or to some other law, or appointment of the Creator. And the natural conclusion from hence is, that it is highly probable in particular, and next to a certainty in general, that such double stars, &c. as appear to consist of 2 or more stars placed near together, do really consist of stars placed near together, and under the influence of some general law, whenever the probability is very great, that there would not have been any such stars so near together, if all those that are not less bright than themselves had been scattered at random through the whole heavens.

After what has been said, it will be natural to inquire, whether, if the stars in general collected into systems, the sun does not likewise make one of some system; and which are those, among the fixed stars, that belong to the same system with himself. Now supposing the stars of one system to be in general, and at a medium, of the same size and brightness with those of another, the number of stars of any one apparent magnitude, would bear the same proportion to the number of stars of any other apparent magnitude, as they would do, in case all the stars were scattered uniformly, and not in systems, provided the eye was not placed in or near to one of those systems. And in this case, the brightness decreasing as the square of the distance inversely, and the sphere, in which they are included, increasing as the cube of the distance directly, the number of stars of any one degree of brightness and upwards, should be as the cube of the square root of that brightness. Supposing then the faintest of the 2000 brightest stars to be less bright than the faintest of the first 70, in the proportion of about 30 to 1 (and I think the difference is not less than this) this number is smaller than we might have expected, if the sun was not one of a system, in the proportion of 2000 to about 12000 or 1 to 6. But Mr. M. lays the less stress on this argument, for want of a more certain determination of the proportion of light which we receive from the stars of different magnitudes.

If, however, on a more accurate examination, it should be found, that the quantity of light above assigned is not far from the truth, or, if the difference of light should be greater than it is above supposed to be, (in which case the argument will be still stronger); this will add a considerable degree of weight to the other arguments drawn from analogy, in favour of the sun's making one of a system of stars.

If we would now inquire, which are probably those stars, which compose part of the same system with the sun; though it will not be possible to point them out with certainty, yet there are some marks, by which we may, with great probability include some, and exclude others, while the rest remain more doubtful. Those stars which are found in clusters, and surrounded with many others at a small distance from them, belong probably to other systems, and not to ours. And those stars, which are surrounded with nebulæ, are probably only very great stars, which, on account of their superior magnitude, are singly visible, while the others, which compose the remaining parts of the same system, are so small as to escape our sight. And those nebulæ, in which we can discover either none, or only a few stars, even with the assistance of the best telescopes, are probably systems that are still more distant than the rest.

The Pleiades, as they appear to the naked eye, have been shown above to be probably a system by themselves; and if we examine them still further by means of telescopes, we shall find that they are surrounded with so large a number of smaller stars, as to increase the odds against the contrary opinion many millions to one. Now supposing the Pleiades to be in reality a system of stars, the probability is at least, Mr. M. supposes, a hundred to one that no one among them, of those visible to the naked eye, belongs to the same system with the sun; but that these are only such stars as are greater than the rest. The exact quantity of this probability depends on the number of stars, visible to the naked eye, belonging to this system; the proportion, that the space occupied by the Pleiades bears to the whole heavens; and lastly, how far the situation of any one of the Pleiades falls in with the general analogy of the stars composing this system, if any such general analogy should appear.

As the nebulæ, and smaller constellations, composed of a great number of stars, within a small distance from one another, belong probably to other systems; so those, which being placed at greater distances from each other compose the larger constellations, and such as have few or no smaller stars near them, when examined with telescopes, belong probably to our own system. Most of the stars of the first and second magnitude have this criterion to distinguish them, as belonging to the same system with the sun, besides several other circumstances, such as their greater brightness; the proper motions * that have been observed among some of them; their being more numerous than we might naturally expect, in proportion to the smaller stars, if they did not compose a part of the same system with ourselves, &c.

* The apparent change of situation that has been observed among a few of the stars, is a strong circumstance in favour of those stars being some of the nearest to us. This apparent change of situation may be owing either to the real motion of the stars themselves, or to that of the sun, or partly to the one, and partly to the other. As far as it is owing to the latter (which it is by no means improbable may in some measure be the case) it may be considered as a kind of secular parallax, which, if the annual parallax of a few of the stars should some time or other be discovered, and the quantity and direction of the sun's motion should be discovered also, might serve to inform us of the distances of many of them, which it would be utterly impossible to find out by any other means.—Orig.

Besides the brighter stars, it is probable there are many of those of the the smaller magnitudes also that belong to the same system with the sun. Now, if this should be the case, many of them being only fainter on account of their less real magnitude, we may stand the same chance of finding a parallax among some of these, as among the brightest ones, provided we pitch on such as happen to belong to our own system: to direct us with some probability to these, we have the circumstances above-mentioned of their composing larger constellations, and their having few stars lying very near them; and perhaps there may be still a little more reason to suspect that those stars form a part of the same system with ourselves, where, besides these circumstances, there have been observed changes of brightness, &c. among several of them in the same neighbourhood, such for instance as the changes which have been observed among several of the stars in the constellations of the Swan, Cassiopeia, &c. We may also venture to add to these, most of those stars that are of a redder hue than the rest, as it is probable that they are a good deal larger, in proportion to their brightness than the whiter stars [see the last note but two]. Many of them also have been observed to have a proper motion of their own, which, with several other concurrent circumstances, tends to make it highly probable that they are some of the nearest to us.

Having thus endeavoured to establish the probability that the sun is one of a system of stars, placed by themselves in this part of the universe, Mr. M. next inquires into some of the consequences of this hypothesis. Now if this is the case, and we suppose the whole number of stars, which belong to this system only (excluding those which belong to others), to amount to about 1000, we shall find it to be an even chance, that the parallax of the nearest among them does not exceed the parallax of one half that number, in a greater proportion than that of 9 to 1, supposing the earth to be placed in or near the centre of the whole system; nor in a greater proportion than that of about 12 to 1, supposing it to be placed very near the edge of the system; for supposing 1000 points to be placed within a sphere of any given magnitude, and that they are equally indifferent to every part of that sphere, if we assume any one of these points as a centre, we shall find, according to the known doctrine of chances, that there is an equal degree of probability, whether any one of the rest shall or shall not fall within a sphere described about the point assumed, of about 7 ten thousandths of the size of the whole sphere; but the radius of such a sphere is about $\frac{8}{90}$ or a little less than $\frac{1}{11}$ of the radius of the whole sphere, that is about $\frac{1}{9}$ of the radius of a sphere of half the size of the greater one; and therefore a sphere of about 9 times this radius will include half the greater sphere, if its centre be assumed near the centre of the greater sphere, and a sphere of 12 times this radius will include half the greater sphere, if the point be assumed almost in the surface of it.

If we assume the stars belonging to our own system to be about 1000; since they are in all respects similar to the sun, excepting perhaps such among them as are liable to frequent changes, and we have nothing to determine us to one magnitude rather than another, we may most reasonably suppose the magnitude of the sun to be a medium among the whole number. On this supposition, he will probably rank only with the stars of the 4th magnitude; and his light therefore, if he was removed to the medium distance of the other stars (viz. a distance equal to the radius of the sphere that would include half the stars of our own system) would hardly exceed a 200th part of the light of Sirius; and consequently, if the parallax of Sirius would be about one second, if he was of the same size and native brightness with the sun, it will be an equal chance that the parallax of one half of the stars, belonging to this system, is not less than one second divided by the square root of 200, that is a little more than 4‴; and it will likewise be an equal chance that the parallax of no one among them exceeds between 9 and 12 times that quantity, or a little more than ⅘ of a second.

If, instead of 1000 stars, we suppose the whole number, belonging to this system, to be only about 350, the sun will then, if he is of a medium size among them, rank probably with the stars of the 3d magnitude, and his light, at a medium distance, on this hypothesis, would be about a 50th part of that of Sirius. And therefore, according to the reasoning above, we should then find it an equal chance that the parallax of one of these 350 stars would not be less than about 8‴½; and there would be the same chance that the parallax of no one among them would be more than between 50‴ and about 1″. In the former supposition of 1000 stars; the apparent magnitude of the sun, when removed to the medium distance, &c. it seems not improbable that the largest star in the system may perhaps exceed the sun, in the proportion of about 1000 to 1; and in the latter supposition of 350 stars, &c. that it may perhaps exceed the sun, in the proportion of about 120 to 1. In whatever proportion the diameter of the sun is greater or less than the medium we have taken for it in the suppositions above, in the same proportion will the parallaxes be increased or diminished; and in the inverse triplicate of that proportion must their magnitudes be diminished or increased.

Let us now examine the circumstances of the Pleiades; and assuming the respective distances of the stars, composing that system, from each other to be, at a medium, equal to those of our own, let us see what will be the consequences of this supposition. Now if the Pleiades do not extend further in the direction of a line drawn between the earth and them, than in a direction at right angles to that line (which, from their composing a system, we have a right to suppose they do not), we can hardly allow the mean distance of those that are next to each other, among the 6 stars visible to the naked eye, to be greater than what would subtend an angle, if seen directly from the earth, of about 40 or 50 minutes. And consequently the distance between them and the earth would be about 70 times that distance, and their apparent brightness, seen from those that are next to each other, must be about 4900 times as great as it appears to us; but η of the Pleiades appears not fainter than Sirius in a greater proportion than that of about 100 to 1; this star therefore must appear brighter to the nearest of those 6, which are visible to the naked eye, than Sirius does to us, in the proportion of about 49 to 1. Let us now suppose all the stars belonging to the Pleiades, as well those discoverable with telescopes as those which are visible to the naked eye, to be contained within a sphere, whose apparent diameter at the earth is 2 degrees; and consequently the mean distance of them from a spectator placed somewhere among them, as it might happen, would subtend an angle, when seen directly from the earth, of about 1 degree. Since therefore we have supposed the distances of the stars of our own system to be, at a medium, equal to those of the Pleiades, and consequently their mean distances from the earth to subtend at the Pleiades an angle of 1°, we shall have the distance of the Pleiades about 57 times as great as the mean distance of the stars of our own system from the earth. Hence, if the largest of the stars of our own system should be at this mean distance from us, and Sirius should be the largest, η of the Pleiades must exceed it in the proportion of about 200 to 1; for removing Sirius to 57 times his present distance, his light would then be fainter than it is, in the proportion of 3249 to 1, that is, fainter than η of the Pleiades in the proportion of 32.49 to 1, supposing η of the Pleiades, as above, to afford us 100th part of the light of Sirius; but the magnitude of stars, supposing them equally luminous, and their distance to be given, is as the cube of the square root of their brightness; therefore η of the Pleiades, on this supposition, must be larger than Sirius, in the proportion of the cube of the square root of 32.49 (that is 185) to 1. But, according to general, and probably universal analogy, in all those nebulæ, in which we discover stars larger than the rest, these stars are placed towards the middle of their respective systems; and if therefore the same thing obtains with regard to our own system, this will make η of the Pleiades still something greater.

If the distance of the Pleiades is greater than the mean distance of the stars of our own system, in the proportion of about 57 to 1, it would be necessary in order to make stars of the same real magnitude among the Pleiades, equally visible to us with those of our own system, to take in a pencil of rays of a greater diameter than the pupil of the eye, in the same proportion: this, after proper deductions for the loss of light, could not well be effected by an object lens of less than 2 feet aperture. Now Dr. Hooke tells us, in his micographia, that with a telescope of 12 feet length, he discovered in the Pleiades 78 stars, and with longer telescopes many more; but if a telescope of 12 feet length, the aperture of whose object lens was probably less than 2 inches, increased the number of visible stars in the Pleiades to 78, we may well suppose that with an object lens of 2 feet diameter, they would amount to more than 1000. What this number would be, must depend however on the gradation of real magnitude among the stars of that system, to which there must necessarily be some limit, and it is not therefore improbable that observations of the increase of the number of stars among the Pleiades, &c. with telescopes of larger apertures, especially if this was carried on to very large sizes, might serve to inform us of many circumstances, both with regard to this gradation, and perhaps some other things, that would enable us to judge with more probability concerning the distances, magnitudes, &c. of the stars of our own system.

If we now imagine a spectator among the Pleiades to take a view of this system from thence, supposing the distance, as before, 57 times the mean distance of our own stars, we should appear to him as a nebulæ, in which there would be no star bright enough to be distinguishable by the naked eye; and with a telescope, the aperture of whose object lens was 2 inches, he would hardly be able to distinguish more than half a score stars at the utmost.

Having hitherto supposed the distances of the stars of our own system to be the same with those of the Pleiades, and examined the appearances according to that hypothesis, let us now, instead of their distances, suppose their magnitudes to be the same. This would make this system, as seen from the Pleiades, to subtend an angle of about 12 degrees instead of 2, and about half a score of the largest stars would be there visible to the naked eye; but a telescope, whose object lens was of 2 inches aperture, would in that case take in almost all the stars belonging to this system of the 4th magnitude and upwards. These appearances fall in less with the general analogy of what we see in the heavens, than the former supposition; but for want of more observations we cannot say this with any certainty: in the mean time however, till we have something further to go upon, it may not perhaps be amiss to take a kind of medium between the 2, and suppose the Pleiades to be at about 20 times the mean distance of the stars belonging to our own system, in which case η will exceed the largest of these, in the proportion of about 8 or 10 to one; or it will exceed the sun, according to our former suppositions of his being of a medium size among 1000 or 350 stars, in one case in the proportion of about 8 or 10 thousand, and in the other, about 1000 or 1200 to 1; its parallax in the former case being about 36'''', and in the latter about $1\frac{1}{4}''$.

Mr. M. concludes this inquiry with one observation more, concerning the appearance of the stars of our own system, as seen from great distances.

Whatever then the real distance and magnitude of these stars may be, provided we have not been greatly out in assigning the proportion of their light, in respect to that of the sun and one another, if they were to be seen from a distance at which the whole system would not subtend an angle of more than 6 or 8 minutes, it would appear only as a nebulæ, no single star being visible with perhaps any telescope, that has ever yet been made; for at this distance, the distance between the earth and the largest star of this system, not subtending an angle of more than about 3′, that is, about a 1200th part of the radius, the stars of this system must appear less luminous than they do to ourselves, in the proportion of the square of 1200, or 1440000 to 1. And supposing the light of Sirius to exceed that of the least visible star, in the proportion of about 1200 to 1, the brightest star therefore would still require to have its light increased in the proportion of 1200 to 1, before it would begin to be distinguishable. To do this, would require a telescope that should take in a pencil of rays of a larger diameter than the pupil of the eye, in the proportion of 35 to 1, that is, a pencil of about a foot diameter, exclusive of deductions; for the pupil of the eye is not less than a third of an inch in diameter, in a clear star-light night, when there is no moon; but to obtain such a pencil, we must not make use of a refracting telescope (with 2 lenses only) of less than 15 inches, nor a reflector of less than nearly 2 feet aperture. This may serve to show us, that those nebulæ in which we cannot distinguish any stars, may yet reasonably be supposed to consist of stars, though too far distant to be singly visible; since this would be the case with our own system, seen from as great a distance as we may well suppose those nebulæ to be from us, if we judge of it from the magnitude of the visible area which they occupy in the heavens.

Of the twinkling of the fixed Stars.

Having never yet seen any solution of the twinkling of the fixed stars, with which he could rest satisfied,* Mr. M. offers the following, which may not perhaps be found he thinks an inadequate cause of that appearance; at least it has undoubtedly some share in producing it, especially in the smaller stars. It is not, he thinks, unreasonable to suppose that a single particle of light is sufficient to make a sensible impression on the organs of sight. On this supposition, a very few particles of light, arriving at the eye in a second of time, will be sufficient to make an object visible, perhaps not more than 3 or 4; for though the impression may be considered as momentary, yet the perception, occasioned by it, is

* Some astronomers have lately adopted, as a solution of this appearance, the extreme minuteness of the apparent diameters of the fixed stars, which, they suppose, must, in consequence of this, be intercepted by every little mote that floats in the air; but, that an object should be able to intercept a star from us, it must be large enough to exceed the apparent diameter of the star by the diameter of the pupil of the eye; so that, if the star was a mathematical point, it must still be equal in size to the pupil of the eye.—Orig.

of a much longer duration: this sufficiently appears from the well-known experiment of a lighted body whirled round in a circle, which needs not make many revolutions in a second, to appear as one continued ring of fire. Hence then it is not improbable that the number of the particles of light, which enter the eye in a second of time, even from Sirius himself, may not exceed 3 or 4 thousand; and from stars of the 2d magnitude, they may therefore probably not much exceed 100. Now the apparent increase and diminution of the light, which we observe in the twinkling of the stars, seems to be repeated at not very unequal intervals, perhaps about 4 or 5 times in a second: why may we not then suppose that the inequalities, which will naturally arise from the chance of the rays coming sometimes a little denser and sometimes a little rarer, in so small a number of them as must fall upon the eye in the 4th or 5th part of a second, may be sufficient to account for this appearance? An addition of 2 or 3 particles of light, or perhaps of a single one on 20, especially if there should be an equal deficiency out of the next 20, would, he supposes, be very sensible: this seems at least probable from the very great difference in the appearance of stars, whose light is much less different than people are in general aware of; the light of the middlemost star in the tail of the Great Bear does not, he thinks, exceed the light of the very small star next to it, in a greater proportion than that of about 16 or 20 to 1; and Bouger tells us, in his Traité d'Optique before-mentioned, that he finds a difference in the light of objects of one part in 66 sufficiently distinguishable.

It will perhaps be objected that the rays coming from Sirius are too numerous to admit of a sufficient inequality, arising from the common effect of chance, so frequently as would be necessary to produce this effect, whatever might happen in respect to the smaller stars; but till we know what inequality is necessary to produce this effect, we can only guess at it, either one way or the other. There is however another circumstance, that seems to concur in the twinkling of the stars, besides their brightness, and this is a change of colour. Now the red and blue rays being very much fewer, he apprehends, than those of the intermediate colours, and therefore much more liable to inequality from the common effect of chance, may help very much to account for this phenomenon, a small excess or defect in either of these making a very sensible difference in the colour.

It will now naturally be asked, why the frequency of the changes of brightness should not be often much greater, as well as sometimes less, than that above-mentioned, and why the interval of the 4th or 5th, or some such part, should be pitched on rather than the 40th or 50th part of a second, or than a whole second, &c. for, according to the length or shortness of the time assumed, the changes that will naturally occur, from the effect of chance, will be smaller or greater in proportion to each other. The answer to this question will, he thinks, tend to render the above solution more probable, as well as to throw a good deal of light on the whole subject. The lengths of the times then between the changes of brightness, if he is not mistaken, depend on the duration of the perception before mentioned, occasioned by the impression of the light upon the eye, than which they seem to be neither much longer nor shorter. Whatever inequalities fall within a much shorter time than the continuance of this perception, will necessarily be blended together, and have no effect, but as they compose a part of the whole mass; but those inequalities which fall in such a manner as that they may be assigned to intervals nearly equal to, or something greater than the continuance of this perception, will be so divided by the imagination, which will naturally follow, and pick them out as they arise.

*XXVIII. Thermometrical Observations at Derby. By Mr. John Whitehurst.** p. 265.

We experienced a much greater degree of cold at Derby, in the late frost, than

* Mr. Whitehurst was born April 10, 1713, at Congleton, in Cheshire, where he inherited a small estate possessed by his ancestors from the conquest; and he died at London, the 18th of Feb. 1788; nearly at 75 years of age. Though his education was confined, and he was brought up to his father's business of clock and watch-making, in this, as well as in other mechanical and scientific pursuits, he soon gave indications of future eminence. On quitting his father, he set up for himself in the same line at Derby, where he made the town-clock at the town-hall, and the clock and chimes in the tower of All-saints-church in that town. While resident there, besides his constant attention to his own professional business, he was consulted as an engineer in most of the undertakings in the neighbouring country, where the aid of superior skill, in mechanics, pneumatics, and hydraulics, was requisite. Being appointed stamper of the legal money-weights, when the act passed in 1775, for regulating the gold coin, he removed to London; where, to the time of his death, his house became the common resort of the scientific and ingenious of all ranks and nations. In 1778, Mr. W. published the first edition of his "Inquiry into the Original State and Formation of the Earth;" an ingenious work, originating from his various scientific researches made in Derbyshire, while he resided in that county. He was elected a F. R. S. in 1779; of which he proved a very useful member, and he communicated some ingenious papers to the Phil. Trans. In 1787, he published "An Attempt towards obtaining Invariable Measures of Length, Capacity, and Weight, from the Mensuration of Time." This is founded on a standard of length, assumed from the measured difference in length, nearly 60 inches, between two pendulums, of which the one was 84 inches in length and vibrated 42 times in a minute, the other of 20 inches vibrating 84 times in a minute. Afterwards, till the time of his death, Mr. W. very much occupied himself in composing a treatise on Chimneys, Ventilation, and the construction of Garden-stoves; a work which was, after his death, published by his friend Dr. Willan. In short, Mr. Whitehurst was indefatigable in his researches after knowledge, in his endeavours to communicate it to others, or in devising and constructing useful machinery. But how respectable soever he was in mechanics, and those other parts of natural science which he more immediately cultivated, he was of still far higher account with his acquaintance and friends on the score of his moral qualities, being like the true Israelite indeed in whom there was no guile.

See a much fuller account of Mr. W.'s life, given by Dr. Hutton as an introduction to the edition of his works printed by him, with notes, in 1792.

perhaps was ever observed in England; and the quick transitions were no less remarkable. On Sunday the 18th of Jan. 1767, at 9 in the evening, the thermometer stood at 20. At half after 9, nearly one degree below 0. At 7 the next morning, 30, external air.

XXIX. An Attempt to Interpret the Legend and Inscription of a very Curious Phœnician Medal, never hitherto explained. By the Rev. John Swinton, B.D., F.R.S. p. 266.

On one side, this medal presents Jupiter sitting in a chair, with his eagle before him, a bunch of grapes in his right hand, and a sort of lance or rather staff in his left. Behind him the legend, BAAL TARZ, or BAAL TARS, formed of Phœnician letters, may be discerned; and the element B, inverted, is visible under the chair. On the other side is a lion seizing on, or rather tearing, a stag; over and under which the two Phœnician words, MIZRERAG MOTH, or MUTH, in their proper characters, seem clearly to appear. The workmanship of all the figures, but particularly of the lion and the stag, is finished in a high manner, and exquisitely fine. Several similar medals have been published by lord Pembroke, M. Morell, and M. Pellerin. From the two Phœnician proper names BAAL TARZ, or BAAL TARS, it seems highly probable, that the medal was struck at Tarsus, the capital of Cilicia, seated in a country abounding with wild beasts, particularly lions and stags, and famous for the birth of the great apostle St. Paul. For BAAL TARZ, or BAAL TARS, is equivalent to JUPITER TARSENSIS, JUPITER OF TARSUS, OF THE LORD OF TARSUS. Mr. S. endeavours to show that this Phœnician coin was struck at Tarsus, the capital of Cilicia, when the Parthians were masters of that country, about 40 years before Christ.

XXX. Remarks on the very Different Accounts that have been given of the Fecundity of Fishes, with fresh Observations on that Subject. By Mr. T. Harmer, p. 280.

The ascertaining the fecundity of the several species of fish, as far at least as we are able to do it, is one point necessary to the making our natural histories perfect; and at the same time opens a view wonderfully affecting to the imagination. The carp, in which Petit is said to have found 342,144 eggs; and the cod, in one of which of middling size Leuwenhoeck, it seems, affirmed there were 9,384,000, have been mentioned as very surprizing instances of this fecundity; and by their being selected by writers, apparently well versed in this part of learning, they seem to be the most memorable we have of this kind.

The accounts however that have been given of the fruitfulness of these two species of fish differ from each other very considerably. For Bradley, the Botanic Professor at Cambridge some years ago, tells us in his philosophical account of the works of nature, a book professedly written on a very celebrated, though unexecuted plan proposed by Mr. Addison in one of the Spectators, that the increase in some fish is surprizing, and to those that are not used to disquisitions of this kind must appear incredible; he however sinks the number of eggs in these two species extremely, when he tells us the roe of a cod-fish must contain about a million of eggs, and that a carp does not spawn less than 20,000; to which he adds, and perhaps a tench half as many. This is making the cod almost 10 times less prolific than the other account, and the carp above 17 times less. Some other writers, who appear also to have been desirous to impress the mind with the wonders of natural history, have made their estimate still lower. One of them, in particular, in one of our monthly publications, whence numbers must take their ideas, who have no opportunity of reading more authentic accounts elsewhere, tells us, that carp and perch have 9 or 10 thousand eggs, and that cod-fish, and herring, are not less prolific; and this he calls wonderful. The increase of cod-fish is indeed, even according to this, very great, but almost 1000 times less than Leuwenhoeck is said to have found it.

Their accounts being so very different, Mr. H. thought he should not improperly employ some leisure hours, if he inquired into this matter afresh, and saw what the fecundity of these species of fish really was, as well as of such other sorts as might fall in his way; and especially as he had found that a small pickerel, whose spawn he had taken a pretty exact account of, from mere curiosity, some time before, contained no fewer than 25,800 eggs: a fish which none of these authors had mentioned, and of which notwithstanding a small one had produced a larger number of eggs than Bradley himself had assigned to the carp, which has been always considered as remarkable for its prolific quality, not to mention the unknown writer, who makes its fecundity much less.

Mr. H.'s way of research, was to weigh the whole spawn very exactly; then to take a piece weighing 20, 30, or 40, or more grains, as was most convenient, and after weighing that parcel with care, and giving the turn of the scales to the weights, not to the eggs, to tell them over very carefully; and then by dividing the number of eggs by the grains, to find how many eggs there were in each grain, or nearly so. He says nearly, for there must, according to this method, have been rather more; but he chose to estimate them after this manner, that there might be no danger of representing the fecundity of these animals greater than the truth. He often boiled the portions of spawn that he told, and after macerating them some hours in water, gently pressed them with a penknife, whose point he afterwards made use of to number them distinctly, by separating them from each other to greater distances, after they were rendered by that gentle squeezing fitter for telling over. The weights he reckoned by are Avoirdupois weights; but there being no weights of that sort small enough to answer all purposes, he was obliged to make use of grains along with them, of which he reckoned 437½ make an ounce avoirdupois.

The table, at the close, gives all particulars, in a very short compass. The first column contains the names of the fish he examined; the 2d their weight; the 3d the weight of their spawn; the 4th their fecundity; and, as he supposed some persons might be desirous to know how large a portion of spawn he weighed in each case, he set down the number of grains in each such portion, in a 5th; the number of eggs found in a grain, by this method is seen in a 6th, by which we may give some guess at the different sizes the eggs of each species are of, when they are excluded; and he made the time of examining each fish respectively a 7th; which possibly may be of some use to those who may have an inclination to repeat any of these observations, as from thence may be learnt something concerning the most advantageous time of examining these creatures, which certainly ought to be as nearly as we can, when the eggs are come to their full size, and before any of them are deposited. However, after all, if his notion is just, that some species deposit a part of their eggs come to their full growth, before others laid the same year are large enough to be told with distinctness, the accounts of the fecundity of such fish must be extremely defective; this he apprehends, among those he examined, is the case of mackarel, carp, tench, and others; in the eggs of herring, &c. there does not appear such a difference

From this table it appears, that the size of the eggs is nearly the same in great and small fishes of the same species, at the same time of the year; that the quantity of spawn is, usually, nearly proportionate to the size of the animal; whence we may give a tolerable guess at the greatest fecundity of each species, if we know to what weight they have been found to grow, while in a breeding state; we may likewise settle their produce at a medium, on learning what is the mean size of each species when in the forementioned state; but we see however that this is not universal, nor consequently perfectly exact, some fish being much more prolific than others of the same size and species.

To conclude, the great fecundity of fish is not the only thing that affects the imagination, when we are examining matters of this sort: the extreme disproportion between their size when they first appear in the water after hatching, and that of their full-grown state, as well as the little proportion that is to be observed between the bulks of fish of different species and that of their eggs, are things that are very amazing to persons of a curious turn. The egg of a smelt, which at its full growth weighs but 2 or 3 ounces, appeared, in those he examined, to be larger than those of a cod-fish, which weighed 18 or 20 pounds, and might have grown to double that bulk; and that of a stickle back, which is the smallest of all known fish, was found to be above 6 times larger than the largest he ever observed in a smelt. What becomes of such amazing numbers of young fish, and why some are made so extremely prolific, the flounder and crab in particular among the smaller sorts, would doubtless be highly entertaining subjects, if duly illustrated.

THE TABLE.

1 Names of the Fish.		2 Their weight.	3 Weight of the spawn.	4 Fecundity.	5 The portion of sp. wgd.	6 No. of eggs to a grain.	7 Time of exam.
Carp	Nº 1	16oz. 12dr.	1265gr.	101,200	46 gr.	80	May 25
	2	25 8	2571	203 109	55	79	April 4
Cod-fish		—	12,540	3,686,760	29	294	Dec. 23.
Flounder	Nº 1	2 14	182½	133,407	23	731	Feb. 21
	2	3 8½	152	225,568	19	1484	Dec. 18.
	3	6 12	598	351,026	26¾	587	March 14.
	4	24 4	2,200	1,357,400	24½	617	Ditto.
Herring	Nº 1	4 3	367	32,663	48	89	Oct. 8, 1763.
	2	5 0	236½	21,285	48½	90	29.
	3	3 13	259	23,569	52½	91	Oct. 2, 1764.
	4	5 10	480	36,960	53	77	25.
	5	4 6½	366	29,646	57	81	Ditto.
	6	4 8	420½	27,753	51	66	Nov. 3.
	7	5 1	490½	32,863	41½	67	Oct. 18.
Lobster	Nº 1	14 8	—	7,227	—	14	April 4.
	2	36 0	1671	21,699	129	—	Aug. 11.
1	Nº 1	20 0	1027	454,961	33	443	June 20 1764
	2	20 0	949	430,846	24½	454	29.
	3	18 0	1223½	546,681	32½	447	18 1765
Perch	Nº 1	8 9	765½	28,323	85	37	April 5.
	2	5 10	502	20,582	85	41	6.
Pickerel	Nº 1	56 4	5100½	49,304	70	9⅔	April 25.
	2	—	3248	80,388	76½	24¾	Nov. 25.
	3	48 10½	3184	33,432	43	10½	March 19.
Prawn	Nº 1	(127 gr.)	—	3,806	—	243	May 12.
	2	(94½ gr.)	—	3,479	—	287	Ditto.
	3	(100½ gr.)	—	3,579	—	247	Ditto.
Roach (or what I took to be of that species)	Nº 1	2 0	114	9,604	—	—	April 4.
	2	6 8	671	43,615	68	65	May 4, 1764.
	3	3 8	346½	29,799	42½	86	Ditto.
	4	2 2	153	9,486	42½	62	5.
	5	10 6½	361	81,586	39	226	2, 1765.
	6	9 10¾	417	113,841	42	273	6.
	7	3 8	213½	45,475	20	213	24.
Shrimp (with light coloured spawn)	No 1	(17½ gr.)	3	3,057		1000	May 3.
	2	(39 gr.)	7	6,807		972	Ditto.
	3		—	4,601		—	Ditto.
Ditto (with dark colour)	Nº 1	(31 gr.)	5	4,090		818	Ditto.
	2	(22 gr.)	4	2,849		712	Ditto.
Smelt	Nº 1	2 0	149½	38,278	30	256	Feb. 21.
	2	(289½ gr.)	50	14,411	—	288	Mar. 21, 1764
	3	1 14	157½	29,925	40½	190	27, 1765
	4	1 12	145½	30,991	20	213	28.
	5	1 7	149	24,287	20	163	Ditto.
	6	1 5	136	23,800	20	175	Ditto.
Soal	Nº 1	14 8	542½	100,362	20	185	June 13.
	2	5 0	179½	38,772	20	216	28.
Tench	Nº 1	40 0	—	383,252	—	—	May 28, 1764
	2	28 8	533½	280,087	25	525	3, 1765
	3	8 14¾	224	83,104	20	371	10.
	4	9 8	284½	108,963	20	383	Ditto.
	5	12 8	366	138,348	22¼	378	Ditto.
	6	27 9½	1969	350,482	23	178	June 11.
	7	14 15	866	138,560	20	160	Ditto.

XXXI. Of a Hydro-enterocele, appearing like a Hydro-sarcocele, and ending in the Death of the Patient, in which the Intestine had passed from the Hernial Sac, into that of the Hydrocele by which the Strangulation was formed. By M. Le Cat, F.R.S., &c. p. 293.

J. Chiquet, aged 65, was admitted into the hospital Jan. 24, 1767. The account which he gave of himself was, that he had been accustomed to a rupture, which he had not been able to reduce for a fortnight past, and that since 8 days he had been seized with a vomiting, and was incapable of taking any nourishment. On examination, the tumor was soft, especially at the upper part towards the ring, which seemed to be so free and disengaged, that the finger with the integuments might be pushed under it: the large cord, which came down to it, was flat, soft, and appeared to be composed entirely of the spermatic vessels enlarged. The extremity of the swelling, which was of the size of a large orange, was evidently a very transparent hydrocele; at the basis of which some hard points were to be felt, which were thought to be schirrous tumors. Mr. Le C. concluded therefore that his complaint was an old hernia, succeeded by a sarcocele and a hydrocele, and that the intestine was at that time returned. He imagined that the vomitings, which were not frequent, might be caused by some other disorder, perhaps by the progress which the sarcocele might have made in the cavity of the abdomen; and he also thought that the weak and almost dying state the man appeared to be in, was a prognostic of the fatal manner in which those cases usually terminate; for his strength was so far exhausted, that he expired in the following night.

Mr. Le C. was very desirous of examining the case, having been always induced to suspect, from the vomitings, and the flatness, softness, and size of what passed through the ring, that there was a descent of the intestine. On opening the common hernial sac, a large portion of intestine presented itself, which was very flaccid, and almost empty; but what surprized him most was, to find that the convoluted extremity of this intestine had insinuated itself into the sac of the hydrocele, which was formed by the vaginal coat of the testicle, and that only this portion of intestine was strangulated, hard, and changed in colour. The redness was so slight, that this strangulation could scarcely have been the immediate cause of the death; but he rather imagined that a universal decay, and waste of strength, for a long time past, had contributed to hasten this event.

XXXII. Specimen of some New Experiments in Electricity. By John Baptist Beccaria, F.R.S. p. 297.

For Mr. B.'s improvements in this science, see his general Treatise on Artificial Electricity, translated into English, and published for J. Nourse, in one large vol. 4to, in 1776.

*XXXIII. Specimen of the Natural History of the Country about the River Wolga. By J. R. Forster.** p. 312.

The region of which the natural history is here given, lies on both sides of the river Wolga, and extends from 48 to 52 degrees of north latitude, bordering on Astrakhan and the Kuban Tartars, where it runs into the Caspian sea. But though this paper is written in a manner which shows the author to have been well-informed on the subject; yet it was judged unnecessary to give an abstract of it, as the information which it conveys may be easily collected from books of travels and other sources.

XXXIV. An Algebraical Problem, and on the Evolution of a certain Mechanical Curve, among Infinite Mechanicals, which is solved by Determinate Equations. By Pius Fantoni, Professor of Philosophy and Mathematics at Bologna. p. 358.

Such mechanical curves, and their solutions, as treated of in this paper, are of endless variety; and being devoid of any particular object of interest or usefulness, it is not considered as of importance to be retained in this Abridgment.

XXXV. On the Most Advantageous Construction of Water-wheels, &c. By Mr. Mallet of Geneva. From the French. p. 372.

SECT. I. The construction of such machines is very simple: they consist of several planes inserted into the same axle placed horizontally above the surface of the water, and in a position perpendicular to the stream. These planes, called float-boards, by yielding to the action of the stream, cause the axle on which they are fixed to turn round, by means of several wheels, which take into each

* John Reinhold Forster, a celebrated linguist and naturalist, and a Prussian by birth, was born in 1729; and he died in 1798; consequently at 69 years of age. He was an ingenious, though an eccentric and imprudent man; which occasioned his frequent migrations from one country to another. In 1748 he entered the university of Halle, in the duchy of Magdeburg, where he studied divinity. He next removed to Dantzic, and commenced preacher. He afterwards went to Russia, in expectation of some considerable preferment, where probably he composed the above specimen of the natural history of the Wolga. But being disappointed there, he next came to England, where for some time he acted as tutor in the French and German languages at Warrington. In 1772 he accompanied Capt. Cook in one of his voyages round the world, having procured an appointment as botanist on that voyage. In 1775 he returned to England, and was honoured by the university of Oxford with the degree of LL.D. Afterwards having published, contrary to the engagement entered into with government, before the voyage, a botanical account of the plants discovered in that voyage, he was treated with such universal coolness, that he left this country, and repaired again to Halle, where he was appointed professor of natural history. In this country he was author of Observations made in a Voyage round the World; and a History of Voyages and Discoveries in the North; also a tract on the Byssus of the Ancients; and several ingenious papers in the Phil. Trans. See also accounts of Dr. Forster, in the Gent. Mag. for 1799, p. 166; and in the Monthly Mag. for 1800, p. 934.

other, and give motion to the part destined to produce some purposed effect, as the mill-stone in a corn-mill. The size of the float-boards, the velocity with which the wheel is to turn, and the number of float-boards to produce the greatest possible effect, are 3 main things proposed to be examined in the following inquiry. In the first place, Mr. M. supposes the total resistance which this wheel has to encounter, on the part of the machine, and which hinders it from moving so swift as the stream, to be expressed by a weight π, suspended to the extremity of a cord fixed to the circumference of a wheel whose radius is d, and which has the same axle as the float-board wheel, so that the effect of the stream is to raise the said weight π, as expressed in fig. 8, pl. 10. He also supposes, that the stream, by its velocity, moves through v feet in one second of time, and that this velocity is the same, though at different depths.

§ II. After these suppositions, the first thing that presents, is to determine what should be the size of the float-boards, for the stream to be capable of raising the weight π with a certain determinate velocity. Let AA BB, fig. 9, be one of the float-boards let into the axle AA, and placed vertically in the water, so as to receive the perpendicular impulse of the stream. Its horizontal length BB $= b$ feet, its vertical height AB $= a$ feet, the velocity of the wheel at the point B, such that it shall run through z feet in a second; n pounds the weight of a cubic foot of water; and suppose the impulse of the stream on a plane perpendicular to it is (as Dr. Daniel Bernoulli has stated in his Hydrodynamica) equal to the weight of a prism of water, whose base is the plane, and its altitude the generating height of the velocity with which the plane is impelled. This being supposed; let AP $= x$, Pp, its differential, $= dx$, which will give the velocity of the float-board at the point $p = \frac{x}{a}z$, and the relative velocity of the stream with which the plane is impelled at the same point $= v - \frac{x}{a}z$, whose generating height is $\frac{1}{60}(v - \frac{x}{a}z)^2$ feet; whence we have the weight of the parallelopiped of that height, and of the base PP pp equal to $\frac{nbdx}{60} - (v \frac{x}{a} z)^2$, pounds, which weight multiplied by the length AP (x) of the lever which tends to turn the plane, gives $\frac{nbxdx}{60}(v - \frac{x}{a}z)^2$ for the total effect of the stream on the little rectangle PP pp, whose integral is $\frac{nb}{60}(\frac{1}{2}vvxx - \frac{2}{3}vz\frac{x^3}{a} + \frac{1}{4}zz\frac{x^4}{aa} - \frac{1}{2}vvff + \frac{2}{3}vz.\frac{f^3}{a} - \frac{1}{4}\frac{zzf^4}{aa})$, putting AC $= f$ for the distance between the axle and the surface of the water when the float-board has only its part CB plunged in the water; which, putting $x = a$, becomes $\frac{nb}{60}(\frac{1}{2}vv - \frac{2}{3}vz + \frac{1}{4}zz.\ aa - \frac{1}{2}vvff + \frac{2}{3}\frac{vzf^3}{aa} - \frac{1}{4}\frac{zzf^4}{aa})$, which will express the effect on the whole plane CCBB, equal πd, the product of the weight π by the length d of the lever on which it acts in opposing the motion of the wheel.

§ III. If the wheel be plunged as deep as its axle, that is, $f = 0$, the equation is changed into this $\frac{1}{60} nbaa\ (\frac{1}{2}vv - \frac{2}{3} vz + \frac{1}{4}zz) = d\pi$, where it appears 1°. That the quantities d, π, v and z remaining the same, we have b inversely proportional to the square of a; whence it follows, that if the length b is to be diminished, without altering the effect of the float-board, the height a must be increased proportionally to the square root of b; for example, if b is to be made 4 times less, it will be sufficient to double the height a. 2° That the velocity of the float-board remaining still the same, the weight π will be in the compound ratio of the length b, and of the square of the height aa. 3° The dimensions of the float-board remaining, the more the quantity z is increased, the more must the weight π be diminished. If z be made $= 0$, we have $\pi = \frac{nbaa}{60d} \cdot \frac{1}{2}vv$, and if $z = v$ we have $\pi = \frac{nbaa}{60d} \cdot \frac{1}{12}vv$, that is 6 times greater than in the first case; which is very conformable to the nature of things; for when the wheel is in motion, the stream then not acting on it but with the excess of its velocity above that of the wheel, it follows, that the greater such velocity is, the more will the effect of the stream be diminished.

It follows from our last remark, that the greatest weight with which the stream can constitute an equilibrium, will be $= \frac{nbaavv}{120d}$; but then the wheel will not have any motion, nor consequently the weight π: if the float-board be increased, or the weight diminished, from that instant the wheel will begin to turn, and the swifter as the float-board is greater, or the weight less; but in most machines, it is required that the weight may be the greatest possible, as also the velocity with which it is raised. A question therefore here offers itself, whose solution is of much importance. What must be the velocity of the float-board whose dimensions are given, that the product of the weight by its velocity shall be the greatest possible?

§ IV. The velocity of the weight π is $\frac{d}{a}$ z feet in a second, which being multiplied by the value of $\pi = \frac{nbaa}{60d} (\frac{1}{2}vv - \frac{2}{3} vz + \frac{1}{4}zz)$ gives the product $\frac{1}{60} nba\ (\frac{1}{2} v^2z - \frac{2}{3} vzz + \frac{1}{4}z^3)$, which must be a maximum; for which purpose make $\frac{1}{2}vvdz - \frac{4}{3} vzdz + \frac{3}{4}z^2dz = 0$; whence we have $z = \frac{8 - \sqrt{10}}{9} = 0.53752v$: this value of z being substituted, makes the equation $\frac{1}{2}vv - \frac{2}{3}vz + \frac{1}{4}zz = \frac{11 + 2\sqrt{10}}{81} vv$, so that we have the equation $baa = \frac{11 + 2\sqrt{10}}{4860} + \frac{d\pi}{nvv} = 280.529 + \frac{d\pi}{nvv}$, which expresses the dimensions of the float-board when the effect will be the greatest possible. If the float-board be plunged no deeper than to cc, as we have at first supposed, the most advantageous value of z may be determined in the same manner, which will be found to be

$$z = \frac{8a^4 - 8af^3 - a\sqrt{(10a^6 + 54a^4f^2 - 128a^3f^3 + 54a^2f^4 + 10f^6)}}{9(a^4 - f^4)}v.$$

If $f = 0$. this value of $z = 0.537v$
$f = 0.25a = \frac{1}{4}a \quad = 0.498v$
$f = 0.3a \quad = 0.486v$
$f = 0.5a = \frac{1}{2}a \quad = 0.436v$
$f = 0.7a \quad = 0.390v$
$f = 0.9a \quad = 0.353v$
$f = a \quad = 0.333v$

By the inspection of these different values it appears, that this value of z diminishes as the plunged part is greater, and that this velocity can never exceed the quantity $0.537v$, nor be less than $\frac{1}{3}v$.*

This value of z, and of its square zz, being substituted in the general formula, § II, we shall obtain from it the following equation:

$$\frac{60d\pi}{nbvv} = \frac{11a^6 - 27a^4f^2 + 32af^5 - 27a^2f^4 + 11f^6 + 2(a^3 - aff)\sqrt{(10a^6 + 54a^4f^2 - 128a^3f^3 + 54a^2f^4 + 10f^6)}}{81(a^4 - f^4)}$$

which, for a given relation between f and a, will show the breadth b for producing the greatest effect.

As the extremity of the float-board must have a certain velocity depending on the relation of the height a to the plunged part, and as the velocity of the weight $\pi = \frac{d}{a}z$, it follows that if we should increase the velocity of π, we must diminish the height a and increase the breadth b, so that the product baa and the relation of f to a may be the same as before. For example, if the wheel be plunged as deep as the axle, to double the velocity of the weight, the height of the float-board must be reduced one half, and its breadth be quadrupled.

§ V. It may so happen, that the channel on which the wheel is placed shall be so shallow and narrow, as not to allow the float-boards the necessary dimensions for raising the weight with a convenient velocity. In this case we are obliged to raise the axle of the wheel above the surface of the water so much, that the lever on which the stream acts may be long enough to recompense the smallness of the float-boards. Herein it is necessary to solve the following problem. The breadth b and the height a, of the float-board AB being given; to find the radius CA (r) of the wheel which shall cause the weight π to ascend with

* If in the value of z we make $f = a$, we have $z = \frac{0}{0}$, which obliges us to take, according to the common method, the differentials of the numerator and of the denominator, considering f as variable, and the relation of these differentials, will give the value of z: but on account of the radical quantity, the calculus being somewhat tedious, and again bringing out $z = \frac{0}{0}$, and that after several similar operations, it is better to have recourse to the equation from which the value of z was deduced; this equation is $\frac{3}{4}avz.\frac{a^3 - f^3}{a^4 - f^4} - \frac{1}{2}aavv.\frac{a^2 - f^2}{a^4 + f^4}$, which by the above operation will be $\frac{3}{4}zz = vz - \frac{1}{4}vv$, and $z = \frac{1}{3}v$.

the velocity $\frac{d}{r+a}z$. The exact solution of this problem might be deduced from the formula, § II, which would render the operation tedious, the equation being of the 4th degree; but it may be rendered much simpler by a supposition which is but little wide of the truth when AB is but small in comparison of CA; and this is, to consider all the points of the float-board AB as affected with the same velocity z.

Let CP, fig. 10, $= x$, we shall have $\frac{1}{60}nb(v-z)^2 fl.xdx$ for the effect of the portion AP, and $\frac{1}{60}nb(v-z)^2 \times (ar + aa)$ for the effect of the whole float-board AB. This quantity must be made equal to $d\pi$, and then, just as in the foregoing cases, such a value of z be sought, that the weight π and its velocity may be the greatest possible; that is, the differential of $z(v-z)^2$ must be made $= 0$, which gives $z = \frac{1}{3}v$. Therefore $d\pi = \frac{nabvv}{270}(2r + a)$, and $r = \frac{270d\pi - naabvv}{2nabvv}$, and the velocity of the weight π will be $= \frac{2ndabvv}{810d\pi + 3nbaav} \times v$.

§ VI. We have seen that the calculus was much simplified by supposing one of the velocities constant for all points of the float-board. For this velocity being c, the effect of the whole float-board will be simply $\frac{1}{60}nb(vc)^2(aa - \frac{1}{2}ff)$. It will therefore not be unuseful to inquire what this velocity c must be, that the effect of the float-board may be the same, as supposing, as we have hitherto done, a variable velocity, and proportional to the distances from the axle, we have only to make

$$\frac{nb}{60}(v-c)^2 \cdot \left(\frac{aa-ff}{2}\right) = \frac{nb}{60}\left(\tfrac{1}{2}vv(aa-ff) - \tfrac{2}{3}vz\frac{a^3-f^3}{a} + \tfrac{1}{4}zz\frac{a^4-f^4}{aa}\right),\ \text{§ IV;}$$

by the equation whence we got the value of z § IV, $\frac{1}{2}vv - \frac{4}{3}\frac{vz}{a}\cdot\frac{a^3-f^3}{a^2-f^2} + \frac{3}{4}zz.\frac{aa+ff}{aa} = 0$, we shall have $c = v\sqrt{\frac{1}{2}vv - \frac{1}{4}zz.\frac{aa+ff}{aa}}$.

If $f = 0$, we have $zz = 0.288vv$ and $c = 0.345v$,
$f = \frac{1}{2}a$ $zz = 0.190vv$ $c = 0.336v$,
$f = a$ $zz = \frac{1}{9}vv$ $c = \frac{1}{3}v$.

so that whatever be the relation of f to a, the velocity c is ever nearly $= \frac{1}{3}v$, and the more exactly so, as f is greater. Therefore we may always assume $\frac{1}{270}nbvv.(aa - ff) = d\pi$ for the effect of the stream on a float-board whose plunged part is $a - f$; this effect will be increased in the ratio of 4 to 9, when the wheel has no motion, for making $c = 0$, we find it $= \frac{1}{120}.nbvv.(aa - ff)$.

§ VII. Hitherto we have all along supposed that the float-board, through its whole plunged part, received the perpendicular impulse of the stream; but it is easily understood that the wheel, coming to turn, presents to the stream the plane of the float-board under an angle which is continually varying, which diminishes its effect every instant as it removes from the vertical: this inconvenience may be remedied by multiplying the number of the float-boards, so that when the first is removed from the vertical as far as a certain point, the next may occupy that advantageous place, to be in its turn replaced some time after by a third, and so on. Now our third inquiry is, to assign the angle contained between two float-boards, or, which comes to the same, the number of float-boards the wheel should consist of, that its effect may be the greatest possible; being of no less importance than the preceding ones. To begin then with the most simple case; we will suppose the wheel immoveable, or that $c = 0$, and proceed to investigate, whether, supposing the number of float-boards to be greater, the sum of the effects will come out greater or less than what results from one single float-board placed vertically.

In order to a general solution of this question, we will suppose 2 float-boards CD and CE, fig. 11, making any angles with the vertical; and let us compare the effect of the single float-board GD, with the effect resulting from the float-boards FE and GD taken together, which will be reduced to FE and OD, because the part OG becomes useless, as the stream is intercepted by FE. Let CB = CD = CE = a, CA = f, cosin. BCD = m, cosin. BCE = μ, which gives CG = $\frac{f}{m}$ CF = $\frac{f}{\mu}$ and CO = ma. Then we shall find, by § VI, the effect of GD = $\frac{1}{140}$ $nbvv$ $(mmaa - ff)$, that of OD = $\frac{1}{140}$ $nbvv$ $(mmaa - \mu\mu aa)$, and that of FE = $\frac{1}{140}nbvv$ $(\mu\mu aa - ff)$; whence it appears, that the sum of the last two is exactly equal to the first, which will ever hold good whatever be the value of f.

Whence arises the following theorem: "Whether the wheel be plunged quite up to the axle, or only in part so, provided it be immoveable, and that one of its float-boards be placed vertically, its effects will be constantly the same, whatever be the number of float-boards opposed to the stream, even though it were infinite." The latter part of this theorem, though flowing from the general demonstration, may be also demonstrated, immediately, in the following manner: let BP, fig. 12, be = x, we have

MO = $\frac{adx}{a-x}$, and CO = $\frac{aa - dx - ax}{a-x}$; CO^2 = $\frac{a^4 - 2a^3x + a^2x^2 - 2a^2dx + 2aaxdx}{aa - 2ax + xx}$, neglecting the dx^2, and aa − CO^2 = $\frac{2aaxdx}{a-v}$; therefore the effect of the stream on OM, which is = $\frac{1}{140}$ $nbvv$ $(aa$ − CO$^2)$ $\frac{\text{CP}^2}{\text{CP}^2}$, will become = $\frac{1}{140}nbvv(2ax - 2xdx)$, whose integral is = $\frac{1}{140}$ $nbvv$ $(2ax - xx)$, where putting $x = a - f$, we have $\frac{1}{140}$ $nbvv$ $(aa - ff)$ for the total effect of the stream on the wheel; which is the same as that of a single float-board AB in a vertical position.

§ VIII. This theorem will also hold true for the case of § V, where we have supposed the height of the float-boards very small, in comparison of the radius of the wheel; we have seen that the effect of a single float-board placed vertically was = $nabvv$ $(2r + a)$; the demonstration of the preceding § will be applicable here after the same manner, and will show that whatever be the number of float-boards, the effect will be ever the same. It does not however follow that the number of float-boards should be indifferent; for the wheel coming to turn the float-board, its lower part, which received the perpendicular impulse, will no longer receive it otherwise than obliquely, and the effect will diminish till the angle formed by two neighbouring float-boards be bisected exactly by the vertical, which will render the first entirely useless; after which the effect will increase anew, and will become again greatest when the 2d float-board is got to the vertical; so that in order to fix on the most advantageous number of float-boards, regard must be had to the sum of the different effects for all the situations of the float-boards during one whole turn of the wheel. Whence it follows, that in this case, wherein they are supposed very small, the greater their number is, the greater will be the sum total of the effects; since, if that number were infinite, there would be a float-board in a vertical position every instant.

§ IX. This will no longer hold good, if the height of the float-boards be more considerable, and it be found necessary to take the different velocity of their different points into consideration; by comparing, fig. 11, the pressure on FE with that on the portion GO, they will be found no longer equal, as in the foregoing case; it is true that the same quantity of fluid acts on these two planes, and the disadvantage which FE has by receiving the impulse more obliquely, is exactly compensated, as before, by the length of the lever, but the difference arises from the different velocity of the corresponding points of FE and GO; those velocities are in the ratio of CF to CG, or as cos. ACG to cos. ACF, which shows that the effect of FE is always less than that of GO, and consequently the effect must be diminished, by adding a greater number of float-boards: the said effect will be greatest when there is only one float-board placed vertically, and least when their number is infinite: let us inquire what it will be in this latter case. We will suppose the same, fig. 12, and the same denominations as in § VII. We had $\text{co} = \frac{aa - ax - adx}{a - x}$, we shall have, (neglecting dx^2, dx^3, and dx^4), $aa - \text{co}^2 = \frac{2aadx}{a - x}$, $\frac{a^3 - \text{co}^3}{a}$, and $\frac{a^4 - \text{co}^4}{aa} = \frac{4aadx}{a - x}$. Now the pressure on OM is, by § II, $= \frac{1}{120} nb \frac{(a-x)^2}{aa} . [vv\,(aa - \text{co}^2) - \frac{4}{3} vz . \frac{a^3 - \text{co}^3}{a} + \frac{1}{2}\,\text{zz} . a^4 - \frac{\text{co}^4}{aa}]$; which, (by putting for co its value) will become $= \frac{1}{120} nb\,(2adx - 2xdx) . (v - \text{z}^2)$, whose integral $\frac{1}{120} nb\,(2ax - xx) . (v - \text{z}^2) = \frac{1}{120} nb\,(v - \text{z}^2)\,(aa - ff)$, making $x = a - f$, will express the effect resulting from an infinite number of float-boards: this least effect will be to the greatest, that is when there is but one float-board, as $(v - \text{z}^2) : vv - \frac{4}{3}\frac{vz}{a} . \frac{a^3 - f^3}{aa - ff} + \frac{1}{2}\frac{\text{zz}}{aa} . (aa + ff)$, or as $(v - x^2) : \frac{1}{2}vv - \frac{1}{4}\,\text{zz}\,(\frac{aa + ff}{aa})$, § VI.

This ratio will be that of 1 : 2 . . . . if $f = 0$

1 : 1.485. . . . . $f = \frac{1}{2}a$

1 : 1 . . . . $f = a$.

§ x. If we take nothing but the most advantageous position into consideration, and preserve the greatest effect entire, it follows that the angle BCD, fig. 13, between two float-boards, must be such, that E should enter the water at the instant when AB quits the vertical, so that the cosine of that angle be $= \frac{f}{a}$; in consequence of which the following table may be constructed, showing what the number of float-boards should be, for a given ratio between f and a.

For 4 float-boards, we have	$f = 0$.	For 10 float-boards, we have	$f = 0.8090a$.
5	0.3090a.	12	0.8669a.
6	0.5000a.	14	0.9009a.
7	0.6236a.	16	0.9239a.
8	0.7071a.	18	0.9397a.
9	0.7660a.	20	0.9510a.
&c.	&c.	&c.	&c.

§ XI. Certain authors, treating of hydraulics, have in this part of it given the same table, as containing the true number of float-boards the wheel should consist of; but we have seen on what principle it was formed, and that it was only to preserve entirely the effect of the vertical float-board; whence it follows not that the number of float-boards which it assigns should be the most advantageous. To which purpose the effect produced from every position of the wheel, and for the different number of the float-boards, should be computed; the number which gives the arithmetical mean between all these effects, the greatest of all, will be that to be chosen, and preferred before what the above table indicates. It may be sufficiently satisfactory to compute only the effect from 1 to 10 degrees: thus, for example, for the wheel entirely plunged we are to find the effect, fig. 14, 1° on OA, 2° on OI and gb, 3° on OH, and fc, 4° on OG and hd, 5° on OF, and pe, 6° on OE, 7° on OD, 8° on OC, and 9° on OB. After which the wheel returns into the same position it had at first; and we are to divide the sum of all these effects by 9, to get the arithmetical mean.

We will next suppose the number of 6 float-boards for the same case of $f = 0$, and compute the following effects: 1° on OG + aA, 3° on OE + nc, 5° on OI + γC, 2° on OF + mb, 4° on OD + nc, 6° on OH + bB. The sum of all these effects divided by 6 will give the effect of the wheel of 6 float-boards.

The same thing, supposing the angle 40 degrees, or 9 float-boards, and as after a revolution of these 40 degrees, the wheel returns into a similar position, the same must be divided by 4. Then for an angle of 30 degrees we are to divide by 3, and so on.

Mr. M. made this computation to great exactness, for the case of $f = 0$, $f = \frac{1}{2}a$, and $f = 0.866\ a = a \cos. 30°$; the result, 1° if $f = 0$, for 4 float-boards, the arithmetical mean $= 0.335\ (\frac{1}{30} nbaavv)$.

It may be observed in this first case, that there is some advantage in taking 6 float-boards instead of 4, shown by the table; the effect will be increased in the ratio of 100 to 118, and yet will be more than about $\frac{92}{100}$ of the greatest effect above calculated for a single vertical float-board; so that the found dimensions must be a small matter altered, and the quantity *baa* increased by $\frac{1}{10}$.

For 6	= 0.396 &c.	For 6 flo.	0.277 &c.
9	= 0.336 &c.	7	0.281 &c.
12	= 0.323 &c.	9	0.285 &c.
18	= 0.295 &c.	12	0.284 &c.
an infinite number	= 0.214 &c.	18	0.276 &c.
		an infinite number	0.238 &c.

2° if $f = \frac{1}{2}a$.

In this second case, 9 float-boards are to be taken, instead of 6 shown by the table, though the difference will be but very small; and we shall have an effect which will be $\frac{86}{100}$ of that of a vertical float-board, and in that ratio that the quantity *baa*, found by the above formulæ, must be increased.

3° If $f = 0.866\, a$,

for 12 fl.	= 0.099 &c.
18	= 0.099 &c.
36	= 0.104 &c.
an infinite number	= 0.103 &c.

In this third case, the difference is still very small, and the effect resulting from 36 float-boards, will be $\frac{93}{100}$ of the effect of a single vertical float-board.

XXXVI. A New Method of Constructing Sun Dials, for any given Latitude, without the Assistance of Dialing Scales or Logarithmic Calculations. By James Ferguson, F. R. S. p. 389.

This new method is given, improved, at p. 95 of the author's Select Exercises, published in 1773.

XXXVII. On the Formation of Islands. By Alex. Dalrymple, Esq. p. 394.

There is not a part of natural history more curious, or perhaps to a navigator more useful, than an inquiry into the formation of islands. The origin of islands in general, is not the point to be discussed; but of low, flat, islands in the wide ocean: such as are most of those hitherto discovered in the vast South-sea. These islands are generally long and narrow, they are formed by a narrow bar of land, inclosing the sea within it; generally, perhaps always, with some channel of ingress at least to the tide, commonly with an opening capable of receiving a canoe, and frequently sufficient to admit even larger vessels.

The origin of these islands will explain their nature. What led Mr. D. first to this deduction, was an observation of Abdul Roobin, a Sooloo pilot; that all the islands, lying off the N. E. coast of Borneo, had shoals to the eastward of them. These islands being covered to the westward by Borneo, the winds from that quarter do not attack them with violence. But the N. E. winds, tumbling in the billows from a wide ocean, heap up the coral with which those seas are filled. This, obvious after storms, is perhaps at all other times imperceptibly effected. The coral banks, raised in the same manner, become dry. These banks are found of all depths, at all distances from shore, entirely unconnected with the land, and detached from each other: though it often happens that they are divided by a narrow gut, without bottom.

Coral banks also grow, by a quick progression, towards the surface; but the winds, heaping up the coral from deeper water, chiefly accelerate the formation of these into shoals and islands. They become gradually shallower, and when once the sea meets with resistance, the coral is quickly thrown up by the force of the waves breaking against the bank; and hence it is that, in the open sea there is scarcely an instance of a coral bank having so little water that a large ship cannot pass over, but it is also so shallow that a boat would ground on it. Mr. D. has seen these coral banks in all the stages; some in deep water, others with a few rocks appearing above the surface, some just formed into islands, without the least appearance of vegetation, and others, from such as have a few weeds on the highest part to those which are covered with large timber, with a bottomless sea at a pistol shot distance.

The loose coral, rolled inward by the billows in large pieces, will ground, and the reflux being unable to carry them away, they become a bar to coagulate the sand, always found intermixed with coral; which sand, being easiest raised, will be lodged at top. When the sand bank is raised by violent storms, beyond the reach of common waves, it becomes a resting place to vagrant birds, whom the search of prey draws thither. The dung, feathers, &c. increase the soil, and prepare it for the reception of accidental roots, branches, and seed, cast up by the waves, or brought thither by birds. Thus islands are formed: the leaves and rotten branches, intermixing with the sand, form in time a light black mould, of which in general these islands consist, more sandy as less woody; and when full of large trees, with a greater proportion of mould. Cocoa nuts, continuing long in the sea without losing their vegetative powers, are commonly to be found in such islands; particularly as they are adapted to all soils, whether sandy, rich, or rocky.

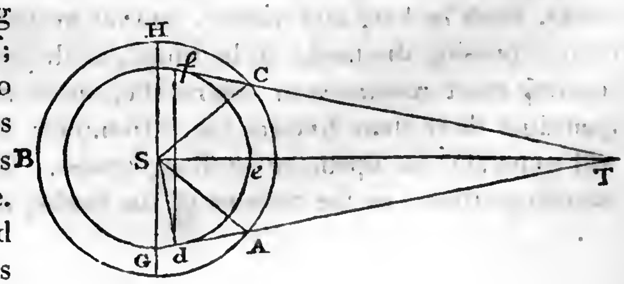
The violence of the waves, within the tropics, must generally be directed to two points, according to the monsoons. Hence the islands formed from coral banks, must be long and narrow, and lie nearly in a meridional direction. For even supposing the banks to be round, as they seldom are when large, the sea, meeting most resistance in the middle, must heave up the matter in greater quantities there than towards the extremities: and, by the same rule, the ends will generally be open, or at least lowest. They will also commonly have soundings there, as the remains of the banks, not accumulated, will be under water. Where the coral banks are not exposed to the common monsoon, they will alter their direction; and be either round, or extend in the parallel, or be of irregular forms, according to accidental circumstances.

The interior parts of these islands, being sea, sometimes form harbours capable of receiving vessels of some burthen, and he believes always abound greatly with fish; and such as he has seen, with turtle-grass and other sea-plants, particularly one species, called by the Sooloos gammye, which grows in little globules, and is somewhat pungent as well as acid to the taste. It need not be repeated that the ends of those islands, only, are the places to expect soundings: and they commonly have a shallow spit running out from each point. Abdul Roobin's observation points out another circumstance, which may be useful to navigators; by consideration of the winds to which any islands are most exposed, to form a probable conjecture which side has deepest water; and from a view which side has the shoals, an idea may be formed which winds rage with most violence.

XXXVIII. An Attempt to determine the Height of the Sun's Atmosphere from the Height of the Solar Spots above the Sun's Surface. In a Letter to Mr. J. Ellicot, F. R. S. from the Rev. Mr. Samuel Horsley, F. R. S. p. 398.

I return you many thanks for your obliging communication of the observations of the late transit of Venus by Mayer and Rohlius. The phenomena which they relate of the atmosphere of that planet are highly curious. They were perhaps the more interesting to me, as they confirmed some conjectures of my own concerning the great height of the atmosphere of the sun, and of those of the two nearer planets. I once attempted to make a rough comparison between the height of the sun's atmosphere and that of our own, by comparing the height of the solar spots above the surface of the sun with that of our clouds above the surface of the earth, which I did in the following manner.

The inclination of the sun's equator to the place of the earth's orbit is so small, that in this inquiry I think it may safely be neglected; and I consider the two planes as one. Let T be the centre of the earth, S that of the sun. Join TS, and let def be a great circle of the sun's sphere, formed by the intersection of the plane of the earth's orbit with the sun's surface. Let ABC be the circle described by the revolution of a spot. From T draw Tf and Td touching the circle def in f and d, and cutting ABC, in C and A. Join df; through S draw HSG, parallel to df. Join SC, SA, Sd. The spots are hid behind the sun 3 days longer than they are visible. That is, they are hid 15 days, and are seen only 12. The earth's



motion in 15 days is $14^{\circ}\ 17'$. The spots traverse the like area in $1^{d}\ 0^{h}\ 50^{m}$ nearly. Hence, if the earth stood still, the spots would be hid only $13^{d}\ 23^{h}\ 10^{m}$, and their whole sidereal period being $25^{d}\ 5^{h}$ they would be visible $11^{d}\ 5^{h}\ 50^{m}$, and the time of their occultation would exceed the time of their appearance by $2^{d}\ 17^{h}\ 20^{m}$. Hence the arc AC is less than the arc ABC, by the motion of $2^{d}\ 17^{h}\ 20^{m}$, that is, by $38^{\circ}\ 52'\ 56''$. And the semicircle being a mean arithmetic between AC and ABC, AC will be less than the semicircle by half as much; that is by $19^{\circ}\ 26'\ 28''$. Hence each of the angles GSA, HSC is $9^{\circ}\ 43'\ 14''$. The angle bsd = dTS = $16'\ 1''\ 27'''$. Therefore dSA is $9^{\circ}\ 27'\ 12''$. Hence SA = 1.013767 such parts as sd is 1.

The distance therefore of these spots from the centre of the sun, is 1.013767 semidiameters of the sun, and their distance from his surface is, in decimal parts of his semidiameter, .013767. Hence it is evident that the height of the solar spots above the surface of the sun, is above 54 such parts, as bear each to the sun's semidiameter, the proportion of one Paris mile to the semidiameter of the earth, which is that of 1 to 3923 nearly. The height of our atmosphere is generally reckoned about 50 miles. That of the lowest clouds fall short of one mile. The whole height of our atmosphere therefore is, at least, 50 times that of our highest clouds. If the whole height of the sun's atmosphere bear as large a proportion to the height of these solar spots or clouds (and I think the proportion is likely to be much larger), the height of the sun's atmosphere is not less in proportion to his semidiameter, than 54 times that of the earth's, and exceeds two-thirds of his semidiameter, being in decimal parts thereof .68835.

The probability seems to be, that the height of the sun's atmosphere is almost double of this; for I question whether the mean height of our clouds exceeds $\frac{1}{2}$ a Paris mile. The solar spots therefore are 108 times as high in proportion; and then, supposing as before, that the whole height of the sun's atmosphere bears the same proportion to the height of his spots, as the whole height of our atmosphere to the mean height of our clouds, the sun's atmosphere will be 108 times as high in proportion to his semidiameter, as ours is, and will rise to the distance of more than $\frac{4}{3}$ of his semidiameter from his surface. Let philosophers consider, whether these indications of the vast height of the sun's atmosphere give any degree of probability to a conjecture of Sir Isaac Newton's, that the dissipation of the sun's substance, which might be expected to ensue from his intense heat, may in great part be prevented by the prodigious pressure of the incumbent atmosphere.

The height of the atmosphere of Venus is considerably greater, according to the observations of Mayer and Rohlius, than they imagined. Rohlius follows Cassini in the estimation of the sun's apogee semidiameter, which Cassini over-rated by 3′ 45‴. This quantity therefore is to be added to the height of Venus's atmosphere (15.5) as stated by Rohlius; which makes the true height 19″.25, that is above $\frac{1}{5}$ of the diameter of the planet. I cannot but reflect with some degree of national triumph, on the great part that our own country may justly claim, in many of the most curious discoveries in all parts of the world. Mr. Meyer generously confesses how much he stood indebted to English artists. You told me that it is your intention to present that curious tract to the Royal Society. You may likewise communicate this if you think it contains any thing worthy of their notice.

XXXIX. Observations of the Sun's Eclipse, Aug. 16th, 1765, taken at Caen, in Normandy. By Nathl. Pigott. p. 204.

Eclipse begins true time at	3^h	48^m	16^s	hence the mid. was at.......... 4^h 24^m 36^s
end..............at	5	4	$56\frac{1}{2}$	and greatest phase obs. at....... 4 18 39
mid..............at	4	24	36	
dur. ....at	1	12	$40\frac{1}{2}$	whence the ecl. incr. for.......... 5 57 of

time, in which the sun's diam. illum. decreased 36″ 14‴;..............

therefore from the sun's diam. illuminated at....................	4^h	18^m	39^s	=	27′	4″	35‴
take the decrease in..	+	5	57	=	0	36	14
leaves the diameter of the sun, at the middle......................	4	24	36	=	26	28	21
which taken from the mean diam. measured					31	43	20
gives					5	14	59

the quantity of the eclipse, or segment of the diameter eclipsed, which is 1 digit and 59′.15 of a digit, or $1\frac{59}{60}$ = 2 digits nearly. This eclipse was observed with an achromatic refractor of 6 feet, and a micrometer made by Dollond. The weather very fine.

The times, as computed from the tables at the end of M. De la Lande's Astronomy.

								difference.
Beginning at	3^h	48^m	24.6^s	observ. at 3^h	48^m	16^s	....	0^m $8^s.6$
Middle..........................	4	25	11.0	 4	24	36	....	0 35.0
End	5	1	57.2	 5	0	56.5	....	1 0.7
Duration	1	13	32.6	 1	12	40.5	....	0 52.1

Also the latitude of the moon was, by observation, 16″ greater than the tables gave it.

XL. Extract of a Letter from John Ellis, Esq. F. R. S., to Dr. Linnæus, of Upsal, F. R. S., on the Animal Nature of the Genus of Zoophytes, called Corallina. p. 404.

I have now finished a collection of that genus of zoophytes, which you call corallina; and with the assistance of Dr. Solander, have made a description of each species: I have taken care to dissect them minutely, and to pass them in review under his eye in the microscope, in order to establish a true general character of this genus. I have attended more particularly to examine the nature of these bodies, in order to confute the opinion of some late writers on zoophytes, who, for want of good microscopes, and a proper care in chemically analysing them, have asserted that they were mere vegetables.

The first of these is Dr. Job Baster, of Zealand, who, in the Philos. Trans., vol. lii. p. 111. (p. 537, vol. xi. of these Abridgments) asserts that the corallines of Linnæus, which he says he has accurately examined, are most evidently true plants of the genus of conferva; because there are no polypes coming out of their tops, and that they have seed inclosed in their cells like other marine plants. But, as another part of this letter is intended for an inquiry into this new discovery of Dr. Baster's, that corallines are confervas; a thing never known even to Mr. Ray, Dr. Dillenius, or any other botanist, I shall now proceed to Dr. Pallas, of Berlin, who has lately resided in Holland, and who has taken great pains in collecting every thing that has been written on the subject of zoophytes, whence he has compiled a book called Elenchus Zoophytorum, where he has ranged the several genera and species of this class of beings in a systematical order. When he comes to the genus of Corallina, he says, p. 418, "They are to be left to the botanists, as they belong to the "vegetable kingdom; but makes this apology for inserting them, lest his book "should be thought imperfect, as Linnæus and Ellis have ranked them as "zoophytes in their works."

He begins with observing, that they come not near to any one genus of zoophytes, either in their structure or chemical principles; that some species have a peculiar appearance, some approach to fucuses, many are like confervas; but that all of them are very distinct from them, and from all vegetables, on account of their lapidescent substance. That they differ in their chemical principles from zoophytes; for when they are burnt, they smell like vegetables: and that, according to Count Marsigli's Experiments, Hist. Mar. p. 73, they neither contain a volatile salt, nor animal oil. That the pores in their calcarious substance, are too small for polypes to inhabit them; and that the pores of fucuses prove them to be as much animals as the corallines, even when their pores are rendered more visible, by having the calcarious substance, that surrounds them, dissolved by an acid. That the great Jussieu, in his diligent researches after marine productions, could see no visible token of life in them. That Mr. Meese, who has lately written a Flora Frisica, has found a coralline growing on a heath in Friesland; which Dr. Pallas says, is a strong proof of their vegetable origin. Lastly, that their fructification is so nearly analogous to those of fucuses and confervas, that he likewise takes that to be a proof of their belonging to the vegetable kingdom.

To proceed then.—Dr. Pallas after telling us that corallines are vegetables, says, that some of them are like fucuses. In this I must agree with him; because his first corallina, which he calls corallina pavonia, is truly of that genus of plants: this most elegant fucus I have particularly described and figured (Essay on Corall. p. 88, T. 33, fig. c, d, e,); it is well known by the name of turkey-feather fucus, and is called, in the Species Plant. p. 1630, Fucus Pavonius. What could have led Dr. Pallas into this mistake? most probably those beautiful farinaceous semicircular stripes on it, which he must have taken for a lapidescent or calcarious substance, one of the most distinguishing characters of a coralline, even according to his own description of this genus. If he had tried this farinaceous substance with an acid, he might observe that it would not ferment: it is of the same nature with the farina that covers many plants, for instance the prima auricula, and almost all the lichenes foliacei and fruticulosi, or liverworts. As to their similitude to the conferva, the contrary will appear, as soon as I come to give the proper definitions to both these, and the corallines. In the same paragraph he says, that the corallines do not come near to any genus of zoophytes.

How far he is mistaken in this assertion, I will endeavour to prove from the following experiments. Break a thin piece from the corallium Anglicum, Essay on Corall. T. 27, N. 1, c, (millepora calcarea, Pallas Elench. p. 265) or of the corallium lichenoides, Essay on Corall. T. 27, N. 2. d; both which, Dr. Pallas, in his Elench. p. 265, has confounded together under the name of millepora calcarea (but which he confesses to be animal); and when you examine them in the microscope, you will find in them both regular series of cells, as figured in Essay on Cor. Tab. 27 fig. D. Split at the same time one of the joints of the corallina officinalis of Linnæus lengthwise, and you will find the series of cells (see pl. 11, fig. 12 and 13), correspond in shape exactly with both the former; which I think proves the organization of these bodies to be the same, and consequently animal.

Besides, compare the structure of the miriozoon of Donati, Phil. Trans. vol. 47, p. 107, tab. 5. (Abridgment, vol. 10, plate 5, millepora truncata, Pallas Elements, p. 249), with those of the corallina rosarium, and corallina incrassata, both which I have carefully dissected and figured in pl. 11, fig. 15, 20, &c. and there appears so great an affinity between their cells, and even in the opercula of the corallina incrassata, that it affords us reason to conclude with great probability, that their mouths, or suckers, are the same. It cannot be amiss to mention the similitude there is between the stony-jointed corallines, and the isis hippuris, or jointed black and white East Indian coral, and the cellularia salicornia, Pallas Zooph. p. 61, or bugle coralline, Essay on Coral. T. 23, which last two are universally allowed to be animals: in all these are found the same kind of fibres that connect their joints, and exactly in the same manner. To prove that these corallines have a smell very different from vegetables, I must appeal to an experiment made publicly before the Society of Arts, Commerce, &c. and which gave them a satisfactory demonstration of the great difference in nature between corallines and vegetable substances. It happened on the following occasion. A gentleman of Wales had sent the society a parcel of lichen tartareus, of Linn. Ed. 2, sp. pl. 1608, as a proper material for dying a red colour, to answer the same purpose of that expensive article among the dyers, called orchell, or canary weed, which is the lichen roccella of Linn. sp. pl. 1622.

As the object was of consequence, the society was very desirous of being fully informed of the nature and appearance of this useful dye: and therefore several curious gentlemen of the society were desired, against the next meeting, to bring some specimens of true orchell. Accordingly some specimens were obtained from the orchell dyers in Southwark, and laid before the society. At the same time Dr. Maningham, a member of that society, produced before the society a specimen, in a paper with orchell written upon it, from Mr. Miller of Chelsea, likewise as the true orchell: but, on examining it, it proved to be the corallina nervo tenuori fragiliorique internodia nectente of Sir Hans Sloane's History of Jamaica, vol. i. tab. 20, fig. 4. Some disputes arising on the different appearance of the specimens, I took the liberty to inform the gentlemen present, that, having lately made some experiments on corallines, I believed that Mr. Miller's specimen was a coralline, or animal substance, and the lichen roccella, or dyers' orchell, was a vegetable; and in order to convince the society of the difference I called for a lighted candle, and having first set fire to the lichen roccella, it yielded the same smell that burnt vegetables usually do; but when the coralline, which was Mr. Miller's specimen, was burnt, it filled the room with such an offensive smell, like that of burnt bones, or hair, that the door was obliged to be opened, to dissipate the disagreeable scent, and let in fresh air.

Another argument that Dr. Pallas offers the world of the vegetable nature of corallines, or rather a proof of their not being of an animal nature, are Count Marsigli's chemical experiments on the corallina officinalis, (Hist. Mar. p. 73), where he says it neither contains animal oil nor volatile salts. But, to prevent such plausible arguments from misleading mankind, I determined to have fair and accurate experiments made on this substance. Accordingly I applied to Mr. Peter Woulfe, F. R. S., a gentleman distinguished for his great knowledge in chemistry; and in order to have the specimens fresh from the sea, I applied to a worthy member of this society, the earl of Hillsborough, for Mr. Potts, the secretary to the Post-Office, to procure me a sufficient quantity of the corallina officinalis from the sea-coast near Harwich: this parcel, about 2 months ago, I sent to Mr. Woulfe; and in answer have received the following letter, with an account of his experiments made on it.

"SIR,—I took 12 ounces Troy of the corallina officinalis, picked clean from every extraneous substance, and put it into a clean stone-coated retort; the retort was set in a reverberatory furnace, and an adopter and quilled receiver luted to it: the fire was very gentle for the first 8 hours; in which time, half an ounce and 18 grains of a transparent and almost colourless liquor came over, which was set aside for examination. The fire was then increased, and in 6 hours time there were distilled 2 drams and 3 grains of a turbid liquor, which had some appearance of oiliness on its surface; this was likewise set apart to be examined. The fire was then increased for 6 hours longer, and during the last 2 hours the retort was quite red hot all over, which ended the distillation. In this 3d and last process the portion of liquor that came over was more turbid than the 2d, and some of it, from the redundancy of its volatile alkali, was crystallized; it also contained rather more than a dram of light empyreumatic oil, very much resembling the smell of hartshorn; in the recipient there were also some crystals of a volatile alkali. The whole of this last product weighed 3 drams and a half. The caput mortuum was quite black, and weighed 10 ounces, 1 dram, and 1 scruple; so that there was a loss of 4 drams and 49 grains out of the 12 ounces of coralline.

The first liquor that distilled slightly effervesced with spirit of salt, and changed syrup of violets green; certain proofs of a volatile alkali. The 2d and 3d portion effervesced strongly with spirit of salt, as did also the volatile salt that came over into the receiver; evident marks of its being a concentrated alkali. Here I must observe, that had this distillation been conducted in a hurry, there would have been no concrete volatile alkali; for then this would have been confounded and dissolved in the first liquor that came over.

Had there been a sufficient quantity of this coralline, I should first have proposed to have taken off the calcarious substance, by an acid menstruum, and afterwards washed the membranaceous part so clean from the acid, as not to change the syrup of violets red. Then the distillation of this part alone would have afforded a much larger proportion of empyreumatic oil, and volatile alkali, and but a very small quantity of caput mortuum."

Doctor Pallas proceeds to prove that corallines cannot be animals, as the pores of their calcarious substances are too minute for any polypes to harbour in. These words of the doctor's seem to imply, as if the coralline substances were only habitations for detached polypes, and not part of the animals themselves. How this affair stands, I hope to have clearly demonstrated long before this, for I have plainly seen, and endeavoured to show mankind, that the softer and harder parts of zoophytes are so closely connected with one another, that they cannot separately exist; and therefore have not hesitated to call them constituent parts of the same body, and that the polype-like suckers are so many mouths belonging to it. Now, for the smallness of the pores, which the doctor has mentioned here, among the corallines, to be a contradiction to animal life; he certainly has forgot one circumstance, when he introduces the corallium pumilum album (Essay Cor. T. 27, *f. c.*) or his millepora calcarea (Pall. Elench. p. 265) as an animal, which is, that he there says, it has absolutely no pores at all.

As there can be no doubt, but every part of what is called coralline is necessary to make out such an animal, or being, it will be very difficult, if not almost impossible, to determine the proportion there ought to be between softer and harder parts; and therefore it cannot be thought unreasonable to say, that in some of this tribe the stony parts are by much the greater part of the whole, especially as doctor Pallas's objection can be only against the crust, or lapidescent part, as the inside of many of them is far from being hard, being exactly like a sertularia, so that I do not know if it would not be a good definition to one well acquainted with that tribe to say, a coralline is a sertularia covered with a stony or calcarious crust; if the mouths should happen to be very small, their number may make up that deficiency. We see in the greatest number of corallines their surface full of holes; we saw the same in escharas and milleporas 30 years ago; since that time magnifying glasses have been improved, so as to show us, that they are all orifices, for polype-like suckers; why should not we now admit that glasses may be still more improved, so as even to make us able to see what may be the intention and use of these minute orifices, which according to all rules of reasoning, we must suppose to approach in nature to those they are most alike. From this extreme minuteness then of the pores of these millepora, confessed to be zoophytes, as well as those of corallina officinalis, as before mentioned, it is no great matter of surprize, that Doctor Jussieu could not perceive any animal life in the corallines, nor Doctor Schlosser in the millepora calcaria. As these experiments ought to be attended with many convenient coinciding circumstances, that do not often happen to persons who only go to the seaside, perhaps for a few days, or hours; so that it is unreasonable to conclude, because they have been unsuccessful, that more accurate observers may not be more fortunate at another time.

I believe I shall be justified in this, by many essays that have been made, by persons of judgment, to observe the polype-like suckers in many, even of the sertulariæ, which they have several times attempted in vain; I must own it has often happened to me in many species, and yet I have not the least doubt of their being true sertulariæ from the similarity there is in their habit and form to others of the same genus; and of this fact I am sure Doctor Pallas is fully convinced.

Another argument made use of by Dr. Pallas, to overthrow the animal existence of corallines, is taken from Mr. Meese's assertion, that he had found on Bergummer heath in Friesland, a substance of the same nature with the corallines. Meese, in his Flora Frisica, p. 75, calls it a lichen; but Dr. Pallas has ventured, in his Elench, p. 427, to rank it among the corallines, under the name of corallina terrestris. See the figure of it in pl. 11, fig. 28. In this Dr.

Pallas is in the right, as I have had an opportunity of examining a small specimen, that my worthy and learned friend Dr. Schlosser of Amsterdam was so kind to procure me: but how such a nice and accurate philosopher as Dr. Pallas could let it escape him to consider the nature and quality of this subject, and how much it differs from any thing else growing on the land, is a thing that surprises me. It only being mentioned by Mr. Meese, as found on Bergummer heath, ought not to have satisfied him so far, as to declare a body with a calcarious crust to be a land production, when no such thing in the whole vegetable kingdom has ever been found; it has always been thought quite the contrary, that a stony or hard substance of that nature, could not be produced but from an animal, and chiefly those that live under water. This should certainly have made him minutely inquire in what manner it was found, if buried under moss, loose on the ground, or perhaps near some of the canals, which communicate with the sea. Many accidents might have brought it thither, which is more probable than to imagine nature to go out of her usual track. It is not improbable that that part of Holland has been overflowed by the sea, and this production left there when the water subsided, or blown there by a storm, which I beg leave to believe till I am better informed. I do not in the least doubt of Mr. Meese's veracity; but as that gentleman was more intent on discovering vegetables than animals, and thinking this very like a dry lichen fruticulosus, he scrupled not to believe it to be one of that tribe; and therefore perhaps neglected to observe all those circumstances that we now wish to be informed of.

The irregular pedunculated figures or fructifications (as Dr. Pallas pleases to call what is represented in pl. 11, fig. 29) seem to be rather a defect in the growth of the ramifications, especially as they differ from each other in shape, and some of them appear beginning to form other branches. In fig. a the whole consists of two opposite curled processes, with a small cavity between them at the top; this cavity is filled up at fig. b, so that the top becomes rounded; in fig. cc there seems to be a beginning of a continuation lengthwise; and in fig. d it is still more plain the beginning of a branch. If the inside of these processes had been hollow, and the outside of a regular figure, I should not have hesitated to consider them to be the ovaries of the coralline; but as they are solid, and of the same structure with the rest of corallines, I shall rather call them defective branches.

Dr. Pallas's last argument to prove that corallines are vegetables is, that the nodules or tubercles, which he has observed in corallines, contain little seeds subanalogous, or somewhat resembling those we find in the fructification of the fucuses and confervas. If this method of reasoning should hold good, what will become of the cellularias, sertularias, and millepora calcarea et agariciformis, with many other zoophytes, that have such roundish ovaries; they must be recalled to the vegetable kingdom, notwithstanding all doubt about their being living animals has been long laid aside.

I come now to Dr. Baster, who carries this matter still farther, and says positively in Philos. Trans., vol. 52, p. 111, (Abridgment, vol. 11, p. 537) that the corallines are true confervas; and in his Opuscula Subseciva, (vol. 1, tab. 1, fig. 3, A and B) he refers us to the figure of the corallina rubens in seed; which, he says, is a true conferva; but the figure is so bad, that I am persuaded nobody can find out what he means to represent by it.

I shall therefore conclude this letter, with recommending to these ingenious gentlemen, to analyse these bodies chemically, and with care; and also to view them with the same attention that I have done, in the microscope; if so, I am persuaded they will be of our opinion. I must defer the sequel of what I intended to another day, which was to give you an account of the discoveries I have made in the fructification of the confervas; these, I flatter myself, will fully convince Dr. Baster of the great difference between these two bodies, and that they belong to two different kingdoms of nature.

Description of plate 11.—Fig. 1, the miriozoon of Donati, or millepora truncata of Pallas.—2. the end of a branch magnified, to show the situation of the pores.—3. The same cut perpendicularly through, to show the trumpet-like suckers in their cells, connected with the middle tubes.—4. The horizontal section of the same, with the suckers extended.—5. The magnified drawing of one of the suckers, with its cells and operculum.—6. The oblique view of the opening of the cell with the sucker and operculum.—7. The cell with the operculum open.—8. The cell covered with its operculum.—9. The corallium lichenoides of Ellis's corallines, with ovaries on it.—10 and 11. The natural and magnified size of a piece of this coral, to show the arrangement of the inside of the cells, which are just the same as in the following.—12. The order of the cells, in a joint of the corallina officinalis, to show the great affinity between them.—13. The natural size of a small piece of the corallina officinalis.—14. The milk-white millepora calcarea, from the Mediterranean, where, though the pores are not visible on the outside, the arrangement of the cells in the inside are the same with the corallium lichenoides, and corallina officinalis.—15. The corallina rosarium, or white-bead band-string of Sloane's Hist. of Jamaica, tab. 20, fig. 3.—16. Two joints magnified, one to show the situation and figure of the pores, and the other to show how the suckers pass from the middle cartilaginous tube through the calcarious covering to the surface.—17. Four of the suckers, and the ovary between them, magnified highly.—18. The ovary.—19. One of the eggs taken out of the ovary.—20. The corallina incrassata, from the West Indies.—21. One of the joints of its natural size.—22. The same magnified a little, to show its pores in its calcarious surface.—23. Part of the inside tubes of the joint, of their natural size.—24. The same magnified, to show the openings of the cells on the surface, connected together.—25. A perpendicular section of half of one of these joints.—26. The same magnified, to show the figure of the vessels leading to the suckers in the calcarious surface.—27. A piece of the calcarious surface highly magnified, to show some of the pores open, and others covered with their convex opercula; letter a shows the figure of one of the trumpet-shaped suckers highly magnified.—28. A small branch of Meese's coralline supposed to grow on a heath, called by Dr. Pallas corallina terrestris.—29. The same magnified, to show the disposition and figures of its supposed fructification at a, b, cc, and d, which are higher magnified at A, B, CC, and D, to show how unlike they are to fructifications.

I come now to answer Dr. Baster, who asserts positively, in his memoir in the Phil. Trans., that all the corallines, which you and I have described, are plants of the genus of conferva. To explain myself, it will be necessary to let him know what I mean by a conferva, and what I would be understood by a coralline, according to your system. By a conferva, I mean a plant with jointed filaments, either single or branched, bearing fruit, which are disposed in different ways. By a coralline, I mean an animal growing in the form of a plant, whose stem is fixed to other bodies. The stem is composed of capillary tubes, whose extremities pass through a calcarious crust, and open into pores on the surface. The branches are often jointed, and always subdivided into smaller branches, which are either loose and unconnected, or joined, as if they were glued together. This difference then will evidently appear by putting each kind into an acid liquor. The coralline will immediately discover the nature of its calcarious surface, by a strong fermentation; when the conferva will not appear in the least affected. This acid liquor will likewise soon dissolve the calcarious substance in the coralline, by which means the minute vessels that lead to the pores on the surface will become visible; whereas the conferva will unalterably remain the same, and be rather preserved than corroded by the acid.

