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THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

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

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

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ERRATUM.

Page 746, in the table, *for* 1846 *read* 1847; and *for* 56 *read* 57 before Christ.

THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON;

ABRIDGED.

XVII. Description of a New Marine Animal. In a Letter from Mr. Everard Home, to John Hunter, Esq., F. R. S. With a Postscript by Mr. Hunter, containing Anatomical Remarks on the same. Dated Sept.* 20, 1784. p. 333.

About 3 years before Mr. H. sent this sea animal from Barbadoes, which was unlike any one he had ever seen. And even after his arrival in England, his inquiries concerning it had been without success. The specimen sent was found on a part of the coast which had undergone very remarkable changes, in consequence of a violent hurricane. These changes were indeed the means of its being discovered, and present a probable reason why it was not discovered before. The animal was found on the south-east coast of Barbadoes, close to Charles Fort, about a mile from Bridge Town, in some shoal water, separated from the sea by the stones and sand thrown up by the dreadful hurricane, which happened in the year 1780, and did so much mischief to the island. The wind, in the beginning of the storm, which was in the afternoon, blew very furiously from the north-west, making a prodigious swell in the sea; and in the middle of the night changing suddenly to the south-east, it blew from that quarter on the sea, already agitated, forcing it on the shore with so much violence, that it threw down the rampart of Fort Charles, which was opposed to it, though 30 feet broad, by the bursting of one sea. It forced up, at the same time, immense quantities of large coral rocks from the bottom of the bay, making a reef along this part of the coast for the extent of several miles, at only a few yards distance from the shore.

The soundings of the harbour were found afterwards to be entirely changed, by the quantity of materials removed from the bottom in different places. In

* This animal seems greatly allied to the *Serpula gigantea* of Linneus and Pallas, and which has sometimes been found on our own coasts, and is described, in the 7th vol. of the Linnean Transactions, by Col. Montagu, under the name of *Amphitrite volutacornis*.

the reef of coral was found an infinite number of large pieces of brain-stone, containing the shell of this animal; but the animals had either been long dead, or more probably destroyed by the motion of the rocks in the storm: some few of the brain-stones, however, that had been thrown beyond the reef, and lodged in the shoal water, receiving less injury, the animals were preserved unhurt. The animal, with the shell, was almost entirely inclosed in the brain-stone, so that at the depth in which they generally lie, they are hardly discernible, through the water, from the common surface of the brain-stone; but when in search of food they throw out two cones, with membranes twisted round them in a spiral manner, which have a loose fringed edge, looking at the bottom of the sea like two flowers; and in this state they were discovered. The animal, when taken out of the shell, including the two cones and their membranes, is 5 inches in length; of which the body is $3\frac{3}{4}$ inches, and the apparatus for catching its prey, which may be considered as its tentacula, about an inch and a quarter.

The body of the animal is attached to its shell, for about $\frac{3}{4}$ of an inch in length, at the anterior part where the two cones arise, by means of two cartilaginous substances, with one side adapted to the body of the animal, the other to the internal surface of the shell: the rest of the body is unattached, of a darkish white colour, about half an inch broad, a little flattened, and rather narrower towards the tail. The muscular fibres on its back are transverse; those on the belly longitudinal, making a band the whole length of the body, on the edge of which the transverse fibres running across the back terminate. The two cartilaginous substances by which the animal adheres to its shell, are placed one on each side of the body, and are joined together on the back of the animal at their posterior edges: they are about $\frac{3}{4}$ of an inch long, are very narrow at their anterior end, becoming broader as they go backwards; and at their posterior end they are the whole breadth of the body of the animal. On their external surface there are 6 transverse ridges, or narrow folds; and along their external edges, at the end or termination of each ridge, is a little eminence resembling the point of a hair pencil, so that on each side of the animal there are 6 of these little projecting studs, for the purpose of adhering to the sides of the shell in which the animal is inclosed. The internal surfaces of these cartilages are firmly attached to the body of the animal, in their middle part, by a kind of band or ligament; but the upper and lower ends are lying loose.

From the end of the body, between the two upper ends of these cartilages, arise what he supposes to be the tentacula, consisting of 2 cones, each having a spiral membrane, twining round it: they are close to each other at their bases, and diverge as they rise up, being about an inch and a quarter in length, and nearly $\frac{1}{6}$ of an inch in thickness at their base, and gradually diminishing till they terminate in points. The membranes which twine round these cones also take their origin from the body of the animal, and make $5\frac{1}{2}$ spiral turns round each, being lost in the points of the cones; they are loose from the cone at the lowest spiral turn which they make, and are nearly half an inch in breadth; they are exceedingly delicate, and have at small distances fibres running across them from their attachment at the stem to the loose edge, which gives them a ribbed appearance. These fibres are continued about $\frac{1}{10}$ of an inch beyond the membrane, having their edges finely serrated, like the tentacula of the Actiniæ found in Barbadoes: these tentacula shorten as the spiral turns become smaller, and are entirely lost in that part of the membrane which terminates in the point of the cone.

Behind the origin of these cones arises a small shell, which, for $\frac{1}{6}$ of an inch from its attachment to the animal, is very slender: it is about $\frac{3}{4}$ of an inch in length, becoming considerably broader at the other end, which is flat, and about $\frac{1}{3}$ of an inch broad; the flattened extremity is covered with a kind of hair, and has rising out of it 2 small claws, about $\frac{1}{6}$ of an inch in length. If the hair, and mucus entangled in it, be taken away, this extremity of the shell becomes concave, is of a pink colour, and the 2 claws rising out from its middle part have each 3 short branches, not unlike the horns of a deer. The body of this shell has a soft cartilaginous covering, with an irregular but polished surface: on this the cones rest in their collapsed state, in which state the whole of the shell is drawn into the cavity of the brain-stone, excepting the flattened end with the 2 claws. Before the cones there is a thin membrane, which appears to be of the same length with the shell just described. In the collapsed state it lies between the cones and the shell in which the animal is inclosed; but, when the tentacula are thrown out, it is also protruded.

The shell of this animal is a very thin tube, adapted to its body: the internal surface is smooth, and of a pinkish white colour: its outer surface is covered by the brain-stone in which it is inclosed, and the turnings and windings which it makes are very numerous. The end of the shell, which opens externally, rises above the surface of the stone on one side half an inch in height, for about half the circumference of the aperture, bending a little forwards over it, and becoming narrower and narrower as it goes up, terminating at last in a point just over the centre of the opening of the shell; on the other side it forms a round margin to the surface of the brain-stone. This part of the shell is much thicker and stronger than that part which is inclosed in the brain-stone: its outer surface is of a darkish brown colour; its inner of a pinkish white. The animal, when at rest, is wholly concealed in its shell; but when it seeks for food, the moveable shell is pushed slowly out with the cones and their membranes in a collapsed state; and when the whole is exposed, the moveable shell falls a little back, and the membrane round each of the cones is expanded, the tentacula at the bases of the cones having just room enough to move without touching each other. The thin membrane which lay between the cones and the inclosing shell is protruded in the form of a fold, and lies over the external shell which projects from the brain-stone.

The membranes have a slow spiral motion, which continues during the whole time of their being expanded; and the tentacula on their edges are in constant action. The motion of the membrane of the one cone seems to be a little different from that of the other, and they change from the one kind of motion to the other alternately, a variation in the colour of the membrane at the same time taking place, either becoming a shade lighter or darker; and this change in the colour, while the whole is in motion, produces a pleasing effect, and is most striking when the sun is very bright. The membranes however at some particular times appear to be of the same colour. While the membranes are in motion, a little mucus is often separated from the tentacula at the point of the cone. On the least motion being given to the water, the cones are immediately, and very suddenly, drawn in. This apparatus for catching food is the most delicate and complicated that he had seen. He annexed 2 drawings of the animal in its two different states, one in search of food, and one while lying at rest.

Mr. Hunter's Postscript.—Animals which come from foreign countries, and which cannot be brought to England alive, must be kept in spirits to preserve them from putrefaction, which makes them less fitted for anatomical examination; for the spirits, which preserve them, produce a change in many of their properties, and alter the natural colours, and texture of the parts, so that often the structure alone of the animal can be ascertained; and where this is not naturally distinct, it becomes often quite obscured, and the texture of the finer parts is wholly destroyed, requiring a very extensive knowledge of such parts in animals at large, to assist us in bringing them to light: this happens to be the case with the animal whose dissection is the subject of this postscript.

The animal may be said to consist of a fleshy covering, a stomach and intestinal canal, and the two cones with their tentacula and moveable shell, which last may be considered as appendages. The body of the animal is flattened, and terminates in two edges, which are intersected by rugæ, the fasciculi of transverse muscular fibres which run across the back being continued over them. On each of these edges is placed a row of fine hairs, which project to some distance from the skin. The fleshy covering consists principally of muscular fibres: those on the back are placed transversely, to contract the body laterally; those on the belly longitudinally, to shorten the animal when stretched out, and to draw it into the shell. The stomach and intestine make one straight canal: the anterior end of this forms the mouth, which opens into the grooves made by the spiral turns of the tentacula round the stem of each of the cones; and the intestine at the posterior end opens externally, forming the anus. From the contracted state of the animal, the intestine is thrown into a number of folds.

On examining the cones and the tentacula, Mr. H. at first believed that the spiral form arose from their being in a contracted state; and that when the tentacula were erected, the cone untwisted, forming a longer cone with the tentacula arising from its sides, like the plume from the stem of a feather; and that this stem was drawn in or shortened by means of a muscle passing along the centre, which threw the tentacula into a spiral line, similar to the penis of many birds; but how far this is really the case, he was not able to ascertain. The internal structure of this animal, like most of those which have tentacula, is very simple; it differs however materially from many, in having an anus, most animals of this tribe, as the Polypi, having only one opening, by which the food is received, and the excrementitious part of it also afterwards thrown out; this we must have supposed, from analogy, to take place in the animal which is here described, more particularly since it is inclosed in a hard shell, at the bottom of which there appears to be no outlet; but as there is an anus this cannot be the case.

It is very singular, that in the leach, polypi, &c. where no apparent inconvenience can arise from having an anus, there is not one, while in this animal, where it would seem to be attended with many, we find one; but there being no anus in the leach, polypi, &c. may depend on some circumstance in the animal economy which we are at present not fully acquainted with. The univalves, whose bodies are under similar circumstances respecting the shell with this animal, have the intestine reflected back, and the anus, by that means, brought near to the external opening of the shell, the more readily to discharge the excrement; and though this structure, in these animals, appears to be solely intended to answer that purpose, yet when we find the same structure in the black snail, which has no shell, this reasoning will not wholly apply, and we must refer it to some other intention in the animal economy. In this animal we must therefore rest satisfied that the disadvantageous situation of the anus, with respect to the excrement's being discharged from the shell, answers some purpose in the economy of the animal, which more than counter-balances the inconveniences produced by it. It would appear, from considering all the circumstances, that the excrement thrown out at the anus must pass from the tail along the inside of the tube, between it and the body of the animal, till it comes to the external opening of the shell, as there is no other evident mode of discharging it.

How the tube or shell is formed in stone or coral is not easily ascertained. It may be asked, whether this animal has the power of boring backwards as the Teredo Navalis probably does, or whether the stone or coral is formed at the same time with the animal, and grows and increases with it: and if we consider all the circumstances, this last would appear to be most probable, and agree best with the different phenomena; for the coral is lined with a shell, which could not be the case if the animal was continually increasing this hole, both in length and breadth, in proportion to its growth; but if the coral and the animal increase together, it is then similar to the growth of all shells, whether bivalve or univalve. The animal does not appear to have the power of increasing its canal, being only composed of soft parts. This however is no argument against its doing it, for every shell fish has the power of removing a part of its shell, so as to adapt the new and the old together; which is not done by any mechanical power, but by absorption. The tribe of animals which have tentacula consists of an almost infinite variety, and many of the species have been described. Of that kind however which has the double cones, he believes hitherto no account has been given. It is most probably to be found in the seas surrounding the different islands in the West Indies; for he once received an animal from St. Vincent's; which, on examination, proved to be the same animal with that above described, only that the moveable shell was wanting.

After writing this postscript, Mr. H. found a description of a double-coned Terebella, published by the Rev. Mr. Cordiner, at Bamf in Scotland, which was found on that coast; in which the cones have their tentacula passing out from the end, and when erected they spread from the cone as from a centre. This proves that the double-coned tentacula also have different species.

Explanation of the Figures.—Fig. 1, pl. 1, is a drawing of the animal after death, as it appeared in spirits. A, the under side of the body; BB, the cartilages which attach the animal to the sides of the cavity in which it lies; c, one of the cones covered by its membrane in a collapsed state; D, the lowest spiral turn of the membrane and its tentacula spread out; EE, the cut edges of the divided membrane, which are turned on each side to show the cone; F, the cone as it appears in the intervals between the spiral turns of the membrane; G, the moveable shell, with the smooth cartilaginous covering, in an outside view; H, the flattened end of the moveable shell, with hair on it; II, the two claws that arise from the surface of the flattened end of the moveable shell; K, the anus, into which a hog's bristle is introduced.

Fig. 2 is a drawing of the animal, with its tentacula expanded in search of food, as it appears in the sea; taken from a sketch made in Barbadoes, where no draughtsman could be procured while the animal was alive: a, the sort of brain-stone in which the animal was discovered; b, the external prominent shell; cc, the membrane which is protruded with the cones and moveable shell, and makes a fold over the edges of the prominent shell; dd, the membranes and tentacula in a state of expansion; e, the inner side of the moveable shell, as it appears when protruded; f, the hole in the brain-stone as it appears when the prominent shell is broken off, and which may be seen in many specimens of brain-stone.

XVIII. A Description of a New System of Wires in the Focus of a Telescope, for Observing the comparative Right Ascensions and Declinations of Celestial Objects, &c. By the Rev. Francis Wollaston, LL. B., F. R. S. p. 346.

The rhombus (for a rhombus, and not a rhomboid, it ought most properly to be called) is very good in theory; but very difficult to get executed with precision, and liable to some inaccuracy in the observation. The truth of it depends on the longer diagonal being exactly twice the length of the shorter one; which requires an awkward angle (53° 7′ 48″) at the vertex, not easily to be hit by the workmen, and therefore seldom sufficiently true. Beside this, as the sides of the rhombus, on which depends the calculation for differences of declination, are but 26° 33′ 54″ declining from the perpendicular or horary wire, a very small error in observing the passage of a star makes a very material difference in the result.

This determined Mr. W. to make trial of a square placed angularly, an addition to M. Cassini's wires at 45°, which seems to answer better. The whole extent of the field is employed as it is in the rhombus (the want of which was said to be Dr. Bradley's objection to M. Cassini's wires); but being formed of right angles or half-right angles, to which workmen are most accustomed, they will always be apt to execute their part better; and the obliques, from the differences being just double to what they are in the rhombus, give the comparative declinations with twice the certainty. To this the number of corresponding observations in the passage of every star add considerably; since we may calculate its distance from the centre, or from the angles, or from one of the intermediate angles, as occasion may require, with double the precision of the rhombus. In each of them, the parallel wires will tell whether the placing of the instrument be true or faulty; because, if truly made and truly set, the same star must take the same time in passing from one wire to its corresponding parallel; which will differ considerably, and in every star the same way, if the position be faulty.

It may be proper to add, what indeed is nothing new, that if the position of the instrument be found erroneous, the formula given by M. De Lalande in his Astronomy will serve to rectify the observation. Calling the larger interval between the passage of any oblique and the horary wire m, and the smaller one n, then $\frac{m^2n + n^2m}{m^2 + n^2}$ will give the difference of declination (in time to be converted into degrees, and multiplied by the cosine of declination) from the angle where that oblique meets the horary; and $\frac{m^2n - n^2m}{m^2 + n^2}$ the difference in right ascension from the same angle. It is almost needless to mention, that where the position is true, half the interval of time between a star's passing any two corresponding obliques, converted into degrees, and multiplied by the cosine of declination, will give the difference in declination of that star from the angle where those obliques meet, as the whole interval does in the rhombus.

But it may perhaps be of service to astronomy, or at least not unacceptable to those gentlemen who use the rhombus, says Mr. W., that I should subjoin another formula (contrived for me the last summer by my son, now Mathematical Lecturer at Sidney College, Cambridge), for investigating the comparative right ascensions and declinations of stars observed by it, when the instrument is not placed truly in the plane of the equator. I was led to wish for some such formula, in consequence of an ingenious paper, kindly communicated to me by Sir H. C. Englefield, Bart., F. R S., giving an account of his method of doing it by a scale and figure; which, though very easy when one is provided with such a scale, appeared to be of less general use than by calculation; and I do not know that any thing of the kind is to be met with in any publication.

Let the angle DLL, fig. 3, pl. 1, which by construction is 63° 26′, be called a; the diagonal LL be called b;

The larger interval observed between the passage of a star by an oblique and the horary wire, as bc, be called m;

The smaller ditto of the same star, as cd, be n;

The large ditto of another star, as $\beta\gamma$, be μ;

The smaller ditto, as $\gamma\delta$, be ν:

Then $\frac{2(m-n)}{m+n}$ = tangent of the angle which LL makes with a parallel of declination: call this q: the angle q being thus found, then $\frac{2(n \backsim \nu) \times \sin.(a+q)}{\text{R} \times \sin. a} \times \cos. q.$ = difference in declination between the two points on the vertical wire where those stars pass it. Which, being in time, must be converted into degrees, and multiplied by cosine of declination as usual, to give the true difference in declination between the stars.

And the same expression, viz. $\frac{2(n \backsim \nu) \times \sin.(a+q)}{\text{R} \times \sin. a} \times \sin. q$ = the difference in A. R. between those two points; to be applied as a correction to the observed times.

The same may be done by the larger intervals m and μ, only by substituting $a-q$ instead of $a+q$, thus: $\frac{2(m \backsim \mu) \times \sin.(a-q)}{\text{R} \times \sin. a} \times \cos. q$ = difference in declination as above; or ..... $\times \sin. q$ = ascensional difference.

If the stars differ too much in declination to come within the expression above (as N° 2 and 3) then the differences of the angles D and E in declination and right ascension may be found thus:

$\frac{2 . b \times \cos. q}{\text{R}}$ = diff. in declin. between D and E; $\frac{2 . b \times \sin. q}{\text{R}}$ = their ascen. diff., and the difference of each star from its respectively nearest angle of the rhombus, may be deduced by the former expression, leaving out the consideration of the other star, thus: $\frac{2 . n \times \sin. (a + q)}{R \times \sin. a} \times \cos. q =$ diff. of the star in declin. from its nearest angle, and . . . $\times \sin. q =$ its difference in right ascension.

The application of these formulæ is very easy: for having found q, if you set down its cosine in one column for declination, and its sine in another column for right ascension, and under each the constant sin. $(a + q)$, and the arithmetical compl. of sin. a; these being added together will make two sums, for the comparative observations of every star which may pass the field; and, unless the field be very large, and the declination of the stars very great, if to the column for declination be added the cosine of declination of the centre of the field, it will adapt itself to all the products.

XIX. Account of a Stag's Head and Horns, found at Alport, Derbyshire. By the Rev. Robert Barker, B. D. p. 353.

About the year 1780, some men working in a quarry of that kind of stone which in Derbyshire is called tuft,* at about 5 or 6 feet below the surface, in a very solid part of the rock, met with several fragments of the horns and bones of animals. Among the rest, out of a large piece of the rock, which they got entire, there appeared the tips of 3 or 4 horns, projecting a few inches from it, and the scapula of some animal adhering to the outside of it. A friend sent the piece of the rock to Mr. B. in the state they got it, in which he let it remain for some time. But suspecting that they might be tips of the horns of some head inclosed in the lump, he determined to gratify his curiosity by clearing away the stone from the horns. On doing which he found that the lump contained a very large stag's head, with two antlers on each horn, in a very perfect preservation, inclosed in it.

Though the horns are so much larger than those of any stag he ever saw, yet, from the sutures in the skull appearing very distinct in it, one would suppose that it was not the head of a very old animal. He had one of the horns nearly entire, and the greatest part of the other, but so broken in the getting out of the rock, that one part will not join to the other, as the parts of the other horn do. The horns are of that species which park-keepers in this part of the country call throstle-nest horns, from the peculiar formation of the upper part of them, which is branched out into a number of short antlers, which form a hollow about large enough to contain a thrush's nest. Below are the dimensions of the different parts of them, compared with the horns of the same species of a large stag, which have probably hung in the place whence he pro-

* Tuft is a stone formed by the deposit left by water passing through beds of sticks, roots, vegetables, &c. of which there is a large stratum at Matlock, Bath, in this county.—Orig.

cured them 2 or 3 or perhaps more centuries; and with another pair of horns of a different kind, which are terminated by one single pointed antler, and which were the horns of a seven-year-old stag.

The river Larkell runs down the valley, and part of it falls into the quarry where these horns were found, the water of which has not the property of incrusting any bodies it passes through. It is therefore probable that the animal to which these horns belonged was washed into the place where they were found, at the time of some of those convulsions which contributed to raise this part of the island out of the sea. Besides this complete head, Mr. B. had several pieces of horns, bones, and several vertebræ of the back, found in the same quarry; some, if not all, of them probably belonging to the same animal.

Dimensions of the three kinds of horns above-mentioned.

	Found at Alport.		Throstle-nest horns.		Seven years stag's.	
	Ft.	In.	Ft.	In.	Ft.	In.
Circumference at their base..........	0	$9\frac{7}{8}$	0	7	0	$5\frac{1}{2}$
Length of the lowest antler..........	1	2	1	0	0	9
Length of second ditto..............	0	$11\frac{1}{2}$	0	$10\frac{1}{2}$	0	10
Length of third ditto	1	$1\frac{1}{2}$	0	$11\frac{1}{2}$	0	10
Length of the horn	3	$3\frac{1}{2}$	2	$7\frac{1}{2}$	2	$8\frac{3}{4}$

XX. On the Sensitive Quality of the Tree Averrhoa Carambola. By Robert Bruce, M.D. p. 356.

The averrhoa carambola of Linneus, a tree called in Bengal the camruc or camrunga, is possessed of a power somewhat similar to those species of mimosa termed sensitive plants; its leaves, on being touched, move very perceptibly. In the mimosa the moving faculty extends to the branches; but, from the hardness of the wood, this cannot be expected in the camrunga. The leaves are alternately pinnated, with an odd one; and in their most common position in the day-time are horizontal, or on the same plane with the branch from which they come out. On being touched, they move themselves downwards, frequently in so great a degree that the two opposite almost touch each other by their under sides, and the young ones sometimes either come into contact or even pass each other. The whole of the leaves of one pinna move by striking the branch with the nail of the finger, or other hard substance; or each leaf can be moved singly, by making an impression that shall not extend beyond that leaf. In this way, the leaves of one side of the pinna may be made to move, one after another, while the opposite continue as they were; or you may make them move alternately, or in short in any order you please, by touching in a proper manner the leaf you wish to put in motion. But if the impression, though made on a single leaf, be strong, all the leaves on that pinna, and sometimes on the neighbouring ones, will be affected by it.

What at first seemed surprizing was, that notwithstanding this apparent sensibility of the leaf, I could with a pair of sharp scissars make large incisions in it, without occasioning the smallest motion; nay, even cut it almost entirely off, and the remaining part still continue unmoved; and that then, by touching the wounded leaf with the finger or point of the scissars, motion would take place as if no injury had been offered. But, on further examination I found, that though the leaf was the ostensible part which moved, it was in fact entirely passive, and that the petiolus was the seat both of sense and action: for though the leaf might be cut in pieces, or squeezed with great force, provided its direction was not changed, without any motion being occasioned; yet if the impression on the leaf was made in such a way as to affect the petiolus, the motion took place. When therefore I wanted to confine the motion to a single leaf, I either touched it so as only to affect its own petiolus, or, without meddling with the leaf, touched the petiolus with any small-pointed body, as a pin or knife. By compressing the universal petiolus near the place where a partial one comes out, the leaf moves in a few seconds, in the same manner as if you had touched the partial petiolus.

Whether the impression be made by puncture, percussion, or compression, the motion does not instantly follow; generally several seconds intervene, and then it is not by a jerk, but regular and gradual. Afterwards, when the leaves return to their former situation, which is commonly in a quarter of an hour or less, it is in so slow a manner as to be almost imperceptible. On sticking a pin into the universal petiolus at its origin, the leaf next it, which is always on the outer side, moves first; then the first leaf on the opposite side, next the 2d leaf on the outer, and so on. But this regular progression seldom continues throughout; for the leaves on the outer side of the pinna seem to be affected both more quickly, and with more energy, than those of the inner, so that the 4th leaf on the outer side frequently moves as soon as the 3d on the inner; and sometimes a leaf, especially on the inner side, does not move at all, while those above and below it are affected in their proper time. Sometimes the leaves at the extremity of the petiolus move sooner than several others which were nearer the place where the pin was put in. On making a compression with a pair of pincers on the universal petiolus, between any two pair of leaves, those above the compressed part, or nearer the extremity of the petiolus, move sooner than those under it, or nearer the origin; and frequently the motion will extend upwards to the extreme leaf, while below it perhaps does not go farther than the nearest pair.

If the leaves happen to be blown by the wind against each other, or against the branches, they are frequently put in motion; but when a branch is moved gently, either by the hand or the wind, without striking against any thing, no motion of the leaves takes place. When left to themselves in the day-time, shaded from the sun, wind, rain, or any disturbing cause, the appearance of the leaves is different from that of other pinnated plants. In the last a great uniformity subsists in the respective position of the leaves on the pinna; but here some will be seen on the horizontal plane, some raised above it, and others fallen under it; and in about an hour, without any order or regularity, which I could observe, all these will have changed their respective positions. I have seen a leaf, which was high up, fall down; this it did as quickly as if a strong impression had been made on it, but there was no cause to be perceived. Cutting the bark of the branch down to the wood, and even separating it about the space of half an inch all round, so as to stop all communication by the vessels of the bark, does not for the first day affect the leaves, either in their position or their aptitude for motion. In a branch, which I cut through in such a manner as to leave it suspended only by a little of the bark no thicker than a thread, the leaves next day did not rise so high as the others; but they were green and fresh, and, on being touched, moved, but in a much less degree than formerly.

After sun-set the leaves go to sleep, first moving down so as to touch each other by their under sides; they therefore perform rather more extensive motion at night of themselves than they can be made to do in the day-time by external impressions. With a convex lens I have collected the rays of the sun on a leaf, so as to burn a hole in it, without occasioning any motion. But when the experiment is tried on the petiolus, the motion is as quick as if from strong percussion, though the rays were not so much concentrated as to cause pain when applied in the same degree on the back of the hand; nor had the texture of the petiolus been any ways changed by this; for next day it could not be distinguished, either by its appearance or moving power, from those on which no experiment had been made. The leaves move very fast from the electrical shock, even though a very gentle one; but the state of the atmosphere was so unfavourable for experiments of this kind, that I could not pursue them so far as I wished.

There are 2 other plants mentioned as species of this genus by Linneus. The first, the averrhoa bilimbi, I have not had an opportunity of seeing. The other, or averrhoa acida, does not seem to belong to the same class; nor do its leaves possess any of the moving properties of the carambola. Linneus's generic description of the averrhoa, as of many other plants in this country which he had not an opportunity of seeing fresh, is not altogether accurate. The petals are connected by the lower part of the lamina, and in this way they fall off while the ungues are quite distinct. The stamina are in 5 pairs, placed in the angles of the germen. Of each pair only one stamen is fertile, or furnished with an anthera. The filaments are curved, adapted to the shape of the germen. They may be pressed down gently, so as to remain; and then, when moved a little upwards, rise with a spring. The fertile are twice the length of those destitute of antheræ.—*Calcutta, Nov.* 23, 1783.

XXI. Experiments on the Loss of Weight in Bodies on being Melted or Heated. By George Fordyce, M.D. F.R.S. p. 361.

Though I have made many experiments on the subject of the loss of weight in bodies on being melted, or heated, I do not think it worth while to lay them all before the Society, as there has not appeared any circumstance of contradiction in them. I shall content myself with relating the following one, which appears conclusive in determining the loss of weight in ice when thawed into water, and subject to the least fallacy of any I have hitherto made, in showing the loss of weight in ice on being heated. The beam I made use of was so adjusted as that, with a weight between 4 and 5 ounces in each scale, $\frac{1}{1000}$ part of a grain made a difference of 1 division on the index. It was placed in a room the heat of which was 37 degrees of Fahrenheit's thermometer, between 1 and 2 in the afternoon, and left till the whole apparatus and the brass weights acquired the same temperature.

A glass globe, of 3 inches diameter nearly, with an indentation at the bottom, and a tube at the top, weighing about 451 grains, had about 1700 grains of New River water poured into it, and was hermetically sealed, so that the whole, when perfectly clean, weighed $2150\frac{31}{32}$ of a grain exactly; the heat being brought to 32 degrees, by placing it in a cooling mixture of salt and ice till it just began to freeze, and shaking the whole together. After it was weighed it was again put into the freezing mixture, and let stand for about 20 minutes; it was then taken out of the mixture; part of the water was found to be frozen; and it was carefully wiped, first with a dry linen cloth, and afterwards with dry washed leather; and on putting it into the scale it was found to have gained about the $\frac{1}{60}$ part of a grain. This was repeated 5 times: at each time more of the water was frozen, and more weight gained. In the mean time the heat of the room and apparatus had sunk to the freezing point.

When the whole was frozen, it was carefully wiped and weighed, and found to have gained $\frac{3}{16}$ of a grain and 4 divisions of the index. Standing in the scale for about a minute, I found it began to lose weight, on which I immediately took it out, and placed it at a distance from the beam. I also immediately plunged a thermometer in the freezing mixture, and found the temperature 10 degrees; and on putting the ball of the thermometer in the hollow at the bottom of the glass vessel, it showed 12 degrees. I left the whole for half an hour, and found the thermometer, applied to the hollow of the glass, at 32°. Every thing now being at the same temperature, I weighed the glass containing the ice, after wiping it carefully, and found it had lost $\frac{1}{8}$ and 5 divisions; so that it weighed $\frac{1}{16}$, all but 1 division, more than when the water was fluid. I now melted the ice, excepting a very small quantity, and left the glass vessel exposed to the air in the temperature of 32 degrees for a quarter of an hour; the little bit of ice continued nearly the same. I now weighed it, after carefully wiping the glass, and found it heavier than the water was at first 1 division of the beam. Lastly, I took out the weights, and found the beam exactly balanced as before the experiment.

The acquisition of weight found by water being converted into ice, may arise from an increase of the attraction of gravitation of the matter of the water; or from some substance imbibed through the glass, which is necessary to render the water solid. Which of these positions is true, may be determined by forming a pendulum of water, and another of ice, of the same length, and in every other respect similar, and making them swing equal arcs. If they mark equal times, then certainly there is some matter added to the water. If the pendulum of ice is quicker in its vibrations, then the attraction of gravitation is increased. For there is no position more certain, than that a single particle of inanimate matter is perfectly incapable of putting itself in motion, or bringing itself to rest; and therefore, that a certain force applied to any mass of matter, so as to give it a certain velocity, will give half the quantity of matter double the velocity, and twice the quantity, half the velocity; and, generally, a velocity exactly in the inverse proportion to the quantity of matter. Now if there be the same quantity of matter in water as there is in ice, and if the force of gravity in water be $\frac{1}{28000}$ part less than in ice, and the pendulum of ice swing seconds, the pendulum of water will lose $\frac{1}{28000}$ of a second in each vibration, or 1 second in 28000, which is almost 3 seconds a day, a quantity easily measured.

I shall just take notice of an opinion which has been adopted by some, that there is matter absolutely light, or which repels instead of attracting other matter. I confess this appears absurd to me; but the following experiment would prove or disprove it. Supposing, for instance, that heat was a body, and absolutely light, and that ice gained weight by losing heat; then a pendulum of ice would swing through the same arc in $\frac{1}{14000}$ less time than a similar pendulum of water; for the same power would not only act on a less quantity of matter, but a counteracting force would also be taken away. I shall only observe, that heat certainly diminishes the attractions of cohesion, chemistry, magnetism, and electricity; and if it should also turn out, that it diminishes the attraction of gravitation, I should not hesitate to consider heat as the quality of diminution of attraction, which would in that case account for all its effects.

We come, in the next place, to take notice of the 2d part of the experiment, viz. that the ice gained an 8th part of a grain on being cooled to 12 degrees of Fahrenheit's thermometer. In this case, a variation may arise from the contraction of the glass vessel, and consequent increase of specific gravity in proportion to the air. But it is unnecessary to observe, that this would be so very small a quantity as not to be observable on a beam adjusted only to the degree of sensibility with which this experiment was tried. In the 2d place, the air cooled by the ice above the scale becoming heavier than the surrounding atmosphere, would press on the scale downward with the whole force of the difference. If a little more than half a pint of air was cooled over the scale to the heat of the ice and glass containing it, that is, 20° below the freezing point, the difference, according to General Roy's table, would have been the 8th part of a grain, which was the weight acquired; but the air within half an inch of the glass vessel being only 1° below the freezing point, I cannot conceive, that even an 8th part of a pint of air could be cooled over the scale to 20° below the freezing point; nor that the whole difference of the weight of the air over the scale could ever amount to the 32d of a grain. I have however contrived an apparatus which is executing, in which this cause of fallacy will be totally removed. I shall therefore rest at present the state of this part of the subject; and leave it only proved, that water gains weight on being frozen.

XXII. Sketches and Descriptions of Three Simple Instruments for Drawing Architecture and Machinery in Perspective. By Mr. Jas. Peacock. p. 366.

These machines, and descriptions of their use, appear to be too complex and operose to be employed in such practical cases of drawing.

XXIII. Experiments on Air. By H. Cavendish, Esq. F. R. S. and A. S. p. 372.

In a paper, printed in the last volume of the Philosophical Transactions, in which I gave my reasons for thinking that the diminution produced in atmospheric air by phlogistication, is not owing to the generation of fixed air, I said it seemed most likely, that the phlogistication of air by the electric spark was owing to the burning of some inflammable matter in the apparatus; and that the fixed air, supposed to be produced in that process, was only separated from that inflammable matter by the burning. At that time, having made no experiments on the subject myself, I was obliged to form my opinion from those already published; but I now find, that though I was right in supposing the phlogistication of the air does not proceed from phlogiston communicated to it by the electric spark, and that no part of the air is converted into fixed air; yet that the real cause of the diminution is very different from what I suspected, and depends on the conversion of phlogisticated air into nitrous acid.

The apparatus used in making the experiments was as follows: The air through which the spark was intended to be passed, was confined in a glass tube M, bent to an angle, as in fig. 4, pl. 1, which, after being filled with quicksilver, was inverted into two glasses of the same fluid, as in the figure. The air to be tried was then introduced by means of a small tube, such as is used for thermometers, bent in the manner represented by ABC, fig. 5, the bent end of which, after being previously filled with quicksilver, was introduced, as in the figure, under the glass DEF, inverted into water, and filled with the proper kind of air, the end C of the tube being kept stopped by the finger; then, on removing the finger from C, the quicksilver in the tube descended in the leg BC, and its place was supplied with air from the glass DEF. Having thus got the proper quantity of air into the tube ABC, it was held with the end C uppermost, and stopped with the finger; and the end A, made smaller for that purpose, being introduced into one end of the bent tube M, fig. 4, the air, on removing the finger from C, was forced into that tube by the pressure of the quicksilver in the leg BC. By these means I was enabled to introduce the exact quantity I pleased of any kind of air into the tube M; and, by the same means, I could let up any quantity of soap-lees, or any other liquor which I wanted to be in contact with the air.

In one case however, in which I wanted to introduce air into the tube many times in the same experiment, I used the apparatus represented in fig. 6, consisting of a tube AB of a small bore, a ball C, and a tube DE of a larger bore. This apparatus was first filled with quicksilver; and then the ball C and the tube AB were filled with air, by introducing the end A under a glass inverted into water, which contained the proper kind of air, and drawing out the quicksilver from the leg ED by a syphon. After being thus furnished with air, the apparatus was weighed, and the end A introduced into one end of the tube M, and kept there during the experiment; the way of forcing air out of this apparatus into the tube being by thrusting down the tube ED a wooden cylinder of such a size as almost to fill up the whole bore, and by occasionally pouring quicksilver into the same tube, to supply the place of that pushed into the ball C. After the experiment was finished, the apparatus was weighed again, which showed exactly how much air had been forced into the tube M during the whole experiment; it being equal in bulk to a quantity of quicksilver, whose weight was equal to the increase of weight of the apparatus.

The bore of the tube M used in most of the following experiments, was about $\frac{1}{10}$ of an inch; and the length of the column of air, occupying the upper part of the tube, was in general from $1\frac{1}{2}$ to $\frac{3}{4}$ of an inch. It is scarcely necessary to inform any one used to electrical experiments, that in order to force an electrical spark through the tube, it was necessary, not to make a communication between the tube and the conductor, but to place an insulated ball at such a distance from the conductor as to receive a spark from it, and to make a communication between that ball and the quicksilver in one of the glasses, while the quicksilver in the other glass communicated with the ground. I now proceed to the experiments.

When the electric spark was made to pass through common air, included between short columns of a solution of litmus, the solution acquired a red colour, and the air was diminished, conformably to what was observed by Dr. Priestley. When lime-water was used, instead of the solution of litmus, and the spark was continued till the air could be no further diminished, not the least cloud could be perceived in the lime-water; but the air was reduced to $\frac{2}{3}$ of its original bulk; which is a greater diminution than it could have suffered by mere phlogistication, as that is very little more than $\frac{1}{5}$ of the whole. The experiment was next repeated with some impure dephlogisticated air. The air was very much diminished, but without the least cloud being produced in the lime-water. Neither was any cloud produced when fixed air was let up to it; but on the further addition of a little caustic volatile alkali, a brown sediment was immediately perceived.

Hence we may conclude, that the lime-water was saturated by some acid formed during the operation; as in this case it is evident that no earth could be precipitated by the fixed air alone, but that caustic volatile alkali, on being added, would absorb the fixed air, and thus becoming mild, would immediately precipitate the earth; whereas, if the earth in the lime-water had not been saturated with an acid, it would have been precipitated by the fixed air. As to the brown colour of the sediment, it most likely proceeded from some of the quicksilver having been dissolved. It must be observed, that if any fixed air, as well as acid, had been generated in these two experiments with the lime-water, a cloud must have been at first perceived in it, though that cloud would afterwards disappear by the earth being re-dissolved by the acid; for till the acid produced was sufficient to dissolve the whole of the earth, some of the remainder would be precipitated by the fixed air; so that we may safely conclude, that no fixed air was generated in the operation.

When the air is confined by soap-lees, the diminution proceeds rather faster than when it is confined by lime-water; for which reason, as well as on account of their containing so much more alkaline matter in proportion to their bulk, soap-lees seemed better adapted for experiments designed to investigate the nature of this acid, than lime-water. I accordingly made some experiments to determine what degree of purity the air should be of, in order to be diminished most readily, and to the greatest degree; and I found that when good dephlogisticated air was used, the diminution was but small; when perfectly phlogisticated air was used, no sensible diminution took place; but when 5 parts of pure dephlogisticated air were mixed with 3 parts of common air, almost the whole of the air was made to disappear. It must be considered, that common air consists of 1 part of dephlogisticated air, mixed with 4 of phlogisticated; so that a mixture of 5 parts of pure dephlogisticated air, and 3 of common air, is the same thing as a mixture of 7 parts of dephlogisticated air with 3 of phlogisticated.

Having made these previous trials, I introduced into the tube a little soap-lees, and then let up some dephlogisticated and common air, mixed in the above-mentioned proportions, which rising to the top of the tube M, divided the soap-lees into its two legs. As fast as the air was diminished by the electric spark, I continued adding more of the same kind, till no further diminution took place: after which a little pure dephlogisticated air, and after that a little common air, were added, in order to see whether the cessation of diminution was not owing to some imperfection in the proportion of the two kinds of air to each other; but without effect.* The soap-lees being then poured out of the tube, and separated from the quicksilver, seemed to be perfectly neutralized, as they did not at all discolour paper tinged with the juice of blue flowers. Being evaporated to dryness, they left a small quantity of salt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a solution of it, burned.

For more satisfaction, I tried this experiment over again on a larger scale. About 5 times the former quantity of soap-lees were now let up into a tube of a larger bore; and a mixture of dephlogisticated and common air, in the same proportions as before, being introduced by the apparatus represented in fig. 6, the spark was continued till no more air could be made to disappear. The liquor, when poured out of the tube, smelled evidently of phlogisticated nitrous acid, and being evaporated to dryness, yielded $1\frac{4}{10}$ gr. of salt, which is pretty exactly equal in weight to the nitre which that quantity of soap-lees would have afforded if saturated with nitrous acid. This salt was found, by the manner in which paper dipped into a solution of it burned, to be true nitre. It appeared, by the test of terra ponderosa salita, to contain not more vitriolic acid than the soap-lees themselves contained, which was excessively little; and there is no reason to think that any other acid entered into it, except the nitrous.

A circumstance however occurred, which at first seemed to show that this salt contained some marine acid; namely, an evident precipitation took place when a solution of silver was added to some of it dissolved in water; though the soap-lees used in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an excess of alkali in it, some purified nitrous acid had been added to it, previous to the addition of the solution of silver. On consideration however I suspected that this precipitation might

* From what follows it appears, that the reason why the air ceased to diminish was, that as the soap-lees were then become neutralized, no alkali remained to absorb the acid formed by the operation, and in consequence scarce any air was turned into acid. The spark however was not continued long enough after the apparent cessation of diminution, to determine with certainty, whether it was only that the diminution went on remarkably slower than before, or that it was almost come to a stand, and could not have been carried much further, though I had persisted in passing the sparks.—Orig.

arise from the nitrous acid in it being phlogisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution however being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means. Hence it is certain that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think that the precipitate, which our salt occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

In the above-mentioned paper I said, that when nitre is detonated with charcoal, the acid is converted into phlogisticated air; that is, into a substance which, as far as I could perceive, possesses all the properties of the phlogisticated air of our atmosphere; from which I concluded, that phlogisticated air is nothing else than nitrous acid united to phlogiston. According to this conclusion, phlogisticated air ought to be reduced to nitrous acid by being deprived of its phlogiston. But as dephlogisticated air is only water deprived of phlogiston, it is plain, that adding dephlogisticated air to a body, is equivalent to depriving it of phlogiston, and adding water to it; and therefore phlogisticated air ought also to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with dephlogisticated air; only the acid formed this way will be more dilute, than if the phlogisticated air was simply deprived of phlogiston.