When Dr. Pallas, who supports the opinion of Dr. Baster, comes to the chemical analysis of the corallines, he tells us that he had not time nor opportunity to try them; but depends on the report of other authors. This dependance on the authority of others, to overturn what I think we have established with very strong evidence, will, I am in hopes, convince him of the propriety of that well chosen motto of the R. S. "Nullius in verba;" which I find he has adopted as the common seal of his epistles to his literary correspondents: and he will now have a further opportunity of complimenting Dr. Baster on making a second apology for what he has advanced against me in the Phil. Trans., by showing him, that they have both been mistaken in blending two very different genera of the animal and vegetable kingdoms of nature together.

To make this difference appear still more evident, I come now to lay before you a new scene of nature; which an accurate examination into the fructification, as well as the articulations, of some of the confervas, afforded me. Indeed the minuteness of these objects would scarcely seem worth while to examine into so critically, if my reputation had not engaged me to show the wide difference between them and corallines. This, joined to some remarkable discoveries, which I made in the year 1754 on the coast of Sussex (in company with Mr. G. D. Ehret, F.R.S.) in the fructification of this class of plants, which before that time were esteemed by botanical writers to have no fructification at all, has induced me to lay a few specimens of them, with their magnified drawings, before the R. S. In examining these plants, I was amazed to find two species of them evidently of your class of diœcia; that is, male parts of fructification on one, and female on the other.

The first of these is the conferva polymorpha, where in pl. 12, at fig. a, is represented a very small branch of the female in its natural size, and at fig. A, the same is magnified; in the transparent capsules of this specimen, we can easily discover the seed as it lies expanded in a watch-glass in water. Letter b represents the natural size of a small branch of the male; and letter B the same branch magnified, showing its amentaceous flowers, or catkins, with its minute male seed in spikes; B1 shows one of them highly magnified.

The other conferva is the plumosa, and is one of our most elegant sub-marine plants. Fig. c represents the natural size of a minute sprig of the female. At fig. c, the same is magnified, where the seeds appear in their capsules. The fig. d shows the natural size of a sprig of the male conferva plumosa; and fig. D the the same sprig magnified, showing the spikes of male seed.

The next is the conferva flosculosa, and is represented at fig. e, in a branch of the natural size. Fig. E is the same magnified. This is one of those remarkable confervas that has footstalks to its flowers or fructification. It appears to have fruit like a strawberry or raspberry, surrounded by a leafy calyx. This was found on the sea-coast, near Yarmouth in Norfolk, by George Whatley, Esq. in the year 1764. When it was fresh, it was of a most vivid carmine colour. The other with flowers, at fig. f, is the conferva geniculata. Fig. F shows the same branch more distinctly, being magnified with flowers surrounding the joints; this, with one which I have called in my catalogue of confervas, conferva florifera, I discovered in the year 1754 near Brighthelmstone in Sussex, when Mr. Ehret was so kind as to make drawings of them while recent. The colour of this, when fresh, is a fine scarlet.

The conferva plumula, at fig. g, is one of the smallest of the tribe, but most elegantly feathered; it is of a pale red colour. The same is magnified at fig. G, which shows the order that the fruit and branches are disposed in. G 1 shows the fruit or seeds, which are of a red colour, surrounded by a clear gelatinous pulp.

The conferva at fig. h, I have called ciliata, from the circle of small fibres at the top of each joint. The magnified drawing at fig. H, shows these tribes like a crown on each joint. This was inserted here to show, with the rest, some of the infinite variety of beautiful forms, which the great Author of Nature has impressed even on one of the lowest classes of the vegetable tribe.

Before concluding, I must observe; That as Dr. Pallas has likewise introduced among his arguments, that the fruit of the fucuses are subanalogous to those of the corallines, I could introduce an infinite variety to show the great difference there is between them; but this part of natural history, too long neglected, requires a volume by itself, to show the amazing variety of vegetables that lie hid from us in the great deep; I may make some observations on them the subject of a future letter, especially as many of them are of the class of diœcia; as well as those which I have already shown in the confervas; which I believe will be new to the botanists.

Description of plate 12.—Fig. a, the female conferva polymorpha; A, the same magnified, to show the seeds in the capsules; b, the male conferva polymorpha; B, the same magnified, with its male flowers; B 1, one of the catkins, or male flowers highly magnified; c, the female conferva plumosa; C, the same magnified, to show its fructification; d, the male conferva plumosa; D, the same magnified, showing its catkins, or male flowers; e, conferva flosculosa; E, the same magnified, showing its pedunculated flowers, or fruit, with their polypetalous cups; f, conferva geniculata; F, the same magnified, to show its flowers surrounding the joints; g, conferva plumula; G, part of it magnified, to show the disposition of its branches; G 1, some of the fruit highly magnified, to show its seeds, surrounded by a clear viscid pulp; h, conferva ciliata; H, the same magnified, to show the little coronets on the joints.

XLI. Of the Actinia Sociata, or Clustered Animal-flower, lately found on the Sea Coasts of the New-ceded Islands: In a letter from John Ellis, Esq. F. R. S., to the Earl of Hillsborough, F. R. S., p. 428.

Among the many curious marine animals, which your Lordship has received from the new-ceded islands in the West-Indies, there is one most uncommonly rare: this is of great consequence to natural history, as it seems to bring together two remarkable genera in the system of nature, which Professor Linnæus had removed far from each other.

The one is the actinia or animal flower, the other the hydra or fresh-water polype. The actinia, called by old authors, as Aldrovandus, Johnston, &c. urtica marina, from its supposed property of stinging, is now more properly called, by some late English authors, the animal flower. This name seems well adapted to it; for the claws, or tentacles, being disposed in regular circles, and tinged with a variety of bright lively colours, very nearly represent the beautiful petals of some of our most elegantly fringed and radiated flowers, such as the carnation, marygold, and anemone. As there are great variety of species of this animal, so these species differ from each other in their form. The bodies of some of them are hemispherical, others cylindrical, and others shaped like a fig. Their substance likewise differs; for some are stiff and gelatinous, others fleshy and muscular; but they are all capable of altering their shape, when they extend their bodies and claws in search of their food. We find them on our rocky coasts at low water, fixed in the shallows to some solid substance, by a broad base like a sucker; but they can shift their situation, though their movement is very slow.

They have only one opening, which is in the centre of the uppermost part of the animal; round this are placed rows of fleshy claws; this opening is the mouth of the animal, and is capable of great extension: it is amazing to see what large shell fish some of them can swallow, such as muscles, crabs, &c. when it has sucked out the fish, it throws back the shells through the same passage. Through this opening it likewise produces its young ones alive, already furnished with little claws; which, as soon as they fix themselves, they begin to extend in search of food.

They are found all round the coasts of England; but the coasts of Sussex and Cornwall furnish us with the greatest varieties of them. The islands in the West-Indies are likewise remarkable for many kinds of them.

Doctor Gaertner, F. R. S., who has described 4 species of the English ones in the Phil. Trans. says, they have the remarkable property of renewing their claws when they are cut off; and ranks them, perhaps very properly, under the genus Hydra of Linnæus, or fresh-water polype: which I shall now give a short description of, that we may judge how near this new animal approaches to both of these.

The hydra or fresh-water polype, is that extraordinary animal so well known to the curious, from the discoveries of Mr. Abraham Trembley, F. R. S., in its re-production after it had been cut into pieces. When it is extended, it is of a worm-shaped figure, and of the same tender substance with the horns of a common snail. It adheres by one end like a sucker to water plants and other substances: the other end, which is the head, is surrounded by many arms or feelers, placed like rays round a centre: this centre is its mouth, and with these arms, which are capable of great extension, it seizes small worms and water insects, and brings them to its mouth, often swallowing bodies larger than itself: when the food is digested in the stomach, it returns the remains of the animals it feeds on, through its mouth again, having no other visible passage from its body. Their manner of multiplying is from eggs, which they produce in autumn; but the most common is from their sides, in which there first appear small knobs, or papillæ; as these increase in length, little fibres are seen rising out of the circumference of their heads, which they soon use to procure food. When they are thus arrived at a mature state, they send forth other young ones from their sides: so that though many of them soon fall off, and provide for themselves, yet the animal frequently branches out into a numerous offspring, growing out of one common parent, each of which not only procures nourishment for itself, but for the whole family.

I come now to your Lordship's new animal; and, for the satisfaction of the Royal Society, lay before them one of your Lordship's specimens preserved in spirits, with a dissection of one of them, to show its internal structure, together with three species of actinia, or animal flowers, sent to your Lordship from the new-ceded islands. This compound animal, which is of a tender fleshy substance, consists of many tubular bodies, swelling gently towards the upper part, and ending like a bulk, or very small onion; on the top of each is its mouth surrounded by one or two rows of tentacles, or claws, which when contracted look like circles of beads. The lower part of all these bodies has a communication with a firm fleshy wrinkled tube, which sticks fast to the rocks, and sends forth other fleshy tubes, which creep along them in various directions. These are full of different sizes of these remarkable animals, which rise up irregularly in groupes near to one another. This adhering tube, that secures them fast to the rock, or shelly bottom, is worthy of our notice. The knobs that we observe, are formed in several parts of it, by its insinuating itself into the equalities of the coral rock, or by grasping pieces of shells, part of which still remain in it, with the fleshy substance grown over them. This shows us the instinct of nature, that directs these animals to preserve themselves from the violence of the waves, not unlike the anchoring of muscles, by their fine silken filaments, that end in suckers; or rather like the shelly bases of the serpula, or worm-shell, the tree-oyster, and the slipper barnacle, &c. whose bases conform to the shape of whatever substance they fix themselves to, grasping it fast with their testaceous claws, to withstand the fury of a storm,

When we view the inside of this animal dissected lengthwise, we find a little tube like a gullet leading from the mouth to the stomach, whence there rise 8 wrinkled small guts, in a circular order, with a yellowish soft substance in them: these bend over in the form of arches towards the lower part of the bulb, whence they may be traced downwards, to the narrow part of the upright tube, till they come to the fleshy adhering tube, where some of them may be perceived entering into a papilla, or the beginning of an animal of the like kind, most probably to convey it nourishment, till it is provided with claws: the remaining part of these slender guts are continued on in the fleshy tube, doubtless for the same purpose of producing and supporting more young ones from the same common parent. The many longitudinal fibres, that we discover lying parallel to each other, on the inside of the semi-transparent skin, are all inserted in the several claws round the animal's mouth, and are plainly the tendons of the muscles, for moving and directing the claws, at the will of the animal; these may be likewise traced down to the adhering tube. As this specimen has been preserved in spirits, the colour of the animal when living cannot certainly be known; it is at present of a pale yellowish brown.

With regard to its name, it may be called actinia sociata, or the cluster animal flower.

Among the critics, I am aware of this; that it may be said, that an animal compounded of many animals has not a very philosophical sound. But it is well known, to those who understand the nature of zoophytes, that there are many kinds of these animals, as well such as swim about freely, as such as are fixed to rocks and shells in the sea, that have a great many mouths in the form of polypes, and yet are but single animals; such as the great variety of pennatulas, or sea pens, among those that swim about, and most of the sertularius, gorgonias, with many others, among those that are fixed. Yet this new animal differs very much from the generality of these. I think I may compare it, to speak in the style of those who maintain that zoophytes vegetate, to a timber tree, that sends out at a distance round it many suckers from its roots, which suckers coming in time to be trees, these may and will, with propriety, be reckoned so many distinct trees, though connected at their roots with the parent tree, and that without any absurdity.

Lest any doubt should still arise in this abstruse part of the operations of nature, it may be proper to explain myself further, by showing that there are a great many zoophytes, which were formerly called corallines, now sertularias and cellularias, that from a creeping adhering tube send up several single animals, others send up several branched animals. To give an instance or two of each, I shall mention the sertularia uniflora, or single bell-shaped coralline (see the Essay on Corallines, pl. 14, fig. A and B) and the cellularia anguina, or snake's head coralline (see the same Essay, pl. 22, fig. C) both which, like our actinia sociata, send up distinct animals with one mouth each.

Whereas the sertularia pumila, or sea oak coralline (see Essay on Coralline, pl. 5, fig. A) and the cellularia bursaria, or shepherd purse corralline (see the same Essay, pl. 20, fig. A) send out animals in the form of spikes or branches, that have many mouths from their own creeping and adhering tubes; and yet both those with one mouth to each, and these with many, I esteem as so many distinct animals, notwithstanding their being connected by an adhering tube, as I have said in the instance of the tree and its suckers.

To conclude, the importance of the discovery of this new animal to natural history is this, that it clears up that much-disputed point, which is, that the extension or increase of the substance of these zoophytes, is of an animal, and not of a vegetable growth, as some late authors would have us think, by thus making the fact more clear and evident to our senses. For the poetical descriptions of some late systematical authors have tended rather to confuse than explain these matters to our ideas; for instance, they call these bodies that rise up like a spike with many mouths, a vegetating stem, and their mouths, which are formed like so many polypes, flowers; though with these supposed flowers, they evidently seize their food, by stretching out their claws, which they call the petals, to convey it to their mouths, that are in the centre of each, to swallow it, digest it, and return the non-nutritive parts back again by the same way. Can this then be called a vegetative life?

But happily this animal is large enough for dissection; and in that state discovers to us, not only muscles and tendons, but a stomach to digest, and intestines to secrete proper nourishment, for the support and increase of itself and its progeny; which I am persuaded is the strongest proof that has yet appeared to convince the learned world, that zoophytes are true animals, and in no part vegetable.

Description of Pl. 13.—Fig. 1, the actinia sociata, or clustered animal flower, with its radical tube adhering to a rock. *a* One of the animals stretching out its claws. 2, a perpendicular dissection of one of these bodies, to show the gullet, intestines, stomach, and fibres, or tendons, that move the claws. *a* A young one arising out of the adhering tube. 3. The actinia aster, or sea star-flower, from the new ceded islands. 4. The actinia anemone, or sea anemone, from the same place. 5. The under part of the same, by which it adheres to rocks. 6. The actinia helianthus, or sea sun-flower, from the same place. 7. The under part of the same. 8. The actinia dianthus, or sea-carnation, from the rocks at Hastings in Sussex: this animal adheres by its tail, or sucker, to the under part of the projecting rocks, opposite to the town; and, when the tide is out, has the appearance of a long white fig: this is the form of it when it is put into a glass of sea-water. It is introduced here as a new variety of this animal, not yet described. 9. The sertularia uniflora, or single bell-shaped coralline magnified. *a* One of its ovaries. 10. The cellularia anguina, or snake's-head coralline, magnified. 11. The sertularia pumila, or sea-oak coralline, magnified. *a* One of its ovaries. 12. The cellularia bursaria, or shepherd's purse coralline, magnified.

XLII. New Observations on what is called Pompey's Pillar, in Egypt. By Edward Wortley Montagu, Esq. F. R. S. Dated from Zante, May 7, 1767. p. 438.

These few lines will probably appear extraordinary, as every traveller that has been at Alexandria has mentioned the famous pillar of Oriental Granite, which is about a mile without the walls of that city, as erected, either by Pompey, or to the honour of Pompey. As I differ in opinion from them all, and think this famous pillar was erected to the honour of Vespasian, you certainly will expect to hear on what foundation I found so extraordinay a conjecture, as so new a one may appear.

By my mens. the capital of the pillar is	9ft.	7 inc.
The Shaft	66	1¾
The Base	5	9¾
The Pedestal	10	5¾
Height from the ground	92	0
Its Diameter	9	1

As soon as I saw this surprising pillar, I was convinced that if it had been erected in Pompey's time, Strabo, or some of the ancients, would have mentioned it: I therefore determined to examine it narrowly. I perceived that the pedestal was of a bad and weak masonry, composed of small and great stones of different sorts, and absolutely unable to sustain so great a weight; I therefore easily concluded such pedestal not originally belonging to the pillar. I attempted to get out a stone, which I did without trouble, and discovered the pedestal to be hollow. After some time, I mean during the course of many days, I made an opening wide enough to enter it; when within it, you will judge how much I was surprised to find this prodigious mass of granite, stood, as on a pivot, on a reversed obelisk, as I then believed it was, only 5 feet square. Curious to know the length of the obelisk, I began to move the earth on one of its sides, but my surprize increased much when I found, after moving a few inches of the soil, that the obelisk was not entire, this pivot being only 4 feet and one inch thick. It is seated on a rock; the stone is of an extreme hardness, and almost a petrification or rather conglutination of many different stones, but all vitrescent. I never met with any stone of this kind any where, except with one small piece on the plain of the Mommies; I broke a piece off it, which Lord Bute has; a small piece too of the pillar was sent, that gentlemen may be convinced it is of red granite, and not a composition, as some have imagined.

This part of the obelisk is covered with hieroglyphics, which are reversed; a plain proof the pillar was not erected while they were held sacred characters. Convinced, therefore, that it was not of the antiquity one would suppose it, from being called of Pompey, I visited it several times, to see if it might not be possible to find out something that would give room for a reasonable conjecture, in honour of whom, or at what time, it was erected. From the inscription I could discover nothing; it is on the west face of the base, but so much injured by time, and I may say too by malice, for the mark of an instrument are plainly discovered effacing it, that one can but imperfectly make out some Greek characters, so imperfectly indeed that no one word can be found.

At length, observing that the cement, or mortar, which closes the small separation of the shaft from the base, was quite destroyed in one part, I was curious to see if any thing was made use of within, to fasten or tie the shaft to the base; I saw there was: being desirous to know if it was lead, and if so, if it was not of that pure sort, of which we still meet with some few medals; I endeavoured with a pretty large hanger to cut off a small piece of the grapple; there was a great number of lizards which had taken shelter there, and which ran out on my introducing the hanger. I then discovered a dark spot, at the distance of more than a foot, within the circumference of the pillar; which, by striking it with the hanger, I found was something stuck fast to the base; after striking it several times, I detached it from its place, and it proved a medal of Vespasian in fine order. ΑΛΤ. ΚΑΙΣ. ΣΕΒΑ. ΟΛΕΣΠ: The reverse is, Victoria gradiens; Dextra spicas, sinis. palmam. [This medal was shewn to the R. S.]

The reversed hieroglyphics are a proof that this amazing monument was not erected before Pompey's time; and as there is no mention of it in Strabo, or any one of the ancient writers that I have met with, it seems plain it was not known before the time of Vespasian. This medal could not by any accident, I think, have been introduced above a foot within the circumference of the shaft; therefore I suppose it was placed there when the pillar was erected, which I thence conclude to have been done to the honour of that emperor; and perhaps on his restoring the cripple to the use of his limbs.

XLIII. Part of a Letter from W. Watson, M. D., F. R. S., to John Huxham, M. D., F. R. S., at Plymouth, giving some Account of the late Cold Weather. Dated London, 14 February, 1767. p. 443.

After as mild a winter as has been known here for many years, the frost has been intense. Until the latter end of December many of the tender annual exotic plants continued alive; such as the African marigold, Nasturtium Indicum, and others of this class. Even the plant usually called balm of Gilead, at that time flourished without shelter. Mr. Miller of Chelsea said that he had known the like but twice in his life; and that was in the years 1717 and 1722. However, at the beginning of last month, January, after some smart gusts of wind at east, it began to freeze; and continued increasing, until the 5th of that month, in the morning, the thermometer, an excellent one made by Bird, stood in the open air somewhat under 20″, in the evening it was 29″. It continued thereabouts till the 9th, when in the morning it stood at 20″ again, and at night at 21″. On the 10th in the morning it stood at 17″, at night at 18″. From this time to the 18th it was never below 23″, but frequently above the freezing point. On the 18th at night it stood at 19″$\frac{1}{4}$. On the 19th at 6 in the morning it stood at 16″, at 8 in the morning 15″$\frac{1}{2}$, at 11 at night 17″. On the 20th at 8 in the morning, it stood at 18″, at 11 at night 22″. On the next day, January 21st, the frost broke; the thermometer at 4 in the afternoon standing at 36″.

In the country it has been observed much colder. On January 10th, at Cardington in Bedfordshire, Mr. Howard, F. R. S., by two thermometers observed the mercury, at 6 in the evening, to stand at 9″. And on the same day the Rev. Mr. Wollaston, at East Dereham in Norfolk, found it so low as 8″. Mr. Ellinet, at Norwich, on the 19th of January, found his thermometer at 8 in the morning to stand at 7″; at noon at 22″, at 5 in the afternoon 18″; at 10 at night at 8″. Seven degrees therefore is the lowest point, at which the thermometer seems to have stood any where in England during the late frost.

January 10 was the coldest day at Plymouth, where Dr. Farr, a very ingenious physician, and Mr. Mudge, who has communicated several papers to the R. S., each observed his thermometer to stand at 23″$\frac{1}{2}$.

The severe frost of 1739, of which no account appears in the Phil. Trans., began December 29, when Lord Charles Cavendish's thermometer in his room stood at 25″; the next evening 21″. From this time his lordship placed his instrument out of the window, and at some distance from it; when in the evening of December 30, it stood at 17″. On the morning of Jan. 5, it stood at 13″. On the 8th at 15″; the 9th at 14″; 22d at 19″; 25th at 17″. On February 5th at 21″. During all this time, the thermometer was seldom above 32″, the freezing point.

Thirteen degrees therefore during this frost was the lowest observation, by Lord Charles Cavendish in Marlborough-street.

At Plymouth, the depth of rain that fell in 1766, was 35 inches.

XLIV. Of an Electrometer invented by Mr. Lane. p. 451.

This portable machine was the contrivance of Mr. Read, mathematical instrument-maker at Knightsbridge near London.

The principle on which this electrometer acts is very simple, being merely this; the coated phial is hereby rendered incapable of accumulating and retaining any more than a certain quantity of the electric fluid, for any intended experiment, when a metallic or non-electric communication is made from a screw to a wire loop of the machine, and that quantity will be proportionate to a certain distance, and consequently the explosion and stroke will be thus regulated. The friction glass is a cylinder turned round its axis in a vertical position.

XLV. Of the Increase and Mortality of the Inhabitants of the Island of Madeira. By Dr. Thomas Heberden, F.R.S. p. 461.

When we consider the number of people in Madeira, and the state of the inhabitants, no place appears more proper for forming an estimate of the increase and mortality of mankind than this island; for the number of persons is upwards of 60,000, all of whom may be supposed to live and die in the same place where they received their existence; the accession of strangers and the egression of the natives being so equally inconsiderable, that if the one does not exactly counterbalance the other, the difference may justly be neglected, as of no consequence in the general calculation. This excited Dr. H.'s curiosity; and he procured a survey from house to house in each of the respective parishes; from which, and the parish registers, he deduced the adjoined account.

An Hypothesis. The number of persons in this island, in the year 1743, was 48234 of 7 years old and upwards. Now supposing the minors were in the same proportion then, as in this present year, the total of the inhabitants was 53,057. Therefore, by the rule of anatocism, they have increased at the rate of 1,0082 per cent. per annum; and by the same rule they double in 84 years 4 months:

and 25 days. From an exact survey, made in the beginning of the year 1767, the number of inhabitants on the island of Madeira, was as follows:

Persons of 7 years old and upward, 58669; under 7 years of age, 5945; total 64614.

Christened in 8 years	17611	Medium for each year	2201$\frac{3}{8}$
Buried in 8 years	10351	Medium for each year	1293$\frac{7}{8}$
Octennial increase	7260	Annual increase	907$\frac{4}{8}$

Proportion of the yearly births to the number of persons, as	1	to	29.35
.... of the yearly burials to the number of persons, as	1	to	49.89
.... of births to burials	100	to	58.77
.... of males born, to females	100	to	96.39
.... of females buried, to males	108.33	to	100
Weddings each year, at a medium	470$\frac{5}{8}$		
Proportion of weddings to births, as 1 to	4.68		
.... of weddings to burials, as 1 to	2.75		

The mortality of spring and summer, to that of autumn and winter, as 115 to 100.

XLVI. *Of some Very Large Fossil Teeth,* found in North America, and described by Peter Collinson, F.R.S.* p. 464.

George Croghan, Esq., who is a deputy of Sir Wm. Johnson, the king's superintendant of Indian affairs in America, in the course of his navigation down the great river Ohio, in the year 1766, after passing the Miame river, in the evening came near the place where the elephants' bones are found, about 4 miles south-east of the Ohio, and about 600 miles distant from and below Pitsburgh, from the nearest sea-coast at least 700 miles. Next morning he met with a large road, which the buffaloes had beaten, wide enough for two waggons to go abreast, leading straight into the great licking-place, to which the buffaloes and all the species of deer resort, at a certain season of the year, to lick the earth and water from salt-springs, that are impregnated with nitrous particles; whether to cleanse their stomachs, or for what other purpose, is submitted to the sentiments of the Society.

Mr. Croghan had been here some years before, and gave some account of the monstrous bones, and teeth, found at this place, called by the Indians The Great Buffaloes' Lick; but being now more at leisure, he carefully examined all its vicinity, and discovered under a great bank, on the skirts of the Lick, 5 or 6 feet below the surface, open to view, a prodigious number of bones and teeth, be-

* These teeth belong to the animal called the American mammoth, which is found only in a fossil state, and is supposed by Mr. Pennant to have been a species of elephant. It is accordingly the American elephant of that author. A complete skeleton was discovered in the districts here described a few years ago, and was, by Mr. Peele, its proprietor, brought over from America, and exhibited in London.

longing to some of the largest sized animals; by the quantity, he computes there could not be less than 30 of their skeletons. By their great teeth, or tusks, of fine ivory, some near 7 feet long, every one that views them will not hesitate to conclude they belong to elephants. It is very remarkable, and worthy of observation, that none of the molares, or grinding teeth of elephants, are discovered with these tusks; but great numbers of very large pronged teeth of some vast animals are only found with them, which have no resemblance to the molares, or grinding-teeth, of any great animal yet known. As no living elephants have ever been seen or heard of in all America, since the Europeans have known that country, nor any creature like them; and there being no probability of their having been brought from Africa or Asia; and as it is impossible that elephants could inhabit the country where these bones and teeth are now found, by reason of the severity of the winters, it seems incomprehensible how they came there.

Probably many of this learned Society are not unacquainted with the fossil elephants' teeth annually found in Siberia, lodged in the banks of the great river Oby, and other rivers of that country. On the system of the deluge, it has been conjectured, that as the extensive kingdom of Siberia lies behind the native country of the elephants in Asia, from west to east, and to the north, by the violent action of the winds and waves, at the time of the deluge, these great floating bodies, the carcases of drowned elephants, were driven to the northward, and at the subsiding of the waters, deposited where they are now found. But what system or hypothesis can, with any degree of probability, account for these remains of elephants being found in America, where those creatures are not known ever to have existed, is submitted to this learned Society.

P. S. The bishop of Carlisle presented to the Royal Society, on the 27th of February, 1766, some fossil teeth and bones from Peru, which have some analogy to the before-mentioned, not so recent, but much more petrified; the pronged teeth are like agate.

A List of the Teeth and Bones sent over by George Croghan, Esq., Feb. 7, 1767, from Philadelphia.

To Lord Shelburne.—Two of the largest tusks, or teeth, one whole and entire, about 6 feet long, the thickness of common elephants' teeth of that length. Several very large forked or pronged teeth; a jaw-bone, with 2 of them in it.

To Dr. Franklin.—Four great tusks, of different sizes. One broken in halves, near 6 feet long. One much decayed, the centre looks like chalk or lime. A part was cut off from one of these teeth, that has all the appearance of fine white ivory. A joint of the vertebræ. Three of the large pronged teeth; one has 4 rows of fangs.

Besides the above, Capt. Owry, an officer who served in the country during the last war, now living at Hammersmith, has a small tusk, as if of a calf elephant, the surface of a fine shining chestnut colour, and a recent look; and a great pronged tooth, larger than any of the above, which were also brought from the same licking-place.

XLVII. Sequel to the Foregoing Account of the Large Fossil Teeth. By P. Collinson, F.R.S. p. 468.

In his observations on the long teeth and grinders, at the last meeting of the R. S., Mr. C. forebore giving his sentiments on these remains of great animals found at the Great Lick, near the river Ohio, being willing the Society should determine for themselves. As he perceived one of the long teeth, or tusks, was channelled or ribbed, near the larger end, he was in some doubt if peculiar to the elephant. To satisfy himself, he went to a warehouse, where there were teeth of all sorts and sizes for sale; on examining them he found as many ribbed or channelled as plain and smooth; so that now he has no difficulty to pronounce them agreeing in all respects with the elephants' teeth from Africa and Asia.

But as the biting or grinding teeth, found with the others, have no affinity to the molares of the elephant, he concludes that they with the long teeth belong to another species of elephant, not yet known; or else that they are the remains of some vast animal, that has the long teeth or tusks of the elephant, with large grinders peculiar to that species, being different in size and shape from any other animal yet known. He had one of these grinders, that weighed near 4 pounds, with as fine an enamel on it as if just taken out of the head of the creature.

The elephant is wholly supported by vegetables; and the animal to which these grinding teeth belong, by their make and form, seemed designed for the biting and breaking off the branches of trees and shrubs for its sustenance; and he concludes from analogy, that the great heavy unweildy animals, such as elephants, and the rhinoceros, &c. are not carnivorous, being unable, from want of agility and swiftness, to pursue their prey, so are wholly confined to vegetable food; and for the same reason, this great creature, to which these teeth belong, wherever it exists, is probably supported by browzing on trees and shrubs, and other vegetable food.

One of these large pronged teeth weighed 3¾ lb.; and was 18 inches round, and 4 inches thick.

XLIX. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1766, pursuant to the Direction of Sir Hans Sloane, Bart. By Wm. Hudson. p. 470.

This is the 45th presentation of this kind, completing to the number of 2250 different plants.

XLIX. An Account of some Neutral Salts made with Vegetable Acids, and with the Salt of Amber; which shows that Vegetable Acids differ from one another; and that the Salt of Amber is an Acid of a Particular Kind, and not the same with that of Sea-salt, or of Vitriol, as alleged by many Chemical Authors. By Donald Monro, M.D., Physician to the Army, &c. F.R.S. p. 479.

Suspecting from the taste and smell of the vegetable acids that they differed materially one from another, Dr. D. M. was induced to prepare neutral salts from them, and to examine the forms of the saline crystals thus obtained. He found in many instances a considerable difference in the forms of these crystals, showing that his conjectures on this subject had been well founded.

He treats, 1. Of Neutral Salts formed with Native Vegetable Acids.

2. Of Neutral Salts formed with Fermented Vegetable Acids.

3. Of Neutral Salts formed with Distilled Vegetable Acids.

4. Of Neutral Salts formed with Flowers of Benzoin and Salt of Amber.

Under § I he describes the Neutral Salts formed with Native Vegetable Acids and the Fossil or Mineral Alkali; viz. 1 with the Acid of Lemons; 2, with the acid of Limes; 3 and 4, with the Acid of Seville Oranges and of Peaches; 5, with the Acid of Currants; 6, with the Acid of Gooseberries; 7, with the Acid of Apples; 8, with the Acid of Wild Sorrel; 9, with the Acid of Tamarinds; 10, with the Acid of Plums; 11, with the Acid of Mulberries; 12, with the Acid of Grapes.

Under § II. he describes the Neutral Salts formed with Fermented Vegetable Acids and the Fossil Alkalies; viz. 1 and 2, with Common Wine and Distilled Vinegar; 3, with Crystals of Tartar; 4, with the Acid of Verjuice of Apples; 5, with the Acid of Perry.

Under § III. he describes the Neutral Salts formed with Distilled Vegetable Acids and the Fossil Alkali; viz. 1 with the Acid of Guaiac Wood; 2, with the Acid of Fir-wood; 3, with the Acid of Honey. Respecting this last, Dr. M. says that he prevailed with Mr. Winter, brother-in-law to Mr. Heineken, apothecary, to distil 4 or 5 lb. of honey in a retort; at first Mr. W. imagined that Dr. M. only wanted the watery phlegm, which has been called by the name of the spirit of honey, and stopped the distillation before the acid came over; but having distilled a 2d quantity, he procured him about 6 oz. of a very acid liquor, which he mixed with the phlegm or spirit he first brought him; he then saturated the whole with the fossil alkali, filtered and evaporated the liquor to a pellicle. After it had stood all night in a cool place, he found the pellicle to be composed of a yellow, bitter, saltish, mucous and oily matter; below which was a dark purplish liquor, which he poured into a tea cup, and there remained at the bottom of the stone gallypot, in which the evaporation had been performed, a yellow concreted matter, somewhat of the appearance of yellow wax, mixed with a little honey; on the surface of which was to be observed a number of globules of the same sort of matter, of the size of mustard-seeds, and interspersed with a black very bitter stuff. Next day, on examining the dark-coloured purplish liquor which he had put into the tea cup, he found that a great part of it had concreted into a very beautiful salt. The crystals were almost all flat, and seemed in general to assume the form of long, narrow parallelograms, or longish squares.

This salt is pleasant to the taste, and evidently generates cold in the mouth in the time of its solution; but he had not quantity enough to try with a thermometer what degree of cold it generated in the time of its solution in water.

SECT. IV.—*Of Neutral Salts formed with Flowers of Benzoin, and Salt of Amber.*

EXPER. 1 and 2.—*With the Flowers of Benzoin.* Most modern chemists have considered the gum benzoin as a resinous substance, which bears the same analogy to the vegetable resins, as the succinum or amber does to the fossil bitumens; and they have esteemed the flowers of benzoin to be an acid salt,* mixed with an oily and a small proportion of an earthy matter; but have brought no proof of its being so.

1. In order to ascertain this fact, Dr. M. put 2½ drs. of the flowers of benzoin into some water, and then dropped into it by degrees a solution of the fossil alkali; every drop raised an ebullition or effervescence, in the same manner as when any common alkaline salt is thrown into an acid liquor. He continued adding the alkaline ley till all ebullition ceased, and the flowers were fully saturated and dissolved; after which he filtered the liquor, and evaporated it till a pellicle began to appear, and then set it in a cool place all night, and next morning he had a fine pure transparent neutral salt. It adhered to the china basin in form of a saline crust, which he removed; and on looking through it in the light, it seemed to be composed of an infinite number of very small crystals; above this lay, in many places, a number of crystals of the figure of small oblong parallelograms. But from the greater part of the surface of the crust there arose a number of very fine thin delicate plates of irregular figures, standing on one edge; some were squares, others parallelograms, and others had more sides.†

This salt, when first made, appeared as transparent and clear as glauber salt, or nitre; but on being exposed to the air, became very soon white and mealy.

In the time of the evaporation of this salt, a saline white mealy crust rose every where on the sides of the china basin in which the operation was performed, and even came over so far, as to cover its whole outside. What rose in this manner had a sweetish taste, and was not so sharp in the mouth as what

* Now known by the name of benzoic acid.

† In the modern chemical nomenclature the salt thus formed by combining the acid of benzoin with the fossil alkali, is termed benzoate of soda.

appeared in a transparent saline form. The superfluous liquor, which remained after the crystallization was completed, being put into a tea cup, concreted in a very uncommon manner. In the middle of the tea cup it arose something like a plant, or a fountain, where the water is discharged from a number of pipes; and spread from the bottom of this, so as to cover both the inside and outside of the cup, with a sweetish, white, mealy, saline crust, which in many places seemed disposed like the fine fibres of plants, or of the leaves of trees.

2. As a further proof of the flowers of benzoin being an acid of a particular kind, Dr. M. saturated some of them with the sal volatile ammoniacum, evaporated and crystallized; and obtained an ammoniacal salt, which had a very singular appearance. It was covered on the top with a very white saline pellicle, below which were a number of thin, flat, white transparent crystals, the greater number of which seemed to be exact squares, some few, oblong parallelograms.*

The flowers of benzoin generated a considerable degree of cold in the time of their saturation with the volatile alkali; they sunk the quicksilver in the thermometer from 52 to 46.

Exper. 3 and 4. *With the Salt of Amber.*—The salt of amber is now generally known to be of an acid nature; but from what Mons. Bourdelin has said of it, in the Memoirs of the French Academy of Sciences for the year 1742, its acid has been considered by many chemists to be exactly of the same nature as the spirit of sea-salt, only mixed with a little of the oleum succini; though some have imagined it to be an acid of the vitriolic kind.

1st, When Dr. M. first mixed this acid with the fossil alkali, he began to believe that what Mons. Bourdelin had alleged was true; for the liquor tasted saltish, like a weak solution of sea-salt in common water; but he was soon convinced of his error; for on evaporating and crystallizing, he had a salt very different in its nature and properties from that of sea-salt, or of glauber-salt, one of which salts it must have been, had the acid been the marine or the vitriolic. This agrees with what Dr. Stockar de Neuforn † has said of this being a particular acid.‡

The crystals Dr. M. obtained in the first experiment he made were large and flat; they were of no certain shape or figure; some were roundish with a number

* The salt thus obtained by combining the acid of benzoin with the volatile alkali, has been termed by later chemists benzoate of ammonia.

† In the year 1760, Dr. Jo. Geo. Stockar de Neuforn, in his inaugural Dissertation de Succino, published at Leyden the 7th of July, 1760, proves by a number of experiments, that the acid of succinum is neither that of vitriol nor of sea-salt; and he mentions two neutral salts made with this acid, the one with the common vegetable alkali, and the other with the volatile.—Orig.

‡ That the salt obtained by distillation from amber is a peculiar acid, is no longer a question among chemists. In the new nomenclature, it is denominated succinic acid.

of sides, others appeared somewhat triangular, and others of different figures; and in some parts the crystallization appeared like a piece of rock-work. He dissolved some of this salt in water, and crystallized it a 2d time, but the crystals were in general smaller than in the first operation; this salt is extremely different in its taste from that of sea-salt, and certainly likewise in its virtues and properties.*

2. To ascertain more fully that the sal succini is an acid sui generis, Dr. M. saturated some of it with the volatile ammoniac salt, crystallized it, and obtained a neutral ammoniacal salt very different from that of the common sal ammoniacum; it was composed of a number of small long narrow flattish crystals, whose sides were made up of 4 flat surfaces, and laid in an irregular order, some lying across others, and some standing on one end.†

The sal succini generates a great degree of cold in the time of its saturation with the volatile alkali; for it sunk the quicksilver in the thermometer from 52 to 40: in this it agrees with the common sal ammoniacum.

The conclusion.—From the experiments above related, it is evident that physicians have hitherto been in a great mistake, in believing that all vegetable acids were nearly of the same nature; for from them it should seem that almost each of the acids, called vegetable, has something peculiar to itself, and on future trials may be found to have different virtues and properties.‡

The different appearance of the neutral salts above-mentioned, from that of those produced by the union of the fossil alkali with any of the mineral acids, seems to make it doubtful whether the vegetable acids derive their origin from the mineral, or whether they are not new substances, generated either in the vessels of plants by means of the vegetative process, or by fermentation, or by the force of fire. If they owe their origin to the mineral acids, they are certainly so much changed in their virtues, and properties, by the combination of new particles, and by the processes they have undergone, that they may be considered as distinct bodies in many respects.

* The salt thus formed by combining the acid of amber with the fossil alkali is termed in the new nomenclature, succinate of soda.

† Dr. Stockar de Neuforn says, that this ammoniacal salt does not precipitate silver from aquafortis, nor change aquafortis into aqua regia; and when put in a silver spoon, and set over the fire, it melts and flies off in form of a vapour.—Orig. In the new nomenclature the salt thus formed by combining the acid of amber with the volatile alkali, is termed succinate of ammonia.

‡ However, it ought to be remarked, that when any of the concentrated saturated liquors stood for 10 or 12 days, before they crystallized, for the most part some crystals of a flat, square, or of a narrow oblong parallelogram figure, were found adhering to the sides of the cup or basin in which the liquor stood; but whether this was owing to the alkaline basis of these salts, or to the acids approaching to each other in their nature, is what can only be determined by future experiments. —Orig.

From what has been said, it is evident that the number of true neutral salts* is infinitely greater than what has been supposed, of late, by chemists; and it is probable that many of the neutral salts, above described, may prove to be excellent remedies in the cure of diseases, as well as useful in many manufactories.

As there is such a variety of vegetable acids, and as each of them produces a distinct neutral salt with each of the 3 alkalies, Dr. M. thinks it would be right to distinguish them from each other by particular names; the salts made with the vegetable alkali may be called vegetable salts, as both the acid and the alkali are vegetable substances; those made with the fossil alkali, neutral salts; and those made with the volatile alkali, ammoniacal salts, as all the neutral salts hitherto made with this alkali have gone by this name. Thus we may call the 3 neutral salts made with lemon juice: 1, vegetable salt of lemons; 2, neutral salt of lemons; 3, ammoniacal salt of lemons.

By means of these neutral salts we may be enabled to discover many of the properties of vegetable acids, and particularly the different degrees of affinity or attraction between them and alkaline salts. Thus, for example, if we dissolve in water some of the neutral salt of currants, and add some lime juice or some vinegar, and then evaporate and crystallize; if we obtain a neutral salt of currants, we conclude that the acid of currants has a greater affinity or attraction to the alkali than the acid of limes or of vinegar; but if we get a neutral salt of limes, or of vinegar, we conclude that these acids have a greater affinity with the alkali than the juice of currants.

As Dr. M. was sensible that this account of vegetable neutral salts was very incomplete, and that he had done little more than to give a very superficial description of their external appearance; and as it would probably require a length of time, and the labours of many, to discover fully their virtues and properties, he recommended it to those who might prosecute this subject, to endeavour to ascertain the following facts: 1. What degree of cold or of heat is generated on the mixture of each acid with the different alkaline salts; and likewise to try the same experiment with each neutral salt at the time of its solution in water. 2. What quantity of pure alkaline salt it takes to saturate any determined quantity of each of the vegetable acids. 3. What figure each neutral salt assumes when it is first crystallized, and likewise after it has been purified, and again dissolved in water and crystallized. 4. What quantity of water it takes to dissolve any determined quantity of sea salt. 5. What effects these salts or their solutions in water have on oils, sulphur, ardent spirits, metals, earths, and other sub-

* By true neutral salt is meant, a salt made with an acid and one of the 3 alkalies; the word true is added to these salts, to distinguish them from neutral salts, made with earths or metals, and acids.—Orig.

stances; what substances they mix easily with, and to what bodies they prove a menstruum, or assist in dissolving. 6. How far they agree in their virtues and properties with the neutral salts made with mineral acids, and with each other. 7. What effects they have on the human body; whether they promote more particularly the perspiration or the secretion by the kidneys, or whether they act more readily on the bowels, and promote the discharge by stool; and to ascertain the exact and proper doses of each. 8. And lastly, what effects fermentation and distillation have on native vegetable acids; and to observe and compare the appearances of the neutral salts made with these acids in their different states; viz. 1. In their native state. 2. When made into wine. And 3dly, when made into vinegar; and likewise when made with acids brought over by the force of fire, or distilled from the same juices in each of the 3 different states mentioned.

And in order to facilitate their labours, Dr. M. concludes this long paper with observing. 1st. That all vegetable juices, used for making neutral salts, ought to be strained through a cloth, and then filtered through paper, before they are saturated with the alkaline salt; and that, after they are saturated, they ought to be allowed to stand for some days, and some of them for weeks, and then be filtered again, before they are evaporated. 2dly. That it is of use to clarify many of these juices, after being saturated, with the whites of eggs. 3dly. That it is sometimes easier to obtain a neutral salt, by evaporating with a boiling heat, than with a slow or gentle fire; as the heat of boiling water coagulates, and throws up a quantity of viscid juices to the surface, which cannot be easily separated by any other means. 4thly. That the sweeter any fruit is, and the more it abounds with saccharine or viscid juices, the more difficult it is to obtain a neutral salt; and for this reason he had not hitherto been able to get any neutral salt from the saturated juices of pears, or of cherries. 5thly. That, in cases where we are obliged to employ water mixed with the fruits cut small, instead of their juices, it is right to peel off the skins before we attempt to saturate the acid; otherwise the alkaline salt is in danger of uniting with, and rendering soluble in water, the gross oils with which the skins generally abound, which afterwards prevent the crystallization of the neutral salts.*

L. Experiments on the Distillation of Acids, Volatile Alkalies, &c. showing how they may be condensed without Loss, and how we may thus avoid Disagreeable and Noxious Fumes. By Mr. Peter Woulfe, F. R. S. p. 517.

In the common manner of distillation, there escapes a great quantity of fumes,

* Though in these experiments Dr. D. Monro omitted to employ all the modes of investigation which should have been resorted to, for the full ascertainment of the object in view; yet it cannot be denied that he is entitled to the merit of showing, though imperfectly, what was afterwards so clearly demonstrated by Scheele, that there are several distinct species of acids in the vegetable kingdom.

which cannot be condensed; and in several operations these fumes are very hurtful to the lungs. By the following method of distillation these fumes are totally condensed, which makes a great saving in some distillations, and the operator is in no danger of being hurt by any pernicious vapours. This new method consists in making the fumes pass by a small glass tube through water, which thus becomes charged with the vapours, that would otherwise escape.

Description of the apparatus, pl. 14.—In fig. 1, A is a retort; B a receiver, with a spout at bottom, for the distilling liquor to run into the bottle C; the recipient has also a small opening on one side at D; E a crooked tube $\frac{1}{10}$ and $\frac{1}{2}$ of an inch bore; F a vessel containing water.

The crooked tube E is fitted to the spout D of the receiver by means of a cork with a hole in its middle, and then well covered with lute; the other end of it goes to the bottom of the vessel F, to the mouth of which it is fitted by a cork, with a semicircular notch in it as at G, but without any lute to fasten it, as there must be a small vent for the escape of the elastic air, and this is the only vent in all the apparatus for that purpose. By this apparatus the fumes are obliged to pass through the water in F, and there deposit all they contain, except their elastic air.

In most distillations there is a quantity of air absorbed at different times during the process; and in this case the external air would press on the water at F, and force it by the tube into the vessel C, which might spoil the distilled liquor. This may be prevented by letting air into the receiver or bottle C, by boring a hole through the lute; this however may be inconvenient, on account of the constant attendance which is necessary; but the following apparatus will prevent it. See fig. 2. It consists in fitting an empty vessel H, to the apparatus described before in fig. 1. By this means the water is forced into H, and by the stopper at L it may be emptied, and put back into the vessel F; the crooked tubes D and I are fitted to H, by a cork with two opposite semicircular notches, as at K, and then well covered with lute.

Exper. 1.—On the distillation of sal ammoniac with quick-lime.—12 lb.* of British sal ammoniac, and 26 lb. of quick-lime were powdered, mixed, and put into the iron body A, fig. 3, and when the apparatus † was luted, a gallon of water was poured on it through the orifice b, which was immediately stopped; the lime growing hot produced a vast quantity of elastic air, which though highly charged with volatile alkali was condensed by the water in F, fig. 2, the air only escaping at the top of this vessel with hardly any sensible volatile alkaline smell.

* In all the experiments avoirdupois weight was made use of.—Orig.

† The spout of the stone head belonging to the body A, fig. 3, is to be luted to the receiver B, fig. 2.—Orig.

Next morning, all being cold, another gallon of water was added as before, and a very slow fire made under the body for 14 hours, in which time there distilled near a pound of volatile alkali; the fire was then made stronger, and continued in that state for 12 hours more, in which time there was obtained, together with what was first distilled, $8\frac{1}{2}$ lb. of volatile alkali, strong and fit for eau de luce; this was taken out of the bottle and set apart. The vessels being cool, 2 gallons more of water were put into the body, and the fire made as before, and continued till there was 7 lb. distilled of weak volatile spirit; this answers better than water for a fresh distillation of sal ammoniac and lime.

During the first 16 hours of the distillation, there continually escaped through the water of F elastic air very slightly charged with volatile alkali, especially when the water got hot; but during the remaining time of the distillation, no elastic air was set free.

Two stone gallon bottles, with 3 quarts of water in each, were made use of to condense the vapours; and when one bottle was got warm by the fumes, the other was put in its place, while it was cooling in a vessel of cold water; and so continually changed during the whole operation. The 6 quarts of water increased by this means 2lb. and $\frac{1}{2}$ in weight; and, by the following experiments it appears, that a pound of this vapour condensed in the water, is to a pound of the volatile alkali, which was set apart for Eau de luce, as 140 to 76, which is nearly twice as long; therefore there was a saving of near 5lb. of volatile alkali, which would have been lost in the common manner of distillation.

The water of the two stone bottles charged with alkaline vapours was mixed, in order to reduce them to the same degree of strength, and as much of it was put into a glass cucurbit, as contained 4 ounces of the alkaline vapour; 4 ounces of the volatile alkali, which was set apart for Eau de luce was put into another cucurbit of the same size, and diluted with water to the same volume of the other.

This last took 1lb. 3oz. of acid of vitriol, diluted with water, to be saturated, and did not become hot; whereas the water containing the 4 oz. of the same acid of vitriol, and got so very hot, that the vessel could scarcely be held in the hand, even after having been diluted at different times with 2 quarts of water. This shows that there is a great difference in the two, and that it is not entirely owing to strength. The heat produced by the vapours passing through the water, was tried at another distillation, and raised the quicksilver in Fahrenheit's thermometer to 110 degrees.

In rectifying caustic volatile alkali with lime, there is likewise a very great quantity of elastic air set free, highly charged with volatile alkali, which condenses in water and heats it. Water may be so strongly charged with this vapour, that it will make very strong Eau de luce, nay, much stronger than that which we said before was distilled and set apart for Eau de luce; but it is necessary, as mentioned before, to make use of 2 stone bottles, changing them as often as they get warm.

Exper. 2.—On the distillation of the acid of salt by means of the acid of vitriol; see the apparatus fig. 2. * A green quart retort, coated with loam, was made use of for this experiment, and it was placed in a reverberatory furnace on a naked fire; 14lb of common salt was put into it, and on that the like quantity of oil of vitriol, which had been diluted the day before with 7lb. of water; the retort was then immediately luted to the recipient; and the distillation conducted in the common manner: the operation continued 16 hours, when hardly any more liquor would come over with a strong fire. To condense the vapours, 2 stone gallon bottles, with 3 quarts of water in each, were made use of, as in the former experiment.

In this operation there was obtained 9lb. 5oz. and $\frac{1}{2}$ of spirit of salt, which dropped into the bottle c; the 6 quarts of water in the stone bottles increased in weight 6lb. 12 oz. and $\frac{1}{2}$; the caput mortuum weighed 18lb. 6 oz.; so that in this operation there was only a loss of 8 oz. which is but $\frac{1}{70}$ part of the whole, which probably was mostly elastic air.

Exper. 3.—The same operation was repeated with a slower fire, which continued for 23 hours, after which time hardly any more liquor would come over with a strong fire. There were here produced 11lb. 10 oz. of spirit of salt, in the bottle c; the 6 quarts of water increased in weight 3lb. 10 oz. and the caput mortuum weighed 19lb. 4 oz.; the loss was the same as in the foregoing experiment.

In order to know the different degrees of strength of the acids produced in these two experiments, they were saturated with a fixed alkali, dissolved in water. Four ounces of the acid in experiment 2, which distilled into c, took of the alkaline liquor to be saturated 13 oz. 5 dr. 2 scr. As much of the water † in experiment 2, as contained 4 oz. of vapour, took to be saturated 1 lb. 9 oz. Four ounces of the acid in experiment 3, which dropped into c, took of the same alkaline liquor to be saturated, 12 oz. $\frac{1}{2}$. As much of the water of experiment 3, as contained 4 oz. of vapour ‡, took to be saturated, 2 lb. 6 oz. Four ounces of oil of vitriol, which was to water in weight as 24 to 13, took of the same alkaline liquor to saturate it 1lb. 10 oz. 7 dr.; which shows that oil of vitriol is not so strong an acid as the vapour of spirit of salt, when condensed in water and distilled slowly, as in experiment 3.

* What goes by the name of a quart retort, holds better than 2 gallons of water.—Orig.

† The water of the 2 bottles was mixed together; for they were of different strength.—Orig.

‡ The water of these 2 bottles were likewise mixed together for the same reason.—Orig.

From the foregoing experiments it appears, that 1 lb. of the spirit of salt vapour, condensed in the water in experiment 2, is to 1 lb. of the acid of salt, which dropped into c of the same experiment, as 200 is to 109, which is nearly double; and therefore the 6 lb. 12 oz. and $\frac{1}{2}$ of the vapour, which condensed in the water, is equal very nearly to 13 lb. 1 oz. of the acid which is distilled in c: so that by this method of distillation, this great proportion of acid is saved, and those disagreeable suffocating fumes avoided.

In experiment 3, 1lb. of the acid vapours, which condensed in the water, is to 1lb. of the acid of salt which dropped into c, as 131 is to 50, or as $2\frac{3}{50}$ to 1; and therefore the 3 lb. 10 oz. of acid vapours, which condensed in the water is almost equal to 9 lb. and $\frac{1}{2}$ of what distilled into c.

It further appears, that the slower the distillation is conducted, the more concentrated are the acid vapours that condense in water. In order to see whether there was any difference in the strength of the acid vapours, which were condensed in the water from the first to the last of the distillation, the following experiments were made.

Five pound of common salt, with 5 lb. of oil of vitriol, were distilled in a tubulated retort; and 3 bottles, with an equal quantity of water in each, were made use of to condense the vapours. The first bottle increased in weight 3 oz. and during this time, which was 12 hours, there was no fire under the retort; that bottle being taken away, another bottle put under, a fire was made: this bottle increased in weight 1 lb. and half an ounce, the third bottle increased 10 oz. and a half.

As much water of each of the 3 bottles, as contained one ounce and a half of the acid fumes, was saturated with an alkali dissolved in water.

The water of the 1st bottle took to be saturated 11 oz. $\frac{1}{2}$ a dram. The 2d bottle took up 10 oz. 2 dr. 2 scr. The 3d bottle 10 oz. 1 dr. An ounce and half of oil of vitriol, which was to water, as 226 to 118 nearly, took up of the same alkali 7 oz. 6 dr.

By which it appears that the fumes, which first arose without fire, are stronger than the second, and the 2d than the 3d. It appears further, that the most concentrated portion of the acid of sea salt is the most volatile, and that in strength it is to the oil of vitriol mentioned before, as $44\frac{1}{2}$ to 31.

* In order to try the purity of the acid vapours, which were condensed in the water, and of the acid which distilled into the bottle c, the following experiments were made, and are marked a, b, c, d.

* This depends on the property of the acid of vitriol, and the acid of sea salt, combined with a calcarious earth; for this earth, combined with the acid of sea salt, forms a very soluble substance; whereas the same earth, with the acid of vitriol, forms a substance insoluble, or almost so, called selenite.—Orig.

(a) Four ounces of the spirit of salt of the 2d experiment, was perfectly saturated with 4 oz. of whiting. (b) Four ounces of the spirit of salt of the 3d experiment, was perfectly saturated with 4 oz. of ditto. (c) As much water as contained 4 oz. of vapour of the 2d experiment, was saturated with 5 oz. of ditto. (d) As much water as contained 4 oz. of vapour of the 3d experiment, was saturated with 6 oz. of ditto. The reason of using more whiting with some than with others, was on account of the different strength of the acids; and as there was a greater quantity of whiting than necessary used in these experiments, to saturate the acids, the undissolved part must consist of whiting; and if any acid of vitriol in the acids, of whiting and selenite. In order to separate the selenite from the whiting, a large portion of distilled vinegar was made use of, which dissolves the whiting, it being a calcarious earth; and in order to promote the solution, heat was made use of.

The undissolved part of (a) being perfectly saturated with a sufficient quantity of distilled vinegar, and afterwards repeatedly washed with pure water, and dried, weighed ½ oz. and 26 gr.—(b) treated as (a) weighed ½ oz. and 52 gr.—(c) treated as (a) weighed 39 gr.—(d) treated as (a) weighed 42 gr.—* One ounce of whiting treated as (a) left 7 gr.

From these experiments it appears, that the 4 ounces of acid marked (a) contain as much acid of vitriol as will make ½ an oz. less 2 gr. of selenite.—4 oz. of acid marked (b) ½ an oz. and 24 grs. of ditto.—4 oz. of the acid vapour marked (c) 4 gr. of ditto.—4 oz. of the acid vapour marked (d) none.—Hence it is evident, that the vapour of the acid of salt condensed in water, when distilled slowly, contains no acid of vitriol; and that even when it is distilled quick, it contains so small a quantity as is not worth notice.

If 10 oz. of sea salt are distilled in the common manner, with an equal quantity of oil of vitriol unmixed with water, there only distil 2 oz. of spirit of salt; whereas, if distilled in this new manner, we not only obtain the like quantity, but likewise 4 oz. and ½ more, which are condensed in the water; so that in making this concentrated spirit of sea salt, there is a saving of above double the quantity, which would be lost in the common method of operating.

Of the heat produced by the vapours of spirit of salt passing through water, spirit of wine, and oil of turpentine.—Three quarts of water were put into a gallon stone bottle, and made use of to condense the vapours, as in experiment the 2d, fig. F; in 3 hours and a half after the fire was made under the retort, the water in the stone bottle had acquired the degree of 212, which is the mark of boiling water in Fahrenheit's thermometer; and at this time there was

* As whiting contains some parts which are not soluble in distilled vinegar, it was necessary to know how much of this an ounce contained, which must be deducted in proportion to the quantity used for the experiments, a, b, c, and d.—Orig.

scarcely 2 oz. of spirit of salt distilled into the bottle, fig. c. The receiver and bottle c seemed cold to the touch; the water at F had increased 2 lb. 3 oz. Another like bottle, with the same quantity of water, being put in the room of this, in some time acquired the same degree of heat. The fumes seem to condense very well until the water acquired a heat within 12 degrees of boiling water. Spirit of wine rectified, made use of instead of water to condense the vapours, acquires a heat equal to 188 degrees; and it becomes of a deep brown colour, though transparent.

Oil of turpentine applied to the same use, acquires a heat of 12 degrees above that of boiling water, or 224 degrees; it becomes of a dark brown colour, though transparent, and has a disagreeable bituminous smell. The thermometer not measuring more than 213 degrees, could not be left in with safety any longer. Another time oil of turpentine was made use of to condense the vapours, which proceeded from 1lb. $\frac{1}{2}$ of sal ammoniac, with 1lb. $\frac{1}{2}$ of oil of vitriol, and $\frac{3}{4}$ of a pound of water: here it did not get near so hot, nor so high coloured, as in the experiment, but was for the most part congealed. The difference of these two experiments may, perhaps, be owing to the smallness of the quantity of the ingredients in the last process; for in the first, there was 14 lb. of salt, 14lb. of vitriol, and 7 lb. of water.

Of the re-absorption of air in distillations.—In all distillations a quantity of elastic air is set free in the beginning; but afterwards there is a re-absorption of the same; the following experiment was made to show how great it is in some cases. For the apparatus, see fig 1.

One pound and a half of foreign sal ammoniac was put into a retort, and 1 lb. and $\frac{1}{2}$ of oil of vitriol (previously diluted the day before with $\frac{3}{4}$ of a pound of water) poured on it, and a recipient well luted to it; the recipient had a tube 31 inches, well fitted and luted to it; and this tube was immersed in a glass vessel containing a quart of water.

	lb.	oz.	dr.
The spirit of salt which was distilled, weighed.	1	2	5
The quart of water increased in weight		5	4
The caput mortuum weighed.	2	3	4
The loss in the operation was only.			3
	3	12	0

The operation was continued till the sal ammoniac began to sublime.

When no more air escaped, which might easily be perceived by its ceasing to bubble through the water, the vessel of water was taken away, and the tube was immersed in a basin of quicksilver; the mercury rose in the tube 23 inches and a half, while the recipient was too hot to bear one's hand on it longer than half a minute; when the recipient was quite cold, the mercury rose to 29 inches and $\frac{7}{16}$, and there was near one inch of spirit of salt on its surface. This experiment was tried the 11th of November, when the barometer was at 30 inches. In order to make this experiment succeed, it is of the utmost consequence to lute well the vessels.

On the Marine Æther.—The Marquis de Courtenveau, of the Royal Academy of Sciences of Paris, has published a very curious memoir in their Transactions, on the making of marine æther, by distilling spirit of wine with the * liquor fumans of Libavius; but no one has succeeded in making it with the pure spirit of salt. It was natural to conclude, from the extremely great acidity of the fumes of spirit of salt, that æther might be made by saturating rectified spirits of wine with them, and on trial it was found to answer, though not in a large quantity. The spirit of wine, charged with the acid vapours, must be distilled and cohobated, and then rectified with a slow degree of fire. †

The method that Mons. Beaumé of Paris proposed to make this æther, and which did not succeed with him on account of his not being able to condense the fumes, answered well with Mr. W., and it consists of combining the vapours of spirit of salt with those of spirit of wine. The apparatus he made use of for this purpose is described at fig. 4, and the process is as follows: 8 lb. of sea salt was put into the retort B, and 2 quarts of rectified spirit of wine into the retort D; 3 pints of the same spirits of wine were put into each of the glass vessels I and K, in order to condense the fumes, one not being sufficient; all being well luted and secured, the spirits of wine in D were made to boil, and then 7 lb. of oil of vitriol was poured on the salt in the retort B, at 10 or 12 different times, 7 minutes between each time, lest the mixture should boil over; then a fire was made under this retort, and both fires kept up till the operation was over. The quantity of liquor in the vessels I and K, increases considerably, from the vapours that condense there; and the vessel I in particular grows very hot, and being highly charged with vapour, is rendered incapable of condensing any more; the vapours then pass on to the vessel K, and heat that also.

The liquor then that was distilled into the vessel F, was mixed with the liquor of the vessels I and K, then being ‡ distilled, cohobated, and rectified slowly with slacked lime, produced a very subtile penetrating æther: it is very remarkable, that this, though free from acid, on mixing it with water, caused a violent ebullition.

* The liquor fumans is made by distilling mercury sublimate with tin, and is composed of the acid of salt united with tin.—Orig.

† As I have shown before, that the vapours of the acid of salt, which condense in water, are free from the acid of vitriol, we may be certain, that the acid of vitriol did not contribute to form this æther.—Orig.

‡ Spirit of wine was used likewise here to condense the vapours; and though the distillation was conducted with a very slow fire, yet the spirit of wiue grew very hot. Spirit of wine was likewise used to condense the vapours in the cohobation, but they did not grow hot. This liquor without cohobation affords æther, but not so great a quantity.—Orig.

An expeditious Method of making Nitrous Æther by Distillation, without Fire.—(See fig. 5.) Pour 6 oz. of the most concentrated spirit of nitre, little by little, on 8 oz. of rectified spirit of wine, shaking the vessel each time in which the mixture is made. Then convey it by a long funnel through the opening of the head at C, into the matrass A; the opening is afterwards secured by a glass stopper; in warm weather this mixture grows hot in 5 or 6 minutes, and distils in a stream into the vessel E, and is over in about half an hour. Before the matrass grows cold, a fresh mixture is put in as above, and so on for 5 or 6 times, till there is liquor enough distilled. This liquor being slowly rectified with slacked lime, makes very fine æther. The spirit of wine, which was put into the vessels E and F, to condense the vapours, is so highly charged with æther, that it will separate on washing with water. This spirit of wine is also an exceeding rich spiritus nitri dulcis. What remains in the matrass, contains a quantity of spirit of wine, which may be separated by distillation.

On the Distillation of the Nitrous Acid; see fig. 2.—The quantity that is condensed in water, during the distillation of this acid spirit, is so small, that it would be scarcely worth saving, if it was not to prevent those noxious fumes, which have such an effect on the lungs of the operator, as frequently to make him spit blood. Water highly charged with these fumes by repeated distillations becomes blue, and retains its colour.* Mr. W. once distilled, in an iron body with a stone head, 30 lb. of nitre, with 60 lb. of green vitriol, which he had calcined to whiteness, and was obliged to make use of two vessels of water, as in fig. 5, at F and G, to condense the vapours: this water became blue in one distillation, and continued so for 18 months, till he made use of it. A great quantity of air was set free from the beginning to the end of the distillation, owing in a great measure to the acid fumes acting on the iron body; for if distilled in a glass or stone vessel, the quantity of air is not near so considerable. The nitrous fumes condensed in water, in making the spiritus nitri fortis, appear to be more acid than the strongest oil of vitriol made use of for the experiments on spirit of salt. Water is not heated by these fumes; owing probably to the smallness of the quantity which condenses in it.

A further Application of this New Method of Distillation.—In the distillation of the oil of vitriol, a great part of the acid comes over sulphureous, and is very hard to condense; but, by passing it through water, this condensation is

* Oil of vitriol was used in this operation, to set free the acid of nitre; and Mr. W. found on trial the fumes condensed in the water to be a pure spirit of nitre; whereas, in the other operation, where calcined vitriol or copperas was used, the fumes contained some acid of salt. This led him to try the common green copperas, and he found it contained a portion of iron united to the acid of salt: whereas the Dantzic copperas, or vitriol, contains no acid of salt, and therefore is fitter to make an aquafortis for the refiners' use.—Orig.

easily obtained: however, a greater quantity of water is necessary for this operation than for the spirit of salt, though the water becomes but slightly acid, yet it is greatly sulphureous, and at the same time acquires no heat. The sulphureous acid obtained by other means, as by distilling the acid of vitriol with mercury, and other substances, is likewise condensible. Further, this sulphureous acid of vitriol may, by 2 or 3 slow rectifications, be deprived of its acid; but it will retain its penetrating sulphureous gas-like smell.

The vapours which arise in the deflagration of nitre, with charcoal, antimony, &c. commonly called clyssus, are very hard to condense; but, by making them pass through water, their condensation is thoroughly effected. See fig. 6.—In the rectification of phosphorus, if water is made use of to condense the vapours, it will become as white as virgin wax, and almost as pliable; which seems to be owing to the water which prevents its burning.—In the distillation and rectification of the vitriolic æther, it is of advantage to make use of spirit of wine to condense the vapours, which otherwise might have been dissipated.—Besides these, a great many other things, too tedious to mention, may be condensed in water, or spirit of wine, to a very great advantage. Mr. W. cannot conclude, without mentioning that this new method of distillation bids fair to discover the mercurial and colouring earths of Becher; for by this method we can condense the most volatile parts of all substances, far better than by any other means. And he acknowledges that he received the first hint of it from the common apparatus for reviving mercury from cinnabar.

Explanation of pl. 14.—Fig. 1, A, a glass retort; B, a glass receiver; C, a bottle to receive what distils; F, a glass, or stone vessel with water; The recipient B, has a spout at the bottom, which conveys the liquor which distils into the bottle C; at the end there is a spout D; E, a crooked glass tube $\frac{1}{10}$ and $\frac{1}{2}$ of an inch bore; O, a cork with a semicircular notch to stop the bottle F.

Fig. 2, A, a glass retort; B, a glass receiver; C, a bottle to receive the distilled liquor; HH, glass or stone vessels, with glass stoppers, ground and fitted to LL; F, a stone bottle with water; D, a crooked tube, as at E, fig. 1; I, another crooked tube; K, a cork, with two semicircular notches to fit the crooked tubes to the vessel H.

Fig. 3, A, an iron body with a stone head, which has a stopper at b; B, a stand to support the receivers and bottles.

Fig. 4, A, the furnace, in which is placed the retort; B, a glass tubulated retort, which is to be coated with loam up to B; C, another furnace; D, a tubulated retort, fixed in a vessel with sand; E, a stone vessel, wherein the vapours of B and D are combined together; F, a bottle to receive the liquor which distils; G, a large tube fitted to E, about $\frac{3}{4}$ inch bore; H, a crooked pipe about $\frac{1}{4}$ inch bore; I and K, glass vessels containing spirit of wine; L, a crooked glass tube.

Fig. 5, A, a glass matrass about $4\frac{1}{2}$ feet high: B a glass head, with a spout and glass stopper C; H, a glass tube; P, the receiver; E, the bottle to receive the liquor which distils; F and G, glass vessels containing spirit of wine, HH, crooked tubes.

Fig. 6, A, an iron or earthen retort; B, the upper part of the retort, with an opening at top, which is to be stopped occasionally; CCCC, crooked stone pipes; DDDD, glass receivers, containing water; E, a crooked spout, proceeding from the last receivers, to let out the air that is set free in the operation.

END OF THE FIFTY-SEVENTH VOLUME OF THE ORIGINAL.

I. On the Eruption of Mount Vesuvius, in 1767. By the Hon. Wm. Hamilton. Dated Naples, Dec. 29, 1767. Vol. LVIII. An. 1768. p. 1.

The late violent eruption began Oct. 19, 1767, and is reckoned to be the 27th since that which, in the time of Titus, destroyed Herculaneum and Pompeii. The eruption of 1766 continued in some degree till the 10th of Dec. about 9 months in all, yet in that space of time the mountain did not cast up a 3d part so much as the quantity of lava which it disgorged in only 7 days, the term of this last eruption. On the 15th of December, last year, within the ancient crater of Mount Vesuvius, and about 20 feet deep, there was a crust, which formed a plain, not unlike the solfaterra in miniature; in the midst of this plain was a little mountain, whose top did not rise so high as the rim of the ancient crater. Vesuvius was quiet till March 1767, when it began to throw up stones, from time to time; in April the throws were more frequent, and at night fire was visible on the top of the mountain; or, more properly speaking, the smoke, which hung over the crater, was tinged by the reflection of the fire within the volcano. These repeated throws of cinders, ashes, and pumice stones, increased the little mountain so much, that in May its top was visible above the rim of the ancient crater. The 7th of August there issued a small stream of lava, from a breach in the side of this little mountain, which gradually filled the valley between it and the ancient crater; so that the 12th of September the lava overflowed the ancient crater, and took its course down the sides of the great mountain; by this time, the throws were much more frequent, and the red hot stones went so high as to take up 10 seconds in their fall. Padre Torre, a great observer of Mount Vesuvius, says they went up above 1000 feet.

The lava continued to run over the ancient crater in small streams, sometimes on one side, and sometimes on another, till the 18th of October, when there was not the least lava to be seen, owing probably to its being employed in forcing its way towards the place where it burst out the following day. Sir W. was not surprised on the 19th following, at 7 in the morning, to perceive every symptom of the eruption being just at hand. From the top of the little mountain issued a thick black smoke, so thick that it seemed to have difficulty in forcing its way out; cloud after cloud mounted with a hasty spiral motion, and every minute a volley of great stones were shot up to an immense height in the midst of these clouds; by degrees, the smoke took the exact shape of a huge pine tree, such as Pliny the younger described in his letter to Tacitus, where he gives an account of the fatal eruption in which his uncle perished. This column of black smoke, after having mounted an extraordinary height, bent with the wind towards Caprea, and actually reached over that island, which is not less than 28 miles from Vesuvius.

Before 8 in the morning, the mountain had opened a mouth, without noise, about 100 yards lower than the ancient crater, on the side towards the Monte di Somma: as soon as it had vent, the smoke no longer came out with that violence from the top. On a sudden, about noon, Sir W. heard a violent noise within the mountain, which split; and, with much noise, from this new mouth a fountain of liquid fire shot up many feet high, and then like a torrent rolled on directly towards him. The earth shook, at the same time that a volley of pumice stones fell thick upon him; in an instant, clouds of black smoke and ashes caused almost a total darkness; the explosions from the top of the mountain were much louder than any thunder he ever heard, and the smell of the sulphur was very offensive. About 2 in the afternoon another lava forced its way out of the same place as the lava came last year, so that the conflagration was soon as great on one side of the mountain as on the other.

The noise and smell of sulphur increasing, Sir W. removed from his villa to Naples; and he thought proper, as he passed by Portici, to inform the court of what he had seen; and humbly offered it as his opinion, that his Sicilian majesty should leave the neighbourhood of the threatening mountain. However, the court did not leave Portici till about 12 o'clock. Sir W. observed, in his way to Naples, which was in less than 2 hours after he had left the mountain, that the lava had actually covered 3 miles of the very road through which he had retreated. It is astonishing that it should have run so fast; as he afterwards saw that the river of lava, in the Arrio di Cavallo, was 60 and 70 feet deep, and in some places near 2 miles broad. When his Sicilian majesty quitted Portici, the noise was greatly increased, and the confusion of the air from the explosions was so violent, that, in the king's palace, doors and windows were forced open, and even one door there, which was locked, was burst open. At Naples, the same night, many windows and doors flew open. Besides the explosions, which were very frequent, there was a continued subterraneous and violent rumbling noise, which lasted this night about 5 hours. Sir W. imagined that this extraordinary noise might be owing to the lava in the bowels of the mountain having met with a deposition of rain water, and that the conflict between the fire and the water may, in some measure, account for so extraordinary a crackling and hissing noise. In the great eruption of Mount Vesuvius in 1663, it is well attested, that several towns, among which Portici and Torre del Greco, were destroyed by a torrent of boiling water having burst out of the mountain with the lava, by which thousands of lives were lost. About 4 years ago, Mount Etna in Sicily threw up hot water also, during an eruption.

Tuesday the 20th, it was impossible to judge of the situation of Vesuvius, on account of the smoke and ashes which covered it entirely, and spread over Naples also, the sun appearing as through a thick London fog, or a smoked glass; small ashes fell all this day at Naples. The lavas on both sides of the mountain ran violently; but there was little or no noise till about 9 o'clock at night, when the same uncommon rumbling began again, accompanied with explosions as before, which lasted about 4 hours; it seemed as if the mountain would split in pieces; and indeed it opened this night by a large orifice. Wednesday 21st was more quiet than the preceding days, though the lavas ran briskly. Portici was once in some danger, had not the lava taken a different course, when it was only a mile and a half from it; towards night the lava slackened.

Thursday 22d, about ten in the morning, the same thundering noise began again, but with more violence than the preceding days. The ashes, or rather small cinders, showered down so fast, that the people in the streets were obliged to use umbrellas, or flap their hats, these ashes being very offensive to the eyes. The tops of the houses, and the balconies, were covered above an inch thick with these cinders. Ships at sea, 20 leagues from Naples, were also covered with them, to the great astonishment of the sailors.

Friday 23d, the lavas still ran, and the mountain continued to throw up quantities of stones from its crater; there was no noise heard at Naples this day, and but little ashes fell there.

Saturday 24th, the lava ceased running; the extent of the lava, from the spot where it broke out, to its extremity where it surrounded the chapel of Saint Vito, is above 6 miles. In the Atrio di Cavallo, and in a deep valley, that lies between Vesuvius and the hermitage, the lava is in some places near 2 miles broad, and in most places from 60 to 70 feet deep. The lava ran down a hollow way, called Fossa grande, made by the currents of rain water; it is not less than 200 feet deep, and 100 broad; yet the lava in one place has filled it up. On this day Vesuvius continued to throw up stones as on the preceding days; during the whole of this eruption it had differed in this circumstance from the eruption of 1776, when no stones were thrown out of the crater from the moment the lava ran freely.

Sunday 25th, small ashes fell all day at Naples: they issued from the crater of the volcano, and formed a vast column, as black as the mountain itself, so that the shadow of it was marked out on the surface of the sea; continued flashes of forked, or zig-zag lightning shot from this black column, the thunder of which was heard in the neighbourhood of the mountain, but not at Naples; there were no clouds in the sky at this time, except those of smoke issuing from the crater of Vesuvius. Monday 26th, the smoke continued, but not so thick, neither were there any flashes of the mountain lightning. Tuesday 27th, no more black smoke, nor any signs of eruption.

This paper was accompanied with some engraved views of the mountain, and the adjacent country; for which, and several other particulars relating to this explosion, we refer to the separate volume published by Sir W., in which he collected all these accounts from the Philos. Trans.

II. Extract of a Letter, dated Vienna, April 4, 1767, from Father Joseph Liesganig, Jesuit, to Dr. Bevis, F. R. S., containing a short Account of the Measurement of Three Degrees of Latitude under the Meridian of Vienna. From the Latin. p. 15.

These 3 degrees measured by father Liesganig, constitute the arc between Sobieski, near Brunn, in Moravia, and Waradin, in Croatia. The terrestrial arc is constituted by 22 large triangles, connected by measured bases, of more than 6000 Paris toises each. The celestial arc was determined by many observations of fixed stars, made with a 10 foot sector, at several stations; by which he deduced the length both of the whole arc, and of several of the intermediate parts. Hence he found the length of the degree between Vienna and Gratz, 186 toises less than that between Vienna and Brunn, towards the north, but that between Waradin and Brunn almost 300 toises greater. A difference, so great, he ascribes to the attraction of certain mountains near some of the stations. A synopsis of the celestial and the terrestrial arcs here follow.

Arc of the Celestial Meridian.

	c Dracon	γ Dracon.	δ Cycni.	α Cycni.	Capra.	β Aurigæ.	Medium.	
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	
Between Sobieski, & Brunn.	....	0 3 35.8	....	....	....	....	0 3 35.8	1
& Vienna	....	1 2 29.6	....	....	....	1 2 27.8	1 2 29.0	2
& Gratz	....	2 10 54.3	....	....	....	2 10 55.8	2 10 55.0	3
& Warad.	....	....	....	....	....	2 56 45.7		
Between Brunn & Vienna	0 58 53.2	0 58 53.6	....	....	....	....	0 58 53.5	4
& Gratz	2 7 18.1	2 7 18.5	....	....	....	....	2 7 18.3	5
Between Vienna & Gratz	1 8 24.9	1 8 24.7	....	....	....	1 8 28.0	8 8 25.8	6
						or	1 8 24.8	7
& Warad.	....	....	....	....	....	1 54 17.9	1 54 17.9	8
Between Gratz & Waradin	....	....	0 45 50.5	0 45 48.9	0 45 50.1	1 45 49.9	0 45 49.9	9

	Amplit. of the celestial arc.	Distance of the parallels.	From the series of triangles.	Magnitude of 1 degree. Vienna toises.	Paris toises.	Red. to the level of the sea.
	° ′ ″					
Between Sobieski & Warad.	2 56 45.8	172808.1		58657.5	57079.8	57077.4
Between Sobieski & Vienna	1 2 29.0	61094.0	I.	58665.9	57088.0	57085.6
	....	61090.7	II.	58602.7	57084.9	57082.5
	....	61088.8	III.	58660.9	57083.1	57080.7
Between Brunn & Vienna	0 58 53.5	57588.5	....	58672.3	57094.2	57091.8
	....	....	Medium.	58665.4	57087.5	57085.1
Between Vienna and Gratz.	1 8 25.8	66681.8	....	58467.2	56894.6	56892.2
or	1 8 24 8	....	....	58481.4	56908.5	56906.1
	....	....	Medium.	58474.3	56901.5	56899.1
Between Gratz & Warad.	0 45 49.9	45032.3	....	58953.5	57367.9	57365.5

III. An Essay on the Force of Percussion. By William Richardson, M. D. p. 17.

Those who maintain that the force of percussion is as the velocity of the striking bodies, when they account for the impressions made in soft bodies (which are found, by experiment, to be as the squares of the velocities), inform us, that the time ought to be taken into the account; which being as the velocity of the impinging body, the impression will of course be as the time into the velocity, or, which is the same thing, as the square of the velocity. Those who, on the contrary, insist that the force of percussion is in proportion to the squares of velocity, finding from experiment that in soft bodies the velocity after percussion falls short of this estimate, would make us believe, that in compressing the parts of those bodies, a certain degree of force must necessarily be lost, which, being added to what remains after percussion, will sufficiently confirm the truth of their doctrine. How then are these different effects to be accounted for, and in what manner are they to be deduced from the same cause? This diversity of appearances, Dr. R. has for some time suspected, might proceed from the nature of cohesion: that while the force of percussion produced an effect on the whole mass of matter which receives the stroke, in proportion to the velocity of the impinging body; it might, at the same time, in separating the cohering parts from each other, produce an effect in proportion to the square of the velocity.

In order to make a further discovery in this matter, Dr. R. determined first to make experiments on such soft bodies as have a considerable degree af cohesion; and then to try those bodies, when dried and reduced to powder, and by that means deprived of their cohesion; which experiments, when compared with each other, would give him an insight into this intricate affair, and at the same time disclose that beautiful simplicity, which nature observes in all her operations. His apparatus for making the experiments consists of 4 balls exactly spherical, 2 iron branches, and a small lead cistern. The balls are each of them 2 inches in diameter; 2 are of brass, and 2 of box-wood; one of each sort is solid, and the other hollow; that which is hollow is only half the weight of the solid one, and may be opened by means of a screw in the middle. The iron branches are to give the balls their proper directions; they have each of them a small brass pulley in the fore part, and in the hind part a kind of hook, which fastens them to staples at different heights; one of the branches is 2 inches long, and the other 4 inches, exclusive of pulleys; by which means the balls when let fall are directed to different parts of the surface they strike upon. The lead cistern is of an oblong form, that the contained matter may, at the same time, receive 2 distinct impressions; either when balls of different weight are let fall, or the same ball is let fall from different heights; its length is 6 inches, its breadth 4 inches, and its depth 2 inches.

The matter he found best suited to the purpose is stiff clay, tempered in such a manner as to be smooth and uniform, with the same reduced to powder, after having been baked in an oven, as also after having, by a still stronger heat, been converted into brick. The cistern is by turns filled with these different materials, which are to be closely and uniformly pressed down, so as to leave the surface quite level: in effecting which, great caution is required, more particularly in regard to the powders; as they wlll not distinctly retain the impressions, unless they have some small degree of moisture, or be very closely pressed down; in both which cases they acquire such a degree of cohesion, as of course must render the experiments more or less imperfect. Things being thus prepared, in order to try the necessary experiments, Dr.R. fixed the staples, and by their means the branches, at the following heights, viz. 2 feet, 4 feet, and 8 feet; the result of which experiments was as follows.

When the brass balls, in weight to each other as 2 to 1, were let fall on tempered clay, from 4 feet to 8 feet respectively, the impressions made were on various trials found to be equal.—When the wood balls, being to each other as 2 to 1, were let fall from the same heights, on dried clay pulverised, that from 4 feet generally made the deeper impression.—When the wood balls were let fall from the same heights on brick-dust, that from 4 feet constantly made the deeper impression.—When the lighter brass ball was let fall on tempered clay, from 2 feet and 8 feet, the impressions were to each other as 1 to 4.—When the lighter wood ball was let fall on dried clay pulverised, from the same heights, the impressions were, so far as the eye could judge, nearly in the proportion of 1 to 3. —When the same ball was let fall on brick-dust, from the like heights, the impressions were not much short of the proportion of 1 to 2.

From these experiments it plainly appears: first, that the impressions made in soft clay are in proportion to the heights, whence the balls are let fall, consequently as the squares of their respective velocities. 2dly. That the impressions in pulverised clay, recede considerably from that proportion, being as it were in the medium between the squares of the velocities and the velocities themselves. 3dly. That the impressions in brick-dust are nearly in a subduplicate proportion o the heights whence the balls are let fall, consequently vary but little from the proportion of the velocities acquired. Whence Dr. R. apprehends it clearly follows, that the impressions made in soft bodies, by hard ones striking on them, vary from each other, according to the degree of cohesion in the respective soft bodies; and that the impressions would be in exact proportion of the velocities, could their form be perfectly retained by bodies quite void of cohesion.

IV. On the Connection between the Parallaxes of the Sun and Moon; their Densities; and their Disturbing Forces on the Ocean. By Patrick Murdoch, D. D., F. R. S. p. 24.

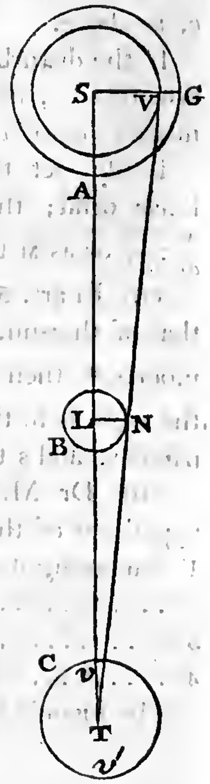
I. The length of a second-pendulum at the equator, and on a level with the sea, being 36 inches $7\frac{1}{10}$ lines, Paris measure, according to the accurate observations of M. de la Condamine, that length, properly corrected, will, by the reasoning in a former letter, (p. 87, &c. of this vol.) give the distance of a moon circulating round an unmoveable earth, equal to 59.95792 semidiameters of the equator. For the logarithm of this number, which is 1.7778438, write l. And let L be the logarithm of some greater mean distance, inferred from observations of the moon's parallax; and if r be the natural number of the logarithm $3 \times (L - l)$, and M be taken equal to $\frac{1}{r-1}$, the mass of the earth will be to that of the moon, as M to 1. Conversely, if M be any how determined, its equal $\frac{1}{r-1}$, and r, with its logarithm $3 \times (L - l)$ are known; $\frac{1}{3}$ of which is $L - l$, to be added to l. For instance, if, with Sir Isaac Newton, we put $M = 39.788$, the distance will be 60.4557, the logarithm L being 1.7814372.

II. If, for each of these three, the moon's mass, her accelerative force on the earth, and her distance from the earth's centre, we write $\varphi = 1$: the accelerative force of the earth on the moon will be represented by M, the mass just now computed. And if F is the sun's accelerative force on the earth, x his distance in semidiameters of the lunar orbit, Q the ratio of a sidereal year to a periodic month; we have (by cor. 2, prop. 4, Princip. 1) $\frac{F}{x} = \frac{M}{Q^2}$; a given ratio in given terms.

III. The terms F, x, therefore, must involve a common factor; by which being divided, the quotient may be $\frac{M}{Q^2}$. And this might be obtained innumerable ways, were we to consider the ratio $\frac{F}{x}$ merely as an abstract quantity, altogether unrestricted; it were only putting $M^n \times Q^p = F$. And $\frac{Q^{2+p}}{M^{1+n}} = x$, or $M^{1-n} Q^p = F$, and $\frac{Q^{2+p}}{M^n} = x$: so as the sum of the indices of M should be unity, and the difference of those of Q should be 2. But though the quantities F, x, are as yet unknown, they are not for that indeterminate and variable, as such a liberty of substitution would import; and all substitutions which imply the contrary, all indices which the theory disowns, or which are inconsistent with observation, are to be rejected. In a word, the indices, n, p, ought each of them to be unique, and determinate (sine compare),* as the

* See Neut. Arith. Universal. in the schol. to prob. xxiv. The maxima and minima of variable quantities; the coordinates belonging to a double point, or to a point of reflexion, or contrary flexure, rays of curvature, limits of ratios, &c. All these are examples of the unique; that is, of quantities in a state that is distinguished from and exclusive of all others—Orig.

quantities F and x are in nature. Dr. M. takes therefore $p = 1$, and $n = \frac{1}{3}$; that is, $F = M^{\frac{1}{3}} \times Q$, $x = \frac{Q^3}{M^{\frac{1}{3}}}$. See the examples in the table subjoined, on different suppositions of the moon's distance.



IV. The accelerative forces of two spherical bodies, A, B, on a third C, are directly as their masses, and inversely as the squares of their central distances ST, LT, or of x and d: which may be thus expressed, $\frac{F}{\phi} = \frac{A}{B} \times \frac{d^2}{x^2}$. But the masses, A, B, being as their respective densities, which call s, m, and the cubes of R, r, the semidiameters of the spheres, conjunctly; if we write for $\frac{A}{B}$ its equal, we have $\frac{F}{\phi} = \frac{s}{m} \times \frac{R^3}{r^3} \times \frac{d^2}{x^2}$. Let the mass C be to B, as M to 1; and f, its force on B, will be ϕM, or $\phi = \frac{f}{M}$, which gives $\frac{MF}{f} = \frac{s}{m} \times \frac{x}{d}$, and $\frac{F}{f} = \frac{sx}{Mmd}$: supposing the apparent semidiameters of A and B to subtend the same angle at the centre of C, and thence R to be to r, as x to d.

V. But if SG, the semidiameter of A, be to the supposed semidiameter SV, as q to 1, then, the density s remaining, the accelerative force of A (proportional to its magnitude) will be increased in the triplicate of that ratio, that is, we now have $\frac{F}{f} = \frac{q^3 s}{Mm} \times x$, putting $d = 1$. And the three bodies A, B, C, representing the sun, moon, and earth; and Q being the ratio of the periods of the earth and moon, it is $\frac{F}{f} = \frac{x}{Q^2}$, by the corol. quoted in art. 2. Whence $\frac{s}{m} = \frac{M}{q^3 \times Q^2}$.

VI. This accelerative force of A remaining, imagine the semidiameter SG to be reduced to its former magnitude SV; and the density of A will, at the same time, be increased to $s^1 = q^3 \times s$, and $\frac{s^1}{m} = \frac{M}{Q^2}$. In which case, namely, when the apparent semidiameters of A and B (the sun and moon) are equal, their powers to raise a tide at v, a vertex of C, will be as the densities s^1, m:* that is, as M the ratio of the earth's mass to the moon's, and Q^2 the duplicate ratio of the year and month.

Or thus: the distances of the bodies A, B, from the third C, being very great, their powers to raise a tide at v, or their disturbing forces on the ocean, will be directly as their accelerative forces at the centre of C, and inversely as their distances from it; that is, writing a, b, for the disturbing forces respectively; and for the sun's distance in semidiameters of C, the letter z, it will be $a : b :: \frac{F}{z} : \frac{\phi}{d}$.

* See the last page of Dr. Sanderson's Fluxions: or the late ingenious T. Simpson's Miscellaneous Tracts, p. 13.—Orig.

For, by the general law, a is to F, as the difference of the squares of z and (z — 1) is to the square of (z — 1); that is, as 2z — 1 to $z^2 - 2z + 1$. And the same way b is to φ, as $2d - 1$ to $d^2 - 2d + 1$; whence, halving the antecedents, and retaining only z^2, d^2, in the consequents, we have the ratio of a to b, as above.

If the disturbing force of B is exerted at v', the opposite vertex of C, b will now be to φ, as $2d + 1$ to $d^2 + 2d + 1$; and, in strictness, we ought to take a mean value of b: but this may be neglected as inconsiderable.

Lastly, let the sun's distance be again expressed in semidiameters of the lunar orbit; that is, if for z we write dx, and unity for φ, we have $a : b :: \frac{F}{dx} : \frac{1}{d}$, or as M to Q^2, as before.

VII. In art. 5, it was found that, m denoting the density of the moon, and s that of the sun, q^3 being triple the ratio of the sun's mean semidiameter to the moon's,* then will $\frac{m}{s} = \frac{Q^2 q^3}{M}$. Whence it will easily follow, that the density of the earth is to that of the sun, as $Q^2 \times S^3$ to P^3; P being the moon's horizontal parallax, and S the sun's apparent semidiameter.

VIII. Dr. M. has applied these rules, as in the following table, to the principal hypothesis of the moon's mean distance.

1° Supposing it of 60.24 (from Dr. M. Stewart).
2° . . . . 60.4 (cor. 7, prop. 37, Princip. 3).
3° . . . . 60.455 (Neut. M. being 39.788).
4° . . . . 60.493 (as by Mr. Short's calculations from the transit of ♀.)

The moon's mean distance, being in semidiameters of the equator.

			60.24	60.4	60.455	60.493
I.	Parall.	☽	57′4″.17	56′55″ $\frac{1}{15}$	56′52″	56′49″.88
II.	Mass	♀ ☽	70 4225	44.823	39.788	36.9908
III.	⊙ dist. to	☽	284.723	356.885	378.293	392.854
IV.	Parall.	⊙	12″ $\frac{1}{37}$	9″.58	9″ $\frac{1}{2}$‴	8″.69
V.	Dens.	☽ ⊙	2.77:1	4.35:1	4.9 :1	5.273 :1
VI.	Dens.	♀ ☽	1.449:1	0.9295:1	0.827 :1	0.7707 :1
VII.	Dens.	♀ ⊙	4.0128:1	4.045 :1	4.0559:1	4.06375:1
VIII.	Tides	☽ ⊙	2.5379:1	3.9873:1	4.5544:1	4.8316 :1
IX.	F. ⊙ on ♀ / F. ♀ on ☽		1.593 :1	1.9968:1	2.1194:1	2.1981 :1

* In the Principia, the semidiameters are 16′ 6″, and 15′ 38¼″; giving 9.0379755 for the logarithm of q^3. Others take a few seconds from each; which does not much alter the value of q^3. —Orig.

Remarks.—1. If it should be thought that the reasoning in art. 3 rests too much on a metaphysical principle of Leibnitz, and requires, if not an apology, at least a more formal proof: the ground of such reasoning, and its extensive use, may be more particularly explained on some other occasion. Suffice it at present to add to the note on that article, that as the given factors Q and Q^3, in F and x, may be joined with M^n, or with M^{1-n} indifferently, the case is similar to that of an equal chance at play, for the stakes n and $1 - n$, where the just expectation of each gamester is $\frac{1}{2}$, whatever be the value of n. The 5th and 6th propositions of element 1, scarcely needed any other demonstration, than that it is manifestly impossible to assign any reason of inequality, of the angles in one of these theorems, and of the sides in the other. In the following proposition, where it is proved, that 2 lines being extended from the extremities of a right line, AB, to a point C, there is no other point on the same side of AB, to which lines from A and B equal to the former can be drawn; he who holds the contrary is supposed to fix on that other point D; but why D? rather than d, d', d'', &c. he is silent; and therefore Dr. M. concludes, there is no such point different from C. And the like may be said of some other simple theorems, that are commonly demonstrated by showing the absurdity of asserting their contraries.

2. The title of this paper renders it almost needless to remind the reader, that the moon's parallax is not here proposed as the properest medium for determining that of the sun. Our data are still too uncertain for that purpose, scarcely one of them having been determined to an unexceptionable precision; and the numbers in the table show how much a small difference in the moon's distance must affect the several conclusions. It may be of use however, to know in what manner those conclusions, as well as the quantities from which they derive,* stand related to each other. For if hereafter the necessary data should be more exactly known, the calculus may be repeated; and if the transit of Venus, which is to happen in 1769, should confirm Mr. Short's calculations from that of 1761, we may thence conclude the true mean distance of the moon, better than in any other way.

3. In the mean time, if any person should have the curiosity to examine the numbers of the table, he will please to take notice: that as no two measurements, nor any two lengths of a second-pendulum, hitherto observed, make the earth of the same spheroid figure, Dr. M. has retained for the ratio of its greatest and least diameters, that of 231 to 230; answering to the hypothesis of its uni-

* The connections of F, x, Q, are manifest; and the relation of M to Q^2 is easily deduced from prop. 59, Princip. book 1.—Orig.

form density: and has thence made a degree of the equator equal to 57200 French toises.*

The measure of a second-pendulum, at the equator, Dr. M. had from M. de la Condamine, as was said before: it was corrected for the centrifugal force, and the resistance of the air; and the moon's distance, as revolving round the earth at rest, was corrected for the sun's disturbing force; which is done either by diminishing that distance in the subtriplicate ratio of 178725 to 177725; or by diminishing the length of the pendulum in the simple ratio of these numbers. In this last correction he was favoured with the advice and assistance of the Rev. Mr. Price, and of the Astronomer Royal, FF. R. S.

V. Observations on the Bones, commonly supposed to be Elephants' Bones, which have been found near the River Ohio in America. By William Hunter, M. D., F. R. S. p. 34.

Naturalists, even those of our own times, have entertained very different opinions concerning fossil ivory, and the large teeth † and bones, which have been dug up in great numbers in various parts of the world. At first, some thought them animal substances, and others mineral. When only a certain number of observations had been collected, these substances were determined to be mineral: but, the subject having been more carefully examined, they were found certainly to be parts of animals. After this point was settled, a dispute arose, to what animal they belonged. The more general opinion was, that they were bones of the elephant; and the great similitude of the fossil tusks to the real elephants' teeth, gave this opinion considerable credit. It was liable however to great objections; the bones were observed to be larger than those of the elephant; and it was thought strange that elephants should have been formerly so numerous in western countries, where they are no longer natives, and in cold countries, Siberia particularly, where they cannot now live.

We had information from Muscovy, that the inhabitants of Siberia believed them to be the bones of the mammoth, an animal of which they told and believed strange stories. But modern philosophers have held the mammoth to be as fabulous as the centaur. Of late years the same sort of tusks and teeth, with some other large bones, have been found, in considerable numbers, near the

* This was computed on the supposition, that a degree of the meridian at lat. $49\frac{1}{3}°$ is 57183 toises, but if that degree is, by a correction of M. Picart's operations, found to be only 57075 toises; the diameter of the equator, here used, ought to be diminished by about $\frac{18}{10000}$ parts.—Orig.

† It has already been observed, in the notes on Mr. Collinson's paper relative to the teeth of this animal, that Mr. Pennant and others have considered it as a species of elephant, and that it is the *American elephant* of Pennant's History of Quadrupeds.

banks of the Ohio, in North America. The French academicians became possessed of some specimens of them; and having compared them with the bones of real elephants, and with those which had been brought to France from Siberia, and with similar bones found in various other parts, determined, with an appearance of probability on their side, that they were elephants' bones.

M. Buffon gives us the following account of this decision: "All this put together leaves us no longer any room to doubt, that those tusks (defenses), and those large bones (ossemens), are truly the tusks and bones of the elephant. M. Sloane had said this, but had not proved it. M. Gmelin has likewise said so, and more positively; and he has given us some curious facts concerning this question; ——— but M. Daubenton appears to be the first who has put the matter beyond doubt, by accurate measures, by exact comparisons, and by reasons founded upon the great knowledge which he has acquired in the science of comparative anatomy."

From the first time that Dr. H. learned this part of natural knowledge, it appeared to him to be very curious and interesting; inasmuch as it seemed to concur with many other phenomena, in proving that in former times some astonishing change must have happened to this terraqueous globe; that the highest mountains, in most countries now known, must have lain for many ages in the bottom of the sea; and that this earth must have been so changed with respect to climates, that countries which are now intensely cold, must have been formerly inhabited by animals which are now confined to the warm climates.

Some time in the last spring, having been informed that a considerable quantity of elephants' teeth had been brought to the Tower, from America; and being desirous of procuring some information concerning them, Dr. H. waited on Mr. Bodington, to know the particulars, and to beg leave to examine them. He obligingly gave him a verbal account of their having been brought from the banks of the Ohio; and on the following day sent him one tusk, and one grinder, as specimens for his examination. The tusk indeed seemed so like that of an elephant, that there appeared no room for doubt. Dr. H. showed it to his brother, and he thought so too: but being particularly conversant with comparative anatomy, at the first sight he said that the grinder was certainly not an elephant's. From the form of the knobs on the body of the grinder, and from the disposition of the enamel, which makes a crust on the outside only of the tooth, as in a human grinder, he was convinced that the animal was either carnivorous, or of a mixed kind. This made Dr. H. think that the tusk itself was not a real elephant's tooth: for Mr. Bodington had told him that there were many grinders, as well as tusks, and that they were all similar to those specimens which he had sent to him. And some time after, when Dr. H. went to the Tower, and examined the whole collection which had been sent over from the Ohio, he saw that the grinders were all of the same kind. He examined two elephants' jaws in his brother's collection: he examined the tusks and grinders of the queen's two elephants: and he examined a great number of African elephants' teeth at a warehouse. From all these observations, Dr. H. was convinced that the grinder-tooth, brought from the Ohio, was not that of an elephant; but of some carnivorous animal, larger than an ordinary elephant: and he could not doubt that the tusk belonged to the same animal. The only difference that he could observe between it and a real elephant's tusk was, that it was more twisted, or had more of the spinal curve, than any of the elephants' teeth which he had seen.

Dr. H. afterwards examined also several more of the tusks and grinders, that had been sent from the Ohio, to Dr. Franklin, and to Lord Shelburne; and on the whole he was now fully convinced, that the supposed American elephant was an animal of another species, a pseud-elephant, or animal incognitum, which naturalists were unacquainted with. He imagined further, that this animal incognitum would prove to be the supposed elephant of Siberia, and other parts of Europe; and that the real elephant would be found to have been in all ages a native of Asia and Africa only.

In order to prove to the satisfaction of the Society, that the incognitum of America is of a very different species from the elephant, Dr. H. added 3 drawings of the jaw-bone of that animal; which the curators of the British museum were pleased to give him leave to take, and which Mr. Rymsdyk executed with a most scrupulous exactness: and that the comparison might be made with ease, he added 3 similar drawings, taken from the largest of the two full-grown elephants' jaws which were in his brother's collection; executed with the same care, by the same artist; and drawn to the same scale. Pl. 15, fig. 1, is an outside view of the half of the lower jaw of the American incognitum, which the Earl of Shelburne deposited in the British Museum. From the top of the condyle to the anterior extremity, the bone measured, in a straight line, 35 inches: the basis alone, in a straight line, 2 feet 4 inches. Fig. 2 is the same view of the same bone in a full-grown elephant, drawn to the same scale. Whoever will take the pains to compare these two figures with a critical eye will see that they differ so very much, not only in size, but in their general character, and in the particular parts and features, that he cannot entertain a doubt of their being the jaws of two very different animals.

Fig. 3, a view of the inside of the same jaw-bone of the incognitum. Fig. 4, a view of the inside of the same jaw-bone of the elephant. In comparing these 2 views, the difference if possible is still more manifest.

Fig. 5, a view from above of the jaw of the incognitum. Fig. 6, the same view of the elephant's jaw-bone.

In the last place, it may be observed, that as the incognitum of America has been proved to have been an animal different from the elephant, and probably the same as the mammoth of Siberia; and as grinder-teeth like those of America have been dug up in various other parts of the world; it should seem to follow, that the incognitum in former times has been a very general inhabitant of the globe. And if this animal was indeed carnivorous, which Dr. H. believed cannot be doubted, though we may as philosophers regret it, as men we cannot but thank Heaven that its whole generation is probably extinct.

VI. Observations made on the Islands of Saint John and Cape Breton, to ascertain the Longitude and Latitude of those Places, agreeable to the Orders and Instructions of the Right Hon. the Lords Commissioners for Trade and Plantations. By Capt. Holland, Surveyor General in Canada. p. 46.

The result of the observations is; that the Magdalen Islands are situated in the Gulf of St. Lawrence, in the latitude of 47° 41′ N., and between 61° and 61° 38′ W. longitude from London. The variation of the compass is 17° 30′ W., anno 1765. The Island of Entry lies in 47° 17′ north latitude, and 61° 20′ W. longitude from London. The Bird Islands are in 47° 55′ N. latitude, and bear from the E. point of Magdalen North 35° E., distant 18 miles. Bryon Island is in 47° 52′ N. latitude, and the E. point bears N. 13° W. of the E. point of Magdalen; distant 12 miles. At Louisburg and Island Battery, the latitude is 45° 54′ N. Cape North, latitude 47° 2′ N. St Paul's Island, north cove, latitude 47° 11′ N. The entrance of Dartmouth Harbour or Baye des Espagnols, latitude 46° 13′ N. Conway, or St. Anne's harbour, latitude 46° 20′ N. The north head of Colvile's Bay, or Niganiche, latitude 46° 44′ N.

VII. A Note concerning the Cold of 1740, and of this Year, 1768. By J. Bevis, M.D., F.R.S. p. 54.

Dr. Bevis's journal of astronomical observations made at Stoke Newington, in a detached or insulated observatory, whose walls were of brick, and 2 feet thick, states, that during most part of the night of the 5th day of January 17$\frac{39}{40}$ the ink in his stand-dish would freeze in a few minutes, if brought within a foot of the wall; and that the surface of the water wherein the ball of the plumb-line hung, for rectifying the position of his mural quadrant, was continually freezing, so that he was obliged to thaw it frequently, by pouring in hot water; yet was there a good fire in the room all the night. At 5 in the morning, of the 6th, a Fahrenheit's thermometer, made by himself, exposed to the north, stood somewhat lower than 10, that is, more than 22 divisions below freezing. This was the coldest night of that year there. This present year, in Brick-court, N° 1, Middle Temple, the same thermometer, exposed out of doors to the north, stood lowest on new-year's-day in the morning, to wit at 17, and once again at the same place.

VIII. Observations on the same Subject. By J. Short, F.R.S. p. 55.

From this account it appears that the cold, at one o'clock in the afternoon on the 29th of December 1739, was so great as to sink the mercury 21 divisions below the freezing point of Fahrenheit's scale, within the room, the windows being shut.

Dec. 31, 1767, at 8 in the morning, a Fahrenheit's thermometer without the window of the same room, where it had remained all night, stood at 19¾ divisions below the freezing point; but a similar Fahrenheit's thermometer within the room stood at 13¾ divisions below the freezing point; therefore the cold, that morning, was greater without the room than within it by 6 divisions of Fahrenheit's scale.

January 7th, 1768, at 8 in the morning, a Fahrenheit's thermometer without the window of the same room, where it remained all night, stood at 19¾ divisions below the freezing point; but a similar Fahrenheit's thermometer within the room stood at 12 divisions below the freezing point; therefore the cold that morning was greater without the room than within it by 8 divisions of Fahrenheit's scale.

From what has been said, we may safely conclude, that the cold, December 28, 1739, at one o'clock in the afternoon, was so great, without the window of the said room, as to sink the mercury 27 divisions at least below the freezing point of Fahrenheit's thermometer.

N. B. No fires were made in the said room, or in the 2 contiguous rooms, in the year 1739, or in the years 1767 and 1768.

IX. An Investigation of the Difference between the Present Temperature of the Air in Italy and some other Countries, and what it was Seventeen Centuries before. By the Hon. Daines Barrington, F.R.S. p. 58.

Mr. B. had long entertained a notion that the seasons are become much milder in the northern latitudes than they were 16 or 17 centuries past; and from this it has happened, that many passages in the classical writers descriptive of the severity of the climates, had struck him more perhaps than they would a common reader.

If this same question should be agitated 2000 years hence, it might receive an absolute demonstration; as a journal of the changes in a well-constructed thermometer would show the temperature which prevailed in any particular place, during the present century. No such accuracy can be expected from any passages in the classical writers; but in order to state the alteration which may have happened in so long a course of years, the most proper method seems to be to compare their accounts with those of more modern travellers, who have equally wanted the assistance of a thermometer for their observations.

Mr. B. chiefly relies on many of Ovid's letters from Pontus (though he was not only a poet, but a writer of most glowing fancy and imagination), in which he describes the effects of cold at Tomos, probably the modern Temisware, during his 7 years' residence there, and afterwards contrast this description with that of later travellers. Ovid was born at Sulmo in Italy, about 90 Roman miles s.w. from the capital. He afterwards resided chiefly at Rome, and was there at the time he received the emperor's orders for his immediate banishment: Mr. B. therefore considers him as then leaving the 42d degree of northern latitude, the climate in which he was born, and continued to live. He was thence removed to Tomos, which Dr. Wells, in his maps of ancient geography, places only in the 44th degree of northern latitude: the change was therefore only of 2 degrees, and yet Ovid immediately describes it as the winter of Hudson's bay, with the Euxine sea frozen over, with people and cattle walking on it; as well as other instances of extreme cold.

Besides the quotations from Ovid, Mr. B. gives several others from the ancients, as Virgil, Strabo, Pliny, &c. descriptive of the excessive cold of that latitude. He then contrasts these with the accounts of modern travellers in that country, who have not noticed any such severities of climate there.

Mr. B. now leaving Tomos, compares the accounts of the weather in Italy, with those of the present times: it being first premised, that the country was better cultivated in the Augustan age than it is now, which should consequently have made the temperature of the air more warm than it is now experienced to be. He begins with some passages from Virgil's Georgics. This most excellent husbandman is constantly advising precautions against snow and ice in the management of cattle; and he may be generally supposed to give these directions for the neighbourhood of Naples, or Mantua his native country, where he does not evidently from the context mean some other parts of Italy. Speaking afterwards of Calabria, the most southern part of Italy, he expresses himself, with regard to the rivers being frozen, as what was commonly to be expected. Pliny too in a chapter, De natura cœli ad arbores, and speaking of Italian trees, says, 'Alioqui arborum frugumque communia sunt, nives diutinas sedere.' But perhaps the strongest proof of that very remarkable fact, the Italian rivers being constantly frozen over, is to be collected from a chapter in Ælian, which consists entirely of instructions how to catch eels while the water is covered with ice. Now, if we may believe the concurrent accounts of modern travellers, it would be almost as ridiculous to advise a method of catching fish in the rivers of Italy, which depended entirely on their commonly being frozen over, as it would be to give such directions to an inhabitant of Jamaica. Mr. B. cannot find that the precautions, which Virgil gives in his Georgics, against the damage which sheep and goats mgh receive from the snow and frost, are now necessary; and both these animals are known to stand the severest winters of the Highlands of Scotland, conceived to be in Virgil's time almost the ultima Thule. On the whole Mr. B. infers, that there appears to have been a general melioration of temperature in the air and the seasons, in many, perhaps most parts of the earth.

X. An Account of Rings consisting of all the Prismatic Colours, made by Electrical Explosions on the Surface of Pieces of Metal. By Joseph Priestley, LL.D., F.R.S.* p. 68.

It was a discovery of Sir Isaac Newton, that the colours of bodies depend on the thickness of the fine plates which compose their surfaces. He has showed

* Dr. Priestley was born on March 13, 1733, at Field-head, in the parish of Birstall, in the west riding of Yorkshire; and he died at Northumberland in the state of Pennsylvania, in America, the 9th of February 1804; consequently at near 71 years of age. His father was concerned in the cloth manufacture, who intended his son Joseph also for trade, but was induced to change his mind by the youth's early attachment to reading and literary pursuits. After a pretty extensive course of classical studies, at 19 years of age he entered as a divinity student the academy of Daventry, under Dr. Ashworth, as successor of that kept by Dr. Doddridge at Northampton. About 3 years after Mr. P. was chosen as an assistant minister to the Independent congregation of Needham-market, in Suffolk. After 3 years spent at this place, he accepted an invitation to be pastor of a small meeting at Nantwich, in Cheshire, where he also opened a school for the education of young gentlemen; and here it was that he composed his English grammar, which was his first publication. Hence he removed, 1761, to join the academy at Warrington, as lecturer in the particular department of belles-lettres; at which place his literary career may be said to have commenced; as hence a variety of his publications soon commenced: as his Biographical and Historical Charts, his writings on subjects of history and general politics, &c. And here, in 1767, was published his History of Electricity. The year after he accepted the office of pastor to a large congregation of dissenters at Leeds; which was soon after followed by numerous theological publications.

In 1770 Dr. P. quitted Leeds, for the situation of domestic librarian to the Earl of Shelburne, or rather his literary and philosophical companion, in the hours that could be devoted to such pursuits.

In 1772 appeared his 'History and Present State of Discoveries relating to Vision, Light, and Colours,' in 2 vols. 4to; which may be considered as a 2d part of a general history of the philosophical sciences; and which indeed proved the last, as the encouragement of this work fell far short of that of the History of Electricity.

In 1775 came out his 'Examination of Dr. Reid on the Human Mind; Dr. Beattie on the Nature and Immutability of Truth; and Dr. Oswald's Appeal to Common Sense." In 1777, 'Disquisitions relating to Matter and Spirit." And soon after, his correspondence with Dr. Price, relative to the same points. In several volumes of the Philos. Trans., as well as in separate publications of his own, are seen his numerous papers of discoveries relating to aëriform fluids, and other chemical subjects; besides many others on theological subjects.

Dr. P.'s engagement with Lord Shelburne having ceased in 1780, he accepted the office of pastor to a congregation at Birmingham; whence soon after issued some of the most important of his theological works: from which arose several controversies on such topics, with Dr. Horsley and other learned men. Dr. P. remained at Birmingham till 1791, when his house and library were burnt, with many others, in a popular commotion in that place. After some little time an invitathat a change of the thickness occasions a change in the colour; differently coloured rays being thus disposed to be transmitted through the plate, and consequently rays of different colours being disposed to be reflected at the same place, so as to present the appearance of different colours to the eye. A variation in the density of the plate, he shows, will occasion a variation in the colour; but still a medium of any density would exhibit all the colours according to the thickness of the different parts of it. These observations he confirmed by experiments on plates of air, water, and glass. He also mentions the colours which arise on polished steel, by heating it; likewise on bell metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air: and he ascribes these colours to the scoriæ, or vitrified parts of the metal, which he says most metals when heated or melted, do continually protrude, and send out to their surface, covering them in the form of a thin glassy skin. Optics, p. 194.

This capital discovery, concerning the colours of bodies depending on the thickness of the fine plates which compose their surfaces, of whatever density those plates be (and which may be of such admirable use to explain the colours, and perhaps in due time the constituent parts and internal structure of natural bodies) Dr. P. hit upon a method of illustrating and confirming, by means of electrical explosions. These being received on the surfaces of all the metals, change their colour, to a considerable distance round the spot on which they are discharged, so that the whole space is divided into a number of concentric circular spaces, each exhibiting all the prismatic colours; and perhaps as vivid as they can be made in any method whatever.

It was not by any reasoning a priori, but by a mere accident, that he first discovered these colours. Having occasion to take a great number of explosions, in order to ascertain their lateral force; he observed that a plate of brass, on which they were received, was not only melted, and marked with a circle, by a fusion round the central spot, but likewise tinged, beyond this circular spot, with a green colour, which he could not easily wipe out with his finger. Struck with this new appearance, he replaced the apparatus, and continued the explosions; till, by degrees, he perceived a circle of red beyond the fainter colours; and, examining the whole with a microscope, he plainly distinguished all the prismatic colours, in the order of the rainbow. The diameter of the red, in this instance,

tion to succeed Dr. Price in a congregation at Hackney gave him a temporary residence; till, in 1794, he sailed for North America, where he settled at the town of Northumberland, in the state of Pennsylvania, for the remainder of his life; of which several particular and ample accounts have been printed; viz. in his Life, by J. Corry, 1804; the same, by his son, and Cooper and Christie; also in the Monthly Magazine, vol. 17; the Monthly Review, vol. 46; the Philos. Magazine, vol. 22; and the Edinburgh Review, vol. 9, N° 17, &c.

happened to be $\frac{1}{8}$ of an inch, and the diameter of the purple about $\frac{1}{4}$. Pleased with this experiment, Dr. P. afterwards pursued and diversified it in a great variety of ways; the result of which he comprises in the following observations.

1. When a pointed piece of metal is fixed opposite to a plain surface, the nearer it is placed to the surface, the sooner do the colours appear, the closer do the rings succeed one another, and the less space they occupy; as, on the other hand, the farther it is placed from the surface, the later do the colours appear; but the rings then occupy a proportionably greater space, and have more room to expand themselves.

2. The more acutely pointed is the wire, or needle, from which the electric matter issues, or at which it enters, the greater number of rings appear. A blunt point makes the rings larger, but fewer; and in that circumstance it is likewise much later before the colours make their appearance at a given distance.

3. In making these rings, the first appearance is a dusky red, about the edges of the circular spot; presently after which (generally after 4 or 5 strokes) there appears a circular space, visible only in a position oblique to the light, and looking like a shade on the metal. This space expands very little during the whole course of the explosions, and it seems to be, as it were, an attempt at the first and faintest red; for by degrees, as the other colours fill the bulk of that space, the edges of this shade deepen into a kind of brown.

4. After a few more explosions, a second circular space is marked out by another shade, beyond the first, generally about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch in diameter, which was never observed to change its appearance, after ever so many explosions. This second shade, by succeeding the first; which as before observed, becomes gradually of a brown, or a light red, seems to be an attempt at the fainter colours, which intervene between the reds.

5. All the stronger colours make their first appearance at the edges of the circular spot; and more explosions make them continually expand towards the extremity of the space first marked out, while others succeed in their place; till, after about 30 or 40 explosions, 3 distinct rings generally appear. If the explosions be continued further, the circle becomes less beautiful, and less distinct; the red commonly prevailing, and suffusing all the other colours.

6. The last formed colours are always the most vivid. Also the last formed rings lie closer to one another than the first.

7. These rings may be brushed with a feather, and even wetted, or a finger may be drawn over them, without their receiving any injury; but they easily peel off, when scratched with one's nail, or any thing that is sharp, the innermost rings being the most difficult to erase.

8. The first circles are sometimes covered with a quantity of black dust; part

of which however may be wiped off with a feather, so as to show the colours under it. An attempt to wipe off more, on the rough side of the steel, took off the colours along with it; but more than half yet remains, with the dust upon it, as it was first formed.

9. It makes no difference whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body; the plate opposed to the point being marked exactly alike in both cases. Also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable distance, often about half an inch, but not very distinctly. The colours likewise return here, in concentric rings, as upon the plate.

10. The more circles that are made at the same time, the more delicate will the colours be; whereas the surface is, as it were torn, or corroded by more violent explosions; which makes the colours appear rough and course. But this roughness is only perceived on the steel. On silver, tin, and polished brass, the colours were always free from that roughness.

11. A polished surface is not necessary, the colours being very manifest on the rough side of the steel, where it is not covered with the black dust mentioned above.

12. These coloured rings appear almost equally well on all the metals on which he made them; viz. gold, silver, copper, brass, iron, lead, and tin.

13. When the pointed wire was made to incline to the plane on which the colours were exhibited, the circular spot was quite round, the centre of it being in the perpendicular let fall from the point; but the colours were projected opposite to the point, in an oblong figure.

On showing these coloured rings to Mr. Canton, Dr. P. was agreeably surprised to find, that he had likewise produced all the prismatic colours from all the metals, but by a different electrical process. His method had been to extend fine wires over the surface of pieces of glass; and when the wire was exploded, he observed that the glass remained tinged with all the colours from all the metals. They are not indeed disposed in so regular and beautiful a manner as in the rings above produced; but they equally demonstrate, that none of the metals discovers the least preference to any one colour more than another. A variety of other very extraordinary appearances occurred in the course of Mr. Canton's experiments in melting wires.

In what manner these colours are formed, it may not be easy to conjecture. In Mr. Canton's method of producing them, the metal seems to be dispersed in all directions from the place of explosion, in the form of spheres, of a very great variety of sizes, tinged with all the variety of colours, some of them too small to be distinctly visible by any magnifier. In Dr. P.'s method, it should rather seem that they are produced in a manner similar to the production of colours on steel

&c. by heat, i. e. the surface is affected, without the parts of it being removed from their places, certain plates only, or laminæ, being formed, of a thickness proper to exhibit the respective colours at certain distances; and that the thickness of these plates is continually changing by the repetition of the explosions.

N. B. The battery made use of in the above-mentioned experiments, was of 21 square feet of coated glass.

XI. A Letter from John Ellis, Esq. F. R. S., on the Success of his Experiments for preserving Acorns for a whole Year without planting them, so as to be in a State fit for Vegetation, with a View to bring over some of the most valuable Seeds from the East Indies, to plant for the Benefit of our American Colonies. p. 75.

Part of a parcel of acorns, Mr. E. sowed Feb. 20, 1767, under the windows of his chambers, in the kitchen garden of Gray's Inn; and on the 22d of the same month he inclosed about 36 of them in bees-wax. Most of those he had sowed in the garden came up in June following 1767, and by the middle of September were 6 inches high. Before mentioning the method in which he treated these acorns, he observes, that though he had formerly been so successful as to preserve both acorns and chestnuts for the space of a year in bees-wax, several of which have afterwards vegetated, and some of them were grown into trees; yet he always found that many of them were rotten when they were taken out of the wax; which made him suspect that it was owing to the too great heat of the melted wax, that so many of them were destroyed. This put him on thinking of the following method to guard the seeds to be preserved from too great heat.

After having chosen out the fairest acorns, laying aside such as had specks proceeding from the wounds of insects, he wiped them very clean till they were quite bright, for fear of any condensed perspiration on the surface, which if inclosed, would turn to mouldiness. He then poured some melted bees-wax into a china plate about half an inch deep, and as soon as the wax was cool, but still very pliable, he cut out with a penknife as much as would inclose one acorn; this he wrapped round it, rolling it between his hands till the edges of the wax were perfectly united: in the same manner he covered about 36 of them with all the caution in his power, so that after they had been set to harden he could not perceive the least crack in them. When they were quite cold and hard, he prepared an oval chip box, of 7 inches long, 4½ broad, and 3½ deep; into this he poured melted bees-wax to the depth of an inch and half; and when he could bear his finger in it, he laid the covered acorns at the bottom in rows as close as he could together; afterwards other rows over them, till the box was full; and when the first wax began to cool, he poured some wax that was barely fluid over the uppermost acorns till they were quite covered. In order to cool them as soon as possible, he set the box near a window, where the sash was raised a little to let in a stream of cold air; when they were almost cold, he perceived the wax had shrunk a little here and there, and left some chinks; these he immediately filled up with very soft wax, pressing it very close and smooth. After it was quite cold and hard, he put on the cover of the box, and placed it on a shelf in a closet till the beginning of August 1767, when he sent it to the care of Mr. Dacosta, clerk to the R. S. to their house in Crane Court, to be produced and examined before the R. S. at some of their first meetings after the long vacation. When they were cut open and examined, their appearance promised success; and they were ordered to be delivered by Dr. Morton, secretary to the R. S. to the care of Mr. William Aiton, botanic gardener to her royal highness the Princess Dowager of Wales, at Kew, that the R. S. might be informed whether they would vegetate.

In March 1768, Mr. E. received a letter from Mr. Aiton, advising him, that he had sent to Mr. Robertson, housekeeper to the R. S., two pots with the young oaks rising from the acorns preserved in wax, which Dr. Morton sent him from the R. S. in December last. And Mr. E. is well persuaded he has carefully attended to an experiment, the success of which, if properly followed, may in a few years put us in possession of the most rare and valuable seeds in a vegetating state from the remotest parts of the world, which in time may answer the great end of the improvement and advancement of our trade with our American colonies.

XII. A Letter from Dr. Donald Monro, F. R. S. inclosing one from Mr. Farley, of Antigua, on the Good Effects of the Quassia Root in some Fevers. p. 80.

Dr. D. M. here states that as the public had not had any further accounts of the quassia root, since Dr. Linnæus published the 6th volume of his Amœnitates Academicæ* in 1764, he had according to Dr. Maty's desire, sent the copy of a letter on the good effects of this root, which he hoped would be acceptable to the R. S. as it might excite physicians to make trials of this medicine which seemed to promise to be of so much use. The original letter was given him by the gentleman to whom it was addressed, while he attended him in 1767, when he was in England for the benefit of his health.

* Dr. Linnæus gives a particular description and figure of the quassia-tree, which grows in the neighbourhood of Surinam, in South America, and of the root having been administered at Surinam, with great success, in malignant, remitting and intermitting fevers: and he tells us that its virtues were first discovered by a slave of the name of Quassi, from whom the tree got its name.—Orig.

Since this account was written, quassia has become a standing article of the materia medica. The root of the quassia amara possesses the greatest degree of bitterness and medicinal efficacy; but the quassia of commerce is said to be from another species, viz. from the quassia excelsa.

Copy of a Letter from Mr. James Farley, Practitioner in Physic in the Island of Antigua, to his Partner, Mr. Arch. Gloster, in London; dated Antigua, July 26, 1767.

"Mr. T——r has been extremely ill since his arrival with a fever, which lasted for many hours; and, on its going off, he could not retain the bark in any shape whatever. Many things were tried to check the vomiting, and enable him to keep down some bark, but to no purpose. At last I tried the quassia root, an account of which I read in one of the magazines for this year; it sat extremely well on his stomach; he had no vomiting after the first dose, and recovered very speedily.

I have lately tried it in 3 or 4 cases, where there has been a tendency to putrefaction, and the bark would not stay on the stomach; a dram of this root has effectually answered every purpose that the bark would. It has this advantage over the bark, that it does not heat the patient.

I have given it in fevers, joined with the radix serpentariæ virginianæ, with success. I had a pound or 2 from Esquebo, and have sent you a little of it. Dr. Warner has sent Dr. Jackson a piece of it; he saw the good effects of this medicine in a patient, Capt. B—n, who sails for London to day. He attended him with me. I could not get the bark to sit on his stomach, for he had a perpetual vomiting, and could not keep down any nourishment whatever. I prepared a decoction of $1\frac{1}{2}$ dr. of the quassia root, and 1 dr. of the rad. serpent. virgin. When it was ready, I sent for Dr. Warner, that he might see the patient before I administered it; he complained of some pain on touching the pit of his stomach, had a very sluggish low pulse, a great pain over his eyes and in his eye-balls, and vomitings. He took the decoction, which surprizingly put a stop to his vomiting; he had no return after the first dose, and kept down every thing. We indeed gave him some camphor and sal succini, on account of the sluggishness of the pulse, but I have tried it alone in a decoction, with infinite advantage."

XI. Meteorological Observations for 1767, made at Carlisle, Bridgwater, and Ludgvan; and communicated by the Bishop of Carlisle, F. R. S. p. 83.

Some monthly accounts, now uninteresting, of the state of the barometer, thermometer, hygrometer, and rain, for the 3 places above mentioned; by which it appears that the total of the year's rain, was at Carlisle 26.268 inches; at Bridgewater 24.85 inches; and at Ludgvan $37\frac{1}{10}$ inches.

XIV. On the Different Species of the Birds called Pinguins. By Thomas Pennant, Esq. F.R.S.* p. 91.

The characters of this genus are, very small wings, and those covered with mere shafts. Four toes on each foot, 3 of which are webbed, the 4th loose, and standing forward.

1. The Patagonian Pinguin.

Size. The length of the stuffed skin was 4 feet 3 inches; and the bulk of the body seemed superior to that of a swan. *Bill.*—Four inches and a half long; slender, straight, bending only on the end of the upper mandible, black, covered on each side the base with soft short brown feathers; the sides of the lower mandible compressed, the lower part or base orange-coloured, the end dusky. No nostrils. *Tongue.*—Half the length of the bill, and singularly armed with strong sharp spikes pointed backwards. *Plumage.*—The most remarkable of all the feathered tribe, each feather lying over the other, with the compactness of the scales of fish; their texture is equally extraordinary; the shafts broad and very thin; the veins unwebbed; the head, throat, and hind part of the neck, are of a deep brown colour; from each side of the head to the middle of the fore part of the neck are two lines of bright yellow, broad above, narrow beneath, and uniting half way down; thence the same colour widens towards the breast, fading away till it is lost in pure white, of which colour is the whole under side of the body, a dusky line dividing it from the colour of the upper part; the whole back is of a very deep cinereous colour, almost dusky; but the end of each feather is marked with a cœrulean spot, those about the junction of the wings larger and paler than the others. *Wings.*—Are extremely short in respect to the bulk of the bird, hang down, and have rather the appearance of fins, whose office they perform; their length is only 14 inches; on the outside they are dusky, and covered with scale-like feathers, or at best with such whose shafts are so broad and flat as scarcely to be distinguished from scales; those on the ridge of the wings consisting entirely of shaft; the larger or quill feathers have some very short webs. *Tail.*—Consists of 30 brown feathers, or rather thin shafts, resembling split whale-bone, flat on their upper side, concave on the under, and the webs short, unconnected, bristly. *Legs and Feet.*—From the knees to the end of the claws 6 inches, covered with strong pentangular black scales; the fore-toe scarcely an inch long, and the others so remarkably short, as to evince the necessity of that strength of the tail, which seems intended as a support to the bird in its erect attitude; in the same manner as that of the wood-pecker is when it clings to the sides of trees; between the toes is a strong semilunar membrane,

* The species of Pinguin here described, is the Aptenodyta Patachonica of the Gmelian edition of the Systema Naturæ. The figure here given (as Mr. Pennant himself has elsewhere observed) does not properly express the general attitude of the bird, which is usually seen with the neck at rest, or drawn down close to the shoulders, as expressed in the small plate of the same species in Mr. Pennant's Genera of Birds, and in the magnificent figure in Miller's plates of Natural History, republished under the title of Cimelia Physica.

continued even up part of the claws; the middle claw is near an inch long, and the inner edge very sharp and thin; the interior toe is small, and placed very high. *Skin.*—Extremely tough and thick, which, with the closeness of the feathers, guards it effectually in the element it is so conversant in. *History.*—This bird was brought by Capt. Mac-Bride, from the Falkland Isles, off the Straits of Magellan; we believe this species to have been undescribed; for the birds that bear the same name are mentioned by every writer, who treat of them as far inferior in size to this; some compare their bulk to that of a duck; but none make it larger than a goose; the colours also of this species are too striking not to have been taken notice of, had it been before discovered.

Captain Mac-Bride was so obliging as to inform us that this was a very scarce species; though he saw in the same place multitudes of the lesser kind, with which it agreed in its manner of life. Since the natural history of each species is the same, we shall give a general view of the economy, &c. from such writers as have treated of them.

It is agreed that they are inhabitants of southern latitudes only, being, as far as is yet known, found only on the coasts of South America, from Port Desiré to the Straits of Magellan; and if we remember right, Frezier says, they are found on the western shore, as high as Conception. In Africa they seem to be unknown, except on a small isle near the Cape of Good Hope, which takes its name from them.

They are found in prodigious numbers on land during the breeding-season; for they seldom come ashore but at that time; they form burrows under ground, like rabbits; and the isles they frequent are perfectly undermined by them, so that it is difficult to walk without falling into their holes, or sinking through the surface up to the shoulders. Such rencontres are disagreeable, as these birds bite extremely hard; and commonly 3 or 4 are found to nestle together in the same hole. Their eggs are said to be rather less than those of a goose; and that they begin to lay the latter end of September, or beginning of October. Their attitude on land is quite erect; and on that account they have been compared by some to pygmies, by others to children with white bibs.

On land they are excessively aukward, by reason of the situation of their legs, which are placed quite behind: they are very tame, and may be driven like a flock of sheep; when pressed, they seek for shelter either in their burrows, or the sea, which seems to be their more natural element. In the water they are remarkably active, and swim with vast strength, assisting by their wings, which serve instead of fins. Their food in general is fish, not but that they will eat grass like geese; for Sir Richard Hawkins observed, in an isle they frequent off Patagonia, a small valley covered with grass, which the birds never burrowed in, as if they meant to reserve it for pasturage.

They are very fat, but taste very fishy, not unlike our puffins: as they are very full of blood, it is necessary to cut off their heads as soon as they are killed, in order that it may run out; it is also requisite they should be flayed, for without these precautions their flesh is scarcely eatable. When salted it becomes a good food, as navigators have often experienced, in particular Richard Hopkins, who preserved that way 16 hogsheads, which lasted above 2 months, and served as beef. These birds and seals seem to have been bestowed in quantity on those desolate shores, as resources in extremity to distressed voyagers.

Name.—The proper name of these birds is Pinguin, on account of their fatness. It has been corrupted to penguin; so that some, imagining it to have been a Welsh word signifying a white head, entertained some hopes of tracing the British colony, said to have migrated into America, under the auspices of Madoc Gwineth, son of Owen Gwineth, A.D. 1170. But as the two species of birds that frequent that coast have black heads, we must resign every hope founded on that hypothesis of retrieving the Cambrian race in the new world.

We give this species the epithet Patagonian, not only because it is found on that coast, but because it as much exceeds in bulk the common kinds, as the natives are said to do the common race of men.

I must not quit this subject without making my acknowledgments to Mr. Banks for communicating this curious bird to me, which he now permits to be laid before the Society for their examination.

XV. The Application of Dr. Saunderson's Theorem for solving Unlimited Equations, to a curious Question in Chronology. By Mr. J. Horsefall, F.R.S. p. 100.

By old tables it appears that Easter day happened on the 22d of March, which is the soonest it ever can happen, in the years of Christ 165, 697, 1229, and lastly in 1761. *Quest.* 1. What is the next year of our Lord, when it will happen so again before 1900? For, from thence to 2199, the paschal full moon, or the golden number 14, which distinguished the years above, will be fixed on the 22d of March; consequently Easter day cannot happen before the 23d of March in that period.

Answer. In the act for altering the style, it appears by the table for finding Easter till 1899, that this can never happen in that period, but when the golden number, or lunar cycle, is 14, and the Sunday letter is D.

Also, by making a solar cycle for that century, the first year of it will fall on 1812, the Sunday letters E D, therefore all the years in that cycle, which have D for the Sunday letter, are 1, 7, 18, 24: and now the question is reduced to this:

Quest. What year of our Lord, in the 19th century, will have the solar cycle either 1, 7, 18, or 24, when the lunar cycle is 14? Or, which is the same thing, what number is there, between 1800 and 1900, which divided by 28 leaves 1, 7, 18, or 24; and being divided by 19 leaves 14?

SOLUTION. Here then in the general theorem $\frac{ar}{l} \times (d - e) + d$, is $a = 28$, $b = 19$, D = 1, 7, 18, or 24; E = 14, $l = 1$. To find * r, the quotients are * 1, 2, 9; drop the first and last, because their number is odd; then the series required will be 0, 1, 2; therefore $r = 2$.

19) 28 (1
9) 19 (2
1) 9 (9

Note, if to any year of Christ be added 9, and the sum divided by 28; the remainder, or 28, if 0 remain, will be the cycle of the sun for that year; and if 1 be added to any year of Christ, and the sum divided by 19; the remainder, or 19, if 0 remains, is the cycle of the moon. Hence, if any year of Christ be severally divided by 28 and 19, and the remainders be d and e respectively; then $d + 9$, or $d + 9 - 28 =$ D; and $e + 1$, or $e + 1 - 19 =$ E. In the present case, taking D = 1, $d + 9 = 1$ cannot be; because d would be negative: but d must not only be affirmative, but also greater than e; therefore make $d + 9 - 28 =$ D $= 1$; therefore $d = 1 + 28 - 9 = 25$; and $e + 1 =$ E $= 14$, therefore $e = 13$. Here D and E are the cyclar numbers, and d and e are the anno domini numbers suited to the theorem.

Here then $(ar \times d - e) + d = (28 \times 2 \times 7) + 20 = 412$; and $412 + 532 + 532 + 532 = 2008$. The first answer therefore, A. D. 412, is too little, and when increased by 3 dionysian periods, or multiples of 28 and 19, is too great, going beyond the century required. So, when this solar cycle is 1, it will not do.

Let D = 7, the rest as before. Then $d + 9 - 28 = 7$; therefore $d = 26$. Here then $(ra \times d - e) + d = (28 \times 2 \times 13) + 26 = 754$; and $754 + 532 + 532 = 1818$. So A. D. 1818 will answer the question.

Let D = 18, the rest as before. Then $d + 9 - 28 = 11$; therefore $d = 37$. Here $(ra \times d - e) + d = (28 \times 2 \times 24) + 37 = 1381$; and $1381 + 532 = 1913$. This goes beyond the century required; so will not do.

Let D = 24, the rest as before. Then $d + 9 = 24$; therefore $d = 15$. Here $(ra \times d - e) + d = (28 \times 2 \times 2) + 15 = 127$; $127 + 532 \times 4 = 2255$; which goes beyond the century required.

So there is but one year in the 19th century, viz. 1818, that will have the conditions required. The cycle of the sun will then be 7; the cycle of the moon 14; and the Sunday letter D; and Easter-day the 22d of March.

XVI. A Determination of the Solar Parallax attempted by a peculiar Method, from the Observations of the last Transit of Venus. By Andrew Planman, of the Acad. of Sciences at Stockholm, &c. From the Latin. p. 107.

§ 1. From the centre of the earth T (fig. 7, pl. 15), conceive lines to be drawn to every point of the sun's disk PQA, which, without sensible error, may be supposed fixed during the whole transit; and that these right lines, on a plane perpendicular to the line TS, joining the centres of the sun and earth, and passing through OB the apparent path of the planet seen from the earth's centre, form the projection IKE, whose centre is in C, cutting the path in the points I and E. If now from C the line CD be drawn perpendicular to the ecliptic, a spectator at T would see the planet in conjunction with the sun in respect to the ecliptic, its centre being at D; also half of the planet's disk will be seen immersed or emerged when its centre is come to I or E. And if from any other point H, of the earth's disk, at the bounds of the light, the sun be viewed, and hence in like manner lines be drawn to every point of the sun's disk, the situation of the sun's projection will be changed according to the magnitude and position of TH, or of its parallel and proportional CL; so that the centre of the sun is not now seen in C, but in L; by which means also the said moments will be changed as shown by the small letters, I being at *i*, D at *d*, E at *e*, by the effects of parallax, which are thus to be determined when the observations of those moments made at H are reduced to the earth's centre. Conceive further that by lines drawn from the sun's centre S to every point of the earth's disk HRZ, there is made, on the same plane on which the sun is projected, the projection of that disk NOL, which also is a circle whose radius CL is = the horizontal parallax of the planet from the sun; TH remaining = the horizontal parallax of the planet: for draw LM parallel to ST, then, because of the very small and equal angles TSH, MLH, will MH be = the sun's horizontal parallax; and therefore TM = TH − MH = CL. If the point H should be not at the boundary of the light, but elsewhere in the earth's disk turned towards the sun, then will CL be either exactly or very nearly equal to the parallax of the planet from the sun, according as the altitude of the planet and of the sun's centre is either nothing or very small. The effect therefore of parallax depends on the divers situation of the centre of the sun's disk in the circle LON, or, which is the same thing, on the divers place of the observer in the hemisphere HRZ; for the projection of that place and of the centre of the sun's disk, seen from the same place, coincides in one and the same point of the circle LON. Therefore the question, of estimating the effects of parallax, is reduced to this, that, for any given time, to determine, in respect of CD, the situation of any given place of the earth projected on the circle LON, which place is reduced to L. For this end, there must be found CL, with the angle LCD, intercepted by a projected vertical circle and circle of latitude. And since that angle depends on the paralactic angle, comprehended by the meridian and vertical circle, this angle must be first found.

§2. Therefore let the circle ANB, fig. 8, represent the earth's hemisphere illuminated and projected, as above, on the plane passing through the path of the planet, whose radius CB is equal to the horizontal parallax of the planet from the sun; and let AB be the projection of the celestial meridian, in which is either the north pole P, or the south P, according as the conjunction of the planet may be at ☋ or at ☊. Also let C be the common centre of projection of the earth and sun viewed from the earth's centre; and L the projection of any given place. Put the latitude of the place L = L; the complement of the sun's declination CP or cp = D; the horary angle CPL or cpL = A; radius = 1; the sun's altitude at the given place and time = C; and the cos. A. cot. L = tang. G; then will sin. $C = \frac{\text{sin. L. cos. L} \pm d \pm G}{\text{cos. G}}$ (I), where the lower signs are to be taken when D < G, otherwise the upper signs, except when A > 90°, when the sign − of G becomes +, and consequently the sum of D and G is to be taken. Lastly put the parallactic angle PCL or pCL = Q, then will sin. $Q = \frac{\text{sin A. cos L}}{\text{cos C}}$ (II).

§ III. As to CL, it will be at least nearly equal to the parallax of the planet's altitude from the sun (§ I), unless the difference of the altitudes of the centres of the sun and planet be neglected; in which case the ratio of this difference is to be had, as the parallax is a little different from CL. Yet, even in this case, the parallax of the planet's altitude from the sun, may be estimated in CL, without sensible error. For this end, the difference of the altitudes of the sun's centre and the star is now to be found. Therefore let AB, fig. 9, be the celestial meridian; PM parallel to the equator; ZN, zn vertical circles corresponding to the place and time; Ll, any given places of the planet before and after the ascensional conjunction; SR, sr, the differences in right ascension of the sun's centre S and the planet, which call a; and LR, lr differences of declination, which call d; also let the angle made by a parallel of the equator, and the line joining the sun's centre and planet, viz. the angle LSR or lsr, be called F; then will the tangent $F = \frac{d}{a}$, and SL or $sl = \frac{d}{\text{sin. F}}$ the distance of the centres of the sun and star. If now, from L, l, the perp. LD, ld be drawn to the vertical circles ZN, zn, then SD or sd will be the difference of altitude sought, which call E; to find which, we have the formula $E = \frac{d.\ \text{sin. } (T \pm Q)}{\text{sin. F}}$ (III), in which T will be given by d and a, which are easily derived from the observations, and Q from the equation (II). With regard to the signs, observe the following rules: viz. the sign + takes place when the observation is made before noon and before the conjunction, or after noon and after the conjunction in right ascension: but the sign −, on the contrary, is used in observations before noon and after conjunction, or after noon and before the same conjunction. These signs are to be changed, using the sign − in the former, and + in the latter case of the rule, when the planet's path is to the north of the sun's centre, as in the next transit of Venus. There will then be now given, for the given place and time, by the equations (I) and

(III), the planet's altitude, being $= C \mp E$; where the sign $-$ is used in the former, and $+$ in the latter case of the rule; excepting in the former case those places of the earth's disk more south than the sun's centre, and in the latter the places more south than the same centre, provided $F +$ the complement of $Q > 90°$; and this exception will also hold when the planet's path, at either node, is more south than the sun's centre. But if the planet transit the sun with north latitude, the exception in respect of the places is to be inverted, so that the sign $+$ takes place in the former case of the rules, when the place of the earth's disk, more north than the sun's centre, gives $F +$ compl. $Q > 90°$; but that being the same, in the latter cases of the rule, in regard to terrestrial points more south than the sun's centre, the sign $-$ takes place. Now put the horizontal parallax of the planet from the sun $= H$, which in investigating the parallax is assumed at pleasure, but yet so as to be not far from the truth: put also the parallax in altitude of the planet from the sun $CL = P$; then will $P = H. \cos (C \mp E)$; or, if E be neglected, $P = H. \cos C$.

§ 4. These being premised, we are now to give formula for computing the effects of parallax about the moments or times of the contacts. Let then K ☋, fig. 10, be the ecliptic at the descending node, or K ☊, fig. 11, at the ascending node; OE the apparent path of the planet as seen from the earth's centre; C the common centre of projection of the earth and sun [§ I and II]; MR the celestial meridian; CD the planet's latitude at the moment of the ecliptic conjunction; L any given place in the earth's disk facing the sun; then it will appear, from what is above, that the angle $PCL = Q$, and $CL = P$. Now join DL; and put $CD = n$; also the angle made by the meridian with the ecliptic, viz. the angle RC☋ or RC☊ $= b$, and the angle $LCD = r$; and there will arise a double form for the value of r, according as the planet's path, at the former or latter node, shall be more south or north than the sun's centre; viz. if the path be more south at ☋, or at ☊ more north than the sun's centre, it will be $r = 90° + b - Q$, where, instead of b is to be taken its supplement to 180° when the observation is made before noon: but the planet's path being at ☋ more north, or at ☊ more south than the sun's centre, it will be $r = Q \mp b \pm 90°$, where the upper signs take place in observations made afternoon, but the lower signs before noon. Further, make $90° - \frac{1}{2}r = t$; and $\frac{n - P}{n + P}$. tang. $t =$ tang. x; also put the angle $CDL = y$; then will $y = t \pm x$, where the sign $-$ takes place when $n > P$; hence $DL = \frac{P. \sin r}{\sin y}$. Since there is given the angle of the planet's path with the circle of latitude, which call e, so that in fig. 10, the angle $EDI = e$, and in fig. 11 the angle $CDE = e$; hence there will also be given the angle LDF or LDE. If now with centre L, and radius equal to the sum or difference of the semidiameters of the sun and planets, which call m, the path be cut in i and e; then, because of the retrograde motion of the planet, will the eastern point i be the place of the planet's centre, when the spectator in L sees the beginning or end of the immerging; but the western point e for the place of the planet's centre about the contact of the emersion. And for the calculations of these contacts, there will be determined the side Di or DE in the triangle, now given, DLi or DLe. Put then $e \mp y = u$; where the sign $-$ takes place in observations after noon, except when $Q > 90° + b$; but the sign $+$ in the forenoon observations, unless $Q < 90° - b$; that is, the planet's path being at ☋ more south, or at ☊ more north than the sun's centre. But when the planet's path at those nodes have the opposite situation, the order of the signs is reversed; viz. $+$ in the afternoon, and $-$ in the forenoon observations; unless when $Q > 90° + b$ and $Q < 90° - b$. Put now $\frac{P \sin. r \sin. u}{m \sin. y} = \sin. z$; then will $Di = \frac{m}{\sin. u}. \sin. (u \pm z)$, (A); and $DE = \frac{m}{\sin. u}$. $\sin. (u' \mp z')$; (B); where (A) serves for the computations of the contacts of the immersion, and (B) for those of the emersion.

Corol. 1. If with the centre C and radius $= m$ the path be cut in I and E; and if there be put $P = 0$; the points L, i, e coinciding with C, I, E respectively; we shall have $DI = \frac{m}{\sin. e}. \sin. (e \pm c)$, (C); and $DE = \frac{m}{\sin. e}. \sin. (e \mp c)$, (D); $\sin. c$ being $= \frac{n. \sin. e}{m}$. To the tenor of these formulas the contacts, as seen from the centre of the earth, are computed. And the upper signs of the equations (A), (B), (C), (D), are to be observed, if the planet transit the sun, at ☋ with south, or at ☊ with north latitude; but in the opposite nodes, the lower signs take place.

Corol. 2. Therefore the effect of parallax about the immersion becomes $\frac{m}{\sin. u}. \sin. (e \pm z) - \frac{m}{\sin. e}. \sin. (e \pm c)$, which converted into time, is to be subtracted if negative; but if positive, it will be added to the moment of observation, which will give the moment of time reduced to the earth's centre. But at the emersion the effect of parallax will be $= \frac{m}{\sin. u'}. \sin. (u' \mp z') - \frac{m. \sin. (e \mp c)}{\sin. e}$, which changed into time, if negative, it is to be added to the moment of observation; but if it come out positive, it must be subtracted from the same, to give the moment of observed time reduced to the earth's centre.

Corol. 3. If $n = 0$, that is, if the planet's path pass through the sun's centre; then the point D coinciding with C, fig. 12, it will be

$Ci = \frac{m}{\sin. v}. \sin. (v \pm s)$ for the immersion, and $Ce = \frac{m}{\sin. v'}. \sin. (v' \mp s')$ for the emersion. But in the equations (C) and (D), cor. 1, it becomes now c; therefore for the earth's centre there remains $CI = CE = m$. And hence, for this case, the effect of parallax will be, at the emersion

$\frac{m}{\sin. v}$ $\sin. (v \pm s) - m$, and at the emersion $\frac{m}{\sin. v'}. \sin. (v' \mp s') - m$. As to the signs, the upper take place both with ☋ and ☊ when, in the forenoon observation, a is not $> 270° - b - e$, or in the afternoon $a > 270° + b$; for in these cases the lower signs obtain. Note also that $\sin. s = \frac{P. \sin. v}{m}$; and $v = e \pm r$, where the sign + takes place in the forenoon observations, and − in the afternoon, unless a be $< 90° - b$, and $a > 90° + b$. Also, for v is to be taken its excess above 180°, whenever $a > 270° - b - e$, or $a > 90° + b$.

§ v. Now the observations by which, on the foregoing principles, the sun's parallax is to be investigated, will be found in the following table; where in the column titled contact 1 is contained the total immersion, in contact 2 the beginning of the emersion, and in contact 3 the total emersion. The longitudes of places, in column 3, are counted from the meridian of Paris. The 1st column contains the names of the places themselves, and the 2d column the names of the observers.

Places.	Observers.	Longitudes.	Contact 1.	Contact 2.	Contact 3.
Cape of Good Hope	Mason	1h 4m 25s	h m s	9h 39m 52s	9h 57m 23s
	Dixon	....	....	9 39 48	9 57 21
Bologna	Frisius	0 35 53	....	9 4 56	9 22 59
	Marinus	....	....	9 4 58	9 23 0
	Matheucius ..	....	....	9 4 58	9 23 7
	Com. Cassalio	....	....	9 5 0	....
Paris	Le Monnier..	0 0 0	....	8 28 19	8 46 47
	De la Lande..	....	....	8 28 26	8 46 50
	Clouet	....	....	8 28 27	8 46 55
	Bardouin	....	....	8 28 27	8 46 46
	Fouchy......	....	....	8 28 29	8 46 40
	Ferner	....	....	8 28 29	8 46 40
	Messier	....	....	8 28 29	8 46 37
	De la Caille .	....	....	8 28 37	8 46 49
	Merville	....	....	8 28 40	8 47 4
	Condamine ..	....	....	8 28 42	8 46 49
	Maraldi	....	....	8 28 42	8 46 54
Gottingen	Mayer	0 30 11	....	8 58 26	9 16 54
Greenwich	Bliss	0 9 10 oc.	....	8 19 0	8 37 9
	Short	....	....	8 18 50	8 37 28
	Dollond	....	....	8 18 58	8 37 14
	Canton	....	....	8 18 58	8 37 21
Calmar..........	Wykstrom ..	0 56 13 or.	3 33 1	9 23 40	....
Stockholm	Wargentin ..	1 2 50	3 39 23	9 30 8	9 48 9
	Klingenstierna	....	3 39 29	9 30 11	9 48 8
Upsal	Bergman	1 1 10	3 37 43	9 28 9	9 46 30
	Mallet	....	3 37 56	9 28 2	9 46 29
	Melander....	....	3 38 2	....	9 46 29
	Stromer	....	3 38 5	9 28 0	....
Cajaneburg	Planman	1 41 30	4 18 5	10 7 59	10 26 22
Torneo	Hellant	1 27 39	4 3 59	9 54 8	10 12 22
Tobolski	Chappe	4 23 45	7 0 30	12 49 23	13 7 42
Selengisk........	Rumousky ..	6 57 5	....	15 21 36	15 39 42
Pekin	Dollier......	7 35 50	10 10 27	15 59 59	16 17 57

§ vi. The next table shows the different horizontal parallaxes of the sun, as computed from the foregoing observations, which have been deduced both by comparing each of the two contacts of the Cape of Good Hope, and each of the three contacts of Pekin, with all the other corresponding contacts. Also the values of several of the letters or quantities, employed in the calculations, either as given by the observations, or by astronomical tables, as follow: viz. D = 67° 18′ 26″; H = 20″.6, supposing the sun's horizontal parallax 8″.2, which is used; n = 579″.6; b = 83° 51′; e = 81° 30′; the sun's diameter = 31′ 35¼″; that of Venus = 57¼″; and the horary motion of Venus in her apparent path = 4′ 1″.

Observers.	Sun's Horizontal Parallax, computed from the Observations.				
	At the Cape of Good Hope.		At Pekin.		
	Contact 2.	Contact 3.	Contact 1.	Contact 2.	Contact 3.
Frisius	8″.18	8″.20	.″..	8″.86	8″.57
Marinus	8 .13	8 .18	....	8 .92	8 .59
Matheucius	8 .13	8 .01	....	8 .92	8 .80
Comes Cassalio.	8 .08	....	....	8 .98	....
Le Monnier	8 .62	8 .21	....	8 .20	8 .60
De la Lande	8 .53	8 .16	....	8 .36	8 .65
Clouet	8 .51	8 .09	....	8 .39	8 .73
Baudouin	8 .51	8 .23	....	8 .39	8 .58
Fouchy	8 .49	8 .29	....	8 .43	8 .42
Ferner	8 .49	8 .29	....	8 .43	8 .42
Messier	8 .49	8 .20	....	8 .43	8 .32
De la Caille	8 .35	8 .18	....	8 .62	8 .63
Merville	8 .30	7 .93	....	8 .68	8 .95
Condamine	8 .28	8 .18	....	8 .77	8 .63
Maraldi	8 .28	8 .11	....	8 .77	8 .71
Mayer	8 .24	7 .80	....	8 .88	9 .22
Bliss	8 .46	8 .43	....	8 .48	8 .26
Short	8 .58	8 .07	....	8 .24	8 .82
Dollond	8 .49	8 .33	....	8 .45	8 .39
Canton	8 .49	8 .22	....	8 .45	8 .59
Wykstrom	8 .39	....	8 .52	8 .63	....
Wargentin	8 .20	8 .29	8 .06	9 .20	8 .51
Klingenstiern	8 .15	8 .31	8 .48	9 .35	8 .48
Bergman	8 .49	8 .24	8 .06	8 .69	8 .64
Mallet	8 .58	8 .26	8 .96	8 .10	8 .61
Melander	....	8 .26	9 .36	....	8 .61
Stromer	8 .62	....	9 .56	8 .00	....
Planman	8 .38	8 .10	8 .64	8 .83	9 .20
Hellant	8 .28	8 .17	8 .28	9 .26	9 .07
Chappe	8 .43	8 .12	8 .39	....	....
Rumousky	8 .26	8 .12	....	....	....
Dollier	8 .46	8 .37	....	....	....
....	....	....	....	....	....
Medium of all	8 .38	8 .18	8 .63	8 .62	8 .65

And the medium of all these means gives 8″.49 for the sun's parallax. And if we reject all the parallaxes which result from the comparisons of the Pekin observations on account of the uncertainty in the longitude of that place, there will result for the sun's parallax 8″.28, being the medium deduced from the observations made at the Cape of Good Hope.

§ VII. But as M. Pingré, in his tract on the sun's parallax, has some doubt about the observations made at the Cape of Good Hope, M. Planman has here deduced the parallax from a comparison of the observations at Paris, and at Bologna, with many of the other observations, as in this following table.

Observers.	Paris. Contact 2.	Paris. Contact 3.	Bologna. Contact 2.	Bologna. Contact 3.
Rumousky	8″.00	7″.98	8″.44	8″.11
Chappe........	8 .83	7 .88	9 .02	8 .12
Hellant	7 .81	8 .00	8 .63	8 .25
Planman	8 .20	7 .75	9 .00	8 .08
Stromer	9 .52	....	10 .25	....
Mallet	9 .33	8 .37	10 .10	8 .66
Melander......	....	8 .37	....	8 .66
Bergman	8 .67	8 .29	9 .65	8 .59
Wargentin	7 .13	8 .69	8 .43	8 .75
Klingenstierna..	6 .83	8 .78	8 .20	8 .83
....	....	....	....	....
Medium of all	8 .22	8 .23	9 .08	8 .45

Hence the medium of these means gives again 8″.49 for the sun's parallax. But rejecting the 3d column, or that having the greatest deviation, the others give for the solar parallax 8″.30, differing very little from the former medium 8″.28, deduced from the best observations. Hence then, from the late transit of Venus, the sun's parallax may be stated at 8″.28, so far as the difference of meridians can be depended on. And it appears to Mr. Planman that this quantity, on more accurate observations from the next approaching transit of Venus, will be found to be rather too much, than too little.

XVII. On the Manner of Inoculating the Small-pox, on the Coast of Barbary, and at Bengal, from a Memoir, written in Dutch, by the Rev. Mr. Chais, at the Hague. By M. Maty, M. D., S. R. S. p. 128.

Having long thought that the Arabs, who, about the middle of the 6th century, were the first who wrote on the small-pox, were likewise the first inventors of the method to prevent the fatal consequences of that disorder, Mr. C. was very desirous to get what information he could concerning the introduction of inoculation in Africa, and in the East Indies. About 20 years before, Cassen Aga, a Tripolitan ambassador at London, informed the people about him, that inoculation was universally practised, as well at his court, as at Tunis and Algiers; but that no certain account could be given, either of the introducers of the method, or of the place whence it took its rise.

One of the chief ministers of state in Holland was so good, on this information, and at Mr. C.'s desire, to send a few queries on that subject, drawn up by himself, to a gentleman, who, for several years, had resided with a public character at Algiers. The following is a summary of the answers to his queries. "The small-pox is, as well as in Holland, a contagious distemper at Algiers, Tunis, and Tripoli, and fully as destructive. To avoid the bad consequences of the natural disorder, many people have recourse to inoculation, which there is performed in a very different manner from what is used in our country. The person, who intends to be inoculated, having found out a house, where the small-pox is, and of a good sort; goes to the bed of the sick person, if he is old enough, or, if a child, to one of his relations; and speaks to him in the following manner; I am come here to buy the small-pox: the answer is, buy if you please. A sum of money is accordingly given, and 1, 3, or 5 pustules (for the number must always be an odd one, not exceeding 5), extracted whole, and full of matter. These are immediately rubbed on the skin of the hand, between the thumb and fore finger. This is sufficient to communicate the infection; and as soon as it begins to take effect, the inoculated patient is put to bed, carefully covered with red blankets; and heating medicines are given him with some honey of roses. He is allowed goat's broth for his nourishment, and for his drink an infusion of some herbs; notwithstanding this treatment, it seldom happens that the small-pox procured in this manner has any bad consequences; and almost never that any body dies of it; but hitherto the proportion of the mortality in the natural, to that in the artificial way, has not been ascertained. Lastly, though the time when this practice was introduced in Africa be unknown, yet it is there very old, and the Arabs are generally thought to have been the inventors of it."

From this account it plainly appears; 1. That in Africa the operation is performed as it is in Wales, by the rubbing in of the matter, and that this is done to prevent the fatal consequences too often following the natural infection; 2. That this inoculation is generally successful, notwithstanding the heat of the climate, and the bad management of the patients; and 3. That the origin of it is very ancient, and ascribed to the Arabs.

Before Mr. C. had received these informations from Algiers, he had engaged some friends settled in 3 different parts of the East-Indies, to procure him some accounts from thence, on the same subject. He, at last, received the following answer from one of them, who resided at Patna, in the province of Behaar, 180 leagues from Bengal. "I have sent for several physicians, to be informed of the things you seem desirous to know about inoculation; the practice is hitherto not used in this province; but having met with a Bengalian doctor, he gave me the following account. Though the first introduction of the operation at Bengal is now unknown, it has been in use in that country for a very long time, and is performed in two different ways. For the first, some of the variolous matter of a good kind, having been gathered, is kept for use.

When a child is to be inoculated, the skin between some of the fingers is pricked by means of two small needles joined to one another. After having rubbed in a little of the matter upon the spot, a circle is made by means of several punctures, of the bigness of a common pustule, and matter is again rubbed over it. The wound is then dressed with lint; a fever ensues, and after some days, the eruption, which if the fever has been strong is observed not to be very copious. To excite the fever, the patient is made to bathe in a tub of water. As this way of managing the operation is very painful, a more easy one has been invented for people of quality and substance. A little of the matter is mixed with sugar, and swallowed by the child in any sweet and pleasant liquid. The same effect is produced, but the first method is thought to be the best."

XVIII. Croton Spicatum, nova Plantæ Species ex America, quam Descriptione et Icone illustravit Petrus Jonas Bergius, M. D., Acad. Scient. Stock. Membr. &c. p. 132.

The plant described in this paper by Dr. Bergius is the Croton Lucidum. Lin. Mantiss. Plantar. Croton foliis ovatis glabris, floribus spicatis, stylis multifidis depresso-pubescentibus, caule frutescente. It is a native of America, and rises with a shrubby branching stem, smooth leaves, and spiked flowers.

XIX. Observations on the Barometer and Thermometer, and Account of the whole Rain in every Month of the Year 1767, taken at the Royal Hospital near Plymouth. By William Farr, M. D. p. 136.

There is nothing of any use now in this register of the weather.

XX. On the Inoculation in Arabia, in a Letter from P. Russell, M. D., at Aleppo, to Alex. Russell, M. D., F. R. S., preceded by a Letter from Dr. Al. Russell. p. 140.

The following account of inoculation in the east, Dr. Alex. Russell states to have been received from his brother (Dr. Patrick Russell) at Aleppo, and though nothing further seemed wanting in this country to remove prejudices against that practice, yet he thought its being made public might be of some use to other European nations, where such prejudices still prevail; and, as a matter of curiosity, would not be unacceptable to the R. S.

Just before his leaving Aleppo, he heard that it was practised among some of the Bedouins there, and went by the name of buying the small pox; but being then much engaged with other business, it quite escaped his memory, and indeed his information was so slight, that he did not think it right to mention it in his Natural History of Aleppo.

In his letter to his brother, Dr. Patrick Russell states, that from the manner in which inoculation is mentioned in the Natural History of Aleppo, he suspected the circumstance of its being a common practice among the Arabs must have escaped him (his brother). He himself was ignorant of it for several years after Dr. A. R. had left Aleppo, and a mere accident brought it at last to his knowledge. About 9 or 10 years before, while on a visit at a Turkish Harem, a lady happened to express much anxiety for an only child, who had not then had the small-pox; the distemper at that time being frequent in the city. None of the ladies in the company had ever heard of inoculation; so that, having once mentioned it, he found himself obliged to enter into a detail of the operation, and of the peculiar advantages attending it. Among the female servants in the chamber was an old Bedouin, who having heard him with great attention, assured the ladies, that his account was upon the whole a just one, only that he did not seem so well to understand the way of performing the operation, which she asserted should be done not with a lancet, but with a needle; she herself had received the disease in that manner, when a child; had in her time inoculated many; adding, that the practice was well known to the Arabs, and that they termed it buying the small-pox.

In consequence of this hint, Dr. P. R. set about the procuring more particular information from the Arabs of Aleppo; and the result of his inquiry was, that the practice of inoculation had been of long standing among them. They indeed did not pretend to assign any period to its origin; but those of 70 years old and upwards remembered to have heard it spoken of as a common custom of their ancestors, and made little doubt of its being of as ancient a date as the disease itself. Their manner of operating is, to make several punctures in some fleshy part, with a needle imbued in variolous matter, taken from a favourable kind of pock. They use no preparation of the body; and the disease communicated in this way being, as they aver, always slight, they give themselves little or no trouble about the child in the subsequent stages of the distemper.

This method of procuring the disease is termed buying the small-pox, on the following account. The child to be inoculated carries a few raisins, dates, sugar plums, or such like, and showing them to the child from whom the matter is to be taken, asks how many pocks he will give in exchange. The bargain being made, they proceed to the operation. When the parties are too young to speak for themselves, the bargain is made by the mothers. This ceremony, which is still practised, points out a reason for the name given to inoculation by the Arabs; but by what he could learn among the women, it is not regarded as indispensably necessary to the success of the operation, and, is in fact, often omitted.

The Bedouins at Aleppo, who are employed in the service of the Harems, more rarely have recourse to inoculation, their children being often brought up in company with those of the Turks, by whom, as Dr. A. observes, the practice is not admitted. But the Bedouins, less connected with the Turks, who dwell within the city; those who live in tents without the city walls, and the Arabs of the adjacent desart under the Emir, do commonly inoculate their children.

It being highly probable that a practice, which was so common in these parts, might be known also to the more eastern Arabs, Dr. P. R. applied for information to several Turkish merchants of Bagdat and Mousul, who occasionally reside a few months in the year at Aleppo. By these he was assured, that inoculation was not only common in both the cities first mentioned, but also at Bassora; and that at Mousul particularly, when the small-pox first appeared in any district of the city, it was a custom sometimes to give notice by a public crier, that such as were inclined might take the opportunity to have their children inoculated.

Dr. P. R. inquired at the same time of the Bagdat merchants, whether the Arabs, who dwell on the banks of the river between that city and Bassora, used the same method of propagating the small-pox. They told him, they believed it to be common also among those Arabs; though (with an ingenuousness not usual in this country) they owned they had never thought of inquiring about the matter, and might therefore perhaps be mistaken. But he afterwards had an opportunity of being better informed by the Arabs, who come to Aleppo with the eastern caravans; from whose accounts it would appear, that inoculation has from time immemorial been a practice among the different Arab tribes with which they were conversant; comprehending, besides those in the numerous encampments on the banks of the Euphrates, and the Tigris below Bagdat, other tribes in the vicinity of Bassora, and in the desart.

For before these several years, very few slaves had been brought from Georgia. From what he could collect among those then at Aleppo, who remembered any thing of their own country, inoculation was well known there; he had seen several old Georgiana women, who had been inoculated, when children, in their fathers' houses. In Armenia, the Turkoman tribes, as well as the Armenian christians, have practised inoculation since the memory of man; but, like the Arabs, are able to give no account of its first introduction among them. To what extent inoculation reaches in the Gourdeen mountains, he did not know with any certainty: it is practised by the Gourdeens in the mountains of Bylan, and Kittis; and he had reason to think, extends much farther. At Damascus, and along the coast of Syria and Palestine, inoculation has been long known. In the Castravan mountains it is adopted by the Drusi, as well as the christians. Whether the Arabs of the desart, to the south of Damascus, are acquainted with this manner of communicating the small pox, he had not hitherto been able to learn; but a native of Mecca, whom he had occasion to converse with this summer, assured him, that he himself had been inoculated in that city.

It has already been mentioned, that the Turks at Bagdat and Mousul make no scruple to inoculate their children. He had seen also some Turkish strangers at Aleppo, who had been inoculated at Erzeroon. Hence it is probable that the Turks, in other parts of the Ottoman empire, do not merely, as fatalists, reject inoculation; but that other considerations, which have influence in countries where fatalists are ridiculed or anathematized, concur likewise in Turkey, to oppose the reception of a practice so beneficial to mankind. The child of a bashaw here, was by his advice inoculated about 8 years before; but that is the only instance he had known among the Turks at Aleppo.

The Jews at Aleppo absolutely reject inoculation; partly from scruples of a religious kind, and partly from the distrust of its success. At Bagdat, Bassora, and in Palestine, having acquired a more favourable opinion of an operation which they see so often performed with success, they had got the better of other scruples, and join in the practice with their neighbours.

He had several times conversed on this subject with the Mufti, as also with some of the Rabbis; but the theology of both was too abstruse for him: their arguments, so far as he was able to comprehend them, seemed to be no less cogent against all chirurgical operations, which were attended with any degree of danger to life, than against inoculation.

In the different countries above-mentioned, inoculation is performed nearly in the same manner. The Arabs affirmed, that the punctures might be made indifferently in any fleshy part: those he had occasion to examine, have all (a very few excepted) had the mark between the thumb and the fore finger. Some of the Georgians had been inoculated in the same part, but most of them on the fore arm. Of the Armenians some had been inoculated in both thighs; but the greatest part (like the Arabs) bore the marks upon the hand. Some of the Georgian women remembered, that rags of a red colour were chosen in preference for the binding up the arm, a circumstance of which he had been able to discover no trace among the Arabs. Buying the small pox, is likewise the name universally applied to the method of procuring the disease. There are, it is true, other terms made use of both in the Arabic and Turkish languages; and at Aleppo, it is principally known to the christians by the name of inoculation.

From the sameness of the name, as well as from the little diversity observable in the manner of performing the operation, it is probable the practice of inoculation in these countries was originally derived from the same source: and that it is of considerable antiquity, can hardly be doubted, if we consider the large extent of country over which it is found to have spread, and the obstacles it must have met with in a progress through various nations, of which some are separated by polity as well as religion, while others, peculiarly tenacious of their customs, are little disposed to admit those of strangers.

That no mention is made of inoculation by Rhazes, Avicenna, or any other of the ancient Arabian medical writers known in Europe, is, he believes, in general supposed; and he was assured by the native physicians here, that nothing is to be found regarding it, in any of a more modern date. Some learned Turkish friends at Aleppo, sometime before were prevailed on at his request to make inquiry, but had not been able to discover any thing concerning inoculation; though they searched not only the medical writers, but also the historians, and some of the poets.

It appears from accounts communicated to the R. S. in the year 1723, by Dr. Williams and Mr. Wright, that inoculation had been known in certain parts of Wales so far back as the last century; and it is remarkable, that it there bore the same name, by which it is most generally known to the Arabs. He thinks it has also been discovered to be an ancient practice among the vulgar in different parts of the continent. If inoculation was really known so long ago in Europe, and the accounts of it till within these 50 or 60 years are found to be merely traditional, the silence of the Arabian writers, on a practice which probably was never adopted by their physicians, is the less to be wondered at. What may perhaps appear more strange, is, that after the year 1720, though the curiosity of the public has, at different times, been excited by the controversies relating to inoculation, the state of that practice in Syria, where there were so many European settlements, should have remained unknown both in England and in France, which probably was the case, as the advocates for inoculation have made no reference to it.

Whether before the account transmitted by Pylarini to the R. S., inoculation had not been mentioned by any of the travellers who had visited these countries, he did not presume to determine. In the books he had had occasion to peruse, there is nothing to be found on the subject. Among the travellers the most likely to have mentioned it was Rauwolf: yet, however rational it may be to think that a practice of such a kind, had it then prevailed, could hardly have escaped the notice of so diligent an observer, it would be rash to infer from his silence that it was not known to the Arabs in the 16th century. The justly celebrated French botanist is equally silent, though in the beginning of the present century he visited several places where inoculation was undoubtedly at that time both known and practised.

Having related in what manner he came to learn inoculation was known to the Arabs, he could arrogate no merit in the discovery; nor would he be thought to insinuate any reflection on the accuracy of the indefatigable M. Tournefort, to whose labours the curious stand so much indebted. Customs the most common, in distant countries, are often of all others the least apt to attract the observation of travellers, who, engaged in other pursuits, must be indebted to accident for the knowledge of such things, as the natives seldom talk of, from the belief that they are known to all the world. This consideration may, in some measure, account for inoculation having been overlooked by those who have transiently passed through these countries; and is all that can be offered as an apology, for the having remained so long unacquainted with a fact in medical history, in a situation where they had so many opportunities of information.

Dated Aleppo, Nov. 26, 1767.

XXI. On the Weather, the Barometer, Thermometer, and Magnetic Declination. By Dr. Wolfe, at Warsaw. Dated Warsaw, April 15, 1768. p. 151.

Our winter has been very long and very severe. The frost was, at a medium, from the middle of December to the middle of March, at 9 degrees of Reaumur's thermometer below the freezing point. Three times it was from 19 to 20 degrees, and very seldom ascended to the freezing point. Last year the frost was at 24 degrees, but of no duration. In the year 1740 it was at 26 degrees, which, perhaps, is the lowest it has been remembered in this country. This year we have had little or no snow, which made the frost exceedingly more sensible. The barometer is here, at a medium, at 28½ inches Rhinland measure: great storms sink it 2 inches lower; and very dry weather raises it 1⅓ inch higher. The declination of the needle at this place is 11 degrees and a half to the west.

XXII. Extract of a letter from Mr. Peter Wargentin, Sec. of the Royal Acad. of Sciences at Stockholm, and F. R. S., dated Feb. 23, 1768.

As it is related in the public newspapers, that the weather * has been uncommonly cold in Germany, England, and France, towards the end of last year, and the beginning of the present year, Mr. W. here sent the degrees of altitude by Reaumur's thermometer, observed since the beginning of November, 1767. The altitudes here set down are the arithmetical means of 3 taken every day, viz. in the morning before sun-rise, a little after noon, and at 10 o'clock at night. Hence it appears that the cold has been moderate here with respect to this climate, and nothing more than common, though it was without intermission, from the beginning of December. The greatest cold in these

* The least height of Fahrenheit's thermometer, set down in the course of the astronomical observations made at the Royal Observatory, was 15° on Jan. 6, at the transit of Venus over the meridian at 8^h 42^m A. M. At which time nearly the same was observed by Lord Charles Cavendish in Great Marlbro'-Street, at 17°. But by a thermometer described in vol. 50, of these Transactions, placed on the top of the same house, in a very bleak situation, Dec. 31, it appeared to have been at 12¼° in the preceding night; Jan. 3, 16°; and Jan. 6, 16°, Jan. 7, 13°.—Nevil Maskelyne.—Orig.

months happened on the 2d day of January in the morning; the quicksilver then stood at 17.2 below the point of freezing.

XXIII. Introduction to two Papers of Mr. John Smeaton, F. R. S., by the Rev. N. Maskelyne, B. D., F. R. S. p. 154.

The two following papers Mr. M. received from Mr. Smeaton, with his desire to communicate them to the Royal Society, if he thought they contained any hints conducive to the improvement of astronomy. As the first paper points out the time of observing the menstrual parallaxes of the planets in those circumstances in which they will be greatest, and at the same time shows how to obviate the error, which would otherwise arise from the inaccuracy of their theories (which must necessarily be used in the calculation), by correcting them from other observations, made on purpose, before and after the first-mentioned observations; and the 2d paper gives a new and accurate method of observing the places of the heavenly bodies out of the meridian, independent of refraction, Mr. M. apprehends they would prove acceptable presents to astronomers.

Mr. M. adds one other remark, that has been suggested by the perusal of Mr. Smeaton's first paper; that, as it is there proposed to find the dimensions of the orbit described by the revolution of the earth about its common centre of gravity and the moon's, by means of the menstrual parallax of Mars, near his opposition, or of Venus, near her conjunction with the sun; the same may also be determined with advantage from the sum of the menstrual parallaxes of these two planets, when they happen to be in the required positions at the same time, which will indeed happen but seldom; or even from the sum of the menstrual parallaxes of Mars and the sun, which may be observed together at every opposition of Mars to the sun; the sum of the menstrual parallaxes of Mars and Venus in these circumstances, according to the numbers used by Mr. Smeaton, will sometimes amount to 87″, and the sum of those of Mars and the sun to 52″.

XXIV. A Discourse concerning the Menstrual Parallax, arising from the Mutual Gravitation of the Earth and Moon; its Influence on the Observations of the Sun and Planets; with a Method of observing it. By J. Smeaton, F. R. S. p. 156.

It is demonstrated by Sir Isaac Newton in the Principia, that it is not the earth's centre, but the common centre of gravity of the earth and moon, that describes the ecliptic; and that the earth and moon revolve in similar ellipses, about their common centre of gravity. The same great author has also investigated, from the different rise of the tides, when the moon is in conjunction or opposition to the sun, to those which happen when the moon is in her quadratures; that the quantity of matter in the earth is to that in the moon, as 39.78 to 1; whence, and the known distance of the earth and moon, it would follow that the common centre of gravity of the two bodies falls without the surface of the earth, by one half of its semidiameter: that is, that the centre of the earth describes an epicycle round the common centre of gravity once a month whose diameter is 3 semidiameters of the earth.

Dr. Gregory, in his astronomy, has laid hold of this circumstance, in order to prove the relative gravity of the earth and moon, by observation; which is the subject of his 60th proposition of the 4th book; in which he has demonstrated, that if an observer on the earth makes a correct observation on the sun's place, when the moon is in one quadrature, it will differ from a like observation, taken in the opposite quadrature (according to a mean elliptic motion) by an angle which the diameter of this epicycle will subtend at the sun. The same learned author has also showed, in the scholium to the same proposition, that this quantity, or parallax, will be twice greater to Mars in opposition, and 3 times greater to Venus, in her inferior conjunction with the sun. The difference thus produced in the apparent place of the sun, and of all the primary planets, being governed by the moon, and having its period the same, may perhaps be not unaptly called the menstrual parallax.

Now if, with Sir Isaac Newton, the relative gravities of the earth and moon be taken between the proportion of 39 and 40 to 1; the menstrual parallax of the sun will come out 13″ on the radius of the earth's epicycle, and will affect the solar observations at the opposite quadratures, by double that quantity, viz. 26″: in like manner, the mean distance from the earth of Mars in opposition, being to the sun's mean distance, as 1 to 2.1; and the least distance of Mars from the earth, to the sun's mean distance as 1 to $2\frac{3}{4}$, the menstrual parallax of Mars will affect the observation on him in that situation, by 56″ and 73″$\frac{1}{2}$, respectively.

The mean distance of Venus from the earth, in her inferior conjunction, being to that of the sun as $3\frac{1}{2}$ to 1 nearly, and not very variable, on account of the orbit of Venus being almost circular; the menstrual parallax would affect the place of Venus, in that situation, by a quantity not less than 92″; and in all other situations in proportion to her distance; which also holds with respect to all the rest of the planets.

These disturbing quantities are by no means to be dispensed with, in the nice and critical state that astronomical observations and calculations have arrived at, in consequence of the discoveries of Dr. Bradley, who may be said to have given a basis to astronomy; however, could we rely on the data, on which Sir Isaac's investigation of the relative gravity of the earth and moon is founded, we should have nothing to do but to apply an equation to the particular cases, according to the diameter of the epicycle, as deduced from the relative gravity; but whoever considers the great obstructions that the water of the sea meets with in its motion to obey the influence of the moon; the great difficulty in ascertaining the true height of the tides, from the many disturbing causes intervening; and the many uncertainties, and want of coincidence, that have attended, and must attend, such observations, must confess, that this matter does not seem capable of such a determination from that quarter, as the present state of astronomy requires.

Accordingly, since the time of Dr. Gregory, those great astronomers Dr. Bradley, De la Caille, and others, have applied themselves to determine the quantity of the menstrual parallax from solar observations: but though these have given cause to suppose that the relative gravity of the earth and moon are not above $\frac{1}{3}$ of the quantity deduced from the tides; yet, as the observation of these small angles principally depends on the observation of the sun's right ascension (which, depending on the measure of time, is less capable of exact observation, than if depending on divided instruments); the deductions thence drawn seem still wanting of that certainty which the subject demands; and if to this we add, from a deduction of Mr. Maskelyne, that the relative gravity of the earth and moon is as 76 to 1, derived from the effect that the moon produces in the nutation of the earth's axis; the relative gravity, and consequently the parallaxes depending on it, will be reduced to almost one half of those resulting from Sir Isaac's determination.

It is true, that the quantity of effect of the menstrual parallaxes will not be great, if computed on Mr. Maskelyne's induction, for as much as that the common centre of gravity will be considerably within the earth's surface; yet, even in that case, the sun's transit over the meridian, when the moon is in one quadrature, will differ nearly one second of time from that observed in the opposite quadrature; and though de la Caille and Mayer have formed equations depending on the moon, to be applied to the equation of time; yet, if we are at an uncertainty, whether the maximum of this equation is one second, two-thirds of a second, or half a second of time, each way, we are still under a material difficulty; for though these differences are so small, that it is not easy to determine them exactly from solar observations; yet, as they are capable of creating a sensible difference in these observations, they will, so long as they remain undetermined, prevent that solidity and firmness to the solar observations, which is the more necessary as they are the foundation of all the rest: but with respect to those planets, that in their periods come nearer to us than we to the sun, the observations on them will be affected by a greater uncertainty.