This being premised, we may safely conclude, that in the present experiments the phlogisticated air was enabled, by means of the electrical spark, to unite to, or form a chemical combination with the dephlogisticated air, and was thus reduced to nitrous acid, which united to the soap-lees, and formed a solution of nitre; for in these experiments those two airs actually disappeared, and nitrous acid was actually formed in their stead; and as moreover it has also been just shown, from other circumstances, that phlogisticated air must form nitrous acid, when combined with dephlogisticated air, the above-mentioned opinion seems to be sufficiently established. A further confirmation of it is that, as far as I can perceive, no diminution of air is produced when the electric spark is passed either through pure dephlogisticated air, or through perfectly phlogisticated air; which indicates the necessity of a combination of these two airs to produce the acid. It was also found in the last experiment, that the quantity of nitre procured was the same that the soap-lees would have produced if saturated with nitrous acid; which shows that the production of the nitre was not owing to any decomposition of the soap-lees. It may be worth remarking, that whereas in the detonation of nitre with inflammable substances, the acid unites to phlogiston, and forms phlogisticated air, in these experiments the reverse of this process was carried on; namely, the phlogisticated air united to the dephlogisticated, which is equivalent to being deprived of its phlogiston, and was reduced to nitrous acid.

In the above-mentioned paper I also gave my reasons for thinking that the small quantity of nitrous acid, produced by the explosion of dephlogisticated and inflammable air, proceeded from a portion of phlogisticated air mixed with the dephlogisticated, which I supposed was deprived of its phlogiston, and turned into nitrous acid, by the action of the dephlogisticated air on it, assisted by the heat of the explosion. This opinion, as must appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident that dephlogisticated air is able to deprive phlogisticated air of its phlogiston, and reduce it into acid, when assisted by the electric spark; and therefore it is not extraordinary that it should do so when assisted by the heat of the explosion.

The soap-lees used in the foregoing experiments were made from salt of tartar, prepared without nitre; and were of such a strength as to yield $\frac{1}{10}$ of their weight of nitre when saturated with nitrous acid. The dephlogisticated air also was prepared without nitre, that used in the first experiment with the soap-lees being procured from the black powder formed by the agitation of quicksilver mixed with lead,* and that used in the latter from turbith mineral. In the first experiment, the quantity of soap-lees used was 35 measures, each of which was equal in bulk to 1 grain of quicksilver; and that of the air absorbed was 416 such measures of phlogisticated air, and 914 of dephlogisticated. In the 2d experiment, 178 measures of soap-lees were used, and they absorbed 1920 of phlogisticated air, and 4860 of dephlogisticated. It must be observed however, that in both experiments some air remained in the tube uncondensed, whose degree of purity I had no way of trying; so that the proportion of each species of air absorbed is not known with much exactness.

As far as the experiments hitherto published extend, we scarcely know more of

* This air was as pure as any that can be procured by most processes. I propose giving an account of the experiment, in which it was prepared, in a future paper.—Orig.

the nature of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalis, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air; so that, though the nitrous acid, by being united to phlogiston, is converted into air possessed of these properties, and consequently, though it was reasonable to suppose that part at least of the phlogisticated air of the atmosphere consists of this acid united to phlogiston, yet it might fairly be doubted whether the whole is of this kind, or whether there are not in reality many different substances confounded together by us under the name of phlogisticated air. I therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet, as some remained unabsorbed, it did not appear for certain whether that was of the same nature as the rest or not. For this purpose I diminished a similar mixture of dephlogisticated and common air, in the same manner as before, till it was reduced to a small part of its original bulk. I then, in order to decompound as much as I could of the phlogisticated air which remained in the tube, added some dephlogisticated air to it, and continued the spark till no further diminution took place. Having by these means condensed as much as I could of the phlogisticated air, I let up some solution of liver of sulphur to absorb the dephlogisticated air; after which only a small bubbl of air remained unabsorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogisticated air let up into the tube; so that if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may safely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

The foregoing experiments show that the chief cause of the diminution which common air, or a mixture of common and dephlogisticated air, suffers by the electric spark, is the conversion of the air into nitrous acid; but yet it seemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, some of this matter might be burnt by the spark, and thereby diminish the air, as I supposed in the above-mentioned paper to be the case. The best way which occurred to me of discovering whether this happened or not, was to pass the spark through dephlogisticated air, included between different liquors; for then, if the diminution proceeded solely from the conversion of air into nitrous acid, it is plain that, when the dephlogisticated air was perfectly pure, no diminution would take place; but when it contained any phlogisticated air, all this phlogisticated air, joined to as much of the dephlogisticated air as must unite to it in order to reduce it into acid, that is, 2 or 3 times its bulk, would disappear, and no more; so that the whole diminution could not exceed 3 or 4 times the bulk of the phlogisticated air: whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogisticated air was, the greater and quicker would be the diminution. The result of the experiments was, that when dephlogisticated air, containing only $\frac{1}{20}$ of its bulk of phlogisticated air, that being the purest air I then had, was confined between short columns of soap-lees, and the spark passed through it till no further diminution could be perceived, the air lost $\frac{43}{200}$ of its bulk; which is not a greater diminution than might very likely proceed from the first-mentioned cause; as the dephlogisticated air might easily be mixed with a little common air while introducing into the tube.

When the same dephlogisticated air was confined between columns of distilled water, the diminution was rather greater than before, and a white powder was formed on the surface of the quicksilver beneath; the reason of which probably was, that the acid produced in the operation corroded the quicksilver, and formed the white powder; and that the nitrous air, produced by that corrosion, united to the dephlogisticated air, and caused a greater diminution than would otherwise have taken place. When a solution of litmus was used, instead of distilled water, the solution soon acquired a red colour, which became paler and paler as the spark was continued, till at last it was quite colourless and transparent. The air was diminished by almost half, and I believe might have been still further diminished had the spark been continued. When lime-water was let up into the tube, a cloud was formed, and the air was further diminished by about $\frac{1}{5}$. The remaining air was good dephlogisticated air. In this experiment therefore the litmus was, if not burnt, at least decompounded, so as to lose entirely its purple colour, and to yield fixed air; so that, though soap-lees cannot be decompounded by this process, yet the solution of litmus can, and so very likely might the solutions of many other combustible substances. But there is nothing, in any of these experiments, which favours the opinion of the air being at all diminished by means of phlogiston communicated to it by the electric spark.

XXIV. Account of the Measurement of a Base on Hounslow-heath. By Major General William Roy, F.R.S., and A.S. p. 385.

The rise and progress of the rebellion which broke out in the Highlands of Scotland in 1745, gave occasion to commence government surveys in that part of the island. These were conducted by Lieut. Gen. Watson, a military engineer, and chiefly under him executed by our author, then a subaltern officer in the same corps. Though this work, which is still in manuscript, says Gen. R., and in an unfinished state, possesses considerable merit, and perfectly answered the purpose for which it was originally intended; yet having been carried on with instruments of the common, or even inferior kind, and the sum annually allowed for it being inadequate to the execution of so great a design in the best manner, it is rather to be considered as a magnificent military sketch, than a very accurate map of a country. It would however have been completed, and doubtless many of its imperfections remedied; but the breaking out of the war of 1755 prevented both, by furnishing service of other kinds for those who had been employed on it. On the conclusion of the peace of 1763, it came for the first time under the consideration of government, to make a general survey of the whole island at the public cost. Towards the execution of this work, the direction of which was to have been committed to our author, the map of Scotland was to have been made subservient, by extending the great triangles quite to the northern extremity of the island, and filling them in from the original map. Thus that imperfect work would have been effectually completed, and the nation would have reaped the benefit of what had been already done, at a very moderate extra-expence.

Certain causes however prevented any progress being made in the work for 12 years longer, previous to the nation's being unfortunately involved in the American war; it was therefore obvious that peace must be once more restored before any new effort could be made for that purpose. The peace of 1783 being concluded, and official business having detained Gen. R. in or near town during the whole of that summer, he embraced the opportunity, for his own private amusement, to measure a base of 7744.3 feet, across the fields between the Jew's Harp, near Marybone, and Black-lane, near Pancras; as a foundation for a series of triangles, carried on at the same time, for determining the relative situations of the most remarkable steeples, and other places, in and about the capital, with regard to each other, and the Royal Observatory at Greenwich. The principal object he had here in view (besides that it might possibly serve as a hint to the public, for the revival of the now almost forgotten scheme of 1763) was, to facilitate the comparison of the observations, made by the lovers of astronomy, within the limits of the projected survey; namely, Richmond and Harrow on the west; and Shooter's-hill and Wansted on the east; when very unexpectedly he found that an operation of the same nature, but much more important in its object, was really in agitation.

In the beginning of October, 1783, Comte D'Adhemar, the French ambassador, transmitted to Mr. Fox, then one of his Majesty's principal secretaries of state, a memoir of M. Cassini de Thury, in which he set forth the great advantage that would accrue to astronomy, by carrying a series of triangles from the neighbourhood of London to Dover, there to be connected with those already executed in France, by which combined operations the relative situations of the two most famous observatories in Europe, Greenwich and Paris, would be more accurately ascertained than they are at present. The execution of this business was confided to the care and diligence of Gen. R., which naturally divides into 2 parts. First, the choice and measurement of the base, with every possible care and attention, as the foundation of the work; 2dly, the disposition of the triangles, by which the base is to be connected with such parts of the coast of this island as are nearest to the coast of France, and the determination of their angles, by means of the best instrument that can be obtained for the purpose, from which the result or conclusion will be drawn.

With regard to the first of these, the choice of the base, it is observed, that Hounslow-heath having always appeared to be one of the most eligible situations for any general purpose of the sort now under consideration, because of its vicinity to the capital and Royal Observatory at Greenwich, its great extent, and the extraordinary levelness of its surface, without any local obstructions whatever to render the measurement difficult; being likewise commodiously situated for any future operations of a similar nature; accordingly a particular inspection of the heath was made, to assign and trace out a proper position for the purpose. Gen. R. then minutely describes the clearing of the ground, and the construction of the steel chain and other instruments employed in the measurement of the base, which must be performed with the utmost care.

After the description of the chain, which consisted of 100 links of 1 foot each, the measuring rods are next described. The bases which had hitherto been measured in different countries, with the greatest appearance of care and exactness, had all, or for the most part, been done with deal rods of one kind or other, whose lengths being originally ascertained by means of some metal standard, were, in the subsequent applications of them, corrected by the same standard. Having thus had so many precedents, serving as examples to guide them in their choice, it was natural enough that they should pursue the same method in the measurement to be executed on Hounslow-heath; taking however all imaginable care, that the rods should be made of the very best materials that could be procured; with this further precaution, that by trussing them, they should be rendered perfectly inflexible, a circumstance not before attended to. Three measuring rods were accordingly ordered to be made, and also a standard rod, with which the former were from time to time to be compared. Their stems were each 20 feet 3 inches in length, reckoning from the extremities of the bell-metal tippings; very near 2 inches deep; and about $1\frac{1}{4}$ inch broad. Being trussed laterally and vertically, they were thus rendered perfectly, or at least as to sense, inflexible.

Next follows a description of the brass standard scales employed in setting off the length of the deal rods. At the sale of the instruments of the late ingenious optician Mr. James Short, Gen. R. purchased a finely divided brass scale, of the length of 42 inches, with a Vernier's division of 100 at one end, and one of 50 at the other, by which the 1000th part of an inch is very perceptible. It was originally the property of the late Mr. Graham, the celebrated watch-maker; has the name of Jonathan Sisson engraved on it; but is known to have been divided by the late Mr. Bird, who then worked with Sisson. The brass standard scale of the R. S. about 42 inches long, which contains on it the length of the standard yard from the Tower, that from the Exchequer, and also the French half-toise, together with the duplicate of the said scale, sent to Paris for the use of the Royal Academy of Sciences, were both made by Mr. Jonathan Sisson, under Mr. Graham's immediate direction. Now, though there seemed to be every reason to suppose that the scale above-mentioned, originally Mr. Graham's property, would correspond with those above-mentioned, which he had been directed by the R. S., with so much care and pains, to provide; yet, that nothing of this sort might remain doubtful, it was judged right, that the two scales should be actually compared. Accordingly the extent of 3 feet, being carefully taken from the Society's standard, and applied to Gen. R.'s scale, it was found to reach exactly to 36 inches, the temperature being 65°. In like manner, the beam compasses being applied to the length of the Exchequer yard, the extent was now found by the micrometer to over-reach that yard by $\frac{69}{10000}$, or nearly $\frac{7}{1000}$ parts of an inch. Having thus shown that his scale was accurately of the same length with the Society's standard, he next points out the use that was made of it, for ascertaining the lengths of the deal rods intended for the operation on Hounslow-heath; which was, by the assistance of Mr. Ramsden, to measure and set off the true length of those rods. Other preparatory matters are then described, such as, the stands for the measuring rods, the boning telescope and rods, the cup and tripod for preserving the point measured to during each night, marks for terminating in a permanent manner the extremities of the base, clearing the ground, &c.

Next, about the middle of June 1784, we enter on the sought measurement of the base with the chain, and determination of the relative heights of the stations by means of the telescopic spirit level, a business which was completed the 22d of the same month, extending from the south-east extremity near Hampton Poor-house, to the north-west extremity near the Magpye public-house at King's Arbour. This measurement gave 19 hypothenusal distances of 600 feet each, and one of 404.55, making in the whole 11804.55 feet, the mean temperature being $62\frac{1}{2}$°. Now, when the length of the chain, in its original state, was ascertained by the dots on the brass pins in the New-England plank, it was found, in the then temperature of 74°, to exceed the 100 feet by near $\frac{1}{4}$ of an inch, or 0.245 inch. Therefore in the temperature of 63°, being that in which the lengths of the deal rods were laid off, and differing very little from what was likewise the mean heat of the air, when applied on the heath, the chain, according to the experiments on the expansion of the very same steel, would exceed the 100 feet by 0.161 inch, or 0.0134 foot. Hence the sum of the 3 sections of the base, 274 chains, being multiplied by 0.0134 foot, we shall have 3.67 feet for the equation of the chain, + 4.55 feet, to be added to its length, which will then become 27408.22 feet from the centre of one pipe to the centre of the other: and this would have been the true length of the base, as given by the rough measurement with the chain, if the surface had been one uniform inclined plane throughout its whole extent. But though the ascent of Hounslow-heath is so small, and so gradual, as to occasion little more than half an inch of reduction, from the 46 hypothenusal to the 46 base distances, into which it is divided; yet each of these hypothenuses containing again many other small irregularities, all of which affect the measurement by the chain, in proportion to their number and height, in every space of 600 feet, their united effects, including the lateral deviations from the true line in measuring, do somewhat more than compensate for the extra-length of the chain, as will be seen hereafter in comparing the length of the base just now obtained with that given by the rods.

Next succeeds the measurement of the base with the deal rods, which amounts to 27404.31 feet, being the total length as given by these rods, without regard to expansion and reduction of the hypothenusal lines, the former of which was found to be very frequent and considerable, from the various degrees of moisture, contrary to what had heretofore been thought to be the case. By examining the numerous observations on this head, it appears that the total expansion on the 1370 deal rods, including the small equation for the lengthening of the standard, amounts to 24.223 inches, or 2.02 feet; which being added to the apparent length of the base 27404.31 feet formerly obtained, we have, for the hypothenusal length, 27406.33 feet: and from this deducting 0.07 foot, the excess of the hypothenusal above the base line, there remains 27406.26 for the distance given, by the deal rods, between the centres of the pipes terminating the base, reduced to the level of the lowest, or that at Hampton Poor-house, in the temperature of 63°, being that of the brass scale when the lengths of the deal rods were laid off. All this however supposes 3 things to be absolutely certain: 1st, that the expansion of the rods has been accurately estimated; 2dly, that no error has arisen from the butting of the rods against each other, in order to bring them into contact; and, 3dly, that no mistake of any kind has been committed in the execution. When we come to give the true length of the base, as ultimately ascertained by means of the glass rods, it will appear, that one or more of these 3 have actually taken place; though it is most probable, that only the first 2 sources of error have contributed their share to the total difference between the two results. It is further remarked, that the last week of July was so wet as to occasion a total suspension of the operations on Hounslow-heath. On the 26th of that month, at 8^h A. M. the temperature being then 63°, the rods were compared with the standard, and found to exceed it, at a medium, $\frac{1}{15}$ part of an inch. Now, if we suppose the whole base to have been measured with the rods in that state, the difference would have amounted to more than $7\frac{1}{3}$ feet, exclusive of what the standard itself might have altered from its original length. Another comparison was made at Spring-grove, in the beginning of September, by which it was found that the dew imbibed only in one night, or a space of time not exceeding 14 hours, occasioned such an expansion in the deal rods, as in the whole base would have amounted to 45.484 inches. It is sufficiently obvious, that this last mentioned experiment was more accurate, in the proportion of about 15 to 1, than any comparison we could at that time have made with the standard. But since immediately after it was finished, the sun shone out very bright, it is by no means certain how soon the rods would again have contracted to their former length, or near it, had they been exposed to his rays. Repeated comparisons for ascertaining facts of this sort, at very short interims, are absolutely incompatible with the nature of such tedious and troublesome operations as the measurement of long bases; and here indeed lies the great objection to the use of deal rods, that at no time can we be certain how soon, after a comparison has been made, they may alter their length in a proportion, and sometimes too even in a sense, different from what was expected.

We then arrive at the description of the glass rods, or rather tubes, ultimately used to determine the length of the base. These were finally resorted to, as less heavy and variable in their length, than metal rods, and not at all varying by humour like the deal rods. Notwithstanding their great length, above 20 feet, and weighing about 61 lbs. each, they were found to be so straight that, when laid on a table, the eye, placed at one end looking through them, could see any small object in the axis of the bore at the other end. After a minute description of the manner of preparing and fitting up these rods, we next arrive at their application in the actual measurement of the base line. This was commenced on the 18th of August, both with the steel chain and the glass rods, for a mutual comparison and check on each other. In this manner they proceeded, and in the course of the day were only able to measure the length of 10 chains, or 1000 feet. Being arrived at this point it was found, that the fine line on the brass slide, marking the extremity of the 10th chain, fell short of another fine line on the same slide, denoting the end of the 50th glass rod, just $\frac{2}{10}$ of an inch. Now it appears by the experiments with the pyrometer, what the real contractions of the chain and glass rods were, for the degrees of difference of temperature * below that in which their respective lengths were laid off, that this small apparent difference of $\frac{2}{10}$ of an inch, between the two modes of measuring the 1000 feet, should have been 0.17938 inches, to have made the two results exactly agree, which is a real difference of only 0.02062 of an inch. Supposing then every 1000 feet of the base to have been measured by the chain with the same attention, and consequently with the same, or nearly the same success, we shall have 27.404 × 0.02062 in. = 0.565 in. or a defect of something more than half an inch only on the whole length of the base.

So nice an agreement between two results, with instruments so very different, could not fail to be considered as astonishing; and as it rarely happens, that the graduation of thermometers will so nearly correspond with each other, as not to occasion a much greater error, all were very desirous that it could have been further confirmed by continuing the operation in the same way through a more considerable proportion of the whole length. But besides the tedious nature of the double measurement, owing to the multiplicity of stands, platforms, coffers, and other articles, that were now successively to be moved forward; the operation had already trained out to a much more considerable length than had been expected; the summer was now far advanced, and the continuance of good weather uncertain; in short, all these reasons contributed to induce them to give up, for the present, any further experiment with the chain, and to proceed with the glass rods alone in the completion of the measurement. Accordingly, on Aug. 19, the operation with the glass rods was continued for the other hypothenuses, and thus continued from day to day till the 30th of the same month, when the measurement was finally completed; when the extremity of the 1370th rod over-shot the centre of the pipe terminating the base towards the south-east by 17.875 inches, or 1.49 foot. Hence, when the several equations for expansions are respectively taken into the account, we find, that the alteration of the

* When the length of the chain was laid off, the heat was 66½°, and that of the glass rods 68°. They will therefore only agree with each other accurately in these respective temperatures. The mean of 20 thermometers for the 4 chain lengths of the 46th hypothenuse gave a heat of 61°.6; and for the 6 chain lengths of the 45th, the mean of 30 thermometers gave 59°.75. The temperature of the 400 feet of glass by the mean of 40 thermometers was 65°.3; and of the 600 feet, by the mean of 60 thermometers, it was 60°.8. Now, from these data, and the expansions of steel and glass, as determined by the pyrometer, the computation will stand as follows:

		°	°	°	In.	In.		In.	
Steel	400	66.5	− 61.6	= 4.9	× 0.03052	= 0.14955	}	= 0.45856	contract. of 1000 feet.
	600	66.5	− 59.75	= 6.75	× 0.04578	= 0.30901			
Glass	400	68.0	− 65.3	= 2.7	× 0.02068	= 0.05584	}	= 0.27918	contract. of 1000 feet.
	600	68.0	− 60.8	= 7.2	× 0.03102	= 0.22334			

The 1000 feet of steel should have contratced more than the 1000 feet of glass .... = 0.17938
But the difference was found to be.... .... = 0.20000
Therefore the error of the chain in defect was .... 0.02062 × 27.404 = 0.565 in. or little more than half an inch on the whole base.—Orig.

deal rods from the humidity of the air, which, by comparison with the standard, was apparently most considerable in the 1st and 2d sections of the base, has now wholly vanished; that is to say, the total amount of it has been over-rated by 20.964 inches.