The determination of the menstrual parallax is of still more importance, as it is a necessary consideration in the determination of the sun's parallax; and this, whether deduced from Mars or Venus, as shall presently be shown more particularly; but first to state the quantity of the menstrual parallax, according to the best data yet known, by a contrary process; and, taking the mean quantity of the sun's parallax, according to the determination of Mr. Short, at 8″.8, and the relative gravities of the earth and moon, according to Mr. Maskelyne, as 76 to 1, and the mean distance of their centres equal to $60\frac{1}{2}$ semidiameters; we shall then have the distance of the earth's centre from the centre of gravity, at $\frac{8}{10}$ of the earth's semidiameter (that is, $\frac{1}{5}$ of that semidiameter within the earth's surface) and the menstrual parallax equal to $\frac{8}{10}$ of the sun's parallax; consequently about 7″; and the double menstrual parallax, or vacillation, arising from the whole diameter of the epicycle, 14″; the mean menstrual parallax of Mars in opposition $29''\frac{1}{2}$; the greatest $38''\frac{1}{4}$; and that of Venus 49″; hence it follows, that, were a person to attempt the sun's parallax, by the diurnal motion of the earth, applied as a basis to Mars in opposition, as has formerly been tried; and should the moon be at new or full at the same time, the change of place of the earth's centre, in its own epicycle, would amount to an angle seen from Mars of 1″.3 nearly; that is, in case the interval between the observations was 8 hours, and Mars at his mean distance; but if Mars was not at his nearest distance, this change would in the same time amount to 1″.7 nearly. In like manner if a transit of Venus happen near the new or full moon, as will be the case next year, the time of the transit will be affected by a change of place, such as the earth's centre will describe in its epicycle, during the time of the whole transit, if the beginning and end are observed in the same place; or during the difference of absolute time, at which the transit appears to begin or end to different observers in distant meridians. Thus, when the same observer sees the beginning and end in the same place, the base described by that observer, from the earth's diurnal motion, must be corrected by the space described by the earth's centre, in the circumference of its epicycle, during that time; which, if it be supposed of 7 hours, will amount to an angle of 1″.9, seen from Venus; but, where the beginning or end is seen by different observers in distant meridians, as the difference of absolute time can hardly amount to above 15 minutes the change of place of the earth's centre will for that time be but small; however, at the rate beforementioned, it will for 15 minutes affect the parallactic angle seen from Venus, by about $\frac{7}{100}$, of a second; and the parallax of the sun, by about $\frac{1}{400}$ part of the whole: but this proportional part will remain the same, whether the distance of meridians be such as produces a greater or less difference of absolute time than 15 minutes.*

From what has been said, Mr. S. supposes it will appear, that the effects of

* If an error of $\frac{1}{400}$ part of the whole may be supposed in the observation for determining the sun' parallax by the transit of Venus, a neglect of the menstrual parallax may make it $\frac{1}{200}$ part of the whole.—Orig.

the menstrual parallax are worthy of consideration; and that nothing has been yet executed, by which it has received a determination sufficiently accurate; for, in regard to observations on the sun, the whole quantity is too small to be minutely observed in right ascension: and with respect to the application to Mars and Venus, as suggested by Dr. Gregory, Mr. S. does not know that any thing has been done; and indeed no wonder, as the theory of the motion of Mars and Venus has not been as yet so critically reduced to computation, as to render their parallaxes (though in themselves much greater) deducible with equal certainty as that of the sun.

What Mr. S. therefore has now to propose, is a method of observing the menstrual parallaxes of Mars and Venus, without laying any undue stress on the theory of their motions. The first opportunity of making an observation for this purpose, will be at the next opposition of Mars; which according to the nautical almanack, will happen the 26th of October next, in the morning. He therefore endeavours to illustrate this matter by taking that as an example. The distance of Mars from the earth will then be somewhat less than the mean distance, that is, as 1 to 2.2; and consequently his double menstrual parallax, according to Mr. Maskelyne, will be near 31″ in the point of opposition. Now, as the moon will be at full, not above 12 hours preceding that opposition, the moon will be nearly in the most favourable situation for the purpose.

For this end, let an accurate observation be made upon the place of Mars at the following times, viz. first, near the time of the new moon, preceding Mars's opposition; or more properly at the nearest opportunity, to the time of the moon's opposition to Mars; which will happen in the night between the 12th and 13th of October: 2dly, let the place of Mars be observed when the moon is nearest her quartile with Mars; that is, between the 19th and 20th of the same month: 3dly, let an observation on Mars be made when the moon is in conjunction with Mars, the nearest to his opposition with the sun; that is, between the 25th and 26th of ditto: 4thly, let Mars again be observed when the moon has moved on to her quartile with Mars, viz. between the 31st of October, and 1st of November: and 5thly and lastly, let the place of Mars be observed, when the moon has again got to her opposition with Mars, which happens between the 7th and 8th of November.

Now it is manifest, that when the moon is in conjunction or opposition to Mars, the centre of the earth, the centre of Mars, and the common centre of gravity of the earth and Mars, will be nearly in a right line, and consequently, that an observer will then see Mars, in the same place in the heavens, as if the common centre of gravity was the same as the centre of the earth; therefore then the place of Mars will be unaffected by a menstrual parallax; and such will be the 1st, 3d, and 5th, of the observations above propounded.

It is equally evident, that when the moon is in quartile with Mars, and moving towards a conjunction, an observer at the earth's centre will see Mars more backward in the ecliptic, than if seen from the common centre of gravity, by $15''\frac{1}{2}$; and that when the moon is in her opposite quartile with Mars, and moving from her conjunction, that then an observer at the earth's centre will see Mars advanced in his orbit, more forward by $15''\frac{1}{2}$, than if seen from the common centre of gravity; and the one observation checqued with the other, will, according to a mean elliptic motion, differ by the quantity of $31''$; and such will be the 2d and 4th observations above propounded.

Now, from the 1st, 3d, and 5th, observations, 3 points of Mars's orbit will be given; which, by the help of the theory of Mars's motion in an elliptic orbit, whose aphelion, eccentricity, and nodes, are known sufficiently near for this purpose; the intermediate places of Mars may be inferred with the requisite degree of accuracy: and particularly, as the two intermediate observations, viz. the 2d and 4th, will be nearly at equal intervals of time between the 3 others: hence it follows, that the difference between the inferred, or computed places, at the quartiles, and the observed places at those times, will be the menstrual parallax required.

It is to be noted, that the times above specified are the most favourable for the observation; and could those be made uninterruptedly from weather, there would be the less occasion for any other: but as much as possible to prevent disappointments of this kind, it will be right to begin the observations, a month preceding making the proper observations, at the conjunctions, quartiles, and oppositions, of the moon with Mars, which will be the means of supplying such observations as may happen to prove abortive before the opposition of Mars, and also, in case any of the observations to be made after that opposition shall prove deficient, the observations may be carried on for a month or competent time afterwards. As a further security against disappointments, as well as cheque, it will also be advisable to make the proper observations the night preceding and subsequent to those in which the quartiles, conjunctions, &c. happen; for, as the quantities will not differ considerably from those obtained on the days specified, with proper allowances they may be brought in support and confirmation of the former.

In like manner, when Venus is moving towards her inferior conjunction with the sun, as will happen next year, the same observations may be made with respect to her: and continued for a necessary time, to get observations of the place of Venus; viz. the first, when the moon is in conjunction or opposition with Venus: a 2d, when the moon is in her quartile with Venus; a 3d, in conjunction or opposition: a 4th, when the moon is in her opposite quartile to the former: and a 5th, again in conjunction or opposition: the same opportunity will also offer when Venus is moving from her inferior conjunction with the sun, and becomes a morning star.

In regard to the observation of Venus, it is remarked by astronomers, that she is to be seen with a good transit telescope, when she is within a few degrees of the sun; but, as she is 3 times nearer the earth, than the sun's mean distance, when her elongation is 25° in the inferior part of her orbit, it is plain, that the necessary observations may easily be made, when her menstrual parallax will be at a medium, 3 times greater than the sun's; and consequently amounting for the whole difference to 42″.

To avoid embarrassment in description, Mr. S. has hitherto supposed, that all the observations are made in the meridian; in which case the right ascensions will be the same as they would appear from the centre of the earth; and consequently the planet's longitudes, thence deduced, nearly the same: but it is easy to see, that if the quartile observations be made when the planets are considerably to the east or west of the meridian, and so chosen, that the place of the observer be further distant from the common centre of gravity, than the centre of the earth is from that centre, that the base of the observations will be considerably enlarged. Thus, in our latitude, supposing that the quartile observations are made 4 hours before and 4 hours after the planet passes the meridian, this will produce an enlargement of the basis by one of the earth's semidiameters: and as the whole base or diameter of the epicycle comes out, according to Mr. Maskelyne, no more than 1.6 of the earth's semidiameters; the base will, according to this method, come out 2.6; and consequently, at the next opposition, the menstrual parallax of Mars will be thus enlarged to 50″, the greatest to 62″ $\frac{1}{2}$, and that of Venus at a mean, to 74″$\frac{1}{2}$.

It must however be acknowledged, that no kind of observations of the places of the planets are of equal validity with those taken with the best instruments in the meridian; those taken with micrometers perhaps not excepted: for however accurately small distances can be measured by the micrometer of Mr. Dollond, yet as these measures can hardly be reduced to the ecliptic, without having the difference of declination or right ascension from other means (except two stars making somewhat near a right angle with the planet should appear within the field of view at once); and as in all these cases the rectification of the places of the stars themselves ultimately depends on meridian observations, we may perhaps be allowed to say, that in the most favourable cases of the micrometer, the determinations thence to be drawn, are not superior to meridian observations, and in less favourable cases, must be inferior: however, as the micrometer observations out of the meridian give an opportunity of repetition as often as we please; and the observations for rectification of the stars concerned, can be repeated in the meridian, as often as we please also; it must be equally allowed, that when these kind of observations are taken, not too near the horizon, when proper stars offer for this purpose, and the whole skilfully managed; these kinds of observations fall but little short of those taken immediately in the meridian. He cannot therefore hesitate to recommend, that the quartile observations be taken out of the meridian, as well as in it: in the first place, by Dollond's micrometer, if stars offer in proper positions; and if not, 2dly, by taking differences of right ascension and declination between the planet and the stars, by the common micrometer, in case proper stars offer themselves for this purpose: but as it frequently happens, that no proper stars offer themselves to micrometers of either kind, and this is still more likely to happen in the observations of Venus, which will be chiefly in the day light; he offers (what to him is) a new method of observation out of the meridian; and which, though he esteems it not equal to micrometer observations of either kind, he apprehends will fall so little short of it, and prove so much superior to any other method now in practice in these cases, he gives this general method of observing out of the meridian, by way of appendix.

In the next observation of Mars, it has been stated, that in the meridian observations alone, the menstrual parallax, according to the smallest estimation, may be expected to amount to 31″ in longitude; which, turned into right ascension, will make about 2^s of time: now if it may be allowed, that a well-practised observer can take the time of a transit to $\frac{1}{4}$ part of a second, over a single wire, if he has 3 wires, or more, as usual, the mean of the 3 should be within $\frac{1}{12}$ part of a second; or within $\frac{1}{24}$ part of the whole quantity in question: it is however a matter of chance, whether the mean of 3 may or may not be within $\frac{1}{3}$ part of the whole; and as equal errors may be committed in the observations of the transits of the stars, with which the right ascensions of the planets in question are compared; which it is an equal chance whether they tend to correct or increase the errors committed in the former; yet if, as has already been proposed, the observations be continued for 2 or 3 months, instead of one; and observations, taken the day preceding and subsequent to the days of conjunction, quartile, and opposition; and this as well out of the meridian as in it; we can hardly doubt but that, if the weather should favour, so many cheques would be formed, that, from the next opposition of Mars alone, the affair may be brought within a 24th part of the whole; and if to this be added the force of such determinations, as may be drawn from observations on Venus, before and after her transit over the sun next year, it can hardly be doubted, but that those 3 will bring us within a single second of a degree, subtended from the nearest planet; and these conclusions will be further strengthened by future observations; as 2 years will scarcely pass without affording one opportunity or more of this kind.

As Mr. S. meant not to embarrass himself with exact computations, he has constantly supposed the distance of the common centre of gravity from the centre of the earth, to be a fixed quantity; whereas it will vary in the same proportion as the moon's distance varies; but, as this and many other minutiæ will properly enter the computation, when the observations are made, he refers them to the learned in this science.

XXV. Description of a New Method of Observing the Heavenly Bodies out of the Meridian. By J. Smeaton, F. R. S. p. 170.

The instrument Mr. S. proposes for this purpose, is a transit telescope, mounted on a vertical axis; for example, such a one as is described in the introduction to the Histoire Celeste of Mr. le Monnier; being one of the instruments made by Mr. Graham for the academicians who went to measure a degree at the Polar circle; this, or any other instrument on equivalent principles, will suffice, that is capable of such adjustments, as to be made correctly to describe an almacanther and azimuth circle; and capable of being retained in any given position; the use will appear by the following example.

Make choice of any fixed star, which, according to the diurnal motion, precedes the heavenly body to be observed by a few minutes, more or less, as it may happen; let the instrument be set to an azimuth, somewhat preceding the fixed star; and carefully observe the time of the star's transit across the vertical wire of the telescope; then wait till the heavenly body comes to the same azimuth; and, when arrived within the field of view, keep gently turning the screw that alters the elevation of the telescope, so as to follow the heavenly body in altitude; keeping it intersected by the horizontal wire of the telescope, till the body passes the middle vertical wire, and carefully note the time of its passage; there leave the telescope fixed as to altitude, and releasing the horizontal motion, turn it round on its vertical axis, till you meet with some star, that in a little time after will, by rising or falling, come to the same almacanther; and on its arrival, carefully note the time of its passage across the horizontal hair of the telescope.

Now, from the right ascensions and declinations of the two stars being previously known, or afterwards determined from meridian observations; the azimuth of the first star, and the altitude of the last, at the time of their respective passages, may be determined by computation; which will give the altitude and azimuth of the heavenly body, for the time of the middle observation, when it passed the intersection of the two wires.

The same end may also be obtained by taking the observations in an inverted order; that is, by chusing a star at such an altitude, that the heavenly body shall in a competent time afterwards arrive at the same altitude, &c. but, as in these latitudes the alteration of azimuth is, especially in those parts that are in the neighbourhood of the zodiac, quicker than that of altitude, he apprehends it to be easier to follow the slower motion with the screw, so as to preserve the intersection, than the quicker, and therefore in general to be preferred; but where it happens otherwise, or the stars lie more conveniently, the inverse method may be pursued.

It is true, that some degree of dexterity and practice may be requisite in the observer in managing the set screw, so as to keep the object intersected by the wire; but if fine smooth screws, such as are used for micrometers to astronomical quadrants, are adapted to the instrument, as well that commanding the horizontal motion as the vertical, the management will be perfectly easy and familiar to an observer otherwise well practised. It is easy to see, that those stars are to be preferred that are nearest the heavenly body to be observed; and that, cæteris paribus, those in such positions, as rise or fall slowly, are best for determining their altitude; and those that alter their azimuth slowly, are best for determining the azimuth.

To avoid intricacy in description, Mr. S. has supposed only 2 wires intersecting each other at a right angle, in the focus of the telescope: but, for the sake of getting a medium in such parts of the observations as depend on time, it will be proper to have, not only 3 perpendicular wires, parallel to each other as common, but also 3 horizontal wires; the proportional distances of which being previously determined by observation, the oblique motions may, in parts not near the pole, be considered as right lines.

This method is the more valuable as it is entirely free from the knowledge of refractions; for since the computation gives the real altitude from the time given independent of refractions; and since the heavenly body is equally affected by refraction, at the same altitude; the computed altitude of the star will give the real altitude of the heavenly body cleared of refraction, which never enters the question: and since such stars may be chosen as will render the time intercepted short, there is the less chance of a change of refraction, during the time between the middle and last observation; and therefore this method will be particularly useful in observations near the horizon.

Observations of this kind may be made on the planets in the day light, by making use of the sun for the first observation, instead of a star; and waiting afterwards for the appearance of the stars.

XXVI. Specimen of a New Method of Comparing Curvilineal Areas; by which many such Areas may be compared as have not yet appeared to be Comparable by any other Method. By John Landen, F. R. S. p. 174.

When a body in motion is continually acted on by a variable force, the space it has passed over at the end of any given time, it is well known, will be expressed by the area of the curve, whose ordinate expresses the velocity of the body, while the time it has been in motion is expressed by the corresponding abscissa. Therefore the facilitating the computation of curvilineal areas will manifestly contribute to the improvement of the doctrine of motion. Which doctrine being a branch of philosophy of no small importance, such improvement will not be considered as a trifling speculation, by the R. S.

1. Geometricians have found, that if A be put to denote the whole area of the curve, whose abscissa is x, and ordinate $(1 - x^2)^{p-1} \times x^{2r-1}$, the whole area of the curve, whose abscissa is x, and ordinate $(1 - x^2)^{p-1} \times x^{2n+2r-1}$, will be $= \frac{r \cdot r+1 \cdot r+2\,(n)}{p+r \cdot p+r+1 \cdot p+r+2\,(n)} \times A$; n being any positive integer, and p and r any positive numbers, whole or fractional.

2. By the preceding article, the whole area, when the ordinate is $(1 + x)^{p-1} \times x^{2r+2z-1}$ is $= \frac{z \cdot z+1\,(r)}{p+z \cdot p+z+1\,(r)} \times \frac{1 \cdot 2\,(z-1)}{p \cdot p+1\,(z)} \times \frac{1}{2}$; the whole area, when the ordinate is $(1 - x^2)^{p-1} \times x^{2z-1}$, being $= \frac{1 \cdot 2\,(z-1)}{p \cdot p+1\,(z)} \times \frac{1}{2}$.

Likewise, by the same article, the same whole area is $=$ $\frac{r \cdot r+1 \cdot r+2\,(z)}{p+r \cdot p+r+1 \cdot p+r+2\,(z)} \times A$. Therefore this last expression is $=$ $\frac{z \cdot z+1\,(r)}{p+z \cdot p+z+1\,(r)} \times \frac{1 \cdot 2\,(z-1)}{p \cdot p+1\,(z)} \times \frac{1}{2}$. From which equation, p and r being positive, as before observed, A, the whole area of the curve, whose ordinate is $(1 - x^2)^{p-1} \times x^{2r-1}$, is found equal to $\frac{1 \cdot 2 \cdot 3\,(r+z-1) \times p+r \cdot p+r+1\,(z)}{p \cdot p+1 \cdot p+2\,(r+z) \times r \cdot r+1\,(z)} \times \frac{1}{2}$, z being any number whatever. Consequently, supposing z infinite, we find $A =$ the ultimate value, or limit of $\frac{1 \cdot 2 \cdot 3\,(z) \times p+r \cdot p+r+1\,(z)}{p \cdot p+1 \cdot p+2\,(z) \times r \cdot r+1\,(z)} \times \frac{1}{2z}$.

Having thus obtained a general expression for the whole area of any curve, whose ordinate is expressed by $(1 - x^2)^{p-1} \times x^{2r-1}$, and that expression for such area consisting of an infinite number of factors multiplied together; to render the same useful in practice, some theorems are requisite for ascertaining the limits of such products. The theorems which Mr. L. has hitherto been able to investigate suitable to that purpose, he gives in the next two articles.

3. The limit of $1 - m^2 \times 2^2 - m^2 \times 3^2 - m^2\,(z) \times \frac{N^{2z}}{z^{2z+1}}$ is $= \frac{2}{m} \times$ sine of ms; N being the number whose hyp. log. is 1, and s the semiperiphery of the circle, whose radius is 1. Whence, by taking m equal 0, we find the limit of $1^2 \cdot 2^2 \cdot 3^2\,(z) \times \frac{N^{2z}}{z^{2z+1}} = 2$s.

4. The limit of $dz + a \times dz + a + d \times dz + a + 2d \times \frac{N^z}{2^{2z} d^z z^z}$ is $= 2^{\frac{a}{d} - \frac{1}{2}}$. Hence, $z + 1 \cdot z + 2 \cdot z + 3\,(z)$ being $= 1 \cdot 3 \cdot 5\,(z) \times 2^z$, it appears, that the limit of $1 \cdot 3 \cdot 5\,(z) \times \frac{N^z}{2^z z^z}$ is $= 2^{\frac{1}{2}}$.

5. Writing A, B, C, D, and E, for the whole areas of the curves, whose ordinates are $\frac{x^{\frac{2}{3}}}{(1-x^2)^{\frac{1}{2}}}$, $\frac{x^{\frac{1}{3}}}{(1-x^2)^{\frac{1}{2}}}$, $\frac{1}{(1-x^2)^{\frac{1}{2}}}$, $\frac{x^{-\frac{1}{3}}}{(1-x^2)^{\frac{1}{2}}}$, and $\frac{x^{-\frac{2}{3}}}{(1-x^2)^{\frac{1}{2}}}$, respectively; we have, by art. 2,

A = the limit of $\frac{1.2.3\,(z) \times 4.\;7.\;10(z)}{1.3.5\,(z) \times 5.11.17\,(z)} \times \frac{2^{2z-1}}{z}$;

B = the limit of $\frac{1.2.3\,(z) \times 7.13.19\,(z)}{1.3.5\,(z) \times 2.\;5.\;8\,(z)} \times \frac{1}{2z}$;

C = the limit of $\frac{1^2.2^2.3^2\,(z)}{1^2.3^2.5^2\,(z)} \times \frac{2^{2z-1}}{z}$ = the area of the semicircle, whose radius is 1;

D = the limit of $\frac{1.2.3\,(z) \times 5.11.17\,(z)}{1.3.5\,(z) \times 1.\;4.\;7\,(z)} \times \frac{1}{2z}$;

E = the limit of $\frac{1.2.3\,(z) \times 2.\;5.\;8\,(z)}{1.3.5\,(z) \times 1.\;7.13\,(z)} \times \frac{2^{2z-1}}{z}$.

Now it appears by the above equations, that $\frac{A}{B}$ is = the limit of $\frac{2.4.\;5.\;7.\;8.10(2z)}{5.7.11.13.17.19(2z)} \times 2^{2z}$; which by art. 3, is = $\frac{6 \times \text{sine } 60^\circ}{12 \times \text{sine } 30^\circ} = \frac{3^{\frac{1}{2}}}{2}$. Therefore A is = $\frac{3^{\frac{1}{2}}B}{2}$.

It appears also, that $\frac{B \times D}{C}$ is = the limit of $\frac{5.7.11.13.17.19\,(2z)}{2.4.\;5.\;7.\;8.10\,(2z)} \times \frac{3}{2^{2z+1}}$; which by art. 3, is = $3^{\frac{1}{2}}$. Therefore D is = $\frac{3^{\frac{1}{2}}C}{B}$.

It likewise appears, that

B × E is = the limit of $\frac{1^2.2^2.3^2\,(z)}{1^2.3^2.5^2\,(z)} \times \frac{3.2^{2z-1}}{z} = 3C$. Therefore E = $\frac{3C}{B}$.

6. Writing F, G, H, I, K, for the whole areas of the curves, whose ordinates are $\frac{x^{\frac{2}{3}}}{(1-x^2)^{\frac{1}{3}}}$, $\frac{x^{\frac{1}{3}}}{(1-x^2)^{\frac{1}{3}}}$, $\frac{1}{(1-x^2)^{\frac{1}{3}}}$, $\frac{x^{-\frac{1}{3}}}{(1-x^2)^{\frac{1}{3}}}$, $\frac{x^{-\frac{2}{3}}}{(1-x^2)^{\frac{1}{3}}}$, respectively, we have, by art. 2,

F = the limit of $\frac{1.2.3\,(z) \times 9.15.21\,(z)}{2.5.8\,(z) \times 5.11.17\,(z)} \times \frac{3^z}{2z}$;

G = the limit of $\frac{1.2.3\,(z) \times 8.14.20\,(z)}{2^2.5^2.8^2\,(z)} \times \frac{3^z}{2^{z+1} \times z}$;

H = the limit of $\frac{1.2.3\,(z) \times 7.13.19\,(z)}{2.5.8\,(z) \times 1.\;3.\;5\,(z)} \times \frac{1}{2z} = B$;

I = the limit of $\frac{1^2.2^2.3^2\,(z)}{2.5.8\,(z) \times 1.4.7\,(z)} \times \frac{3^{2z}}{2z}$;

K = the limit of $\frac{1.2.3\,(z) \times 5.11.17\,(z)}{2.5.8\,(z) \times 1.\;7.13\,(z)} \times \frac{3^z}{2z}$.

By which equations it appears, that $\frac{F}{H} = \frac{F}{B}$ is = the limit of $\frac{1.3.\;3.\;5.\;5.\;7\,(2z)}{5.7.11.13.17.19\,(2z)} \times 3^{2z}$; which by art. 3, is = $\frac{4 \times \text{sine of } 90^\circ}{12 \times \text{sine of } 30^\circ} = \frac{2}{3}$. Therefore F is = $\frac{2B}{3}$.

It appears likewise, that

$\frac{G}{B}$ is = the limit of $\frac{1.3.5\,(z) \times 8.14.20\,(z)}{2.5.8\,(z) \times 7.13.19\,(z)} \times \frac{3^z}{2^z}$

= the limit of $\frac{1.3.5\,(z) \times 4.7.10\,(z)}{4.7.10\,(2z)} \times 6^z$

$$= \text{the limit of } \frac{1 . 3 . 5 (z)}{3z + 4 . 3z + 7 . 3z + 10 (z)} \times 6^z;$$

which, by art. 4, is $= 2^{-\frac{1}{3}}$. Therefore G is $= \frac{B}{\frac{1}{3}}$.

It also appears, that

I is $=$ the limit of $\frac{1^2 . 2^2 . 3^2 (z)}{2 . 3 . 5 . 7 . 8 . 10 (2z)} \times \frac{3^{2z+1}}{2}$; which, by art. 3, $=$
$\frac{2S}{4 \times \text{sine of } 60^\circ} = \frac{S}{3^{\frac{1}{2}}} = \frac{2C}{3^{\frac{1}{2}}}$.

Moreover it appears, that

$$\frac{B \times K}{I} \text{ is } = \text{the limit of } \frac{1 . 4 . 7 (z) \times 5 . 11 . 17 (z)}{2 . 5 . 8 (z) \times 1 . 3 . 5 (z)} \times \frac{1}{3^{z-1}}$$

$$= \text{the limit of } \frac{3z + 2 . 3z + 5 . 3z + 8 (z)}{1 . 3 . 5 (z)} \times \frac{1}{2^z . 3^{z-1}};$$ which, by art. 4, is
$= \frac{3}{2^{\frac{1}{3}}}$. Therefore K is $= \frac{3I}{2^{\frac{1}{3}} B} = \frac{2^{\frac{2}{3}} . 3^{\frac{1}{2}} C}{B}$.

And in like manner may a great number of other areas be compared.

Note.—All the whole areas above-mentioned are supposed to begin where x begins, and to stand on a base $= 1$.

XXVII. Experiments and Observations on a Blue Substance, found in a Peat-moss in Scotland. By Sylvester Douglas, Esq. p. 181.

The blue coloured substance, which is the subject of the following observations, was accidentally dug up in the summer 1759, to mix with some other materials for the purpose of manure, to be laid on some ground, then in Mr. D.'s possession, in the north of Scotland, about 12 miles from Aberdeen. Mr. D. had not met with a description of this substance in any naturalist. Kentman indeed in a few lines mentions a blue earth, which he calls cœruleum patavinum, which agrees with this substance in one remarkable circumstance; that it is at first of a white colour, and becomes blue only by being exposed to the air. Mr. Da Costa's ochra friabilis cœrulea, Nat. Hist. of Foss. p. 103, would also have been found probably to correspond with it, if a more particular account could have been given of the circumstances in which it was found, and its appearance before the air had acted on it. Mr. Cronstedt, in his late System of Mineralogy, mentions a blue substance, which seems to be of the same kind, and which, he says, is found somewhere in Prussia.

The place, where it is dug up, is of a marshy nature, in the corner of an exhausted peat-moss. Immediately under the sward lies a stratum, about a foot deep, of common peat; next to that is the substance itself, with irregular striæ of a peaty matter all through it, to the depth of near another foot; and below this, he thinks, there is clay. While it is thus wet, and shut out from the air, it is of a white colour, and seemingly of a fatty consistence, not unlike lime that has been prepared for cement. All the water in the neighbourhood of the place is in some measure impregnated with iron. When this substance is exposed to the air, it gradually as it dries assumes the blue colour; the peaty matter intermixed with it continuing of the same appearance as before. The whole mixed mass is of a very friable texture, easily crumbling between the fingers; and the blue part, gently rubbed between them, feels like a fine impalpable powder. It has hardly any sensible taste; what it has, approaches a little to that of sulphur: the smell, when it is first taken up, is sensibly sulphureous, and if a piece of paper, with part of it adhering to it, be kindled, it shows a flame similar to burning sulphur.

The only means of separating it from the black matter is by elutriation. When water is poured on it, and they are shaken together, and then left at rest for some time, the black part subsides to the bottom, and the blue can be poured off still diffused in the water, from which however it soon separates, and falls to the bottom. It is not possible entirely to free the blue from the peaty matter, for, after above 20 different additions of water, there were still streaks of black interspersed through it, when it was allowed to subside; neither had he ever been able to separate all the blue from any of the black part. When a little water is added to a quantity of it, it acquires some degree of tenacity, and when a small portion of water is allowed to stand on its surface for a day or two, the water becomes of a yellowish colour.

In order to find whether there was any part of it soluble in water, he passed a large quantity of water, which he had used in separating the black from it, through a filter, and then set it to evaporate in B. M; but there was nothing left in the vessel after the evaporation, except some earth, which the water had probably contained in itself. To a quantity of the blue powder, he added the common vitriolic acid of the shops; a degree of effervescence ensued; and a considerable froth remained for some time on the surface; the whole was changed into a dark brown colour, and when filtrated, the solution was a transparent brown liquor. A considerable sediment remained behind on the filter; but he was inclined to think, that this consisted chiefly of the peaty matter, which had not been entirely separated; for when the experiment was repeated several times with different parcels of the blue, it appeared more or less soluble according as the black had been more or less perfectly separated; and when he added the vitriolic acid to a quantity of the black, though it turned it all of a brown colour, it only seemed to dissolve a quantity equal to the portion of blue, which still adhered to it. The nitrous acid, added to the blue powder, produced pretty much the same effects, only the filtrated solution was of a much lighter brown.

The fixed vegetable alkali dissolved also a considerable part of it; but whether the whole or not, he could not say. The solution was an opaque brown liquor, which did not become transparent after being twice filtrated, though it deposited no sediment upon standing several days. He added a small quantity of volatile alkali to it, which seemed to dissolve part of it, and turned the rest obscurely green. To the solution in vitriolic acid, he joined some fixed vegetable alkali: an effervescence arose, and a light curd of a colour between green and blue was thrown to the top, which soon subsided, and became white. A similar precipitate was obtained from the nitrous acid, only it was not at first thrown up to the surface in the same manner as the foregoing.

From the solution in fixed vegetable alkali, a reddish brown precipitate was obtained, by the addition of vitriolic acid. Equal quantities of the blue powder and of black flux were mixed together, and being put into a crucible, were kept in a a strong degree of heat for several hours: on being removed, and taken out of the crucible, the whole was found concreted into a spongy mass, the bottom of which was crusted over with something that had a kind of metallic appearance. This mass was powdered, and the lighter parts washed off; after which a magnet was applied to what remained, and it attracted many of its particles strongly, without being brought in contact with them.

Part of the white precipitate from vitriolic acid was mixed with a little fixed alkali, and being laid on a piece of charcoal, the flame of a candle was directed to it by means of a blow-pipe. It was thus kept in a red heat for about an hour, and on being removed, the magnet was applied to it, but none of the powder was attracted by it. The quantity that can be examined in this way does not exceed a few grains.

To a small quantity of the white precipitate, he added an infusion of tea; which turned it blue, approaching to the original colour, but not so deep. To another parcel of the same, he added some infusion of galls, and shook them together. The liquor became of a dark blue colour, and what part of the powder remained at the bottom of the glass was of the same colour. This was not so bright as that of the original powder diffused in water, but entirely such as might be expected from the diffusion of it in a brown liquor like infusion of galls; and, to show this, he poured some of the infusion of that astringent on the blue substance itself, and on shaking them together, they produced a colour almost entirely the same. A quantity of the brown solution in vitriolic acid was diluted with water till it became very pale. He then poured to it some infusion of galls, which turned it immediately black. A parcel of the blue substance, being placed at the distance of a foot from the fire, was changed to a greenish colour.

These experiments, compared with its natural history, seem to throw some light on the nature and composition of this curious production. It is the known property of all vegetable astringents, to affect the colour of iron, either when it is combined with vitriolic acid, in the form of green vitriol, or by itself; and he believed they had no such effect on any other metal. The colour they produce with it is various, inclining indeed to black, but almost of every different shade between black and blue; and it seems, that they occasion a more pure black with vitriol, and a purple blue with iron itself, as is seen for instance on dropping a little infusion of tea on a knife.

Now we find, that when a vegetable astringent is added to a solution of this substance in vitriolic acid, it strikes a black colour with it, and restores the original blue to the white precipitate from that acid. We also find, that there actually is iron contained in it; because, when fluxed with the black flux, its particles are attracted by the loadstone; and we can draw no argument from our not having discovered iron in the experiment with the blow-pipe, against the presence of that metal, as so little can be examined in that way. He therefore thinks it probable, that the principal ingredients of it, and those on which its colour depends, are iron and some vegetable astringent. The situation in which it is found favours this conjecture very strongly; for, in the first place, the water in the neighbourhood of it is all impregnated with iron; and secondly, in almost every peat-moss, there are the remains of oak trees, still fresh dispersed through them; and both their wood and bark are of a strong astringent nature.

He did not pretend to say, that these are its only ingredients. He thinks we may conclude, from the lightness of the substance, that iron does not form a very great part of it; and the smell, and the particular flame it exhibits in burning, would seem to show the presence of sulphur in it. This, however, can be only in a very small proportion, since so much of it is soluble in acids, which do not at all affect sulphur. He supposes the precipitate from acids consists chiefly of iron and earth.

He had made some trials on the blue powder after it was partly well freed from the black matter, in order to see how far it might be useful as a paint: a quantity of it was rubbed in a glass mortar with oil of walnuts; but, after being thoroughly mixed with the oil, its colour was changed to black. It is probable, therefore, that little can be expected from it as an oil colour; but it retains its natural brightness when mixed with gum-water; and, as it is naturally in a very fine powder, it is diffused intimately through it without any difficulty, so that, if it could be got in sufficient quantity, it would be a cheap and useful water-colour. He thinks there is reason to believe, that it might be found in most peat-mosses, as what seem to be materials of which it is composed are present almost in all of them. Two or 3 years ago a gentleman sent him a parcel of it, which he found in a moss on his estate, 5 or 6 miles distant from the place where he first observed it. He was informed, that Mr. Da Costa had specimens of a blue earth sent him from different parts of England: what Sir Hans Sloane gave him from Ireland seems also to have been the same, and from what he had quoted from Kentman and Cronstedt, it would appear, that it is obtained on several parts of the continent. From all this, he thinks we may conclude, that it might be procured in sufficient quantity to be a cheap paint; particularly as it is in a manner levigated and prepared by nature. It is to be lamented, that its colour is so easily affected by alkalies, especially the volatile alkali, which abounds so much in the atmosphere in towns, and by any considerable degree of heat. He had, however, never found any change produced on it from being exposed for a considerable time to the air, or to the heat of a room where a fire was kept constantly burning.

XXVIII. Two Medical Observations by Dr. Joseph Benevuti, Physician at Lucca. p. 189.

1. *Of a sick man surprizingly recovered from a fever.*—A man 40 years of age, of a plethoric constitution, and of a low size, having a malignant fever, became on the 9th day delirious, and continued so during the 10th night: when, several bad symptoms appearing, it was thought he must die soon. Early on the 11th day in the morning, he bid the by-standers quit his room, and expressed a desire of going to sleep; his friends were unwilling to withdraw, unless they first stripped him of his shirt and dried him of the sweat he was in. But the patient refusing, and at last being angry, they were obliged to yield to his will. About an hour after, a woman went into the bed-room, and not finding the man, she called the servants, who searched the house, and the well, into which they feared he had thrown himself; but to no purpose. The keeper of the baths at Lucca gave orders for every body to make a diligent search; and on the 3d day the sick man was at last found in a vineyard, about 2 miles from his house, hidden in a hut, where, he said, that the day before, he with great astonishment found himself, without at all knowing how he came there. It seemed to Dr. B. that he must have got down by the window of the bed-chamber, which was not far from the ground. What seems most extraordinary is, that, in order to quench his thirst, this man swallowed a large quantity of snow, with which the earth was covered, it being in the winter; and that neither this sort of drink, nor the cold air, did in the least affect him; for though he had gone away from home all in a sweat, and with no other covering than his shirt, yet he was freed from his fever, and was restored to his former health.

2. *Of an extraordinary great head.*—Not long before, Dr. B. went to Benabii, a town situated in the territory of Lucca, to see a man, whose head, he had heard, was much larger than usual. Dr. B. saw the man, 30 years of age, and yet of the size of a boy 7 years old, who was sitting on a couch seat, with his

head, which indeed was quite out of size, inclined on the right side and resting on a pillow; which, when he wanted to move, he supported with his hands, as it lay on a very small neck. This man had enjoyed good health till he was 6 years old; he then had a diarrhœa, which lasted 9 months, and, on its stopping, his lower extremities were seized with the palsy, and lost their motion, but their feeling remained. From that time his head increased yearly, together with his face, nose, ears, eyes, mouth, &c. but the remainder of his body did not grow at all. The circumference of his scalp measured 37 inches, and 8 lines, English measure. The length of his face was 12 inches and 3 lines. He eat greedily, slept well, but discharged his fæces and his urine involuntarily. The strength which he had in his hands was very surprizing, being such, that it was difficult for any person to get loose from him, when he held fast. He was besides quick as to his understanding; he talked and had a good memory; seldom or never forgetting what he had read.

XXIX. Of a particular Species of Cameleon. By James Parsons, M. D., F. R. S. p. 192.

Among the quadrupeds of the earth, the class of cameleons is one of the most curious families; insomuch as to have engaged the attention of many natural historians; not only on account of the particular structure of its parts, but also of several curious phenomena which are peculiar to it, in its several species, in the different parts of the world. This animal is ranged by authors under the generical name lacerta, which comprehends a great variety of all sizes, from the crocodile to the smallest lizard: but as the cameleon has its various species, and each such properties as are not common to any others under the tribe of lacertæ, they indeed deserve to be regarded as a particular genus. However, since authors have been very full in their accounts of these creatures; which every one, curious in their inquiries into the history of animals, may have recourse to, collected in an excellent work entitled, Dictionnaire raisonné des Animaux, Dr. P. only gives a description of a species of cameleon which he considered as a nondescript, having made a careful research concerning this animal among authors, and seen several kinds of them, as well as various figures in every history he was acquainted with; from all which the subject before us is very different.

It is chiefly in the structure of the head that this difference appears; for the head is very large in proportion to the rest of this animal, and all others of the same class; and the more so, if we measure from the two anterior flat processes, to the posterior extremity or process of the cranium, which measures 3 inches and a quarter. This posterior process extends backwards, over the neck, to the first vertical process of the spine; and the interior processes, one on each side, project forwards and upwards in an oblique direction over the nasal hole, and are bluntly serrated all round; the surface of the entire face is covered with tubercles and scales, which, by being in a dry state, have lost their protuberance and lustre, which the scales certainly were endowed with while the animal was alive.

The length of the two mandibles is equal, and is 2 inches and a quarter from the articulation of the lower with the upper jaw, to the apex of each; both being furnished with a fine set of small pointed teeth; all of a size, and so set, that on the animal's closing his mouth, the teeth do not meet, but those of the upper fall in with those of the under alternately. There are no molares nor canine teeth.

The orbits are extremely large and deep, so that this cameleon must have had very great eyes, and very globular; for they are each more than a third of the whole length of the mandible in diameter. From a close inspection of the skin, which is now contracted and dried close to the skeleton, it appears scaled all over; the larger scales are on part of the head and on the sides of the neck; the smaller, under the jaws, on the neck, and over the whole body; but we can form no idea of its proper colour while the animal is alive, yet do not doubt of its having had a very beautiful covering.

Almost every species of lacerta have 5 fingers on each extremity; all the cameleons have them, but they differ in the disposition of the fingers; this before us has the tarsal, metatarsal, and 3 bones to each finger, as it is in human hands: in this cameleon the fingers are very long, and terminated with pointed nails bending downwards; 3 of the fingers of each anterior extremity are inwards in the place of the thumb, and the other two are outwards; whereas in the posterior extremities, 3 are outwards, and the other two inwards, having between them such a large space or division, as is between the thumb and fingers of men. But this distribution of the fingers he saw in one of the triangular-headed cameleons: other species have the 5 fingers together, and very short like stumps; but that described by Pitfield, from the dissections of the Royal Academy, has its fingers disposed in the same manner with this, and is one of those with a triangular head and crest.

The vertical edge of the spine is scolloped all along from the neck to the extremity of the tail, and has on each side a row of knobs, or processes, as far as the articulation of the thigh with the bone that runs up towards the spine; but from thence, where the tail begins, there is a second lateral row of knobs, which continue all along the tail. There does not appear any passage into the head for hearing, nor any other but the mouth and nasal holes; which is also taken notice of by the Royal Academy in their observations on that mentioned above. This made Bellonius imagine, that these nasal holes serve cameleons for hearing as well as breathing; so that it should seem, that more species than one are destitute of auditory holes.

XXX. Astronomical Observations, made in several Parts of the Kingdom of Naples and Sicily. By J. A. Rizzi Zannoni, Member of the Acad. of Sciences at Gottingen, and Geographer to his Sicilian Majesty. From the French. p. 196.

The transit of Mercury over the sun, in the year 1743, was observed at Naples, by Father P. N. with a telescope of 18 palms, made by Campani. He determined the first contact at $1^h\ 57^m\ 25^s$, and the second at $2^h\ 0^m\ 35^s$. The transit of the same planet in 1753 was observed at Naples, in the Royal College, by Father Carcani, with a telescope of $18\frac{1}{4}$ palms. First contact at $23^h\ 5^m\ 51^s$; emersion of centre, $23^h\ 7^m\ 28^s$; second contact at $23^h\ 9^m\ 5^s$.

The transit of Venus over the sun, of the year 1761, was observed at Naples, at the same place, by the same astronomer, with an excellent telescope of 24 palms. The first contact at $21^h\ 16^m\ 55^s$; the second contact, $21^h\ 35^m\ 20^s$.

The same transit was likewise observed at Malta by several people. A serjeant in the marines, who is an excellent pilot, has posted himself at Valetta, and has an excellent clock, and a Newtonian reflector of 3 palms. He observed the beginning of the emersion at $21^h\ 17^m\ 50^s$, and the total emersion at $21^h\ 36^m\ 33^s$. Finally, at Tarentum, the latitude of which place is the same with that of Naples, Mr. Wm. Felton observed the transit of Mercury over the sun of 1753, with a very good reflector of 2 feet. The first contact at $11^h\ 18^m\ 26^s$; the second contact, $11^h\ 21^m\ 36^s$.

XXXI. Some Experiments, by Mr. Miller of Cambridge, on the Sowing of Wheat. By W. Watson, M.D., F.R.S. p. 203.

Mr. Charles Miller is a very ingenious person, and an excellent naturalist. He is the son of our worthy brother Mr. Philip Miller, from whose knowledge of, and publications in, botany, agriculture, and gardening, the public has received very great information and advantage. In consequence of Dr. W.'s desire Mr. Charles Miller informed him, that having made, in the autumn of 1765, and in the spring of 1766, an experiment of the division and transplantation of wheat, by which near 2000 ears were produced from a single grain; and he having reason to think, from the success attending this experiment, that a much greater quantity might be produced, he determined to repeat the experiment next year. Accordingly, on the 2d of June, 1766, he sowed some grains of the common red wheat; and on the 8th of August, which was as soon as the plants were strong enough to admit of a division, a single plant was taken up, and was separated into 18 parts. Each of these parts was planted again separately. These plants having pushed out several side shoots about the middle of September, some of them were then taken up, and divided: and the rest of them between that time and the middle of October. This 2d division produced 67 plants. These plants remained through the winter; and another division of them, made between the middle of March and the 12th of April, produced 500 plants. They were then divided no further, but permitted to remain.

The plants were in general stronger than any of the wheat in the fields. Some of them produced upwards of 100 ears from a single root. Many of the ears measured 7 inches in length, and contained between 60 and 70 grains. The whole number of ears, which by the process before mentioned were produced from one grain of wheat, was 21109, which yielded 3 pecks and 3 quarters of clear corn; the weight of which was 47 lb. 7 oz.; and, from a calculation made by counting the number of grains in one ounce, the whole number of grains might be about 576,840.

By this account we find, that there was only one general division of the plants made in the spring. Had a second been made, the number of plants, Mr. Miller thinks, would have amounted at least to 2000, instead of 500; and the produce have been much enlarged. For he found by the experiment made the preceding year, in which the plants were divided twice in the spring, that they were not weakened by the 2d division. He mentions this to show that the experiment was not pushed to the utmost. The ground, in which this experiment was made, is a light blackish soil, on a gravelly bottom, and consequently a bad soil for wheat. One half of the ground was very much dunged; the other half was not prepared with dung, or any other manure: no difference was however discoverable in the vigour or growth of the plants, nor was there any in their produce.

XXXII. On the Theory of Circulating Decimal Fractions. By John Robertson, Lib. R.S. p. 207.

Regiomontanus, it is said, first among Europeans, added to the then known arithmetic, an operation by decimal fractions; which he exemplified in his triangular table. Its utility was readily seen, and embraced in many nations, and particularly in this; where it appears to have been cultivated in its theory, and facile modes of operation, more than in other places. Mr. R. here adverts to several particulars in the theory of these numbers, but several later publications, on a more extensive scale, have rendered this paper of little or no use at present.

XXXIII. A Letter from Mr. J. R. Forster, F.A.S., containing some Account of a new Map of the River Wolga. Dated Warrington, Oct. 24, 1768. p. 214.

This letter states that the map of the river Wolga, which Mr. Forster sent to the R. S. was constructed from measurements chiefly taken by himself, when he was formerly in that country. He says he first presented it to the Academy of Sciences at Petersburg, where it was much admired; but yet no use was made of it.

XXXIV. An Account of the Lymphatic System in Birds. By Mr. Wm. Hewson,† Reader in Anatomy.* p. 217.

Having been so fortunate, in a series of experiments made with that view, as to trace out the lymphatic system in birds, Mr. H. ventured to offer the following account of it, to be presented by Dr. Hunter to the R.S.; and he flattered himself this discovery would be considered as some acquisition to physiology.

The lymphatic system has been supposed to be wanting in birds; and absorption in that kind of animals to be carried on by branches of the common veins. Physiologists were led into this opinion by observing, that though the lacteals and mesenteric glands were easily seen even in the smallest quadruped, yet the

* The chief part of this paper is inserted in the 2d vol. of Mr. Hewson's collected works, but is there unaccompanied by the plate: on this account it is reprinted in this Abridgment.

† This celebrated demonstrator of anatomy (as we are informed in a letter addressed by his widow to Dr. Simmons) was born at Hexham in Northumberland in 1739. He received the rudiments of his education at a grammar-school in his native town, where his father practised as a surgeon and apothecary. With him Mr. H. acquired his first medical knowledge; being ambitious to increase that knowledge, he placed himself first under an eminent surgeon (Mr. Lambert) in Newcastle, and afterwards resided some time at London, Edinburgh, and Paris, then settled in London, and entered into a partnership, as dissector and lecturer in anatomy, with the late Dr. Hunter, with whom his connexion continued until 1770, when a disagreement arose, and a separation was the consequence. On this occasion Mr. Hewson's place was supplied by Mr. Cruikshank.

Mr. H. died in 1774. His father having had a numerous family, and himself having married a lady whose recommendations were a good understanding and amiable disposition, without wealth, he owed his advancement in life wholly to his own industry. A better son and husband (such are the words of his widow), or a fonder father never existed. He was honoured with the friendship of many respectable scientific characters; and the late Sir John Pringle showed him particular marks of regard. From the same authority we learn, that his manners were gentle and engaging; that his ambition was free from ostentation; that his prudence was without meanness; and that he was more covetous of fame than of fortune.

Mr. Hewson's name will be associated with the names of the first anatomists and physiologists, of which this country has to boast. His papers on the lymphatics in birds, fishes, and amphibious animals, as well as those on the blood, were first published in the Phil. Trans., and have since been reprinted in a collected form, amounting to 3 vols. 8vo, under the title of Experimental Inquiries. The Appendix to the 1st vol. relates to the dispute between him and Professor Monro of Edinburgh, concerning the priority of discovery of the lymphatics in birds and certain amphibious animals.

most acute anatomists had not been able to find in any bird the least appearance either of those vessels or glands. The difficulty of discovering the lacteals in birds was no doubt principally owing to the transparency, or want of colour, in the fluid which they contain. In quadrupeds the lacteals are easily found, as they are filled with chyle, which is mostly opaque and white; whereas in birds the chyle is as pellucid and colourless as the vessels themselves. The want of mesenteric glands was another cause of our remaining so long ignorant of those vessels.

This system may be divided in birds, as it is in quadrupeds, into the branches, viz. the lacteals and lymphatics, and their trunk, or thoracic duct. The lacteals indeed, in the strictest sense, are in birds the lymphatics of the intestines, and like the other lymphatics carry a transparent lymph. And instead of one thoracic duct there are two, of which one goes to each jugular vein. In these circumstances it would seem that birds differ from quadrupeds, so far at least as he could judge from the dissection of a goose, which was the bird he chose as most proper for this inquiry.

So much being premised, he next gives a description of what he had seen of those vessels in this fowl; and to illustrate the description he adds a figure from the same subject, in which those vessels were filled with quicksilver. The lacteals run from the intestines on the mesenteric vessels. Those of the duodenum (a) * pass by the side of the pancreas (a), † and probably receive its lymphatics: afterwards they get upon the cœliac artery, of which the superior mesenteric is a branch. While they are upon this artery they are joined by the lymphatics from the liver (b): here they form a plexus, which surrounds the cœliac artery (cc); at this part they receive a lymphatic from the gizzard (d); and a little farther, another from the lower or glandular part of the œsophagus (e). Having now got to the root of the cœliac artery, they are joined by the lymphatics from the renal capsulæ; and near the same part, by the lacteals from the other small intestines, which vessels accompany the lower mesenteric artery. These last-mentioned lacteals, before they join those from the duodenum, receive from the rectum a lymphatic, which runs with the blood-vessels of that gut. Into this lymphatic some small branches from the kidneys seem to enter, which coming from those glands upon the mesentery of the rectum, at last open into its lymphatics. At the root of the cœliac artery, the lymphatics of the lower extremities probably join those from the intestines. The former he had not yet traced to their termination, though he had distinctly seen them on the blood-vessels of the thigh; and in one subject, which he injected, some vessels were filled, contrary to the course of the lymph, from the network near the root of

* See pl. 16 in the outlines. fig. 1.—Orig. † See the same plate, fig. 2.—Orig.

the cœliac artery; these vessels ran behind the cava, and down upon the aorta, near to the origin of the crural arteries, and he presumes they were the trunks of those branches which he had seen in the thigh. At the root of the cœliac artery, and on the contiguous part of the aorta, a network (f f) is formed by the lacteals and lymphatics above described. This network consists of 3 or 4 transverse branches, which make a communication between those which are lateral. In the subject from which his drawing was made, there were 4. From this network arise the 2 thoracic ducts (gg); of which one lies on each side of the spine, and runs upon the lungs obliquely up towards the jugular vein, into which it opens (l and n); not indeed into the angle between the jugular and subclavian, as in the human subject, but into the inside of the jugular vein, nearly opposite to that angle. The thoracic duct of the left side is joined by a large lymphatic (h), which runs upon the œsophagus, and can be traced as far as the lower or glandular part of that canal; from which part, or from the gizzard, it seems to issue. The thoracic ducts are joined by the lymphatics of the neck (and probably by those of the wings) just where they open into the jugular veins.

The lymphatics of the neck * generally consist of 2 pretty large branches, on each side of the neck, accompanying the blood-vessels. Those 2 branches join near the lower part of the neck; and the trunk is in general as small, if not smaller, than either of the branches. This trunk runs close to the jugular vein (ii), gets on its inside, and then opens into a lymphatic gland (kk). From the opposite side of this gland, a lymphatic comes out, which pours the lymph into the jugular vein. On the left side, the whole of this lymphatic joins the thoracic duct of the same side (l); but on the right, one part of it goes into the inside of the jugular vein a little above the angle (m), while another joins the thoracic duct, and with that duct forms a common trunk, which opens into the inside of the jugular vein, a little below the angle which that vein makes with the subclavian (n).

To this description it may be necessary to add, that though it may be taken from one subject, yet in 3 others of the same species which he examined carefully, he saw nothing which disagreed with it. He particularly attended to the number of the thoracic ducts, suspecting, that possibly in this subject, the 2 that he had seen might be only a variety, which is a circumstance that, as we are told, has occurred even in the human body. But in 3 others of this species, which he likewise successfully injected, he still saw 2 ducts; and therefore he was inclined to believe, that this is the constant number. He likewise carefully attended to the vessels coming from the gland on the right side: and in the only 2 subjects in which the lymphatics of the neck were properly filled, he observed

* It is but doing justice to the ingenious Mr. John Hunter, to mention here, that these lymphatics in the necks of fowls were first discovered by him many years ago.—Orig.

that one part of it opened immediately into the vein, and the other joined the thoracic duct of that side; while on the left side, the vessel which issued from the gland wholly joined the thoracic duct. In all the 4 subjects he evidently saw that the thoracic ducts open into the inside of the jugular veins.

This system in birds differs most from that in quadrupeds in the following particulars. 1st. In the chyle being transparent and colourless. 2dly. In there being no visible lymphatic glands, neither in the course of the lacteals, nor in that of the lymphatics of the abdomen, nor near the thoracic ducts. 3dly. In the several parts of this system in birds being more frequently enlarged, or varicose, than in quadrupeds. In particular, this appears to be the case of the vessels which constitute the network at the root of the cœliac artery in that subject from which the drawing was taken. The lacteals are frequently enlarged in some places; so are the thoracic ducts; and the lymphatics on each side of the neck are commonly, when taken together, larger than their trunk which opens into the lymphatic gland. In one subject, where instead of 2 lymphatics on the left side, he found only one, that vessel was as large as a crow quill; while the lower part of it, which entered the gland, was much smaller. Thus far the account of what he saw: he next begs leave to observe, that, as the supposed want of this system in birds has been considered as a strong argument in favour of absorption by the common veins, now, since we find it not wanting, that theory must be much weakened. And he further adds, that absorption seems to be carried on in birds, as in quadrupeds, by this system, at least principally; indeed he was inclined to believe, entirely; for no arguments brought in favour of absorption by the common veins appeared to him of equal validity with those that can be urged against it. The contrary opinion is indeed embraced by the most learned and acute physiologist of the present age, who, treating of this subject, expresses himself in the following manner: "it is a strong argument in favour of absorption by the common veins, that neither birds, amphibious animals, nor fish with cold blood, have either the lacteal or the lymphatic system. Nature commonly observes a pretty strict analogy in her works, and makes use of similar organs to perform similar functions. Now in all animals, quadrupeds and the whale excepted, we must admit of absorption by the mesenteric veins, if in those animals there is no other way for the chyle to get into the blood. And if those veins in birds and amphibious animals absorb the chyle, it is very probable they likewise absorb it in quadrupeds, in which they equally exist." But the existence of this system in birds is not the only fact which might be adduced to invalidate the above opinion; for he had seen a part of it very distinctly in one of the amphibia, viz. the turtle.* Whether it is to

* The part of this system, which he saw in the turtle, was the lacteals. He filled them with quicksilver as far as the root of the mesentery, where they formed a considerable net-work into which a lymphatic of the spleen entered. He had not an opportunity of tracing them farther, having

be found in fish, he had not yet determined. Since he saw it in birds and in the turtle, he had made indeed some inquiries after it in fish, but hitherto without success. Yet, that they are not without such vessels, he thinks, is probable, from considering that the lymphatics are so general, as to be found in quadrupeds, birds, and amphibious animals. And from the consideration of the extensiveness of this system through so many classes of animals, he was inclined to think that opinion most probable, which Dr. Hunter advanced some time before, when he printed his discovery on the use of those vessels, viz. 'That the lymphatics are the only absorbents.' See his Commentaries, ch. 5.

For the sake of those who may incline to prosecute this inquiry farther, he next relates the method by which these vessels may be demonstrated; and that is, having chosen a young and very lean goose, and fixed it on a table, let the abdomen be opened while it is yet alive, and a ligature be passed round its mesenteric vessels, as near the root of the mesentery as possible. The lacteals will begin to appear near the ligature in a few minutes after it is made, especially if the bird has been well fed 3 or 4 hours before the experiment. The lymphatics in the neck may be shown in the same manner; that is, by making a ligature on the jugular vein at the lower part of the neck; and to be more certain of including the lymphatics, which are near it, we must take care not to pass the needle too close to that vessel. When they are to be injected, they must be opened at a convenient part, and a proper pipe fixed in them for that purpose.

For the greater satisfaction of those who might think this paper worthy their attention, he had prepared 2 birds, whose lymphatic systems were filled with quicksilver, in order to be compared with the figure: these had been shown to several members of the R.S., who honoured him with their presence while the subjects were fresh; and who, he flattered himself, were then satisfied with the exactness of the drawing.†

Explanation of Figs. 1 *and* 2, *Pl.* 16.

N.B. The small letters refer to the outlines, fig. 1, and the capital letters in general refer to the figure, fig. 2, except where the contrary is specified.

A the neck; BB the clavicle divided near its middle; C the left subclavian artery; DD the jugular

---

taken the mesentery out of the animal before he had thought of looking for these vessels, as he was not at that time intent on this inquiry. The lacteals in that animal agreed with those in the bird above described, in not having any mesenteric glands. From this circumstance, and from another observation which he made, he was inclined to believe, that the whole system in this animal will be found to agree pretty exactly with that of birds. These vessels he observed so long ago as in the winter 1763—64.—Orig.

† Mr. Hewson begs leave to add, that since the above paper on the lymphatic system in birds was put into the hands of the secretary of the R.S., he had discovered the same system in fish; and had likewise been so fortunate as to procure a turtle, whose lymphatic system he had traced out, and had got delineated. An account of those dissections, with the figures, he intended to lay before the Society.—Orig.

veins.—See the outlines; EE the pulmonary arteries; FF the two branches of the trachea; GG the lungs; H the aorta—in the outlines; I the cœliac artery—in the same; L the œsophagus, turned to a side; MM the renal capsulæ, or glandulæ renales—in the outlines; N a small part of the liver fixed to a rib by a thread—in the outlines; OOO intestines; P the duodenum; Q the pancreas fixed to a rib by a thread; R the gizzard. a the lacteals which come from the duodenum; b the lymphatics of the liver; cc a plexus formed by the above-mentioned lacteals and lymphatics, which surrounds the cœliac artery. d a lymphatic from the gizzard; e a lymphatic from the lower part of the œsophagus; ff a network formed by the lymphatics upon the aorta; gg the two thoracic ducts; ii the trunks of the lymphatics of the neck; kk the glands through which the lymphatic vessels of the neck pass. That of the left side is oblong, and could not well be represented in a figure; l the thoracic duct of the left side, and the lymphatic vessel of the neck, opening together into the inside of the jugular vein; m a part of the lymphatic vessel of the right side of the neck, opening into the jugular vein; n the thoracic duct of the right side, joined by a part of the lymphatic vessel of the neck, and then opening into the inside of the jugular vein.

XXXV. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1767, pursuant to the Direction of Sir Hans Sloane, Bart. By Wm. Hudson. F. R. S. p. 470.

This is the 46th presentation of this kind, completing to the number of 2300 different plants.

XXXVI. Interpretation of the Inscription on a Punic Coin, struck in the Isle of Gozo, near Malta, never hitherto explained. By the Rev. John Swinton, B. D., F. R. S. p. 235.

On one side of this coin is the head of a woman veiled, and on the reverse that of a sheep, under which stands a word formed of 3 Punic characters. The piece itself is well enough preserved, and the letters in particular have almost entirely escaped the injuries of time. The first of these characters is taken for Koph. The 2d strongly resembles a form of the Punic or Phœnician Lamed, the other being indubitably one of Nun. Admit this, and the word may be read KAVLIN, or CAVLIN; though the Jod, after the Punic and Phœnician manner, is here suppressed, such a suppression among the Phœnicians and Carthaginians being by no means uncommon. We shall, perhaps, not find it so difficult to point out the place where all these medals were struck.

There is a small island in the Mediterranean only 5 miles from Malta, denominated anciently ΓΑΥΛΟΣ, or GAVLOS, both by the Greeks and the Romans; as we learn from Diodorus Siculus, Mela, and Pliny. This island, which is about 3 miles in circumference, was occupied by the Phœnicians in very early times, and afterwards by the Greeks. When the latter were possessed of it, the capital, named also GAVLOS, was one of those cities called by the Greeks ΑΥΤΟΝΟΜΟΙ, governed by its own laws, and consequently, as it should seem, a kind of free independent state. The Carthaginians therefore, who probably succeeded the Greeks in the occupation of this island, may reasonably be presumed likewise to have coined money in that capital, with a Punic inscription on it. Nor can this well be denied, as medals of Cosyra, or Cossura, denominated by the moderns Pantallaria, adorned with such an inscription, sometimes occur; though that island, whatever figure it might have made when possessed by the Carthaginians, was considerably smaller than GAVLOS, known at present by the name of Gozo, the place probably where the piece was struck, though the time when cannot now be ascertained.

XXXVII. Elucidation of an Etruscan Coin of Pæstum, in Lucania, emitted from the Mint there, about the Time of the Social War. By the Rev. John Swinton, B. D., F. R. S. p. 246.

This coin, adorned with Etruscan characters, was some years since communicated to the world by Sig. Passeri; and ascribed to the city of Pæstum, in Lucania, of which such noble ruins are still extant, by that ingenious author. This notion, on farther examination, will be found by no means remote from truth. The head, with curled hair, which is on one side of this curious minute coin, may not improbably, as Sig. Passeri believes, be the effigies of some famous hero, or general, if not the founder of a city, that anciently bore a relation to the place where the piece was struck; or it may possibly, as the same learned gentleman also suggests, be allowed to point out to us some local deity. Two of the symbols on the reverse undoubtedly represent a dolphin and an acrostolium, though what that between these two was intended to point out to us, Mr. S. cannot, with the same facility, take upon him to decide. Hence, and from the elements on the reverse, Mr. S. reads it ZIVVTZIS, PHISTVVIS; which, according to the cacography, or uncouth manner of writing, of the Etruscans, may, with sufficient propriety, be deemed equivalent to the Latin PÆSTVM.

XXXVIII. Remarks on a Denarius of the Veturian Family, with an Etruscan Inscription on the Reverse, never before published. By the Rev. John Swinton, B. D., F. R. S. p. 253.

This piece is a Samnite-Etruscan denarius, brought some years since by a gentleman of this university from Rome, resembling one of the Veturian family, formerly explained by Mr. S. in every particular but the legend or inscription, on the reverse, and the letter in the exergue; the elements in which he considers as equivalent to the Romans LVPO, and part of the name LVPONIUS.

The Lucanian forces acted under the orders of M. Lamponius and Tiberius Cleptius, two generals of very considerable note, in the social war. The former of these, according to Appian, distinguished himself by the defeat of a body of Roman troops, under the command of Licinius Crassus, and the siege of Grumentum, in Lucania, either in the 1st or 2d campaign of that war. As therefore, by such a blow, he must have rendered no small service to the common cause, it is natural to suppose, that the allies did him the honour of impressing his name on some of their coins. And that this was really the case, we may fairly presume, as Papius Mutilus and Tiberius Veturius, 2 other Italian generals, his cotemporaries, were actually treated with the same mark of distinction, at the same time, for their laudable conduct in the social war. This we learn from several ancient medals, adorned with the names of those commanders, that have been heretofore explained. Mr. S. would therefore convert the M. LAMPONIVS of the printed copies of Appian into M. or NI. LVPONIVS, the prenomen and name pointed out to us by this coin.

XXXIX. Description of a Punic Coin appertaining to the Isle of Gozo, hitherto attributed to that of Malta, by the Learned. By the Rev. John Swinton, B. D., F. R. S. p. 261.

On one side of this piece, is the head of a woman veiled, and on the other 3 Egyptian figures, according to the Marquis Scipio Maffei. It is observable that this medal, as well as that communicated to the learned world by the last mentioned author, exhibits a sort of wings fixed on the hips of the two exterior figures, though nothing like such wings is visible on the similar medal published by Sig. Abate Venuti. That this medal was at first adorned with a short Punic inscription on the reverse, formed of the letters koph, lamed, and nun, and consequently struck in the isle of GAVLOS, or Gozo, near Malta, is plainly deducible from the draughts of it published by the Marquis Scipio Maffei and Sig. Abate Venuti. It may not be improper to remark, that the piece now considering will bring a fresh accession of strength to what has been advanced in a former paper, relative to this species of coins, as well as to the Punic or Phœnician name of the people anciently inhabiting the isle of Gozo.

XL. Observations on an Inedited Coin, adorned with two Punic Characters on the Reverse. By the Rev. John Swinton, B. D., F. R. S. p. 265.

This is a small brass medal, exhibiting on one side the head of a woman, decked with ears of corn; and on the other a horse standing still, and looking behind him, or towards his tail, one of the usual symbols on the reverses of such Punic coins. The medal is pretty well preserved, and adorned with two Punic characters; one of which is placed near the horse's breast, and the other under his belly. Neither of them seems to have suffered much, if at all, from the injuries of time. These two Punic letters may, Mr. S. apprehends, be safely pronounced Aleph and Koph, and must be considered as forming the first part of the name of some noted city, either in Sicily or Africa. As Mr. S.

could not meet with any ancient noted city of Africa, that had a mint erected in it, and a name beginning with the letters this piece exhibits; he could not attribute it to any town in that part of the world. He rather thinks it might have belonged to Agrigentum, an ancient city of Sicily, where money was coined, when that part of the island which appertained to the Carthaginians, was in alliance with that people, or had some commercial connections with them.

XLI. Introduction to the following Observations, made by Messrs. Charles Mason and Jeremiah Dixon, for determining the Length of a Degree of Latitude, in the Provinces of Maryland and Pennsylvania, in North America. By the Rev. N. Maskelyne, B. D., F. R. S. p. 270.

Messrs. Mason and Dixon, who observed the last transit of Venus over the sun at the Cape of Good Hope, under the direction of the R. S., had been since engaged, by Lord Baltimore and Mr. Penn, to settle the limits between the rovinces of Maryland and Pennsylvania, in North America; which they performed partly by trigonometrical, and partly by astronomical observations. In the course of this work, they traced out and measured some lines lying in and near the meridian, and extended, in all, somewhat more than 100 miles; and, for this purpose, the country in these parts being all over-grown with trees, large openings were cut through the woods, in the direction of the lines, which formed the straightest and most regular, as well as extensive vistos that, perhaps, ever were made.

They perceived that a most inviting opportunity was here given for determining the length of a degree of latitude, from the measure of near a degree and half. And, one remarkable circumstance very much favoured the undertaking, which was, that the country through which the lines run, was, for the most part, as level as if it had been laid out by art.

The astronomical observations had been taken with an excellent sector of 6 foot radius, constructed by Mr. Bird, the first that ever had the plumb-line passing over and bisecting a point at the centre of the instrument. This instrument was so exact, that they found they could trace out a parallel of latitude by it, without erring above 15 or 20 yards; in doing which, it should be observed, that they generally used the same stars, commonly 6 or 8 or 10 in number, at the several stations, and made a double set of observations at each station, with the limb of the sector turned both to the east and to the west. This sector had been set up at the northernmost point of the lines before-mentioned as proper for determining the length of a degree of latitude. In order to determine the difference of latitude between this point and the southernmost point of the lines, or the amplitude of an arch of a meridian contained between their parallels, it was necessary that the sector should be also set up at the southernmost point, and the like observations repeated there, on the same stars, which had already been observed at the northernmost point.

This plan of a measure of a degree in North America, Messrs. Mason and Dixon submitted to the consideration of the council of the R. S., and offered to carry it into execution, at the expence of the society, if they thought proper. The council determined that so useful and important a work should be completed, and accordingly sent out instructions to Messrs. Mason and Dixon for the regulation of their operations; particularly requiring them to measure the lines carefully over again with fir-rods, which they sent to them, together with a brass standard, of 5 feet, with which the rods were to be compared frequently, and the difference noted, and also the height of the thermometer at the time; for the lines had been all measured before with a standard chain, which, though sufficient for the common purposes of surveying, was by no means to be depended on in so nice an operation as that of measuring a degree of latitude. Mr. Penn was pleased, at the request of the R. S., to grant the further use of his sector, before mentioned, and other instruments, to the observers, for compleating this measure.

The method pursued in this work, is that which the level disposition of the country pointed out. But the result may be expected to be more accurate on this account, as measures taken in a straight line, and on a level surface, are known to be capable of great exactness; and no adventitious errors are here introduced from any possible errors of a chain of triangles. Messrs. Mason and Dixon having also determined the angle which the oblique line made with the meridian, by proper astronomical observations, and the amplitude of the arch of their meridian line by several observations of zenith distances of fixed stars, made at both ends of the meridian, with the limb of the sector turned both east and west at each extremity; this measure of a degree seems to be as well stated, and as much to be depended on, as any that has been made; and will probably be thought a valuable addition to the other measures of degrees, which have been taken with great care and pains, by various learned men, particularly the members of the Royal Academy of Sciences at Paris, who have acquired so much just reputation by their valuable labours bestowed on this subject.

It may not be improper to remark, that the level disposition of the country this degree passes through, which, it appears, also obtains further to the south, and in a great measure to the north of the limits of the same, gives some advantage to this measure, with respect to the use that may be made of it in inquiring into the figure of the earth; as there is no room for suspicion that the plumb-line of the sector could be deflected materially from its proper position by the attraction of any mountain, or even elevated ground of a more moderate height, continued for a great length; which latter circumstance, not taken notice of before, the learned Father Boscovich has shown, may produce a very considerable deviation of the plumb-line, in the elaborate treatise of the measure of a degree of the meridian between Rome and Rimini, taken by himself and his learned coadjutor, Father le Maire.

XLII. Observations for Determining the Length of a Degree of Latitude in the Provinces of Maryland and Pennsylvania, in North America. By Messrs. Mason and Dixon. p. 274.

In this work, the first thing to be considered was, how to continue a right line: and this was done by setting up marks with the assistance of an equal altitude or transit instrument (for it was contrived so as to serve either purpose at pleasure), made by Mr. John Bird, of the same construction with that described by M. Le Monnier, in the preface to the single volume of the French Histoire Celeste. The cylindrical ends of the cross axis of the telescope were laid in two angles of the supporters, which rose perpendicularly from a horizontal bar, that was fastened firmly to the upper part of the vertical axis. The axis of the telescope was set truly horizontal, by a spirit level hung on its cylindrical ends. The brass frame, which receives the vertical axis, was screwed to a post fixed in the ground, in the direction of the line which was to be continued. When the vertical wire in the telescope was brought to bisect any mark, it was kept in that direction, by confining firmly, between two pushing screws, a horizontal arm that projected from a collar that surrounded the vertical axis; and, to prove that a small shock would not alter its position, a small pressure was applied against one of the supporters, which being removed, it was carefully noted, whether the wire returned again to bisect the mark.

At every station, or mark, the telescope was turned 2 or 3 times after the mark was fixed in the line, to prove that the said mark was truly set. In general, the distances between the marks did not exceed a mile, nor were they less than half a one. The telescope magnified about 25 times. Three or 4 marks were always left standing, and on a little rising ground they could all be seen in a right line, the vertical wire in the telescope bisecting their centres without sensible error. The marks used in continuing the lines, were concentrical circles of black and white, painted on both sides of a board 14 inches square. This board moved in mortices made in two posts, which were driven into the ground; and when the centre of the mark was brought, by means of signals, into the line, it was fastened by wedges to the posts. By means of a plummet, a peg was driven into the ground, and a notch cut in it, under the centre of the mark, to secure the line. In the evening, when we left off, a mark was placed before, and 2 or 3 left behind us; and in the morning the instrument was again set up in the same place, to prove that the marks were not moved.

The tremor of the air, caused by the sun's rays, was often very great; and to avoid any error that might arise from the fluttering of the marks, we intermitted our operations sometimes for 5 or 6 hours in a day, and were often obliged to make use of the morning or evening twilight. In the continuation of the line, a person was left at the mark, behind the instrument, till another mark was set forward, to prove with a plummet that its centre was not moved. The visto cut through the woods, in this work, was about 8 or 9 yards wide, and in general seen about 2 miles, beautifully terminating to the eye in a point. The zenith distances of the stars, for determining the celestial arc, answering to the interval of the parallels of the northernmost and southernmost points of the lines, were made with an excellent sector of 6 feet radius, constructed by Mr. John Bird.

In the course of the work, for dividing the provinces of Maryland and Pennsylvania, the following lines were traced out, that offered themselves for determining the length of a degree of latitude.

In fig. 3, pl. 16, let N represent the northernmost point, and A the most southern of the said lines. Beginning at N, a meridian was traced from N to P = 14 mil. 64 cha. 8 links. In this line there were some hills, which were measured horizontally with a level, but the plains were measured with a chain. PC = 2m. 79 ch. 27 lin.; C being in the parallel of latitude with P, which was determined by the sector. DC a meridian = 5m. 2ch. 43lin. in which are 3 or 4 small ascents and descents. The points B, D, E, and M, are in a right line. BD = 22cha. 51lin. and the angle CDM = 86° 32¼′ nearly. Hence, B is south of D = 1ch. 36lin. = D *g*. The line AB = 81m. 78ch. 31lin., in which is one gentle rising hill, about half a mile over; all the rest of the line is an entire level or plain.

These measurements, expressed in English statute miles and parts of the same, were made with a chain, established from a brass statute yard, which was proved and corrected, in the course of the work, by another statute chain, kept only for that purpose, made from the said brass yard. They were only designed for dividing the provinces of Maryland and Pennsylvania: the same lines were re-measured afterwards with wooden rectangular levels, for the purpose of determining the length of a degree of latitude, as will appear in the sequel of this work.

The point C was placed in the parallel of latitude of P, thus. Let N, fig. 4, represent the north pole of the terrestrial globe; P and R two places lying in the same parallel of latitude RCP; PR an arch of a great circle = 10′ joining the said points; and PN, RN two meridians. PN, or the complement of the latitude of P, being = 50° 16′ 42″, the angle NPR, or the azimuth of the great circle PR, was found by calculation to be 89° 55′ 51″. The going of the clock being found by equal altitudes of stars, the times were computed when the same or other stars would pass the azimuth of the line PR; and, at the time computed for any star, the intersection of the cross wires of the transit instrument being brought to cover the star, the telescope was turned down to the horizon, and a land mark was fixed up at the distance of about half a mile, answering to the intersection of the wires. In like manner, by other stars, several other marks were fixed up, and the mean of all was taken. In this direction the line PR was continued; and though it was at first intended to extend it only to R, to the distance of 10′ of a great circle, it was in fact prolonged somewhat further, to S, PS being = 12.312 miles, or 10′ 45″ of a great circle. Now PC being = 2.991 miles, or 2′ 37″ of a great circle, the angle NPC is = 89° 58′ 55″; from which NPS = 89° 55′ 51″ being subtracted, there remains the angle SPC or *a*PC = 3′ 4″, whence *a*C, or the distance of the parallel PCR at C, south of *a*, should be 14.1 feet. But it having been made a rule, in dividing the provinces of Pennsylvania and Maryland, to trace out the parallels of latitude by the observations taken with the astronomical sector only, the sector was put up at P and S successively (see fig. 3), and the zenith distances of the stars Capella, α Lyræ, and others, were observed at both places; whence the point S was concluded to be 43 yards or 129 feet = SQ, more northerly than P; and thence it was found by calculation, that the parallel of latitude PQ at the point C should be 45½ feet, = *a* C, distant from the great circle PS, and to the south of the same; and the point C was placed accordingly, by laying off 45½ feet = *a*C, at right angles to the line P*a*, from the point *a* towards the south.

C*a* found by the sector being 45½ feet, and found by the azimuth of the line PS, being 14.1 feet only, it follows, that had the position of the point C been determined by the latter method, instead of the former, it would have been placed 31.4 feet more to the northward than it was found by the sector; and consequently the length of the degree of latitude would have come out 21 feet longer. But the difference is so small, that it only serves to confirm the exactness of the work, and renders it unnecessary to enter into any consideration, which of the two methods ought to be preferred. The meridians NP, CD, and AM, were found by celestial observations. The method of proceeding was as follows: To find the meridian AM, and the angle that the line AB makes with the said meridian: The equal altitude instrument being set up at the point A, with its vertical axis over the said point, equal altitudes of stars were observed for finding the motion of the clock. The time was next computed when some northern stars would pass the meridian by the clock, at which instant, shown by the clock, the vertical wire in the telescope was brought to bisect the star; and, the vertical axis of the instrument remaining fixed, the telescope was turned down in the same azimuth to the horizon, and a candle placed opposite to the vertical wires, as a point in the meridian. And the time of stars passing an azimuth in the direction of the line AB, for determining the angle BAM, was found by bringing the vertical wire in the telescope to bisect a candle placed, about 1¼ mile from A, in the line AB; the telescope was then elevated to the star, and the time when it passed the said vertical wire taken.

The observations for determining the meridian AM, and the angle that the line AB makes with the said meridian, were then set down. At the instant when the clock showed $22^h\ 53^m\ 30^s$, the vertical wire was brought to bisect the star α Ursa major; and then the vertical axis was made fast (the level showing the horizontal position of the axis of the telescope and the line of collimation being just), the telescope was then brought down to the horizon, and by means of a candle seen through a small hole in a board, a mark at the distance of 21 cha. 42 lin. was placed in a line with the said vertical wire. In like manner, when γ Ursa majoris, and the pole star passed the meridian, as known by the clock, the vertical wire was brought, at those instants of time, to the said stars, and the telescope turned down to the horizon, in the same manner above-mentioned, and the wire bisected the candle, when held to the 1st mark, as near as could be judged. At $2^h\ 57^m\ 10\frac{1}{4}^s$ β Ursa minor was bisected by the vertical wire for finding a meridian; and the telescope then turned down to the horizon, and by bringing a candle at the distance of a mile, to be bisected by the vertical wire, we there placed a mark. After these observations, the clock was wound up, in doing which it was stopped about 23^s.

Again at $2^h\ 58'\ 41''$ the wire was brought to β Ursa minor for finding a meridian as before; and, by means of a candle at the distance of a mile, a mark was placed, which fell 3 inches east of that placed the 14th. In this last observation the axis of the telescope was turned end for end; that is, the telescope itself was turned upside down. This proved the ends of the cylinder to be good.

On the 17, in the evening, by means of a candle placed behind a board, with a small hole in it over the mark placed the 13th instant, the line was extended to the marks at a mile distance, and there a mark placed, which fell ¼ of an inch east of the mark placed the 14th instant.

From the whole, there are 6 observations, all within the space of about 3 inches, at the distance of a mile: The mean was taken as a point in the meridian, north of the point A. At this meridian point m, we laid off the line mp at right angles to the meridian A, m, M, and, by a candle being placed at o, in the right line AB, about 1¼ miles from A, another candle was advanced along the line mp, till the vertical wire in the telescope bisected both candles: Under the candle, at the intersection of mp, with AB, viz. at p, a mark was placed in the ground. The ground between m and p being made smooth (it was level as a floor by nature) the distance mp was measured twice, and found to be 5 chains, 14 feet; and $\frac{3}{10}$ of an inch. With this same chain the distance m A was measured = 80 chains exactly.

For the angle BAM, by celestial measure.

The AR of the meridian when α Ursa major passed an azimuth in the direct. of the line AB.	22h 16m 53s.8	by observ. made Oct. 12,	
	22 16 55. 1	D° on the......... 13,	
	22 16 54. 7	D° on the......... 16,	
Mean.................. =	22 16 54. 5	=	334° 13′ 38″
AR of α Ursa major..........			162 16 54
Angle at the pole = ...			171 56 44
AR's of the meridian when β Ursa minor passed the direct. of the line AB.	1 58 7. 8	by observ. made Oct. 14,	
	1 58 14. 2	D° on the......... 15,	
	1 58 7. 1	D° on the......... 16,	
Mean of the first and last = ...	1 58 7. 5	=	29 31 52
AR of β Ursa minor..........			222 53 30
Angle of the Pole..........			166 38 22

Now having the distance of the *s from the pole, the angles at the pole, and the latitude of the point A, per spherics, we find the star's azimuth from the north = the angle BAM.

And for the said angle by terrestrial measurement. As AM : Rad :: pm :

Tangent angle pAM. = the angle BAM. = ...	3° 43′ 40″	
Angle per α Ursa minor..........	3 43 25	
D° per β Ursa minor	3 43 25	
Mean..........	3 43 30	= Angle BAM.

In the same manner as the point m, in the meridian from A was found, points in the meridian, north of P and N, were also found; and having two points given, with them a right line was extended as follows: first NP in a line with N e.—— At N the equal altitude instrument was set up, and the vertical wire in the telescope was brought to bisect the mark at e; and there the vertical axis made fast. The spirit level showing the axis of the telescope to be horizontal. The vertical axis being well secured, the telescope part is taken off the supporters, and turned to point to the southward (carefully taking off, and putting it on the supporters so as not to move the axis); then on the farthest rising ground that could be seen, another mark was placed at l, in a right line with the vertical wire. A mark being left at N, the instrument is taken, and set up 3 or 4 feet south of the mark l, and having brought the vertical wire in the telescope in a right line with the marks at l and N, the vertical axis is then made fast as before, the telescope immediately turned, and a third mark placed to the southward; and so the operation was continued.

In the same manner the lines PS, CD, AB, and AE were traced out; and to prove that by this method a right line may be extended, we shall here give the result of continuing the lines AB and AE. A and D being two points between which a right line was to be drawn. The point H was known to be nearly in the line AD. At A and n marks were placed, and 3 or 4 feet north of n, which was $\frac{1}{4}$ of a mile from A, the instrument was set up; then, in the same manner as above, the vertical wire in the telescope was brought in a line with A and n, and the vertical axis made fast; the telescope was then turned to point to the northward, and a third mark placed, &c. &c. In this manner the line A n was continued to B. Having continued the line to B, it fell 22 cha. 51 l. west of the point D: we then returned and laid off eastward off-sets from the line AB, at every 5th mile from B, proportional to the distance from A, and at the end of every off-set placed a post, to form the line AD.

Off-sets were measured very correct at { about $10\frac{1}{2}$ miles = ab, the 10^{th} mile = fc, about the 9^{th} d° = hd, } and at the 3 points, b, c, d, 3 marks were placed. At b the instrument was set up, to see if the 3 marks were placed in a right line; when it appeared they were not exactly so; but, on moving the middle one $\frac{1}{2}$ an inch east, they then made a right line.

In this direction we continued the lines b, c, d, to A, which fell 2 feet 2 inches west of the point A, at q. The distance A q being so small a quantity, gradually rising, in 10 miles, we thought it would be superfluous to change the direction, and therefore returned to the point b, and extended the line northward, proceeding in the same manner as before. Having continued the line to E, it fell 16 feet 9 inches east of the point D. Hence the off-sets from this line to the true line AD. And as we passed by the off-set posts made from AB, we measured the distance of this line AE from the said off-set posts.

These measures give the difference of the results of the two lines AB and AE; that is, the off-set posts from the line AB, which form the true line AD: And the off-set posts from the line AE traced last, that also form the said line AD, will be distant from each other, at every 5th mile, as was set down; which was a sufficient proof that the line AB is the arch of a great circle; but, as a further confirmation that no error could arise, they observed at different points in the line the right ascensions of the meridian when the star δ ursæ minoris passed an azimuth corresponding to the direction of the line, being in the upper part of its circle. The method of proceeding was in the same manner, as described before, for finding the angle BAM.

Then are set down, as they were observed, with their reductions, at length, a great number of observations of several stars, with the plane of the sector turned both to the east and west, to determine the magnitudes, in degrees, minutes, and seconds, of the celestial arcs, answering to the several parts measured of the terrestrial meridian.

Remarks on re-measuring the lines with two rectangular levels or measuring frames.—The levels used in this work were each 20 feet in length, and 4 feet in height. They were made of pine, an inch thick, and in form of a rectangle;

the breadth of the bottom board was $7\frac{1}{4}$ inches, that of the top = 3 inches, of the ends = $4\frac{1}{2}$ inches, and the bottom and the top were strengthened with boards firmly fixed to them at right angles. The joints were secured with plates of iron, and the ends were plated with brass. The plumb-lines used in setting them level, were = 3 feet and 2 inches in length, and hung in the middle of the levels, being secured in a tube from the wind, in the manner of carpenters' levels; on which account we called these by the same name. When the plumb-line bisected a point at the bottom, the ends were perpendicular. Where the ground was not horizontal, or there were logs, &c. to pass over, one end of the level was raised by a winch and pully.

The level being set, a short staff was driven into the ground, very near and opposite the plumb-line, in the top of which moved a thin plate of iron, about 12 inches long; at the ends of which were points, directed to the intersections of lines, drawn on the board that covered the plumb-line. By bringing the points in a line with one of the said intersections, if the level was by accident moved, it might be discovered, and brought again to its place.

A level being thus marked, the end of the other was brought in contact with it, and marked in the same manner, before the first was moved; the first was then taken up, and set before the last. And so the operation was continued. Mr. Dixon attended one plumb-line and staff, and Mr. Mason the other. The measure was carried on in a straight line, and in the proper direction, by pointing the levels to the farthest part of the visto that could be seen; this was readily and accurately done, on account of their lengths. The levels were frequently compared with the brass standard, of 5 feet, provided for that purpose, and the difference was noted between 8 times the brass standard, and the length of the two levels taken together. This difference serves for reducing the measure taken with the levels, to what it would have been if it had been taken with the brass standard itself. For facilitating this comparison of the levels with the brass standard, pieces of brass were fixed into the bottom boards of the levels on each of which was drawn a faint line. And $\frac{1}{10}$ of an inch at the end of the brass standard being divided into 10 parts or 100ths of an inch, the difference between 8 times the brass standard, and the two levels joined together, was with the help of a magnifying glass of a short focus, determined to great accuracy. The brass standard being liable to alter with the changes of heat and cold, a further correction becomes necessary on that account, in order to reduce the measures to the temperature of 62° of Fahrenheit's thermometer, which is the term to which the former operations of this kind have been reduced. For this purpose, the rate of expansion of brass is taken from Mr. Smeaton's experiments, made with a pyrometer of his invention (see Philos. Trans., vol. xlviii, p. 482, vol. x, of these Abridgments) which is $\frac{232}{10000}$ of an inch on a length of one foot for a variation of 180° of Fahrenheit's thermometer; whence the expansion answering to 4 times the length of the brass standard, or 20 feet, or the length of one level, would be $\frac{464}{1000}$ of an inch for the same difference of the thermometer; and $\frac{258}{100000}$ of an inch for 1° of the same thermometer. Therefore, in order to find the correction of column 7th, the constant quantity ,00258 was multiplied by the difference of 62, and the degree of the height of the thermometer; and that product, again multiplied by the number of levels measured, gave the correction required in inches and decimal parts of an inch: which was additive or subtracive, according as the thermometer was higher or lower than 62 degrees.

These geographical measurements are then all set down very particularly. Hence the line AB = 21696.47 levels + 892.34 inches + 94.51 inches = 434011.64 feet. The breadth of the rivers was found by measuring a base, and taking angles with a Hadley's quadrant.

Note.—The reckoning was kept by stretching a rope in the line to be measured, in general = 12 levels, which was often proved: and it was almost impossible that an error could arise; as they always began the rope with the same level, and ended it with the other; the rope not being removed till the last level was set. The person that stretched the rope, sometimes Mr. Dixon, and sometimes Mr. Mason, kept the count of the number of ropes measured: though the mile posts in the lines AB and DC were sufficient for that purpose, as the lines had been so often measured before. In the line NP there were no mile posts, but 2 or 3 intermediate marks, which they found to agree in a general law with the levels. Supposing the levels exactly = 20 feet each; then in the line NP a mile per chain measure = a mile and 9.44 feet by the levels; and in the line CD a mile per chain measure = a mile and 9.86 feet by the levels. They took notice of these differences as they measured from B to A, always finding the miles greater by the chain measure; which shows that the chain was continually extending itself by use; as they had direct proof of, being obliged to contract it every day, and re-adjust it to its proper length by means of the standard chain.—For observations of several stars, they found the latitudes of the points A and N; viz. the mean lat. of N 39° 56′ 18″.9; arch between N and A = 1° 28′ 44″.9; which leaves the lat. of the point A 38° 27′ 34″, and the mean lat. 39° 11′ 56″.

The Length of a Degree of Latitude in the Province of Maryland and Pennsylvania, deduced from the foregoing Operations; by the Astronomer Royal.

The difference of latitude of the points N and A, or the amplitude of the celestial arch, answering to the distance between the parallels of latitude passing through N and A, has been found by the sector, to be 1° 28′ 45″.0. The terrestrial measure of the distance of the said parallels is next to be found. This is composed of the sum of the lines NP, CD, Dg, and AR, the last-mentioned line being the reduction of AB to a meridian line passing through A; therefore BR expresses a parallel of latitude passing through B. Let Bt be an arch of a great circle drawn perpendicular to the meridian line AR produced. The triangle BAt, on account of the smallness of its sides with respect to the radius of the earth, and the smallness of the angle BAt = 3° 43′ 30″, may be taken for a plane rectilinear triangle, in what follows, without any sensible error, as will appear to any one who makes the trial.—Therefore it will be, by proportion, as radius is to the cosine of the angle BAt = 3° 43′ 30″, so is AB = 434011.6 English feet, to At = 433094.6 English feet. But this is to be lessened by the small quantity Rt, or the distance of the parallel circle BR from the great circle Bt, which is to a 3d proportional to the diameter of the earth and the line BR, as the tangent of the latitude of the point B, to the radius. Whence Rt = 15.8 feet; which subtracted from At just found =

433094.6 leaves	AR = 433078.8 feet
To which add as found before	NP = 78290.7
	CD = 26608.0
	Dg = 89.7
The sum is..........	= 538067 feet

= an arch of meridian intercepted between the parallels of latitude passing through the points N and A, answering to the celestial arch 1° 28′ 45″.

Then say, as 1° 28′ 45″ : is to 1° :: so is 538067 feet, to 363763 English feet, which is the length of a degree of latitude in the provinces of Pennsylvania and Maryland. The latitude of the northernmost point N, was determined from the zenith distances of several stars, = 39° 56′ 19″, and the latitude of the southernmost point A = 38° 27′ 34″. Therefore the mean latitude expressed in degrees and minutes is = 39° 12′.

To reduce this measure of a degree to the measure of the Paris toise, it must be premised, that the measure of the French foot was found on a very accurate comparison, made by Mr. Graham, of the toise of the Royal Academy of Sciences at Paris, with the Royal Society's brass standard, to be to the English foot, as 114 to 107. See Phil. Trans. vol. xlii, p. 185, or p. 606, vol. viii, of these Abridgments. Therefore say as 114 : is to 107 :: so is 363763 the measure of the degree in English feet, to 341427 the measure of the degree in French feet, which divided by 6, the number of feet in a toise, gives the length of the degree = 56904½ Paris toises, in the latitude 39° 12′ north.

Such is the length of a degree in this latitude, supposing the 5 feet brass standard made use of in this measure to have been exactly adjusted to the length of the Royal Society's brass standard. It was really adjusted by Mr. Bird, by his accurate brass scale of equal parts, which he makes such excellent use of in dividing astronomical instruments, and which is just $\frac{1}{1000}$th part of an inch shorter than the Royal Society's brass standard on a length of 3 feet. If we would take notice of so small a difference, the length of a degree just found must be lessened by $\frac{1}{36000}$th part, or by 10 feet, to reduce it to the measure of the Royal Society's standard. In treating of such niceties, may it be allowed to add, that the 5 feet brass standard having been again compared with Mr. Bird's scale, since its return from North America, appeared both to Mr. M. and Mr. Bird to be just $\frac{1}{1000}$th part of an inch shorter than the scale, on that side on which the 100ths of an inch are placed at one end, and $\frac{2}{1000}$ths of an inch shorter than the scale on the opposite side? which diminution of its length is undoubtedly owing to the small wearing or battering which it has met with in the frequent use that was made of it. But the divided side of the rod having been that which was made use of in measuring the levels, is what is to be regarded in the present case. If we would allow for the wearing of the rod, we may suppose it to have suffered a gradual diminution; and then we must take a mean between its first length which was the same with Mr. Bird's scale, and its present length, which is $\frac{1}{1000}$th of an inch shorter; as we may suppose it a medium to have been $\frac{1}{2000}$th part of an inch shorter than Mr. Bird's scale; on which account the length of the degree should be further diminished by $\frac{1}{120000}$th part, or 3 feet, which added to 10 feet, the correction required on account of the difference of Mr. Bird's scale and the Royal Society's standard, gives 13 feet to be subtracted from the length of the degree calculated above. The whole correction will perhaps be thought scarcely deserving of notice, especially as an error of only 1″ in the celestial measure would produce an error of no less than 67 feet in the length of the degree. It is probable also that the length of a degree has been already taken 10 or 20 feet too short, by placing the point c too far to the southward; which would about balance the small correction in question. Therefore, all things being considered, the length of the degree may be stated as given above, viz. = 363763 English feet, or 56904$\frac{1}{2}$ Paris toises. It must however be observed, that the accuracy of this reduction into Paris toises depends on a supposition that the length of the French toise, which is of iron, was laid off by the gentlemen of the Royal Academy of Sciences, on the brass rod sent over to them for that purpose by Mr. Graham, which was afterwards returned to him, in a room where the heat of the air answered to 62 of Fahrenheit's thermometer, or 15 of Reaumur's, or nearly so, which is probable enough, but is a point that does not appear to have been ascertained. For, on account of the difference of expansion of brass and iron, 2 rods made of those metals, however accurately they may be made of equal lengths at first, will only agree together afterwards in the same temperature of the air in which they were originally adjusted together. It is fortunate that the uncertainty in the present case is but small, since 20° difference of Fahrenheit's thermometer or 10° of Reaumur's, produces, according to Mr. Smeaton's experiments, a difference of the expansions of brass and iron, of only $\frac{1}{13500}$th part, which would cause an error of only 27 English feet, or about 4 Paris toises in the length of the degree.

It is however to be wished, that the proportion of lengths of the French and English measures might be again ascertained by another careful experiment, in which the temperature of the air, as shown by the thermometer, might be noted at the time.

Postscript by the Astronomer Royal.

Mr. Maskelyne having, some time ago, acquainted M. De la Lande of the Royal Academy of Sciences at Paris, by letter, of this measure of a degree of latitude in North America, and at the same time expressed some doubts about the certainty of reducing it to French measure, from the proportion of the English to the French foot found by Mr. Graham; principally because no notice had been taken of the height of the thermometer at Paris, when the length of the French iron toise was laid off upon the brass rod sent thither by Mr. Graham, whence the proportion of the two measures was afterwards determined by him; and having also mentioned an opinion of the expediency of making another experiment of the proportion of the two measures, in which every necessary circumstance should be noted; and that he might probably request the favour of M. De la Lande to take the trouble to cause a French toise to be made for him, and to see it exactly adjusted to their standard, and then sent to Mr. M.; he has been pleased to lend Mr. M. two toises, which he says are exactly adjusted to the standard of the toise used by Mess. De la Condamine and Bouguer in the measure of the degrees of latitude at Peru, in order to their being compared with the English measure. This comparison has been made by Mr. Bird, with his usual accuracy, while Mr. M. was present, and also examined the same, since his account of the length of the degree of latitude aforegoing was printed; the result is, that the longest of the two toises (for there is a small difference between them), and which has since been marked with the letter A, is equal to 76.738 inches by Mr. Bird's brass scale of equal parts, and the shortest toise, which is marked B, is = 76.735 inches by the same scale; the height of Fahrenheit's thermometer in the room being 61 degrees. The mean of the lengths of the two toises is therefore = 76.7365 inches by Mr. Bird's scale. But Mr. Bird's scale is, $\frac{1}{1000}$th of an inch on 3 feet shorter than the R. S.'s brass standard, and consequently $\frac{21}{10000}$th too short for the same on 76,7365 inches; therefore $\frac{21}{10000}$th of an inch must be subtracted from 76,7365; which leaves 76,7344 for the length of the Paris toise in measures of the R. S.'s brass standard, in the temperature of 61° of Fahrenheit's thermometer. In the temperature of 62° it will be a little shorter; or it may be taken = 76,734 inches in measures of the R. S.'s brass standard. This is $\frac{24}{1000}$th or about $\frac{1}{42}$d of an inch longer than was determined by Mr. Graham's experiment. Hence it appears, that Mr. M. was mistaken in supposing, that the uncertainty about the true proportion of the English and French measures was but small, since the error in the former determination now appears to have been $\frac{1}{3197}$th of the whole, or equivalent to what might have been produced by a difference of 84° of Fahrenheit's thermometer. Whence it arose Mr. M. cannot pretend to say, neither is it very material to inquire; but the fact is plain, and fully justifies the propriety of repeating the experiment.

Mr. M. now states the length of the degree, measured by Messrs. Mason and Dixon, first in English feet, according to the R. S.'s standard, and then reduced to the French measure by the proportion just established. From 363763 English feet, the length of the degree found by the 5 feet brass standard, subtract 10 feet for the difference between Mr. Bird's scale and the R. S.'s standard, and 3 feet for the wearing of the brass rod; and there remain 363750 feet, according to the R. S.'s standard, for the length of the degree. But to this it seems proper to add 21 feet, to correct the position of the point C, determined by the sector, which cannot be so certain as that inferred from the azimuth of the line PS. Therefore the true length of the degree, according to the R. S.'s brass standard, in the temperature of 62° of Fahrenheit's thermometer, is 363771 feet, or 68,8960 English statute miles. To reduce this to the measure of the Paris toise, by the proportion above established, say as 76,734 is to 72, so is 363771 to 341328 French feet, or 56888 Paris toises, of the standard of that used in the measure of the degrees of the meridian at Peru.

The method used by Mr. Bird, in finding the length of the toises by his scale, was as follows, which may serve as a direction for the like purpose on any future occasion. Two brass pins were driven into a strong deal board 4 inches thick, and longer than the toise; and two brass cheeks were made very square, and the ends brought upon the pins. The toise was then put in between the cheeks, one of which was made to slide so as to be easily brought into contact with the end of the toise, and the other end at the same time touching the other cheek, the moveable cheek was screwed fast; and thus the toise was exactly contained between the cheeks without any shake, and it is evident that the interval between the cheeks was exactly equal to the length of the toise. In order to measure this interval, the toise being taken away, very fine lines were drawn with a fine point, at the end of each cheek, upon the brass pins which were in the same plain with the board: then the cheeks were removed, and fine points made at the outer extremity of each line, and this distance being taken between the fine points of a beam compass, was transferred to the scale, and thus the length of the toise was found in measures of the scale, which is divided by a vernier to thousandths of an inch. The toises and brass scale had been left together in the same room, and near one another all the night before, and till the very time of making the comparison of the toises with the scale, in order to be sure that they were all affected with the same degree of heat.

As it may be agreeable to the reader to see the result of the principal measures of degrees of latitude, that have been taken with later instruments and proper accuracy, brought together into one view, the following table is here added.

Length of a degree in Paris toises.	Mean latitude.			Names of the observers.	Years in which the degrees were measured.
57422	66°	20′	N	M. de Maupertuis, &c.	1736 and 1737
57074	49	23	N	M. de Maupertuis, &c. and M. Cassini	1739 and 1740
57091	47	40	N	P. Liesganig	1768
57028	45	0	N	M. Cassini.	1739 and 1740
57069	44	44	N	P. Beccaria	1768
56979	43	0	N	Le Pere Boscowich and le Maire	1752
56888	39	12	N	Mess. Mason and Dixon	1764 to 1768
56750	0	0		M. Bouguer and M. de la Condamine	1736 to 1743
57037	33	18	S	Abbé de La Caille	1752

If this degree be compared with the degree measured at the equator = 56750 toises, in the hypothesis of the earth's being an oblate spheroid, the ratio of the equatorial to the polar diameter will come out as 494 to 493. But, if it be compared with the degree measured in Lapland, in the latitude 66° 20′, = 57419 toises (having subtracted 3 toises, because the toise used in Lapland was $\frac{1}{20}$th or $\frac{1}{25}$th of a line less than the toise used in Peru, see M. De la Lande's Astronomy, Article 2107), the ratio of the diameters will be as 142 to 141. The great difference of these results is a fresh proof of what has appeared from the comparison of the measures of the several degrees taken before, either that the figures of the meridians are not accurately elliptical, or that the inequalities of the earth's surface have a considerable effect in deflecting the plumb-line from its true situation, or both. Mr. M. had indeed supposed that any deflections of the plumb-line were not to be feared with respect to this particular measure of a degree, at the end of his Introduction to Messrs. Mason and Dixon's account of the same, by arguing, perhaps too far, from the level disposition of the country through which the degree passes. But Mr. Henry Cavendish has since considered this matter more minutely; and having mathematically investigated several rules for finding the attraction of the inequalities of the earth, has, upon probable suppositions of the distance and height of the Allegany mountains from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be produced in the length of the degree, from the attraction of the said hills, and the defect of attraction of the Atlantic; and finds the degree may have been diminished by 60 or 100 toises from these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be very sensibly affected by the attraction of hills, and defect of the attraction of the Mediterranean Sea and Indian Ocean.

The rules which Mr. M. used in calculating the ratio of the equatorial diameter to the polar axis, from the North American degree, compared with those measured in Peru and Lapland, are those given by Mr. John Robertson, Librarian to the R. S., in his elements of Navigation, p. 597, as deduced by him from Dr. Letherland's Geometrical Analysis of the problem, which he has also given to the public in the same place, together with some other problems depending on it, which were necessary to complete the subject.

XLIII. *Astronomical Observations, made in the Forks of the River Brandiwine in Pennsylvania, for determining the going of a Clock sent thither by the Royal Society, in order to find the Difference of Gravity between the Royal Observatory at Greenwich, and the Place where the Clock was set up in Pennsylvania; to which are added, an Observation of the End of an Eclipse of the Moon, and some Immersions of Jupiter's First Satellite observed at the same Place in Pennsylvania. By Charles Mason and Jeremiah Dixon.* p. 329.

The place where these observations were made is the northernmost point of the lines that were measured for a degree of latitude, or point N, (see pl. 16, fig. 3) relative to that measure; it lies 31 miles west, by measurement; and 10″.5 south of the southernmost point of the city of Philadelphia, as found by the sector.

Having observed the time by the equal altitudes of several fixed stars, it is then added—From these observations we have the time of Capella's passing the meridian, and the rate of the clock's going as follows:

1766.		* passed meridian per clock.	Clock loses of Sid. time per day.	Mean state of therm.
December	24	4h 57m 40s +	16s.3	35°
	28	56 35	18.0	23
1767	30	55 59	13.4	6
January	1	55 32 +	14.8	37
	7	54 3	17.0	20
	8	53 46	16.3	37
	16	51 36	16.0	31
	19	50 48 +	15.63	33
	27	48 43 +	15.35	28
February	4	46 40½	15.5	30
	8	45 38½	15.9	35
	25	41 8 —		

1767. March.		☽ eclipsed. Time per watch.		
♄	17	8h 4m 10s		Eclipse of the ☽ ended.
		8 58 46	10h 27m 30s	Equal altitude of regulus.
		9 1 16	29 41	
		4 5	32 9	

The watch went very regular sider. time.

—Hence the eclipse ended at 8h 21m 59s apparent time, in the forks of the river Brandiwine.

N. B. The clock was firmly screwed to a piece of timber, 22 inches in breadth and 5¼ inches thick; the piece of timber was let 4 feet into the ground, which was composed of a very firm, dry, hard clay. The clock was placed in a tent, with Fahrenheit's thermometer hung to its side; and a blanket was wrapped round the clock and thermometer, to secure it from any wind that might enter the tent. The pendulum was adjusted to the upper scratch, with N° 3 at the index, as directed by the Rev. Mr. Maskelyne, Astronomer Royal: but the spring at the suspension of the pendulum having been broken (when the ship, in which it was sent, was wrecked on the Jersey coast) we cannot be certain that the pendulum is now of the same length as it was when sent from London.

A journal is then given of the height of the thermometer observed 2 times a day; also of the extent of the arch vibrated by the pendulum, which was commonly from 1° 35′ to 1° 40′ on each side of the perpendicular. But no inference is drawn touching the difference of gravitation; probably owing to the uncertainty of the length of the pendulum by being broken.

XLIV. On the Extraordinary Heats observed at Rome this last Summer. By Mr. James Byres. Dated Rome, Aug. 27, 1768. p. 336.

The excessive heat of this summer is much greater than has been known in Rome for many years. Friday, the 19th instant, the mercury in a well-regulated thermometer according to Fahrenheit's scale, exposed at a north window, where there was no sun and very little reflection, stood from 10 o'clock in the morning until about 5 in the evening at 99. About half an hour after sunset it fell to 90, and at midnight was fallen to 85, where it remained all night. This is the hottest day we have had; but for these 3 weeks past at mid-day the mercury has been always above 94, and at midnight seldom under 83; which is the more extraordinary as he did not remember to have observed any other summer above 89 at mid-day, nor above 75 at midnight. Notwithstanding this great heat, there was never a more healthy summer at Rome; all the hospitals are almost empty.

XLV. An Easy Method of Making a Phosphorus, that will Imbibe and Emit Light, like the Bolognian Stone; with Experiments and Observations. By John Canton, M.A., F.R.S. p. 337.

To make the Phosphorus.—Calcine some common oyster shells, by keeping them in a good coal fire for ½ an hour; let the purest part of the calx be pulverized and sifted; mix with 3 parts of this powder one part of the flowers of sulphur; let this mixture be rammed into a crucible of about 1½ inch in depth, till it be almost full; and let it be placed in the middle of the fire, where it must be kept red hot for one hour at least, and then set by to cool: when cold turn it out of the crucible, and cutting or breaking it to pieces, scrape off, on trial, the brightest parts; which, if good phosphorus, will be a white powder; and may be preserved by keeping it in a dry phial with a ground stopple.

The quantity of light a little of this phosphorus gives, when first brought into a dark room, after it has been exposed for a few seconds, on the outside of a window to the common light of the day, is sufficient to discover the time by a watch, if the eyes have been shut, or in the dark, for 2 or 3 minutes before. By this phosphorus celestial objects may be very well represented; as Saturn and his ring, the phases of the moon, &c. if the figures of them, made of wood, be wetted with the white of an egg, and then covered with the phosphorus. And these figures appear to be as strongly illuminated in the night, by the flash from a near discharge of an electrified bottle, as by the light of the day.

Exp. 1. Having put some of the same parcel of the phosphorus into two glass balls, and sealed them hermetically; he placed one of them on the outside of a window facing the south, that it might be very much exposed to the direct rays of the sun, where it remained from the 25th of December 1764, to the 25th of December 1765. The other was kept during the same time in darkness. After this, they were both exposed to the light, and carried into a dark room together; where the phosphorus in each appeared equally bright.

Exp. 2. Some of the phosphorus finely powdered, being put into a glass ball, with as much water as would make it adhere to the glass, so as to cover the inside of the ball, which was hermetically sealed, was found gradually to lose its property of imbibing and emitting light, but faster in summer than in winter; so that at the end of the first year, it could not in the least be perceived to shine, when taken from the strongest day light, and carried into a dark room. It was also observed to lose its whiteness by degrees, and to become of a very dark colour, especially on that side of it next to the glass. Some of the phosphorus which was made to stick to the inside of a glass ball hermetically sealed, by means of common spirit of wine, was found after one year to be a little impaired; but some made to stick by means of an ætherial spirit, was found not to be impaired at all.

According to Dr. Nic. Lemery (in his Course of Chemistry, 11th edition) the exposing the Bolognian stone to the sun wears it out. But by the first experiment it appears, that a phosphorus of the same kind was not hurt by the sun in 12 months. Water, indeed, in the 2d experiment, was found in that time to destroy it. Therefore it is probable, that what the Dr. imputed to the light of the sun, was caused by the moisture of the air.

Exp. 3. Mr. C. mixed a small quantity of the phosphorus with a considerable quantity of spirit of wine in one glass ball, and with æther in another, and sealed them hermetically. When the balls were shaken, each of the fluids appeared like milk; but the phosphorus would soon subside when the balls were at rest, and leave the spirit of wine and æther quite clear. After some months the spirit of wine was found to be tinged with yellow; but the æther to this time remains unaltered. By shaking the balls while they are exposed to the light, the whole of the fluid in each will appear luminous when carried into a dark room. The æther gives as much light now as it did at first; but the spirit of wine a little less.

Exp. 4. He exposed the dry phosphorus, in one of the glass balls mentioned in the first experiment, to the light of the day, by holding it on the outside of a north window about half a minute; after which it was kept in darkness for 2 days and a half, and was then found to shine, by putting the glass ball that contained it into a basin of boiling water. On the morrow it was exposed to the light again; and after it had been kept 4 days and a half in the dark, it gave light when put into boiling water, though not so much as before. In summer he finds it will not give any light by the heat of boiling water after keeping it 15 days; but in winter it will afford a little, after keeping it a month.

Exp. 5. The phosphorus in each of the 2 glass balls, mentioned in the first experiment, was illuminated at the same time and to the same degree, and carried into a dark room. One of the balls was immediately put into a basin of boiling water, and then the phosphorus in it became much brighter than that in the other, and continued so for a short time, but parted with its light so fast, that in less than 10 minutes it was quite dark. The other phosphorus still gave a considerable degree of light, and remained visible for more than two hours after, when even the heat of the hand would plainly increase its light.

Bolognian phosphorus is said, by Lemery, and also by Musschenbroek,* to imbibe less light when hot than when cold, as it appears less bright when carried into a dark room. But this appearance may be caused by its parting with the light it has received faster when in the former state, than when in the latter, according to the last experiment; as it must lose more when hot than when cold, during the time of conveying it from the place where it takes the light, to a place dark enough to observe it in. And this seems to be the cause also, why Bolognian phosphorus never appears so bright after it has been illuminated, and consequently in some measure heated, by the direct beams of the sun, as after it has only been exposed, in the shaded open air, to the common light of the day.

Exp. 6. The balls used in the last experiment were kept in the dark for two days after, and then each at the same time was put into a basin of boiling water in a dark room; that which had parted with its light in the hot water before, was not visible; but the other appeared luminous for a considerable time.

When the phosphorus has once lost as much of the light it had received as the heat of boiling water will cause it to part with, it has never after been found, if kept in darkness, to give any more light by that degree of heat. But if it be

* See his Introductio ad Philosophiam Naturalem, § 1697. See also § 1701 and 1686.—Orig.

exposed again to the common light of the day, the experiments may be repeated with the same success as before. This has frequently been done, with some dry phosphorus in glass balls which have been hermetically sealed, about 4 years, without the least injury to the phosphorus; as it appears to be as good now as it was at first.

Exp. VII. Let one end of a bar of iron, of about an inch square, or a poker, be made red hot, and laid horizontally in a darkened room, till by cooling it ceases to shine, or is but barely visible. Then bring a little dry phosphorus, which has been exposed to light in a glass ball hermetically sealed, as near the hot iron as possible, by holding the ball in contact with it; and the phosphorus, though invisible before, will in a few seconds begin to shine; and will discharge its light so very fast as to be entirely exhausted of it in less than a minute; and then will shine no more by the same treatment, till after it has been exposed to light again. By this heat, light received from a candle, or even from the moon, may be seen several days after. And phosphorus that will afford no more light by the heat of boiling water, will shine again by the heat of the iron. By this heat also, phosphorus which had been kept in darkness more than 6 months, was found to give a considerable degree of light.

It was the opinion of the great Sir Isaac Newton, that the rays of light are very small bodies emitted from shining substances, and not motion propagated through a fluid medium; for several reasons which he has given in his Optics. Notwithstanding which, it has been urged since his time, that light is nothing but a repellent fluid put into very violent vibrations. Now Mr. C. thinks it impossible, if light be nothing but motion propagated through a fluid medium, and not particles emitted from the luminous body, to account for the phenomena in the 5th, 6th, and 7th experiments. That a substance should either give light or not, when its parts are agitated by the same degree of heat, according as it has or has not been exposed to light, for a few seconds of time, more than 6 months before; seems plainly to indicate a strong attraction between that substance and the particles of light; by which it keeps many of them, in the common heat of the air, a long time, if not always: for the light the phosphorus gives by being heated to a certain degree appears to be caused by its throwing off adventitious particles, and not by any of its own; since its light will decrease and be entirely gone, before the phosphorus will be hot enough to shine of itself, or to emit particles of light from its own body.

A writer against the Newtonian doctrine of light is pressed with a great difficulty, and asks, if it be possible that a particle can move so far as from the sun to the earth, and not frequently impinge upon other particles, when, he says every part of space must contain thousands of them? But this difficulty will nearly vanish, if a very small portion of time be allowed, between the emission of every particle and the next following in the same direction. Suppose, for instance, a lucid point of the sun's surface to emit 150 particles in one second, which are more than sufficient to give continual light to the eye, without the least appearance of intermission; and then the particles, on account of their great velocity, will be behind one another more than 1000 miles, and leave room enough for others to pass in all directions.

XLVI. Astronomical Observations made at Swetzingen, in 1767 and 1768; extracted from several Letters written by Father Christian Mayer, F.R.S. p. 345.

These observations are chiefly of the meridian altitudes of the sun and fixed stars; with some observations of the eclipses of Jupiter's satellites: of no use now to be retained in these Abridgments.

XLVII. Observations of the Transit of Venus over the Sun, and the Eclipse of the Sun, on June 3, 1769, made at the Royal Observatory. By the Rev. N. Maskelyne, B.D., F.R.S. p. 355.

The weather, which had been cloudy or rainy here, with a south wind, for the greatest part of the day, began to clear up at 4 o'clock in the afternoon, the wind having returned to the west, the same quarter in which it had been the afternoon before, which was remarkably fine and serene, though it changed early in the morning preceding the transit. Towards the approach of Venus's ingress on the sun, the sky was become again very serene, and so continued all the evening, which afforded as favourable an observation of the transit here as could well be expected, considering that the sun was only 7° 3′ high at the external, and 4° 33′ at the internal contact. Mr. M. observed the external contact of Venus at 7^h 10^m 58^s apparent time, with an uncertainty seemingly not exceeding 5^s; and the internal contact, by which he means the completion of the thread of light between the circumferences of the sun and Venus, at 7^h 29^m 23^s apparent time, with a seeming uncertainty of only 3^s; for so long was the thread of light in forming, or the sun's light in flowing round and filling up that part of his circumference which was obscured by Venus's exterior limb. Yet he would not hence infer, that observations made by astronomers in distant places should agree together within such narrow limits; for he knows they will not even in the same place, and that a difference in the skill or judgment of the observers, in the telescopes, and perhaps in some other little circumstances, not easily distinguished, may produce much greater disagreements, especially if the sun be low, as it was here; in like manner as in observing the eclipses of Jupiter's satellites, the immersion or emersion shall often seem instantaneous, or nearly so, equally to two observers in distant places, and yet the absolute times of the observations may differ a minute of time or more from each other, owing to the difference of telescopes, weather, or other circumstances. Indeed, in the present case, the limit of differences is certainly much narrower; but what it is he does not at present venture to suggest, as that may be better done when all the observations that shall have been made of the transit are collected together. The telescope which he used was an excellent reflecting one of 2 feet focus, made by the late ingenious Mr. Short, and is the same with which the last transit was observed here by Mr. Charles Green. Mr. M. applied the magnifying power of 140 times, and used smoked glasses to defend the sight, which are much preferable to black or red glasses, as showing the objects more distinct, and being much more pleasant to the eye.

It had been thought by some, that Venus's circumference might probably be seen, in part at least, before she entered at all upon the sun, by means of the illumination of her atmosphere by the sun; Mr. M. therefore looked out diligently for such an appearance, but could see no such thing. He was also attentive to see if any penumbra or dusky shade preceded Venus's first impression on the sun at the external contact, such a phenomenon having been observed by the Rev. Mr. Hirst, F.R.S., at the former transit of Venus, in 1761, which he observed with much care and diligence at Madras, in the East Indies; but Mr. M. could not discern the least appearance of that kind. He would not however be therefore thought to call in question either Mr. Hirst's discernment or fidelity; as he is sensible that the tremors of the limbs of the sun and Venus, occasioned by the vapours at the altitude of 7°, might easily obscure a faint object.

When Venus was a little more than half immerged into the sun's disk, Mr. M. saw her whole circumference completed, by means of a vivid, but narrow and ill-defined border of light, which illuminated that part of her circumference which was off the sun, and would otherwise have been invisible. This he might probably have seen sooner, if he had attended to it. He continued to see it till within a few minutes of the internal contact, and grew apprehensive that it would prevent the appearance of the thread of light, when it came to be formed; but it disappeared about 2 or 3 minutes before, as well as he can remember: after which the regularity of Venus's circular figure was disturbed towards the place where the internal contact should happen, by the addition of a protuberance, dark like Venus, and projecting outwards, which occupied a space on the sun's circumference, which bore a considerable proportion to the diameter of Venus. Fifty-two seconds before the thread of light was formed, Venus's regular circumference, supposed to be continued as it would have been without the protuberance, seemed to be in contact with the sun's circumference supposed also completed. Accordingly, from this time, Venus's regular circumference, supposed defined in the manner just described, appeared wholly within the sun's circumference; and it seemed therefore wonderful that the thread of light should be so long before it appeared, the protuberance appearing in its stead.

At length, when a considerable part of the sun's circumference, equal to $\frac{1}{4}$ or

$\frac{1}{4}$ of the diameter of Venus, remained still obscured by the protuberance, a fine stream of light flowed gently round it from each side, and completed the same in the space of 3 seconds of time, from 7^h 29^m 20^s to 7^h 29^m 23^s apparent time; and Venus appeared wholly within the sun's lucid circumference; but the protuberance, though diminished, was not taken away till about 20^s more, when, after being gradually reduced, it disappeared, and Venus's circular figure was restored.

An ingenious gentleman of his acquaintance having desired him to examine if there was any protuberance of the sun's circumference about the point of the internal contact, as he supposed such an appearance ought to arise from the refraction of the sun's rays through Venus's atmosphere, if she had one; he carefully looked out for such a circumstance, but could see no such thing; neither could he see any ring of light round Venus, a little after she was got wholly within the sun; but he did not re-examine this latter point afterwards, when she was further advanced upon the sun, at which time other persons in the observatory saw such an appearance.

How far the ring of light, which he saw round that part of Venus's circumference which was off the sun, during the immersion, may deserve to be considered as an indication of an atmosphere about Venus, Mr. M. does not at present inquire; but he thinks it very probable, that the protuberance, which disturbed Venus's circular figure at the internal contact, was owing to the enlargement of the sun's diameter, and the contraction of that of Venus, produced by the irregular refraction of the rays of light through our atmosphere, and the consequent undulation of the limbs of the two planets; the altitude of Venus being only 4° 48′, though the sun's limb was more distinct and steady than usual at that altitude. This conjecture seems corroborated by two circumstances; one is, that Venus's limb, from its first appearance to the total immersion, as well as afterwards, was very ill defined, and undulated very much; the other is, that her horizontal diameter, which Mr. M. measured soon after the internal contact, with an excellent achromatic object-glass micrometer, fitted to the 2-feet reflecting telescope, was only $55\frac{3}{4}''$, by a mean of 8 trials, or about 3″ less than it should have been, from the observations made with the like instrument, at the transit of Venus in 1761, by Mr. Short, Mr. Canton, Mr. Haydon, and Mr. Mason, when the sun was at a considerable altitude; and most likely the sun's diameter was enlarged in proportion, though it might have been difficult to have ascertained it by actual measure, had time allowed Mr. M. to make the experiment with the same micrometer before the sun entered into a black cloud near the horizon.

Six other persons also observed the contacts of Venus here, and noted some other phenomena. Their names are, the Rev. Malachy Hitchins, a gentleman well acquainted with astronomy and astronomical calculations, who has made and examined many belonging to the Nautical Almanac, and has been so obliging as to come here and assist in making astronomical observations, during the absence of Mr. M.'s assistant, Mr. Wm. Bayley, who is gone to the North Cape, by appointment of the R. S., to observe the transit of Venus there. The others are, the Rev. Wm. Hirst, who observed the former transit of Venus in 1761, at Madras; John Horsley, Esq. a gentleman whom Mr. M. had the pleasure of first commencing an acquaintance with during his voyage from St. Helena to England, in the Warwick East India ship, and who then, and in several voyages since to the East Indies and home again, observed and calculated the longitude from distances of the moon from the sun and fixed stars with the greatest accuracy; Mr. Samuel Dunn, who has had a good deal of practice in making astronomical observations, and who carefully observed the former transit of Venus, in 1761, at Chelsea; Mr. Peter Dollond, whose great skill in constructing achromatic and reflecting telescopes; and Mr. Edward Nairne, whose skill likewise in the same way, and in making all kinds of mathematical and philosophical instruments, are sufficiently known to the public.

Mr. Horsley and Mr. Dunn observed with Mr. M. in the great room; Mr. Hitchins and Mr. Hirst in the eastern summer-house; and Mr. Dollond and Mr. Nairne in the western summer-house; by 3 clocks placed in the respective rooms, which were compared with the clock in the transit room, before the external contact, and again after the internal contact was passed; whence the times of the observations, as noted by the clocks, were reduced to the time of the transit clock, and thence to apparent time. The observations are given in the following table, as reduced to apparent time.

	External contact.	Regular circumferences in contact.	Thread of light completed, or the internal contact.	Telescope made use of.	Magnifying power.
N. Maskelyne	7h 10m 58s	7h 28m 31s	7h 29m 23s	2 feet reflector	140
Mr. Hitchins	7 10 54	7 28 47	7 28 57	6 f. reflector	90
W. Hirst	7 11 11	— — —	7 29 18	2 f. reflector	55
J. Horsley	7 10 44	7 28 15	7 29 28	10 f. achromatic	50
S. Dunn	7 10 37	7 29 28	7 29 48	3½ f. achromatic	140
P. Dollond	7 11 19	— — —	7 29 20	3½ f. achromatic	150
E. Nairne	7 11 30	— — —	7 29 20	2 f. reflector	120

Mr. Dollond and Mr. Nairne used telescopes of their own construction; but they did not wait till the thread of light was formed at the internal contact, but noted the time when they judged it was just ready to be formed. The 3½ feet achromatic telescopes were those made with 3 object-glasses.

The differences between the different observations seem pretty considerable, and greater than Mr. M. expected, considering that all the telescopes may be reckoned pretty nearly equal, excepting the 6-feet reflector, which is much superior to them all; and to its greater excellence and distinctness he principally attributes the difference of 26^s by which Mr. Hitchins saw the internal contact before him; as he can depend on his observations. Possibly the greatness of the differences might arise from the low altitude of the sun and Venus; and then the like differences would not be so much to be feared in places where the observation may be made at higher altitudes; otherwise the sun's parallax will not be deducible from the transit of Venus with that accuracy which has been expected.

The other appearances about Venus, noted by the 6 observers, which they communicated to Mr. M., are as follows: Mr. Hitchins remarks, that at the first contact, though there was a tremulous motion in the sun's limb, yet that part of it which the planet entered was very well defined, and the first impression of Venus appeared to be instantaneous, and as a black, sharp point. At the internal coincidence of circumferences, the fluctuation of the sun's limb was increased, and the limb of Venus being affected in like manner, there was an uncertainty of about 10^s in estimating the said coincidence; but at the breaking in of the thread of light between the limbs, there was not a greater uncertainty than a second and a half of time. At the internal coincidence of circumferences, the limb of Venus next to that of the sun being protuberant, her vertical diameter appeared to be longer than the horizontal one; but when the sun approached the horizon, and was scarcely above a degree high, Venus's horizontal diameter appeared to be sensibly longer than the vertical, which was probably owing to refraction. After the internal contact, there appeared a luminous ring round the body of Venus, about the thickness of half her semidiameter; it was brightest towards Venus's body, and gradually diminished in splendor at greater distances, but the whole was excessively white and faint. This radiancy round the planet seemed to him to be greater in Mr. Nairne's 2-feet telescope than in the 6-feet Newtonian reflector.

After the 2d or internal contact, Mr. Hirst left off observing with Mr. Dunn's 2-feet reflector, and had a sight of Venus in the 6-feet Newtonian reflector in which he thought he perceived a glimmering of light about the upper part of the circumference of Venus, or that part of the planet which entered last into the solar disk. After Venus was got within the sun's disk, a light a little weaker than that of the sun, of a purplish colour, appeared to Mr. Horsley, to the left hand of Venus, which is really to the right, the telescope inverting objects. This light he saw for 6 or 7 minutes. From $7^h\ 28^m\ 26^s$ to $7^h\ 28^m\ 30^s$ apparent time, Mr. Dunn saw a very faint rim of light at Venus's exterior limb. After Venus was wholly on the sun, he saw a faint ring of light surrounding her, both with the $3\frac{1}{2}$-feet telescope, and Mr. Nairne's 2 feet reflector.

When $\frac{1}{2}$ of Venus's diameter was entered on the sun, Mr. Dollond first saw a light about the exterior limb of the planet; this light, during all the time of its continuance, appeared rather reddish, and in all respects like irregular refracted light. After Venus was wholly entered on the sun, he saw a faint ring surrounding her. After Venus was wholly entered on the sun, and her exterior limb was near one of her semidiameters distant from the sun's circumference, Mr. Nairne saw a faint light round the planet, rather brighter and whiter than the body of the sun.

Fortunately, the weather was as favourable for the observation of the sun's eclipse the next morning, as it had been the evening before for that of the ingress of Venus on the sun; which is of the more consequence, as the comparison of it with the observations which may be made of it in the northern and eastern parts of the world will serve to settle the longitudes of those places, and consequently render the observations which may be made there of the transit more useful and valuable. Mr. M. observed the beginning of the eclipse at $18^h\ 38^m\ 54^s$, and the end at $20^h\ 23^m\ 30^s$ apparent time, with the 2-feet reflector, using the magnifying power 90 times. And at $19^h\ 29^m\ 31^s$ apparent time, he observed the greatest eclipse, at which time he found the remaining lucid parts of the sun 15′ 15″ with Dollond's micrometer, assuming the horizontal diameter of the sun 31′ 31″, whence the value of the scale of the micrometer was determined for the present purpose. Hence the eclipsed parts of the sun were 16′ 16″ or 6° 11′.62 on the northern part of his disk.

Mr. Hitchens observed the beginning of the eclipse with a 3½-feet achromatic telescope magnifying 150 times (the same with which Mr. Dollond observed the contacts of Venus), at $18^h\ 38^m\ 59^s$, and the end of the eclipse with the 6-feet reflector with the magnifying power 90, at $20^h\ 23^m\ 35^s$ apparent time. And Mr. Samuel Dunn observed the beginning of the eclipse at $18^h\ 39^m\ 9^s$, and the end at $20^h\ 23^m\ 33^s$ with the other 3½-feet achromatic telescope, magnifying 140 times, the same with which he observed the contacts of Venus. Several inequalities in the moon's circumference, seen on the sun's disk during the eclipse, were distinctly discerned by all of them, the air being very clear, and the objects steady.

The whole series of measures of the lucid parts, which Mr. M. took with the achromatic object glass micrometer applied to the 2-feet telescope, was as annexed.

Apparent time.			Lucid parts.	
19^h	22^m	13^s	15′	40″.5
	24	21	15	26 .5
	26	9	15	20 .9
	28	26	15	15 .6
	30	14	15	14 .5
	31	44	15	16 .4
	32	30	15	16 .4
	33	19	15	19 .8
	34	28	15	25 .4
	36	19	15	35 .9
	37	56	15	49 .1

END OF THE FIFTY-EIGHTH VOLUME OF THE ORIGINAL.

I. A new Manner of preparing Salep. By Mr. J. Moult. Vol. LIX. p. 1.

The roots, says Mr. M., which I have hitherto made use of, are those of the orchis morio mas foliis maculatis of Parkinson, the cynosorchis morio mas of Gerard, and the cynosorchis major, vulgo dog-stones; though, from a specimen of the orchis palmata major mas of Gerard, which you have among the salep, that root likewise appears capable of being made to answer the same purposes as the others. The best time to gather the roots, is when the seed is formed, and the stalk going to fall; for when the new bulb, of which the salep is made, is arrived to its full size, and may be known from the old one, whose strength is then spent by the preceding germination, by a white bud rising from the top of it, which is the germ of the plant of the succeeding year. This new root, being separated from the stalk, is to be washed in water, and a fine thin skin, that covers it, to be taken off with a small brush; or, by dipping in hot water, it will come off with a coarse linen cloth. When a sufficient quantity is thus cleaned, they are to be spread on a tin plate, and set into an oven, heated to the degree of a bread oven, where they are to remain 6, 8, or 10 minutes; in which time they will have lost their milky whiteness, and have acquired a transparency like that of horn, but without being diminished in size. When they are arrived at this state, they may be removed to another room to dry and harden, which will be done in a few days; or they may be finished in a very slow heat in a few hours. I have tried both ways with success.

The orchises above-mentioned grow spontaneously in this part of the country, and throughout the whole kingdom. They flourish best in a dry, sandy, barren soil. As the method of curing this root is so easy, I hope it will encourage the cultivation of so nutritious a vegetable, so as to reduce it from its present high price, which confines it to people of fortune, to one so moderate as would bring it into common use, like other kinds of meal or flour; and so become a valuable addition to our present list of eatables, its quality of thickening water being to that of fine flour nearly as 2½ to 1, with this difference, that the jelly of salep-powder is clear and transparent, whereas that of flour is turbid and white.*

II. A Short Account of the Parabolic Burning Mirrors made by the late Mr. Hoesen of Dresden, now by Mr. Ehrard. By Dr. Wolfe. From the Latin. p. 4.

This concave parabolic segment is composed of many pieces of solid wood, and on the convex part are pieces both diverging from the vertex and transversely, nicely fitted and strengthened; the concavity is laid over with copper plates ⅛ of an inch in thickness, their length 4½ feet, and breadth 2½ feet, so as to appear

* Salep may be prepared from various other species of the orchis besides those mentioned in this paper.

like one piece finely polished. It was curiously supported, and so light in proportion to its magnitude, as to be easily managed and directed by one hand. The anterior part of the speculum is subtended by an iron arch, of ¼ an inch thick, in the middle of which, in the place of the burning focus, it was perforated into a ring, supporting from both sides an iron fork, to receive the body to be examined. Four of Mr. Ehrard's mirrors of this kind had the following dimensions:

N°	Perimeter. Ft.	Inc.	Diam. or ordinate. Ft.	Inc.	Depth or abscissa. Ft.	Inc.	Dist. of the focus. Ft.	Inc.
1	29	4	9	7	1	4	4	0
2	21	0	6	8	0	10½	3	1
3	16	4	5	1	0	10¼	1	10
4	13	2¼	4	2	0	7	1	9

The effect of these mirrors, Dr. W. says, in burning, calcining, melting, vitrifying, far exceeds any thing of the kind ever known. The hardest stones hardly resisted a few seconds; metals were quickly perforated; vegetables in the twinkling of an eye were burnt to a cinder and vitrified, and bones of animals the same; of which several instances as usual are recorded. One experiment in particular is, that if 2 of these mirrors be placed directly opposite to each other, and at a good distance, as 50 paces; and in the focus of one of them any words be whispered, or a pocket watch be placed there, those whispers, or the ticking of the watch, may be distinctly heard in the focus of the other mirror.

III. An Extraordinary Case of Three Pins Swallowed by a Girl, and discharged at her Shoulder. By Dr. Lysons, of Gloucester. p. 9.

Eleanor Kaylock, a strong girl, aged 22, was admitted a patient in the Gloucester infirmary, May 29, 1766, for a pain in her side proceeding from 3 pins swallowed ¾ of a year before. The occasion of the accident was thus: being employed in the business of a kitchen, as she was scumming the pot, her mouth being open, and 3 pins in it, she received a quantity of the vapour, which obliged her to swallow, and the pins at the same time passed into the œsophagus, where they remained for 8 weeks, notwithstanding various methods were used for their removal; but they were at last forced down by the whalebone instrument used by surgeons for that purpose. While the pins were in her throat, the parts became inflamed and swoln, which occasioned a hoarseness, attended with great pain, and difficulty of breathing; being also capable of receiving but very little nourishment, and that liquids, she was reduced to so weak a state as not to be able to get out of her bed. After the pins were removed she could swallow solids, and recovered strength sufficient to go out again to service in her former employment. She was hired as an under-servant in a gentleman's kitchen, but was soon obliged to quit her place, and apply for relief, any extraordinary motion aggravating her complaints, and occasioning violent convulsions, from which she did not recover for 8 or 9 hours. When she came to the infirmary, she appeared full of flesh, of a ruddy complexion, and in perfect health, excepting the following complaints.

She had a pain in her right side, below the false ribs, which she first felt immediately on the removal of the pins from the œsophagus, and it continued to the time of her admission at the hospital, but was most violent when she moved the trunk of her body forwards round towards the left, or lifted up her right arm. At her admission, and from the time of the removal of the pins, the hoarseness she was troubled with soon after the pins first stuck in her throat continued; she often spit up blood, and had a violent cough, by which, as well as by labour, or any excess of motion, the pain in her side being greatly aggravated, she was obliged to sit or fall down immediately, and could not recover herself, so as to be able to stand, in less than an hour. In these paroxysms she had always a pain in her head, was sick at stomach, and frequently brought up blood. While she was in the infirmary, the violence of the pain 3 times occasioned convulsion fits, by which the musculus rectus superior of the right eye was so violently affected, that, notwithstanding the eye was open, yet the pupil was entirely covered by the eye-lid; and, after one of these fits, continued so for a fortnight. The left eye was also inverted in the same manner, but the constriction was removed in a week. When these spasmodic affections left her, she did not recover her eye-sight for some days, the optic nerve being probably oppressed; but the left eye always recovered sooner than the right, being never so strongly convulsed. None of the other muscles appeared to be affected, except in the paroxysms.

While the pins were in the œsophagus, the surgeon was utterly at a loss where to direct his instruments, as there was no certain indication where the pins were lodged. And the physician's practice could be only palliative, using bleeding, with anodyne and lubricating medicines, according as the various symptoms occasionally required. In this manner things went on to the beginning of August, when a small painful tumour, the size of a man's thumb, appeared on the right shoulder, which disappeared in the compass of a week without coming to suppuration. Afterwards such another small tumour appeared on the left shoulder, which increased, and, by the care of Mr. Crump, the attending surgeon, was brought to suppuration, and opened by him, August 20, when a large table spoonful of matter was discharged. On removing the dressings the next day, a larger quantity of matter flowed out, and with it issued one of the pins. Mr. Crump then examined with his probe if he could find either of the others, but could not: however, the day following, the other 2 pins were also discharged at the same wound. These pins were all of the same length, each measuring $\frac{3}{4}$ of an inch. The wound at which these pins were discharged was on the superior part of the scapula. After the girl had received her cure, and was discharged from the infirmary, which happened Sept. 4, Dr. L. compared her shoulder with Cowper's Anatomical Tables on the Muscles; and, as near as he could guess, the wound was on the fleshy belly of the trapesius. And yet the pain in the patient's side attended her as long as the pins remained in the wound, but left her soon after they were discharged, as did also her cough, and spitting of blood. Being obliged to lead a sedentary life in the infirmary, and to keep herself as quiet as possible, her catamenia left her; but her spitting of blood could not be attributed to that defect, because she was very regular before her admission, and yet she had spit blood from the time the pins were removed from the œsophagus, which was some months before she came to the infirmary.

It would be matter of considerable satisfaction, could the exact course be ascertained which was taken by these pins, in their passage from the œsophagus to their exit at the left shoulder. From the cough and spitting of blood one should suppose that the lungs were injured by them. From the pain under the false ribs, it may be imagined that the diaphragm was affected. And yet from their being discharged at the shoulder it may be presumed, that neither of these parts were ever wounded; but that the pins, being forced through the substance of the œsophagus into the muscles of the neck and shoulder, passed thence to the part whence they were discharged.

Since Dr. L. drew out the above account, he had seen a case nearly similar to it, recorded in the Phil. Trans., N° 461. A small needle being lodged in a woman's left arm, about 6 inches below the shoulder, passed thence to her right breast, whence it was extracted many months after it first entered the body. About a month after the accident, she felt a pain above the place where the needle ran in, which extended up her shoulder. It lasted there 3 or 4 days, and then returned by fits. About 17 weeks before the needle was extracted, she felt a pain at her stomach, was sick, and had retchings to vomit. These symptoms continued to afflict her, especially in the morning, until within 2 days of the needle being extracted, at which time she thought a pin had got into her right breast. This directed the surgeon to make an opening there, and he extracted the same needle that had entered at her arm from the part where the pricking pain was; after which she had never any return of pain in her breast, stomach, shoulder, or arm.

IV. Further Particulars on Mount Vesuvius, and other Volcanos in the Neighbourhood. By the Hon. Wm. Hamilton. Dated Villa Angelica, near Mount Vesuvius, Oct. 4, 1768. p. 18.

All the peasants here agree in their account of the terrible thunder and lightning, which lasted almost the whole time of the eruption, on the mountain only. Besides the lightning, which perfectly resembled the common forked lightning, there were many meteors, like what are vulgarly called falling stars. A peasant, in his neighbourhood, lost 8 hogs by the ashes falling into the trough with their food: they became giddy, and died in a few hours. The last day of the eruption, the ashes, which fell abundantly on the mountain, were as white almost as snow; and the old people here say that is a sure symptom of the eruption being at an end. Sir William had been this summer in the island of Ischia; it is about 18 miles round, and its whole basis is lava. The great mountain in it, nearly as high as Vesuvius, formerly called Epomeus, and now San Nicolo, he is convinced was thrown up by degrees; and that the island itself rose out of the sea, in the same manner as some of the Azores. He is of the same opinion with respect to mount Vesuvius, and all the high grounds near Naples; having not yet seen, in any one place, what can be called virgin earth. He saw a well sunk a few days ago near his villa, at the foot of Vesuvius, and close by the sea-side. At 25 feet below the level of the sea they came to a stratum of lava, and God knows how much deeper they might have still found other lavas. The soil all round the mountain, which is so fertile, consists of stratas of lavas, ashes, pumice, and now and then a thin stratum of good earth, which good earth is produced by the surface mouldering, and the rotting of the roots of plants, vines, &c. This is plainly to be seen at Pompeii, where they are now digging into the ruins of that ancient city; the houses are covered, about 10 or 15 feet, with pumice and fragments of lava, some of which weigh 3 lb. (which last circumstance he mentions to show, that in a great eruption, Vesuvius has thrown stones of this weight 6 miles, which is its distance from Pompeii, in a direct line); upon this stratum of pumice, or rapilli, is a stratum of excellent mould, about 2 feet thick, on which grow large trees, and excellent grapes. We have then the Solfaterra, which was certainly a volcano, and has ceased emptying, for want of metallic particles, and over-abounding with sulphur. You may trace its lavas into the sea. We have the Lago d'Averno and the Lago d'Agnano, both of which were formerly volcanos; and Astroni, which still retains its form more than any of these. Its crater is walled round, and his Sicilian Majesty takes the diversion of boar-hunting in this volcano; and neither his Majesty, nor any one of his court, ever dreamed of its former state. We have then that curious mountain, called Montagno Nuovo, near Puzzole, which rose, in one night, out of the Lucrine Lake; it is about 150 feet high and 3 miles round. He does not think it more extraordinary that mount Vesuvius, in many ages, should rise above 2000 feet; when this mountain, as is well attested, rose in one night, no longer ago than the year 1538. It is composed of stratas, like mount Vesuvius, but without lavas. On the whole, if Sir W. was to establish a system, it would be, that mountains are produced by volcanos, and not volcanos by mountains. He adds, Vesuvius is quiet at present, though very hot at top, where there is a deposition of boiling sulphur. The lava that ran in the Fossa Grande during the last eruption, and is at least 200 feet thick, is not yet cool; a stick put into its crevices takes fire immediately. On the sides of the crevices are fine crystalline salts; as they are the pure salts, which exhale from the lava that has no communication with the interior of the mountain, they may perhaps indicate the composition of the lava.

V. On the Trees which are supposed to be Indigenous in Great Britain. By the Hon. Daines Barrington, F.R.S. p. 23.

Before entering into other particulars, Mr. B. lays down some general rules, from which it may be decided, whether a tree is indigenous or not in any country. 1. They must grow in large masses, and cover considerable tracts of ground; nor must such woods end abruptly, by a sudden change to other trees, except the situation and strata become totally different. 2. If the tree grows kindly in copses, and shoots from the stool, it must for ever continue in such a wood, unless grubbed up with the greatest care; nor is it then easily extirpated. 3. The seed of such tree must ripen kindly: nature never plants but where a succession may be easily continued, and in the greatest profusion. Lastly, many places in every country must receive their appellation from indigenous trees which grow there; as no circumstance is more striking, when a tract of ground is to be described or distinguished: hence so many towns, villages, and farms are named from the oak and ash, which are the most common trees of Great Britain. When the instances of this are singular, it will prove directly the contrary.

Having premised these general rules, by which it may be determined, whether a tree has been planted by the hand of nature or not; he begins by considering the proofs which are commonly relied on, with regard to the Spanish, or sweet chestnuts being indigenous in Great Britain. And, first, the very name of Spanish seems most strongly to indicate the country from which it was originally introduced here, as much as if a particular species of oak was known in Spain by the name of the English oak. There may be some doubts perhaps whether this tree is really a native of any part of Europe, as Pliny informs us, chestnuts were brought from Sardis to Italy, and that they were improved in taste by Tiberius, who took particular delight in cultivating them. Though so much has been said of late, with regard to the excellence of this wood for building, Mr. B. cannot, on inquiry, find that it is greatly prized for this purpose either in Spain, Italy, or the south of France; but it is chiefly valued for the fruit, which forms a considerable article of food for the inhabitants, as well as of exportation. Nor can he hear that this tree is to be found in any considerable masses, till the traveller is at least 200 miles to the south of Paris. With us the nuts by no means ripen kindly, though they are sometimes eaten very good from English trees. In Scotland, neither the walnut nor chestnut produce good fruit, though there are some very fine and promising timber-trees, of the latter kind, at the Earl of Bredalbane's, in the highlands. All these circumstances seem to afford a strong inference, that the Spanish chestnut cannot be a native of Great Britain. But he next considers the proofs which are generally adduced to the contrary.

Mr. Miller, in his Gardener's Dictionary, has endeavoured to prove, that the Spanish chestnut grew in great profusion to the northward of London, by a citation from Fitz-Stevens, which only implies, that there were large forests in the neighbourhood of the metropolis, without either the chestnut, or any other tree, being specified. Most antiquaries suppose, that Old London was chiefly built with this kind of timber from these forests; there is not the least appearance however of any such tree at present within 20 miles of London, which may not be accounted for, as being of infinitely a more modern introduction than the time of Henry the Second, when Fitz-Stevens wrote. Dr. Ducarel, in his Anglo-Norman Antiquities, has inserted a note of some length, to prove, that Old London was not only built with chestnut timber, but that there still continues a large tract of chestnut wood near Sittingbourn in Kent, which he conceives to be a full demonstration, that this tree is indigenous in England. Mr. B. had no sooner read this account, than he determined to examine these woods himself, as well as what trees might be found in their neighbourhood. The result of a very minute inspection of them is, that he found those parts which consist of Spanish chestnut to be planted in beds or rows, about 5 yards distant from each other; nor are there any scattering trees to introduce them, which is what must be expected near woods of natural growth. Dr. Ducarel next relies on a manor in the neighbourhood of Sittingbourne being called Chastenye or Castenye, from the circumstance of its being supposed to be among chestnut woods. This however is a single instance of such a name to any place in England; and therefore the chestnuts being indigenous can be no more inferred from it, than that box naturally grows in this country, from the name of Box-hill, in Surrey. Now we happen to know that this hill was so called from an Earl of Arundel's having introduced this tree there, in the time of James or Charles the First;* and, from many circumstances, Mr. B. supposes that the chestnut plantations near Sittingbourne are not of a much more ancient date.

The oldest tree we have any account of, perhaps in Europe, is a Spanish

* "This place (viz. Box-hill) was first planted by that famous antiquary (the Earl of Arundel), with box wood, designing to have built a house there; but want of water made him alter his resolution, and build one at Albury, hard by; now belonging to the Earl of Aylesford." Journey through England, vol. i. printed in 1722. See also the article Box-hill, in an Account of the Environs of London, printed for Dodsley.—Orig.

chestnut which grows in a court at Tortworth in Gloucestershire: it is supposed by Evelyn and Bradley to have been planted in the time of King John, from mention of it in deeds of that antiquity. Mr. B. however procured more accurate information from Lord Ducie, to whom this tree belongs; and finds that the notion of its great age rests merely on a very uncertain tradition. But though we should suppose it to have been planted in the time of King John, it affords no stronger argument of the tree's being indigenous, than those mentioned by Dr. Ducarel to grow at Hagley; especially as there are no straggling chestnuts to be found in the neighbourhood of either of these places.

Having dwelt thus long on the point of the Spanish chestnut's being indigenous or not, Mr. B. next adverts to some observations relative to the pine commonly called the Scotch fir, which certainly is not to be found in any part of England at present, except where the plantation appears most evidently to be of modern date. Cæsar indeed informs us, that no sort of fir was to be seen in this country at the time of his invasion. There are however so many well-attested facts, both by Camden and others,* of firs being found at a very considerable depth under the surface of the ground, that we cannot withhold our assent to them, extraordinary as it may appear at present, when throughout England we have no such trees, which affords the least grounds to contend that they are of indigenous growth. And the same in Scotland. There may be two causes assigned, why these bog-firs may be found in places where there is no such tree at present. The first is, that no pine or fir ever shoots from the stool; and the second, that being a resinous wood, it is very easily set on fire by lightning, after a dry summer; and thus whole tracts of them may be destroyed without their revegetating. Mr. B. was indeed informed by an old man at Ranoch-bridge, that his grandfather used to mention a tradition of the fir wood in that neighbourhood having continued burning for a considerable time, and that the Irish came over to see the conflagration. A wood of this kind is still growing near the western end of Loch Ranoch. There seems to be little doubt therefore that the fir was formerly an indigenous tree in the northern parts of England; nor does this contradict any of the rules before laid down, as they have been found in great masses under ground, and their not continuing to grow in the same spot or neighbourhood has been accounted for.

Mr. B. next mentions some other trees, which do not seem to be indigenous, though they are commonly conceived to be so, as well as by some great botanists, who have treated of English plants and trees. He cannot think that the elm, which we see every where, is indigenous. His reason is that he has never seen it out of a hedge-row, avenue, or clump, though it is a tree which shoots vigor-

* See Camden in Lancashire, and Phil. Trans. N° 67, where such subterraneous firs are said to be found in great quantities in the island of Axholm in Lincolnshire.—Orig.

ously from the stool. The wych, or broad-leaved elm, however, is certainly of natural growth in this country; though it is more common in Scotland, and the northern parts of England, than in the southern counties. For the same reasons, he cannot allow the lime to be indigenous, though in some years the seed becomes mature. The greater part of the limes, which we now see in every part of England, have been planted since the time of Charles the 2d, and were introduced by a French gardener, whose name was Le Notre, at the same time with the horse-chestnut. The greater maple, or sycamore, as it is improperly called, is certainly a foreign tree, though very common in Scotland, the northern counties of England, and some parts of Wales. He never saw the tree but in a hedge-row, avenue, or clump; it must be admitted, however, that its seed comes to its full perfection in almost every year.

Mr. B. next mentions some trees and shrubs, which he had doubts whether they are natives of Great Britain or not, though they are so considered by the botanists; he cannot pretend however to be so positive as in some of the former articles. He never saw the yew where it grew in large masses, or appeared to have been sown by the hand of nature. The most considerable numbers which he has happened to meet with are on some of the Surry hills: these however scarcely in any instance cover more than an acre of ground. If he should be right in supposing that this tree is of foreign growth, it may then be perhaps contended, that we have no ever-green tree or shrub which is indigenous, except the holly, the juniper, and the ivy. Every church-yard indeed proves, that this tree has been for many centuries introduced into England; it seems however very extraordinary, that we should have no account when, or for what purpose, this so very general a practice has so long prevailed with us. To the catalogue of doubtful trees, he also adds the abele, or white poplar, never having seen it growing according to the rules above laid down with regard to indigenous trees. He has likewise his suspicions with regard to the privet and spindle tree.

VI. An Account of a Case in which the Upper Head of the Os Humeri was sawed off, a large Portion of the Bone afterwards exfoliated, and yet the entire Motion of the Limb was preserved. By Mr. White, Surgeon, at Manchester. p. 39.

Reprinted in this Author's Cases in Surgery, 8vo. 1770.

VII. Extracts of Letters from the Rev. Dr. W. Borlase, F. R. S., Rector of Ludgvan, Cornwall, and from Mr. Rosewarne, of Truro, to Dr. Borlase; giving an Account of a Specimen of Native Tin found in Cornwall. p. 47.

Perhaps you may not, says Dr. B., have forgot that, in the year 1765, I sent a specimen of native tin, to be deposited in the R. S.'s museum; and though the account of it, published in the Transactions of the year following, was not such as I wish, yet I am steadily intent on paying my duty to the Society, and obviating (as far as lies in my power) all doubts relating to natural knowledge.

Last post I received a letter from Mr. Rosewarne, of this county, with an account of his having met with another specimen of native tin; and I send it inclosed, for your inspection, to be returned at your leisure.

To the Rev. Doctor W. Borlase.

Truro, Jan. 27, 1769.

I have the pleasure to acquaint you, says Mr. Rosewarne, that I have at last met with a specimen of native tin, which is so evidently so at first sight as not to admit of the least doubt or objection. The description of it is this: some streamers in the parish of Luxilion brought in a parcel of tin ore to a blowing-house I am concerned in at Sthurtle; among the parcel was a great number of tin diamonds of a most beautiful nature, of the rozin kind; one was eminently superior to the rest, being almost transparent, and seemed to have something in the centre which, through the stone, looked like gold. This induced me to break the stone; which was no sooner done than I found it to be native tin, in the very centre of the diamond. The specimen is so small, that I am at a loss which way to send it for your inspection, for fear it should be lost. I shall set out for London on Monday next, and intend carrying this curiosity with me. I'll not leave it behind me; but when I come back you shall see it, and through you deposit it with the R. S. for the satisfaction of the curious."

VIII. On the Origin of a Natural Paper, found near the City of Cortona in Tuscany. By John Strange, Esq., F. R. S. p. 50.

In August 1763, some low grounds, in a farm about four miles south-west of Cortona, which had been flooded, were found covered with a substance very much like a finer sort of common brown paper. A circumstantial account of the fact was communicated to the public the September following, in a letter from Mr. Coltellini to Dr. Lami, professor of theology at Florence. This account surprised, and excited the curiosity of the naturalists in Italy; and various were the conjectures offered on the occasion. The prevailing opinion however attributed the formation of this paper to a casual aggregate of the fibres of different kinds of filamentous plants, collected together by the waters, and left on the surface of the ground at their retreat. This supposition seemed plausible enough, since such a mechanism could be produced only by filamentous plants; most of which are commonly the spontaneous productions of such low, marshy ground. But on considering that, in the paper manufactures of different countries, various degrees and methods of maceration are requisite, according to the respective qualities of the fibres of different plants; it appeared very difficult to conceive, that a paper of so delicate and uniform a texture as that of Cortona should owe its origin to so complicated and remote a cause.

To bring the matter in question to a more certain issue, Mr. S. examined the threads of this paper with a good microscope; and found them to consist of mere filaments of the common species of conferva, without the intervention of any other plant whatever. It was easy enough to ascertain the identity of the conferva, the filaments of which it is composed being of a peculiar structure, and very different from those of any terrestrial plant; besides, as they are solitary in their natural state, they undergo no other alteration by the above mechanism, than the loss of the parenchyma that invests them, the structure of the filaments themselves remaining as perfect as ever.

To confess the truth, says Mr. S., I was but very superficially acquainted with this species of conferva till I had made the above discovery; since the descriptions of it, which we find in the books of botany, by no means afford an adequate idea of the structure of the plant. Dillenius, in his description of it, pretending to correct Pliny, for a supposed impropriety in the term fistulosæ densitatis, says, that there is no cavity observable either in this or other larger species of conferva, excepting, perhaps, in his conferva dichotoma; in which he is certainly mistaken; since the filaments of the common conferva, when examined with a good microscope, evidently appear to be capillary tubes divided at equal distances by parallel septa or diaphragms, exactly like the 25th species of the same genus in Dillenius's Tables. Pliny's epithet, therefore, so far from being improper, is a real characteristic of the thing in question.

As the systematical botanists generally take their leading characters from the external figures of plants, we need not be surprised to find them inaccurate in their descriptions of the smaller tribes; more especially as they neglect the use of proper glasses, by which alone they can acquire a knowledge of them. Dillenius and Linnæus himself have both been led into mistakes, from this omission. The former, in the preface to his Historia Muscorum, confesses, that he made use of common glasses only, in order that the figures of the smaller plants, which he was to represent in his Tables, might not deviate too much from the natural appearance of the plants themselves to the naked eye: and it is pretty evident that the glasses he used were but of moderate powers, since, besides other mistakes, they left him quite undetermined whether his 4th and 5th species of conferva had ramifications or not, though this very distinction forms a separate series in the first ordo. Linnæus's generical character of this plant is certainly less defective than that given by Dillenius, inasmuch as he takes notice of the tubercula omitted by the former, and calls the fibres of the conferva capillary; but as he does not expressly say, whether these fibres are tubes or not, and takes no notice of the septa or diaphragms distributed at equal distances along them, Mr. S. apprehends that he equally neglected examining the plant with proper glasses. Perhaps he adopted the term capillaris from professor Van Royen's Synonyme, which he quotes; especially since, in his divisions and specific characters of the confervá, he has fallen into the same mistakes with Dillenius, whom he chiefly followed in his class of the Cryptogamia. If the systematical botanists have not therefore acquired an adequate knowledge of the structure of the minuter confervæ, by neglecting to use proper glasses, their descriptions of these plants must necessarily be imperfect.

The specimens of paper, which Mr. S. sent with the copy of his letter, are, 1st. A specimen of the natural paper of cortona. 2dly. An artificial paper made of the same substance with the natural paper of cortona; which substance he proved to be the common conferva. 3dly. A specimen of a much better and stronger paper made of the same conferva, by Sir Alexander Dick, baronet, near Edinburgh. 4thly. A specimen of another artificial paper, which Mr. S. made of the genista juncea macerated in warm water, and prepared afterwards in the common manner.

IX. Experiments on the Lateral Force of Electrical Explosions. By Joseph Priestley, LL. D., F. R. S. p. 57.

Dr. P. being informed, in accounts of damages done by lightning, of persons and things being removed to considerable distances, without receiving any hurt, he was excited to try whether he could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power; but he did not know that this effect had ever been taken notice of by any electrician. The experiments he presently found to be very easy; and he thinks it not difficult to ascertain the cause of this striking effect, and the manner in which it is produced.

If pieces of cork, wood, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar, or battery, they will not fail to be moved out of their places, at the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described in the printed account of his experiments, and large corks be strewed along, or near the path intended for it, it is surprising to observe with what violence they will be driven about the room; and this dispersion is in all directions from the centre of the explosion; and it makes no difference whether the rods, between which it is made, be sharp-pointed or otherwise. The effect of this lateral force is very remarkable in attempts to fire gunpowder in electrical explosions. If the gunpowder be confined ever so close in quills or cartridges, and they be held fast in vices, yet, when the explosion is made in the centre of them, it will sometimes happen, even when a wire has been melted in the midst of the powder, and the fragments have been seen red-hot for some time in different parts of the room, that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity, makes it so extremely difficult to fire gunpowder by electrical explosions; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of explosion.

This lateral force is exerted not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly; and the smaller the wire, and the more complete the fusion, the greater is the dispersion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the explosion (in which case it generally assumes a dusky red, which lasts but for a moment), there was a small dispersion from every part of the wire, but by no means so great as it would have been if it had been melted, or only heated to a greater degree. By a considerable number of trials Dr. P. found, that a greater force of explosion would move light bodies at a greater distance; but the smaller the bodies were, the less was this difference; so that he supposed, that if they had no weight at all, they would probably be moved at the same distance by the explosion from any quantity of coated surface, charged equally high; but there was a great difference in the weights removed by different forces at the same distance. Placing the same piece of cork at the same distance from the place of explosion, he found that the discharge of one jar removed it $\frac{1}{4}$th of an inch, 2 jars $1\frac{1}{4}$th, three $1\frac{3}{4}$ths, and 4 about 2 inches; so that he does not wonder at very heavy bodies being moved from their places, and to considerable distances, by strong flashes of lightning.

That the immediate cause of this dispersion of bodies in the neighbourhood of electrical explosions, is not their being suddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged with it, is, he thinks, evident from the following experiments and observations. He never observed the least sensible attraction of these light bodies to the brass rods, through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though he used several methods which could not have failed to show it, if there had been any such thing. Sometimes he suspended them in fine silken strings, and observed that they had contracted no electricity after they had been agitated in the manner described above. Sometimes he dipped them in turpentine, and observed that no part of it was found sticking either to the brass rods themselves, or to any part of the table between them and the place where the light bodies had been laid. He even found that the explosion of a battery made ever so near to a brass rod did not so much as disturb the equilibrium of the electric fluid in the body itself: for when he had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, he found that the balls were not in the least moved at the time of explosion, which they would have been, if part of the electric fluid, natural to the body, had been driven, though but for a moment, towards the opposite end. He also observed, that the effect was the same, when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds interposed between the explosion and the bodies removed by it, as paper, tin-foil, and even glass; for when some grains of gunpowder were put into a thin phial, close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

Dr. P. therefore thinks it most probable, that this lateral force is produced by the expulsion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage; and this air, being displaced suddenly, gives a concussion to all the bodies that happen to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them. The only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. Dr. P. tried to make this experiment in a Torricellian vacuum, but could not succeed at that time. Besides, as the electric matter, of which an explosion consists, must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this lateral force was not, as far as he could perceive, much increased.

Willing to feel what kind of an impulse it was that acted on bodies, when they were driven away by this lateral force of electricity; Dr. P. held his finger near the path of an explosion of the battery, passing over the surface of a green leaf, when he felt a stroke, as of something pushing against his finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion. Recollecting that this power, which he now calls the lateral force of electrical explosions, must be the same with that which gives the concussion to water, mentioned in his experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes; and being determined to make a more satisfactory trial of it than he had ventured to do before, he laid a green leaf on the palm of his hand, intending to make the explosion pass over the leaf; but the leaf was burst, and torn to pieces, and the explosion, passing over his hand, gave it a violent jar, the effect of which remained, in a kind of tingling, for some time.

Lastly, in order to judge the most perfectly of this force, Dr. P. laid a chain communicating with the outside of the battery on his bare arm, above the wrist, and bringing the discharging rod near the flesh, within about 2 inches and a half of the chain, he made the explosion pass over that quantity of the surface of the skin. Had he taken a greater distance, he was aware that the explosion would have entered the flesh; which, he was sensible, would have given a painful convulsion to the muscles through which it passed. In this case the sensible effect was very different from that, being the same external concussion as before; and he sometimes thought, that the sensation is not disagreeable. However, the hairs on the skin were singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it: also the papillæ pyramidales of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain lay, had a redness which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.

X. Various Experiments on the Force of Electrical Explosions. By Joseph Priestley, LL. D., F. R. S. p. 63.

Making the explosion of a battery pass over the surface of a green cabbage-leaf, he observed that it left a track near ¼th of an inch in breadth, exceedingly well defined, and distinguishable by a difference of colour from the rest of the leaf. Along this path also the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after, it turned yellow, withered, and became perfectly brittle.

Willing to try the effect of this explosion passing along the surface of other substances, he laid a piece of common window-glass on the path, pressed by a weight of six ounces; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the blackside of a piece of cork-wood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of the piece of cork, it was furrowed all the way, as if it had been cut with a file, but not near so deep as before. Many of the small pieces, which had been rubbed off in the explosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off, a little way into it.

He made this explosion on the surface of some red wine, in a small dish, and kept a part of the same quantity exposed in a similar manner: but he could perceive no difference between them after several days.

The track of an electrical explosion on the surface of the cabbage-leaf, being so well defined, suggested an experiment to ascertain whether there was any sensible momentum in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose he made the explosion pass over the leaves when they were cut in right and acute angles; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle; and observed that it was not diverted from its course, in the least degree, by the rapidity of its own motion; but that it had turned exactly at the angle, and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage-leaf, having pursued them a little way, at least having sensibly affected them, wherever it met with them in its passage.

This experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being diverted from a right-lined course, and made to turn in a great number of angles. To do this, he first found, by a great number of trials, what length of a small iron wire he was able to melt with a battery of about 20 square feet, in the middle of a circuit of about 3 yards of brass wire, considerably thicker than the iron, and stretched in two right lines, suspended on silken strings. The length of the iron wire, melted in these circumstances, was about 3 inches. He then took the same brass wire, and fixing pins into a board of baked wood, twisted it about them, making it turn in a very great number of acute angles; and he put 3 inches of the same iron wire in the middle of this crooked circuit, that he had done in the straight one; so that the electric matter in the explosion was obliged to make a great number of turns at acute angles, before it could come to the iron wire; but he always found that the same length of iron wire was melted in these circumstances as in the other, and not the least difference was perceived in the force. But though the form of the wire through which an explosion passed, made no difference in its force, he found a very remarkable difference occasioned by the length of the circuit in wires of the same thickness; and which surprised him very much.

To ascertain the practicability of firing mines by electrical explosions, Dr. P. took 22 yards of small brass wire (but so thick, however, that he could not have melted the least part of it by the force of any battery he had ever constructed), and extending it along a dry boarded floor, with a small piece of iron wire, and a cartridge of gunpowder about it, in the place that was most remote from the battery; he found that, on the discharge, the wire was not melted, nor the gunpowder exploded; also the report was very faint. In other circumstances, a charge of the same battery was able to melt more than 9 inches of this iron wire; and this same cartridge was easily fired near the battery, connected with shorter pieces of the same brass wire; so that the diminution of force must have been owing to the length of the circuit.

In the place of this small brass wire, Dr. P. substituted an iron wire $\frac{1}{5}$th of an inch thick, when about half an inch of the small iron wire was exploded; so that the force was not lessened so much in a circuit of the thick iron wire, as it had been in one of the small brass wires. To judge how much of the force might be lost by nearer circuits, consisting of less perfect conductors, he joined the middle of the circuit made by the iron wire with water, in which both the wires were immersed. The effect was, that the small iron wire was only made red-hot, but not exploded as before. Being sensible how much depended on avoiding lesser circuits, by which part of the fire of an explosion might return to the battery, without reaching the extremity of the circuit, where he intended the whole of its force to be exerted, in the remaining experiments, he insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit; for if there was but one passage to, or from the middle of it, there could be but one from, or to it. In this method it was easy to ascertain what loss of force was occasioned by the length of the circuit, as every other circumstance was carefully excluded; and it presently appeared to be very considerable; for though he could melt 9 inches of the small iron wire at the distance of 15 yards from the battery; when he tried 20 yards, he found that he was just able to make 6 inches of it red-hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by silken strings into a garden adjoining to the house.

Mentioning this loss of force occasioned by the length of the circuit in electrical explosions to Dr. Franklin, he said that the same observations had occurred to him, and that he had also been disappointed in an attempt to fire gunpowder at a distance from his battery. Struck with this appearance, Dr. P. endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric fire would chuse preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire, mentioned above, he disposed it in the manner described below, connecting one of the ends with the outside of the battery, and the other with the inside. In the first place, he brought the parts near the two extremities into contact, and, on the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter it again in another. Afterwards he removed these parts to a small distance from one another, and, on the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, he found the explosion to pass above $\frac{1}{3}$ of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. He then took 4 or 5 yards of iron wire $\frac{1}{16}$th of an inch thick, when the passage through the air was still half an inch; and taking $3\frac{1}{2}$ yards of wire that was $\frac{1}{5}$th of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance, or $\frac{1}{4}$th of an inch. When he kept the place of near contact about the middle of this wire, and made the explosion at the extremities of the whole wire, he was obliged to bring them about as near again, i. e. to little more than the 8th of an inch, before the passage would be through the air; so that the force of the whole explosion must have been greatly weakened by its passing through so much of the wire. Lastly, he took a pair of kitchen tongs, the legs of which were 2 feet, and the smallest part of them above half an inch in diameter; when the circuit was made about $\frac{1}{6}$th of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through 4 feet of that thick iron.

Notwithstanding this evident passage of the electric matter through the air, at the same time that a metallic circuit was provided for it; it was certain that the whole of the charge did not pass in the air: for when he extended $\frac{1}{3}$ of an inch of small iron wire between the nearest parts, it was only made ret-hot by the discharge; whereas above 2 inches of it would have been exploded, if there had been no other metallic circuit at all.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, Dr. P. makes no doubt but that it is considerably obstructed in passing through metallic circuits of any thickness whatever; and that it would prefer a very short passage through the air, if they were made even of no great length. In this method the different degrees of conducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N. B. A common jar answers as well, in these experiments, as a large battery. It is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit; but that, if inferior circuits be open, part will pass in them at the same time. Of this Dr. P. made the following satisfactory trial. He took an iron chain, and laid it upon a table, in contact with a charged jar; so that the parts of it made two circuits for the discharge, which he could vary at pleasure; and he observed that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the charge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the shortest circuit only.

It is evident, that when the wires of a battery are not in close contact, there must be some loss of force in the discharge; but this never appeared to be very considerable. To ascertain it by experiment, he first found, by repeated trials, what length of a piece of iron wire he was able to melt with a battery consisting of 20 jars, with the wires and connecting rods quite loose, and a chain to join the rods belonging to each row of jars, which is the manner in which he had generally constructed them. In these circumstances, he found the battery was able to melt something more than 2 inches and a half of the wire. He then soldered the wires of each jar to the rod which connected them, and also soldered another rod to all these, instead of the chain which he had used; so that he avoided near 100 sparks in the discharge, at each of which there must have been some loss of force; but he did not find, after many trials, that the strength of the battery had been thus sensibly diminished: for he could not melt 3 inches of the same piece of wire in these circumstances. It was only made red hot, which is equivalent to the melting and exploding of little more than 2 inches and a half.

XI. Account of an Earthquake at Macao, and a short Description of a singular Species of Monkeys without Tails, found in the Interior Part of Bengal. By Stephen De Visme, Esq. at Canton, in China.* p. 71.

The following account of an earthquake, at Macao, was sent to Mr. D. from that place, in a letter, dated Nov. 23, 1767, viz. "Last night, at 50 minutes after 9 o'clock, we were all surprized with a heavy shock of an earthquake which continued above a minute. This shock was so great that the house rocked, and I was afraid we were all going down into the bowels of the earth. Another shock we felt 5 minutes after 11 o'clock, but not so great: and at 3 this morning another pretty great. In all we have had 5 shocks, but the first the greatest. It came with a rolling, and a dreadful noise in the air; so that at first some people thought it to be the firing of guns, or thunder at some distance. At the first shock I could hardly hold my feet; but, thank God, no bad accident has happened. The wind was northerly, but faint, and it was sultry hot; the sky close and cloudy, and not a star to be seen. The oldest people here say, they never remember to have felt so violent a shock, and of so long continuance. The ships in the harbour shook and whirled about, and those on board imagined at first that it had been a whirlwind."—At Manilla earthquakes are often very violent, so as to overturn steeples, houses, and other buildings; and I observed, when I was there, that, to prevent such accidents,

* The species of ape here mentioned by Mr. De Visme is the Simia Lar, once described by Linnæus under the name of Homo Lar. It is figured in Miller's plates of Natural History, pl. 27.

their timbers in building are placed in a very particular manner; they have no attic story, only warehouses, and one floor over them.

Perhaps the drawing, now sent you, of a singular sort of monkeys, male and female, may not prove unacceptable. These animals are called golok, or wild people, and are thought to be originally a mixture with the human kind, having no tails. They come out of the forests in the interior part of Bengal, from the country called Mevat. They inhabit the woods: their food is fruit, leaves, bark of trees, and milk: flesh only when caught. They are very gentle, and extremely modest. They are of the height of a man; their teeth are as white as pearls; and their legs and arms are in due proportion to their body.*

XII. Demonstration of a Law of Motion, in the Case of a Body Deflected by Two Forces tending constantly to Two Fixed Points. By Mr. John Robertson, Lib. R. S. p. 74.

The late Mr. Machin, who was, for many years, Sec. R. S.; and Gresham Prof. of Astronomy, gave to the editor of the English edition of Sir Isaac Newton's Principia, published in the year 1729, a tract entitled, " The Laws of the Moon's Motion according to Gravity," which was annexed to that impression: Mr. Machin, in the postscript to that tract, after apologizing for not mentioning the fundamental principles of the demonstration of the propositions relating to the moon's motions, says, " Some of which, I am apt to think, cannot easily be proved to be either true or false, by any methods which are now in common use." One of these principles he gives in the following words: There is a law of motion which holds in the case where a body is deflected by two forces, tending constantly to two fixed points.

" Which is, That the body, in such a case, will describe, by lines drawn from the two fixed points, equal solids in equal times, about the line joining the said fixed points."

And, after observing that Sir Isaac Newton has proved, that Kepler's law of bodies describing equal areas in equal times about the centres of their revolution

* The monkey, of which Mr. De Visme has sent this drawing and short account, seems to be very like, if not the same with the ape without a tail, described by Mr. De Buffon in the 14th volume of the Histoire Naturelle, p. 92, under the name of the Gibbon, which it bears in some parts of the East-Indies. This species is found, he says, along the coast of Coromandel, at Malacca, on the Molucca islands, and on the confines of China. It grows to be upwards of 4 feet, walks on its hind legs, and sometimes on all four. The hair, with which it is covered, is either brown or black: round about its face is a circle of greyish hairs; its eyes are large, but sunk in its head; its ears naked; its face flat, and of a copper colour. It is of a placid disposition; its motions are gentle; it was fed with bread, fruits, almonds. But the most singular characteristic is, the great length of its arms; and though Mr. De Visme takes no notice of this circumstance in his description, his drawing seems to indicate it; but in a less striking manner, than that of Mr. De Buffon, who adds, that, when the animal is upright, it can touch the ground with its hands.—Orig. M. MATY.

cannot hold, whenever the body has a gravity or force to any other than one and the same point, further says, "there seems to be wanting some such law as I have here laid down, that may serve to explain the motions of the moon and satellites, which have a gravity towards two different centres."

About the year 1742, discoursing with that eminent mathematician, the late William Jones, Esq. F. R. S. on the above-mentioned law, he showed me its demonstration, and permitted me to take a copy of it; and which I conceive to be highly worth preserving.

Prop.—If a body P, fig. 1, pl. 17, projected in a given direction, be constantly drawn towards two fixed points, S and T, which are not both in the same plane with the direction, the triangle SPT, formed by right lines drawn from the body P to those fixed points S and T, shall describe equal solids STPP′, STP′P″ in equal times, about the right line ST joining the said points.

For, suppose a body projected in the direction PP′, fig. 2, and acted on by two centripetal forces towards the fixed points S and T; the angles P′PS, P′PT lying in different planes. Let the time be divided into equal moments. In the first moment, suppose the body, by its given force, should move along the line PP′; and in the second moment, if no new force was added, it should continue to move in the same right along $P'p = PP'$; but when the body has come to P′, suppose it acted on by the two centripetal forces, in the directions P′T, P′S; and let those forces be in proportion to that in the direction PP′, as the lines $P't$, $P's$ to the line $P'p = P'P$. With these three right lines $P'p$, $P't$, $P's$, complete the parallelopipid P′P″; and the body in P′, being acted on by these three forces, in the directions $P'p$, $P't$, $P's$, which forces being as these three lines, shall move along the diagonal of the parallelopipid made by these three lines; so that, in the second moment of time, the body, instead of moving from P′ to p, shall move from P′ to P″.

Draw the lines SP′, SP″ and TP′, TP″, as also sp, Tp. Now, the solid STPP′ = solid $STP'p$; for they stand on equal bases TP′P, $TP'p$, and have one common vertex S, or their common altitude is the perpendicular drawn from S to the plane PTp. And the solid STP′P″ = solid $STP'p$; for they stand on the same base STP′ and lie between the same parallel planes pP'', st. Therefore the solid STPP′ = solid STP′P″.

In like manner, in the third moment of time, the body at P″ being acted on by three forces, in the directions P′P″, P″S, P″T, shall move along the line P″P‴, so as to make the solid STP″P‴ = solid STP′P″; and so in all succeeding equal moments of time, the triangle formed by right lines drawn from the body to the two fixed points ST, shall constantly describe little solids, each equal to the solid STPP′. Therefore the moments of the solids being proportional to the moments of the time in which they are described; the solid itself is proportional to the time in which it is described. Q. E. D.

Some difficulties may perhaps seem to arise on a slight view of only particular cases of this proposition; but it is conceived all such must vanish, when the same is thoroughly considered. For, as in two bodies T and S; if T is acted on by S, so as to describe a right line, that is, if T falls directly on S, no area can then be described by the right line connecting T and S; but yet, this is certainly one of the cases by which S and T may possibly act upon each other. So in three bodies, S, T, and P; if P moves in the same plane with S and T, no solid can then be described by the plane whose right lined sides are the lines connecting P to T and S; but yet, this must be one of the cases by which S, T, and P, may possibly act on each other.

XIII. On the Effects of Lightning on Buckland Brewer Church. By the Rev. William Paxton, Rector of that Parish. p. 79.

On Thursday, March 2, about 4 in the afternoon, a cloud, of a most uncommon blackness, gathered in the west-north-west, and, taking its course to the east-south-east, diffused a most prodigious darkness, accompanied with a very copious shower of hail. It passed immediately over the church tower, remarkable for the height both of its situation and structure, and, bursting with incredible fury, poured forth an amazing body of fire, which threw down the south-east pinnacle on the church, and entering, it seems, at the breach, shivered a table on which the commandments were written, scorched and discoloured two tomb-stones, broke the windows, and shattered the walls and roof to a great degree. The south-east corner suffered most; where it chiefly forced its way, and tore up the ground on the outside, where it found vent. There is something very extraordinary in the dispersion of the stones of the pinnacle to every point of the compass, and to different distances; some of which were 700 pounds weight. Mr. P. picked up one that weighed almost 8 pounds, at the distance of 60 perches from the church; and doubts not but others, and perhaps larger stones, were carried farther. Several of the stones, some of which were not small, though they appeared close and firm, yet, on a very slight impression of the fingers, mouldered into powder. The explosion, on the opening of the cloud, was as instantaneous as terrible, and equalled the discharge of at least a hundred cannon at once.

XIV. Abstract from a Meteorological Register kept at the Royal Hospital, near Plymouth, during the Year 1768. By W. Farr, M. D. p. 81.

This is a register, for several days in each month of the year 1768, of the barometer, thermometer, and depth of rain that fell, which was 51.215 inches in the whole year.

XV. Two remarkable Auroræ Boreales, observed at the Observatory of the

Marine at Paris. By M. Messier, of the Royal Acad. of Sciences, and F. R. S. Translated by J. Bevis, M. D., F. R. S. p. 86.

The morning of the 6th of August, 1768, was for the most part, serene, and the afternoon was quite so. At near 9 at night, the western horizon was illuminated with a very sensible twilight, which increased greatly on that which the sun had left. About 10 o'clock, the sky being perfectly clear, excepting one thick cloud, the aurora was considerable; several streams of light had then shot up from the horizon. At half after 10, the aurora occupied nearly one half of the horizon, extending from the west to the north-east, and the horizon seemed to be covered with an uneven thick smoke, from which issued several streamers of light; two of which, to the westward, arose to a great height, passing through the tail of the Great Bear, and were sensibly inclined to each other, tending to unite in the zenith. Both these luminous streamers kept in a continual agitation, which lasted the whole time of their existence, that is till 11 o'clock. At the foot of these lights was the furnace, which glowed with rays of light less elevated, and sensibly inclined to the horizon; these were also in continual agitation. At 11, 6 streamers, parallel to one another, shot up in the north, under the constellation of the Little Bear; they ascended not so high, but were more conspicuous than the two preceding ones, and their undulations were not so quick. About half past 11, the sky began to be clouded; at midnight it was so all over.

Dec. 5, 1768, about 7 in the evening, the northern quarter was enlightened with an incipient aurora borealis, which increased gradually. At 11 at night it was very conspicuous, numbers of luminous streamers darting up from below the horizon; some of them reaching the zenith; but none of the streamers lasted any considerable time, and their light was but feeble. Several whitish clouds appeared in the north; the furnace occupied a great part of the horizon. This phenomenon lasted almost the whole night.

The same aurora was observed at Berlin, where it lasted from 6 in the evening till 9. It was also observed at Vienna, where it is said that the needle of a compass lost, during this phenomenon, its usual direction, shifting at first 2 degrees eastward, and afterwards 4 degrees the contrary way; it was at the same time remarked that the electrical machine had acquired an uncommon degree of force.

XVI. Observations on the Expectations of Lives, the Increase of Mankind, the Influence of great Towns on Population, and particularly the State of London with respect to Healthfulness and Number of Inhabitants. By Mr. Richard Price, F. R. S. p. 89.

For these observations, see p. 167, of Dr. Price's Treatise on Reversionary Payments, 2d edit. 1772; where it is re-published with corrections, and several additions, particularly a postscript.

XVII. Dissertatio epistolaris de Ossibus et Dentibus Elephantum, aliarumque Belluarum in AmericaSeptentrionali, aliisque borealibus Regeonibus obviis; qua indigenarum Belluarum esse ostenditur. Auct. R. E. Raspe. p. 136.

Mr. Raspe in this Latin paper, after observing that the bones of elephants, and other large animals allied to them in general appearance, are found in various parts of the northern world, and that the hypotheses relative to their translation from warmer climates are untenable, concludes by giving it as his own opinion, that such animals were in reality natives of the regions in which their remains are found; and that they have been gradually extirpated in the earlier ages by the efforts of mankind, as has been the case with several animals now no longer found in countries in which many centuries ago they abounded. Thus the wolf, the bear, and some other animals, mentioned by Cæsar as found in particular parts of Germany, have been long ago extirpated.

With respect to the bones themselves, Mr. Raspe observes, that the hinge of the question turns on their particular nature, viz. whether truly elephantine, or allied to such animals as could not be supposed to have lived in cold climates. The bones from North America undoubtedly are not those of common elephants, as is plain from the form of the grinders. That they were not those of marine animals seems clear from their being never found imbedded among marine substances; and that they were not transported by the waters of the Noachic deluge seems clear for a similar reason. But surely, he adds, we may suppose, that some very large species of elephant of a different kind from those which are natives of warm climates, might have formerly existed in cold climates, and that they have gradually become extinct. As to the question, how it comes to pass that such a number of skeletons should be found in so small a space as the particular swampy spot in which they are discovered near the banks of the Ohio, Mr. R. imagines that, seduced by the delight of licking the salt soil in that spot in considerable numbers, they might have suddenly sunk by their own weight, and have been thus destroyed.