After this follows a description of the microscopic pyrometer, invented by Mr. Ramsden, and employed for determining by experiment the expansion of the chief instruments concerned in the measurement of the base, viz. the standard scale, the steel chain, and the glass rods, with experiments on the same. After which the ultimate determination of the length of the base, by bringing to account the several particulars, is given as follows:

	feet.
The hypothenusal length of the base, as measured by 1369.925521 glass rods of 20 feet each + 4.31 feet, being the distance between the last rod and the centre of the north-west pipe, was........	27402.8204
The reduction of the hypothenuses to the horizontals	0.0714
Hence the apparent length of the base, reduced to the level of the south-east extremity, becomes	27402.7490
The apparent length is to be augmented by the excess of the expansion above the contraction of the glass rods, = 4.1867 inches, reduced to the heat of 62°, as has been usually done in former operations of this nature	0.3489
The apparent length is further to be augmented by the equation for 6° difference of temperature of the standard brass scale* between 62° and 68°, this last being the heat in which the lengths of the glass rods were laid off = 20.3352 inches, as deduced from the experiments with the pyrometer............	*1.6946
Hence we have the correct length of the base in the temperature of 62° reduced to the level of the lowermost extremity near Hampton Poor-house	27404.7925
This last length requires yet a small reduction for the height of this lowermost end above the mean level of the sea, supposed to be 54 feet, or 9 fathoms	0.0706

* There occurs a small oversight in this article, as was remarked in the Philos. Trans. of 1795, being an account of another measurement of the same base by Col. Williams, Capt. Mudge, and Mr. Dalby. By which it appears that the equation for 6° difference of temperature above-mentioned, should consist of the difference between the numbers for brass and glass, and not of that for brass alone; that is, it should be $6° \times (3.38938 - 1.41658) = 11.8368$ inches $= .9864$ feet, instead of 1.6946 above employed, which made the base come out too much by 0.7082. This being deducted from 27404.7219 the ultimate number above found, leaves 27404.0843, for what should be Gen. Roy's length of the base as measured with the glass rods; being only about $2\frac{3}{4}$ inches less than it was made by the other measures.

Hence the true or ultimate length of the base, reduced to the level of the sea, and making a portion of the mean circumference of the earth, becomes 27404.7219

XXV. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, 1784. *By Thomas Barker, Esq. Also of the Rain at South Lambeth, Surrey; and at Selbourn and Fyfield, Hampshire.* p. 481.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.						
		Highest	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	S. Lamb. Surry.	Selbourn Hamp.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29.96	28.49	29.34	40½	28	33½	40	15½	27	1.877	2.54	3.18	2.44
	Aftern.				42½	29	34	48½	24½	32½				
Feb.	Morn.	30.00	28.50	29.23	47	30	36	45	9	29	1.225	1.49	0.77	1. 7
	Aftern.				47½	31	37	52½	23	36				
Mar.	Morn.	29.63	28.59	29.23	47½	36	39½	45	21	32½	1.096	2.63	3.82	2.24
	Aftern.				48½	36	40½	52	38½	40½				
Apr.	Morn.	29.74	28.44	29.26	50½	35½	43½	51	29	38½	1.741	2.56	3.92	2.10
	Aftern.				53	36	45	60½	35	48				
May	Morn.	29.92	29.17	29.62	66½	47	57½	63	41	52	2.890	1.36	1.52	1.57
	Aftern.				69½	48	59½	78	48½	65				
June	Morn.	29.92	28.98	29.43	62	55½	58	61½	48	54	3.810	3.45	3.65	2.45
	Aftern.				63½	56½	59½	71	53	63½				
July	Morn.	29.85	28.74	29.48	69	56	61	66	51	56	5.080	2.26	2.40	2.80
	Aftern.				72	57	63	79½	57½	67				
Aug.	Morn.	29.92	29.04	29.56	65	54	59	60½	42½	52	2.814	2.84	3.88	2.79
	Aftern.				67	55	60	71½	51	63				
Sept.	Morn.	29.90	29.01	29.55	66½	53	61	57	39	54	1.740	1.65	2.51	2. 7
	Aftern.				71½	54	63	73½	51½	64				
Oct.	Morn.	30.00	28.98	29.62	52½	42	48½	45	27½	39½	0.223	0.83	0.39	0.17
	Aftern.				53½	43	49½	55½	40	49				
Nov.	Morn.	29.85	28.75	29.38	51	39½	45	51½	23½	38	2.376	5.60	4.70	3.14
	Aftern.				51	41	45½	53	33½	44				
Dec.	Morn.	29.75	28.15	29.26	43½	31½	36½	41	13½	29	2.335		3.06	1.72
	Aftern.				43	32½	37½	46½	19	32½				
Mean of all				29.41		49				46	27.207	27.21	33.80	24.56

END OF VOL. SEVENTY-FIVE OF THE ORIGINAL.

I. Observations on the Graduation of Astronomical Instruments; with an Explanation of the Method invented by the late Mr. Henry Hindley, of York, Clockmaker, to divide Circles into any given Number of Parts. By Mr. John Smeaton, F. R. S. Anno 1786, *Vol. LXXVI.* p. 1.

Perhaps no part of the science of mechanics has been cultivated by the ingenious with more assiduity, or more deservedly so, than the art of dividing circles for the purpose of astronomy and navigation. It is said, that Tycho Brahe and Hevelius laboured this part of their instruments with their own hands. Dr.

Hook, in his animadversions on the Machina Cœlestis of Hevelius, published in the year 1674, has given an elaborate description of a quadrant, whose divisions were formed, and afterwards read off, by means of an endless screw, working on the outermost border of the limb of a quadrant; which, he says, "does not at all depend on the care and diligence of the instrument-maker, in dividing, graving, or numbering the divisions, for the same screw makes it from end to end;" yet he has given us no account of any particular care or caution that he used, in preventing the same screw from making larger or smaller paces, in consequence of unequal resistance, from a different hardness of the metal in different parts of the limb; nor any method of correcting or checking the same; nor of making a screw, the angle of whose threads with the axis shall be equal in every part of the circumference; therefore the whole of this business (in which accurate mechanists well know consists the whole of the difficulty) he refers to the ingenious workman; and in particular to the then celebrated Mr. Tompion, who he says was employed by him to make his instrument, and who had thereby "seen and experienced the difficulties that do occur therein:" but was any ingenious workman now to pursue the directions of Dr. Hook, so far as his communication extends, we may conclude that he would make a very inaccurate piece of work, far inferior in performance to what the doctor seems to expect from it. But yet I believe it was the first attempt to apply the endless screw and wheel, or arch, to the purpose of forming divisions for astronomical instruments; for the doctor says himself that the perfection of this instrument is the way of making the divisions; that it "excels all the common ways of division:" and in the table of contents it is entitled, "An Explication of the new Way of dividing."

This method however, of Dr. Hook's, was not laid aside without a very full and sufficient trial: for Mr. Flamsteed, in the Prolegomena of the 3d volume of Historia Cœlestis, informs us, that he contrived the sextant, with which his observations were chiefly made, from his entrance into the Royal Observatory in the year 1676, to the year 1689. This sextant was first made of wood, and afterwards of iron, with a brass limb of 2 inches broad, by Mr. Tompion, at the expense of Sir Jonas Moore; its radius was 6 feet $9\frac{1}{4}$ inches; it was furnished with an endless screw on its limb of 17 threads in an inch, and with telescopic sights. Of this instrument Mr. Flamsteed gives the figure at the latter end of his Prolegomena before-mentioned, sufficiently large to see the general design; the whole being mounted on a strong polar axis of iron, of 3 inches diameter.

Though, in the full description of this instrument, Mr. Flamsteed mentioned the limb's being furnished with diagonal divisions, distinguishing the arch to 10 seconds; yet it is pretty clear, that it had not these originally on it; but that the dependance was wholly on the screw divisions, when it came out of Mr. Tompion's hands. This one may reasonably infer from the observations themselves; for the first observation, set down as taken with this instrument, being on the 29th of October, 1676, it was not till the 11th of September, 1677, that the column which contained the check angle by diagonal lines was filled up; and there was also a space of time, antecedent to that last-mentioned, wherein no observations are recorded as taken with this instrument, in which time the diagonal divisions might be put on; and this will be put beyond a doubt, as he says expressly, that finding, in the year 1677, that the threads of the screw had worn the border of the limb, he divided the limb into degrees himself, and drew a set of diagonal divisions; and then comparing the two sets of divisions together, he sometimes found them to differ a whole minute; therefore, for correction thereof, he constructed a new table for converting the revolutions and parts of the screw into degrees, minutes, and seconds; and which he applied in the observations taken in 1678. However, notwithstanding this correction, in looking over the observations noted down as deduced each way, I often find a difference of half a minute; not unfrequently 40″; but in an observation of the moon, of the 9th June, 1687, I find a difference of 55″, which on a radius of 6 feet 9 inches amounts to more than $\frac{1}{50}$ part of an inch.

In the year 1689, Mr. Flamsteed completed his mural arc at Greenwich; and in the Prolegomena before mentioned, he makes an ample acknowledgement of the particular assistance, care, and industry of Mr. Abraham Sharp; whom, in the month of August, 1688, he brought into the observatory, as his amanuensis; and being, as Mr. Flamsteed tells us, not only a very skilful mathematician, but exceedingly expert in mechanical operations, he was principally employed in the construction of the mural arc; which in the compass of 14 months he finished, so greatly to the satisfaction of Mr. Flamsteed, that he speaks of him in the highest terms of praise. This celebrated instrument, of which he also gives the figure at the end of the Prolegomena, was of the radius of 6 feet $7\frac{1}{2}$ inches; and, in like manner as the sextant was furnished both with screw and diagonal divisions, all performed by the accurate hand of Mr. Sharp. But yet, whoever compares the different parts of the table for conversion of the revolutions and parts of the screw belonging to the mural arc into degrees, minutes, and seconds, with each other, at the same distance from the zenith on different sides; and with their halves, quarters, &c. will find as notable a disagreement of the screw-work from the hand-divisions, as had appeared before in the work of Mr. Tompion: and hence we may conclude, that the method of Dr. Hook, being executed by two such masterly hands as Tompion and Sharp, and found defective, is in reality not to be depended on in nice matters. From the account of Mr. Flamsteed it appears also, that Mr. Sharp obtained the zenith point of the instrument, or line of collimation, by observation of the zenith stars, with the face of the instrument on the east and on the west side of the wall: and that having made the index stronger (to prevent flexure) than that of the sextant, and thereby heavier, he contrived, by means of pullies and balancing weights, to relieve the hand that was to move it from a great part of its gravity.

I have been the more particular relating to Mr. Sharp, in the business of constructing this mural arc; not only because we may suppose it the first good and valid instrument of the kind, but because I consider Mr. Sharp as the first person who cut accurate and delicate divisions on astronomical instruments; of which, independent of Mr. Flamsteed's testimony, there still remain considerable proofs: for, after leaving Mr. Flamsteed, and quitting the department abovementioned;* he retired into Yorkshire, to the village of Little Horton, near Bradford, where he ended his days about the year 1743; and where I have seen not only a large and very fine collection of mechanical tools (the principal ones being made with his own hands,) but also a great variety of scales and instruments made with them, both in wood and brass, the divisions of which were so exquisite, as would not discredit the first artists of the present times: and I believe there is now remaining a quadrant, of 4 or 5 feet radius, framed of wood, but the limb covered with a brass plate; the subdivisions being done by diagonals, the lines of which are as finely cut as those on the quadrants at Greenwich. The delicacy of Mr. Sharp's hand will indeed permanently appear from the copper-plates in a quarto book, published in the year 1718, intitled, "Geometry improved by A. Sharp, Philomath," of which not only the geometrical lines on the plates, but the whole of the engraving of letters and figures, were done by himself, as I was told by a person in the mathematical line, who very frequently attended Mr. Sharp in the latter part of his life. I therefore consider Mr. Sharp as the first person that brought the affair of hand division to any degree of perfection.

Some time about the establishment of the mural arc at Greenwich, the celebrated Danish astronomer Olaus Roemer began his domestic Observatory, which he finished in the year 1715, as we are informed by his historian Peter Horrebow, in the 3d volume of his works, in the tract, intitled, Basis Astronomiæ, published in the year 1741. In this tract is the description of an instrument, which not only answered the purpose of the meridian arc; but, its telescope being mounted on a long axis, became also in reality what we now call a Transit Instrument; and which furnished, so far as I have been able to learn, the first idea of it. One end of the axis of this instrument being the centre of the

* Mr. Sharp continued in strict correspondence with Mr. Flamsteed so long as he lived, as appeared by letters of Mr. Flamsteed's found after Mr. Sharp's death; many of which I have seen.—Orig.

meridian arc, and carrying its index, M. Roemer thus avoided the errors arising from the plane of the mural arc not being accurately a vertical plane; and which Mr. Flamsteed endeavoured to check, by observing the passage of known stars nearly in the same parallel of declination; that is, passing nearly over the same part of the plane of the arc; by which he was enabled to correct or check the errors of the arc in right ascension. But it is the peculiar method in which Roemer divided his instruments, that occasions him here to be introduced.

Though it is a very simple problem by which geometricians teach how to divide a given right line into any number of parts required; yet it is still a much more simple thing to set off on a given right line, from a point given, any number of equal parts required, where the total length is not exactly limited; for this amounts to nothing more than assuming a convenient opening of the compasses, and beginning at the point given, to set off the opening of the compasses as many times in succession, as there are equal parts required; which process is as applicable to the arch of a circle as it is to a right line. Of this simple principle Roemer endeavoured to avail himself. For this purpose he took 2 stiff, but very fine-pointed pieces of steel, and fixed them together, so as to avoid, as much as possible, every degree of spring that would necessarily attend long-legged compasses, or even those of the shortest and stiffest kind when the points are brought near together. The distance of the points that he chose was about the $\frac{1}{10}$ or $\frac{1}{12}$ of an inch. This, on a radius of $2\frac{1}{2}$ or 3 feet, would be about 10 minutes. With this opening, beginning at the point given, he set off equal spaces in succession to the end of his arch, which was about 75°. Those were distinguished on the limb of the instrument by very fine points, which were referred to by a grosser division, the whole being properly numbered. The subdivision of those arches of 10 minutes each was performed by a double microscope, carried on the arm or radius of the instrument, the common focus being furnished with parallel threads of single silk, of which 11 being disposed at 10 equal intervals, comprehending together one 10′ division, the distance of the nearest threads became a very visible space, answerable to 1′ each, and therefore capable of a much further subdivision by estimation. The divisions of this instrument were therefore, properly speaking, not degrees and minutes; but yet, if exactly equal, would serve the purpose as well, when their true value was found, which was done by comparison with larger instruments.

Now, if it be considered, that in going step by step of 10′ each, through a space of 75° there will be a succession of 450 divisions, dependant on each other; if it be also considered, that the least degree of extuberance in the surface of the metal, where each new point is set down, or the least hard particle (with which all the base metals seem to abound) will cause a deviation in the first impression of a taper point, and so produce an inequality in the division; it is evident that though this inequality may be very small, and even imperceptible between neighbouring divisions, yet among distant ones, it may and will arise to something considerable; which, in the mensuration of angles, will have the same ill tendency as near ones. Now, as M. Roemer has given no means of checking the distant divisions, in respect of each other, it is very probable that no one has followed his steps, in cases where great accuracy was required, in a considerable number of divisions. For in reality this method is likely to fall far short of Dr. Hook's; as Dr. Hook's divisions being cut in a similar successive manner, by the rotation of the sharp edge of the threads of a screw against the exterior edge of the limb of the instrument, a very slight degree of pressure will bring a fine screw of 30 threads in an inch, which he prescribes, to touch against an arch whose radius is 4 or 5 feet in more than 1, 2, 3, or 4 threads at once; so that the threads supporting each other, a small extuberance, or even a small hard particle in the metal, will be cut through or removed by the grinding or rather sawing motion of the screw; and which, in regard to its contact, being in reality an edge, will be much more effectual, that is, more firm, in its retention, than a mere simple point: and a repetition of the operation, from the support of the threads to each other, will tend to mend the first traces; whereas, in Roemer's way, a repetition will make them worse; for whatever drove forward or backward the point on first entering, will, from the sloping of the point, be confirmed and increased in driving it deeper.

When Dr. Halley was chosen Astronomer Royal, Mr. Flamsteed's instruments being taken away by his executors, Mr. Graham undertook to make a new mural quadrant, about the year 1725; who, uniting all that appeared valuable in the different methods of his predecessors, executed it with a degree of contrivance, accuracy, and precision, before unknown: and he performed the division with his own hand. The model of this quadrant, for strength, easy management, and convenience, has been ever since pursued as the most perfect. What I apprehend to be peculiar in it, was the application of the arch of 96°; not only as a check on the arc of degrees and minutes, but as superior to it, being derived from the more simple principle of continual bisection. To make room for this, he has entirely rejected the subdivision by diagonals, and has adopted the method of the vernier; but the subdivision of the vernier divisions he, as I apprehend for the first time, measured by the turns of the detached adjusting screw, making it in fact a micrometer, by which the distance of the set of the instrument was to be measured from the perfect coincidence of one of the actual divisions of the limb with the next stroke of the vernier; by which means the observation could not only be read off with all the precision that the division of the instrument was capable of, but the two sets of divisions could be checked and compared with each other. Another thing that I apprehend to be peculiar in this instrument, was the more certain method of transferring and cutting the divisions, from the original divided points, by means of the beam-compass, than could possibly be done from a fiducial edge, as had doubtless been constantly the practice in cutting diagonals; for, placing the steady point of the beam-compass in the tangent line to that part of the arc where each division was to be cut, the opening of the compass being nearly the length of the tangent, the other point would cut the division in the direction of the radius nearly; and though in reality an arch of a circle, yet the small part of it in use would be so nearly a right line, as perfectly to answer the same end; all which advantages put together, it is probable, induced Mr. Graham to reject the diagonals.

Soon after the completion of this quadrant, Mr. Graham undertook to execute a zenith sector for the Rev. Dr. Bradley, which was fixed up at Wanstead, in Essex, in the year 1727. The very simple construction that he adopted for this instrument, the plumb-line itself being the index, did not admit of the use of a vernier: he therefore contented himself with dividing the arch of the limb of this instrument by primary points, as close as he thought necessary, that is, by divisions of 5′ each, and measuring the distance from the set of the instrument to the next point of division by a micrometer screw, in the construction of which screw he used uncommon care and delicacy. I have mentioned this instrument to introduce this observation; that I think it highly probable, had Mr. Graham constructed the great quadrant after the zenith sector had been fully tried, he would have rejected not only the diagonals but the verniers also, as containing a source of error within themselves which may be avoided by a well-made screw.

It seems also, that Mr. Graham, at the time he constructed both these instruments, was not aware how much error could arise from the unequal expansions of different metals by heat or cold: for in both, the radius, or frame of the instrument, was iron, while the limbs were of brass. They however remain n the Royal Observatory, perfect models, in all other respects, of every thing that is likely to be attained in their respective destinations, and monuments of the superlative abilities of that great mechanician Mr. Graham.*

Mr. Graham lived till the year 1751; and during his time there were few instruments of consequence constructed without his advice and opinion. They were for many years done by Mr. Sisson, to whom doubtless Mr. Graham would fully communicate his method of division; and from this school arose that very eminent and accurate artist Mr. Bird, whose delicate hand, joined with great care and assiduity, enabled him still further to promote this branch of division;

* I have been informed, that Dr. Maskelyne has caused this objection to the sector to be rectified, since its removal to the Royal Observatory, by substituting an iron limb instead of that of brass, the points being made upon studs of gold.—Orig.

and which being carried by him to a great pitch of perfection, the Commissioners of Longitude did themselves the credit, by a handsome reward, to induce him to publish to the world his particular method of dividing astronomical instruments; which being drawn up by himself, in the year 1767, this matter is fully set forth to the public: I shall therefore only take this opportunity of observing, that there seems to be one article in which Mr. Bird's method may be still improved.

I apprehend that no quadrant, which has ever undergone a severe examination, has been found to form a perfect arch of 90°; nor is it at all necessary it should: the perfect equality of the divisions throughout the whole is the first and primary consideration; as the proportion of error, when ascertained by proper observations, can be as easily and readily applied, when the whole error of the rectangle is 15″, as when it is but 5. In this view, from the radius taken, I would compute the chord of 16° only. If I had an excellent plain scale, I would use it; because I should expect the deviation from the right angle to be less than if taken from a scale of more moderate accuracy; but if not, the equality of the divisions would not be affected, though taken from any common diagonal scale. This chord, so prepared, I would lay off 5 times in succession, from the primary point of 0 given, which would complete 80°. I would then bisect each of those arches of 16°, as prescribed by Mr. Bird, and laying off 1 of them beyond the 80th, would give the 88th degree: proceeding then by bisection, till I came to an arch of 2°, laying that off from the 88th degree, would give the point of 90°. Proceeding still by bisection, till I had reduced the degrees into quarters = 15′ each, I would there stop; as from experience I know, that when divisions are too close, their accuracy, even by bisection, cannot be so well attained, as where they are moderately large. If a space of $\frac{4}{10}$ of an inch, which is a quarter of a degree, on an 8 feet radius, be thought too large an interval to draw the index over by the micrometer screw, this may be shortened, by placing another line at the distance of $\frac{1}{3}$ of a division on each side of the index line; in which case the screw will never have to move the index plate more than $\frac{1}{3}$ of a division, or 5′; and the perfect equality of these side lines, from the index line, may be obtained, and adjusted to 5′ precisely, by putting each of the side lines on a little plate, capable of adjustment to its true distance from the middle one, by an adjusting screw. The above hint is not confined to the chord of 16°, which prohibits the subdivisions going lower than 15′: for if it be required to have divisions equivalent to 5′ on the limb itself; then I would compute the chord of 21° 21′ only; and laying it off 4 times from the primary point, the last would mark out the division 85° 20′, pointed out by Mr. Bird; supplying the remainder to a quadrant, from the bisected divisions as they arise, and not by the application of other computed chords.