XVIII. Observations on a Particular Manner of Increase in the Animalcula of Vegetable Infusions, with the Discovery of an Indissoluble Salt arising from Hempseed put into Water till it becomes Putrid. By John Ellis, Esq., F. R. S. p. 138.

Having, at the request of Dr. Linnæus, made several experiments on the infusion of mushrooms in water, in order to prove the theory of Baron Munchausen, that their seeds are first animals and then plants; which he takes notice of in his System of Nature, p. 1326, under the genus of Chaos, by the name of Chaos fungorum seminum: it appeared evidently that the seeds were put into motion by very minute animalcula which proceeded from the putrefaction of the mushroom; for by pecking at these seeds, which are reddish, light, round bodies, they moved them about with great agility in a variety of directions, while the little animals themselves were scarcely visible, till the food they had eate had discovered them. The satisfaction Mr. E. received from clearing up this point led him into many other curious and interesting experiments. Mr. E. looked carefully over Mr. Turbervill Needham's very ingenious memoir on thi subject, vol. 45, p. 615, of the Phil. Trans., he means as to the experiments, many of which succeeded with him, some not.* I own, says Mr. E. his reason-

* The ingenious Mr. Needham supposes, those little transparent ramified filaments, and jointed or coralloid bodies, which the microscope discovers to us on the surface of most animal and vegetable infusions when they become putrid, to be zoophytes or branched animals: but to me they appear, after a careful scrutiny with the best glasses, to be of that class of fungi called mucor or mouldiness, many of which Michelius has figured, and Linnæus has accurately described. Their vegetation is so amazingly quick, that they may be perceived in the microscope, even to grow and feed under the eye of the observer. Mr. Needham has pointed out one that is very remarkable for its parts of fructification. This, he says, proceeded from an infusion of bruised wheat.

Mr. E. has seen the same species arise from the body of a dead fly, which was become putrid b lying floating for some time in a glass of water, where some flowers had been, in the month of August, 1768. This species of mucor sends forth a mass of transparent filamentous roots, whence arise hollow stems, that support little oblong-oval seed vessels with a hole on the top of each; from these he could plainly see minute globular seeds issue forth, in great abundance, with an elastic force, and turn about in the water as if they were animated. Continuing to view them with some attention, Mr. E. could just discover, that the putrid water, which surrounded them, was full of the minutest animalcula, and that these little creatures began to attack the seeds of the mucor for food, as he had observed before in the experiment on the seeds of the larger kind of fungi or mushrooms. This new motion continued the appearance of their being alive for some time longer: but soon after many of them arose to the surface of the water, remaining there without motion; and a succession of them afterwards coming up, they united together in little thin masses, and floated to the edge of the water, remaining there, quite inactive during the time of observation. As this discovery had cleared up many doubts, which Mr. E. had conceived from reading Mr. Needham's learned dissertation, he put into the same glass several other dead flies, by which means this species of mucor was propagated so plentifully, as to give him an opportunity of frequently trying the same experiment to his full satisfaction.

Lastly, those jointed coralloid bodies, which Mr. Needham calls chaplets and pearl necklaces, Mr. E, has seen frequently very distinctly. These appear not only on an infusion of bruised wheat, when it becomes putrid, but on most other bodies, that throw up a viscid scum, and are in a state of putrefaction. These then are evidently no more than the most common mucor, the seeds of which are every where floating in the air; and bodies in this state afford them a proper and natural soil to grow on. Here they send downwards their fine transparent ramified roots into the moisture which they float upon, and from the upper part of the scum their jointed coralloid branches rise full of seed into little grove-like figures. When a small portion of these branches and seeds are put into a drop of the same putrid water the scum floats upon, many of the millions of little animalcula, with which it abounds, immediately seize them as food, and turn them about with a variety of motions; as in the experiment on the seeds of the common mushrooms; either singly or two or three seeds connected together, answering exactly to Mr. Needham's description; but evidently without any motion of their own, and consequently not animated.

I am satisfied, says Mr. E. that Mr. Needham's observations have convinced him long before this, that they must be vegetables: for my part, I own I have never seen a zoophyte extend its branches, ing is very specious and plausible, but too metaphysical for a natural historian. Yet I cannot forbear relating one of the experiments which I tried in consequence of his discovery, that animalcula were produced in various infusions, notwithstanding the greatest heat was given to the liquor.

On May 25, 1768, Fahrenheit's thermometer 70 degrees, I boiled a potatoe in New-river water till it was reduced to a meally consistence. I put part of it, with an equal proportion of the boiling liquor, into a cylindrical glass vessel that held something less than half a wine pint, and covered it close immediately with a glass cover. At the same time, I sliced an unboiled potatoe, and, as near as I could judge, put the same quantity into a glass vessel of the same kind, with the same proportion of New-river water, not boiled, and covered it with a glass cover, and placed both vessels close to each other. On May 26, 24 hours afterwards, I examined a small drop of each by the first magnifier of Wilson's microscope, whose focal distance is reckoned at $\frac{1}{50}$th part of an inch, and to my amazement they were both full of animalcula of a linear shape, very distinguishable, moving to and fro with great celerity; so that there appeared to be more particles of animal than vegetable life in each drop. This experiment I have repeatedly tried, and always found it to succeed in proportion to the heat of the circumambient air, so that, even in winter, if the liquors are kept properly warm, at least in 2 or 3 days the experiment will succeed.

In Mr. Needham's experiments he calls these spermatic animals; what I have observed are infinitely smaller than real spermatic animals, and of a very different shape; the truth of which every accurate observer will soon be convinced of, whose curiosity may lead him to compare them; and I am persuaded he will find they are no way a-kin to that surprising part of nature. And though some philosophers of great reputation have agreed in sentiment with Mr. Needham, yet I am satisfied, that whenever this subject is taken up again, and properly attended to, the world will be convinced they have been too hasty in their conclusions.

At present I shall pass over many other curious observations, which I have made on two years experiments, in order to proceed to the explaining a hint, which I received last January from Mr. De Saussure, of Geneva, when he was here; which is, that he lately found one kind of these animalia infusoria, that increases by dividing across into nearly two equal parts. I had often seen this appearance, in various species, a year or two ago; but always supposed the animals in this dividing state to be in coition. Not hearing till after M. De Saussure had left this kingdom, from what infusion he had made his observation;

and grow out of water. I hope I have already cleared up that point, in showing the absurdity of Dr. Pallas's Corallina terrestris, Phil. Trans. vol. 57, p. 415.—Orig.

his friend, Doctor de la Roche, of Geneva, informed me, the latter end of February last, that it was from hempseed.

I immediately procured hempseed from different seedsmen, in distant parts of the town: some of it I put into New-river water, some into distilled water, and some I put into very hard pump-water; the result was, that in proportion to the heat of the weather, or the warmth in which they were kept, there was an appearance of millions of minute animalcula in all the infusions; and some time after, some oval ones made their appearance, as at pl. 18, fig. 1, *b. c.* These were much larger than the first, which still continued; these wriggled to and fro in an undulatory motion, turning themselves round very quickly, all the time that they moved forwards. I was very attentive to see these animals divide themselves; and at last I perceived a few of the appearance of Fig. 1, *a*, as it is represented by the first magnifier of Wilson's microscope; but I am so well convinced by experience, that they would separate, that I did not wait to see the operation: however, as the following sketches, which I have drawn from 5 other species, will very fully explain this extraordinary phenomenon, there will be no difficulty in conceiving the manner of the first. See fig. 2, 3, 4, 5, and 6.

The proportion of the number of the animals, which I have observed to divide in this manner, to the rest, is scarcely 1 to 50: so that it appears rather to arise from hurts received by some few animalcula among the many, than to be the natural manner in which these kind of animals multiply: especially if we consider the infinite number of young ones which are visible to us through the transparent skins of their bodies, and even the young ones that are visible in those young ones, while in the bodies of the old ones.

But nothing more plainly shows them to be zoophytes than this circumstance; that when, by accident, the extremity of their bodies has been shrivelled for want of a supply of fresh water, the applying more fresh water has given motion to the part of the animal that was still alive; by which means this shapeless figure has continued to live and swim to and fro all the time it was supplied with fresh water.

I cannot finish this part of my remarks on these animals, without observing, that the excellent Linnæus has joined the beroe with the volvox, one of the animalia infusoria. The beroe is a marine animal found on our coasts, of a gelatinous, transparent nature, and of an oval or spherical form, about half an inch to an inch diameter, divided like a melon into longitudinal ribs, each of which is furnished with rows of minute fins, by means of which this animal, like the animalia infusoria, can swim in all directions with great swiftness. In the same manner I have seen most of these minute animals, which move so swift that we could not account for it, without supposing such a provision of nature, which is really true; but cannot be seen till the animals grow faint for want of water; then, if we attend, we may, with good glasses, plainly discover them.*

I come now to a singular property, which I have discovered in hempseed, of producing an indissoluble salt, when infused for some time in water: and as hempseed is known to be an efficacious medicine in some particular cases, these experiments may demand a stricter inquiry from the professors of physic, which may possibly turn to the benefit of mankind.

Exper. I. On Feb. 25, last, Mr. E. put half an ounce of hempseed to about 2 ounces of New-river water in a phial, and covered it close with paper, to prevent the dust coming to it: by the 25th of March it became very putrid, and had thrown up a viscid scum to the top. Fahrenheit's thermometer in the house was, during this time, from about 44 to 52 degrees. He examined this scum with a common magnifier, of about an inch focus, and could discover it to be full of regular-shaped salts, which lay on the surface; some of a square, others of an oblong figure. Applying some of the scum to a slip of glass, he placed it in the single or Wilson's microscope, making use of the 4th magnifier, and it exhibited the crystals represented at fig. 7, pl. 18; but as the stirring of the scum had obscured the precise figure of the salts, he applied a hair pencil to them, dipt in clean river water, and separated them from the mucilage that had besmeared them; yet, notwithstanding this addition of water, their figures were not in the least impaired or melted, but their outlines were rather more exactly defined. Nor were the millions of minute animals that were swimming over them, and all round them, in the least affected by the salt.†

Mr. E. further observed, that the crystals that appeared, first increased in size, and began to vary their forms; for instance, many of the crystals, at the latter

* I have lately found out, by mere accident, a method to make their fins appear very distinctly, especially in the larger kind of animalcula, which are common to most vegetable infusions, such as the terebella; this has a longish body, with a cavity or groove, at one end, like a gimblet: by applying then a small stalk of the horse-shoe geranium, or geranium zonale of Linnæus, fresh broken, to a drop of water in which these animalcula are swimming, we shall find, that they will become torpid instantly, contracting themselves into an oblong-oval shape, with their fins extended like so many bristles all round their bodies; the fins are in length about half the diameter of the middle of their bodies. Before I discovered this expedient, I tried to kill them by different kinds of salts and spirits; but though they were destroyed by this means, their fins were so contracted, that I could not distinguish them in the least. After lying in this state of torpidity for two or three minutes, if a drop of clean water is applied to them, they will recover their shape, and swim about immediately, rendering their fins again invisible.——For the different states of this animalcule, see fig. 5, *a*, *b*, *c*, *d*.—Orig.

† Mr. Needham observes, in his curious Memoir before mentioned, p. 649, that salt destroys these animalcula; this, I believe, is very true of the common kinds of salt; and which renders the nature of this kind of salt still more singular.—Orig.

end of April, among the rest, were of the form of those in the line of fig. 8. About the 5th of May, many of them appeared as at fig. 9; and at the latter end of May, about the 20th, many of them were of the form of those at fig. 10: most of the variety of forms appearing at the same time.

It was objected by some very ingenious men, to whom I had imparted this discovery, that these salts might be owing to something in the water that I had made use of, which, joined to the oil in the hempseed, might produce this appearance. To obviate this:

Exper. 2.—Mr. E. prevailed on Mr. P. Woulfe, F.R.S. to furnish him with some water that had been most carefully distilled, by a very slow process; and at the same time he procured hempseed from a different part of the town. On April 30, he put an ounce of this hempseed to about 4 ounces of this distilled water, into a glass cylindrical vessel, and covered it carefully with a glass cover; and on the 12th of May he examined the scum, and found it more transparent, but full of the crystals of salts, as represented at fig. 12. Some of the first hempseed put into the same water produced much salt, but not so regular in its figures; these figures, by some means unknown to him, after their crystallization being broken irregularly at their ends, see fig. 13. But yet in this infusion there were many of the original seminal figured salts.

Exper. 3.—Mr. E. was determined to see what effect the hard pump water of Gray's-Inn, after a month's dry weather, would have on the hempseed in infusion; particularly as he was persuaded from experience, that this water contained a large portion of calcarious earth. Accordingly, on the 5th of May, he put an ounce of the same hempseed with the last which he had obtained, into 4 ounces of this pump water; and on the 17th of May he perceived the crystals, which, on being put into the microscope, with the same magnifier, gave the appearance represented at fig. 14. The crystals of this infusion seemed larger and flatter, and something different in their shape; but on examining the mucilage that lay among the seeds at the bottom of the glass, he found an infinite number of the same shaped crystals with those he called seminal crystals; which were likewise found in the mucilage of the New-river water infusion, and in the distilled water infusion among the seeds.

Mr. E. further observes, that the calcarious earth floated in great abundance among the scum of the pump water, as soon as the putrefaction was advanced; which did not appear on the surface of the distilled water, and scarcely any on the river water. The grains of salt produced in these experiments were about the size of the finest basket salt, and of a pale yellowish colour when dry.

Postscript.—Mr. E. found the same kind of crystals in an infusion of flax-seed in New-river water, and also in wheat that has been infused in boiling hot water; but the crystals were fewer, and did not appear so soon in the flax-seed as in the hemp-seed And the experiment of wheat infused in boiling hot water does not always succeed. He likewise found salts not unlike those of the hemp-seed, in infusions of a variety of pulse and grain from the East-Indies, such as lupines, kidney-beans, vetches, millet, Guinea corn, and the sesamum or oily grain; but the last yielded a much larger quantity of salt, and in a shorter time than any of the rest. The salts of these different substances were also not dissolvable on applying clean water to them; but by letting the infusions continue to putrefy some weeks longer, they by degrees assumed irregular shapes, and disappeared. He concludes then with this query, Are not these the oily parts of the vegetables, which float in the scum, on the surface of the infusion, crystallized?

Explanation of the Figures of Plate 18.

These 5 different kinds of animalia infusoria belonging to the genus of volvox of Linnæus, are here represented both in their perfect and in their divided state.

Fig. 1 represents the volvox ovalis, or egg-shaped volvox: at c and b it is expressed in its natural shape: at a the manner in which it becomes two animals, by separating across the middle: this was found in the infusion of hemp-seed, but is found in other vegetable infusions, particularly in that of tea-seed.

Fig. 2 is the volvox torquilla, or wryneck. At a is represented its divided state, at b and c its natural shape; this is common to most vegetable infusions, as is the following.

Fig. 3 is the volvox volutans, or the roller. At a the animal is separated, and becomes two distinct beings, each swimming about and providing for itself; this is often the prey of another species of this genus, especially while it is weak by this separation, not being so active for some time till it can recover itself. At c the animal appears to be hurt on one side; this impression, in a little time, is succeeded by another on the opposite side, as at b, which soon occasions a division. At d is the side view, and at e the front view of the natural shape of the animal.

Fig. 4 is the volvox oniscus, or wood-louse. At a is the natural shape of it, as it appears full of little hairs both at the head and tail; with those at the head it whirls the water about, to draw its prey to it; the feet, which are many, are very visible, but remarkably so in a side view at d. At b it is represented beginning to divide, and at c the animals are ready to part: in this state, as if in exquisite pain, they swim round and round, and to and fro, with uncommon velocity, violently agitated till they get asunder. This was found in an infusion of different kinds of pine branches.

Fig. 5 is the volvox terebella, or the gimblet. This animal is one of the largest of the kind, and is very visible to the naked eye. It moves along swiftly, turning itself round as it swims, just as if boring its way; a and b are two views of its natural shape; c shows the manner of its dividing. When they are separated, the lower animal rolls very aukwardly along till it gets a groove in the upper part; d represents one of them lying torpid, by means of the juice of the horseshoe geranium, with its fins extended. This animal is found in many infusions, particularly of grass or corn.

Fig. 6 is the volvox vorax, or the glutton. This animal was found in an infusion of the Tartarian pine; it varies its shape very much, contracting and extending its proboscis, turning to and fro, in various directions, as at a, b, c, d, e. It opens its proboscis underneath the extremity, when it seizes its prey. The less active animals, that have lately been divided, such as those at fig. 2, a, and fig. 3, a, serve it as food when they come in its way: these it swallows down instantly, as it is represented at fig. 6, h and i. At f it is ready to divide, and at g it is divided, where the hinder part of the divided animal has got a proboscis or beak, to procure nourishment for itself, and soon becomes a distinct being from the fore part.

Fig. 7 represents the appearance of the salts in hemp-seed, after a month's infusion, from the 25th of February to the 25th of March, in New-river water.

Fig. 8 the salts, about a month after, April 25, appeared in this manner.

Fig. 9, these figures represent them about the 5th of May, or 10 days after.

Fig. 10, about the 20th of May, they exhibited the figure of precious stones.

Fig. 11, these Mr. E. called seminal salts, as these small figures are to be seen in most of the infusions, rising at different times, and exhibiting these shapes, when they first appear distinctly.

Fig. 12 represents the salts of hemp-seed in distilled water, that had been infused from the 30th of April to the 12th of May.

Fig. 13 shows the form of the salts when the putrefaction had begun to separate their parts into laminæ in the distilled water.

Fig. 14 are the figures of the salts that appeared from the hemp-seed, infused in hard pump-water about 12 days, from the 5th of May to the 17th.

XIX. On the Computation of the Sun's Distance from the Earth, by the Theory of Gravity. By the Rev. Mr. Horsley, F. R. S. p. 153.

A little treatise, that has lately been published, against Dr. Stewart's method of determining the distance of the sun by the theory of gravity, has put me on reconsidering a subject which had been long dismissed from my thoughts. I am far from being convinced that Dr. Stewart's conclusions are "erroneous on his own principles," as his antagonist affirms; and I am well satisfied that there is no error in the principles themselves. I have always been sensible that an extreme precision was requisite in determining the mean quantity of the solar force affecting the moon's gravity towards the earth, in order to obtain an accurate estimation of the distance; and this circumstance was mentioned by me in a paper that I communicated to the Society about 2 years ago,* before it had been remarked, that I knew of, by any other writer on the subject. I must now declare, that the imperfection of the method arising from this circumstance is much greater than I was at first aware of. I owe this better information entirely to the revisal of Dr. Stewart's Theorems, not to any thing that has been written on the subject by others. I find that if I increase the mean quantity of the sun's disturbing force, as determined by Dr. Stewart in the 9th proposition of his Supplemental Tract, and by myself, in my former paper, by $\frac{1}{25000}$ part of itself, I obtain, by my own method of compututation, 9″ 3‴.394 for the sun's mean horizontal parallax; which seems to be so nearly the mean of the quantities of the parallax deduced from the best observations of the transit of 1761, that it would be ridiculous to set up, any longer, the conclusions of this theory in opposition to observation. It is much more probable that the theory should err in so small a matter as $\frac{1}{25000}$ of the sun's disturbing force, than that observation should err in more than $\frac{2}{9}$, that is nearly in $\frac{1}{4}$ of the whole quantity in question. I beg the favour of you to communicate this to the Society.

* See Philos. Trans., vol. 57, pp. 179, 183. Or these Abridgments, vol. 12, p. 411, &c.

XX. Meteorological Observations for 1768, *made at Bridgewater, Somerset, and at Ludgvan, Cornwall. Communicated by Dr. J. Milles, Dean of Exeter, and F.R.S.* p. 155.

This account shows the highest and lowest states of the barometer, at Bridgewater and Ludgvan, also the depth of rain, for each month in the year 1768. The whole depth of rain being, at Bridgewater 39.291 inches, and at Ludgvan 50.890 inches.

XXI. Proposal of a Method for Securing the Cathedral of St. Paul's from Damage by Lightning; in Consequence of a Letter from the Dean and Chapter of St. Paul's to James West, Esq., Pr. R.S. p. 160.

The following Letter to the President was read, dated St. Paul's, March 6, 1769.

Sir, The consideration of the old church of St. Paul's having twice suffered by lightning, and a solicitude to secure the present fabric from similar accidents; which, but for the interception of the storm by St. Bride's church, within these few years, might have already happened; induce us, the Dean and Chapter of this cathedral, to request the opinion of the Royal Society (so justly eminent for the abilities of its members in every branch of science), relative to the best and most effectual method of fixing electrical conductors. We shall esteem ourselves obliged to the very respectable body over which you preside, for their sentiments and directions on this subject, and are, with much regard, Sir, your most obedient, humble servants, Thomas Bristol, D. Chr. Wilson, S. Barrington, J. Lich. and Cov.

In consequence of this application, it was desired that John Canton, M.A. Edward Delaval, Esq. Benjamin Franklin, LL.D. Wm. Watson, M.D. and Mr. Benjamin Wilson, be a committee to consider the above letter, and report their opinion thereon to the Society; and accordingly, June 8, 1769, Dr. Watson, at the meeting of the Society, read in his place, a report from the committee appointed to consider the application from the Dean and Chapter of St. Paul's, relating to the preservation of that elegant structure from damage by lightning; for which report, thanks were ordered to the committee, and returned to Dr. Watson: and it was also ordered, that a copy of the said report be transmitted to the Dean and Chapter of St. Paul's, signed by the secretary.

Report from the Committee appointed to consider of the properest means to secure the Cathedral of St. Paul's from the Effects of Lightning. Addressed to James West, Esq., President of the Royal Society.

Sir,

As, in consequence of a letter addressed to the Royal Society from the Dean and Chapter of St. Paul's, the Society did us the honour to appoint us a committee to examine that magnificent structure, and, as far as our experience would enable us, to prevent mischief to it from lightning, by a properly disposed apparatus; we lay before you the following as our opinion on it, to be communicated, if you think proper, to the Royal Society. And here, sir, you will permit us to take notice of, and acknowledge, the obligations we were under to Mr. Mylne, a very worthy member of this society, who is surveyor of St. Paul's, and attended several meetings of the committee. This gentleman furnished us with a great variety of information, in regard to the structure of the several parts of this fabric, which, without his assistance, could not easily have been obtained.

As all metals are now known readily to conduct or transmit the electric fluid, or, which is the same thing, lightning through them; the large quantity of lead, and some iron, disposed in different parts of St. Paul's church, will, by having its several parts connected, where there is at present no such connection, prevent the erecting a considerable part of the apparatus, which otherwise we should judge absolutely necessary.

We are of opinion that, cæteris paribus, all buildings on the same level are liable to be injured by lightning in proportion to their height: and that the danger is increased by crosses, weather-cocks, or pieces of metal, in any form, placed upon or near their tops, unless there is a complete metallic communication from these to the bottom of the building, which metal should terminate either in water or moist ground.

In St. Paul's church, the objects of our more particular attention were the dome and its lantern, and the two towers at the west end. The roof over the body of the church, being completely covered with lead, will, we conceive, prevent mischief to it from lightning; and the more so, as the lead on the roof joins to that of the several leaden spouts, which come down the sides of the building, and terminate in the ground at a considerable depth. For our more certain information, one of these spouts was examined; and it was found to descend perpendicularly about 3 feet under the surface of the earth: and then, after being laid about 7 feet in an inclined direction, it ended in a brick drain, which communicates with the sewer. These circumstances induce us to conclude, that what has been just now described is a sufficient metallic communication between the roof of the church and the ground.

No part of this whole fabric seems to be in so dangerous a situation of being injured by lightning as the stone lantern placed above the dome. This danger arises not only from its height, but from the different pieces of metal in different parts of it, being at present detached and separated from each other. This stone lantern is supported by a truncated cone of brick-work, of no more than 18 inches, or 2 bricks thick. To the honour however of the architectural sagacity of Sir Christopher Wren, who was formerly our president, this support of the lantern, which has already stood much above half a century, has not in the least given way in any of its parts. How far it would sustain the violence of a stroke of lightning will, it is to be hoped, never be tried: and what we have now to propose will, we flatter ourselves, lessen the probability of its being injured by it. The first object of our attention, therefore, was to make a complete metallic communication between the cross, placed over this lantern, and the leaden covering of the great dome; as from its height, if any lightning was in its neighbourhood, it would most probably affect the cross.

This cross with the ball, both composed of metal, are supported by, and connected with 7 iron rods. These descend perpendicularly through the small leaden dome, which covers the lantern, and are inserted into and pass through a strong frame of timber, placed horizontally under that dome. The lower extremities of these iron rods are fastened to the under surface of this timber frame with iron nuts and screws.

From this timber work, several large iron bars, placed at some distance from the ends of the above-mentioned iron rods, descend obliquely, and are fixed in the stone-work of the lantern. The upper ends of each of these oblique iron bars pass through the frame of timber before mentioned, and are fastened to its upper surface with iron nuts and screws. Between these iron bars and the leaden covering of the great dome, there is at present no metallic communication. To this arrangement, therefore, is owing the danger from lightning, which the committee apprehends that this part of the building is liable to. To obviate which, we are of opinion, that 4 additional iron bars, each not less than an inch square, should be securely placed over the frame of timber before mentioned in such a manner, that one end of each of these 4 additional iron bars may be in contact with one of the perpendicular iron rods, and the other end of each be in contact with one of the iron nuts and screws, which fasten the obliquely descending iron bars to this frame of timber. At the bottom of these oblique iron bars, just above where they are inserted into the stone-work, the committee recommends, that a ring, made of bar iron, of about an inch square, should be placed so as to be fastened to, and be in contact with, these iron bars.

From this proposed ring to the upper part of the lead which covers the great cupola, the distance is about 48 feet. In this space, we are of opinion, that 4 iron bars should be placed, each not less than an inch square. These should be fixed within the lantern in such a manner, that the upper end of each should be fastened to, and in contact with, the iron ring before mentioned, and their lower ends in contact with the lead on the upper part of the cupola; from which the metallic communication is complete to the lower end of the pipes, that discharge the water from the circular part of the great cupola, on the floor of the stone gallery.

From the bottoms of these pipes, which terminate with a shoe of lead within half a foot of the floor of the stone gallery, the metallic communication is again interrupted to the top of the leaden pipes, which convey the water from thence. Here it is proposed that conductors of lead, not less than 4 inches in breadth and half an inch in thickness, should be placed so as to be in contact with the bottom of 4 of the pipes that come from above, and with the top of 4 of those that descend. Lead is recommended to be employed here, as more readily adapting itself to the various curvatures it must meet with in the now proposed arrangement.

These last pipes, after descending below the colonnade, near the circular staircases, make their appearance on the outside of the drum-part of the cupola; where they are bent at obtuse angles, and discharge their water upon the roof of the church. From these angles to the roof the distance is about 5 feet. Here then is another interruption to the metallic communication. This is proposed to be completed by conductors of lead similar to those before mentioned, which should be so placed as to be in contact both with the bottom of the pipes and the adjoining roof.

From the roof, as has already been mentioned, the leaden pipes are continued below the surface of the earth, and terminate in a drain; and thus, by the method now directed, the metallic communication will be completed from the cross on the top of St. Paul's church to some feet below the surface of the ground.

The committee then turned their thoughts towards the two towers at the west end of the church; and here they beg leave to observe, that in one of these towers, between the pine apple and the leaden bell-shaped covering near it, placed at the top of each of these towers, there is no metallic communication deserving notice, till you come to the lead on the roof of the church. This distance is 88 feet. To this tower therefore it is proposed to adapt a rod or bar of iron, not less than an inch and a quarter square, in such a manner, that one end of the bar should be in contact with the metal communicating with the pine apple on its top, which is of copper, and the other end with the lead on the roof of the church.

In the middle of the other tower, in which the great bell is hung, there is an iron stair-case of considerable height, which is placed in the middle of it, for the more conveniently coming at the clock-work. The top of this stair-case is at no great distance from the leaden covering on the top of the tower: but from the bottom of this stair-case to the roof of the church, between which there is no metallic communication, the distance is considerable, not less than 40 feet.

The committee recommend therefore, that a bar of iron, of an inch and a quarter square, may be placed between the pine-apple, or the lead in contact with it, and the upper part of this stair case; and that another iron bar, similar to this last, may be adjusted so as to pass from the bottom of the stair-case to the lead on the roof of the church. The roof, as has been already mentioned, communicates with the leaden pipes, and these with the ground.

These towers, from their near situation to the cupola, which is a building so much higher, may possibly be less liable to mischiefs from lightning than if they were erected at a more considerable distance. As the direction of the lightning is, however, uncertain, from a variety of causes, as also to what extent one building will protect another, the committee are of opinion, that this apparatus to the towers will be expedient.

It is to be remarked, that wherever iron is employed as a conductor of lightning, especial care must be taken to prevent its becoming rusty; as, from being long exposed to the moist atmosphere, it will be corroded to a considerable depth: and so much of the iron as is corroded ceases to be of use as a conductor; the committee therefore have, in directing the size of these iron bars, made some allowance for the waste of the iron by rust.

The size, as well as number, of the iron bars recommended here by the committee, are only to be considered as applicable to St. Paul's, and not as a standard for any church or building of less dimensions; as in these last, conductors of a smaller size, and fewer in number, may answer the purpose as securely as the larger. But St. Paul's church is particularly circumstanced; it is an edifice not only of great height, but its cupola, to say nothing of the lead on the body of the church, presents a large surface of metal to the clouds; on which account it is very liable to receive greater quantities of the electric fluid; and, from large quantities of such an elastic power, great mischiefs may arise to this magnificent building, in consequence of obstructions the fluid may meet with in passing through it. For these reasons we have recommended very large conductors, that it may pass through them into the ground, as readily as it enters.

These, Sir, are our sentiments in regard to the matter, referred to us by the Royal Society, on the request of the Dean and Chapter of St. Paul's. If they should be acceptable to the Society, and by their means to the Dean and Chapter; and if, by being carried into execution, they should at all contribute to the preservation of that noble fabric, it will be a great satisfaction to us. We are, with very great respect, Sir, your most obedient, humble servants, W. Watson, B. Franklin, B. Wilson, John Canton, Edward Delaval.

XXII. Observation of the late Transit of Venus. By Mr. James Horsfall, F. R. S. p. 170.

These observations were made with a Gregorian telescope, magnifying at least 100 times. The time undermentioned is equal time. The rate of going of the two clocks had been correctly ascertained by Dr. Bevis and Mr. H. by observing the sun pass the meridian for several days preceding the day of the transit.

Mr. H.'s situation was upon a platform laid on the ridges of his own chambers near the Middle Temple Hall: consequently the great volumes of smoke arising from the houses to the north-west were no small impediment to a good observation: this was very remarkable at the time of internal contact. The sun's limb undulated then prodigiously, and there was also a gust of wind which made the telescope vibrate, but not so much as once to lose the planet out of the field. Mr. H. pronounced the moment he saw the internal contact, as soon as he saw a lambent light (not a well-defined light) whirl round the opaque limb of the planet; whence he thinks he pronounced that too early by 2 or 3 seconds.

The first part of this rare phenomenon, which Mr. H. beheld, was a kind of penumbra, at $7^h\ 8^m\ 50^s$. He then counted 1, 2, 3 to 8^s, and plainly discerned the dark limb of Venus make a dent very near the vertex of the sun's limb. At $7^h\ 26^m\ 34^s$, he perceived the lambent light above mentioned; at which time the sun was not above $\frac{1}{4}$ of a degree above the top of a chimney.

XXIII. An Account of the Observations of the Transit of Venus and of the Eclipse of the Sun, made at Shirburn Castle and at Oxford. By the Rev. Thomas Hornsby, M. A., F. R. S. p. 172.

The weather, on the morning of the 3d of June, was so very unfavourable, both at the observatory of the earl of Macclesfield, and also here at Oxford, that there was very little reason to expect that we should be able to make any observation. But here, a few minutes before noon, the clouds began to break, and Mr. H. was enabled to observe the transit of the sun's consequent limb over the meridian. At one o'clock in the afternoon, the sky was again overcast, and it rained for some time; but towards 3 o'clock, the clouds were dispersed, the sun shone out clearly, and at 5 o'clock there was hardly a cloud to be seen. The preceding evening was also so very favourable, that the several persons who proposed to make observations of the transit, had an opportunity of adjusting their instruments.

The Earl of Macclesfield made use of an excellent refracting telescope of $3\frac{1}{2}$ feet, made by Mr. Dollond, with a treble object glass, magnifying 150 times; and at $7^h\ 7^m\ 49^s\frac{1}{2}$ apparent time, was certain that the planet had sensibly advanced on the sun's disk, having seen a small impression on the zenith part of the sun's limb near a minute sooner. At $7^h\ 23^m\ 13^s$ mean time, or $7^h\ 25^m\ 28^s\frac{3}{4}$ apparent time, as reduced from sidereal time, his lordship determined the internal contact, which he judged to happen when the dark penumbra, which was so sensibly perceived between the limbs of the sun and planet, was lost on the completion of the thread of light. His lordship observed at a small distance from the observatory, by means of a stop-watch, which was let go at the instant he judged the total ingress to happen, and immediately compared with the observatory clock.

Mr. Bartlett, a very excellent observer, who has been constantly employed in the observatory for many years, observed with a 14 feet refractor on the north side of the observatory, within hearing of the clock, the seconds of which were counted by Mr. Phelps, the other assistant observer. At $7^h\ 7^m\ 4^s$ apparent time, Mr. Bartlett first saw Venus on the sun; and at $7^h\ 23^m\ 10^s\frac{1}{2}$ mean time, or $7^h\ 25^m\ 26^s$ apparent time, he judged the ingress to happen, the telescope magnifying near 60 times.

Lady Macclesfield was also pleased to attend to the observation; and at $7^h\ 25^m\ 16^s\frac{1}{2}$ apparent time, judgedthe second internal contact to happen, with a refracting telescope of 6 feet, through which the penumbra before mentioned was hardly to be distinguished.

The sky, though free from clouds, was charged with vapour, which occasioned a constant undulation of the limbs of the sun and planet; and the wind sometimes blew so hard as to incommode the observers.

On the next morning the sky was very favourable to observation, and Mr. Phelps determined the eclipse of the sun to begin at $18^h\ 32^m\ 45^s.7$ mean time, or $18^h\ 34^m\ 56^s.7$ apparent time, and to end at $20^h\ 17^m\ 23^s.5$ mean time, or $20^h\ 19^m\ 33^s.8$ apparent time. The Earl of Macclesfield observed the end to happen 1 second later, making use of Mr. Dollond's refractor. The latitude of the observatory at Shirburn Castle is 51° 39′ 22″, as determined by observations of the pole star, at several different times; and is $3^m\ 57^s$ of time west of Greenwich, and $1^m\ 6^s$ to the east of Oxford, as appears by computing the difference of meridians between Mr. Short's house, Shirburn Castle, and Oxford, as they result from the observations of the sun's eclipse on April 1, 1764.

Mr. H. proposed to observe the transit of Venus and the sun's eclipse in the upper room of the tower of the schools, which, though the floor of it be very unsteady, yet from its elevated situation afforded him the clearest view of the north-west part of the horizon, and was indeed the best place for making occasional observations in different parts of the heavens, and at different altitudes, which the place then afforded. The clock, furnished with a compound pendulum, was for some time carefully compared with another clock of the same construction, which was fixed in a small observatory in the house where he lived, and which he had altered from sidereal to mean solar time, for the easier comparison of those clocks, which several gentlemen had procured, in order to observe this rare and curious phenomenon. The time was determined by meridional transits of the sun, taken with a transit instrument made by Mr. Bird, and placed very exactly in the plane of the meridian, the focal length of the object-glass being 43 inches. The motion of both clocks was perfectly even and regular.

The atmosphere was so loaded with vapour, and the limb of the sun was in such a constant state of undulation, that Mr. H. determined to observe the external contact with a refractor of 12 feet, furnished with a system of eye-glasses, and magnifying 68 times. He had found, by a previous computation, that the planet would make the first impression on the sun's upper limb, about 19′ to the right hand of a vertical circle passing through the sun's centre. He therefore kept his eye constantly fixed on that part, and at $7^h\ 5^m\ 58^s$ apparent time, he perceived that a small part of the planet's diameter had certainly entered on the sun's disk; the impression, which he had observed for a few seconds before, having continued on that part. While the planet was passing over the sun's edge, Mr. H. determined, with the old micrometer applied to the 12 feet glass, the annexed differences of declination between the northern limb of the sun, and the southern limb of Venus, with as much accuracy as the unsteadiness of the floor would permit.

				Mean Time.	
At	7h.	8m.	48s.	3	35.2
	7	11	37	3	44.5
	7	13	28	3	46.4
	7	14	57	3	50.1
	7	15	42	3	53.6

But as the time of the internal contact began to draw nigh, he directed a refractor of $7\frac{1}{2}$ feet, with a double object-glass, to the sun, made by Mr. Dollond, and magnifying 90 times; and soon after $7^h\ 21^m$ mean time, perceived that the planet appeared to be wholly entered on the sun, though the limbs of the sun and Venus were not actually separated; that part of the sun's edge, where the ingress happened, being very sensibly obscured by a penumbra, and the limbs appearing to be united, by a kind of ligament of a considerable breadth. This ligament became narrower and narrower, and was at length reduced to a point, and actually broken at $7^h\ 21^m\ 57^s\frac{1}{4}$ mean time, or $7^h\ 24^m\ 13^s\frac{1}{4}$ apparent time. At $7^h\ 24^m\ 23^s$ apparent time, the thread of light between the edges of the sun and Venus, which was before completed, now appeared of a very sensible breadth, and to equal $\frac{1}{10}$th of the planet's diameter. If he had estimated this breadth properly, the true internal contact must have happened considerably more than a minute sooner. The Swedish astronomers have described this appearance very nearly as Mr. H. saw it; but according to the account given by Mr. Mallet, the interval of time between the true and apparent ingress, when the limbs appeared perfectly to coincide, and when the ligament was observed to be broken, did not exceed 53^s, according to Mr. Melander's observation, and amounted to 56^s, according to Mr. Wargentin. This appearance, in all probability, is occasioned by the refraction which the rays of the sun suffered in passing through the high and dense atmosphere of the planet, and was perhaps rendered more sensible by the vapours near the horizon; as a similar appearance was observed at the 2d internal contact, in 1761, at very considerable altitude, though in a smaller degree. But it will, Mr. H. fears, occasion a much greater uncertainty in the quantity of the sun's parallax deducible from these observations, than was reasonably expected.

By a mean of 6 observations, Mr. H. found the planet's diameter = 58″.1; being not greater than 59″.0 from 4 of the observations, all agreeing precisely to the same part of a second; nor less than 56″.9 by the least of the other two. About 15 minutes after the internal contact, a very thick and black cloud, which moved towards the east, with a slow motion, along the skirts of the horizon, prevented any further observations. The next morning, the sky being perfectly clear, and the limb of the sun undulating but in a small degree, Mr. H. made the following observations of the sun's eclipse:

Apparent time. At $18^h\ 33^m\ 45^s$ beginning of the eclipse.—$20^h\ 18^m\ 36^s$ nearly ended.—$20^h\ 18^m\ 42^s\frac{1}{4}$ ends.

Many irregularities were observable on the moon's limb; though none of them were so pointed as some which he observed in the eclipse of the sun, on August 16, 1765.

On the top of New College tower, the Rev. Mr. Lucas, fellow of New College, with an excellent acromatic telescope of 6 feet, magnifying 60 times, was certain that the external contact of Venus with the sun was passed at $7^h\ 6^m\ 12^s$ apparent time, having perceived a small impression on the sun's edge several seconds sooner; and the Rev. Mr. Clare, Fellow of St. John's College, with the same instrument, judged the thread of light to be completed at $7^h\ 24^m\ 28^s$, having observed the limbs to be in contact several seconds sooner.

The next morning Mr. Lucas observed the beginning of the sun's eclipse at $18^h\ 33^m\ 47^s$, and the end at $20^h\ 18^m\ 37^s$.

Mr. Sykes, of Brazen Nose College, with an acromatic refractor of $3\frac{1}{2}$ feet, made by Mr. Dollond, first saw Venus on the sun at $7^h\ 6^m\ 0^s$, and observed the thread of light to be completed at $7^h\ 24^m\ 22^s$.

Mr. Shuckburgh, of Balliol College, observed there the external contact of Venus with the sun at $7^h\ 6^m\ 8^s$ apparent time, and the internal contact at $7^h\ 24^m\ 25^s$; though at $7^h\ 23^m\ 16^s$, he judged that the centre of the planet was removed more than its own semidiameter from the sun's limb, or that the true internal contact was then actually passed. He is of opinion that the observation of the completion of the thread of light could not be made nearer than to 8^s or 10^s, on account of the undulation of the limbs: and he further adds, that when Venus was wholly entered on the sun, he could no longer perceive the penumbra that attended the planet before the apparent contact; but that instead of it there appeared a small circle of light, somewhat more luminous than the surrounding parts of the sun. Mr. Shuckburgh also observed the beginning of the eclipse at $18^h\ 33^m\ 51^s$, and the end at $20^h\ 18^m\ 38^s$, with the appulse of the moon to several of the spots.

In an unfurnished room of the hospital, that commanded the north-west part of the horizon, Mr. Nikitin of St. Mary Hall, and inspector of the Russian gentlemen sent here for their education by the empress of Russia, and Mr. Williamson, of St. Alban Hall, both well versed in the mathematics, made the annexed observations of the transit, with a reflector of 10 inches, and a refractor of 8 feet.

	1st ext. cont.	Ingress.
Mr. Nikitin	7h 6m 44s	7h 24m 15s½
Mr. Williamson	7 6 29	7 24 10½

The transit and the eclipse were also observed here by the Rev. Mr. Horsley, F. R. S. and Mr. Cyril Jackson, A. B. and student of Christ Church.

The latitude of Oxford is 51° 45′ 15″, as determined by himself, from several observations of the pole star, both above and below the pole, with an excellent mural quadrant, of 32 inches, made by Mr. Bird; the focal length of the telescope being 34 inches. Mr. H. is the rather induced at present to mention this, as the latitude of Oxford, given by Mr. de la Lande in the Connoissance des temps, and attributed to him, was determined by the late Professor Bliss, from observations made with a smaller and less perfect instrument. The longitude of Oxford is $5^m\ 3^s$ or $5^m\ 4^s$ to the west of Greenwich, the former quantity being deduced from a comparison of the sun's eclipse, observed by himself, with Mr. Short's observation, an allowance being made in the computation for the figure of the earth, in the effect of the moon's parallax.

XXIV. Venus observed on the Sun at Oxford, June 3, 1769. *By Samuel Horsley, LL.B., F.R.S.* p. 183.

Mr. Horsley's regulator was moved to the place of observation on Wednesday evening, and set a going on Thursday; and between that time and 9 o'clock on Sunday morning, many comparisons were made of it with Mr. Hornsby's observatory clock, by which its rate of going and distance for Mr. Hornsby's clock, at the time of observation, were pretty well determined. At 10^m before 7, by his regulator, he began to observe and to count the seconds, and about 3^m and 3 or 4^s after 7, he described a very small black notch on that part of the sun's limb where he expected the planet; but it was then so small, that he was in doubt whether it was any thing more than an appearance occasioned by the horizontal vapours, which were more copious than he could have wished, and made the sun's edge, as usual, appear ragged in many parts. But by 5^m after 7, this notch was grown so large, that no doubt remained that it was the planet.

This was Mr. H.'s observation of the external contact, which he wrote however chiefly by recollection; for having had no experience of this observation before, not having observed the transit of 1761, he had conceived a prejudice that it would not be possible to observe the external contact with any accuracy, and therefore he neglected to make any other minute of what he saw of it, but that he was certain that the planet was on the sun by 5^m after 7, by his regulator. Mr. Cyril Jackson, a student of Christ Church, who observed in the same room with Mr. H., said, when all was over, that he thought he had notice of the planet's approach, by a more vehement undulation in that part of the sun's limb where the planet entered, than in any other, which he perceived a very short time before he saw the planet. Mr. H. confessed that he was not sensible of this circumstance. He observed with an 18 inch reflector; Mr. Jackson used a refractor of Mr. Dollond's of 9 feet. The wind was high, and very troublesome to both of them, by the motion it gave to the instruments.

When the planet had been so long on the sun's limb, and so large a part of its circle was plainly entered, that he thought the internal contact was near at hand, he was much astonished to find the shape of the black spot suddenly altered from a large segment of a circle, to what appeared drawn out like a neck, where the lower part, which still seemed the segment of a circle, was connected with the sun's limb, by a kind of ligament of darkness terminated on each side by right lines. The ligament detached itself from the sun's limb; and the light, as he thought, was visible, all round the planet, at $7^h\ 21^m\ 52^s$, by his regulator, and not earlier to his eye. And this he set down as the internal contact. The moment that he perceived the ligament detached from the sun's limb, he turned his eye to the clock, to catch the minute, and to be satisfied that he was right in counting of the seconds. And when he returned his eye to the telescope, which was before or not later than the 55th second, he found that the thread of light between the limb of Venus and the limb of the sun had sensible breadth, and the shape of the planet was perfectly circular. Mr. Jackson reckoned the internal contact at $7^h\ 21^m\ 51^s$, by the regulator. He judged of it as Mr. H. did, by the detachment of the ligament, which he saw, as well as Mr. H. from the sun's limb. The regulator, at the time of the internal contact was $25^s\frac{1}{2}$ or 26^s too slow for Mr. Hornsby's observatory clock. Mr. H. was much surprized, on comparing notes with Mr. Hornsby, to find that he had judged the internal contact $14^s\frac{1}{2}$ or 15^s earlier than he did.

After writing the above, Mr. H. received from Mr. Hornsby a minute of the difference of his clock from mean time, at the time of observation. He now subjoins his observations reduced to mean time at Oxford, reckoning Mr. Hornsby's clock too fast for mean time by $5^s\frac{1}{2}$ at the hour of observation.

External contact	7^h	3^m	$23^s\frac{1}{2}$
Detachment of the ligament	7	22	$12\frac{1}{2}$

Mr. Horsley likewise obtained from his brother, Mr. John Horsley, a minute of his observation made at Greenwich with an excellent refractor of Mr. Dollond's, which magnified, however, only 50 times. His brother assured him, that he did not see the ligament above described, though it was seen by Mr. Maskelyne and by others, at Greenwich. He set down, however, two different dates of the total ingress. One, which he called close contact without any light, appearing between the limbs of Venus and the sun, at $7^h\ 28^m\ 15^s$, apparent time at Greenwich. Another, which he marked thus: 'a thread of light, fine as you can imagine, appearing between,' at $7^h\ 29^m\ 28^s$. Here is an interval of 73^s between the close contact and the appearance of light. The time of the appearance of the light being reduced to mean time, and to the meridian of Oxford (reckoning the meridian of Oxford $3^m\ 4^s$ west of Greenwich, as it is stated in Mr. Maskelyne's Tables), was $7^h\ 22^m\ 9^s$, which is only 3^s earlier than Mr. H.'s observation of the detachment of the ligament. Now hence he concludes, that though the magnifying power of the telescope, which his brother used, was too small to show him the shape of the ligament, yet the ligament had its effect with respect to obstructing the sun's light, which he perceived about the same time as others, who used glasses of greater force; which seems to be a strong confirmation of the reality of what he saw: or that there actually was a part of the sun's disk, which remained obscure for several seconds after the limbs of the planet and the sun were separated.

XXV. Observations of the last Transit of Venus, and of the Eclipse of the Sun the next Day; made at the House of Joshua Kirby, Esq., at Kew. By J. Bevis, M.D., F.R.S.* p. 189.

In the morning of June 2, 1769, Dr. B. fixed his equal altitude instrument, and carefully rectified it; and applying the proper correction to the fore and afternoon's corresponding altitudes of the sun, he found that Mr. Kirby's clock, whose rate of going was well regulated to mean solar time, at noon was $2^m\ 5^s$ before the mean time; whence he deduced the apparent times of his observations. June 3, in the evening, he was alone in a room where he had a very commodious view of the sun. His telescope was a very good reflector, of about 3 feet and a half focal length, with an aperture of near 6 inches, and a magnifying power of 120 times; it was steadily supported, and governed by rack-work, and he had a stop-watch in his hand. Mr. Kirby at the clock.

June $3^d\ 7^h\ 9^m\ 59^s$ apparent time, was perceived a sudden boiling or tremor at the very summit of the sun's limb, very different from what is usually called an undulation of his limb: 8 or 9^s after which, Dr. B. called out now! on dis-

* Mr. Kirby's house is exactly $4^s\frac{3}{4}$ of time east of his majesty's domestic observatory, and $1^m\ 14^s$ w. of the royal observatory at Greenwich.—Orig.

cerning, at the same place, a very small indentation of Venus. He thinks he may put the external contact about 3^s sooner.

At $7^h\ 28^m\ 8^s$ the planet seemed quite entered on the disk, her upper limb being tangential to that of the sun: but instead of a thread of light, which he expected immediately to appear between them, he perceived Venus to be still conjoined to the sun's limb by a slender kind of tail, nothing near so black as her disk, and shaped like the neck of a Florence flask.

At $7^h\ 28^m\ 17^s$ the said tail vanished at once, and, for a few seconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower limb, somewhat like the lesser end of an egg, but soon resumed its rotundity.

In a few minutes more the whole circumference of Venus became very ill defined, and beset with asperities. These were amazingly agitated by a sort of curling quick motion, not easily to be described. A gentleman of his acquaintance fancied Venus, in this circumstance, to resemble a black wafer on the head of a beaten drum. In the transit of Venus, in 1761, which Dr. B. observed at Savile-house, he saw not the least of such appearance at the exit. The planet was then perfectly circular and well defined. The sky, though for the most part of the day clouded over, was all this while very fine.

At $18^h\ 36^m\ 16^s$ the sun's eclipse began, perhaps, 2 or 3^s sooner.

At $20^h\ 22^m\ 33^s$ the eclipse ended, very exact.

XXVI. Observations of the Transit of Venus, June 3, 1769, and of the Eclipse of the Sun the next Morning. By John Canton, M.A., F.R.S. p. 192.

About half a minute before the total ingress, when the bright cusps of the sun were at some distance from each other, there appeared a faint light between them, a little lower than the cusps, or nearer to the centre of the planet: this increased till the time of the internal contact; which fully convinced Mr. C. that there is an atmosphere about Venus. The longitude of Spital-square, west of the royal observatory, Mr. C. formerly found by Rocque's survey, to be $16^s\frac{3}{4}$ of time; and lately, by observing the explosions of rockets, it was found to be $17^s\frac{1}{10}$. He therefore adds 17^s to his time, to bring it to that of Greenwich. The magnifying power of his telescope was 95.

	h	m	s	
1st external contact at	7	8	28½	mean time.
1st internal contact at	7	26	59½	mean time.
Duration of the ingress		18	31	
Equation of time		2	15¾	add
1st external contact at	7	10	44¼	apparent time.
1st internal contact at	7	29	15¼	apparent time.

The diameter of the sun, from 3 observations, was .... 31′ 35″½

.... Venus, from 4 observations 59

At $7^h\ 38^m\ 31^s$, apparent time, the right ascension of ♀ was greater than that of the ⊙ by 8′ 7″.

Of the Sun's Eclipse.

The beginning at	18h	36m	40s	mean time.
End at	20	21	7	
Duration	1	44	27	
Equation of time		2	10¾	
Beginning at	18	38	50¾	apparent time.
End at	20	23	17¾	
Digits eclipsed	6°	14′½		

XXVII. Of Several Sepulchral Inscriptions and Figures in Bas-relief, discovered in 1755, *at Bonn in Lower Germany. By J. Strange, Esq., F.R.S.* p. 195.

About May 1755, in digging some foundations in a garden belonging to his serene highness the Elector of Cologne, at Bonn in Germany, several ancient Roman sepulchral stones were found. Eight of these, being thought curious on account of the bas-reliefs and inscriptions carved upon them, were soon after fixed up against the wall of an inner open court of the electoral palace at Bonn, where they still remain. The drawings which are sent, are faithful copies of these antiquities, which being in the highest preservation, and the inscriptions on them containing nothing more than the usual form, it would be impertinent to enter into any particular description of them. Mr. S. however remarks the singular barbarity of the Roman soldiers' names in these inscriptions.

XXVIII. An Account of the Lymphatic System in Amphibious Animals. By Mr. Wm. Hewson, Lecturer in Anatomy. p. 198.

Reprinted in the 2d vol. of Mr. Hewson's collected works.

XXIX. An Account of the Lymphatic System in Fish. By the same. p. 204.

May be consulted in Mr. Hewson's collected works, above referred to.

XXX. On the Solubility of Iron in Simple Water, by the Intervention of Fixed Air. By Mr. Lane, Apothecary, Aldersgate-street. p. 216.

After premising that the various impregnations of mineral waters have always been very difficult to explain, and that whoever has read the divers, and often contradictory reasonings on the subject, must clearly perceive that there is still room for discoveries in this part of natural history; Mr. L. observes that Mr. Cavendish, by his account of fixed air, and of Rathbone-place water, related in the last vol. of the Phil. Trans, had obliged the public with many additional lights on this branch of knowledge; and from his known accuracy and diligent pursuits in most philosophical inquiries, the learned world had great reason to hope for many other new and useful improvements. To his judgment therefore he submitted the following experiments: which were intended to show, that iron is soluble in simple water, by the intervention of fixed air; and thence, that it is very probable, many different chalybeate springs sustain their metallic charge by this means only.

The solution of iron in mineral waters, especially in such as, by exposure, readily lose the property of striking a purple colour with astringent vegetables, has usually been attributed to some subtile gas, or volatile acid. Chemistry however does not discover any acid solvent for iron, but what has greater affinity with alkalies; and by means of which therefore this metal will be precipitated. Hence if any water appears, with a predominant alkali, which has also the power of tinging with galls, and, on being exposed to the open air, lets fall the iron, and loses that property; may we not conclude the metal to have been suspended by some other medium? This, for example, is plainly the case in German spa water, which Dr. Brownrigg has proved to abound with fixed air. Mr. Cavendish's very curious experiments, before cited, clearly showed, that calcarious earths might be suspended in water by this principle of fixed air. And these had led Mr. L. to examine, whether iron might not be dissolved by the same natural means.

He would not however be supposed to deny, that iron is frequently found united with an acid. The fact is sufficiently evinced in the pyrites and vitriolic earths. Nor could he doubt, but that these substances do largely contribute to the primary impregnation of waters, they being so readily soluble in them. But as an alkali, or absorbent earth, is often found more than sufficient to saturate the acid in mineral waters; this would effectually disengage every particle dissolved by an acid, unless the metal was supported by some other menstruum. His endeavours therefore to detect this solvent, by experiments, are what he here offers.

Exper. 1.—A wide-mouthed bottle, containing ½ a pint of distilled water and 60 grs. of steel filings, was suspended 48 hours over some distillers melasses, in brisk fermentation; so as to receive the fixed air escaping from the fermenting liquor; the surface of which was 10 inches below the mouth of the bottle. Immediately after its removal, the clear water was decanted from the filings and ochrous sediment. This liquor had a brisk and ferruginous taste, with a flavour of the melasses. An infusion of galls, or green tea, soon changed part of it to a colour like ink. The remainder, being exposed to the open air, presently became turbid, threw up a party-coloured pellicle, and deposited a yellowish sediment. The water now retained but very little power of tinging with galls; and in a few days lost this property entirely.

Exper. 2.—Fourteen oz. of coarse sugar, dissolved in 7 pints of water, were mixed with ½ a pint of yeast, in a bottle capable of holding more than twice the above quantity. One end of a bent tube was luted into this vessel, so that no air might escape but through the tube; the other end was loosely inserted 2 inches within the mouth of another large bottle, charged with 400 grs. of iron filings, and 60 oz. of distilled water. After remaining 12 hours in this situation, the sugar working briskly, an ounce phial was let down gently into the bottle, and filled. The water from the phial, with one drop of tincture of galls,* changed in a few minutes to a light rosy purple. The liquor being shaken, and another phial full taken up soon after, one drop of the tincture gave a deeper colour than before. In 1 and ½ hour more, after being shaken again, the phial full received a still deeper purple, from the like quantity of tincture. The bottle continuing as before near 5 hours longer; when the quantity of fixed air from the fermenting liquor was supposed sufficient to have saturated the water; the liquor appeared very turbid on being shaken; and, after standing another hour, under the tube, to settle, the whole was filtered.

Thirty oz. of the clear liquor was poured into 2 Florence flasks, and the remainder into phials, which were afterwards well corked. Two of these phials had their corks dipped into melted resin, so as to cover the mouths of the bottles. Two others were enclosed with a paste of lute. Notwithstanding the above precautions to prevent the escape of air, the liquors in each soon became turbid, and by the next morning deposited yellow sediments.

This water had a smart chalybeate taste, somewhat resembling spa water; with a slight flavour of the fermenting liquor. One drop of tincture of galls gave a rosy purple colour to a wine-pint of this water. Syrup of violets turned it green.† Soap lees, or even alkaline salts, either fixed or volatile in their natural states, soon changed this liquor green, and rendered it turbid, whence a yellow sediment ensued. But neither of the alkaline salts, when previously saturated with fixed air, produced any perceptible alteration. Nor did any visible change happen on the addition of acids.

The 30 oz. of water, in the flasks before-mentioned, after being boiled 20 minutes, to expel the air, became very turbid, and let fall sediments. The clear liquor being decanted, the remainder was passed through a filter, and, after drying, the paper appeared to have gained 2¼ grs. This ochrous residuum could not be again dissolved in water, by means of fixed air; but was soluble in the vitriolic acid. The solution, diluted and filtered, received no colour from galls, until alkali was added to saturate the redundant acid; after which it struck

* This tincture was made by infusing ½ oz. of powdered galls in 8 oz. of proof spirit, for 4 days, without heat.—Orig.

† Simple distilled water, saturated with fixed air, by any means I have tried, makes no change in syrup of violets: and, when mixed with soap, does not curdle. Orig.

a purple, as in common solutions of iron. The liquor, decanted after boiling, neither changed colour with galls, nor showed any precipitation with lime-water.

Exp. 3. A common quart bottle was half filled with distilled water, to which were added 100 grs. of steel filings. To these was introduced, by means of the bent tube, as much fixed air, obtained from a solution of alkaline salt in the vitriolic acid, as was judged sufficient to fill the bottle. The whole being then shaken, with his hand over its mouth, the bottle stuck like a cupping-glass. About the same quantity of air being again added, the bottle, after shaking, had less adhesion than before. On repeating this experiment, a third time, with fresh air, the adhesion was scarcely perceptible. And after the 4th trial, a small portion of air was observed to issue from the bottle. The water now gave a deep colour with tincture of galls. This experiment was repeated with fixed air from different combinations: as also by passing this air through a vessel of pearl-ash, to arrest any acid which might escape from the effervescing mixtures: but the solutions of iron, in all the trials, appeared to be exactly similar, except some trifling difference in taste and smell.

Exp. 4. A bottle, with the like quantity of steel-filings and distilled water as in the first experiment, remained in a room many weeks; yet though it was frequently shaken, and had an ochrous sediment, it gave no colour with tincture of galls.

Exp. 5. A single grain of iron in solution,* diluted with a pint of distilled water, changed to a deep blue purple, with the tincture of galls. Half a pint of the like mixture, exposed many days in a wide-mouthed glass covered loosely with paper, let fall a slight precipitation; but its property of tinging with galls was not sensibly diminished. The same quantity being boiled 4 or 5 minutes in a Florence flask, became turbid, and deposited a small portion of an ochrous sediment. Yet the tincture of galls gave as deep a colour to the clear liquor as it would have done before boiling.

The foregoing experiments seem to prove, that iron remains quite unaffected by pure water, but may easily be dissolved in it on the addition of fixed air; and that in whatever manner this air is generated, the event will appear the same. The last experiment shows, that where iron is suspended in water, by an acid, neither exposure nor boiling will destroy its property of tinging with galls; which is the reverse of what we find to be the case with many ferruginous waters. The 2d experiment more particularly teaches, that the iron, dissolved in water by fixed air, is at least equal in quantity to what is commonly ascribed to most chalybeate springs: that this air, by which the metal is held in solution, is similar

* Iron-filings were dissolved in diluted oil of vitriol to saturation; and by experiment, one grain of the metal, with about 2¼ grs. of the acid, were found to be contained in 68 grs. of the solution. —Orig.

to that elastic vapour, so often mentioned by writers on these subjects; which cannot be wholly retained by the closest corking, but, gradually escaping, suffers the ochrous matter to subside. And that fixed air has greater affinity with alkalies than with iron, because the addition of alkaline substances, not saturated with fixed air, will disengage the metal, while such as are charged with this principle produce no alteration.

These conclusions seemed to account for many particulars relating to medicated springs; but as all Mr. L.'s trials had been made with iron in its metallic state, which is rarely found in nature, it was necessary to repeat them on this mineral in the state of ore. Mr. L. proceeded therefore to different experiments on various ores; but did not find any of them to answer his expectation, except what is called iron sand ore, which seems to contain a perfect iron. This at first offered a material objection to his former inference. But on a little consideration, it occurred, that waters, being first charged with pyritical matter, might afterwards have their acid neutralized with alkaline or calcarious substances, and the iron yet remain suspended by air generated in the saturation. And he was the more ready to adopt this opinion, as it would explain, very naturally, the application of fixed air to this business of solution; which he confessed had hitherto been to him somewhat difficult to account for. It was necessary however to examine the truth of this theory, by the test of experiment, which he did in the following manner:

Exp. 6. To a pint of distilled water, mixed with 1 gr. of iron dissolved, as in exp. 5, were added 40 grs. of soap-leys. And to 2 oz. of lime-water, diluted with 14 oz. of distilled water, was added 1 gr. of iron in solution. In both cases the point of saturation was intended. The two mixtures instantly turned green, became turbid, and let fall sediments of the same colour. The liquors, being filtered, gave no tinge with tincture of galls.

Exp. 7. A quart of water was mixed with 2 grs. of iron, as before. To one moiety, 3 grs. of salt of tartar in solution was instilled. It first appeared green, soon changed yellow, and gave an orange-coloured precipitate. To another portion 2 grs. of powdered chalk being added, it presently became straw-coloured, and after continuing 9 hours in a well-corked phial, was yet turbid, with a yellow sediment. The waters being both filtered, part of each changed to a purple, with tincture of galls. The remainder being boiled, a further precipitation ensued, and the clear liquor no longer produced any alteration with galls.

This experiment being repeated with magnesia, and with the earth of alum, showed no sensible difference. The quantity of iron, left dissolved in the liquor, was found in proportion to the volume of air generated during the saturation. If the quantity of alkali or absorbent earth was insufficient to saturate the acid, part of the iron would remain in the water after boiling. All which were discoverable by the tincture of galls.

Exp. 8. To a pint of distilled water, being saturated with fixed air, and containing 4 grs. of chalk, was added 1 gr. of iron in solution; the mixture continued pellucid.

Another grain of dissolved iron was diluted with a like quantity of water, previously saturated with air from a solution of pearl-ash in vitriolic acid; 8 grs. of salt of tartar, crystallized with fixed air, and dissolved, were added to this mixture: a slight degree of cloudiness ensued, but disappeared on shaking; after which the liquor much resembled spa-water; only it tasted stronger of the iron. The quantity of chalk, and of alkaline salt, in this experiment, was more than sufficient to neutralize the acid.

Both the above mixtures, on exposure, became turbid, threw up pellicles, deposited ferruginous sediments, and lost their power of striking a purple with galls.

Solutions of iron, and of its ores, in the marine and nitrous acids, as also pyrites dissolved in rain water, were substituted, by turns, instead of the original iron in vitriolic acid; and they all gave a purple colour with tincture of galls.

The trials were repeated with lime-stone, marble, island crystal, osteocolla, and magnesia, instead of chalk; and with volatile and mineral alkali, replete with air, instead of salt of tartar; but no material difference was observed.

The success of these experiments completely answered his expectations. They satisfied him, that any acid holding iron dissolved, and diluted with water, might not only be neutralized, but the water charged with an excess of alkaline or earthy matter, without precipitating the metal; and that the solvent, in these cases, could be no other than fixed air. Since the iron remained in solution, only where this principle originally abounded in the water, or was afterwards generated in the saturation.

Thus much being determined, it seemed easy to apply the discovery to the more perfect analyzation of some waters; and to the re-production of others, by art, which should exactly resemble those of natural medicated springs. This is a task he should probably have undertaken at leisure; had he not been informed, that Dr. Watson, junior, by whose conversation his thoughts were first led to the subject, was then engaged in something of this sort. This gentleman saw many of the foregoing experiments repeated; and, as he afterwards went to the German spa, his abilities and application would sufficiently improve the opportunity: he should gladly therefore wait the result of his inquiries.

To conclude, it appeared to Mr. L. highly probable, that fixed air is generally necessary to the impregnation of mineral springs. That by the right knowledge of this principle, we might now solve most difficulties that had arisen on this subject; and very possibly be able to imitate nature, in the formation of medicated waters.

XXXI. Of Several Phenomena observed during the Ingress of Venus into the Solar Disk. By the Rev. W. Hirst, F.R.S. p. 228.

The telescope Mr. H. used was a reflector 2 feet in length, and magnified 55 times. Expecting the planet to enter the solar disk at or near the zenith, he kept his eye constantly fixed at that part of the sun a considerable time before the beginning of the transit. The first intimation which he had of the near approach of the planet, was by the sudden appearance of a violent corruscation, ebullition, or agitation of the upper edge of the sun, as in pl. 17, fig. 3, when he called out to governor Vansittart, who was taking the time, and desired him to take care. Mr. H. had not taken this precaution above 5 or 6 seconds, when he plainly saw a black notch breaking in upon the sun's limb, and which seemed a portion of a much less sphere than that of Venus, as in fig. 4. Instantly he desired Mr. Vansittart, by the word now! to mark the time, which was $11^h\ 57^m\ 35^s$ sidereal time, by Dr. Halley's little clock, belonging to the royal observatory. The last-mentioned time, allowing for the necessary corrections, and reduced to apparent time, is $7^h\ 11^m\ 11^s$.

As Mr. H. imagined, from the instructions of Dr. Halley, that the precise and accurate time of observing the internal contact, is when the thread of light should break in between the concave edge of the sun and the convex edge of Venus, as in fig. 8; he waited till that particular period, which was when Dr. Halley's clock marked $12^h\ 15^m\ 45^s$, sidereal time, or $7^h\ 29^m\ 18^s$, apparent time, the difference being $18^m\ 7^s$ of apparent time.

The same phenomenon of a protuberance, which Mr. H. observed at Madras, in 1761,* at both internal contacts, he observed again at this last transit: at both times, the protuberance of the upper edge of Venus diminished nearly to a point before the thread of light between the concave edge of the sun, and the convex edge was perfected, when the protuberance instantaneously broke off from the upper edge of the sun, but Venus did not assume its circular form till it had descended into the solar disk, at least to the distance, by estimation, from the upper edge of the sun, as described in fig. 9.

Though at the time of the contacts the atmosphere was remarkably clear, yet as the sun descended towards the horizon, the atmosphere became more and more hazy, so that the edge of the sun, as well as the edge of the planet, became more and more tremulous, and caused the planet to assume, in appearance, different configurations, resembling sometimes a prolate and sometimes an oblate

* Phil. Trans. vol. lii. p. 396.—Orig.

spheroid, till he lost sight of the sun by its being intercepted by a dark cloud, or rather fog-bank, some time before the calculated time of sun-set. Though these last phenomena are entirely optical deceptions, owing to the state of the air at that particular time, or to horizontal vapours, yet Mr. H. begs it may be here very carefully remarked, that, at the times of the contacts, the air was perfectly clear and favourable, so that the observations were then certain, and not subjected to any fallacy in vision. The following circumstance is a proof of this assertion. The first warning which he had of the near approach of Venus to the sun's external edge was, as before said, by the sudden appearance of a violent corruscation, ebullition, or agitation of the upper edge of the sun, 5 or 6 seconds before the edge of Venus broke in upon the sun; where alone he observed the violent agitation, the edge on each side remaining perfectly quiescent, as a, b, fig. 3. If this appearance had been owing to the state of our atmosphere only, then would the edge of the sun be universally fluctuating or trembling; but as this was not the case, the undulation must be imputed to some other cause, not improbably to an atmosphere about Venus. Mr. H. was the more minute on this circumstance, because Mr. Maskelyne, in a late publication,* had taken notice, that when Mr. H. took the observation of the transit of Venus at Madras, in the year 1761, he saw " a kind of penumbra or dusky shade, which preceded the first external contact 2 or 3 seconds of time, and was so remarkable, that he was thereby assured the contact was approaching, which happened accordingly."

On the foregoing paragraph Mr. H. remarks, that in the transit of this present year, he did not take notice of the same phenomenon as he did of the transit of Venus in India, in the year 1761; but he here again insists on it, that such penumbra or dusky shade he then actually saw; but he did not recollect that he then saw the least undulation, ebullition, or corruscation, as happened in the transit of this present year. Yet both phenomena were conducive to the same purpose, and served to give him notice of the near approach of the planet Venus, to the solar disk, the event, in both instances, justifying the presage; and both appearances might be the consequences of the same cause; which cause, as before observed, might be nothing less than the atmosphere of Venus. He says might be, for he would not be understood to assert here any thing dogmatical, preserving at this time the same diffidence in expression as he made use of when he observed the transit of Venus in India, where he was apprehensive, that " to be able to discern an atmosphere about a planet at so great a distance as Venus, may be regarded as chimerical;" yet he might venture to say, that his observa-

* Instructions relative to the observations of the ensuing transit of the planet Venus over the sun's disk, on the 3d of June 1769. By the Rev. Nevil Maskelyne, astronomer royal, p. 32.—Orig.

tion of the transit of the present year seems to corroborate his assertion, in the account of the transit observed in India, in 1761; however, he does not here peremptorily assign the cause, leaving such remarks to be made by others.

On his return from India, Mr. H. was glad to find he was not singular in remarking the strange phenomenon of the oblongation of the orb of Venus at the time of both the internal contacts. It was with sensible pleasure he saw in the Philos. Trans. that 4 astronomers at Upsal, in Sweden, as well as Mr. Dunn, in England took notice of the same or similar circumstances. The appearance of this protuberance or ligament must now be universally confirmed, especially by all observers of the transit of the present year, at least by all such as have viewed it through telescopes of sufficient magnifying powers, and who have sense enough to believe their own eyes, or candour enough to embrace and acknowledge conviction, malgré all prejudice and preconceived opinion.

Fig. 3 represents the first presage Mr. H. had of the approach of Venus to the sun's external disk. Fig. 4 is the appearance of the black notch, when he noted the time of the first external contact. Fig. 5 is the body of the planet within the solar disk, adhering to the sun's upper edge, the thread of light not yet formed. Fig. 6 the protuberance forming, and the undulation at c, d, very violent, better seen in fig. 8. Fig. 7 the undulation decreasing, and the protuberance forming itself into a point, the luminous filaments darting between the edges of the sun and the planet. Fig. 8 the luminous filaments cease to move, and the upper edge of the planet is well defined; its whole orb more opaque, but not yet divested of its oval appearance, the thread of light at c, d, is formed; and at this period he marked down the time of the internal contact. Fig. 9 the planet is restored to its circular figure.

XXXII. Observations made at Leicester on the Transit of Venus over the Sun, June 3, 1769. By the Rev. Mr. Ludlam, Vicar of Norton, near Leicester. p. 236.

The telescope, used for viewing the planet, was made by Mr. Dollond, with a triple object glass of 33⅓ inches focal distance, and was found by experiment to magnify 54 times. The clock was firmly fixed; its pendulum rod was made of wood. The transit telescope was not accurately adjusted either to the meridian or horizon, but the transits of the sun and of η Bootis were sufficient to show the rate of the going of the clock, and the corresponding altitudes of the sun, its error a few days before the transit of the planet; whence the necessary reduction of the time then shown by the clock to apparent time may be easily derived. If we suppose then the clock to be $1^m\ 11^s$ faster than mean time, on June the 2d at noon, and to gain at the rate of 2½ seconds in a day, as deduced from those observations, then at the time of the transit of the planet it was 1^m and 1 slower than apparent time.

At $7^h\ 6^m$, according to the time shown by the clock, a small indenture appeared on the sun's limb; the increase of it at $7^h\ 6^m\ 14^s$, showed plainly that it was made by the expected planet. The internal contact was first noted at $7^h\ 23^m\ 56^s$, and at $7^h\ 24^m\ 8^s$, the divided part of the sun's limb seemed wholly united. The edge both of the sun and planet were in a continual tremor; at the internal contact the limb of the sun seemed, for several seconds, to be alternately united and again separated by a kind of shootings of the planet. These observations, reduced to apparent time, give the external contact at $7^h\ 7^m\ 1^s$, the internal contact at $7^h\ 25^m\ 9^s$, the duration $18^m\ 8^s$.

The solar eclipse was observed by the same clock and telescope. It was manifestly begun at $18^h\ 34^m\ 26^s$, according to the time shown by the clock. The ending was exactly noted at $20^h\ 20^m\ 8^s$. The sun's limb appeared very well defined all the morning. These observations, reduced to solar time, make the beginning of the eclipse at $18^h\ 35^m\ 21^s$, the end at $20^h\ 21^m\ 2^s$, the duration $1^h\ 45^m\ 41^s$.

By the mean of a great number of observations, made for the purpose, the latitude of Leicester was found to be 52° 37′ 3″.

The latitude of Market Harborough, in Leicestershire, from the mean of several accurate observations of the sun's image, projected into a dark room, by S. Rouse, was 52° 28′ 30″.

XXXIII. On a Rare Plant found in the Isle of Skye. By John Hope, M. D., F. R. S. p. 241.

This plant was found, September 1768, in a small lake in the island of Skye, by James Robertson, whom Dr. H. had sent there in search of new or rare plants. The whole of it, except the head and top of the stalk, was under the surface of the water. Wherever the water was shallow, the bottom of the lake was covered with this plant, whose roots were so closely interwoven, that in some places large patches were torn up by the agitation of the waters, or other violence, and found floating on the surface, matted together. The plant, when seen without its flowering stem, resembles somewhat the calamaria Dill. Histor. Musc. tab. 80. Though it differs, in many circumstances, from the generic characters of the eriocaulon. yet Dr. H. was inclined to think it the eriocaulon decangulare, which had never been described, or properly figured.

XXXIV. Astronomical Observations made by Samuel Holland, Esq. Surveyor General of Lands for the Northern District of North America, and others of his Party. p. 247.

March 8, 1769, observed by Samuel Holland, Esq. at his house, bearing south, 56° west from Quebec, distance from the castle of St. Lewis 2½ miles, with Bird's astronomical quadrant, the latitude, viz.

Zenith angle of the sun's upper limb	51°	4′	0″
Deduct for the sun's southern declination	4	34	31
Remains	46	29	29
Add the sun's semidiameter		16	9
Ditto refraction		1	42
North latitude by observation	46	47	20

Eight more observations of the latitude were also taken by the same; the mean of all giving 46° 47′ 15″ for the latitude north.

Obs. of long. March 11, 1769, observed by the same at the same place, with Dollond's refracting telescope, an immersion of the first satellite of Jupiter, at 15 hours, and 45 seconds, mean or equal time. Several more similar observations were also made.

Obs. of the tran. June 3, 1769, observed by the same at the same place, with the same instrument, the transit of Venus, as follows: at $2^h\ 28^m\ 1\frac{1}{4}^s$ perceived a luminous point on the lower part of the sun's limb, by appearance; and in the same place $1\frac{1}{4}^s$ afterwards, the first external contact was formed, which rectified, as the clock or time-piece of Graham was 15^s too fast at the time of observation (as proved by equal altitudes of the sun taken with Bird's astronomical quadrant, on the 1st, 2d, 4th, and 5th instant) the equal or mean time of observing the first external contact will be at $2^h\ 27^m\ 48^s$. Mr. St. Germain, of the seminary of Quebec, observed the same contact, at the same instant, with Short's 2-feet reflecting telescope. Clouds, intervening, prevented the observation of the first internal contact; but at 6 o'clock the planet might be seen with the naked eye on the sun's disk, through the haziness of the atmosphere.

Obs. of lat. January 2, 1768, observed by Ensign George Sproule, of the 59th regiment of foot, on the south point, at the entrance of the basin of Gaspée, with Hadley's quadrant, and an artificial horizon, the latitude, viz.

Double angle of the meridian altitude of the sun's centre	36°	38′	0″
Apparent altitude of the sun's centre	18	19	0
Refraction		2	4
True altitude of the sun's centre	18	16	19
Taken from	90	0	0
Sun's zenith distance	71	43	41
Sun's declination reduced to the meridian of Gaspée	22	56	10
North latitude by observation	48	47	31

There were 14 more observations made of the latitude, by the same person; and the result of the whole 15 make the place of observation 48° 47′ 32″ north latitude.

Obs. of long. January 29, 1768; observed by the same person, at the same place, with Short's 2-feet reflecting telescope, an immersion of the first satellite of Jupiter, at 14 hours, 11 minutes, and 3 seconds, mean or equal time.

XXXV. Observations made on the Island of Hammerfost, for the R. S. By Jeremiah Dixon. p. 253.

The Transit of Venus.

	Time p. clock.	
1769, June 3,	13^h 40^m	Saw the planet Venus on the sun about half immerged.
	43.	Not totally immerged.
	50.	The planet seemed to be completely on the sun, but no thread of light; this was an instantaneous view, and through a thin cloud. The air all this time very hazy.
	19 47.	Saw Venus on the sun's limb, the 2d internal contact being past. After this, all cloudy as before.

The latitude of the observatory is, per observ. June 2, at noon 70° 38′ 22″
4, at noon 70 38 23.

Note, The error of the line of collimation of the quadrant did not, on trial, appear to be any thing worth notice.

The longitude of the observatory is, by comparing the observations of the eclipse of the sun with those made at Greenwich.

Per	1st measurement with the micrometer	1^h	34^m	40^s	These observations were made before the middle of the eclipse.
	2d ditto	1	34	35	
	3d ditto	1	34	44	
	13th ditto	1	35	7	These observations were after the middle.
	15th ditto	1	35	13	
	End of the eclipse	1	35	10	
		1	34	55 = 23° 43′ 45″	

east of the Royal Observatory at Greenwich.

XXXVI. Astronomical Observations made at the North Cape, for the R. S. By Mr. Bayley. p. 262.

From many corresponding altitudes the going of the clock is determined as below.

	App. noon per clock, per equal altitudes.	Clock too slow for sidereal time.	Rate of clock with resp. to sider. time.	
May 6	2^h 53^m $31^s.0$	1′ 1″.0	+ 4″.6	
8	3 1 25 .2	0 52 .3	+ 1 .2	
15	3 28 59 .2	0 43 .2	− 1 .3	
20	3 48 43 .9	0 49 .6		Clock stopped.
June 3	4 45 5 .1	1 13 .6	− 0 .5	
9	5 9 46 .4	1 16 .6	+ 1 .3	
10	5 13 56 .9	1 15 .3	+ 2 .1	
11	5 18 7 .3	1 13 .2	− 0 .2	
15	5 34 42 .5	1 14 .0	+ 0 .07	
18	5 47 10 .3	1 14 .2		
		Mean rate of clock	+ 0 .91	

Transit of Venus, observed at the North Cape.

1769, June 3. At $13^h\ 46^m\ 40^s$ per clock, or $9^h\ 0^m\ 2^s$, apparent time, the sun came out from under a cloud, with Venus on it, about $\frac{1}{4}$ of her diameter; and at $14^h\ 0^m\ 41^s$ or $9^h\ 14^m\ 1^s$, apparent time, Venus's outer limb seemed to be in contact with the sun's limb; but no light of that part of the sun's limb could be seen, Venus being apparently joined to the sun's limb by a black ligament, which gradually diminished in breadth; and at $14^h\ 1^m\ 36^s$, or $9^h\ 14^m\ 56^s$, the sun's light broke through it, and Venus and the sun were to appearance perfect (this was certain to about 10 or 15 seconds of time), and the black ligament contracted itself, so that Venus was considerably within the sun's limb, suppose $\frac{1}{10}$ of her diameter.

During these observations the air was red and hazy, and the sun's limb very tremulous, and the spots in the sun very indistinct, and Venus seemed very ill defined when on the sun. By a mean of 12 observations the diameter of Venus measured 55″.32; also the sun's horizontal diameter 31′ 37″.61, and his vertical diameter 30′ 50″.73.

Eclipse of the Sun, observed at the North Cape.

June 3, 1769. At $1^h\ 48^m\ 4^s$, the clouds clearing away, Mr. B. saw the sun, when the moon had made a small impression or notch in the sun's limb; by observing the increase of the eclipse, he supposed it began 4, 5, or 6 seconds sooner than he first saw it, or at $1^h\ 48^m\ 0^s$ per clock, or $20^h\ 59^m\ 19^s$ apparent time, nearly.

Clouds came on, so that he saw the sun no more till $3^h\ 38^m\ 0^s$ per clock, and it broke away very clear, and continued clear to the end, which was at $3^h\ 48^m\ 19^s$ per clock, or $22^h\ 59^m\ 17^s$ apparent time. The air being very clear, the end seemed certain to about two seconds. The telescope used was a reflector of 2 feet focus, made by Mr. Dollond; and the magnifying power, applied for the ingress of Venus, and the beginning and end of the solar eclipse, was 100. The magnifying power used with the micrometer, was 50.

Sun's horizontal diameter, measured directly after the eclipse ended, by a mean of 5 was 31′ 35″.9.

By comparing the end with Mr. Maskelyne's observation at Greenwich, the difference of meridians comes out $1^h\ 44^m\ 6^s$ of time, = 26° 1′ 30″ east, or difference of longitude of Mr. B.'s observatory from Greenwich to the east. Whence the longitude of the point called the North Cape is 25° 49′ east of Greenwich.

From the foregoing zenith distances of the sun, and of Arcturus, and α Lyræ, the latitude of the observatory is determined by a medium, 71° 0′ 47″. Whence the latitude of the point of land called the North Cape is 71° 10′ north. By a great many trials with a very good compass, of Dr. Knight's construction, Mr. B. found the variation to be 6 degrees west; and by a dipping needle, he found, by repeated trials, the dip of the north end of the needle to be 79 degrees.

May 15, at $13^h \frac{1}{4}$ P. M. apparent time, or $1^h\ 7^m$ after high water, by a mean of 7 observations, he found the dip of the horizon of the sea, from the observatory, to be 12′ 18″. Height of the barometer 29.70 inches; thermometer without, 24°; thermometer within, 28°. And May 20, at $7^h \frac{3}{4}$ P. M. apparent time, or $7^h\ 33^m$ after high water, from a mean of 8 observations, he found the dip = 12′ 25″.5; barometer 29.70 inches; thermometer without, 43°, and within 40°. Both these observations were made on the N. N. E. point of the true compass. Mr. B. found it was high water, at the full and change of the moon, at $3^h\ 44^m$ P. M. apparent time, at the Cape; and, by a series of observations, he found the water to rise 8 feet 1 inch, nearly, perpendicular at the spring tides; and at neap tides 6 feet 8 inches, perpendicular; and the tides seemed to follow very regular, as they ought to do when not disturbed by bad weather. June 8, he found the height of the observatory 140 feet 6 inches above low water mark.

XXXVII. An Observation of the Transit of Venus, made at Isle Coudre near Quebec. By Mr. T. Wright, Surveyor. p. 273.

Having, June 1 and 2, made many observations of the sun's altitude, Mr. W. found the clock too fast for mean time 21^m 44 on June 1, and $20^m\ 46'\frac{1}{4}$ on June 2; consequently the clock lost $57\frac{1}{4}$ seconds in 24 hours.

Saturday, June 3, the Morning cloudy, no Altitudes taken.

At $2^h\ 49^m\ 22^s$ by the clock, Mr. W. happened to take his eye off from the very point where he afterwards found the external contact happened, imagining he saw it something more to westward; but, finding his mistake, he returned to the former point, where he found Venus had made a very small impression at $2^h\ 50^m\ 25^s$.

At $3^h\ 7^m\ 48^s$ Venus appeared completely round to the eye, and to appearance rather detached, and joined by a small dark thread or ligament, which prevented the rays of light from appearing.

At $3^h\ 8^m\ 19^s$ the rays of light just appeared, at the internal contact.

The following contains the above times, as shown by the clock, reduced to apparent time, by allowing a proportion of 57 seconds, its regular losing in 24 hours; as appears by the preceding and the following corresponding altitudes.

Short — $2^h\ 49^m\ 22^s$—$17^m\ 32^s$=$2^h\ 31^m\ 50^s$ apparent time of the 1st observation.
— 2 50 25—17 32 =2 32 53 apparent time of the 2d observation.
— 3 7 48—17 31 =2 50 17 ap. time of 1st obs. of internal contact.
— 3 8 19—17 31 =2 50 48 ap. time of 2d obs. of internal contact.

The weather, at the time of the transit, was not clear enough to observe the least appearance of an atmosphere round the planet, supposing there really had been one.

By one set of observations, the latitude was found to be 47° 16′ 51″ north, and by another 47° 16′ 41″. The place of observation on the island of Coudre, by an actual survey, bears from Quebec, N. 41° 30′, E. by the true meridian, distance 55 statute miles, = 52 marine; which gives dif. latitude = 39′ and dep. 34′ = 50′ dif. longitude = 3^m 20^s of time between Quebec and Coudre.

Remarks by the Astronomer Royal.

The instruments made use of by Mr. Wright, in the foregoing observations, were a 2 feet reflecting telescope; a pendulum clock beating half seconds; a brass Hadley's sextant, of about 15 inches radius, with a magnifying glass to read off the observations; and a rectangular reservoir for holding quicksilver, or any other fluid, which is sheltered from the wind by two glass sides inclined to one another, and ground truly plane: this last for taking the sun's double altitude by reflection with the Hadley's sextant. By a more accurate calculation of the times than Mr. Wright had used, I find the equation of corresponding altitudes, for the noon of June 1 to be — 5″.0, June 2,—4″.5, and June 3 for midnight + 9″.6. Hence the true time of noon, by the clock, June 1, was 12^h 19^m $10^s.0$; June 2, 12^h 18^m $20^s.0$; and June 3, midnight, 12^h 17^m $11^s.1$; and hence the true time of noon, June 3, should be 12^h 17^m $34^s.1$, and the clock is losing 46^s per day on apparent time. Hence the apparent times of Mr. Wright's 4 observations will come out as follows:

App. time.

2^h 31^m 53^s No visible impression made by Venus yet.

2 32 56 Venus had made a small impression.

2 50 19 Venus appeared completely round to the eye, and rather detached, but joined by a ligament.

2 50 50 The rays of light appeared at the internal contact.

Taking Isle Coudre to bear N. 41° 30′ east from Quebec, distant 55 statute miles, as, Mr. Wright says, was found by an actual survey; the distance in geographical miles is 47.65. Therefore the place of observation is 35′ 41″ north of Quebec, and 31′ 34″ east of it, = 46′ 32″ difference of longitude, = 3^m 6^s of time.

XXXVIII. Extract of a Letter from Mr. B. Gooch, Surgeon of Shottisham, near Norwich. p. 281.

May be consulted in Mr. Gooch's work entitled Medical and Surgical Observations, published in 1773, being an Appendix to a former vol. under the same title.

It relates to a morbid separation of the cuticle from the cutis, from every part of the body; and from the hands and fingers in such manner as to have the appearance of a glove.

XXXIX. Observation of the Immersion of Venus on the Sun, June 3, 1769, *made at Gryphswald, by Andr. Mayer, Royal Professor. From the Latin.* p. 284.

Mr. Mayer used a very good Dollond's 7-foot tube. The first exterior contact he saw at $8^h\ 4^m\ 35^s$ and the interior at $8^h\ 22^m\ 44^s$; so that the duration of the immersion was $18^m\ 9^s$. During the ingress the planet's disk was greatly agitated and distorted: after the middle of the immersion, her horizontal diameter was nearly 3 times the vertical one; and the solar light about the margins of the sun and Venus seemed very yellow, and even reddish. A little before the interior contact something of a fascia was seen connecting Venus with the sun's margin. No satellite of Venus was seen; nor any vestiges of an atmosphere, which in 1761 so distinctly appeared, unless we may refer to this cause, the commotion at the first immersion, and the change of colour at the margins of Venus and the sun.

XL. Observation of a Solar Eclipse June 4th 1769, *at the Observatory at Austhorpe, near Leeds. By J. Smeaton, F. R. S.* p. 286.

Beginning by mean time, A. M.	6^h	33^m	1^s
Middle	7	26	38
End	8	20	16
Total duration	1	47	15
Digits eclipsed	6°	$46'$	

The beginning and end of the eclipse were observed by an excellent 3½ feet treble object-glass telescope, constructed by Dollond, with the smallest magnifier, which enlarged the diameter somewhat above 80 times. As there is no defect in quantity of light from the sun, the object glass was contracted by an aperture to 2½ inches, and the object was perfectly sharp and distinct. The quantity was taken by a parallel wire micrometer, upon an equatoreal apparatus, which rendered it very commodious for the purpose; by which the part of the sun's diameter, remaining uneclipsed, measured at right angles to a line joining the horns, was 889 such parts as the sun's diameter, taken the same day at 1½ in the afternoon, measured between two parallels of declination, 2041.