In my introduction to M. Roemer's method of division, I have shown, that divisions laid off in succession, by the same opening of the compasses, either in a right line, or in the arch of a circle, being in its idea geometrically true, and in itself the most simple of all processes, it has the fairest chance of being mechanically and practically exact, when cleared of the disturbing causes. The objection therefore to his method is, the great number of repetitions, which depending on each other in succession (requiring no less than 540 to a quadrant, when subdivided to 10′ each,) the smallest error in each, repeated 540 times, without any thing to check it by the way, may arise to a very sensible and large amount: but in the method I have hinted, this objection will not lie; for, in the first case, the assumed opening is laid off but 5 times; and in the latter case but 4 times; nor does this repetition arise out of the nature of the thing; for, if you like it better, you may, in the former case, at once compute the chord of 64°; and in the latter that of 85° 20′, and then proceed wholly by bisection; supplying what is wanted to make up the quadrant, from the bisected divisions, as they arise. Mr. Bird prescribes this method himself, for the division of Hadley's sextants and octants.

I suppose he was the first who conceived the idea of laying off chords of arches, whose subdivisions should be come at by continual bisection; but why he mixed with it divisions that were derived from a different origin, as prescribed in his method of dividing, I do not well conceive. He says, that after he had proceeded by the bisections, from the arc of 85° 20′, the several points of 30°, 60°, 75°, and 90°, all of which were laid down from the principle of the chord of 60° being equal to radius, fell in without sensible inequality; and so indeed they might; but yet it does not follow that they were equally true in their places as if they had been, like the rest, laid down from the bisection from 85° 20′, and therefore being the first made, whatever error was in them, would be communicated to all connected with them, or taking their departure from them. Every heterogeneous mixture should be avoided where equal divisions are required. It is not the same thing, as every good artist will see, whether you twice take a measure from a scale as nearly the same as you can, and lay them off separately; or lay off 2 openings of the compasses, in succession, unaltered; for though the same opening, carefully taken off from the same scale a 2d time, will doubtless fall into the points made by the first, without sensible error; yet as the sloping sides of the conical cavities made by the first point will conduct the points themselves to the centre, there may be an error which, though insensible to the sight, would have been avoided by the more simple process of laying off the opening twice, without ever altering the compasses.

The 96 arc was, I have no doubt, invented by Mr. Graham, from having perceived, in common with all preceding artists, how very much more easy a given line was to bisect, than to trisect, or quinquisect: and therefore the 96 arc which proceeded by bisections only, or by laying off the same identical openings, which, as already shown, is still more simple and unexceptionable, was wholly intended by him by way of checking the division of the arc of 90, which required trisections and quinquisections. But experience soon showed the superior advantage of it so strongly, that the use of the 90 arc is now wholly set aside, where accuracy is required; whereas the ingenuity of Mr. Bird having shown a way to produce the 90 arc by bisection, when this is really pursued quite through the piece, by rejecting all divisions derived from any other origin, the 90 arc will have nothing in it to prevent its being equally unexceptionable with the 96 arc; and consequently if, instead of the 96 arc, another arc of 90 was laid down, which being on a different radius, its divisions will stand totally unconnected with the former, then these two arcs would in reality be a check on each other; for being of equal validity, the mean might be taken: and if, instead of vernier divisions, strokes at the distance of any odd number, as 7, 9, 11, or 13, are marked on, and carried along with the index plate; these will produce a check on neighbouring divisions; and the angle may then be deduced from the medium of no less than 4 readings.

The last works that have been made known to the public in the line of graduation, so far as have come to my knowledge, are those of the very ingenious Mr. Ramsden, which were published, by order of the Board of Longitude, in the year 1777. From his own information I learn, that in the year 1760 he turned his thoughts towards making an engine for dividing mathematical instruments; and this he did in consequence of a reward offered by the Board of Longitude to Mr. Bird, for publishing his method of graduating quadrants; for as several years previous to that period, he had taken great pains to accomplish himself in the art of hand-dividing, in which line Mr. Bird had acquired his eminence, he conceived by this publication of Mr. Bird's he should be reduced to the same standard of performance with the rest of the trade. He therefore, partly to save time, and that kind of weariness to an ingenious mind that ever must attend the endless repetition of the same thing from morning to night; partly still to preserve the pre-eminence he had then gained; and partly to procure dispatch in the great increase of demand for Hadley's sextants and octants, in consequence of the successful application of the moon's motion to the purpose of ascertaining the longitude at sea, which instruments for this purpose required a degree of accuracy and certainty in the division, by no means necessary when applied to the simple purpose of observing latitudes; I say, for these considerations, Mr. Ramsden determined to set about something in the instrumental way, that should be sufficient effectually to answer these purposes.

Accordingly, considering the nature of the endless screw, he set about an engine whose divided wheel or plate was of 30 inches diameter; and though the performance of this first essay was inferior to his expectations and wishes, yet with it he was able to divide theodolites with a degree of precision far superior to any thing of the kind that had been exhibited to the public. This engine I saw in the spring of the year 1768; and it appeared to me not only a very laudable attempt towards instrumental divisions, but a very good model for the construction of an engine of the most accurate kind for that purpose. And at the same time he showed me the model or pattern for casting a wheel of a much larger size, which he proposed to make on the same plan, and with considerable improvements. This being effected some time in or about the year 1774, its accuracy was proved by making a sextant, afterwards subjected to the examination of Mr. Bird; who in consequence approved the method, not only as fully sufficient for the division of Hadley's sextants and octants for any purpose whatever, but in fact for dividing any instrument whose radius did not exceed that of the dividing wheel, which was 45 inches in diameter: on which the Board of Longitude very properly and usefully resolved to confer a handsome reward on Mr. Ramsden, for delivering a full explanation of his method of making the said engine; which, in consequence, was published by order of the Board of Longitude in the year 1777, above-mentioned; the designs of which are so full and explicit, that whoever could not understand that description, so as to enable him to make it, would be unfit to undertake it on other accounts.

From what I have said on the works of the different artists above-mentioned, it would seem that the art of graduation was brought to such a degree of excellence that nothing material can now be added to it: and I should have been apt to have thought so myself, if I had not happened, in the course of my life, to have had a communication made to me, under the seal of secrecy, which seems to promise yet further light and assistance in perfecting that important art; and every impediment to the discovery of it being now removed, I shall in the remainder of this essay give the clearest description of it that I am able, with such elucidations and improvements as seem to be naturally pointed out by the method itself.

In the autum of the year 1741, I was first introduced to the acquaintance of that then eminent artist, Mr. Henry Hindley, of York, clock-maker. He immediately entered with me into the greatest freedom of communication, which founded a friendship that lasted till his death, which did not happen till the year 1771, at the age of 70. On the first interview, he showed me, not only his general set of tools, but his engine, at that time furnished with a dividing plate, with a great variety of numbers for cutting the teeth of clock wheels, and also, for more nice and curious purposes, furnished with a wheel of about 13 inches diameter, very stout and strong, and cut into 360 teeth; to which was applied an endless screw, adapted to it. The threads of this screw were not formed on a cylindric surface, but on a solid whose sides were terminated by arches of circles. The whole length contained 15 threads; and as every thread, on the side next the wheel, pointed towards the centre, the whole 15 were in contact together; and had been so ground with the wheel, that, to my great astonishment, I found the screw would turn round with the utmost freedom, interlocked with the teeth of the wheel, and would draw the wheel round without any shake or sticking, or the least sensation of inequality. How long this engine might have been made before this first interview, I cannot now exactly ascertain: I believe not more than about a couple of years; but this I well remember, that he then showed me an instrument intended for astronomical purposes, which must have been produced from the engine, and which of itself must have taken some time in making.*

I in reality thought myself much indebted to Mr. Hindley for this communication; but though he showed me his engine, and told me that the screw was cut by the rotation of the point of a tool, carried round on a strong arm, at the distance of the radius of the wheel from the centre of motion, which arm was carried forward by the wheel itself, and the wheel was put forward by an endless screw, formed on a cylinder to a proper size of thread, cut by his chock lathe; though he showed me also this chock lathe, and the method employed to make the threads of the screw equiangular with the axis, that is, to free the screw

* This instrument was of the equatorial kind; the wheel parallel to the equator, the quadrant of latitude, and semi-circle of declination, being all furnished with screws containing 15 threads each, framed and moved in the same manner as that of the engine; the whole of which instrument was already framed, and the telescope tube in its place, which was intended to be of the inverting refracting kind, and to be furnished with a micrometer. This however was not completed till some years after; but in the year 1748 I received it in London for sale. It was with me 2 years, in which time I showed it to all my mechanical and philosophical friends, among whom was Mr. Short, who afterwards published in the Philos. Trans. vol. 46, an account of a portable observatory, but without claiming any particular merit from the contrivance. However, the model of it differs from Hindley's equatorial only in the following articles. He added an azimuth circle and compass at the bottom. He omitted the endless screws, placing verniers in their stead; and at the top, a reflecting telescope instead of a refractor. This instrument of Hindley's being afterwards returned to him unsold, I pointed out the principal deficiencies that I found in it; viz. that, in putting the instrument into different positions, the springing of the materials was such as in some positions to amount to considerable errors. This remained with him in the same state till the year of the first transit of Venus, viz. 1761; when it was sold to ——— Constable, Esq. of Burton Constable, in Holderness. Mr. Hindley, to remedy the evil above-mentioned, applied balances to the different movements. He soon afterwards completed one, de novo, on this improved plan, for his Grace the late Duke of Norfolk. A method of balancing in much the same way, without the knowledge that it had been done before, has been fully explained, and laid before the Society, by our ingenious and worthy brother Mr. Nairne. Phil. Trans. vol. 59, p. 108.—Orig.

from what workmen term drunkenness; and also showed me how, by the single screw of his lathe, he could cut, by means of wheel-work, screws of every necessary degree of fineness,* and by taking out a wheel, could cut a left-handed screw of the very same degree of fineness; by which means he was enabled not only to adapt his plain screw to the size of the teeth of his wheel, but also to prevent any drunkenness that otherwise the curved screw would be subject to in consequence of being produced from the plain one; also, that the screw and wheel, being ground together as an optic glass to its tool, produced that degree of smoothness in its motion that I observed; and lastly, that the wheel was cut from the dividing plate: yet, how the dividing plate was produced, he for particular reasons reserved to himself.

Nor can he be blamed for the reservation of this one secret; as he had, even at the time of my early acquaintance with him, a kind of foresight that from the superior merit of Hadley's quadrant, a demand for that, and other instruments for the purpose of navigation, was likely to increase; and that he might live to see a public reward offered for a method of dividing them with greater accuracy and dispatch than had at that time appeared. Indeed he had himself an idea, from the satisfactory success that had attended his operations in dividing, that a screw and wheel, produced from his engines of 1 foot diameter, would have as much truth as the 8-feet quadrant at Greenwich: and though he doubtless greatly over-rated the accuracy of these miniature performances, yet it does not follow, as his methods were not confined to so narrow a compass, but that, his scale of operation being proportionably enlarged, a degree of accuracy in the graduation of astronomical instruments may be attained in proportion.

I must here beg leave to observe, that there appears to me to be a natural limitation to the accuracy of instruments, consisting of considerable portions of a circle, such as quadrants, &c.† I do not find that the finest stroke on the limb of a quadrant, made by Bird's own hand, if removed from its coincidence with its index, can be replaced with any degree of certainty nearer than the 4000th part of an inch, though aided by a magnifying glass.‡ A 4000th part of an inch being then determined to be the minimum visibile by the strokes of an instrument, this will be less than 1″ of a degree on a radius of 4 feet; and

* A machine for cutting the endless screw of Mr. Ramsden's engine, on principles exactly similar, is fully and accurately set forth in his description of his dividing engine above-mentioned.—Orig.

† The zenith sector consists but of few degrees, with little variation of its position in using it.—Orig.

‡ It will be to little purpose to attempt it with a greater power. Double microscopes can doubtless be formed to magnify objects, far less than a 4000th part of an inch, to distinct surfaces; but then the advantage of such degrees of magnifying power is chiefly on the organized bodies of nature. Let a dot, or the finest point that can be made by human art, be so viewed, and it will appear not round, but a very ragged irregular figure.—Orig.

therefore, if the whole set of divisions on the limb could be preserved true to this aliquot part of an inch, the 8-feet quadrants of Greenwich might be expected to be true to half a second. How far they are from this, I do not exactly know; but I have reason to think they vary from it some seconds: nay I believe it is generally allowed that our largest quadrants, even when executed by the accurate hand of Mr. Bird, do not exceed those of a less size, by the same hand, in proportion to their increase of radius: nor can it well be expected that they should; since, as the weight necessarily increases in a triplicate ratio of the radius, the great weight of the Greenwich quadrants in moving and fixing them, as they could not be divided in their place, may easily derange the framing; or even the internal elasticity of the materials may give way, by a change of position, to so minute a quantity as a 4000th part of an inch. It therefore appears to me, that since the divisions of a quadrant of 4 feet radius are more than sufficient, and even those of 3 feet admit of all the distinctness that in other respects is wanted, a 3-feet quadrant, in point of size, is capable of all attainable exactness; and would be as much to be depended on as any of those now in being of 8 feet. By adopting quadrants of this smaller size, we shall of course get rid of $\frac{18}{19}$ of the present weight; and consequently of much cumber, unhandiness, and derangement, that must arise from that weight, as well as the fear of totally discomposing them, if ever moved out of their place.

It is now time to open a principle on which there is a prospect of effecting such an improvement. I have shown that a 4000th part of an inch is the ultimatum that we are to expect from sight, though aided by glasses, when observing the divisions of an instrument. But in the 48th volume of the Philos. Trans. for the year 1754, I have shown the mechanism of a new pyrometer, and experiments made with it, by which it appears that, on the principle of contact, a 24,000th part of an inch is a very definite quantity. I remembered very well that I did not then go to the extent of what I might have asserted, being willing to keep within the bounds of credibility: but on occasion of the present subject, I have re-examined this instrument, and find myself very well authorized to say, that a 60,000th part of an inch, with such an instrument, is a more definite and certain quantity, than a 4000th part of an inch is to the sight, conditioned as above specified. The certainty of contact is therefore 15 times greater than that of vision, when applied to the divisions of an instrument: and if this principle of certainty in contact did not take place even much beyond the limit I have now assigned, we never should have seen those exquisite mirrors for reflecting telescopes, that have already been produced.

These reflections apply immediately to my present subject, as Hindley's method of division proceeds wholly by contact, and that of the firmest kind; there being scarcely need of magnifying glasses in any part of the operation.

In the year 1748 I came to settle in London; and the first employment I met with was that of making philosophical instruments and apparatus. In this situation, my friend Hindley, from a principle the reverse of jealousy, fully communicated to me, by letter, his method of division; and though I was enjoined secrecy respecting others, for the reasons already mentioned, yet the communication was expressly made with an intention that I might apply it to my own purposes. The following are extracts from 2 letters, which contain the whole of what related to this subject; and since I have many things to observe on them, so that the paraphrase would be much greater than the text, I think it best not to interrupt the description with any commentary, as perhaps his own mode of expression will more briefly and happily convey the general idea of the work, than any I can use instead of it.

MY DEAR FRIEND, York, 14 Nov. 1748.

As to what you was mentioning about my brother's knowing how I divided my engine plate, I will describe it as well as I can myself; but you will want a good many things to go through with it. The manner is this: first chuse the largest number you want, and then chuse a long plate of thin brass; mine was about 1 inch in breadth, and 8 feet in length, which I bent like a hoop for a hogshead, and soldered the ends together; and turned it of equal thickness, on a block of smooth-grained wood, on my great lathe in the air, (that is, on the end of the mandrel:) one side of the hoop must be rather wider than the other, that it may fit the better to the block, which will be a short piece of a cone of a large diameter: when the hoop was turned, I took it off, cut, and opened it straight again.

The next step was to have a piece of steel bended into the form as per margin;* which had 2 small holes bored in it, of equal size, one to receive a small pin, and the other a drill of equal size. I ground the holes, after they were hardened, to make them round and smooth. The chaps formed by this steel plate were as near together as just to let the long plate through. Being open at one end, the chaps so formed would spring a little, and would press the long plate close, by setting in the vise. Then I put the long plate to a right angle to the length of the steel chaps, and bored one hole through the long plate, into which I put the small pin; then bored through the other hole; and by moving the steel chaps a hole forward, and putting in the pin in the last hole, I proceeded till I had divided the whole length of the plate.

The next thing was to make this into a circle again. After the plate was cut

* The figure is considerably less than the real tool should be.—Orig.

off at the end of the intended number, I then proceeded to join the ends, which I did thus: I bored 2 narrow short brass plates* as I did the long one, and put one on the inside, and the other on the outside of the hoop, whose ends were brought together; and put 2 or 3 turned screw pins, with flat heads and nuts to them, into each end, which held them together till I rivetted 2 little plates, one on each side of the narrow plate, on the outside of the hoop. Then I took out the screws, and turned my block down, till the hoop would fit close on; and by that means my right line was made into an equal divided circle of what number I pleased. The engine plate was fixed on the face of the block, with a steel hole fixed before it to bore through; and I had a point that would fall into the holes of the divided hoop; so by cutting shorter, and turning the block less, I got all the numbers on my plate.

I need not tell you, that you get as many prime numbers as you please; nor that the distance of the holes in the steel chaps must be proportioned to the length of the hoop. You may ask my brother what he knows about my method of dividing; but need not tell him what I have said about it; for I think neither he nor John Smith know so much as I have told you, though I believe they got some knowledge of it in general terms.†——I desire you to keep the method of dividing to yourself, and conclude with my best wishes, &c.

HENRY HINDLEY.

Though the above letter was in itself very clear and explicit, as to the general traces of the method, yet some doubts occurring to me, a further explanation became necessary. A copy of my letter not being preserved, the purport of it may be inferred from the answer, which was as follows:

DEAR FRIEND, *York, March* 13, 1748-9.

I think in your last you seem to be apprehensive of some difficulties in drilling the hoop for dividing: First, that the centre of the hole in the hoop might not be precisely in the centre of the hole of the steel chaps, it was drilled in; but if I described fully to you the method I used, I can see no danger of error there: for my chaps were very thick, and the two corresponding holes were a little conical, and ground with a steel pin; first one pair, and then the other, alternately, till the pin would go the same depth into each. Then for drilling the hoop, I took any common drill that would pass through, and bore the hole. After that I took a five-sided broach, which opened the hole in the brass between the steel chaps, but would not touch the steel; so consequently the centre of

* These I shall hereafter distinguish by the name of saddle-plates.—Orig.

† The persons here referred to were both bred with him. His brother, Mr. Roger Hindley, who has many years followed the ingenious profession of a watch-cap-maker in London, was so much younger as to be an apprentice to him. Mr. John Smith, now dead, had some years past the honour to work in the instrument way, under the direction of the late Dr. Demainbray, for his present Majesty.—Orig.

the holes in the brass must be concentric with the holes in the chaps: and for alterations by air, heat, cold, &c. I was not above 2 or 3 hours in drilling a row of holes, as far as I remember.

2dly. For drilling in a right line, I had a thin brass plate, fastened between the steel chaps, for the edge of the hoop to bear against, while I thrust it forward from hole to hole. What you propose of an iron frame with a lead outside, will be better than my wooden block; but considering the little time that past, between transferring the divisions of the hoop to the divisions of my dividing plate, I did not suffer much that way. It was when I drilled the holes in my dividing plate that I used a frame for drilling, which had one part of it that had a steel hole, that in lying on the plane of the dividing plate was fixed fast in its place for the point of the drill to pass through: then, at the length of the drill, there was another piece of steel, with a hole in it, to receive the other end of the drill to keep it at right angles to the plane of the plate. This piece was a spring, which bended at the end, where it was fastened to the frame of the lathe, at about 18 inches from the end of the drill; so it pushed the drill through with any given force the drill would bear: and though that end of the drill moved in the arch of a circle, it was a very small part of it, being no more than equal to the thickness of the dividing plate.

HENRY HINDLEY.