The estimated latitude is 53° 48′. The supposed longitude is 6^m of time west of Greenwich; this is deduced from its position with Wakefield, whose longitude is set down in Maskelyne's British Mariner's Guide, as determined from an observation of the transit of Venus, 1761.

The exact knowledge at what point of the sun's circumference to look for the beginning, Mr. S. found of great use; as he believes he saw the first discernible impression; he however allowed 2^s for the time elapsed between the first perception, and the being sure it was the approach of the moon that affected that part of the sun's limb; and which latter only could be noted by the clock. The first approach did not however affect the sun's circumference by any thing like a penumbra or shade; but began by some asperities of the moon's limb, seeming to thrust themselves into that of the sun; and which appeared before any continued part of the sun's circumference was cut off; or perhaps it might be occasioned by the first approach of the moon's limb, disturbing the little protuberances on the sun's circumference, occasioned by the undulation of the air, and which, when rendered exceedingly distinct, appeared almost like the teeth of a fine saw. This whole appearance, to a telescope less distinct, would probably look like a penumbra or shadow.

Some time before the great spot was immerged, there appeared two parts of the moon's circumference more protuberant than the rest, near the right hand horn; which so remarkably interrupted the regularity of the curve, that it was taken notice of by all about him; and which was doubtless occasioned by two mountains on the moon's surface, remarkably higher than the rest; and he doubts not but the same thing will have occurred to other observers.

Mr. Smeaton was prevented, by clouds, from observing the entrance of Venus on the sun, the evening before.

XLI. Account of the Transit of Venus over the Sun's Disk, as observed at Norriton, in Philadelphia, June 3, 1769. By William Smith, D.D. John Lukens, Esq. Surveyor-General; David Rittenhouse, A. M.; and John Sellers, Esq. the Committee appointed for that Observation, by the American Philosophical Society, held at Philadelphia, for promoting useful Knowledge. p. 289.

The above-mentioned gentlemen having provided a variety of excellent instruments, telescopes, quadrants, sectors, micrometers, clocks, &c. made all the necessary preparations before-hand, by examining and proving them, and making numerous observations of equal altitudes, Jupiter's satellites, meridian transits, &c., for determining the latitude and longitude of the observatory, the rate and time of the clock, &c.; and having taken every precaution to observe and secure the times of the contacts, and any other appearance, the first-named three gentlemen, with each a telescope, &c., patiently watched for the commencement of the transit; which, with several other appearances as indications of an atmosphere about Venus, &c. each of these observers recorded separately, and afterwards communicated to each other their notes and observations, which are here all recorded in a very circumstantial manner, unnecessary now to particularize. But the result of the whole was as follows:

General table of the contacts of the limbs of the sun and Venus, as observed at Norriton, June 3, 1769, reduced to apparent time.

June 3, by the before-mentioned tables of the work, the sun's centre was on the meridian, at $11^h\ 58^m\ 49^s$ by the clock, and June 4, at $11^h\ 59^m\ 2^s$, and therefore gained 13^s in 24 hours of apparent time. Therefore at noon June 3, the clock being $1^m\ 11^s$ slow of apparent time, it was only $1^m\ 10^s$ slow at the observation of the contacts. Whence

The apparent time of the different contacts was:

External contact, by Dr. Smith.	h m s s	Extern. contact, by Mr. Lukens.	h m s s	External contact, by Mr. Rittenhouse.	h m s
1st visible impression on the sun's limb, in form of a quivering dusky shadow, with many points	2 12 50 to 53 Uncertain to 3^s or 4^s	....		Judged of as described in his account	2 12 49
A well defined black dent in the sun's limb, at	2 13 15	A small dent in the sun's limb........	2 13 13		
Internal contact.		Internal contact.		Internal contact.	
Judged from a thread or crescent of light, closing round the dark body of Venus, with a tremulous motion, at	2 30 15		2 30 8 to 14		2 29 55

When Venus was fully entered on the sun's limb, and the gentlemen had satisfied themselves by comparing their different notes of the contacts, which were thrown together on the table of the observatory, they prepared for the micrometer, and other observations. The greatest part of the micrometer observations were taken by Dr. Smith, while Mr. Rittenhouse undertook to take another set of observations; namely, the appulses of the limbs of the sun, and the centre of Venus, to the cross hairs of the equal altitude telescope; Mr. Lukens taking and writing down the time for him.

From a mean of the observed 6 diameters of Venus on the sun, allowing for the error of adjustment, as mentioned below:

Venus's diameter, for the day of the transit, was .... 0′ 57″.3

The sun's semidiameter, from a mean of 5 horizontal diameters, taken the same day 15 47.0

Or, from a mean of 4, taken that day, leaving out the second, which Mr. Lukens thinks he may have taken too large 15 45.0

All the micrometer observations were separately reduced to their value in minutes and seconds, both by Mr. Rittenhouse and Dr. S. Many more might have been taken; but as so many persons were desirous of looking through the telescope, they could not well be denied; and the number above set down are found fully sufficient for all the purposes of the projection; especially as they have been found to agree so well with each other.

The observations being thus finished, Mr. Rittenhouse was pleased to undertake the projection of the transit from them; which he delineated in an exact manner.

From the preceding observations and calculations, it appears that the latitude of the observatory was 40° 9′ 56″ north, and its longitude 5^h 1^m 34^s west of Greenwich.

XLII. Observations of the Transit of Venus over the Sun, June 3, 1769, *made in Sweden, and communicated to the R. S. by Mr. Peter Wargentin, F. R. S. and Sec. to the Royal Academy of Sciences at Stockholm. From the Latin.* p. 327.

The Royal Academy of Sciences of Stockholm sent several astronomers to different parts of the Swedish dominions, to make observations on this transit of Venus, &c. viz. Mr. Mallet, royal astronomical observer at Upsal, went to the villa called Pello, being the northern extremity of the terrestrial meridian arc measured by the French astronomers in the year 1736. Mr. Planman, professor of Natural Philosophy in the academy of Aboen, returned to Cajaneburg, a town situated on the confines of Finland and Lapland, where he before observed the transit of 1761. A third observer, Mr. Hellant, of the town of Tornea, an experienced and skilful observer, was to make his observations at that place. Besides these, 5 other gentlemen were appointed to make the observations at Upsal; viz. Mr. Stroemer, emeritus professor of astronomy; Mr. Melander, professor of astronomy; Mr. Bergman, professor of chemistry; Mr. Prosperin, deputy astronomical observer; and Mr. Salenius, master of philosophy. In addition to these, three other persons attended to take the observations at Stockholm; viz. Mr. Ferner, the royal chancellor, and formerly astronomy professor; Mr. Wilcke, lecturer on experimental physics; and Mr. Wargentin himself.

The 2 gentlemen, Mr. Mallet and Mr. Hellant, were prevented from fulfilling their mission, by the intervention of clouds and bad weather. But Mr. Planman was rather more fortunate; for though the sun was obscured by clouds that morning, as well as the day before, yet it afterwards cleared up, when at 9^h 8^m he had a sight of the sun, when he perceived that $\frac{1}{4}$ part of the planet's diameter had entered on the sun's disk. And at 9^h 20^m $45^s\frac{1}{2}$ it was totally immerged, when he saw suddenly break off the fasciola, which had hitherto connected the margins of the sun and Venus, and the body of the planet appeared completely surrounded by the sun's light. Very soon after this the sky became quite obscured with clouds, till at 3^h 21^m afternoon he had another glimpse of the sun, when he found the planet about $\frac{1}{4}$ part emerged; after which he watched attentively for the final or exterior contact, which happened at 3^h 32^m 27^s, which he saw with a good tube of 20 feet. The next day, June 4, the sky being clear, Mr. Planman observed the sun's eclipse, viz. the beginning at 9^h 0^m 43^s, and the end at 11^h 0^m $0\frac{1}{4}$. The latitude of the place, Cajaneburg, is 64° 13′$\frac{1}{2}$, and its longitude nearly $1^h\ 50^m\ 47^s$ east of the Greenwich observatory, as concluded at the time of the last transit in 1761.

Of the Upsal Observations.—The whole day of June 3, at Upsal, was quite serene; so that there was nothing to obstruct the certainty of the observation of the planet's immersion, but the undulation of the sun's margin, which, in an elevation of only 2 or 3 degrees above the horizon, could not but be great and inconvenient.

The observations of the immersion by the 5 observers at Upsal were as follow:

Mr. Stromer with a 3-foot reflector, observed the interior contact at	8^h	39^m	58^s
The separation of their dark connecting fasciola at	8	40	32
Mr. Melander, with a 20-foot tube, observed the first approach....	8	22	1
The interior contact of the margins of the sun and Venus....	8	39	57
The breaking of the connecting fasciola	8	40	12
Mr. Bergman, with a 21-foot tube, saw the separation of the fasciola	8	40	9
Mr. Prosperin, with a 16-foot tube, saw the first contact at	8	22	12
Breaking of the fasciola after the internal contact	8	40	12
Mr. Salenius, with a 12-foot tube, observed the 1st contact	8	22	15
This exterior contact at	8	39	46
Breaking of the dark fasciola at	8	40	15

The Stockholm Observations.—Here, not only the undulation of the sun's limb, but also the haze near the horizon, proved troublesome. However the observations proved better than might have been expected, and were as below:

Mr. Ferner, with a Dollond's 10 feet tube, magnifying 90 times, noted the first approach at	8	24	8
Total immersion, or the sun's horns inclosed Venus	8	41	48
Mr. Wilcke, with a good $1\frac{1}{2}$ foot tube, the 1st approach........	8	24	5
Interior contact, but the edges still cohering	8	41	2
The fasciola a little thinner, but still visible	8	41	30
The same fasciola broke, and the limbs separated	8	41	45
Mr. Wargentin, with a 21-foot tube, in numerous fluctuating inequalities in the sun's border, the 1st approach at	8	23	51
The visible interior contact at	8	41	32
Separation of the cohering appearance at.................	8	41	47

XLIII. Observations of the Transit of Venus over the sun. By Dr. Alexander Wilson, Professor of Astronomy in the University of Glasgow.* p. 333.

Besides two reflectors of Mr. Short's described below, Dr. W. had 3 other

* Dr. Alex. Wilson, besides his professorship of astronomy, was a very respectable man in other arts and sciences, author of several ingenious papers in the Philos. Trans. and was remarkably eminent in his profession of a founder of printing types, an art which he carried to very great perfection. Dr. W. died Oct. 18, 1786, and was succeeded in both his professions by his ingenious and learned son.

instruments at the observatory; the first was an achromatic tube of Dollond's, 29 inch focus, by which an image of the sun was formed, of about 6 inches diameter, on a board covered with paper. The telescope being mounted on a frame, by which it could be turned about as the sun moved, and the room properly darkened. This instrument was managed by Dr. Williamson and Dr. Reid, at the west window of the room of the observatory, where the astronomical clock stood; the other two instruments were placed without, at the south and north windows of the same room, one being a refractor of 13 feet, by which Dr. Irvine observed; the other a 12 inch reflector of Short's, by which Dr. W.'s son observed. These two observers looked directly at the sun, having their instruments armed with smoked glasses; another person stood at the clock, and counted the seconds by coincident beats upon a piece of board, which he held in his hand for that purpose, and who named every fifth second, so that all the observers could hear him distinctly. The motion of the clock, made by Shelton, was carefully adjusted by many transits of the sun and fixed stars, over the meridian, both before and after the day of the transit; the clock, by which Dr. W.'s observations were made, was adjusted by Shelton's, by means of signals made every hour, for some hours before and after the transit. Having made these preparations, they thought they had nothing to fear but the clouds; and indeed the western part of the heavens was covered with thick clouds all the afternoon, till a short time before the external contact; but they drove away towards the north, and left the sun perfectly bright, excepting that now and then a cloud passed over him. But they soon found that the constitution of the air was otherwise unfavourable to their observations; the image of the sun on the white board, made by the achromatic telescope, was bright enough; but there was a remarkable undulation in the limb, owing to the state of the air. Besides this undulation in the sun's limb, there was also a considerable tremor round the planet Venus, when she was seen on the sun's disk, and, in consequence of this, an indistinctness in her limb, which made it impossible to measure her diameter by the object glass micrometer, or otherwise. After the centre of Venus had passed the sun's limb, she appeared not to be circular, but oblong, the longest diameter being that which passed through the sun's centre. As the internal contact approached, Venus appeared to adhere to the sun's limb, by a dark protuberance or neck, both the length and breadth of which varied every moment by a constant undulation: neither did this neck break off instantaneously, but changed its colour from black to a dusky brown, till at last the interval between Venus and the sun's limb appeared quite clear. Each of the observers wrote down his observations on the spot. Dr. W. reduced them, together with his own, to apparent time, from the observations he had made on the going of the clock, and they were as follow:

		h	m	s
By Doctor Wilson,	External contact at	6	54	31 .4
	Venus's centre judged to be on the limb	7	1	33 .4
	Sun's light appeared between Venus and the limb	7	11	56 .7
	The beginning of the solar eclipse next morning, observed by Short's 18 inch reflector	18	30	14 .2
	Middle, from a series of observations with the object glass micrometer, fitted to a 9 inch reflector of Short's	19	18	47 .7
	End not visible			
By Dr. Williamson and Dr. Reid.	External contact at	6	54	28
	Internal contact, or when the sun's light appeared between Venus and the limb	7	12	24
	Venus's centre judged to be on the limb, by Dr. Reid	7	1	24

Dr. Reid marked the time when he conceived the internal contact would have happened, if the dark protuberance on Venus had been taken away, and her disk reduced to a circle, viz. 7h 10m 24s.

According to Mr. P. Wilson, external contact 6h 54m 28s internal contact 7h 12m 24s.

This gentleman remarked that his first observation should be considered as only a posterior confirmation of Dr. Williamson's and Dr. Reid's external contact; the fact was, that when these gentlemen perceived the first contact, their keenness made them call out, and it was not till then that he saw the phenomenon with perfect certainty. He was conscious however, that he fluctuated concerning the reality of the appearance for about 12 seconds before that time, during which his determinations were suspended, through an apprehension of anticipating the real time, which was heightened by so close a neighbourhood with the other observers, all of whom he could not help being sensible were still expecting the phenomenon. On the whole, he was rather of opinion that he would have set down the external contact at least 8 seconds sooner had he been observing apart. His 2d observation, by which he means the instant when the interval between Venus and the sun's limb first appeared obvious, was taken down without the least knowledge of what was passing among the other gentlemen who observed.

Dr. Irvine made the external contact 3 seconds sooner than the rest; but his internal contact was some seconds later. Mr. Anderson, F. R. S. fitted up a clock and apparatus in the college steeple; his clock was regulated as above, by signals from the observatory; he observed the transit with a large reflector, and his assistants observed with refractors; they were all of them uncertain about the external contact, owing to the state of the atmosphere, and a tremor given to the steeple by the wind; but none of their other observations varied, above 3 seconds, from Dr. W.'s, as related above.

Latitude of the observatory 55° 51′ 32″. Longitude by corresponding observations 0° 17′ 11″ of time from Greenwich west.

XLIV. An Account of the late Transit of Venus, observed at Hawkhill, near Edinburgh. By James Lind, M.D., at Edinburgh. p. 339.

James Hay observed in the house, on the ground-floor, in the room with the house-clock, with the 3½ feet achromatic telescope with triple object glass; Lord Alemoor observed on the floor above, with the 18 inch reflector, and a watch that showed seconds, set a few minutes before the transit began, and compared after each contact; Dr. Lind was in the observatory with a 2 feet achromatic telescope, a mathematical instrument maker counting seconds from the clock. The following is the account of all their observations:

	Ext. cont.	Int. cont.	
	Mean time.		
Lord Alemoor	$6^h\ 57^m\ 33^s$	$7^h\ 14^m\ 32^s$	18 inch reflector.
James Hay	6 57 30	7 14 35	3½f. achromatic, mag. 150.
Dr. Lind	6 57 41	7 14 37	2f. achromatic, magn. 100.

In the internal contact they all observed the black ligament or protuberance, which was not broken for some seconds after the regular circumference of Venus seemed to be within the sun; and the observation set down was, as near as could be judged, about the time this protuberance was going to break. Lord Alemoor also, and he only, observed the regular circumferences of the sun and Venus in contact, at $7^h\ 14^m\ 10^s$, mean time.

The morning promised ill, yet they got 9 very good altitudes of the sun near the prime vertical. About noon the day was terrible, with thick clouds, and like settled rain. About 2 o'clock the wind began to change from the south to the westward; about 3 o'clock it was west, and the clouds breaking; so that they got 5 very good corresponding altitudes. There was, about 4 o'clock, a very hard thunder shower, and calm, after which the wind began to blow briskly from the north-west; the clouds blown away, and those near the horizon depressed and held down, the sun shone clearer than ever he saw it, and not a cloud was to be seen in that quarter. It remained so till after both contacts; when, not half a minute after, small flying clouds passed over the sun. The night continued clear and serene, as did the next morning, till after the eclipse; half an hour after which it began to overcast, and put on the same cloudy appearance it has wore for some months past. Though the morning was so favourable, they lost the beginning of the eclipse, from being too long in getting to their posts; however they observed the contact with the different spots on the sun's disk, and the end of the eclipse, which was at $20^h\ 17^m\ 30^s$.

Remarks by the Astronomer Royal

Hawkhill is said by Dr. Lind to be about 1½ miles N. E. of Edinburgh. It is the seat of Lord Alemoor, one of their judges, who is fond of astronomy, and has built a small observatory there with a moveable roof, on Mr. Smeaton's plan. The corresponding altitudes, for determining the time of the observations of the transit of Venus, were taken, by reflection, from a basin of quicksilver or treacle, with a brass Hadley's sextant, made by Mr. Ramsden; the surface of the fluid being defended from the wind by a glass ground truly plane. They find that the equal altitudes seldom differ above 2 or 3 seconds in determining the time of noon; so that, by taking a great many at once, and taking the mean, they think they cannot fail of coming very near the truth. Mr. M. examined the equal altitudes made about the time of the transit, and the times of the contact are given corrected in the foregoing account. The clock in the observatory seems to go pretty well, though it only beats dead quarter seconds; it has a mahogany pendulum, and was made by Mr. Cummins. In the house was a clock beating seconds, and set, by means of the other, in the afternoon, before the beginning of the transit. The latitude of the place was also determined by meridian altitudes, taken by reflection with the sextant, and, by the mean of 10 observations, which all agree within the compass of 2 minutes, is 55° 57′ 37″ N. The end of the solar eclipse was observed by two persons with the two achromatic telescopes, with treble object glasses, and they agreed to a second.

Dr. Lind writes, another time, that, being from home, at Lees, near Coldstream, 7 miles west of Berwick, he observed the latitude of the place about 55° 37′.

The foregoing particulars are extracted from letters received from Dr. Lind. He has also communicated the following observations of the transit of Venus and solar eclipse, made by the Rev. Mr. Brice, at Kirknewton.

He is a very good astronomer, and is a writer in the Phil. Trans. Kirknewton is in lat. 55° 54′ 30″ N. and about 17 miles W. of Hawkhill, from measuring it on Lawrie's map of the environs of Edinburgh. The clock had been tried by several transits of a fixed star, and always found to measure time so exactly, that in the space of 5 days it did not differ one second from the truth; it was also examined by taking equal altitudes of the sun, and found to be 18 slow. The day was cloudy, with flying showers, till about 2 o'clock in the afternoon; then it grew somewhat clear, and about 4 the sun shone out exceedingly bright, when he observed carefully the spots on the sun; the brightness continued till about 15^m before 7^h, when a cloud came over the sun, which was not seen till 6^h 55^m 40^s mean time, as shown by the clock, and then Venus had made a sensible impression on the upper limb of the sun's zenith, and $\frac{1}{10}$, as he judged, on the sun.

Half on the sun, as he thought, ...	7^h	3^m	55^s
Internal contact clearly seen..............................	7	11	55
18^s added for the clock too slow		+	18
Makes	7	12	13

And if Venus takes 19^m from the first impression to the internal contact, the transit began at .... 6^h 53^m 13^s
Seen going till the sun set in a cloud near the horizon 8 24 39

When near the horizon, Venus's edge was full of notches and protuberances, and she appeared as if moving round like a wheel.

Eclipse of the Sun, June 4, common reckoning.

Beginning of the eclipse	6^h 27^m 50^s
Eclipse ended	8 15 50

There was an evident irregularity in the under edge of the moon, which entered on the south side of the sun, and traversed it from south to north.

The clock 18^s slow, to be added to the several observations. The internal contact is, when the thread of light was completed. From the above observation, and from every one of any credit, the Hawkhill gentlemen appear to be late in the external contact.

XLV. Observation of the Transit of Venus, and other Astronomical Observations made at Gibraltar; By Lieut. Jardine. p. 347.

To regulate the clock, an equal altitude instrument was fixed, nearly such as is described in Smith's Optics, vol. 2, p. 328, on which was mounted a small telescope with cross hairs, and the altitudes observed of the sun's upper and lower limbs.

Transit of Venus.

By 3 observers, with two 7½ feet refractors, and one 2 feet reflecting telescope.

June 3,	Venus's 1st external contact with the sun, at	6^h	49^m	58^s
	1st internal contact with the sun, at	7	7	11
	Sun set behind a hill........	7	8	3
	Clock before mean time	0	1	8.8

For the latitude of the place, they observed by a Hadley's quadrant, divided into minutes, the double meridian altitudes of Jupiter and Cor Scorpii, reflected from water. And for the longitude of the place, they observed several eclipses of Jupiter's satellites.

Eclipse of the Sun.

June 4,	First contact at........	6^h	6^m	54^s	seen perhaps a little too late.
	Last contact at........	7	19	28	exact.
	Clock before mean time	0	1	9	

Elev. of ☉'s limb by Hadley's quad. at { 1st contact 14° 41′ / 2d contact 28 55 } both exact.

Dip of the horizon, for 160 feet above the level of the sea is to be allowed.

Remarks by the Astronomer Royal.

By re-computing these observations, I find, that the external contact of Venus happened at 6^h 51^m 8^s, the internal contact at 7^h 8^m 21^s, the beginning of the eclipse of the sun at 18^h 8 0^s, and the end at 19^h 20^m 33, all apparent time; and that the latitude of the place, by the mean of the 4 altitudes of Cor Scorpii, is 36° 4′ 44″, N. The dip of the horizon of the sea, for an elevation of 160 feet, may be reckoned 12′ 5″. *Nevil Maskelyne.*

XLVI. Observations of the Transit of Venus over the Sun, June 3, 1769. *By John Winthrop, Esq., F. R. S.* p. 351.

Their apparatus having been wholly destroyed by fire, about 5 years before, they had procured a new set of astronomical instruments, made by some of the most eminent makers in London. A clock, by Mr. Ellicott, with the pendulum of his invention, having the bob supported by levers. An astronomical quadrant, of 2 feet radius, made by Mr. Sisson; and an equal altitude instrument, by Mr. Bird; each having 3 horizontal wires in the telescope. A reflecting telescope, of 4 feet focus; another of 2; and another of 1 foot; fitted with an object glass micrometer, of 21½ feet focus; all 3 made by the late Mr. James Short.

Mr. W. was obliged to remove the clock to another apartment, for the sake of the transit, which he did on the 23d of May, when he took some equal altitudes. By reason of an almost continual succession of cloudy weather till the end of that month, he could make but few material observations afterwards for regulating the clock. But the weather cleared up on the 1st of June, about noon, and continued fine for several days, when many observations of that kind were made.

Observation of the Transit of Venus.—Mr. W. chose to observe the transit with the 2-feet telescope, as he supposed most of the observations in other parts would be made with telescopes of that size; and he used a power that magnified 90 times, which gave a very distinct view of the spots then on the sun. Soon after 2 o'clock, he began to look on the sun's upper limb, where the planet was to enter. The first impression, he perceived, was at $2^h\ 27^m\ 51^s$, by the clock, the sun being then perfectly clear. He then rested his eye, which was pretty much fatigued, to prepare it for the total ingress or interior contact. At $2^h\ 45^m\ 15^s$, he began to be doubtful whether the internal contact was not formed; but at 20^s was satisfied that it was past, the sun's limb being restored to its integrity in the place where it had been interrupted by the planet. During this interval of near 5^s, there seemed to be a duskishness in the place of contact. By the foregoing equal altitudes it appears, that the clock was now $2^m\ 13^s +$ too slow. Mr. W. therefore states the observation as follows:

First visible impression of Venus on the sun	2^h	30^m	4^s	app. time.
Internal contact	2	47	30.	

This time of internal contact, he thinks, cannot differ above 2^s from the truth, and perhaps may not differ 1^s. But about 4″ of Venus's diameter must have entered on the sun before he perceived the impression. At 9 in the morning he observed the sun's diameter, in the horizontal direction, to be 1 21 1 parts of the micrometer, $= 31'\ 33''.2$. At $5^h\ 34^m\ 23^s$, the sun's north limb was distant from Venus's south limb 9 3 of the micrometer, $= 6'\ 16''.2$. At

5^h 37^m 23^s, he found no sensible difference; and the sun's north limb was then distant from Venus's north limb 7 14¼ of the micrometer, = 5′ 17″.6. This gives Venus's diameter 58″.6; and the least distance of centres 9′ 59″.7. Hence, the true duration of the ingress should be 18^m 56^s; but this being here contracted 15^s, by parallax, is reduced to 18^m 41^s. So that the first contact, strictly so called, happened $1\frac{1}{4}^m$ before the impression was discovered; and the central ingress was at 2^h 38^m 5^s. The nearest approach was nearly, he supposed, at 5^h 37^m.

After Venus was entered on the sun, Mr. W. viewed her attentively several times, with a power of the great telescope which magnified 260 times, but could perceive no such duskishness round her as he saw at the internal contact, nor that imperfect light on her disk, especially near the centre, which Mr. Dunn speaks of; neither could he discover any satellite. Soon after 6, the western sky began to be overcast, so that for a considerable time before sun-set the sun was hid. The latitude of the place is nearly 42° 25′ N. and the difference of meridians west from London about 4^h 44^m. But this may be further ascertained by the following emersions of Jupiter's satellites, which Mr. W. observed with the 2-feet reflector.

Emersions of Jupiter's first satellite.

	Apparent time.							
1768	April 25. . 9^h	13^m	52^s	1769	May 14. . 10^h	19^m	7^s
	May 18. . 9	27	27		Aug. 23. . 7	31	50
	June 10. . 9	37	25	Emersion of ♃'s second satellite.			
	July 3. . 9	45	54		June 7. . 9	1	15.

XLVII. Of the different Quantities of Rain, which appear to fall, at different Heights, over the same Spot of Ground. By Wm. Heberden, M. D., F. R. S. p. 359.

A comparison having been made between the quantity of rain, which fell in two places in London, about a mile distant from each other, it was found, that the rain in one of them constantly exceeded that in the other, not only every month, but almost every time that it rained. The apparatus used in each of them was very exact, both being made by the same artist; and on examining every probable cause, this unexpected variation did not appear to be owing to any mistake, but to the constant effect of some circumstance, which not being supposed to be of any moment, had never been attended to. The rain-gage in one of these places was fixed so high, as to rise above all the neighbouring chimneys; the other was considerably below them; and there appeared reason to believe, that the difference in the quantity of rain in these two places was owing to this difference in the placing of the vessel in which it was received. A funnel was therefore placed above the highest chimneys, and another on the ground of the garden belonging to the same house, and there was found the same difference between these two, though placed so near each other, which there had been between them, when placed at similar heights in different parts of the town. After this fact was sufficiently ascertained, it was thought proper to try, whether the difference would be greater at a much greater height; and a rain-gage was therefore placed on the square part of the roof of Westminster Abbey, being at such a distance from the western towers, as probably to be very little affected by them, and being much higher than any other neighbouring buildings. Here the quantity of rain was observed for a twelvemonth, the rain being measured at the end of every month, and care being taken, that none should evaporate, by passing a very long tube of the funnel into a bottle through a cork, to which it was exactly fitted. The tube went down very near to the bottom of the bottle, and therefore the rain, which fell into it, would soon rise above the end of the tube, so that the water was no where open to the air except for the small space of the area of the tube: and by trial it was found, that there was no sensible evaporation through the tube thus fitted up. The following table shows the result of these observations.

From July 7, 1766, to July 7, 1767, there fell into a rain-gage fixed

		Below the top of a house. inch.	On the top of a house. inch.	On Westminster Abbey. inch.
1766	from July 7 to the end	3.591	3.210	2.311
	August	0.558	0.479	} 0.508
	September	0.421	0.344	
	October	2.364	2.061	1.416
	November	1.079	0.842	0.632
	December	1.612	1.258	0.994
1767	January	2.071	1.455	1.035
	February	2.864	2.494	1.335
	March	1.807	1.303	0.587
	April	1.437	1.213	0.994
	May	2.432	1.745	1.142
	June	1.977	1.426	} 1.145
	from July 1 to the 7th	0.395	0.309	
		22.608	18.139	12.099

By this table it appears, that there fell below the top of a house above a 5th part more rain than what fell in the same space above the top of the same house, and that there fell on Westminster Abbey not much above one-half of what was found to fall in the same space below the tops of the houses. This experiment has been repeated in other places with the same event. What may be the cause of this extraordinary difference has not yet been discovered; but it may be useful to give notice of it, in order to prevent that error, which would frequently be committed in comparing the rain of two places without attending to this circumstance.

It is probable, that some hitherto unknown property of electricity is concerned in this phenomenon. This power has undoubtedly a great share in the descent of rain, which hardly ever happens, if the air and electrical apparatus be sufficiently dry, without manifest signs of electricity in the air. Hence it is, that in Lima, where there is no rain, they never have any lightning or thunder; and that, as M. Tournefort was assured, it never rains in the Levant but in winter, and that this is the only season in which any thunder is heard. If this appearance therefore could be accounted for, it would probably help us to some more satisfactory causes of the suspension of the clouds, and of the descent of rain.

XLVIII. Of an Eclipse of the Moon, observed at Hawkhill, near Edinburgh. By James Lind, M.D. p. 363.

Eclipse of the moon, Dec. 12, 1769, at Hawkhill.

	Sidereal time.				Mean time.		
1st contact of darkness	9^h	59^m	19^s	=	16^h	30^m	51^s
Moon's limb broken	10	7	50	=	16	39	21
Copernicus, central	10	30	32	=	17	1	59
Middle of eclipse	11	32	37	=	18	3	54
Moon's limb completed	12	57	24	=	19	28	27
2d contact of darkness	12	59	24	=	19	30	27
End of smoky appearance	13	5	25	=	19	38	27

Since Dr. L. wrote last, he had taken another meridian observation for the latitude, and made it 55° 57′ 30″ N.

Remarks by the Astronomer Royal.—The beginning of the eclipse was observed at the Royal Observatory at $10^h\ 20^m\ 29^s$, and the bisection of Copernicus at $10^h\ 43^m\ 23^s$ sidereal time; which, compared with the correspondent observations above, give $12^m\ 39^s$ and $12^m\ 51^s$ of time, for the difference of meridians of Hawkhill and Greenwich. *Nevil Maskelyne.*

XLIX. Of Two Auroræ Boreales observed at Oxford. By the Rev. John Swinton, B.D., F.R.S. p. 367.

Mr. S. casting his eye towards the northern part of the hemisphere, on Sunday, Feb. 26, 1769, about $8^h\ 30^m$ P. M. discovered there a pretty bright aurora borealis of the common kind. For a short time, there was a very quick succession of lucid columns, and corruscations; which seemed smaller than they usually are in such meteors, that appear often enough here. But what principally engaged his attention, was the gradual approach of the aurora towards the south, insomuch that though it was at first most apparently an aurora borealis, and that of the common kind; yet, by the gradual variation of its original position, seemed to have commenced a sort of aurora australis.

Being in his parlour, with the sashes down, on Saturday, September 9, 1769, at 8^h 20^m P. M., Mr. S. observed, with no small degree of astonishment, through the glass, such a redness in the sky as proceeds from the reflection of a great fire. This he was at first inclined to consider as a sort of deception, occasioned by the glass through which so uncommon an object seemed to present itself to his view, but stepping out immediately into the yard, he found it to be a real appearance, resembling a flame, in the atmosphere, and consequently a very unusual sight. The meteor was, however, of no very considerable extent; being almost entirely confined to that small tract of the heavens occupied by Ursa Major, part of Ursa Minor, and the intermediate space, containing the tail of Draco, between those 2 constellations. It remained about 20^m after he first discovered it, without any material change, or variation; and at 8^h 40^m almost instantly disappeared.

The wind on the 9th was, for the most part, W. and S. W. and the weather showery. The rain, however, notwithstanding the favourable situation of the wind, was somewhat cold, and the whole day had a lowering winterly aspect. A small shower fell, just before he discovered the phenomenon here described. The light cast by it was nearly equal to that of the full moon on a cloudy night. The 10th the wind continued in the same quarter as before; and the weather was much the same, attended by a disagreeable chillness in the air, as that of the preceding day. All the principal stars of the above-mentioned constellations very clearly and distinctly appeared, through the seemingly fiery vapour, with which the tract occupied by them was so strangely and so remarkably tinged.

As the luminous appearance seen at London, between 8 and 9 o'clock, the same night, from the short account given of it in one of the public papers, seems to have agreed in all respects with that observed at Oxford, at the very same time, it may be considered, without any impropriety, as the very meteor here described. Admit this, and it must be allowed, that the atmosphere was at London in the same disposition, with regard to the exhibition of this species of meteors, as at Oxford, the very same instant of time; and impregnated in both places with the same kind of luminous vapour, at that instant, which occasioned the production of the phenomenon he has here been endeavouring to describe.

L. Observations of the Transit of Venus, June 3, 1769, and the Eclipse of the Sun on the following Day, made at Paris, and other Places. Extracted from

Letters addressed from M. De la Lande, to the Astronomer Royal; and from a Letter from M. Messier to Mr. Magalhaens.* p. 374.

M. Messier, with the best achromatic telescope at Paris, of 12 feet focus, made by M. Antheaulme, observed the first internal contact at $7^h\ 38^m\ 46^s$ apparent time reduced to the Royal Observatory, and, he thinks, without an uncertainty of 2 seconds: and this is the observation in which M. D. most confided. M. Du Sejour, and M. Cassini, the son, at the Royal Observatory, with much less telescopes, observed it also at $7^h\ 38^m\ 43^s$. M. De Fouchy, M. Bailly, M. De Bory, and two opticians, who were at the Meute, observed the contact at $7^h\ 38^m\ 45^s$, reduced to Paris. M. Cassini de Thury, at the Royal Observatory, noted it at $7^h\ 38^m\ 53^s$; M. the Duke de Chaulnes at $7^h\ 38^m\ 57^s$; both with new achromatic telescopes of Dollond of $3\frac{1}{2}$ feet. M. Maraldi, at the Royal Observatory, observed at $7^h\ 38^m\ 50^s$, with a good achromatic telescope of 3 feet, made at Paris, but he thinks the observation liable to an error of 10 seconds. M. Le Monnier, at St. Hubert, observed at $7^h\ 38^m\ 51^s$, reduced to Paris, by adding $1^m\ 58^s$. M. Fouguere, at Bourdeaux, at $7^h\ 38^m\ 50^s\frac{1}{2}$, reduced to Paris, taking in the difference of parallax, which is 2 seconds greater at Paris than Bourdeaux.

They received several observations of the eclipse of the sun. M. de Thury saw the beginning at $6^h\ 46^m\ 49^s$ apparent time. M. Jeaurat, at $6^h\ 46^m\ 40^s$, at the Military School, which is $7^s\frac{1}{2}$ to the west of the observatory. M. Maraldi saw the end at $8^h\ 27^m\ 11^s$. M. Jeaurat at $8^h\ 27^m\ 4^s$, or $8^h\ 27^m\ 11^s\frac{1}{4}$ reduced to the observatory. M. Messier at $8^h\ 27^m\ 24^s$.

The observers at Rochefort, Lyons, and Avignon, did not see the transit of Venus; it was observed at Brest at $7^h\ 12^m\ 5^s$, or 40 seconds later than at Paris, if we suppose the difference of the meridians to be well known. This point shall be examined hereafter. M. D. could not observe the internal contact of Venus; he was precisely in the place where the clouds came on 25 seconds too soon; neither was it observed at the Military School, which is close to Paris. There was also another observer at Brest, M. Verdun, an officer of the marine, who observed the internal contact of Venus at $7^h\ 11^m\ 37^s$ apparent time, which makes $7^h\ 38^m\ 58^s$, reduced to Paris. The end of the solar eclipse was observed at Brest at $7^h\ 56^m\ 33^s$, and $7^h\ 56^m\ 44^s$, by the different observers.

M. Pingré, at Cape Francois, observed the two contacts of Venus in the latitude of 19° 47 at $2^h\ 26^m\ 12^s$, and $2^h\ 44^m\ 44^s$, apparent time, with a 5 feet achromatic telescope, but we do not yet know the longitude of the place

* This celebrated astronomer, Hieronimus De Lalande, died at Paris the 7th of April, 1807, at 75 years of age. By his will he ordered his body to be dissected, and his skeleton to be placed in the Museum of Natural History. But his friends, regardless of the injunction, caused the body to be interred; and his funeral was attended by most of the members of the National Institute.

sufficiently. At Martinico, one of our missionaries observed the contacts at $3^h\ 15^m\ 14^s$, and $3^h\ 33^m\ 57^s$; when we have the longitude exact, this observation will also be of use.

Here are some observations of the 1st satellite made at Gottingen, just received from Mr. Liunberg.

1769	April 21	Im. 1 Sat.	13^h	21^m	37^s	Refractor of 14 feet.
	28	....	15	14	34	Ditto of 10 f. of Liberkuhhn.
	30	....	9	43	21	The same.
	May 23	Em. 1	12	6	31	

So far M. De Lalande.

The Extract of M. Messier's Letter to M. Magalhaens is as follows:

I observed the transit of Venus, June 3, 1769, at the college of Louis le Grand at Paris, which is 2^s to the east of the meridian of the Royal Observatory. I had an achromatic telescope, of 12 feet focus, which had an aperture of 3¾ inches, and magnified 180 times, with the view of making my observation more correspondent to that which M. the Abbé Chappe was to make, in California, with a telescope of the same length, the same magnifying power, and equal goodness. The first contact could not be seen, on account of a very thick cloud; there even fell some rain. I waited for the 2d; the sun then was pretty clear. But there were some vapours, which caused such great undulations as to hinder me from seeing the disk of the sun, and that of Venus, well defined. At $7^h\ 38^m\ 45^s$ apparent time, or $7^h\ 38^m\ 43^s$ reduced to the meridian of the Royal Observatory, the 2d contact appeared decisively to me. Two seconds after, a very fine thread of light appeared between the limb of Venus and that of the sun; so that in my observation there is not an uncertainty of 2 seconds in the moment of the internal contact. After this observation, I viewed the sun with different glasses, which rendered him alternately red and white. I saw Venus, with this last colour, with a crescent of a bluish colour; and a little inclined towards the limb of the sun: with the glass which made the image of the sun red this crescent disappeared; but I saw Venus flattened in the direction of the crescent. I measured the greatest and least diameter; the greatest was 56″½, and the least 53″½. Perhaps this crescent was only visible by the effect of some optic illusion; but I relate only what I saw. At $7^h\ 52^m\ 8^s$, apparent time, I measured the interval between the limb of Venus and that of the sun, which I found 46″½; and at $7^h\ 58^m\ 4^s$, the first limb of Venus touched the horizon.

By a letter from M. l'Abbé Bourriot to Mr. Magalhaens it appears, that Messrs. de Fouchy and Bailly, at the Meute, each made use of reflecting telescopes of 30 inches focus and 4½ inches aperture; that M. Bory made use of an achromatic telescope of 5 feet focus, and 2 inches aperture; and M. l'Abbé Bourriot made use of a very good achromatic telescope of 6 feet long, and 2¼ inches aperture, made by himself, magnifying 120 times.

LI. Transit of Venus over the Sun, observed June 3, 1769, *by Alex. Aubert,* in Austin Friars, London,* 3 *Seconds of Time East of St. Paul's, with a Cassegrain Reflector of J. Short, having a Metal of* 2 *feet focal Length, and magnifying about* 110 *Times.* p. 378.

External contact at.	7^h	8^m	13^s	mean time.
Internal contact at	7	26	45	interval 18^m 32^s.

At 7^h 26^m 45^s Venus appeared in contact with the sun, and about 6^s after Mr. A. saw the sun's limb compleated. The clock could be depended on to less than 1 second, having been compared with a number of equal altitudes of the sun, some days before and after the transit.

LII. Some Account of an Oil transmitted by Mr. George Brownrigg, of North Carolina. By Wm. Watson, M. D., R. S. S. p. 379.

Dr. W. here lays before the R. S. some pods of a vegetable, and the oil pressed from their contents. They were sent from Edenton, in North Carolina, by Mr. George Brownrigg, whose brother, Dr. Brownrigg, is a worthy member of the R. S.; and are the produce of a plant well known, and much cultivated, in the southern colonies, and in the American sugar islands, where they are called ground nuts, or ground pease. They are originally, it is presumed, of the growth of Africa, and brought from thence by the negroes, who use them as food, both raw and roasted, and are very fond of them. They are therefore cultivated by them in the little parcels of land set apart for their use by their masters. By these means, this plant has extended itself, not only to the warmer American settlements, but it is cultivated in Surinam, Brasil, and Peru.

The plant, which produces these, has been mentioned, and described, by the botanical writers of the later times. Ray, in his History of Plants, calls it Arachis Hypogaios Americanus. It is the Arachidna quadrifolia villosa of Plumier. Sir Hans Sloane, in his History of Jamaica, calls it Arachidna Indiæ utriusque tetraphylla. Piso and Marcgrave both mention it among the Brasilian plants, under the name of Mundubi. Linnæus has constituted a genus of this plant, of which only one species is as yet known, under Mr. Ray's generical name of Arachis.

This plant, with a very few of the trifoliate tribe, has the property of burying its seeds under ground, which it does in the following manner. As soon as the

* Alex. Aubert, Esq. F. R. S., of Highbury-House, died in Nov. 1805, in the 76th year of his age. He was also vice-president of the Society of Antiquaries, and governor of the London Assurance Company. Mr. A. was not unskilled in other sciences, but he was more particularly attached to that of astronomy, and he possessed one of the neatest private observatories, and collections of instruments in any country. For his particular suavity of manners too, and elegant gentlemanly deportment, Mr. A. was very generally admired.

plant is in flower, its flower is bent towards the ground until it touches it. The pointal of the flower is then thrust into the ground to a sufficient depth, where it extends itself, and forms the seed-vessel and fruit, which is brought to maturity under ground, whence it is dug up for use. Being a native of warm climates, it cannot be cultivated to advantage in Great Britain, or in the northern colonies; but, according to Mr. Brownrigg, in southern climates its produce is prodigious; and what adds to its value is, that rich land is not necessary for its cultivation, as light sandy land, of small value, will produce vast crops of it. Besides what the negroes cultivate for their own use, some planters raise a considerable quantity of it, for the feeding of swine and poultry, which are very fond of the ground pease; and, when they are permitted to eat freely of them, soon become fat.

Mr. Brownrigg considers the expressing oil from the ground pease, as a discovery of his own: it may perhaps at this time be very little practised, either in North Carolina, the place of his residence, or elsewhere. But certain it is, that this oil was expressed above 80 years before; as Sir Hans Sloane mentions it, in the first volume of his History of Jamaica; and says, that this oil is as good as that of almonds. It is probable however, that small quantities only were expressed, and that even at that time the knowledge of it did not extend very far. Mr. Brownrigg therefore is highly praise-worthy in reviving the remembrance of procuring oil from these seeds. It is obtained, by first bruising the seeds very well, and afterwards pressing them in canvas bags, as is usual in procuring oil from almonds or linseed. To have the oil in the best manner, no heat should be used. The heating the cheeks of the press increases the quantity of the oil, but lessens its goodness, where it may be intended to be used as food, or as a medicine. For other purposes, the larger quantity of oil, obtained by heat, will answer equally well. Neither the seeds nor oil are apt to become rancid by keeping. These seeds furnish a pure, clear, well-tasted oil; and, as far as appears, may be used for the same purposes, both in food and physic, as the oils of olives or almonds. It may be applied likewise to many, if not all, the economical purposes with the former of these.

But what greatly adds to the merit of what Mr. Brownrigg has informed us of, is the low price at which this oil may be obtained. He says, that 10 gallons of the pease, with the husks unshelled, will, without heat, yield one gallon of oil; if pressed with heat, they will afford a much larger quantity. The value of a bushel of these, in Carolina, does not exceed 8 pence, or thereabouts. These will furnish a gallon of oil, the labour and apparatus to procure which, cannot cost much. This price will not amount to so much as a 4th of what the best Florence oil of olives costs in England. This therefore ought to be considered as valuable information, as, on account of its cheapness, a larger portion of mankind than at present may be permitted to use oil with their food, from whom it is now withheld on account of its price.

Great quantities of olive oil are sent from Europe to America. New England alone, Mr. Brownrigg says, annually consumes 20,000 gallons. The quantities used in his majesty's other dominions in America must be prodigious. The oil from ground pease, of which any quantity desired may be raised, may and would supply this consumption of olive oil. It would likewise bear exportation to any of those places where the oil of olives is usually carried; and thereby become a valuable article of commerce. After the oil has been expressed from the ground pease, they are yet excellent food for swine.

LIII. A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1768, *pursuant to the Direction of Sir Hans Sloane, Bart. By Wm. Hudson, F. R. S.* p. 384.

This is the 47th presentation of this kind, completing to the number of 2350 different plants.

LIV. A Description of the Lymphatics of the Urethra and Neck of the Bladder. By Henry Watson, Surgeon to the Westminster Hospital, and F. R. S. p. 392.

The valvular lymphatics, as a system of vessels, sui generis, are allowed to have a very considerable office in the animal economy; but an office, subordinate to that of the blood-vessels: at least they have been supposed by many of the physiologists, not of so much consequence, in preserving the health and life of the animal. If we consider, that an obstructed thoracic duct, which is in fact but a large lymphatic, will destroy life as effectually as a ligature made upon the aorta itself; we must conclude the lymphatics to be vessels of much greater importance than some have imagined: nearly of as much consequence, in supporting and carrying on the animal functions, as the arteries and veins themselves: for if an obstruction of the aorta, or great artery, can produce a very quick, or sudden, death; an obstructed thoracic duct will as certainly lead to a tedious and lingering one. The case of the obstructed duct, though not indeed often seen, yet is every now and then to be met with. It is the one cause of a marasmus not known, or not attended to: generally owing to an enlargement of the lymphatic glands that lie near to, and in contact with the duct: generally too attended with obstructions in the more external conglobate glands; therefore always to be suspected, where we have these appearances, accompanied with a gradual wasting of the solids. In children and young subjects we meet with proofs of this disease; a disease, which never could have been learnt, but from the dissection of morbid bodies.

The lymphatics are said to be the true, and only system of absorbing vessels. I will suppose they are; though perhaps this opinion may yet admit of some doubts: however, they certainly are the vessels that take up the watery latex from most parts of the body, and return it back to be again mixed with the blood. This free absorption of the lymph is the great security against suffocation injurious pressure, and an obstructed circulation in every part of the animal. Many valuable discoveries have lately been made, of the existence of these vessels in birds, fish, and amphibia. That most accurate and indefatigable anatomist, Dr. Hunter, has written fully and explicitly on the lymphatics in the human body; and yet, still it is to be wished, we knew more about them.

We have not been able to see their origins in any one instance; we have not traced them through the whole body, as we have done the blood-vessels. It is reasonable to suppose they abound universally; but it is doubtful whether in many parts they exist, or not; for the most eminent anatomists confess that, there are many parts, in which hitherto they have not been able to discover them.

It may not therefore be unentertaining to this learned society, who so studiously promote every useful inquiry, not only to have a demonstrative proof of the existence of lymphatics in a part of the human body where they have not as yet been discovered; but also to have an opportunity of knowing that the true origin of these vessels may easily be shown. As to their precise origin, it has indeed been conjectured, and very reasonably, from experiments à posteriori. It has been supposed they arise from all the surfaces and cavities of the body; because thin fluids and subtle particles will be taken up from such cavities, or surfaces, and will be readily enough conveyed into the blood: but then it has never been shown, that they do arise from any one such surface or cavity.

Commonly, the lymphatics are never filled from their beginnings, or little orifices. When they have been injected, it has always been done by using some violence; either by cutting into them, bursting, or tearing them asunder; so that the injection rather gets in some how at the side, and not at the extremity of the vessel.

The lacteal vessels perhaps cannot, at least have never been, to my knowledge, injected from the cavity of the intestine in the dead body. It is presumed that, as the lymphatics are similar to these in other respects, their origins must be also similar: that if the orifices of the lacteals are too fine to be discovered, the mouths of the lymphatics are also too delicate to be traced out. But with regard to the lymphatics of the human urinary bladder, it is certainly otherwise. When the part is fresh and sound, we may, with a little trouble, blow into the mouths of these vessels, small as they are; or introduce a fine bristle into them, if we have but a steady hand and a good eye. I have frequently done both, in the presence of many witnesses; so that, without using the knife or lancet, the least force or violence, air may be thrown into the lymphatics from their very beginnings; and mercury may be made to pass by the same orifices.

Though I have said we may easily have an ocular demonstration of the origin or mouth of the lymphatic, in this part of the human body, I must confess it is not always we can have that satisfaction: no part is more frequently diseased: inflammation solders up the mouths of these little vessels; and it is not to be expected we can show their orifices when the urethra is in such a state. It will always require some dexterity to catch the opening of the lymphatic; but the bristle, once fairly introduced, will generally pass with great ease some way within the vessel. Here then we may satisfy ourselves in what manner the lymphatics do actually begin from surfaces: and to those who, without ever having seen the origin of a lymphatic, have nevertheless reasoned so well, and so justly, on this subject, it may perhaps afford some pleasure and satisfaction to find their conjectures agreeing so perfectly with the structure.

The situation of the lymphatics, in general, is superficial; that is to say, they are mostly to be seen on surfaces; though there are some deeper seated ones, which accompany the blood-vessels. They have been well described by authors, as exceedingly fine, tender, and transparent vessels, frequently joining into one another, and intersected by a number of very delicate membranous pouches or valves; so that, having an injection thrown into them, they give the appearance of being full of little knots. The lymphatics are apparent enough, when they unite and grow large; but from their exility, want of colour, and transparency, they are very difficult to be discovered before.

Owing to these circumstances it is, that their origins have never before been seen; and that in many parts of the body, where they are nevertheless supposed to exist, they still lie unnoticed. Haller, after speaking of these vessels in many other parts of the body, goes on thus: ‘ Quæ a pene veniunt mihi minus nota sunt, sed dicta Cowpero. Alia huc a vesiculis seminalibus tendunt, aut certe ab earum vicinia, aut a vesicæ urinariæ sede, aut ab ipsa demum vesica, quæ quidem vascula iterum fateor mihi nondum visa esse.’ So that Haller, who knows so well the structure of the human body, knows nothing of these lymphatics of the bladder, or membranous portion of the urethra. The lymphatics of the urinary human bladder and urethra, first appear on each side the verumontanum or caput gallinaginis, and by very little orifices take their origin from the internal membrane that lines the urethra and bladder, on whose surface they open. In their natural state, they appear like so many fine threads lying close together, but diverging afterwards, as they pass over the prostate gland and neck of the bladder, and inosculating or communicating very frequently, they form a kind of network or embroidery. Hence they are continued through the cellular membrane, behind the internal coat of the bladder, and seem to join with the lymphatics of the vesiculæ seminales, to be continued with them to the neighbouring glands, and so on to the thoracic duct.

I have not been able to find lymphatics in any other part of the urethra; indeed this canal seems to be perfectly void of them till we come to its membranous portion, where we meet with these I have been describing; and it may be remarked, that here they are placed in that part of the urethra where the greatest quantity of moisture is supplied. Very probably the sealing up the mouths of these delicate vessels, by frequent inflammation and induration, may give rise to that obstinate stillicidium which is seldom if ever cured; owing to an accumulation of thin fluids, with a faulty absorption.

These lymphatics of the urethra and bladder also point out the road, by which any subtle virus may pass, with the lymph, directly into the mass of blood, and contaminate the whole habit, without giving the least appearance of any local disorder. To have a clearer idea of the vessels I have been treating of, I must beg leave to refer to the drawing annexed, in which these lymphatics of the urinary bladder and urethra, in the human body, are carefully and very accurately delineated.

Fig. 11, pl. 17, is an exact representation of the lymphatics of the urethra and neck of the bladder, as they appear after having been injected with mercury, and preserved in spirits.

A is the membranous portion of the urethra slit open. BB, bristles in the ducts from the vesiculæ seminales. CC, the prostate gland. DD, the inferior part of the bladder laid open. EE, bristles in the ureters where they open into the bladder. FF, the lymphatics.

LV. Eclipses of Jupiter's First Satellite, the Eclipse of the Moon, and Occultation of Fixed Stars by the Moon. Observed at the Royal Observatory at Greenwich, 1769. p. 399.

These observations may now be consulted in the general collection of all the Greenwich observations, which have since been published by Dr. Maskelyne, under the authority of the Royal Society.

LVI. Eclipses of Jupiter's First Satellite, with an Eighteen Inch Reflector of Mr. Short's. Observed by Dr. Wilson at the Glasgow Observatory. p. 402.

				Equal time.				Apparent.				
1762	Sept.	11	Im.	10h	55m	33s	21th	10h	55m	33s	21th	good.
	Oct.	4	Im.	11	1	58	—	11	13	22	21	good.
	Nov.	14	Em.	6	8	33		6	23	43	55	indifferent.
		21	Em.	8	1	31	38	8	16	39		good.
1763	Oct.	23	Im.	12	50	24	—	13	5	56	.53	good, air extremely clear
	Nov.	1	Im.	9	12	18	43	9	28	29	18	good, like the former.
1764	Oct.	25	Im.	16	8	48	—	16	24	40		indifferently good.
1765	Febr.	14	Em.	6	18	28		6	3	55		good.
	March	7	Em.	12	2	24		11	51	29		good.
	Nov.	29	Im.					15	53	26		indifferent.
	Dec.	22	Im.	15	54	4	—	15	54	25		indifferent.
1766	April	20		8	4	23	—	8	5	41	15	good.

Solar Eclipse.

1769 March 31	Beginning	20h	58m	16s	20h	54m	15s	9th
	Gr. obs.	22	19	3	22	15	2	
	End	23	46	55	23	42	52	

1769 Nov. 6. ☾'s west limb. Merid.	Alt. of ☾'s under limb.	Parts of 96. parts.
6h 52m 6s	20° 47′ 36″+	22°. 1′. 14

Barom. inches.	Thermom. within.	Thermom. without.	
29.66	49°	46°	This is the only one of the moon since the quadrant was in order.

The equation of time made use of, is taken from De la Caille's Ephemeris.

Observations of Eclipses of Jupiter's First Satellite, proper to be compared with the foregoing ones, in order to determine the Difference of Meridians of Greenwich and Glasgow. Communicated by the Astronomer Royal.

				apparent time.				
1762	Sept.	11	Im.	11h	12m	43s	Surry-street, 2 feet reflector.	By N. Maskelyne.
	Oct.	4	Im.	11	31	1	Surry-street, 2 feet reflector.	Observed by J. Short.
	Nov.	3	Em.	15	56	57½	Greenwich, 6 feet reflector.	By C. Green.
	....	12	Em.	12	10	56	Surry-street, 2 feet reflector.	By J. Short.
1763	Oct.	16	Im.	11	27	39	Surry-street, 2 feet reflector.	By J. Short.
	Nov.	1	Im.	9	45	25	Surry-street, 2 feet reflector.	By J. Short.
1764	....	4	Im.	13	3	37	Greenwich, 2 feet reflector.	By C. Green.
	....	10	Im.	14	57	11	Greenwich, 6 feet reflector.	By C. Green.
1765	Feb.	19	Em.	13	46	42	Greenwich, 6 feet reflector.	By C. Green, good observation.
	Dec.	1	Im.	10	40	11	Greenwich, 18 inch reflector.	By N. Maskelyne
	....	8	Im.	12	31	34	Greenwich, 6 feet reflector, air very clear.	By N. Maskelyne
	....	15	Im.	14	22	3	Greenwich, 6 feet reflector.	By N. Maskelyne
	....	22	Im.	16	12	19	Greenwich, 6 feet reflector.	By N. Maskelyne
	....	24	Im.	10	39	27	Greenwich, 6 feet reflector.	By N. Maskelyne
1766	April	11	Em.	11	56	30	Greenwich, 6 feet reflector.	By J. Dymond.

The late Mr. Short's house in Surry-street, where some of the above observations were made, is 26s½ of time west of Greenwich.

LVII. Extract of a Letter to the Rev. Nevil Maskelyne, Astronomer Royal. From Mr. Benedict Ferner, F.R.S. Dated Stockholm, June 9, 1769. Translated from the French. p. 404.

I am more surprized that the times of the contacts of Venus and the sun's limbs, observed here, by different observers, with different instruments, agree so near together, than I am at their difference; for the nearness to the horizon, and the extraordinary quantity of vapours with which the atmosphere was then loaded, not only caused the limb of the sun to tremble and undulate, but also give it as it were the form of a large saw, the eminences being luminous and the cavities black, which shifted places like the waves of a tempestuous sea.

There was no reason for fixing the moment of the ingress of Venus sooner than she had made a greater cavity in the limb of the sun than the depth of the waves or black notches; and then one might be very sure of the fact: but certainly at that time some seconds must have been passed from the beginning of the ingress. Therefore, I am very well persuaded, that $8^h\ 24^m\ 9^s$ apparent time, which I took for the beginning of the ingress, is 4, 5, or 7 seconds too late. I hoped to see the internal contact with more certainty; but I was mistaken; for I found as great difficulty there, though of another kind. When I judged, by means of the circular figure of the sun's disk, that Venus should be entirely within the sun, I could not yet see the luminous cusps of the sun join together behind Venus, who on the contrary appeared to carry the limb of the sun along with her, which appeared to bend towards Venus, leaving a black cavity in his limb; and a moment after, when I thought I saw the whole body of Venus in the sun, a little black column appeared to proceed from Venus towards the imaginary limb of the sun. The whole of this phenomenon was certainly, in my opinion, the effect of the tremors of the limbs of the sun and Venus; but I took $8^h\ 41^m\ 48^s$ for the moment of the internal contact, when the thread of the sun's light closed behind Venus.

The limbs of Venus were at least as tremulous and ill defined as those of the sun. Sometimes Venus had black eminences, which projected so much that they were not unlike a pointed truffle. The first notch made by Venus in the sun was not round, but resembled an obtuse angle. The diameter of Venus, which was perpendicular to the sun's limb, appeared the greatest while Venus was passing over the sun's limb; but after Venus had passed the sun's limb, the same diameter appeared the smallest; so that Venus presented herself in both these cases under an oval form, but in contrary directions.

Clouds hindered us from observing the beginning of the eclipse of the sun; but I observed the end of the eclipse, at $10^h\ 4^m\ 53^s$ ap. time, with an achromatic telescope of Dollond, of 10 feet, magnifying 96 times; the same telescope which I used in observing the transit of Venus. The difference of meridians between Stockholm and Upsal is $1^m\ 40^s$ of time.

LVIII. Observations of the Transit of Venus over the Sun, on June 3, 1769; *and the Eclipse of the Sun the next Morning; made at East Dereham, in Norfolk. By the Rev. Francis Wollaston, F.R.S.* p. 407.

The telescope Mr. W. used was a reflector of Short's, of 12 inches focus, with a power that magnifies about 55 times. His clock was made by Holmes. It escaped dead seconds, had a pendulum with a wooden rod, and was firmly fastened to a stack of chimneys in the room where he made his observations. He regulated it by transits of the sun and stars, taken with a transit instrument of a peculiar kind; and also by equal altitudes of the sun, taken with another instrument of his own make. By several transits of the sun and fixed stars, observed in the latter end of May and the beginning of June, he had brought it to go at the rate of mean solar time very nearly, i. e. losing but 1^s in 14 days; and by transits of the sun, it appeared to be 1^s.5 too slow for mean time, on the 3d of June. But by a mean of 8 days' observations of equal altitudes, taken in June and the beginning of July, the clock appeared 5^s.2 slower than was found by the transit telescope, which was not perfectly adjusted; though, as its error continued always the same, it was very sufficient for determining the rate of the going of the clock. Hence, on the day of the transit, the clock was 6^s.7 or 7^s too slow for mean time; and the observations that follow are corrected accordingly.

In watching for the first contact of Venus, Mr. W. kept his eye on the sun's edge where the contact was expected; keeping that point nearly in the centre of the field; and the first impression which he saw, without any penumbra or atmosphere that he could perceive, was at $7^h\ 12^m\ 32^s$ by his clock, or $7^h\ 12^m\ 39^s$ mean solar time. The dark part on the sun did not appear with a smooth edge; and yet he could not discern any undulation in it. Mr. W. tried his strongest magnifying power of 110, but to no purpose; for there was an undulation on the sun's edge, by that time, so great, that he thought it best to return to the former power of 55. Before the internal contact, at about $7^h\ 24^m$, he lost the sun entirely, and though there were a few breaks in the clouds, he never appeared more that evening.

As to the eclipse the next morning, Mr. W. saw that more perfectly; though at first there were many flying clouds. It began here at $18^h\ 42^m\ 17^s$ by the clock, or $18^h\ 42^m\ 24^s$ mean time (a cloud had just passed; but he can be sure it was not visible 2 or 3^s before); and it ended at $20^h\ 28^m\ 1^s$ by the clock, or $20^h\ 28^m\ 8^s$ mean time. The latitude of the place, as well as he could determine by various observations, was $52^\circ\ 40'\ 20''$ north.

LXIX. Observations of the Transit of Venus over the Sun, June 3, 1769; *made by Mr. Owen Biddle and Mr. Joel Bayley, at Lewestown, in Pennsylvania.* p. 414.

May 26, 1769, the above observers arrived at Lewestown (on Cape Hinlopen at the mouth of Delaware Bay), being ordered there by the American Philosophical Society, held at Philadelphia, for promoting useful knowledge, to take an observation of the ensuing transit of Venus over the sun's disk, and immediately set about fixing their time-piece, telescopes, and equal altitude instrument. The 27th, they got some good corresponding altitudes of the sun, by which they set the clock; and took equal altitudes of some of the fixed stars, to prove the rate of the clock. After this it continued cloudy, with rain at times, and a high wind at north-east, till the 31st, when the clouds broke a little. During this time, they employed themselves in measuring the distance of the place of observation from the stone fixed at the beginning, or east end, of the east and west line, which is the boundary between the three lower counties and Maryland, and is situated on Fenwick's Island; the latitude and longitude of this place being accurately determined by Messrs. Dixon and Mason.

The meridional difference of the latitude of the place of their observation, north from Fenwick's Island, at the beginning of the east and west line, as before described, being the easternmost end of the southern boundary between the lower counties and Maryland, is 19′ 41″ 24‴; and the meridional difference of longitude of the place of observation, west from the point aforesaid, in Fenwick's Island, 5′ 45″ of a degree. These data, with the latitude and longitude of the Station point, will determine exactly the place of observation.

June 2, the weather being clear, had good corresponding observations of the sun.

June 3, the weather being remarkably fine, they had good observations to set the clock. About 12 o'clock they began to direct their glasses to the sun, keeping it continually in the field till the time the observation was past. They agreed to watch the telescope one minute in turn, till about 7 or 8 minutes before the contact was expected, lest, by too steady an attention to the glasses, their sight should be impaired, so as to disable them from discerning the contact clearly. Mr. Biddle had left his telescope the minute preceding the contact, intending to apply himself steadily to it, from the next minute, till the observation was past; and when the 48th second was called, he applied himself to the glass, and by the time 3 seconds were elapsed, he perceived, on that part of the sun's limb where he expected the contact, a small impression, which proved to be the limb of Venus in contact with the sun. All the limb of the sun, which appeared at that time in the field of the telescope, had a small undulatory motion, which he apprehends was owing to dense vapours, which arose at that place, being near the sea. At Venus's first appearance, it was only like one of those waves on the limb or border of the sun, increased in so small a proportion, that he was doubtful for several seconds, whether it was any thing else; thus it continued, making a deeper impression, with that tremulous motion, for about 10 seconds, when the tremor where Venus was in contact ceased, and the indenture was truly circular, with an even termination.

His absence from the telescope, just before the contact occurred, deprived him of an opportunity of judging whether there was any appearance of an atmosphere preceding the western limb of Venus in contact; but when Venus had entered nearly one half of its diameter into the sun's disk, the observers both saw a luminous crescent, which enlightened that part of Venus's circumference which was off the sun, so that the whole of her circumference was visible, but did not continue so until the internal contact; and at the time of the first internal con tact, the eastern or external limb of Venus seemed to be united to the sun's limb by a black protuberance or ligament, which was not broken by the entrance of the thread of light, till 4 seconds after the regular circumference of Venus seemed to coincide with the sun's.

The telescope Mr. Biddle made use of for viewing the transit, was a reflecting one, belonging to the Philadelphia Library Company, the speculums of which are 2½ feet apart, and the lenses in the eye tube 4 inches apart; it was the least magnifying power that he used, as he found the tremulous motion too much magnified by the other power. The small one was in good order, and defined the sun's limb, and spots on its disk, very clearly. He had applied a polar axis to it, and made some rack-work, by which he could keep the same part of the sun's limb in the field with ease; his companion was not so well provided with a telescope, the one he used being of Dollond's refracting glasses of 4½ feet. This they fixed, with a ball and socket to a post, by which it was easily directed to the sun. Thus furnished, they found the contacts to take place as follows, reduced to mean time.

Owen Biddle's external contact at	2^h	11^m	53^s
.... internal one at	2	29	53
Joel Bayley's external contact was lost by an accident, but seen by him, after it had taken place, at	2	12	15
.... internal ditto	2	29	53

The internal contact, given by Owen Biddle, is at 4 seconds before the thread of light had broken the dark ligament or protuberance, by which Venus's limb was united to the limb of the sun, that being the time he estimated the two limbs to be in contact. The internal contacts they think may be relied on; the external happening sooner than expected, occasioned a doubt at its appearance, which made the exact second of its appearance a little uncertain.

The difference of latitude of the place of observation, north of Middle Point, 21.93 miles. The meridian distance of the place of observation, east of Middle Point, 30.6356 miles.

The latitude and longitude of Middle Point were taken by Messrs. Dixon and Mason, and probably communicated to the R. S.

Remarks by the Astronomer Royal.—From the data given above, and the length of a degree of latitude, found by Messrs. Mason and Dixon, in these parts = 68.896 English miles, the difference of latitude of Lewestown and the Middle Point above-mentioned, which is the same with the point A, in Messrs. Mason's and Dixon's measure of a degree preceding, is 19′ 53″; but the latitude of the point A was found, by Messrs. Mason and Dixon 38° 27′ 34″; therefore that of Lewestown is 38° 47′ 27″ north; and the difference of its meridian, and that of the point A, or their difference of longitude, is 34′ = $2^m\ 16^s$ of time, Lewestown being to the east. But if the difference of longitude of Lewestown east of the stones on Fenwick's Isle be supposed truly given, in the former account, 5′ 45″ of a degree, then the difference of longitude of Lewestown and the point A will come out about 1′ of a degree, or 4^s of time less; for Mr. Dixon says, that the distance of the stone on Fenwick's Isle, east of the point A, is 35 English miles wanting 100 yards. Now this is equal to 30′ 26″ of a great circle = 38′ 51″ of longitude; from which subtracting 5′ 45″, there remain 33′ 6″ for difference of longitude of Lewestown and point A = 2^m $12\frac{1}{2}^s$ of time, or $3\frac{1}{2}^s$ less than found before; and this latter I take to be nearest the truth. If this be so, Lewestown is very nearly under the same meridian with the southernmost part of the city of Philadelphia, or more accurately 13″ of longitude, answering to 1^s of time, east of it. For, by Messrs. Mason's and Dixon's measure of a degree, the point N, in their survey, is 2′ 19″ of longitude west of the point A; and N, by measurement, is 31 English miles due west of the southernmost part of the city of Philadelphia, answering to 35′ 12″ of longitude; from which subtracting 2′ 19″, there remain 32′ 53″, answering to 2^m $11\frac{1}{2}^s$ of time, for the difference of longitude of the southernmost part of Philadelphia, east of the point A. But Lewestown is found above to be 33′ 6″ of longitude = 2^m $12\frac{1}{2}^s$ east of the point A, and consequently is 13″ of longitude, or about 1^s of time east of the southernmost part of the city of Philadelphia. *Nevil Maskelyne.*

LX. Observations of the Transit of Venus over the Sun, made at the Round Tower in Windsor Castle, June 3, 1769. By Daniel Harris, Master of the Royal Mathematical School in Christ's Hospital, and F. R. S. p. 422.

The latitude of St. Paul's, or, which is the same thing, of the Royal Mathematical School in Christ's Hospital, by the mean of a great number of observations, Mr. H. makes to be 51° 30¾′ N. and by a mean of several double altitudes of the sun, taken in a saucer of treacle and water, screened from the wind, he finds the latitude of Windsor Castle to be 51° 28¼′ N. the difference of latitude therefore between those two places is $2\frac{1}{2}$ geographical miles; with which, and the bearing of St. Paul's from the castle N. 82° 30′ E. variation $20\frac{1}{2}$ degrees allowed for, he makes the difference of longitude between them (by Mercator) to be $30\frac{1}{2}$ miles, which is equal to 2^m 2^s of time. Therefore, with the distance of 22 miles, between Windsor and St. Paul's, equal to 19 geographical ones, and the difference of latitude, by observation, between the two places $2\frac{1}{2}$ miles, Mr. H. finds the departure to be 18.8 miles, which gives 30.2 miles of longitude, equal to 2^m 1^s of time, agreeing within a second to the former method.

The difference of longitude, or difference of meridians, therefore, between the round tower Windsor castle, and St. Paul's, London, Mr. H. ventures to put at $30\frac{1}{2}$ miles, or 2^m 2^s of time, though he is persuaded, if any thing, it is rather more than less; to which if we add the difference of longitude in time between St. Paul's and Greenwich, which is $22\frac{1}{2}^s$, it will give $2^m\ 24\frac{1}{4}^s$ of time, for the difference of longitude between the round tower at Windsor castle, and the Royal Observatory at Greenwich.

Mr. H. observes, that the only inconvenient circumstance, during the time of observing the transit, was the wind; which, blowing rather hard, and directly into the telescope, together with the smallness of the sun's altitude at that time, made the limb so very ill defined and undulating, that it is possible there may be an error of 5 or 6 seconds, at least, in the time of the external contact; being anxious therefore of having the internal contact as exact as possible, he changed the magnifying power of the telescope from that of 125 times to that of 55 times, the least of all, which succeeded beyond expectation; for by this means that undulating motion of the sun's limb was greatly reduced, though not entirely taken away, appearing much better defined than before, as did likewise that of the planet Venus; insomuch that the error, if any, in the time of the internal contact, by which is meant the completion of the thread of light formed by the sun's circumference, cannot exceed 3 seconds. The observing of the 2 contacts with so different magnifying powers as that of 125 and that of 55, must occasion some difference in the times, and duration between the 2 contacts, from what they would have been, had they both been observed with the same magnifying power; which is to be allowed for.

	By the clock.			Mean time.		
External contact of Venus with the sun	7^h	4^m	30^s ..	7^h	6^m	14^s p.m.
The internal contact at.	7	22	38 ..	7	24	22.
Duration between the contacts, the clock being $1^m\ 44^s$ too slow for mean time				0	18	8
Venus's diameter measured 3 different times					0	$59\frac{1}{2}$.

LXI. An Attempt to Elucidate Two Samnite Coins, never before fully explained. By the Rev. John Swinton, B.D., F. R. S. p. 432.

The first of the coins Mr. S. proposes to consider here, is a Samnite denarius of Papius Mutilus, published by Sig. Olivieri and M. Pellerin, with the word SAFINIM on the reverse, in Samnite-Etruscan characters; an interpretation of which has, as he apprehended, been ineffectually attempted by the Marquis Scipio Maffei, Sig. Olivieri, Sig. Avvocato Passeri, and M. Pellerin. The other has the initial letter of the name of a town on the reverse, indicating the place where it was struck. These pieces are of a species different from that of the coins of Papius Mutilus, Tiberius Veturius, and Ni. Luponius, of which Mr. S. has largely treated in some of his former papers. The first species appertains to cities, the other to the Samnite, or Italian, commanders, whose names they bear, for the most part, on the reverse. Hence it seems, at first sight, extremely probable, that the word SAFINIM, on the reverse of Papius Mutilus's medal, cannot be equivalent to SABINI, THE SABINES, or SAMNITES, THE SAMNITES, as some of the most celebrated Etruscan antiquaries of the present age have not scrupled to assert; but must be taken, agreeably to the nature and genius of such coins, for the name of one of the Italian generals, who distinguished himself in the social war. This seems extremely probable, not only from the nature and genius of the coin itself, but likewise from the similarity and analogy it bears to other coins, with the names of Italian captains most evidently on them, attended by the same symbol that occurs on the medal which is the object of attention here. But the truth of what is here advanced will, as Mr. S. apprehends, even to demonstration, appear, if we consider, with proper attention, the legend on the reverse of a Roman denarius of the Servilian family, in conjunction with that on the reverse of a Samnite coin of Papius Mutilus. The first two of these medals are so perfectly similar, that were not the characters with which they are adorned different, and the caps or helmets worn by two Castors visible only on one of them, they might absolutely be considered as duplicates of the same coin. Now the Roman denarius has preserved the legend SERVEILIM on the reverse, and the Samnite one here elucidated the inscription SAFINIM likewise on the reverse, in Samnite, or Samnite-Etruscan letters. As therefore SERVEILIM is apparently equivalent to SERVEILI M., SERVEILIVS MARCI, or, in the Roman style, SERVEILIVS MARCI FILIVS; the legend SAFINIM may be considered as equivalent to SAFINI M., or SAFINIVS MARCI, which, in the Samnite mode of expression, answers to the Latin, or Roman, SAFINI M. F. that is SAFINIVS MARCI FILIVS. Hence it undoubtedly pointed at one of the Italian heroes, famous for his conduct and bravery in the war carried on, towards the decline of the seventh century of Rome, by the confederated Italian states, against the Romans. With regard to the Safinian family, Mr. S. observes, that it was a family of pretty considerable note. We are told by Sig. Olivieri, that C. Safinius had a hand in the seditions of L. Apuleius, which so much disturbed the repose of the republic, about the middle of the 7th century of Rome. The names of several members of this family occur in some of the Latin, or Roman, inscriptions, published by Gruter and Muratori, to omit what has been said on the same subject by other writers.

2. In M. Pellerin's third Supplement, published in the year 1767, there is a denarius attributed by that gentleman to the city of Corfinium, the capital of the Peligni, where the deputies assembled, to regulate the operations of the war entered on against the Romans, by the confederated Italian states, towards the decline of the 7th century of Rome. This notion he founds on the appearance of the letter C on the reverse; which he takes, with great reason, to be the initial letter of the word Corfinium, the name of that town. The coin of

which Mr. S. now sends a short account, agrees with that denarius in every particular, but the letter on the reverse; which is E, not C. But this is so far from overturning M. Pellerin's notion, that it will, he thinks, strongly support, if not entirely confirm it. For that these coins, and others similar to them, first appeared about the time of the social war, must be allowed extremely probable, from the symbols on the reverse which most of them exhibit. The letter E, on the reverse of this denarius, has a Samnite, Samnite-Etruscan, or oriental, direction, from the right hand to the left; which will, notwithstanding the Roman letters in the exergue, sufficiently announce it a Samnite, or Samnite-Etruscan, coin. This also will, in some measure, be evinced by the character itself; which more resembles the ancient Etruscan form of E, than the later Roman, or Latin figure of that element. Now the old name of the city, to which both the medals here mentioned may be assigned, was Corfinium, and the new one, given it by the confederated Italian states, Italica, as we learn from Strabo. As the Samnites therefore and old Romans are known to have used E sometimes for I, the element E, on the reverse of this medal, may very well be supposed to have been the initial letter of the word Etalica, for Italica, the new name mentioned by Strabo. That the Samnites sometimes used E for I, we may infer from the word EMBRATVR, for IMPERATOR, on some of Papius Mutilus's Samnite coins, to omit others that might, with equal facility, be produced; and that the ancient Romans likewise did, not unfrequently, the same thing, is indisputably clear. Whence we may, as Mr. S. apprehends, fairly collect, that M. Pellerin's denarius was struck about the time the league was formed, or concluded, in commemoration of it; and this after the commencement of the war, which was the immediate consequence of that league.

LXI. Observation of the Transit of Venus, June 3, 1769. By John Leeds, Esq., Surveyor General of the Province of Maryland. p. 444.

Having no other instruments to observe with but a pocket watch and a reflecting telescope about 20 inches long, of Sterrup's make, on the 3d instant, (June) when the sun was on the meridian, Mr. L. set his watch to 12, and at half an hour past, began to observe, keeping his eye to that part of the sun's limb a little north of the vertex, where he expected Venus to come on. At $2^h\ 10^m\frac{1}{4}$ he perceived a small dent in the sun's limb; at $2^h\ 25^m\frac{3}{4}$ Venus was totally within, so that the upper edge of the sun and Venus seemed to touch. Mr. L.'s situation was lat. 38° 45′, under a meridian, as near as he can guess, 10 miles east of Annapolis, their chief town or city; and about 12 miles west of Cape Henry, at the mouth of Chesapeak Bay, as laid down by Fry and Jefferson in their map of Virginia and Maryland.

LXII. Experiments to prove that the Luminousness of the Sea arises from the Putrefaction of its Animal Substances. By John Canton, M. A., F. R. S. p. 446.

Exper. 1. Into a gallon of sea-water, in a pan about 14 inches in diameter, Mr. C. put a small fresh whiting, June 14, 1768, in the evening; and took notice that neither the whiting, nor the water when agitated, gave any light. A Fahrenheit's thermometer in the cellar, where the pan was placed, stood at 54 degrees. The 15th, at night, that part of the fish which was even with the surface of the water was luminous, but the water itself was dark. Mr. C. drew the end of a stick through the water, from one side of the pan to the other, and the water appeared luminous behind the stick all the way, but gave light only where it was disturbed. When all the water was stirred, the whole became luminous, and appeared like milk; giving a considerable degree of light to the sides of the pan that contained it; and continued to do so for some time after it was at rest. The water was most luminous when the fish had been in it about 28 hours, but would not give any light by being stirred, after it had been in it 3 days.

Exper. 2. Mr. C. put a gallon of fresh water into one pan, and a gallon of sea-water into another, and also into each pan a fresh herring of about 3 ounces. The next night the whole surface of the sea-water was luminous without being stirred, but much more so when put in motion; and the upper part of the herring, which lay considerably below the surface of the water was very bright. The fresh water was quite dark, as was also the fish that was in it. There were several very bright luminous spots on different parts of the surface of the sea-water; and the whole, when viewed by the light of a candle, seemed covered with a greasy scum. The 3d night, the light of the sea-water while at rest was very little, if at all, less than before; and when stirred, its light was so great, as to discover the time by a watch; and the fish in it appeared as a dark substance. After this, its light was evidently decreasing, but was not quite gone before the 7th night. The fresh water, and fish in it, were perfectly dark during the whole time. The thermometer was generally above 60.

Exper. 3. Into a gallon of fresh water Mr. C. put common or sea-salt, till he found by an hydrometer it was of the same specific gravity with the sea-water. In another gallon of fresh water he dissolved 2lbs. of salt: and into each of these waters he put a small fresh herring. The next evening the whole surface of the artificial sea-water was luminous without being stirred, but gave much more light when it was disturbed. It appeared exactly like the real sea-water in the preceding experiment, and its light lasted about the same time, and went off in the same manner.* The other water, which was almost as salt as it could be made, never gave any light. The herring, which was taken out of it the 7th night, and washed from its salt, was found firm and sweet; but the other herring was very soft and putrid; much more so than that which had been kept as long in the fresh water of the last experiment. If a herring, in warm weather, be put into 10 gallons of artificial sea-water, instead of one, the water will still become luminous, but its light will not be so strong.*

The artificial sea-water may be made without the use of an hydrometer, by the proportion of 4 oz. avoirdupois of salt, to 7 pints of water, wine-measure.

From the 2d and 3d experiments it is evident, that the quantity of salt contained in sea-water hastens putrefaction; as the fish that had been kept in water of that degree of saltness was found to be much more putrid than that which had been kept the same time in fresh water. This unexpected property of sea-salt was discovered by Sir John Pringle, in the year 1750, and published in the 46th vol. of the Phil. Trans., with many curious and useful experiments on substances resisting putrefaction; but the greatest quantity of salt there mentioned, is less than what is found in sea-water: it is probable therefore, that if the sea were less salt, it would be more luminous. And here it may be worth remarking, that though the greatest summer heat is well known to promote putrefaction, yet 20 degrees more than that of the human blood seem to hinder it: for, putting a very small piece of a luminous fish into a thin glass ball, the water of the heat of 118 degrees destroyed its luminousness in less than half a minute; which, on taking it out of the water, it would begin to recover in about 10 seconds, but was never after so bright as before.

Mr. C. then adds to these experiments the two most circumstantial accounts he could find of the sea's luminous appearance. Mr. Boyle, in the 3d volume and 91st page, of Birch's edition of his works, says, "When I remember how many questions I have asked navigators about the luminousness of the sea; and how in some places the sea is wont to shine in the night as far as the eye can reach; at other times and places, only when the waves dash against the vessel, or the oars strike and cleave the water; how some seas shine often, and others have not been observed to shine; how in some places the sea has been taken notice of, to shine when such and such winds blow, whereas in other seas the observation holds not; and in the same tract of sea, within a narrow compass, one part of the water will be luminous, whilst the other shines not at all: when, I say, I remember how many of these odd phenomena, belonging to those great masses of liquor, I have been told of by very credible eye-witnesses, I am tempted to suspect that some cosmical law or custom of the terrestrial globe, or, at least,

* Several river-fish, as the bleak, the dace, the carp, the tench, and the eel, were kept in artificial sea-water to putrefy, without producing any light that could be perceived, but a piece of a carp made the water very luminous, though the outside, or scaly part of it, did not shine at all.—Orig.

of the planetary vortex, may have a considerable agency in the production of these effects." The other is a paper in which Father Bourzes has given a still more particular account of the luminous appearance of the sea; to be found in vol. 6. p. 53, of these Abridgments.

LXIV. A Series of Astronomical Observations made at the Observatory of the Marine at Paris. By. M. Messier, F. R. S. p. 454.

These observations are 1° of Jupiter's satellites in 1767 and 1768. 2° on the shadows of Jupiter's satellites. 3° on the variation of the belts on the disc of that planet. 4° of a spot on the disc of the 3d satellite. 5° of the belts of saturn. 6° of the moon's passage over the Pleiades, in 1767. 7° of a partial eclipse of the moon, Jan. 3, and of a total one, December 23, 1768. 8° of two Auroræ Boreales, Aug. 6, and Dec. 5, of the same year. But they are all of little or no use now.

LXV. Astronomical Observations made by Order of the R. S. at Prince of Wales's Fort, on the North-West Coast of Hudson's Bay. By William Wales and Joseph Dymond.* p. 467.

These consist of a pretty long series of observations, preparatory to that of the transit of Venus, from Sept. 14, 1768, till Aug. 25, 1769. These are, 1st, of

* Mr. Wm. Wales, F. R. S., rose from a private situation little connected with learning, by his natural talents aided by close application, to some of the first ranks in literary pursuits. In the different stages of his life we notice his attempts and his success in various subjects, poetry, mathematics, astronomy, controversy, political arithmetic, &c. So early as 1762 we find he published an Ode to the Right Hon. Wm. Pitt, the first Earl Chatham. Soon after we observe his labours in the correspondence of the Ladies' Diary, that useful little annual work, which has formed most of our eminent mathematicians. Here, and in some other periodical publications, for many years is observed the gradual improvement of Mr. Wales in the various mathematical sciences, till his arrival at a very high pitch of eminence, both in science and composition. By this his first paper in the Phil. Trans. we find he was deemed a proper person to be sent, in 1768, to a distant station, to observe the expected transit of Venus in 1769; and the manner in which he discharged that trust did honour to his talents, and credit to his patrons. Mr. W. next accompanied Capt. Cook in his first voyage, 1772—1774, as one of the astronomers, and again in his other voyage of 1776—1779; at other times, his ordinary employment was giving private mathematical lessons in London. In 1770 Mr. W. published his Observations at Hudson's Bay; in 1777 his Observations on a Voyage with Capt. Cook; and in 1778 Remarks on Dr. Forster's Account of the Voyage, in which he showed considerable talents as a controversial writer. Soon after his return from the last voyage, Mr. W. was elected a F. R. S., to which he proved a very useful member; and on the death of Mr. Daniel Harris he was appointed mathematical master to Christ's Hospital, London; and, some years after, secretary to the board of longitude; both which offices he held till his death, which happened in 1798, at about 64 years of age. In 1781, Mr. W. published an Inquiry into the State of the Population in England and Wales: and in 1794 his Treatise on the Longitude of Time-keepers. It has also been said that he was assisting in the composition of Lord Mulgrave's account of his Voyage towards the North Pole, published in 1773; and that he was author of one of the dissertations on the achro-

equal altitudes and meridian passage of the sun, to ascertain and regulate the going of the clock; 2d, zenith distances, for the latitude; 3d, the height of the barometer (50 feet above low-water mark), also of the thermometer both within doors and without. Hence it appears that the greatest height of the thermometer was on the 3d of July, that within doors being at 70, and that without at 79; and the lowest state of the same was on Dec. 11, the thermometer within being — 31, or 31 below zero, and that without — 42.

The 2d is another series of observations, of zenith distances with 3 different sectors, and of the barometer and thermometers, also of the sun and different stars being on the meridian. The 3d is a series of occultations of fixed stars by the moon. The 4th is the series of observations on the transit of Venus itself, being the grand object of the whole undertaking. These show the times of all the 4 contacts, with the distances of the limbs of the sun and Venus, and the diameters of both, observed at a great number of different times. The chief results are, that the exterior contact at the ingress was observed at $0^h\ 57^m\ 0.6^s$ ap. time, by J. D. (Jos. Dymond), but at $0^h\ 57^m\ 7.6^s$ by W. W. (Wm. Wales); the interior ditto at $1^h\ 15^m\ 21.3^s$ by W. W., but at $1^h\ 15^m\ 25.3^s$ by J. D. Also at the egress, the internal contact was at $7^h\ 0^m\ 45^s\frac{1}{4}$ by W. W., but $7^h\ 0^m\ 48^s\frac{1}{4}$ by J. D.; and external contact at $7^h\ 19^m\ 1^s\frac{1}{4}$ by W. W., but at $7^h\ 19^m\ 20^s\frac{1}{4}$ by J. D.

Remarks.—The heavens at the beginning, and for a considerable time both before and after, were frequently obscured by clouds; but in the intervals, the air was very clear, and the sun's limbs extremely well defined. Soon after Venus was half immerged, a bright crescent, or rim of light, encompassed all that part of her circumference which was off the sun; thereby rendering her whole periphery visible. This continued very bright until within a few minutes of the internal contact, and then vanished away gradually. They took, for the instant of the first internal contact, the time when the least visible thread of light appeared behind the subsequent limb of Venus: but before that time, Venus's limb seemed within that of the sun, and his limb appeared behind her's in two very obtuse points, seeming as if they would run together in a broad stream, like 2 drops of oil; but which, nevertheless, did not happen, but joined in a very fine thread, at some distance from the exterior limb of Venus. This appearance was much more considerable at the egress than at the ingress; owing, as apprehended, to the bad state of the air at that time. They took for the instant of internal contact, at the egress, the time when the thread of light disappeared

nical rising of the Pleiades, annexed to Dr. Vincent's Voyage of Nearchus, 1797. Besides these, Mr. W. wrote some ingenious papers in the Phil. Trans., and in different periodical publications, particularly the Ladies' Diaries, sometimes under the signature of his own name, and sometimes under various fictitious signatures, as G. Cetii, Felix, M'Carthy, &c.

before the preceding limb of the planet, from which time W. W. took notice that he had told about 24^s when the limbs of the sun and Venus were apparently in contact: a circumstance which he did not venture to attend to at the ingress. They saw nothing like the appearance of an atmosphere round Venus (unless the above-mentioned phenomena may be thought to proceed from thence) either at the beginning, end, or during the time of the transit: nor could they see any thing of a satellite; though they looked for it several times. It may not be improper to add, that the haziness, complained of at the egress, was not owing to any accidental bad quality of the air at that time; it is continually so here to 10° or 12° above the horizon, and often even to 16° or 18°, in what may be called the clearest state of the heavens.

Observations for determining the Magnetic Variations at Prince of Wales's Fort, on the N. W. Coast of Hudson's Bay, by W. W.