Whoever attentively considers the communication contained in the above letters will see, that more happy expedients could not have been devised to procure a set of divisions, where there should be the most exact equality among neighbours; and which, for the purposes of clock-making, is the principal thing to be wished for. But herein, as in M. Roemer's method, there were no means of checking the distant divisions, which run on to 360: now such a check, when the expansion of metals is considered, and particularly the difference of expansion between brass and steel, seems absolutely necessary for the purpose of divisions on instruments, where the accurate mensuration of large angles is required, as much as the equality of neighbouring divisions.* With this view the invention of this ingenious person suggested to him the thought of making his curved screw to lay hold of 15 teeth or degrees together: this, in effect, becomes a pair of compasses, 24 removes of which complete the whole circle, and produce 24 checks in the circumference: and whoever considers the very exquisite degree of truth that results from the grinding of surfaces in contact, as already

* The ingenious Mr. Stancliffe, some years a workmen of Hindley's, has suggested, that the difference of expansion between the steel chaps and the brass hoop may be avoided by making the chaps of brass also, with hard steel holes set separately in them, somewhat similar to the jewelled holes of watches.—Orig.

noticed, must expect a very great degree of rectification of whatever errors might subsist in the wheel after its first cutting.

What degree of truth it might in reality be capable of, on its first production and adjustment, is not now to be ascertained, he never having used it for the graduation of any capital instrument. Those that he made with a view to an accurate measure of angles, he always made with a screw and wheel, or parts of circles cut by his engine into teeth, and ground together as before-mentioned; but I have reason to think that its performance, if put to a strict test, was never capable of the accuracy that he himself supposed it to have. The method itself however, from its simplicity and ease of execution, seems to be a foundation for every thing that can be expected in truth of graduation; and in consequence for reducing instruments to the least size that is capable of bringing out all that can be expected from the largest; when it shall, like manual division, have received those advantages that the joint labours of the most ingenious men can bestow on it. That I may not appear to be without grounds for my expectations, I beg leave to propose, what near 40 years occasional contemplation has suggested to me on the subject; and as I can describe the process I would pursue, where different from Hindley's, in fewer words than I could make out a regular criticism on his letters, I will immediately proceed to the description of it.

Proposed Improvements of Hindley's method.—I would recommend the number of parts into which the circle is to be reduced, to be 1440, that is 4 times 360; which divisions will therefore be quarters of a degree; the distances of the holes in the chaps will therefore, to a 3-feet radius, be $\frac{157}{1000}$ of an inch nearly; that is, between the $\frac{1}{6}$ and $\frac{1}{7}$ of an inch distance centre and centre. Having provided myself with a stout mandrel, or arbor, for a chock Lathe, properly framed, that would turn a circle of 6 feet diameter, I would prepare a chock, or platform, for the end of it, of that diameter, or a little more, composed of clean-grained mahogany plank, all cut out of the same log; which, when finished, to be about $1\frac{1}{2}$ inch thick, and formed in sectors of circles, suppose 16 to make the circle; the middle line of each sector lying in the direction of the grain of the wood, this will consequently every where point outward: the method of framing this kind of work is well known.

The way of getting a slip of brass to answer the circumference of this platform is suggested in Mr. Bird's Account of constructing Mural Quadrants. Let a parallelogram of brass, of about 3 feet long, and of a competent substance, suppose half an inch, to make it when finished about $\frac{1}{20}$ of an inch in thickness, be cast of the finest brass; and this to be rolled down till it becomes of sufficient length for the hoop, and about a 5th part more. I would then cut off, from the whole length, somewhat better than a 6th part, the whole being sufficiently reduced to a thickness by the rollers. Perhaps no way will be more ready and convenient to get such a long strip of brass reduced to an equal breadth, than the method prescribed by Hindley; viz. by turning it on the chock prepared; but I would not make it wider on one side than the other, like the hoop of a cask, as he describes, but exactly to fit the chock, when truly cylindric; for the internal elasticity of the brass, in so great a length, will be very sufficient for fitting it on tight enough, without any tapering. This I will now suppose done; and a pair of steel chaps, as described by Hindley, to be also prepared, and ready for grinding; which, by such a careful admeasurement as can easily be made, will give the length of the hoop sufficiently near, on its first preparation.

Method of forming a pair of straps as a check to the divisions.—The part first cut off must be again cut into 2 equal parts in length; which, for distinction sake, I will call the straps; and which are to serve as checks to every 60th and every 120th division of the circle. A steel plate, of about half an inch in breadth, the same thickness as the straps, and in length equal to the breadth of the hoop plate, must be soldered with silver solder to one end of each of the straps, by which means their length will be increased half an inch by the steel. A hole must then be made through each steel plate, of the same size as those through the chaps, and answerable to the middle of the straps; but so near the border of the steel, that when the chaps are put on, and adapted to the steel hole, the next hole will fall through the brass. The steel plates must then be hardened; and a pin being put through the two holes and the two plates, these must be wrought to a right line in contiguity to each other; by this means the straight edge of each of the straps will be reduced to the same distance from the steel hole: the hard steel edges may be rectified by the grindstone, if necessary.

This being done, not only the holes in the chaps, but the holes in the two steel plates, applied to each other, like the two sides of the chaps, must be respectively ground together; not with a taper pin, as prescribed by Hindley; but so as not only to be cylindrical, but that the same cylindrical pin shall equally fit them all, and leave them smooth and polished; which is a process no ways difficult to a curious artist, and of which therefore a minute description is unnecessary. The chaps being then put on one of the straps, with its straight edge uppermost, and a pin put through the holes on the left-hand, and through the steel hole in the strap under operation, the chaps must be set upright, so that the line joining the centres of the holes shall be parallel to the upper edge of the strap; the brass plate, mentioned by Hindley, between the chaps, as a guide for directing them always to that upright position, may be then adjusted and fixed to the inside of the chap next the operator.*

The performance of the ensuing part of this work should be at a season when the temper of the air is not very variable; rather above the mean temper, suppose at 60°, than below it; but above all things the artist should be himself cool; that is, not in a state of sensible perspiration; and there should be a free circulation in the room. Things being thus conditioned in respect to temperature, he may begin to drill the holes in one of the straps; the pin being first put through the chaps and through the steel hole of the strap; and the next hole, being drilled through the brass with a common drill, that and every hole as it goes is to be finished with a taper broach, as prescribed by Hindley; and he may then prove or finish every hole by the application of a thorough broach, made so full as to require a degree of pressure to force it through; and this broach being a little tempered, and the holes quite hard, there will be no fear of injuring the steel holes. Calling the hole in the steel plates o, and observing the time of beginning, you may proceed to drill 60 holes as prescribed by Hindley; and noting how long you have been about it, you may lay the work aside a length of time, equal to the time you took in drilling; that any addition of warmth it may have acquired in handling or working may be again lost in a great degree. After this pause you may begin again, and go on to finish 60 holes more; that is, to the length of 120 holes from the beginning; you then proceed in the same manner with the other strap.

Method of Drilling the hoop.—You are now prepared to commence the work on the long or hoop-plate; and you proceed with it, in forming the first hole with the chaps, as before directed by Hindley, and this first hole you call o. You then place the straps one on each side the hoop, with their gaged edges upward, and put the pin through the holes denominated 60 on the straps, and through the first hole already made, and denominated 0 on the hoop; then, bringing the gaged edges of the steel plates to be even with the upper or working side of the hoop, you pinch them together in the vise, and drill and broach the hole through the steel plates, which will make the hole, number 60, on the hoop. This done, you put the pin through the left-hand hole of the chaps, and the hole marked o on the hoop-plate first made, and proceed to drill with the

* It would be well, previous to the drilling of the steel chaps, that another hole was drilled in the chaps, that should be somewhat above the upper edge of the straps, and in the middle between side and side, to receive a steady pin in, before drilling the main holes; for then a tempered steel pin, a little taper, will, by driving it in as far as necessary, constantly answer this purpose from first to last, so as to regulate the bodies in grinding, to be truly opposite: proper holes should also be drilled for fixing the brass guide plate to one of the chaps.—Orig.

chaps to 59 holes inclusive, which will fill up the whole space from o to the 60th division before obtained. You now again have recourse to the straps, and placing them one on each side the hoop-plate, you put the pin through the 120th hole of the straps, and through the hole marked o on the hoop-plate; and regulating the steel plates to the hoop-plate as before, you drill and form a hole with the steel plates, which will correspond with the 120th hole on the hoop-plate; and afterwards filling up the 59 holes wanting, by means of the chaps, you then have all completed to the 120th division, which is $\frac{1}{12}$ of the whole circle.

You then proceed, in like manner, with another set of 120 holes; that is, placing the 60th hole of the straps to the 120th hole of the hoop-plate, and from it producing the 180th hole; you, in like manner as before, fill up this 60 with the chaps; and afterwards placing the 120th hole on the straps in the 120th hole on the hoop-plate, you will obtain the 240th hole; so that filling up this last set of 60 divisions, you have obtained 241 holes, including 240 spaces or divisions of the hoop; and repeating this process 10 times more, you will in like manner obtain 1441 holes, comprehending 1440 spaces.* And this process, being carried on in temperate weather, the manner of working produces 12 similar operations, wherein the materials and tools concerned will not only be subject to very little change of temperature, but that change, whatever it is, will be nearly similar in each set of 120 holes: we may therefore infer, that the greatest inequality, or indeed any that can be sensible, must be at every 60 divisions, that is, between the 59th and 60th, and between the 119th and 120th, both which will be equally repeated 12 times in the whole length which is to compose the circumference of a circle, and which will thus be checked 12 times in the circumference, and 12 times more at the intermediate distances; that is, with 12 master checks, and 12 subordinate ones, in the whole round.

It is proper here to observe, that in M. Roemer's method even 60 divisions could scarcely be trusted in an affair of great accuracy, on account of the objections already made, arising from the points having such slight hold in the surface of the brass; but here the parts are held so exceedingly firm, and the operation carried on with so much power, that any small inequality in the hardness of the brass, or irregularity of surface, cannot be supposed to affect the place of the centre of the hole; nor will any small inequality that may be suspected from the wear of the steel holes sensibly affect the centre of the hole, to which every thing is ultimately referred.

Method of Joining the Hoop.—A more happy thought than that of Hindley's,

* It will be proper, for reasons hereafter to be mentioned, to continue the divisions to 20 holes more, making in the whole 1461 holes.—Orig.

for joining the two ends of the hoop, could scarcely have been wished for, in regard to preserving the same equality of the space between the holes contiguous to the joint, as in the other parts: for though, geometrically speaking, the two saddle plates, in which the little cylindrical bolts are fixed, for bringing the terminating holes of the hoop plate to their due distance, being one applied within the hoop, and the other without, will belong to circles of different radii; yet this difference being exceedingly small in such thin metal, and so great a radius, and one being as much too large for the hoop as the other is too little, when the bolts are put in, and the hoop in that part set nearly to a circle by a mould; the mean between them assumed by the hoop, from the elastic compressibility of the materials, will be the truth. It must however be remarked, that in the use of the straps, the joining of the hoop should not be made at any part between a 119th and a 120th division, as some inequality must be supposed there, unless the saddle plates were adapted thereto. The method the most easily practised, will be to continue the division on the hoop, about 20 more than the completion of the number intended to form the circle, and to cut off all the overplus ones at the beginning.

The saddle plates I would recommend to contain 10 holes each; so that if the divisions are carried on to 20 more than what will be contained in the circle, there will be a piece containing 20 to cut off; and this again being cut in the middle will afford 10 holes to make each saddle plate; so that there will be a place for a bolt on each side the joint, and then putting a bolt through every other hole, there will be 3 bolts at an end. The pieces destined for the saddle plates, thus obtained, being broader than can be admitted when put to this use, I would advise to divide the breadth of the plate into 3 equal parts; and with a cutting hook, which perhaps will be attended with the least violence in the separation, to separate the 2 outside pieces from the middle piece: by this means the 2 saddle plates, though double, will occupy $\frac{1}{3}$ only of the breadth of the hoop in the middle; and 2 of the pieces cut off being applied, one on each side of the saddle plate on the outside, will answer in like manner for the rivet plates.

The last operation to complete the joining of the hoop is the putting on the rivet plates: to complete this, I would advise a piece of brass, of 3 or 4 inches in length, to be filed so as to answer to the inside of the hoop, when reduced to a true circular form; and being $\frac{3}{8}$, or $\frac{1}{2}$ inch in thickness, to file the opposite side somewhat nearly concentric to it; apply the middle of its convex arch to the inside of the hoop at the joint, and then bringing on the middle of one of the rivet plates to the joint of the hoop, confine the 3 together by a couple of narrow-chapped hand vises, leaving a space between them capable of receiving a couple of pins as rivets on each side the joint; the holes for the rivets are then to be drilled through all, and a little smoothed with a broach at their entry, into which smooth taper pins are to be driven; not with violence, but moderately, that no sensible stretching of the solid parts may take place; then cutting off and smoothing the heads, shift the vises so as to receive another couple of holes, and a third couple in the same end of the hoop; and proceed progressively in the same manner, from the middle to the other end of the rivet plate; then gently separate the internal brass mould with a thin knife, or such like instrument; and cutting off, and very lightly rivetting the inner ends, proceed to fix the other rivet plate, in the same manner, on the other side: by this means the hoop will be firmly joined in the very position given it by the saddle plates and mould. These plates may then be removed, the inside of the hoop cleared and smoothed, if necessary; and the outside will have the middle part clear where the divisions lie, and that without sensible loss or gain in the juncture.

Method of Transferring the Divisions of the Hoop to a Dividing Plate.—The hoop being thus refitted for the chock, that should be turned down to leave a shoulder on one side, that the hoop, now reduced to an equal breadth, may be forced against it; and the divisions, being equally distant from one of its edges, will be all found in a circle, as if turned on it. It should be very carefully fitted to the chock, that it may go on with a sufficient degree of tightness, and without the necessity of much forcing; and it will be no inconvenience now, if it goes on a very slight degree of taper of the chock, as the internal spring of the materials will easily accommodate it to this shape without any injury to its general truth: a slight degree of a groove should be turned in the place where the divisions will come, that any conical pin, that is to serve as an index, let drop into the divisions or holes, may not, by reaching through this thin plate, abut on the wood, rather than on the sides of the holes: and thus this hoop is made into a wheel of 1440 equal divisions, moveable round on its own axis, on which it was formed.

Against the time that this is completed, there must be prepared a flat circular plate or wheel of brass, the rim of which should be of about $3\frac{1}{2}$ inches breadth, and about $\frac{2}{10}$ of an inch in thickness when finished, to make a dividing plate; the external diameter of this is to be such, that when laid flat on the surface of the mahogany platform, its extreme edge will exceed the diameter of the hoop by about half an inch all round. There must also be prepared brass arms, suppose 8 in number, of an equal substance with the outer rim, and all connected with a circular plate in the middle; and, the whole of this work being framed beforehand, is to be let on flat upon the mahogany platform; whose face is supposed to be turned truly flat, and sufficiently affixed with screws: in this situation, the outward edge is to be turned, and the outward face of the rim turned flat. The centre plate, which may be about 12 inches diameter, is also to be turned as flat as possible, and a centre hole, of about half an inch diameter, to be very carefully turned in it.

A piece of clean, straight-grained, well seasoned mahogany, of about 2 feet long, 3 inches thick, and 5 or 6 inches broad, is then to be well affixed to some part of the general frame of the lathe, which must now have its position altered, so that the platform will become horizontal; and therefore the frame should be originally made with this view.* The piece of mahogany is to be affixed so that one of its larger faces shall be in a parallel plane to the face of the platform, and so low as to clear the under side of the platform in its rotation; and so far distant from the centre, that an index may be fixed on this upper face of the piece of wood, so as conveniently to drop into the holes of the hoop; while the common cutter frame of a clock-maker's engine shall be firmly attached on the same face of the wood, and so fixed as to cut the edge of the dividing plate into teeth, answerable to the several divisions of the hoop. The teeth need only to be cut with a common cutter, making a parallel notch: and here it will be proper to observe, that not only both the index and cutter are to be founded on the same piece or base of wood; but that the nearer they are together, the more free they will be from the effects of all variations of expansions by variations of temperature.

The Equalizing the Teeth of the Dividing Plate by Grinding.—The object of transferring the divisions of the hoop to the teeth of the dividing plate, is still further to equalize the teeth by grinding; especially those that, falling within the compass of each set of 120 divisions, may be supposed, if any, to be mended by it; but as it may be incommodious to construct a curved screw, of such a length and size, in Hindley's method, as would be sufficient for the purpose, I would propose to use 2 screws of brass, cut from a cylinder in the way set forth by Mr. Ramsden, each of which, with a very little grinding on this large circumference, would lay hold of 10 or 12 teeth together. I would place the 2 screws, that is, their middles, to be 90 divisions asunder; of consequence, when one of the screws is between the 59th and the 60th, or between the 119th and 120th division of each set, the other will be in the middle of the space divided by the chaps only. The threads of these screws I would advise to be cut a little taper, so that as they grind in, they may fill the notches of the teeth; which also, by this means, will acquire a little tapering towards their extremities; and by cutting the notches parallel, as mentioned, the true ground part will always be certain of being at the extremity.

When the screws have been used in grinding till they are found to have the effect of a perfectly equal and easy rotation all round, and all the teeth reduced to a sensible taper, and regular bearing, I would then totally remove the screws from the square block of wood, on whose upper face I suppose them to have been

* After changing the position of the lathe, the collar of its mandrel should be removed, and the neck made to move within three planes, so as to preserve an exact centre, in the manner of an equal altitude instrument.—Orig.

mounted; in like manner as I suppose the index and cutting frame to have been removed, to make room for the mounting of the screws. I now consider the teeth of the dividing plate, so formed, as having all the equality that the present known state of human art has pointed out; and the whole convertible on the axis mandrel on which it has been originally formed, and the central hole of the plate concentric with it: I therefore consider the ground faces of the teeth of the plate as the actual divisions. It now remains to show how they are to be transferred, to form the divisions of an instrument.

Preparation of the Dividing Plate for Graduating Instruments.—If a small cylinder of hard steel be duly polished, and made of a size so as just to chock in between the extremities of the teeth, then the centre of that cylinder will be a fixed point, in respect to the circumference of the wheel: if another cylinder be applied in like manner, at the distance of a number of divisions, suppose it a prime number, so as to cross all former divisions, viz. 17 or 19, then the middle of the line joining the centres of the two cylinders will remain in the direction of the same radius, though one of them should force in a minute quantity farther than another; and if a point be assumed in the direction of a tangent to a circle at this middle-point, then though both the cylinders should drop in a minute quantity farther at one time than another, yet the middle-point would remain at the same distance from the point in the tangent; provided that point was removed to a competent distance, as to 5 or 6 inches. On this principle I would construct an index, the two cylinders being fixed in a frame, convertible about the middle-point, and to be centred in the end of the lever, representing the tangent; then this lever being again convertible about the point in the tangent line, the middle-point would always have a fixed distance from the point in the tangent, and there hold it steadily fast; the tangent point being placed on the fixed block before-mentioned.

Use of the Dividing Plate in the Graduation of Instruments.—Our dividing plate is now ready for the reception of an instrument; suppose it a quadrant, whose radius however must not exceed the radius of the dividing plate: it is to be laid on the face of the dividing plate, and a weight, or weights, equivalent to that of the quadrant, is placed on the opposite side, to balance it. It must also be supposed, that the quadrant is made with a view to be divided by this engine; and consequently that the central cylinder is so well adapted, and nicely fitted to the centre hole of the quadrant, that the centre cylinder can be removed, in order for the limb to be divided, and again replaced without sensibly altering its centre. This being the case, let a piece of metal be turned, to apply to the quadrant, perfectly like its centre cylinder at the upper end, and turned nicely to fit the central hole in the dividing plate, at the lower end; then, the quadrant being fixed with proper fastening screws, I would cut the divisions with a beam compass; and, if a fixed point be assumed, viz. the centre of the tangent point for the index; then the beam compass being always opened to the computed length of the tangent of the circle of divisions, it will be sufficiently near for cutting the divisions, square to the circular arches between which they are placed.

It will also be proper, to prevent unequal expansions, that the beam of the compass should be formed of a piece of clean-grained white fir; and that the length between the points be inclosed in a tube of tin or brass; without touching the beam, except at the terminations, which will in a great measure protect it from both alteration of moisture, and of heat from the body of the artist, during the operation. It will be likewise proper to have a lever, or some equivalent contrivance, to bring the dividing plate forward; that after lifting the little cylinders out of the divisions, and resting them on the tops of the teeth, they may be brought gently forward with an equal drag, and ultimately snap in between the teeth, by the strength of the spring commanding the index; by this means the drag of the friction of the whole will be constantly the same way.

Conclusion.—Now if, as it has been shown, a quadrant of any radius may be read off to the 4000th part of an inch, then this quantity on a radius of the 3 feet will not be so much as $1\frac{1}{3}$ second; and as the whole of the process is carried on by contact, in which a greater error than that of a 60,000th part of an inch cannot be admitted in any single operation, I should assuredly expect a 3-feet quadrant, so divided, to be true in its divisions, and read off to at most 2 seconds. But, after all, in an instrument like this, I should expect the greatest source of error to be in the want of perfect coincidence of the centre of the divisions with the actual centre on which the index revolves; and therefore, that if, instead of a quadrant of 3 feet radius, a complete circle of 5 feet diameter was divided, and its divisions read off from the two opposite points, taking the mean, then the errors of the centre will be wholly avoided. For this reason, I am very clearly of opinion, that the sagacious proposition of Mr. Ramsden, to use circles instead of quadrants, or other portions of circles, will bid much the fairest for perfection in actual practice; and that his ingenious method of making them both stiff and light, by the use of hollow conical tubes by way of spokes, in the manner of a common wheel, will enable him to mount them of 5 feet diameter, on hollow axes, in the nature of a transit. By this means we shall have all the good properties of both the quadrant and transit united in one instrument; and observations both of right ascension and declination, through the very same telescope, as long since attempted by M. Roemer; and to a degree of perfection and certainty, in point of declination, hitherto unattainable by the largest instruments that have yet been made.

N. B. In matters of very nice determination, small circumstances often come to be of consequence; and it is in this view that I mention what follows. It was a practice of Hindley's of many years standing, and since followed by myself and others, wherever he made any use of the vernier, to lay the vernier plate in the same plane, or cylindrical surface continued, on which the principal divisions are cut. It is of equal utility, though the vernier be rejected, to lay the index stroke in the plane of the divisions. In this way the divisions being by convenience on the external border of the limb,* 2 sets of divisions are thus rendered incommodious; but those that wish 2 sets, as a check, will in a great measure aid themselves, by reading from 2 different parts of the same set of divisions; which is very easily provided for, by putting an additional stroke on the index plate, at the distance of 9, 11, or any prime number of divisions to 19, 23, or more; and reading off from that stroke also; as before recommended for great quadrants, where the vernier is proposed to be rejected: † so that they will thereby be mutually checked by divisions that had no correspondence in their original formation.

II. A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the Star marked δ by Bayer, near the Head of Cepheus. By John Goodricke, Esq. Dated York, June 28, 1785. p. 48.

Mr. Goodricke's first observation was Oct. 19, 1784, and they were continued almost daily, till June 28, 1785. From this series he settled, that the star has a periodical variation of $5^d\ 8^h\ 37\frac{1}{2}^m$, during which time it undergoes the following changes: 1. It is at its greatest brightness about 1 day and 13 hours. 2. Its diminution is performed in about 1 day and 18 hours. 3. It is at its greatest obscuration about 1 day and 12 hours. 4. It increases in about 13 hours. When it is in the first point it appears as a star of between the 4th and 3d magnitude; but its relative brightness does not seem always to be quite the same, being sometimes between ζ and ι Cephei, and sometimes only equal to, or something less than, ι Cephei, or between ζ Cephei and 7 Lacertæ. In the third point it ap-

* It has been objected, that laying the divisions on the extreme edge of the limb of the instrument subjects it to injury: but, to obviate this, in a Hadley's quadrant made for me, by my direction, by the late Mr. Morgan, in the year 1756, wherein the vernier is laid even with the divisions, those are protected by a projection of the solid part of the limb, beyond the divisions; a rabbet being sunk in the edge of the limb, to clear the vernier.—Orig.

† I would not have it thought, from my proposal of rejecting the vernier, that I have any quarrel with it; I think it a very simple and ingenious contrivance, where it is properly applicable; that is, where the strokes of the vernier, or their estimated halves, are sufficient for all the precision required or expected from the instrument, as in Hadley's quadrants, theodolites, &c.: but where still more minute divisions are required than can easily be had by estimation from the vernier; to do this by a screw, as a supplement to the vernier, appears to me in the light of bringing a more accurate tool to supply the deficiencies of one less accurate; when the former might, with more propriety, supply the place of the latter altogether.—Orig.

pears as a star of between the 4th and 5th magnitude, if not nearer the 5th; and its relative brightness is as follows: nearly equal to ε and ξ Cephei, and considerably less than 7 Lacertæ. Some observations of Mr. Pigott's are also given, which tend to confirm the results above given.

Mr. G. then adds, that the greatest brightness of δ Cephei does not seem to be always quite the same, is not peculiar to this star, but is also to be observed in the other variable ones. He remarked in a late paper, that the greatest brightness of β Lyræ is subject to considerable alterations, and thought then that it might be owing to some fallacy of observation; but now he alters, in some measure, his opinion on this head. Even Algol does not seem to be always obscured in the same degree, being perceived to be sometimes a little brighter than ρ Persei, and sometimes less than it. These seeming irregularities however do not appear to affect the period; for if the same precise phases are compared together, it will be found still regular. This he supposes may be accounted for, by a rotation of the star on its axis, having fixed spots that vary only in their size.

III. Magnetical Experiments and Observations. By Mr. T. Cavallo, F.R.S. being the Lecture founded by the late Henry Baker, Esq. F. R. S. p. 62.

The object of this lecture is to show the properties of some metallic substances with respect to magnetism; and the experiments here related, says Mr. C., seem to ascertain some new and remarkable facts. The magnetic properties have been generally thought to belong only to iron, or to those substances which contained that metal; comprehending under the general name of iron not only the metal, commonly so called, but also its more perfect and more imperfect states, viz. steel iron ores, among which is considered the magnet, and the calces of iron, excepting only those which are very much dephlogisticated, for they possess no magnetic property whatever. Some other metallic substances, and especially platina, brass, and nickel, on which the magnet has some action, were thought to be magnetic so far as they contained some portion of iron, the presence of which may be manifested by chemical methods in many cases, but not always; because the quantity of iron may be so very small in proportion to the weight of the other metal in which it is concealed, as not to be discoverable by chemical analysis, and yet it may be sufficient to affect the magnetic needle. The following experiment will show, that an exceedingly small quantity of iron will render a body sensibly magnetic.

Having chosen a piece of Turkey-stone, which weighed about an ounce, I examined it by a very sensible magnetic needle, and found that it had not the least degree of magnetism, the needle not being moved from its usual direction by the vicinity of any part of the surface of the stone; I then weighed a piece of steel with a pair of scales that turned with the 20th part of a grain, and afterwards drew one end of it over the surface of the stone in various directions. This done, the piece of steel was weighed again, and was found to have lost so small a part of its weight as not to be discernible by that pair of scales; yet the Turkey-stone, which had acquired only that small quantity of steel, affected the magnetic needle very sensibly. Chemistry seems not to afford any means by which so small a quantity of iron may be decisively detected in a body that weighs 1 oz. Hence it follows, that though no iron is to be discovered in a body by chemical methods, yet it should not be concluded, that the said body, if it affect the magnetic needle, does not owe its magnetism to some small quantity of iron concealed in its substance.

The most of my experiments are relative to the properties of brass; and they seem to prove that this compound metal, which is often magnetic, does not owe its magnetism to iron, but to some particular configuration of its component particles, occasioned by the usual method of hardening it, which is by hammering. In some specimens of brass, and especially in that which has often passed from the work-shop to the furnace, and from the latter to the former, there are sometimes pieces of iron sensible not only to the magnet, or the chemical analysis, but even to the sight, which render the brass strongly magnetic. But the brass generally used in my experiment swas such as, when quite soft, had no sensible degree of magetism.

Examination of the Magnetical Properties of Brass.—A few years ago, being intent on making some magnetic experiments, in which brass was concerned, I used to examine first whether the pieces of brass had any magnetism or not, and rejected those pieces which had an evident degree of that power. In the course of those experiments I remember to have observed, that those pieces of brass which had been hammered were generally magnetic, and much more so than others; in consequence of which I made no use of hammered brass in those experiments. But lately, having ordered a theodolite at a philosophical instrument shop, I particularly enjoined the workmen to try the brass, both soft and hammered, before they worked it, and to make no use of that which had any magnetism. They found that hammered brass, even such as before the hammering had no magnetism, could afterwards disturb the magnetic needle very sensibly. These observations induced me to make the following experiments. Mr. C. here describes 6 experiments on this metal, from which he draws the following conclusions:

1st, That most brass becomes magnetic by hammering, and loses the magnetism by annealing or softening in the fire. 2dly, That the acquired magnetism is not owing to particles of iron or steel imparted to the brass by the tools employed. 3dly, Those pieces of brass which have that property, retain it without any diminution after a great number of repeated trials, viz. after having been repeatedly hardened and softened. But I have not found any means to give that property to such brass as had it not naturally. 4thly, A large piece of brass has generally a magnetic power somewhat stronger than a smaller piece; and the flat surface of the piece draws the needle more forcibly than the edge or corner of it. 5thly, If only one end of a large piece of brass be hammered, then that end alone will disturb the magnetic needle, and not the rest. 6thly, The magnetic power which brass acquires by hammering has a certain limit, beyond which it cannot be increased by further hammering. This limit is various in pieces of brass of different thickness, and also of different quality. 7thly, Though there are some pieces of brass which have not the property of being rendered magnetic by hammering; yet all the pieces of magnetic brass, that I have tried, lose their magnetism by being made red-hot, excepting indeed when some piece of iron is concealed in them, which sometimes occurs; but in this case, the piece of brass, after having been made red-hot and cooled, will attract the needle more forcibly with one part of its surface than with the rest of it; and hence, by turning the piece of brass about, and presenting every part of it successively to the suspended magnetic needle, we may easily discover in what part of it the iron is lodged.

From those observations it follows, that when brass is to be used for the construction of instruments in which a magnetic needle is concerned, as dipping needles, variation compasses, &c. the brass should be either left quite soft, or it should be chosen of such a sort as will not be made magnetic by hammering, which sort however does not occur very easily.

Examination of the Magnetic Properties of some other Metallic Substances.—The result of the experiments on brass induced Mr. C. to examine other metallic substances, and especially its components, viz. copper and zinc: though the result of the experiments has not been very remarkable, excepting with platina, which metal has properties in a great measure analogous to those of brass.—Having examined various pieces of copper, by means of the suspended magnetic needle, and having never found them magnetical, except only sometimes in such places as had been filed, and where some particles of steel might have been left by the file, he next proceeded to hammer some pieces of it, not only in the usual way, but also between flints: the result however was very dubious; for though generally they had no effect whatever on the needle, yet sometimes he thought the needle was really attracted by some pieces of hammered copper; but then this attractive power was so exceedingly small as not to be depended on. Zinc, either not hammered, or hammered as far as could be done without breaking it, showed no signs of magnetism whatever, when presented to the magnetic needle. Neither had a mixture of zinc and tin any action on the needle. A piece of a broken reflector of a telescope, which consisted of tin and copper; a mixture of tin, zinc, and a little copper; a piece of silver, both soft and hammered; a piece of pure gold, both soft and hammered; a mixture of gold and silver, both hard and soft; and another mixture of a great deal of silver, a little copper, and a less quantity of gold, either before or after hammering, had not the least action on the magnetic needle. Platina was the metal he last examined, and the experiments made with it seem to deserve particular attention, as they showed sometimes some small degree of magnetism in this substance.

As far as I could observe, says Mr. C., those pieces which would not acquire any magnetism by hammering, had not a very shining appearance before the hammering, though afterwards they could not be distinguished from the others by their appearance; and they seemed not to spread under the hammer so easily as the others. In general 3 or 4 strokes are sufficient to render a grain of platina evidently magnetic, but about 10 strokes give it the full power it is susceptible of.

If it be true, as those experiments seem to prove beyond a doubt, that magnetism may exist, or may belong to other substances, independent of iron, it must follow, that the attraction of a few particles of an unknown substance by the magnet is not a sure sign of the presence of iron. Hence those substances, which hitherto have been considered as containing ferrugineous particles, for no other reason but because the magnet attracted a small quantity of them, must be considered as dubious; and the conclusion of the existence of iron should not be admitted, except when those particles, which have been separated by the magnet, appear to be iron by some other trial; for though it is true, that iron is always attracted by the magnet, yet it does not hence follow, that whatever is attracted by the magnet must be iron. After scrutinizing this matter by still further experiments, Mr. C. concludes: It seems therefore to be demonstrated, as far as the subject will admit of demonstration, that the magnetism acquired by brass, when hammered, is not owing to iron contained in it; and consequently that magnetism, or the power of being attracted by, and attracting, the magnet, may exist independent of iron.

In the course of my experiments on the magnetism of brass, I have twice observed the following remarkable circumstance: A piece of brass, which had the property of becoming magnetic by hammering, and of losing the magnetism by softening, having been left in the fire till it was partially melted, I found, on trial, that it had lost the property of becoming magnetic by hammering; but having been afterwards fairly melted in a crucible, it thereby acquired the property it had originally, viz. that of becoming magnetic by hammering. I have also often observed, that a long continuance in a fire so strong as to be little short of melting hot, general diminishes, and sometimes quite destroys, the property of becoming magnetic in brass. At the same time the texture of the metal is considerably altered, becoming what some workmen call rotten. From this it appears that the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts, than to the admixture of any iron; which is confirmed still further by observing, that Dutch plate-brass (which is made not by melting the copper, but by keeping it in a strong degree of heat while surrounded by lapis calaminaris) also possesses that property; at least all the pieces of it which I have tried, have that property.

Further dissertations on this matter may be consulted in the latter part of Cavallo's Treatise on Magnetism.

IV. On Infinite Series. By Edw. Waring, M. D., F. R. S. p. 81.

In the paper, before printed in these Transactions, on Summation of Series, is given a method of finding the sum of a series, whose general term $\frac{P}{Q}$ is a determinate algebraical function of the quantity z, the distance from the first term of the series, which always terminates when the sum of the series can be expressed in finite terms. The terms of every infinite series must necessarily be given by a function of z, or by quantities which can be reduced to a function of z. Dr. W. here pursues the same intricate subject at considerable length, as in his former papers printed in these Abridgments, and in his separate works.

In the 2d part of this paper he observes that the doctrine of proportional parts was probably very early known in the æra of science; for when men could not find the exact value of a quantity, they were induced to find near approximations by trials, and thence by proportion an approximation still nearer: which method is commonly denominated the rule of false. This was often found to deviate considerably from the exact value; and the same operation was repeated, which frequently produced a nearer approximate value, and so on. This method of approximations, the most general yet known, has been used in resolving problems by several of the most eminent mathematicians in different ages, and in this particularly by M. Euler.

The following observation, he believes, was first published in the Meditationes, in the year 1770, viz. that the convergency of the approximate values, found by the rule of false and method of infinite series, generally depended on this, viz. how much nearer the approximate assumed is to one value of the quantity sought, possible or impossible, than to any other, and not to the quantity itself: hence, when two or more (n) values of the quantity sought are nearly equal, it is necessary to recur to more difficult rules, viz. to 3 or more trials; as, for example, let 2 roots be nearly equal, and write a, $a + \pi$, and $a + \varrho$, for the unknown quantity in the given equation made $= 0$, and let the quantities resulting be A, B, and C, then will more near approximations to the two roots nearly equal of the given equation be $a +$ the two roots (x) of the quadratic

$(\frac{A}{\pi\rho} + \frac{B}{\pi(\pi - \rho)} + \frac{C}{\rho(\rho - \pi)}) x^2 - (A \times \frac{\pi + \rho}{\pi\rho} + B \times \frac{\rho}{\pi . (\pi - \rho)} + \frac{C\pi}{\rho . (\rho - \pi)})x + A = 0$: for write 0, π, and ρ, respectively for x in the equation, and there will result the quantities A, B, and C. He then gives some general approximating equated values of the roots of equations, which he says will nearly be the same as found, where a near approximate is given, from the method given by Vieta, Harriot, Oughtred, Newton, De Lagny, Halley, &c.

Sir Isaac Newton found the sum A, of the $2n^{th}$ power of each of the roots of a given equation, and then extracted the $2n^{th}$ root of A, viz. $\sqrt[n]{A}$, for an approximate value of the greatest root of the equation; and further added some similar rules on the same principle. In the Miscell. Analyt. and Meditationes the same principle is applied in different rules for finding approximates to the greatest and other roots of the given equation; and also limits of the ratios of the approximate values of the roots found by these rules to the roots themselves are given. It is observed in the Meditationes, that from these rules in general to find the greatest root, it is often necessary that the greatest possible root be greater than the sum of the quantities contained in the possible and impossible part of any impossible root of the given equation: for example, if $a + b\sqrt{-1}$ be an impossible root of the given equation, then it is necessary that the greatest possible root be greater than $a + b$. It may further be observed, that in equations of high dimensions, unless purposely made, it is probable that the number of impossible will greatly exceed the number of possible roots; and consequently these rules most commonly fail.

M. Bernoulli assumed a fraction whose numerator is a rational function of the unknown quantity, and denominator the quantity which constitutes the equation; and reduced the fraction into a series, whose terms proceed according to the dimensions of the unknown quantity; and thence found an approximate value of the greatest or least root of the given equation or its reciprocal, by dividing the co-efficient of any term of the series resulting by the co-efficient of the preceding or subsequent term.

The rule of false has been found very useful in finding approximates to the two unknown quantities contained in two given equations, and has been applied to n equations having n different unknown quantities: for example, it has been observed, that if 2 or more m values of an unknown quantity x are nearly equal to each other, and to its given approximate value x', the unknown quantity $v = x - x'$ will ascend to 2 or more m dimensions in one of the resulting equations; or in more equations than one will be contained such powers of the quantity v, that if the more equations were reduced to one whose unknown quantity is v, the resulting equation will contain m dimensions of the quantity v. Hence it appears, that in this case also the convergency of the approximate values found will depend on the given approximate being much more near to one root than to any other.

In the 3d part Dr. W. remarks that the first algebraists divided quantities, and extracted their roots, no further than the quantities themselves: they did not perceive the utility of proceeding any further, otherwise the operation would have been the same continued. Mr. Gregory St. Vincent, and Mr. Mercator divided, and Sir Isaac Newton divided and extracted the roots of quantities, in which only one unknown quantity x is contained, by the operations then used by arithmeticians, into series ascending or descending, according to the dimensions of x in infinitum. They clearly saw the utility of it in finding the fluents of fluxions, as Dr. Wallis and others some little time before had found the fluent of the fluxion $ax^{\frac{m}{n}}\dot{x}$; or, which is the same, the area of a curve whose ordinate is $ax^{\frac{m}{n}}$ and abscissa is x. M. Leibnitz asked from Mr. Newton the cases in which the above-mentioned serieses would converge; for it would be altogether useless when they diverge, and of little use when they converge slowly.

To this question an answer, Dr. W. believes, was first given in the Meditationes, viz. reduce the function to its lowest terms; and also in such a manner that the quantities contained in the numerator and denominator may have no denominator: make the denominator $Q = 0$, and every distinct irrational quantity contained in it $= 0$; and also every distinct irrational quantity H contained in the numerator $= 0$; then, let α be the least root, affirmative or negative, (but not $= 0$) of the above-mentioned resulting equations, the ascending series will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the above-mentioned series will not converge. If the above-mentioned series, S, be multiplied into $\dot{x}$, and its fluent found; then will the series denoting the fluent contained between two values a and b, of the quantity x, converge, when a and b are both contained between α and $-\alpha$: the fluent always converges faster than the series S, the unknown quantity x having the same values in both. The infinite series $a^m + ma^{m-1}x + m \cdot \frac{m-1}{2} a^{m-2}x^2 +$ &c. $= (a + x)$ will always converge when a is greater than x, and diverge when less; and consequently its convergency does not depend on the index m, unless when $x = \pm a$: and in the Meditationes Analyticæ are given the cases in which it converges or diverges when $\mp a = x$; and also if the series $x^m + max^{m-1} +$ &c. $= (x + a)^m$ descends according to the dimensions of x, when it converges or diverges.

Sir Isaac Newton, in the binomial theorem, reduced the power or root of a binomial into a series proceeding according to the dimensions of the terms contained in the binomial. M. de Moivre reduced the power or root of a multinomial into a like series; but in all cases, except the most simple, we must still recur to the common division, extraction of roots, &c. Messrs. Euler, Maclaurin, and other mathematicians, finding tha the serieses before-mentioned often converged slowly, or, if the truth may be confessed, commonly not at all, to deduce the area of a curve contained between two values a and b of the abscis, or fluent of a fluxion between two values a and b of the variable quantity x, interpolated the series or area between a and b; that is, found the area or fluent contained between the abscissæ a and $a + \alpha$, then between the abscissæ $a + \alpha$ and $a + 2\alpha$, and then between the abscissæ $a + 2\alpha$ and $a + 3\alpha$, and so on, till they came to the area between $b - \alpha$ and b. M. Euler observed, that when the ordinate became 0 or infinite, the series expressing the area converges slowly; and therefore, in order to investigate the area near the points of the absciss where the ordinates become 0 or infinite, he transforms the equation, and finds serieses expressing the area near those points, in which serieses the abscissæ or unknown quantities begin from those points.

In the Meditationes it is asserted, that in a series proceeding according to the dimensions of x, if any root f the above-mentioned equations be situated between the beginning of the absciss 0 and its end x, the series will not converge; it is therefore necessary to transform the absciss so that it may begin or end at each of the roots of the above-mentioned equations, and consequently where the ordinates become 0 or infinite, &c.; those cases excepted where the ordinate becomes 0, and its correspondent abscissa is a root of a rational function W of x without a denominator, and $\int \mathrm{WP}\dot{x}$ is equal to the given series; and in general the abscissæ ought to begin from the above-mentioned points; for if they end there, the series will converge very slow, if at all. It is further asserted, that if a and b, the values of the abscissæ between which the area is required, be both more near to one root of the above-mentioned equations than to any other, and n serieses are to be found, whose sum expresses the area contained between a and b; then that the sum of the n serieses may converge the swiftest, the distances of the beginnings of each of the n abscissæ from the adjacent root will form a geometrical progression.