The variation compass, which Mr. W. received from Mr. Robertson, by order of the Royal Society, was, when he received it, a very good one, as appeared by several trials which he made of it in London, before it was put on board the ship; but when they arrived in Hudson's Bay, and were ready to make use of it, they had the mortification to find that the needle had, by some cause or other, entirely lost its magnetic virtue. As the cold was, by the time that they made this discovery, much more intense than it probably was at the time that Mr. Ellis complains of a similar circumstance happening to him in those parts, Mr. W. was naturally led to try whether he could not benefit by his experience, and accordingly removed the compass into the room where they lived; which was kept very warm by a large fire, and by the house stove; and there it remained ever after, but without the least effect. In order to remedy this misfortune as much as lay in his power, he applied to Capt. Richards, as soon as he arrived in the river that year; and desired he would send him his azimuth compass on shore, with which request he very kindly complied the next day; but the cloudy weather prevented him from making any observations before the 22d of August. The compass was of the common form, and he judged it would be best to make the observations about noon, when the sun's azimuths change the fastest, and to note the times by the clock. By a medium of a great many observations, the variation was 9° 41′½ west, in the month of August 1769.

The latitude of Prince of Wales's Fort on the N. W. coast of Hudson's Bay, deduced from a medium of a great multitude of observations of the sun and stars, was 58° 47′ 32″.

The instruments used in making the preceding observations were: 1. A clock, made by Mr. Ellicot, with an apparatus for correcting the effects of heat and cold; the same which Messrs Mason and Dixon had to the Cape of Good Hope in the year 1761. 2. An astronomical quadrant, made by Mr. Bird, of one foot radius.

3. Two reflecting telescopes, of 2 feet focus, made by Mr. Short; and a divided object-glass micrometer, made by the same gentleman, of 501.45 inches focal length.

They used the micrometer with a magnifying power of 60; the contacts of Venus with the sun's limb were observed with a magnifying power of 120, and all the other observations with one of 90. Both the thermometers, used in the preceding observations, were according to Fahrenheit's scale.

LXVI. Extract from the Journals of the Royal Society, June 23, 1768, *respecting a Letter addressed to the Society by a Member of the House of Jesuits at Pekin in China. By Charles Morton, M.D., Sec. R.S.* p. 489.

This letter, the original of which is in the French language, consists of 28 pages in close folio; to which are subjoined 44 pages of notes; and 27 pages of drawings, to which the letter and notes refer for illustration. It relates to some disquisitions of Mr. Tuberville Needham, F.R.S., concerning a supposed connection between the hieroglyphical writing of ancient Egypt, and the characteristic writing now in use among the Chinese.

Several of the Society remember Mr. Needham's tract on this subject, which was printed at Rome in the Latin tongue, 1761, addressed to this and the Antiquarian Societies. This conjecture of Mr. Needham's, pregnant with so many curious consequences, engaged the attention of the literati of Europe: most of them wishing success to it; and some, either from a particular information, or for other reasons, opposing it. Mr. Desguignes of Paris, F.R.S., Mr. Bartoli of Turin, antiquary to the King of Sardinia; the late Abbé Winkelman, antiquary to the pope, and Mr. Montagu, F.R.S., were the principal of those who thought themselves concerned to oppose Mr. Needham; and what they have been pleased to communicate, either in print or manuscript, has been already laid before the Society; and the last gentleman, viz. Mr. Montagu, has also sent to England a cast of the bust of Turin, now in the British Museum, inscribed with certain characters, which gave occasion to Mr. Needham's conjectures.

The subject in question seemed sufficiently interesting to seek an answer from the only competent judges, the literati of China; and your secretary, first by the encouragement of Thomas Hollis, Esq., F.R.S., and subsequently by the assistance of Thomas Wilcocks, Esq., F.S.A., and the particular favour of the Directors of the East-India Company, has at length obtained it. In order to this, a letter was written, in conjunction with Mr. Alban Butler, late of Pall Mall, who had some interest among the Jesuits at Pekin, stating the matter in question, and desiring the favour of an answer; which answer is the letter that has been read to the Society.

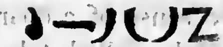
The particulars which were stated to the Jesuits at Pekin, and have been recited to the Society, were as follow; viz. 1. Whether certain characters, to the number of 29, copied from the bust at Turin, together with several other characters, to the number of 200, copied from undoubted monuments of Egypt, are really and indeed Chinese characters; and if they be, of what dialect, and of what age are they? 2. What sense does each of these characters express; and what is the particular interpretation? 3. Does the history of China, or popular tradition, or any analogy with the modern or ancient method of writing of any other nation, afford ground for supposing that these characters have been received from foreigners: or were they invented by the Chinese themselves? 4. Are there any monuments or customs among the Chinese, which resemble those of the ancient Egyptians; or which should induce us to think, that there has ever been any communication between the two nations?

The answer received from China takes notice only of the small number of characters which were copied from the bust of Turin; occasioned probably by some accident or failure in the packets, of which there were 3 copies sent, and one of them containing the Turin characters only; the answer is dated from Pekin, October 20, 1764, addressed to the members of this Society, but with no subscription, or signature, excepting 4 stars, and this addition 'of the company of Jesus.'

The author's method, or order, is as follows: 1. An introductory preface. 2. A state of the inquiry, as collected partly from the letter, and partly from Mr. Needham's printed book. 3. What the author calls an historical picture of the Chinese tongue and its characters. 4. An application of this historical delineation, in the way of principles, to decide concerning the 29 characters of the bust of Turin. 5. A more general application of the said principles, in order to elucidate the hieroglyphical writing, and consequently the antiquities of Egypt, by a proposed collation with the ancient symbolical writing of China, exemplified by various instances: and lastly the notes, containing circumstantial details of some particulars, as well historical as critical, which might otherwise have broken the thread of the letter.

The particular branch discussed in this letter, as well as the general learning of China, are subjects in a manner new to Europe; and the various books of the Chinese are called by the author a Potosi, which might enrich Europe; especially with regard to laws, government, the useful arts, natural history, and the like. Some strictures from the letter are to this effect: 1. In the preface, mention is made of the insufficient attempts of the Greeks and Romans to explain the hieroglyphical writing of Egypt; and of the later attempts of father Kircher, and Mr. de Mairan, who anticipated Mr. Needham in the idea of explaining them by the characteristic writing of China; which idea they quitted almost as soon as they had formed it. 2. The state of the question is expressed as follows: 'Mr. Needham has observed, that the symbols or hieroglyphical characters of the Isis of Turin, appear like several Chinese characters, such as they are found in the great dictionary Tching, tsee, tong: on which he conjectures, 1st, that the Chinese characters are the same in many respects as the hieroglyphics of Egypt; and 2dly, that one may be able to discover the sense of hieroglyphics by the comparative and appropriated signification of the Chinese characters.'

3. The author having mentioned the difficulty of rendering himself intelligible to the literati of Europe, for want of a certain acquaintance with proofs of fact and history, criticism and grammar, proceeds to the historical detail, being the subject he chiefly enlarged on. He notes the enthusiasm of Vossius for the antiquity of the Chinese, and the rage of Renaudot against it; and then declares his own opinion, that they have subsisted as a nation, from the time of the great emigration which followed the confusion of tongues. He dates the antiquity of Egypt from the same epoch, and gives reasons, particularly in the notes, for the probability of their different routes. He inquires into the use of writing; and declares his opinion, that it was already established in the antediluvian world; and might be derived in common to the two nations in question. He makes light of any supposed variation of it, at the confusion of tongues: and wishes that Mr. Needham had expressed his own opinion concerning the commencement of it. He affirms, that there is not the least mark or trace now remaining of any subsequent communication between the Chinese and Egyptians. But whether our author's opinion of the origin of writing, or the contrary one, of each nation having invented its own, be adopted; he candidly owns, that any connection between the two modes of writing, is hardly discernible at this day. He affirms, that the Chinese language is one of the most ancient; and perhaps the only one which has been spoken without interruption; and is yet a living language; the small number and the shortness of its words having so guarded it from changes, that they could scarcely extend further than the pronunciation.

They distinguish in the Chinese language, 1st, the Kou-ouen, the language of the book called the King, and other books written in this taste. The harangues of the Chou-king, and the songs of the Chi-king, prove that it was spoken formerly. It is exceedingly laconic. 2dly, The Ouen-tchang, the language of relevees, elevated compositions, and books. This language, excepting some proverbs, axioms, and forms of compliment, is no longer used in speaking. 3dly, The Kouan-hoa, the language of men in office. This is the only language spoken at court, and in good company, and used in books; and this alone runs through the empire. 4thly, The Hiang-tan, patois or provincial jargon; of which, each province and town, and almost every village has its own. Notwithstanding these varieties, the Chinese tongue counts but about 330 words. Hence shapes and the symbols have passed from a contour sufficiently regular, to some lines or strokes oddly assembled: and that the strokes themselves have been yet decompounded, and melted, into these 6,  out of which, at present, are composed all the characters in use. The simplest are made of 1 or 2 of these strokes; and they count as far as 20 or 30, or more of them, in the more compound characters. To avoid the confusion and obscurity which this great abbreviation would have caused, they have fixed the number of the strokes of the characters which represent the 200 elementary images and symbols spoken of. These abbreviations thus fixed are called pou, classes, or tribunals, as Mr. Fourmont translates. For example; the pou of man, of woman, of trees, of diseases, of great, of small, of vase, &c. In brief, for greater clearness, and to range the characters in the dictionaries, there is in each character a distinctive or differencing pou, which predominates, and under which the character is placed. This differencing pou is the part of the character which has most influence in its signification; saving the exceptions, and oddities, from which the Chinese is no more exempt than other tongues. A bare inspection into the dictionary Tching-tsee-tong, will render these details intelligible.

The misfortune, and a very great one, of the Chinese characters is, that these abbreviations have been made by little and little, in different places, and without rule: so that there are characters which have been abridged, or more properly truncated, and disfigured a great number of ways: and the most part, so much, as to be no longer knowable by the primitive form. To give some idea of this, the author has caused to be copied the variations of 4 characters; and one may judge by this sample, how frightfully disfigured must those characters be, which are woven out of several other characters. For the different characters which are thus united to make one only, are curved, lowered, lengthened, drawn in, or contracted, to the end that each stroke may be so placed, as that all together may make the contrast of a simple character, and occupy no more space than it does. A like constraint ought to disfigure many of the elementary characters which are joined together to make one only. But when we add to this, the abbreviations and various readings, it is clear that they can no longer be knowable by their primitive characters. And this, to observe it en passant, is one of the reasons which has rendered the edition of the king under the han so difficult, and perhaps is the principal cause of their obscurity. In effect, the primitive images and symbols being altered, how can one find the sense of them? It is no more according to the rule of the Lieou-y. The decomposition of the elementary characters of which it is composed, no longer gives its true analysis. The more one seeks the sense which ought to result from their assemblage, the farther one is from it: because that this assemblage is not the true one. It is as if we should read (in French) delires for delices. This change of the c into r subsisting, all the significations that we shall seek to delires, will never arrive at the idea presented by delices.

If the comparison be lame, it is because it represents not clearly how far a Chinese character separates from its true signification, by the alteration of some one of the strokes that compose it. The destruction of the books by fire has rendered the evil without a remedy. When peace was restored to letters, they spared neither care nor inquiry to recover the king, and other ancient books. But few copies having escaped the flames, and those not in the best preservation, they were deprived of the great advantage to be drawn from collations, to discover the primitive characters. Writing had changed; tradition was almost extinguished. It was necessary to be learned, even to decypher the manuscripts: how should they be able to pursue the discussion so far as the various readings: and unravel, among abbreviations almost unknown, the true symbols and likeness of which a character was woven. The editors were not sparing of their labour herein; but each had his system, and his conjectures. Who would venture to say that the edition which has prevailed, has not many mistaken characters? and let it be even the best, yet learned men, who have since laboured in the analysis of the characters, are not agreed among themselves; and they each bring reasons capable of suspending the judgment of critics. This variety of opinion has caused much variety in the orthography, if we may so call the manner of writing a character with such or such a pou. The manner accordingly has been floating and uncertain, for many characters, till the great dictionary Kang-hitse-tien, which has fixed it.

The author winds up this curious detail with the following remark, which he says is essential. All that has been said of the various readings and abbreviations of the characters is independent of the 5 sorts of writing usually counted by lettered men. The 1st is called Kou-ouen: this is the most ancient form of writing; and there remains now hardly any more traces of it. The 2d, Tchoang-tsee, also read Tchouen-tsee, has succeeded the Kou-ouen; and has lasted even to the end of the dynasty of the Tcheou. It was this which was in use from the time of Confucius, and of which the abbreviations and various readings have been most fatal. The 3d, Li-tsee, began under the reign of Chi-hoang-ti, the founder of the dynasty of the Tsin, and the great enemy of letters and of lettered men. The 4th, Hing-chou, is destined for impression, as with us the Roman and Italic. The 5th sort, Tsao-tsee, was invented under the Han, and would have destroyed every thing, if it had prevailed. It is a sort of writing with the stroke of a pencil, with a very light and well-experienced hand: but it disfigures the characters beyond expression. It has no course, but for the prescriptions of physicians, prefaces of books, inscriptions of fancy, &c.

To return to the various readings, and abbreviations: though it be true that these different sorts of writing have augmented the number of them; yet the last 3 have done no great harm; because they have been directed by learned men, consecrated by public authority, and bear more on the general form of the characters, than on their orthography. Thus the literati do not complain, further than their having caused the loss of the ancient characters, which it would have been well to consult, to have had the true analysis of several of the characters of this day, which they think ill written and disfigured.

And thus, at length, having completed his historical detail, our author decides concerning Mr. Needham, viz. that the characters of the bust of Turin, (though 4 or 5 of them, viz. N° 2, 3, 8, 9, 31, have a sensible resemblance to the like number of characters in the Chinese dictionary), are not genuine Chinese characters; having no connected sense, nor a proper resemblance to any of the different forms of writing; indeed the whole inscription has nothing of Chinese in the face of it. As a further proof, our author took the opinion of several of the Chinese literati, whose province it is to study the ancient writings; who all declared the same thing; and that they did not understand them, nor had ever seen the like of them. It is owned however, that, according to the Chinese interpretation of the 5 resembling characters, they are simple ideas, or symbols, not characterized by the further circumstantiating strokes; and are, without coherence, in the way of Nomenclator.

But finally, to enable the society to judge for themselves; our learned correspondent has sent a collection of very ancient inscriptions, above 100 in number, which may be compared with the inscription of Turin; as also, some drawings of vases, and other antiquities. The particular matter of inquiry, viz. the characters of the bust of Turin being thus disposed of, our author, who is against renouncing Mr. Needham's general conjecture, without further examination, as it may notwithstanding conduct to many discoveries, applies himself, 5thly, to a further and more general investigation, by an actual collation of such Egyptian hieroglyphics as do undoubtedly resemble ancient characters, yet remaining among the Chinese: in order to which, he has given us drawings of 73 such hieroglyphics, collected chiefly from Kircher, as he had no better materials, and has placed by them the corresponding Chinese characters, both ancient and modern. He is sufficiently diffuse and curious, in two or three examples, to point out the method and most interesting subjects of inquiry, viz. the leading notions concerning the deity, and the religion of the primitive times; and he also describes the properties of the symbolical animals, which are supposed to be significant of the rational and moral qualities; but he enters a caution against these, as being probably the invention of later times. He argues strenuously for the early and uninterrupted theism of the Chinese; and concludes with an apology for the condition of a missionary, the duties of whose profession, and separation from divers necessary means of information, render him, in his own opinion, very unfit for literary inquiries.

LXVII. Observation of the Transit of Mercury over the Sun, Oct. 25, 1743. *By John Winthrop, F. R. S., New England.* p. 505.

This observation, Mr. W. thinks, will determine the longitude of Cambridge, New England, with more exactness than any of the observations that have been used for that purpose. He adjusted his clock by correspondent altitudes of the bright star of Aries, taken the night before the transit with a quadrant of 2 feet radius; and on the day of the transit by correspondent altitudes of the sun; all of which agreed within 5^s; and allowed for the difference of the sun's declination, morning and afternoon. The morning was fair and calm, but hazy; yet he had a good view of the planet, and with a 24 feet telescope observed that at $8^h\ 17^m\ 5^s$ ☿ in his egress touched ☉'s limb. At $8^h\ 18^m\ 58^s$ it went off entirely. Mr. W. could not be so certain of the moment when the planet left the sun, as of its interior contact. For the sun's limb, undulating in the vapours of the horizon, made it somewhat difficult to judge when the indenture, formed by the planet, entirely ceased. However, he believes this latter observation may be relied on to 4 or 5^s. The comparison of this observation with those made in Europe will, he presumes, determine the difference of meridians within a few seconds of time.

*LXVIII. A Method of Working the Object Glasses of Refracting Telescopes Truly Spherical. By the late Mr. James Short, F. R. S.** p. 507.

Prepare two plates or tools of brass, the one convex, and the other concave, being both portions of a sphere of the same radius as the focal length of the object glass wanted, or rather of a radius somewhat longer than the focal length wanted, for a dioptrical reason; let these plates or tools be between 2 and 3 times the breadth of the object glass desired; or, in long focal lengths, twice the breadth will be sufficient: let these tools be of a sufficient thickness in proportion to their breadth or diameter, and let them be ground with fine emery exactly true to one another, working them alternately, the one above the other, to preserve the same focal length; or, if it is desired longer, you must work the convex above the concave; or, if desired shorter, you must grind the concave above the conxex.

After this, prepare another brass-plate or tool, of the same breadth and thickness as the two former, and of the same radius of concavity; its being truly turned on a lathe will be sufficient for this purpose; which tool is to serve after-

* This paper, which was delivered, sealed up, by Mr. Short, at the society, on the 30th of April, 1752, was, after his death, opened by the council, and ordered to be printed.—Orig.

wards for the polishing of the two surfaces of your object glass, and therefore called the polishing tool.

Prepare a piece of straw-coloured glass, of the plate glass kind, of the proper diameter for the object glass desired, which ought always to be broader than the proper aperture for that length; let this piece of glass be ground flat, in another tool, on both sides, and as nearly parallel as may be, and somewhat polished, in order to discover whether there are any veins or flaws in the glass. When you are satisfied of the goodness of the glass, you are then to prepare a handle to fasten your glass to. Great care must be taken in this, for fear of bending the glass by the handle: my method is this: I take a flat piece of brass, or rather of the concavity of the sphere, to which the glass is to be ground; this piece of brass should not be thicker than $\frac{1}{3}$ of the thickness of the glass, of a circular form, less in breadth somewhat than the glass itself, and having sides of the same form, at right angles to the flat piece of brass, and these sides ought to be of such a shape as that the fingers may easily apply to it in working, and these sides should be as low as may conveniently be, and no thicker than about $\frac{1}{3}$ of the glass. This handle is to be fastened to the glass, by warming the glass and handle gently before a fire, and laying some pitch on the glass thus warmed, till it becomes soft like melted wax; and then laying the brass handle, a little heated, on the pitch, you press it a little, till you are sure there is nothing between the glass and handle but pitch; you then lay down the glass and handle on something flat, taking care that the handle is in the middle of the glass, till it is entirely cold. It is very material to know, that the pitch, to be used for fastening the handle to the glass, must be soft pitch, that has never been used nor melted; for any other pitch will infallibly bend the glass.

You then grind your glass in the concave tool with emery, and give it the proper figure and smoothing for the last polish, in the common manner.

In order to give the glass the last polish, which is the most difficult part of the whole work, prepare some pitch for covering the before-mentioned polishing concave tool, which is done in this manner: take some pitch, and melt it in an iron ladle, and let it boil about $\frac{1}{4}$ of an hour; by this boiling, the pitch, when cold, will become hard and brittle; or you may shorten this operation, by melting equal quantities of pitch and rosin, and then there is no occasion to let it boil so long. The pitch being thus prepared, melt it, and take it off the fire, and let it stand till the pitch becomes pretty cold, or of a thickish consistence; and having warmed the polishing tool a little, to make the pitch stick to it, pour out of the ladle, on the polishing tool, as much of the pitch as you judge will cover the whole tool; when spread out, to about the thickness of $\frac{1}{8}$ of an inch; then invert this tool with the pitch on it, and press it on the convex tool, which must be quite dry, clean, and cold, in order to give it the figure of the convex tool. In case it has not spread out so as to cover the whole surface of the polishing tool, warm the pitch by holding it before the fire, and press it on the convex tool, as before, till it has entirely covered the surface of the polishing tool; then plunge it into cold water, till the brass is quite cold.

To know if the pitch is hard enough, press the edge of the nail of your thumb on it, and if it receives an impression, the pitch is not hard enough.

Then proceed to prepare this polishing tool, for the last polish of the glass, by grinding this polishing tool on the convex tool with pretty coarse emery, and a small quantity of water, in the common way that tools are ground one on another; but this must be done only for a short time, and the polishing tool must have no other pressure than its own weight, for fear of some of the emery sticking in the pitch, and you must never allow the emery to grow dry; when you have ground the pitch so as to be all over of the same colour, you then wash the pitch from all the emery with a brush and clean water; after this take a bottle of water, and holding the pitch tool in a sloping position, pour water out of the bottle so as to fall on every part of its surface.

Then place the polishing tool in a horizontal position, and put on it some putty, washed from all its gritty particles, but it need not be the finest washing, and put a good deal of water on the polishing tool, mixing the putty and it together, and polish the glass on this pitch polisher in the common manner of polishing glasses.

After having polished the glass about 10 minutes, again grind the polisher on the convex tool with emery, as before, for fear the pitch has, by working, lost any of its proper figure; and the oftener you do this, the truer will be the figure of the glass; and in this manner you proceed till the glass is quite polished.

Then take the glass off its handle, by holding it before the fire, till it is so warm that you can slide the handle off the glass; and while the glass is warm, take off as much of the pitch as you can with the sharp edge of a knife; then lay the glass down to cool, and when quite cold drop some spirits of wine on it; and this with a cloth, will wipe off the rest of the pitch.

Then examine the centre of the surfaces of the glass; and if it lies to one side of the centre of the glass, mark that place with a spot of ink, and then put on the handle as before, on the side that is now polished, with its centre over the spot of ink, and grind the glass as before, till the circular remaining part of the glass to be ground is as much distant from the centre of the glass on the other side from the spot, as the spot was from the centre of the glass; then by heat return the handle to the centre of the glass, and proceed to grind and polish this side of the glass as before.

The concave and convex tools should be ground with fine emery, after you have done one side of the glass, for the oftener these are ground together, you will be the more sure of having the figure true.

END OF THE FIFTY-NINTH VOLUME OF THE ORIGINAL.

END OF VOLUME TWELFTH.

C. and R. Baldwin, Printers,
New Bridge-Street, London.

The Vegetable Fly.

Fig. 1.

Fig. 2.

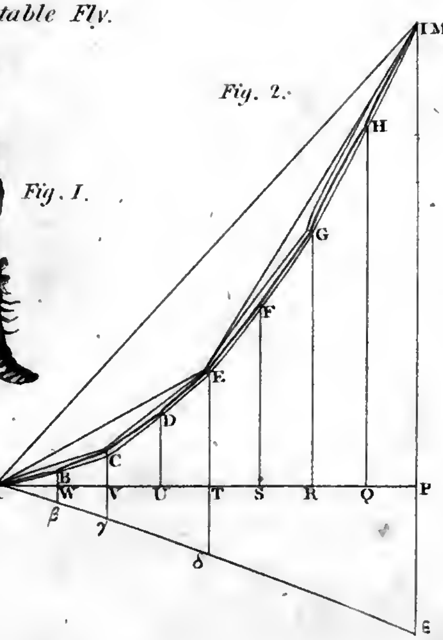


Fig. 3.

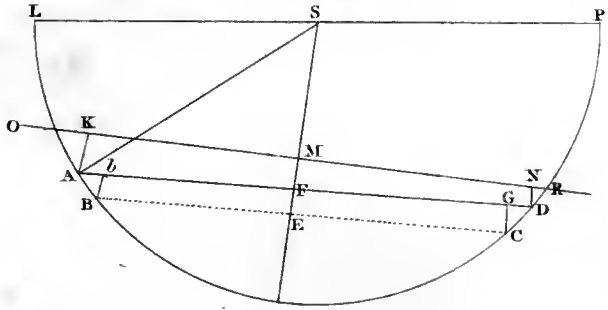


Fig. 4.

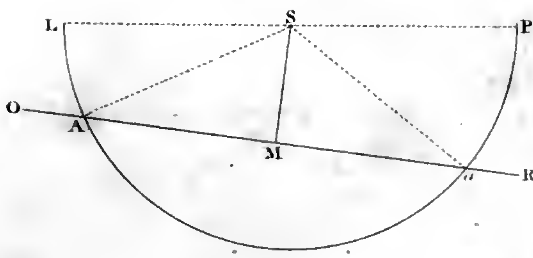
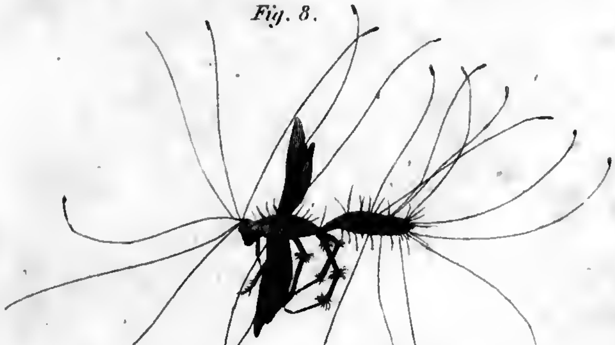


Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.



Wasp from Jamaica.

Fig. 9.

Locust from Jamaica.

Mutlow Sc. Russell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.

The concave and convex tools should be ground with fine emery, after you have done one side of the glass, for the oftener these are ground together, you will be the more sure of having the figure true.

END OF THE FIFTY-NINTH VOLUME OF THE ORIGINAL.

END OF VOLUME TWELFTH.

C. and R. Baldwin. Printers,
New Bridge-Street, London.

The Vegetable Fly.

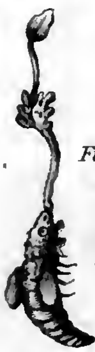


Fig. 1.

Fig. 2.

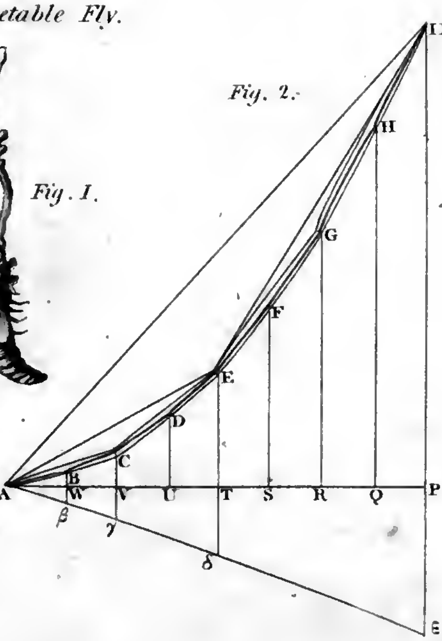


Fig. 3.

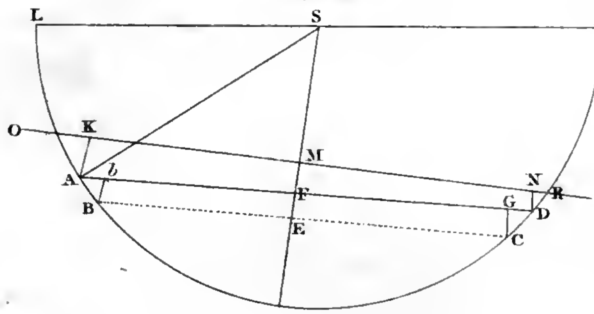


Fig. 4.

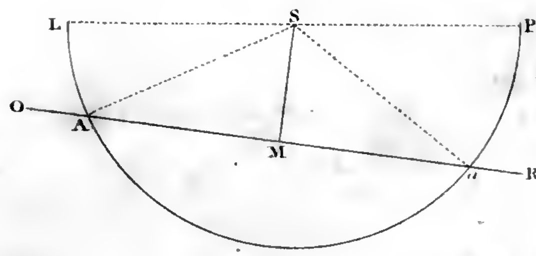
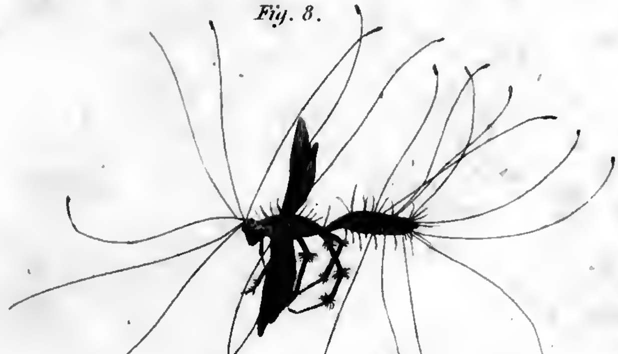


Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.



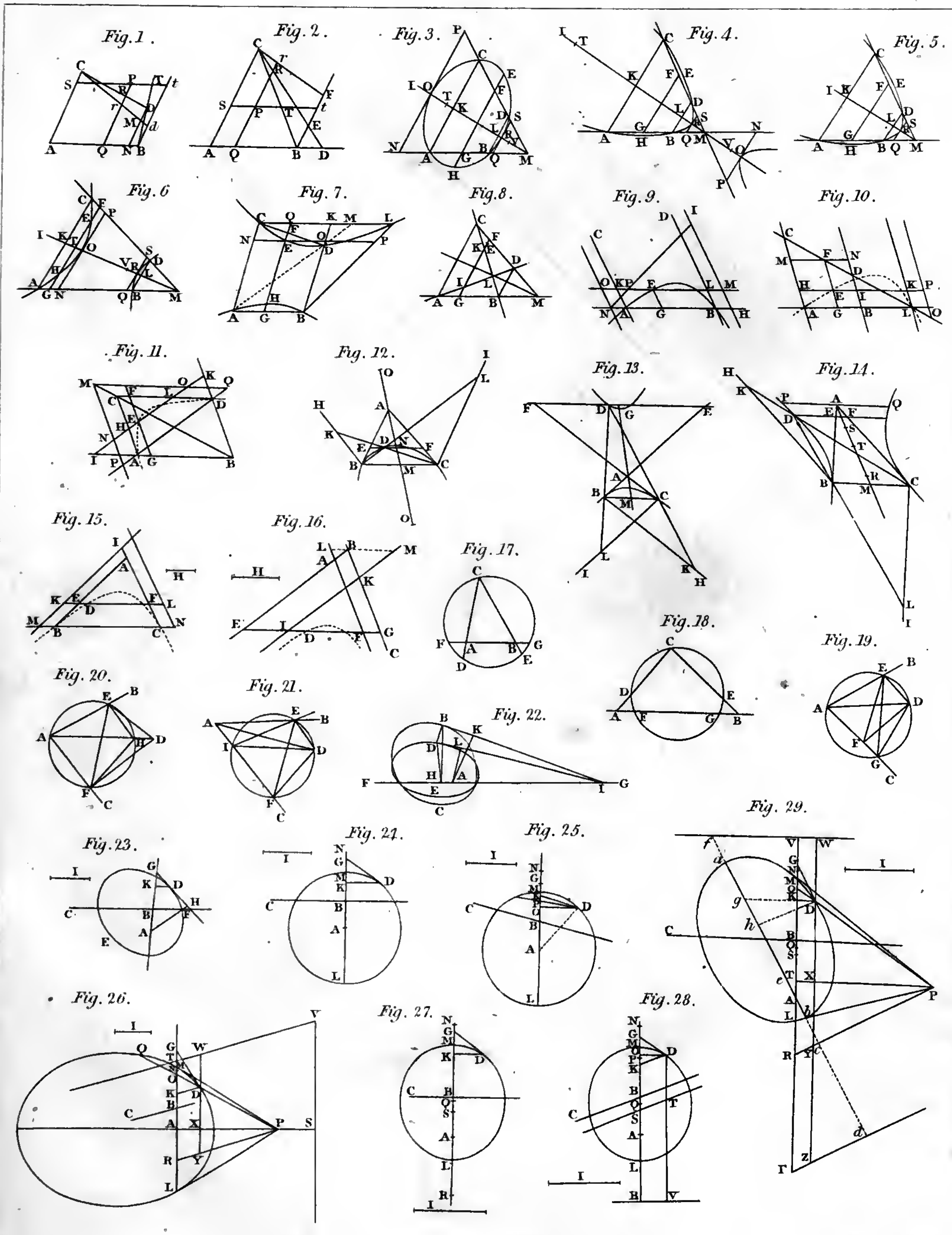
Wasp from Jamaica.

Fig. 9.

Locust from Jamaica.

Mutlow Sc. Russell Ct.

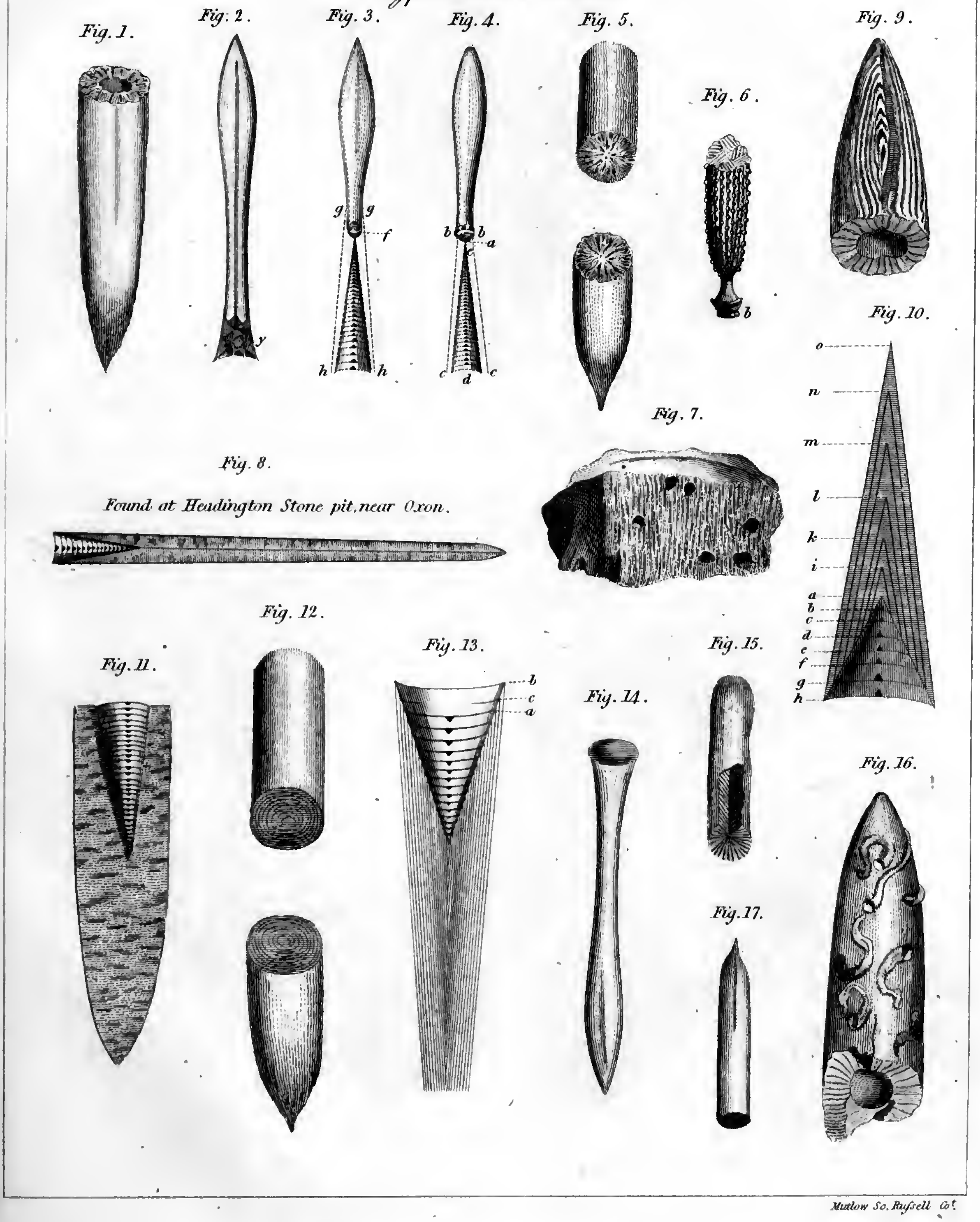
Published by C. & R. Baldwin, of New Bridge Street, London, 1807.



Mutlow Sc. Russell Ct.

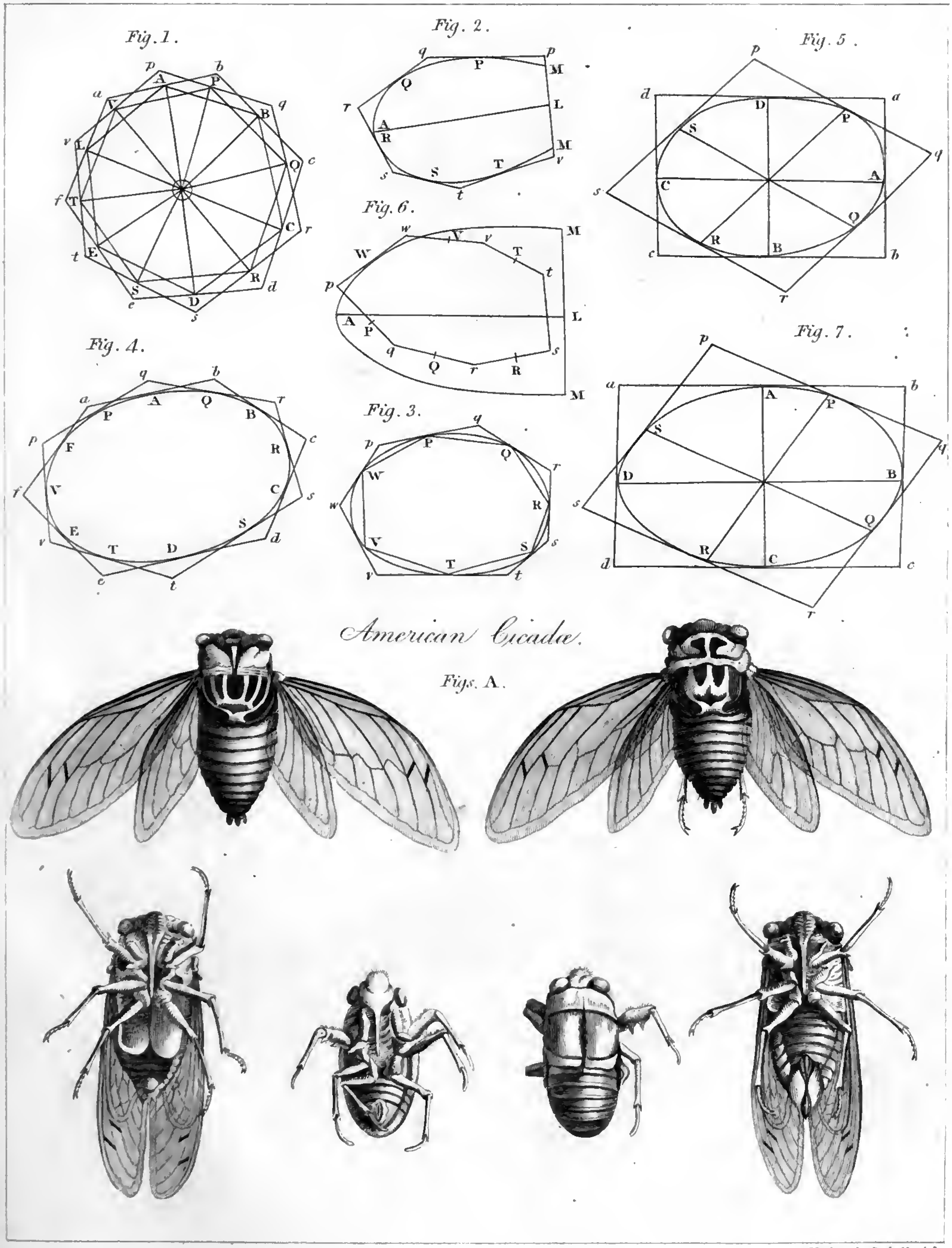
Published by C & R. Baldwin, of New Bridge Street, London, 1807.

Different Belemnites.



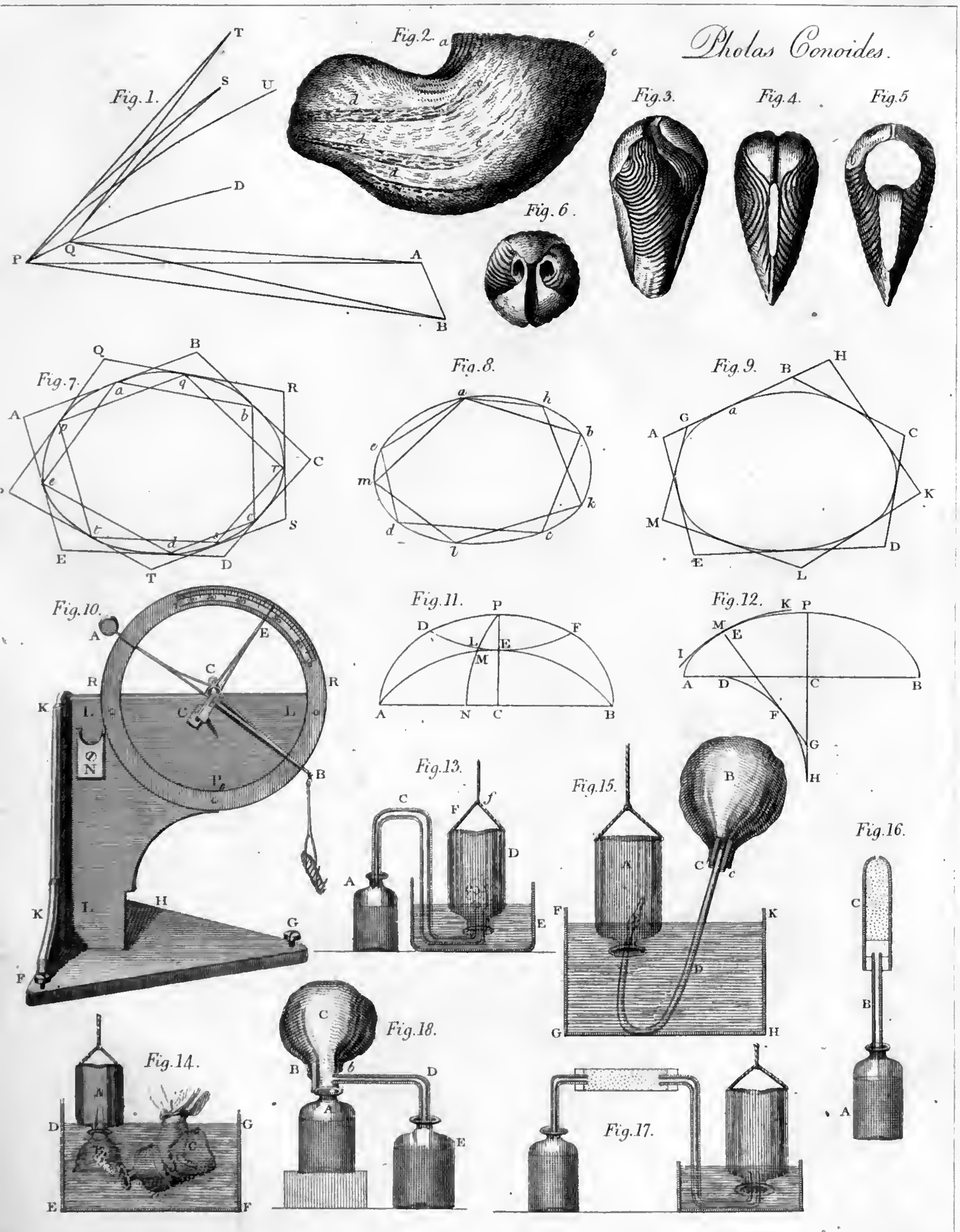
Mutlow So. Russell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.



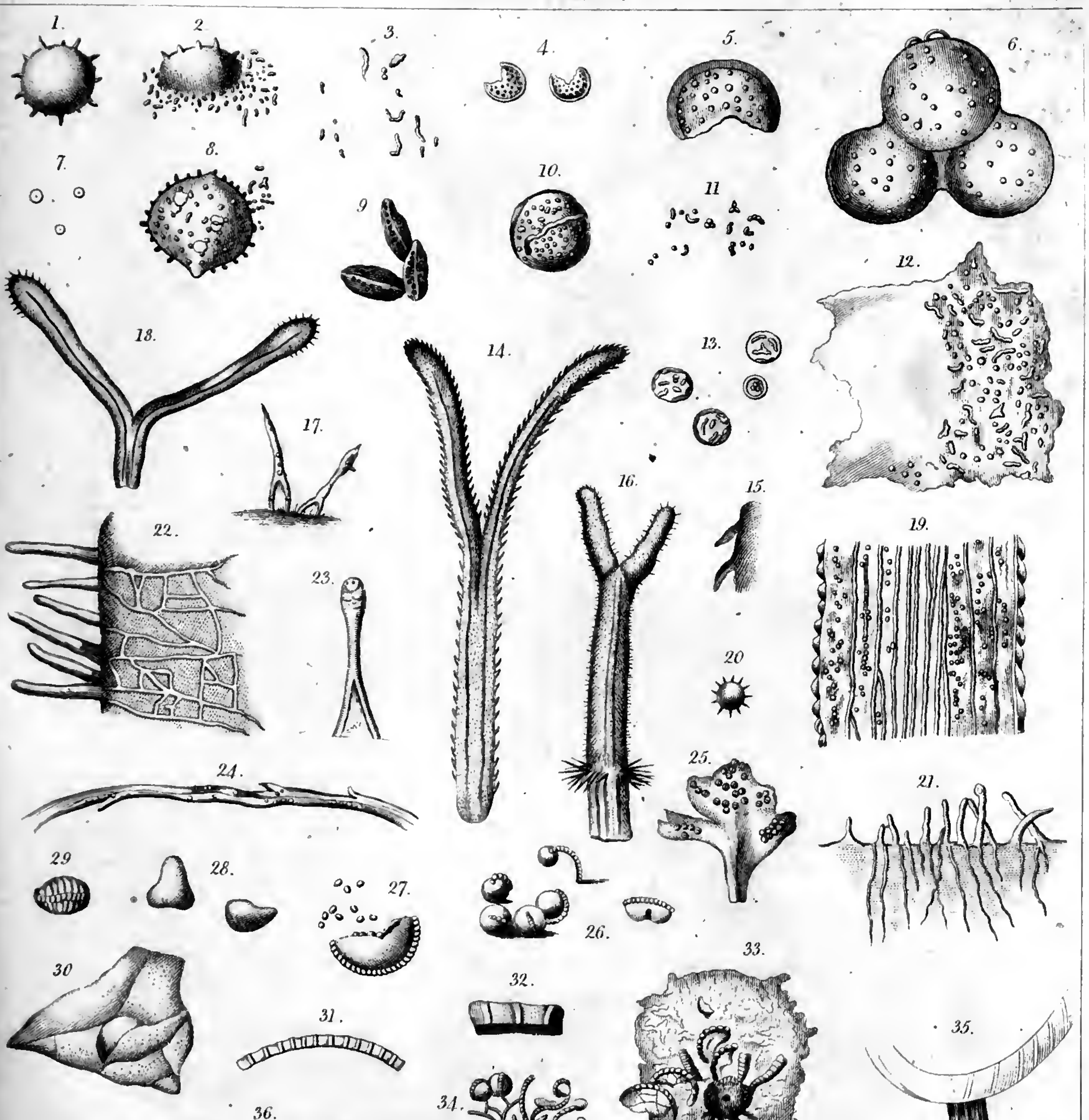
Mutlow Sc. Russell Del.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.



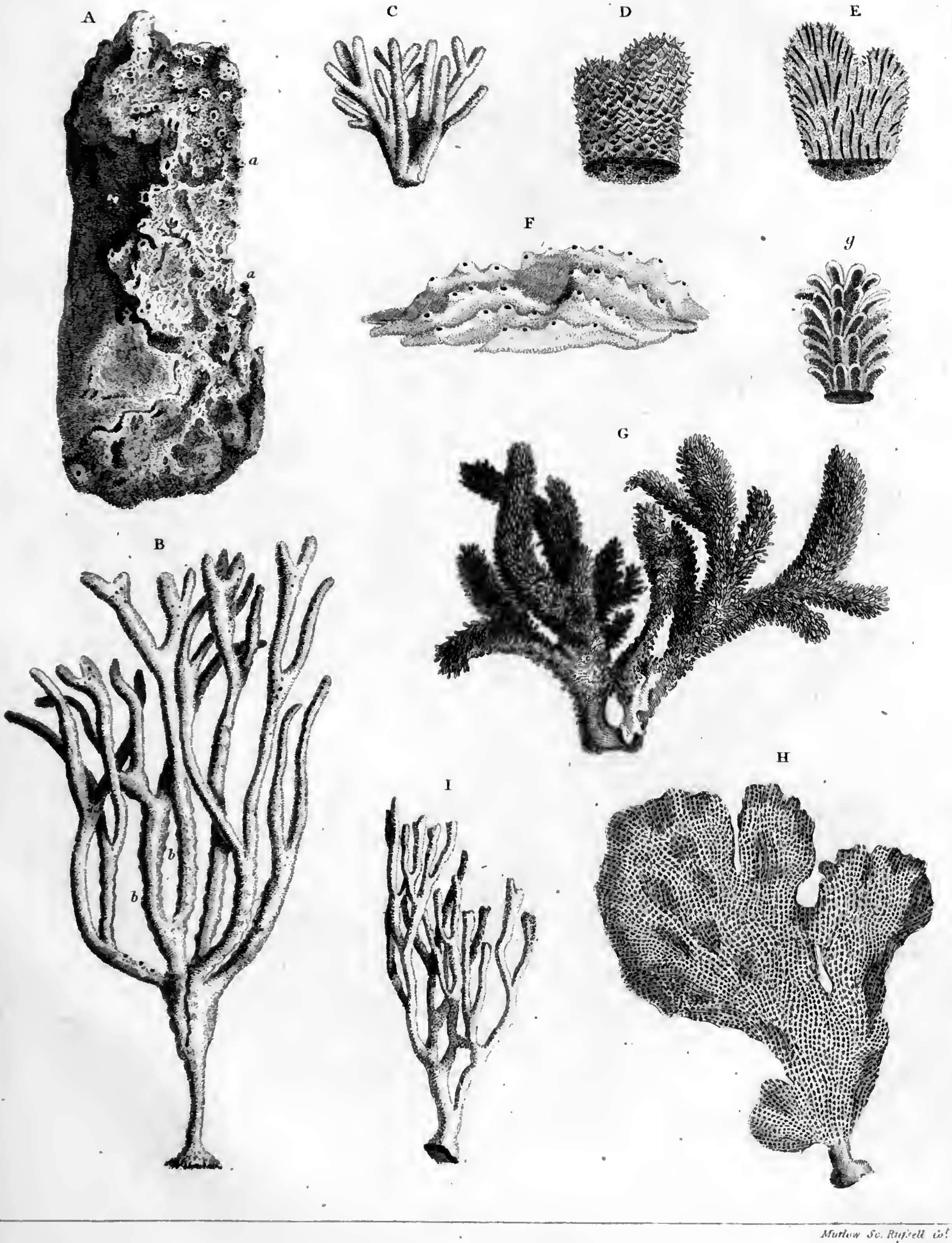
Mutlow Sc. Rufsell Co.

Published by C & R Baldwin, of New Bridge Street, London 1807



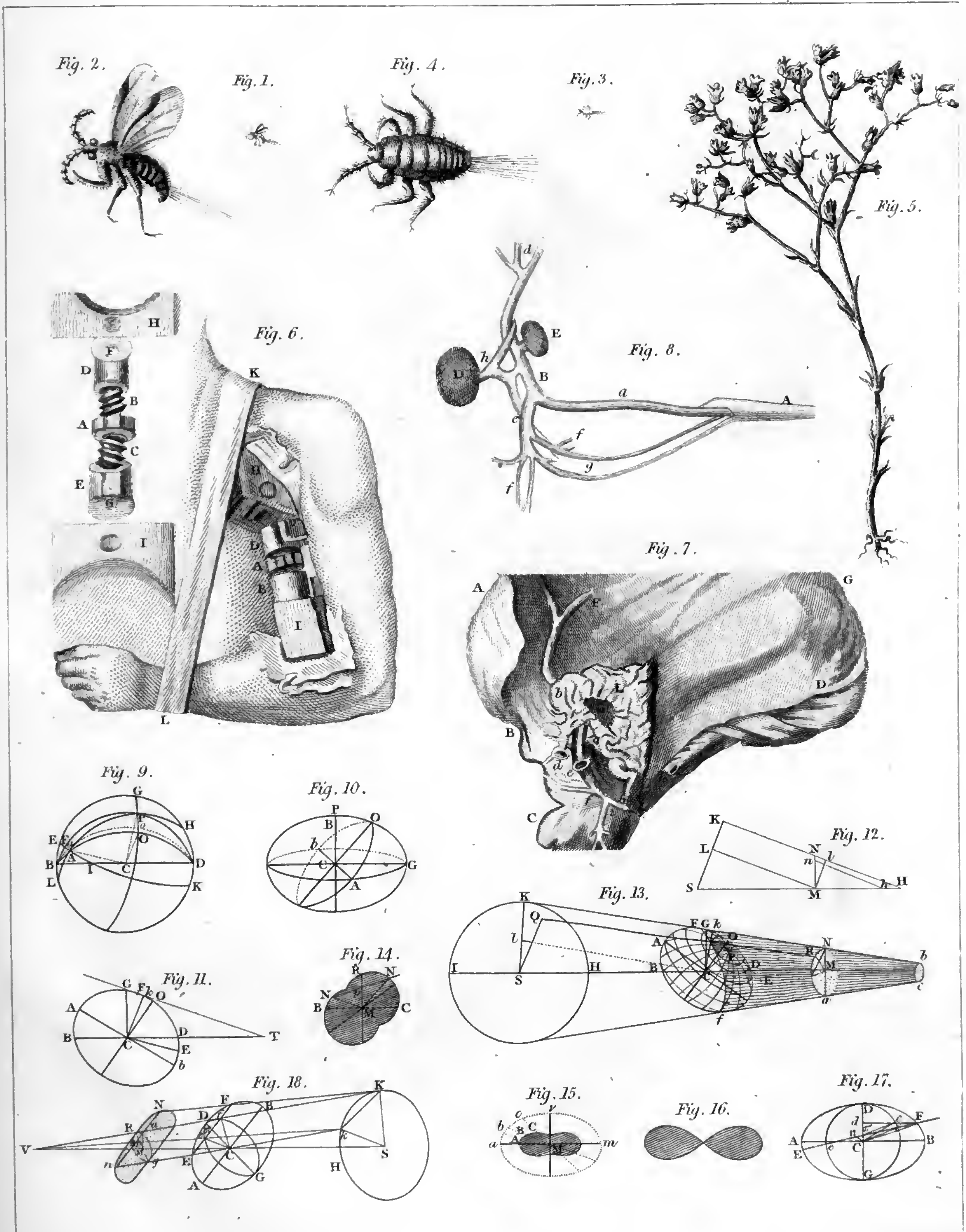
Mutlow Sc. Russell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.



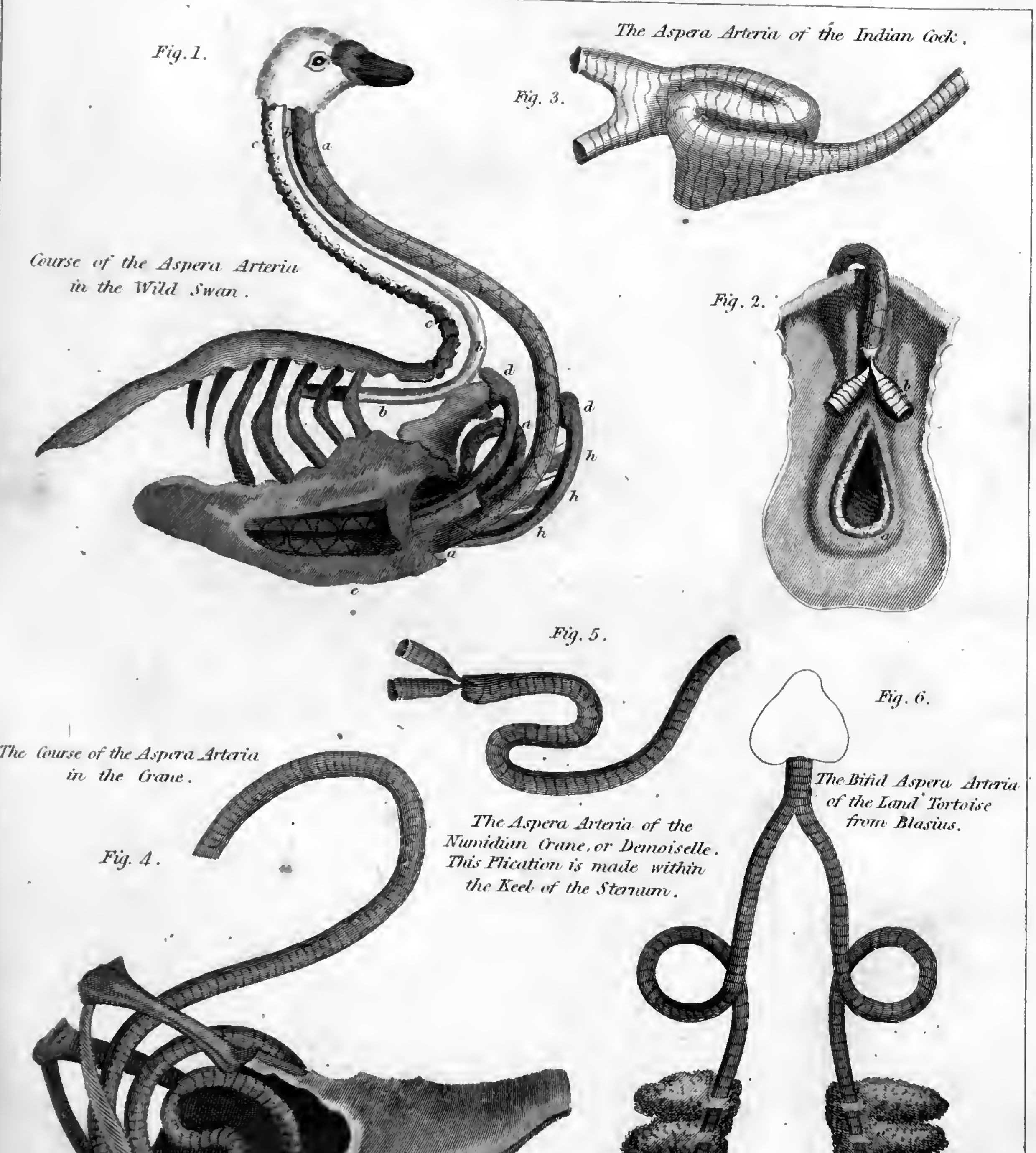
Mutlow Sc. Russell Col.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.



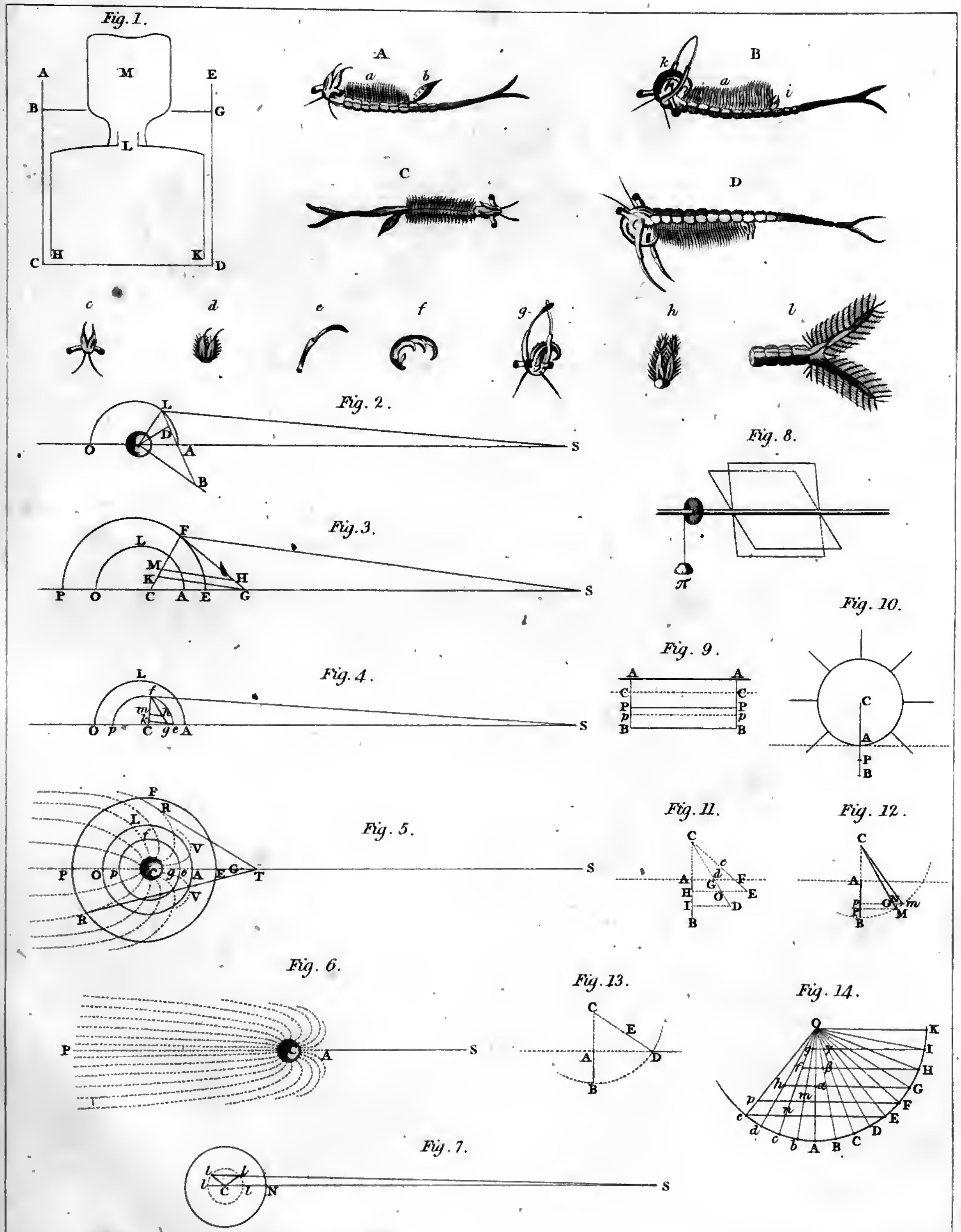
Mutlow Sc. Russell Ct.

Published by C & R Baldwin, of New Bridge Street, London, 1807.



Medlow Sc. Russell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.



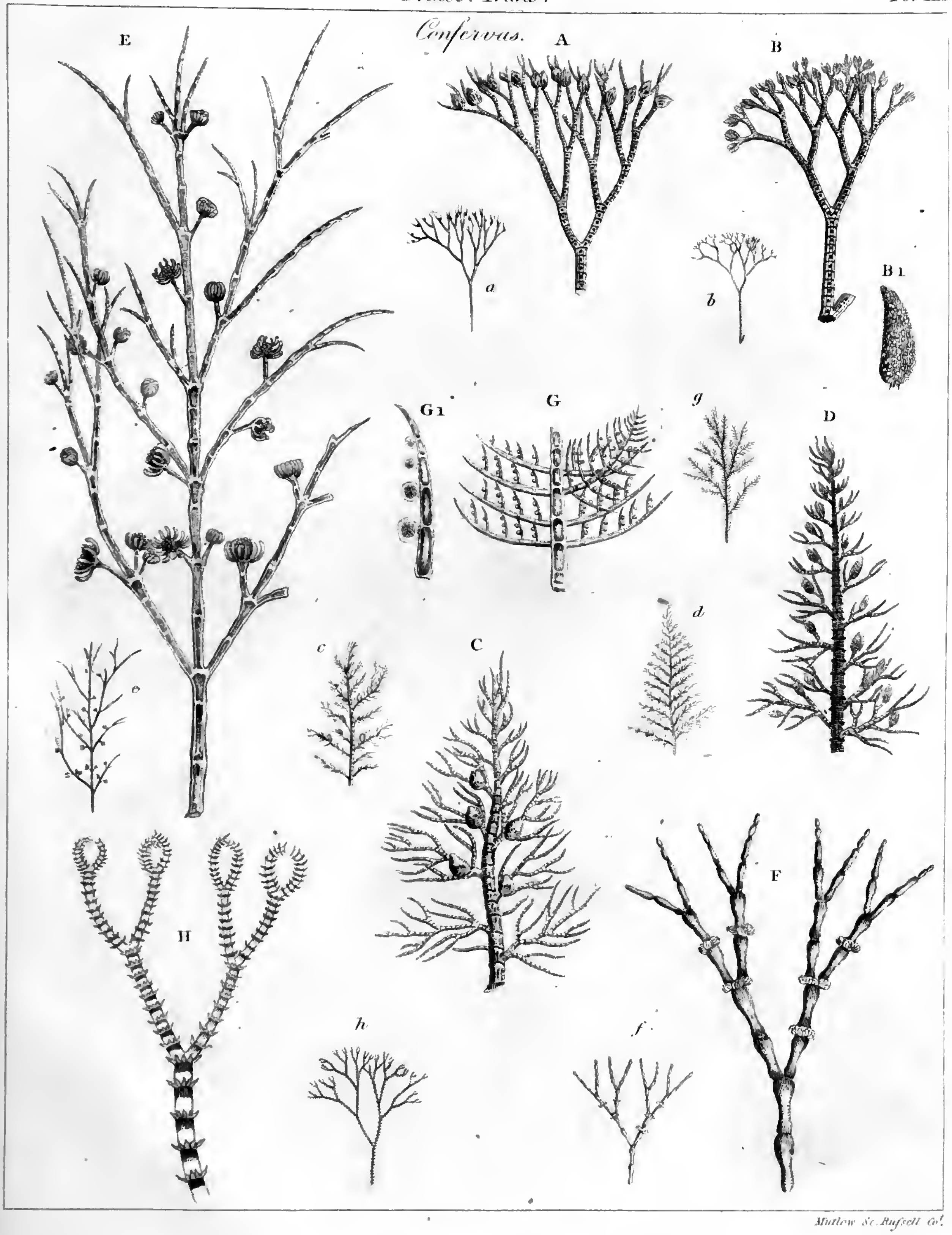
Mutlow Sc. Russell Ct.

Published by C. & R. Baldwin. of New Bridge Street. London. 1807.

Corallines.

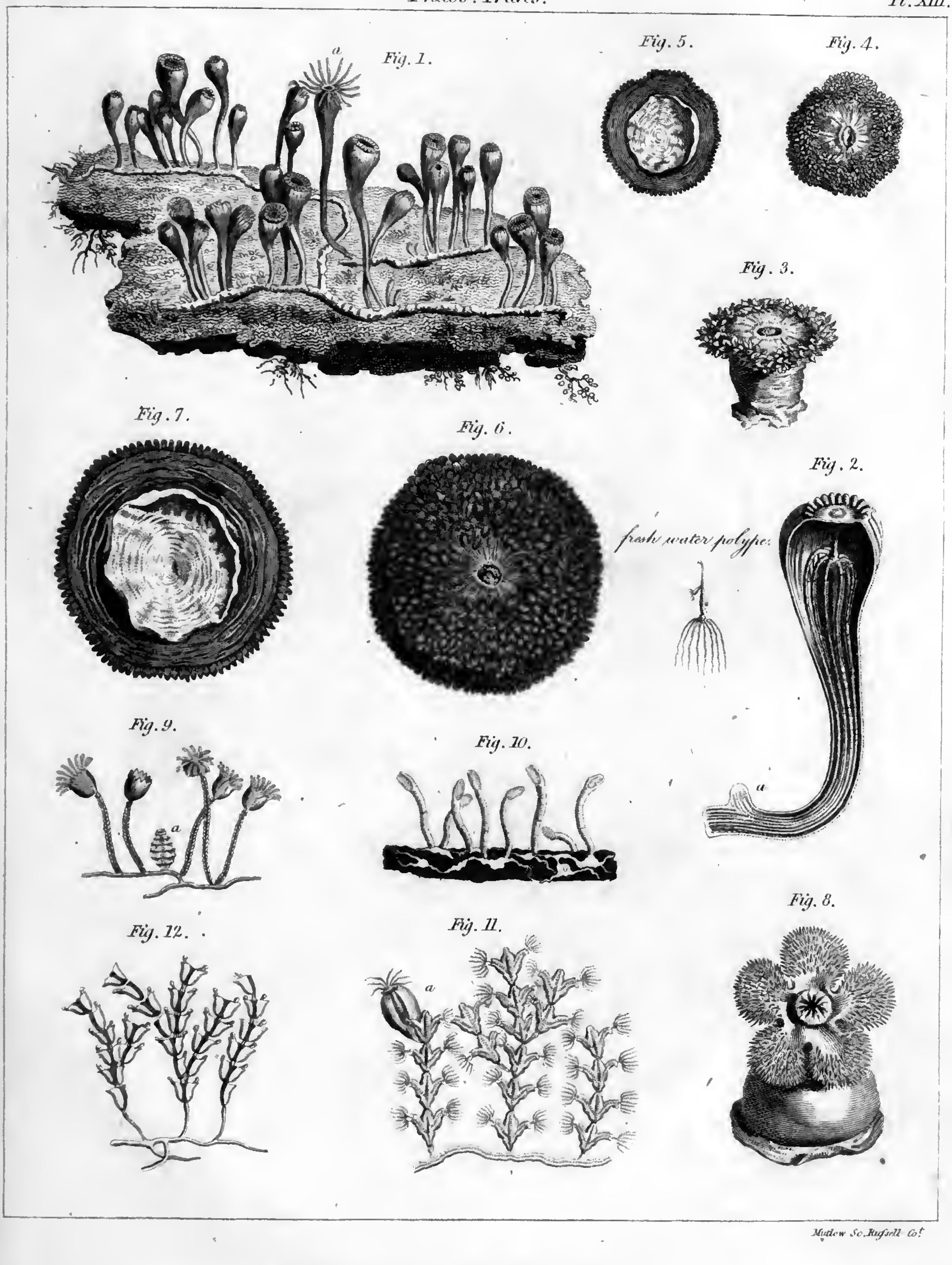
Mutlow Sc. Rufsell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.



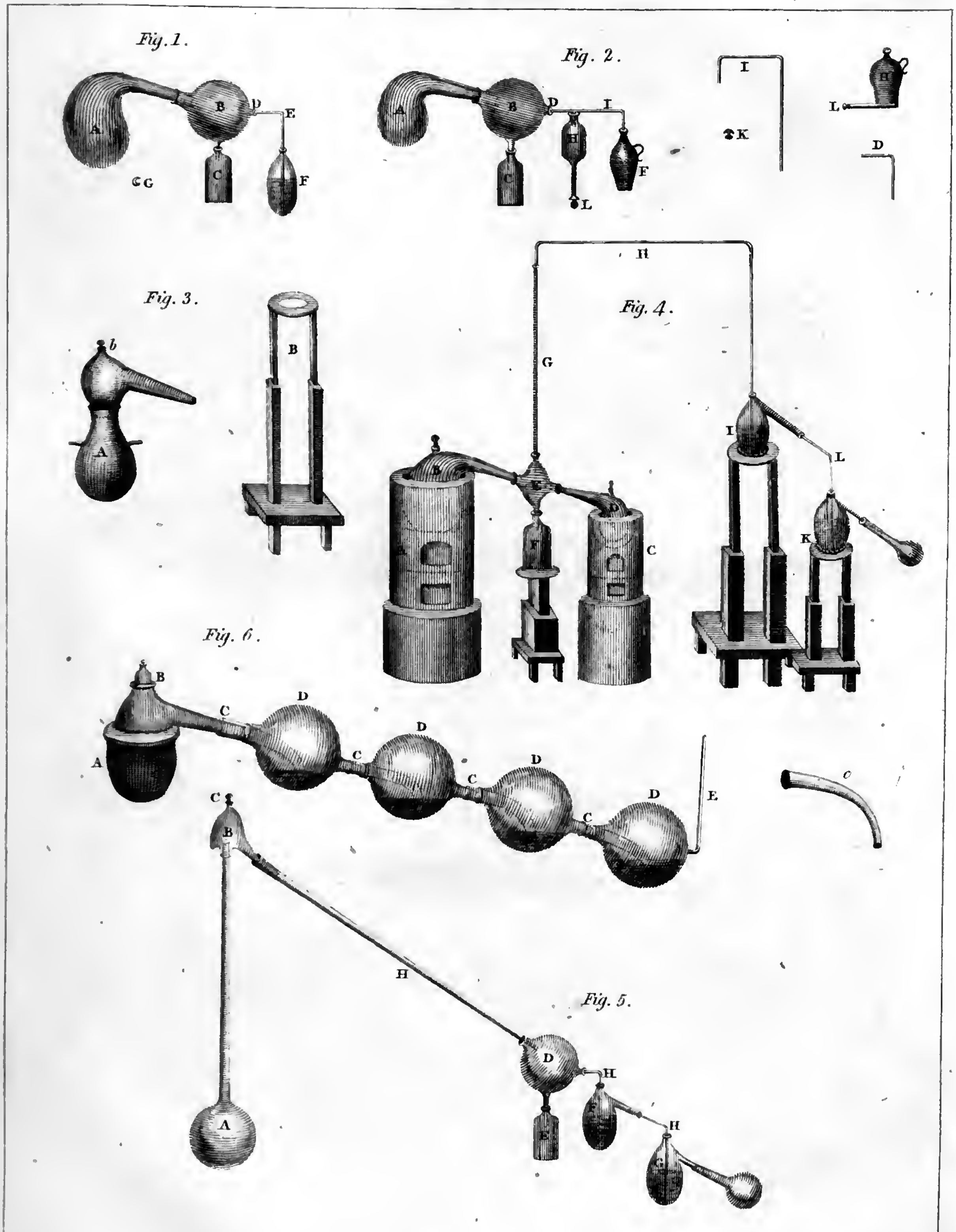
Mutlow Sc. Rufsell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.



Mutlow Sc. Russell Cot.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.



Mutlow Sc. Russell Co.

Published by C & R. Baldwin, of New Bridge Street, London, 1807.

Fig. 1.

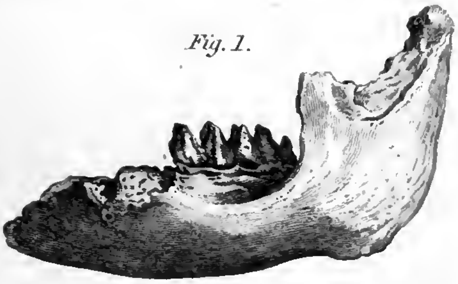


Fig. 2.

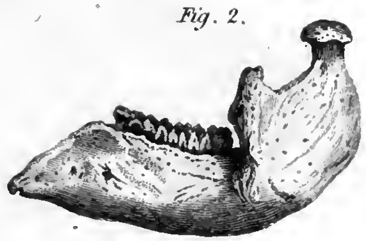


Fig. 4.

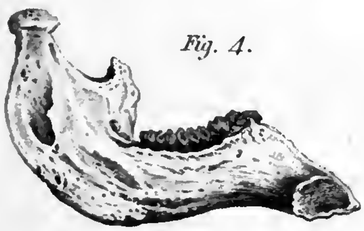


Fig. 3.

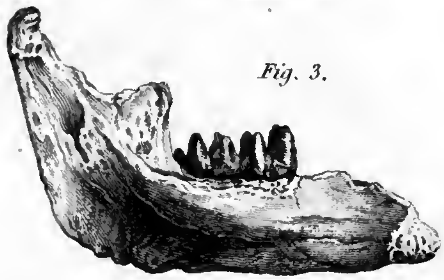


Fig. 5.

Fig. 6.

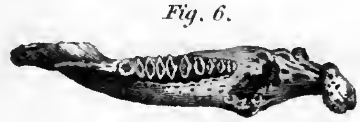


Fig. 7.

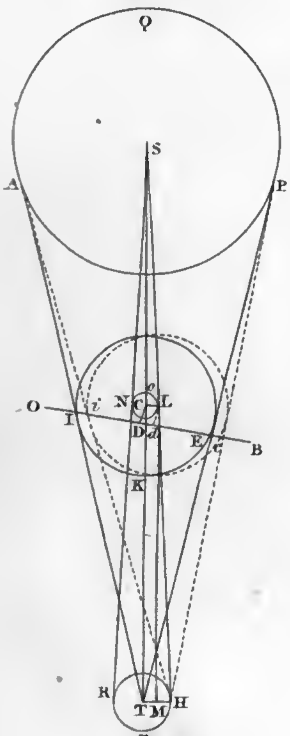


Fig. 9.

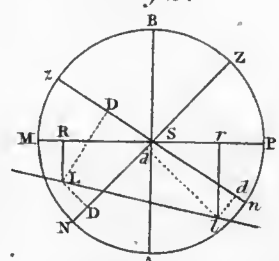


Fig. 8.

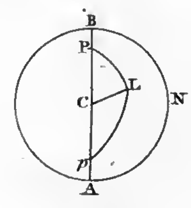


Fig. 10.

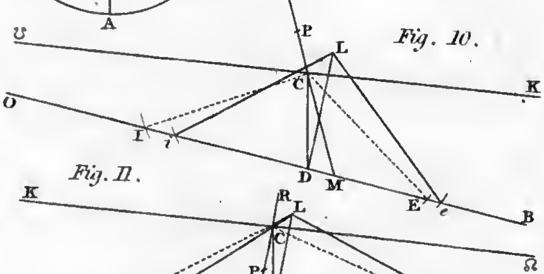


Fig. 11.

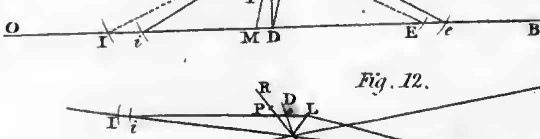
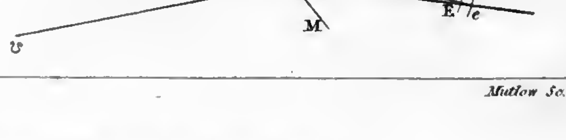
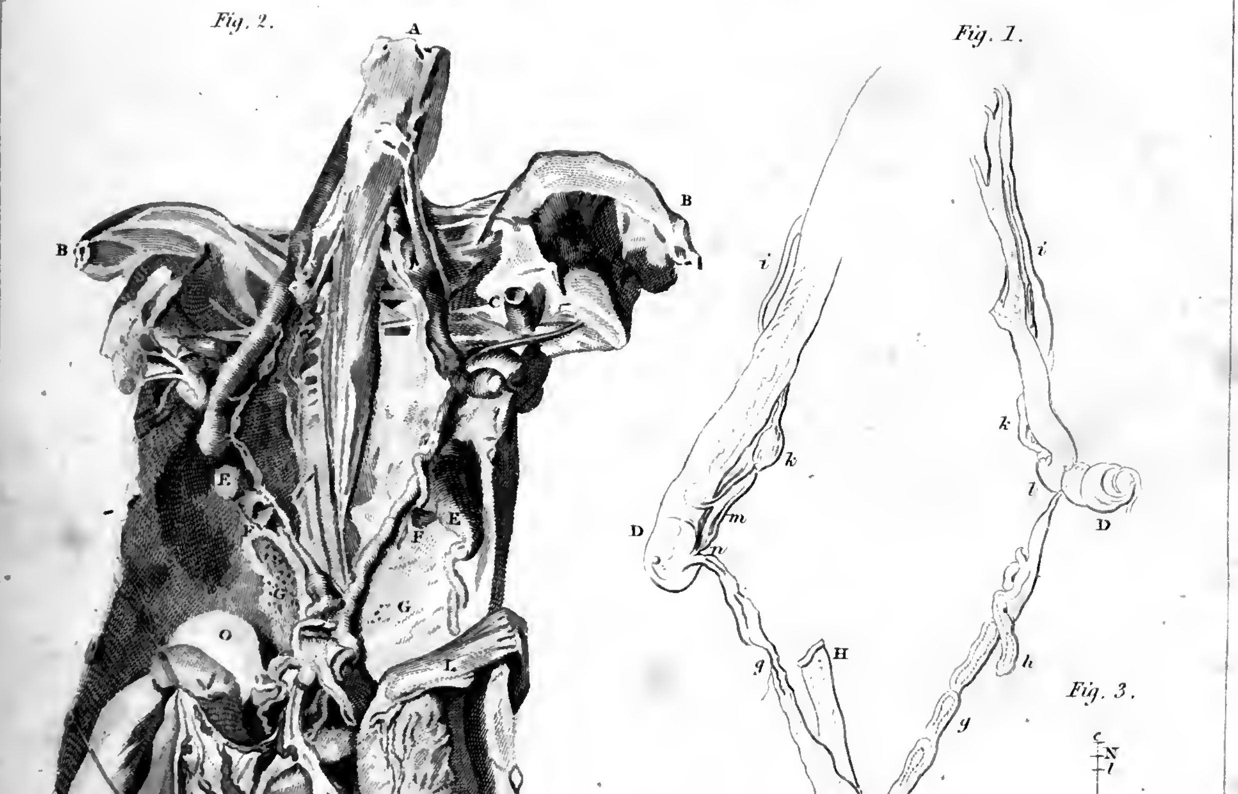


Fig. 12.



Published by C & R. Baldwin, of New Bridge Street, London 1807.



Mutlow Sc. Rufsell Ct.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.

Fig. 3.

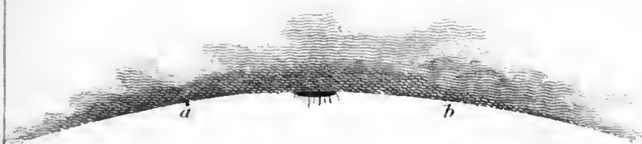


Fig. 4.

Fig. 5.

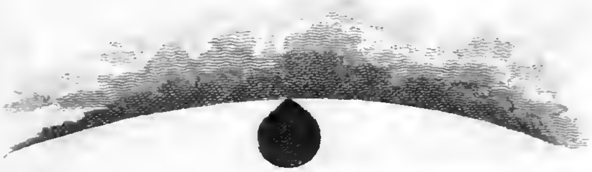


Fig. 6.

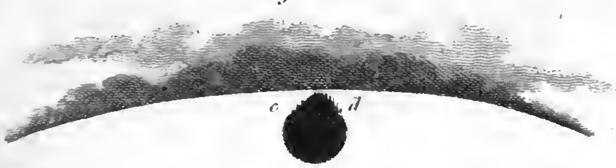


Fig. 7.

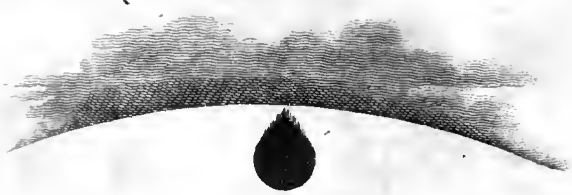


Fig. 8.

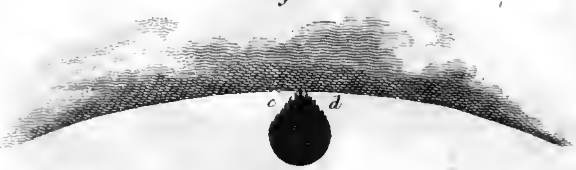


Fig. 9.

Fig. 10.

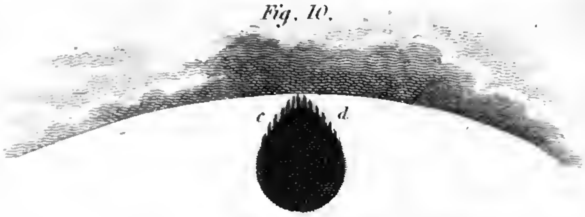


Fig. 1.

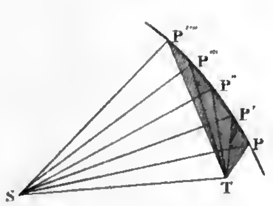


Fig. 2.

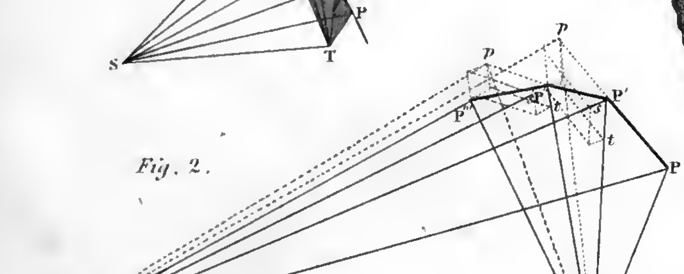
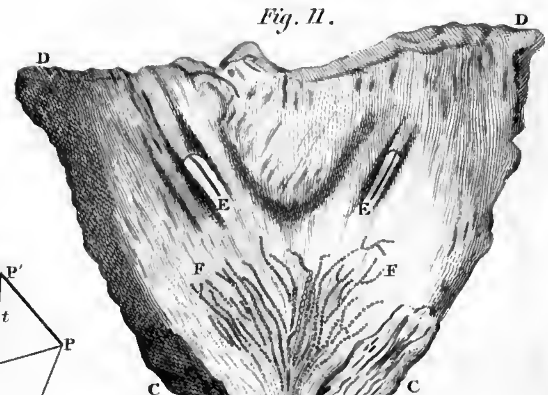
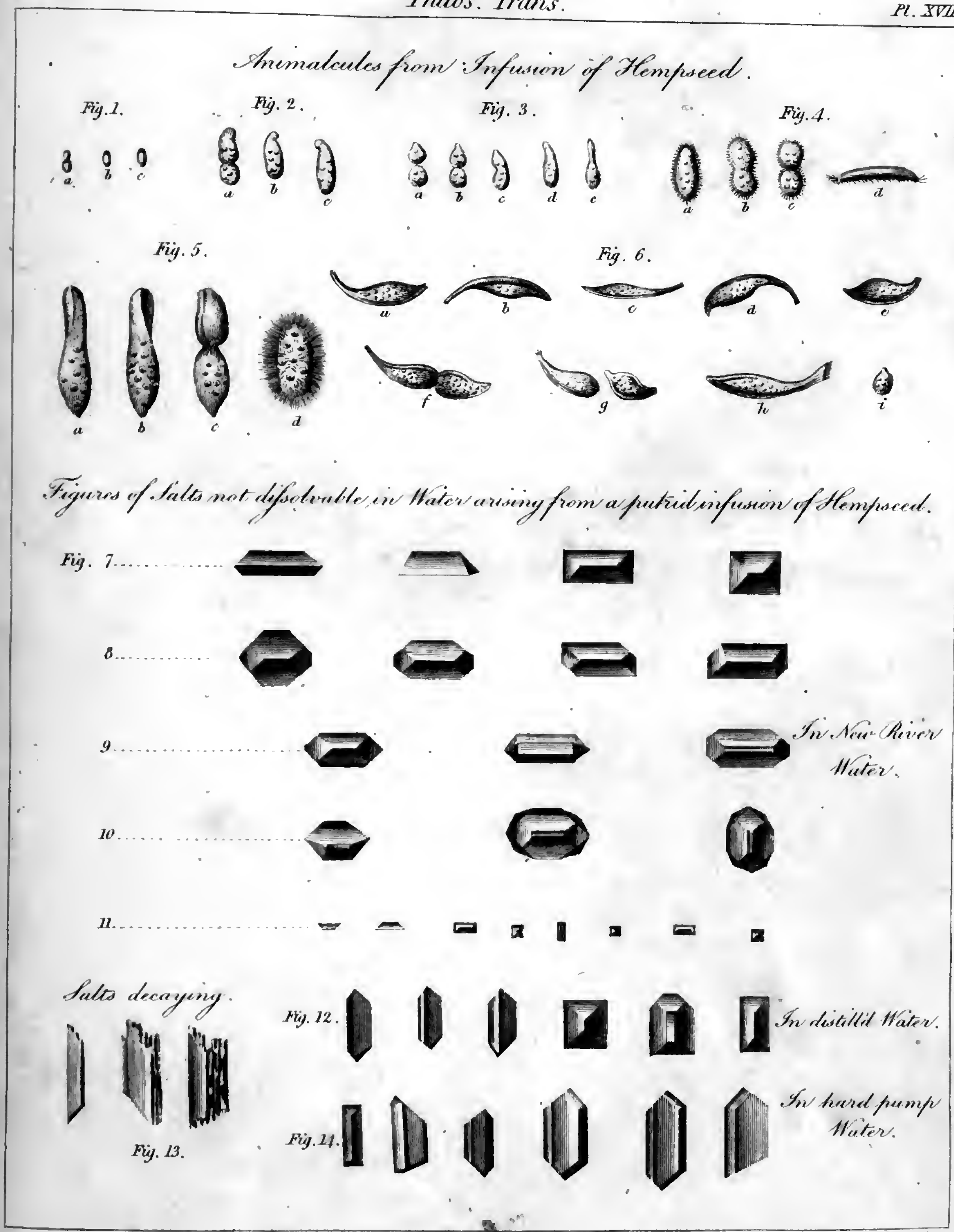


Fig. 11.



Mutlow Sc. Russell Co.

Published by C. & R. Baldwin, of New Bridge Street, London, 1807.



Mutlow Sc. Russell Ct.

Published by C & R Baldwin, of New Bridge Street, London, 1807.