Mr. Craig found the fluent of any fluxion of the formula $(a + bx^n + cx^{2n} +$ &c.$)^m x^{\theta - 1}\dot{x}$ by a series of the following kind $(a + bx^n + cx^{2n} +$ &c.$)^{m+1} \times x^\theta \times (\alpha + \beta x^n + \gamma x^{2n} +$ &c. in infinitum). Sir Isaac Newton, by serieses of the same kind, found the fluents of fluxions of this formula $(a + bx^n + cx^{2n} +$ &c.$)^l \times (e + fx^n + gx^{2n} +$ &c.$)^m \times$ &c. $x^{\theta - 1}\dot{x}$; the same principle is extended somewhat more general in the Meditationes. Mr. John Bernoulli found the fluent of any fluxion $\int n\dot{z} = nz - \frac{z^2\dot{n}}{2\dot{z}} + \frac{z^3\ddot{n}}{2\,.\,3\dot{z}^2} -$ &c. from the principles which Mr. Craig published for finding the fluents of fluxions involving fluents. In the Meditationes something is added of the convergency of these series; and also, in them a new method is given of finding approximations. Let some terms in the given quantity be much less or greater than the rest; then reduce the quantity into terms proceeding according to the dimensions of the small quantities, or according to the reciprocals of the great quantities, and it is done. If the fluent of the quantity resulting be required, find it from the common methods, if possible; but if not, reduce the terms not to be found into an infinite series, and then find approximate values to each of the terms, &c. M. Euler, and others, reduced the series $Ax^r + Bx^{r+s} + Cx^{r+2s} +$ &c. into a series A' sin. $r\alpha$ + B sin. $(r+s)\alpha +$ &c. &c. where α denotes the arc of a circle, whose sine is ax, &c. It may be easily reduced into infinite other serieses proceeding according to the dimensions of quantities, which are functions of x; but it is most commonly preferable to reduce it into serieses proceeding according to the sines, cosines, tangents, or secants of the arcs of circle, which sines, &c. can immediately be procured from the common tables.

It has been observed in the first part, that to find the root of an equation, an approximate value much more near to one root of the equation than to any other must be given. In this part it is further observed, that serieses deduced from expanding given quantities, so as to proceed according to the dimensions of the unknown or variable quantities, will not converge if the unknown quantities be greater than the least roots of the above-mentioned equations; and that they will not converge much, unless the unknown quantities have a small proportion to the least roots: and if the given quantities be expanded into serieses descending according to the dimensions of the unknown quantities, then the serieses resulting will not converge if the greatest roots of the equations before-mentioned be greater than the unknown quantities; and unless the unknown quantities have a great ratio to the greatest roots the serieses will converge slowly: for example, the serieses $\int \frac{\dot{x}}{1+x} = x - \frac{1}{2}x^2 +$ &c., $\int \frac{\dot{z}}{1+z^2} = z - \frac{1}{3}z^3 + \frac{1}{5}z^5 -$ &c., $\int \frac{\dot{y}}{\sqrt{1-y^2}} = y + \frac{1}{6}y^3 +$ &c. will never converge if x, z, or y, be greater than 1; but will always converge when less than ± 1 or $\pm 1\sqrt{-1}$ the least or only roots of the equations $1 + x = 0$, $1 - y^2 = 0$, and $1 + z^2 = 0$. The series $y + \frac{1}{6}y^3 +$ &c. will always converge when y is situated between $+1$ and -1, in which case alone it is possible. The same is true also of a series arising from expanding the $\int (ax^m + bx^{m-1} + cx^{m-2} + \text{&c.})^{\frac{1}{2}m}\dot{x}$ into a series proceeding according to the dimensions of x, if the equation $ax^m + bx^{m-} + cx^{m-2} +$ &c. $= 0$ have only 2 possible roots α and $-\alpha$, which are less in the manner before-mentioned than any impossible root contained in it.

If in either of the above-mentioned serieses the unknown quantity x, z, or y, has a great proportion to 1, the series will converge very slow; for example, if $x = 1$, ten thousand numbers at least are to be calculated, to procure the sum of the series true to 4 figures; therefore, in these and most other serieses, it is necessary first to find a near value, viz. when x either $= z$, when e is very small;

or $= e$, when z is very small; and then write $z + e$ for x in the quantity, and reduce it in the former case into a series proceeding according to the dimensions of e, in the latter case according to the dimensions of z, and there will arise 2 serieses, of which the fluents properly corrected, viz. by adding the fluent contained between the values a and e to the latter, and that between a and z to the former, will give the same fluent. The first term of the series, in which e is supposed very small, will be the fluent of the given fluxion, when $x = z$.

If a fluxion $P\dot{x}$, where P is a function of x, be transformed into another $Q\dot{z}$, where Q is a function of z, and they be reduced into serieses A and B, proceeding according to the dimensions of x and z respectively; find α and π, correspondent values of the quantities x and z; then in ascending serieses, if α has a less ratio to the least root of the equation $P = 0$, than π has to the least root of the equation $Q = 0$, the series A (exceptis excipiendis) will converge swifter than the series B.

Dr. Barrow, in some equations, expressing the relation between the absciss x and ordinate y, found y in the first 2 terms of x, viz. $y = a + bx$, which is an equation to the asymptotes of the curves. Sir Isaac Newton, from an algebraical equation given, expressing the relation between y and x, found a series proceeding according to the dimensions of x, expressing y in terms of x. M. Leibnitz performed the same problem by assuming a series $Ax^n + Bx^{n+r} + Cx^{n+2r} +$ &c. with general co-efficients, and substituting this series for y in the given equation, &c. from equating the correspondent terms he deduced the indexes and co-efficients. M. De Moivre, Mr. Maclaurin, &c. observed, that when the highest terms of the given equations have 2 or more (m) divisors equal; for example, $(y - ax^n)^m$; to which we must add, and when a value of y in this case is required nearly equal to Ax^n, a series $Ax^n + Bx^{n+\frac{r}{m}} +$ &c. is to be assumed, whose indexes differ only by $\frac{r}{m}$, &c. if otherwise they would differ by r. This reduction seldom answers any other purpose than finding the fluents of fluxions, as $\int y\dot{x}$, &c.; or the asymptotes, &c. of curves, which depend on some of the first terms of the series; but will very seldom be used for finding the roots of an equation. The rule of false, or method given by Vieta, will ever be substituted in its stead.

The values of x may be required between which the above-mentioned series $Ax^n + Bx^{n+r} + Cx^{n+2r} +$ &c. will converge, as the infinite series answers no purpose when it diverges. First, if an ascending be required, write for y in the given algebraical equation an infinite quantity, and find the roots of x in the equation thence resulting $P = 0$; which for y write in the same equation, and find the roots of x in the resulting equation which contain irrational quantities, viz. if one root be $x = a$; then let it contain $(x - a)^m$, where m is not a whole number; find the roots of the equations resulting from making every irrational function of x contained in the given equation $= 0$, there being no irrational function of y contained in it; then, if x be greater than the least root not $= 0$ of the above-mentioned equations, the series will not converge; but if it be a series descending according to the dimensions of x, and x be less than the g eatest root of the above-mentioned equations, the series will not converge.

In interpolating serieses to investigate the fluent contained between two values a and b of the fluxion $(\text{A}x^n + \text{B}x^{n+r} + \&c.)\ t\dot{x}$, it is preferable to make the abscissæ begin from every one of the above-mentioned roots contained between a and b. Most commonly these serieses will not converge unless x be less, &c. than other quantities not investigated by this rule.

Sir Isaac Newton gave an elegant example of this rule in the reversion of the series, $y = ax + bx^2 + cx^3 + \&c.$ from which the investigation of the law of the series has never been attempted. In the year 1757 I sent the first edition of my Meditationes Algebraicæ to the R. S., and published it in 1760, and afterwards in 1762, with another part added, on the properties of curves, under the title of Miscellanea Analytica, in all which was given the law of a series for finding the sum of the powers of the roots of an equation from its co-efficients. That great mathematician M. Le Grange and myself printed about the same time an observation known to me at the time that I printed the above-mentioned book, that the law of its powers and roots, if it proceeds in infinitum, is the same; from which series of mine, with great sagacity, M. Le Grange found the law which Sir Isaac Newton's reversion of series observes. In the Meditationes the law is given, and the series is made to proceed according to the dimensions of x, &c.

It is asserted in the Meditationes, that in most equations of high dimensions, unless purposedly constituted, the sum of the terms which, from the given hy pothesis, become the greatest, being supposed $= 0$, only an approximate to the value $\text{A}x^n$ of y in the resulting equation can by the common algebra be deduced. In this case the approximate to the quantity A is to be found so near as the approximate value of the quantity sought requires; or perhaps it is preferable to correct in every operation the approximate values of the quantities A, B, C, &c. in the series required $\text{A}'x^n + \text{B}'x^{n+r} + \text{C}'x^{n+2r} + \&c.$ In the equation the quantity $z \pm e$ may be substituted for x, and from the equation resulting a series expressing the value of y may be found, proceeding either according to the dimensions of the quantity z, or its reciprocals, according to the conditions of the problem.

If there be more than one (n) equations having $n + 1$ unknown quantities, x, y, z, &c., in each of the equations, for y, z, &c. write respectively $\text{A}x^n$, $\text{A}'x^m$, &c.; and suppose the terms of each of the equations, which result the greatest from the given or assumed hypothesis = 0, and from the resulting equations may be found the first approximates Ax^n, $A'x^m$, &c. either accurately or nearly; then, in the given equations for y, z, &c. write $y' + (A + a)\, x^n + Bx^{n+n'}$ + &c. $z' + (A' + a')\, x^m + B'x^{m+m'}$, where a, a', &c. are very small quantities; and suppose the terms of each of the equations which become greatest from the above-mentioned hypothesis respectively = 0, and from the equations resulting deduce the quantities a, a', &c.; n', m', &c.; B, B', &c.; and so on: or assume $y = (A + 1a + a1 + \&c.)\, x^n + (B + 1b + b1 + \&c.)\, x^{n+n1} + \&c.$; $z = (A' + 1a' + a'1 + \&c.)\, x^m + (B' + 1b' + b'1 + \&c.)\, x^{m+m1} + \&c.\ \&c.$; substitute these quantities for their values in the given equations, and from equating the correspondent terms of the resulting equations may be deduced the quantities required.

The differences of the indexes n', &c. m', &c. may be deduced by writing x^n, x^m, &c. for y, z, &c. in the given equations, from the differences of the indexes of the quantities resulting. The same principles may be applied in finding the above-mentioned differences, when two or more values are Ax^n, &c. which were applied in a like case to one equation having two unknown quantities. The same principles which discover the cases in which a series deduced from an equation having two unknown quantities will converge, may be applied for the same purpose to these series.

In finding the series which expresses the value of y in terms of x, there will always occur as many invariable quantities to be assumed at will, as is the order of the fluxional equation, provided the series begins from its first terms; and to find them there will result equations easily reducible to homogeneous fluxional equations, of which the orders do not exceed m.

*V. Experiments on Hepatic Air.** *By Rich. Kirwan, Esq. F.R.S.* p. 118.

Hepatic air is that species of permanently elastic fluid which is obtained from combinations of sulphur with various substances, as alkalis, earths, metals, &c. It possesses many peculiar and distinct properties; among which the most obvious are, a disagreeable characteristic smell emitted by no other known substance; inflammability, when mixed with a certain proportion of respirable or nitrous air; miscibility with water, to a certain degree; and a power of discolouring metals, particularly silver and mercury. These properties were first discovered by that incomparable analyst Mr. Scheele. This air acts an important part in the economy of nature. It is frequently found in coal-pits; and the truly excellent and ever to be regretted M. Bergman has shown it to be the principle on which the sulphureous properties of many mineral waters depend, and thus happily terminated the numerous disputes which the obscurity of that

* Termed sulphuretted hydrogen gas, in the new chemical nomenclature.

subject had occasioned. There is also great reason to think that it is the peculiar product of the putrefaction of many, if not all, animal substances. Rotten eggs and corrupt water are known to emit the smell peculiar to this species of air, and also to discolour metallic substances in the same manner. M. Viellard has lately discovered several other indications of this air in putrefied blood.

Yet, deserving as this substance appears to be of a thorough investigation, it has as yet been very little attended to. The experiments of M. Bergman have not been sufficiently numerous, and thence he has been led into some mistakes. Dr. Priestley has entirely overlooked it. The researches of the ingenious Mr. Sennebier, of Geneva, have indeed been very extensive; but as, for particular reasons, he operated on this air over water, by which it is in great measure absorbed, instead of quicksilver, his conclusions appear in many respects objectionable. The experiments now laid before the Society were all made over quicksilver, and several times repeated.

Of the Substances that yield hepatic air, and the means of obtaining it.—It is well known, that saline liver of sulphur is formed, in the dry way, of a mixture of equal parts of vegetable or mineral alkali and flowers of sulphur, melted together by a moderate heat, in a covered crucible. When this mixture was slightly heated, it emitted a bluish smoke, which gradually became whiter as the heat was increased, and at last, when the mixture was perfectly melted, and the bottom of the crucible slightly red, became perfectly white and inflammable. To examine the nature of this smoke, Mr. K. made a pretty pure fixed alkali, by deflagrating equal parts of cream of tartar and nitre in a red-hot crucible in the usual way; and mixing this salt perfectly dry with flowers of sulphur in much smaller quantity, he gradually heated the mixture in a small coated glass retort, and received the air proceeding from it over quicksilver. The first portion of air that passed with a very gentle heat, was that of the retort itself, slightly phlogisticated. It amounted to 1.5 cubic inches, and with Dr. Priestley's nitrous test, (that is, an equal measure of nitrous air) its goodness was 1.29. It contained no fixed air. The 2d portion of air obtained by increasing the heat amounted to about 18 cubic inches. It was of a reddish colour, and seemed a mixture of nitrous and common air. It slightly acted on the mercury. The 3d portion consisted of 20 cubic inches, and appeared to be of the same kind, but mixed with a little fixed air. This was succeeded by 64 cubic inches of almost perfectly pure fixed air; and the bottom of the retort being now red, some sulphur sublimed in its neck. When all was cold, liver of sulphur was found in the bulb of the retort.

Hence we see that the blue smoke consists chiefly of fixed air, and the white or yellow smoke of sulphur sublimed; and that no hepatic air is thus formed; nor vitriolic air, unless the retort be so large as to contain a sufficiency of common air to admit the combustion of part of the sulphur. 2dly, That the aërial, or any other acid, combined with the alkali, must be expelled, before the alkali will combine with the sulphur. Liver of sulphur exercises a strong solving power on the earth of crucibles, and readily pierces through them.

The best liver of sulphur is made of equal parts of salt of tartar and sulphur; but as about $\frac{1}{3}$ of the salt of tartar consists of air which escapes during the operation, it seems that the proportion of sulphur predominates in the resulting compound; yet as some of the sulphur also sublimes and burns, it is not easy to fix the exact proportion. 100 grs. of the best, that is, the reddest liver of sulphur, afford, with dilute marine acid, about 40 cubic inches of hepatic air, in the temperature of 60°: a quantity equivalent to about 13 grs. of sulphur.

The marine acid is the best adapted to the production of hepatic air. If the concentrated nitrous acid be used, it will afford nitrous air; but having diluted some nitrous acid, whose specific gravity was 1.347, with 20 times its bulk of water, he obtained, with the assistance of heat, as pure hepatic air as with any other acid. The concentrated vitriolic acid, poured on liver of sulphur, afford but little hepatic air without the assistance of heat, though it instantly decomposes the liver of sulphur; and it is partly for this reason that the proportion of air is so small; for it is during the gradual decomposition of sulphureous compounds that hepatic air is produced. Distilled vinegar extricates this air in the temperature of the atmosphere; but it is not pure, its peculiar smell being mixed with that of vinegar. The acid of sugar also produced some quantity of this air in the temperature of 59°. Having made some liver of sulphur, in which the proportion of sulphur much exceeded that of the alkali, Mr. K. poured on part of it some oil of vitriol, whose specific gravity was 1.863: by this means he obtained hepatic air, so loaded with sulphur, that it deposited some in the tube through which it was transmitted, and on the upper part of the glass receiver. This air he transferred to another receiver; but though it was then perfectly clear and transparent, and amounted to 6 cubic inches, yet the next morning the inside of the glass was thickly lined with sulphur, and the air reduced to 1 cubic inch, which was pure vitriolic air. He also procured this air from a mixture of 3 parts filings of iron and one of sulphur, melted together, and treated with marine acid. It is remarkable, that this sulphurated iron, dissolved in marine acid, affords scarce any inflammable, but mostly hepatic air. A mixture of equal parts filings of iron and sulphur, made into a paste with water, after heating and becoming black, afforded hepatic air when an acid was poured on it; but this hepatic air was mixed with inflammable air, which probably proceeded from the uncombined iron. After a few days, this paste lost its power of producing hepatic air. Mr. Bergman has remarked, that combinations of sulphur with some other metals yield hepatic air.

Mr. K. attempted to extract air from a mixture of oil of olives with caustic vegetable alkali. It immediately whitened, and on applying heat effervesced so violently as to pass over into the receiver: nor had he better success on adding an acid. The event was different when on a few grains of sulphur he poured some of the oil, and heated them in a phial with a bent tube; for as soon as the sulphur melted, the oil began to act on it, got red, and emitted hepatic air, similar to that produced by other processes. He also obtained this air in great plenty from a mixture of equal parts sulphur and pulverized charcoal, out of which its adventitious air had been as much as possible expelled by keeping it a long time heated to redness, in a crucible on which a cover was luted, with a small perforation to permit the air to escape. This air was inflammable, as appeared by holding a lighted candle before it during its emission; yet it is hardly possible to free charcoal wholly from foreign air, for it soon re-attracts it when exposed to the atmosphere. This last mixture, when distilled, affording a large quantity of hepatic and some inflammable air.

Six grs. of pyrophorus, made of alum and sugar, effervesced with marine acid, and afforded 2.5 cubic inches of hepatic air. This pyrophorus had been made 6 years before, and was kept in a tube hermetically sealed, and for many summers exposed to the strongest light of the sun. It was so combustible, that some grains of it took fire while it was introduced into the phial out of which the hepatic air was expelled. A mixture of 2 parts white sugar (previously melted in order to free it from water) with 1 part sulphur, when heated to about 600 or 700 degrees, gave out hepatic air very rapidly. This air had a smell much resembling that of onions; it contained no fixed air, nor saccharine, or other acid. But sugar and sulphur, melted together, gave out no hepatic air when treated with acids. Water, spirit of wine, and marine acid, decompose this mixture, dissolving the sugar, and leaving the sulphur. A mixture of sulphur and plumbago afforded no hepatic air.

On the General Characters of Hepatic Air.—Mr. K. found the absolute weight of this air by weighing it in a glass bottle, previously exhausted by Mr. Hurter's new improved air-pump, whose effect is so considerable as to leave in general only $\frac{1}{800}$ and frequently but $\frac{1}{1000}$ part of unexhausted air. This bottle contained nearly 116 cubic inches; and this quantity of hepatic air weighed 38.58 grains, the thermometer being then 67°.5, the barometer 29.94, and M. Saussure's hygrometer 84°, the weight of 116 cubic inches of atmospheric air being at the same time 34.87 grs; hence a cubic foot of hepatic air weighs, in these circumstances, 574.7089 grains, and 100 cubic inches of it weigh about 33 grains: and its weight, relatively to that of common air, is as 10000 to 9038.* This hepatic air was extracted from artificial pyrites by marine acid.

* Hence the weight, says Mr. K., which I have assigned to common air in my first paper, after

The inflammability of this air has been already mentioned. It never detonates with common air; nor can it be fired in a narrow-mouthed vessel, unless mixed with a considerable proportion of this air. M. Scheele found it to inflame when mixed with $\frac{2}{3}$ of this air. According to M. Sennebier it cannot be fired by the electric spark, even when mixed with any proportion of respirable air. Mr. K. found a mixture of one part of hepatic air and 1.5 of common air to burn blue, without flashing or detonating. During the combustion sulphur is constantly deposited, and a smell of vitriolic air is perceived. A mixture of half hepatic and half nitrous air burns with a bluish, green, and yellow lambent flame; sulphur is also deposited, and in proportion as this is formed, a candle dipped in this air burns more weakly, and is at last extinguished. A mixture of 2 parts nitrous and 1 of hepatic air partially burns, with a green flame, and a candle is extinguished in the residuum, which then reddens on coming in contact with atmospheric air. He made a mixture of 1 part nitrous and one part hepatic air, and to this admitted 1 part also of common air; the instant the common air was introduced, sulphur was precipitated, and the 3 measures occupied the space of 2.4 measures; this burned on the surface with a wide greenish flame, but the candle was extinguished when sunk deeper. A mixture of 4 parts common air and 1 part hepatic burned blue and rapidly; but a mixture of 1 part dephlogisticated and 1 part hepatic, which had stood 8 days, went off with a report as loud as that of a pistol, and so instantaneously that the colour of the flame could scarcely be discerned.

With respect to solubility in water, hepatic airs extracted from different materials differ considerably. In the temperature of 66°, water dissolves, by slight agitation, $\frac{2}{3}$ of its bulk of alkaline or calcareous hepatic air, extracted by marine acid; $\frac{3}{4}$ of its bulk of martial hepatic air, extracted by the same acid; $\frac{8}{10}$ of that extracted by means of the concentrated vitriolic acid, or the dilute nitrous or saccharine acids in the temperature of 60°; $\frac{7}{10}$ of sedative hepatic air; $\frac{9}{10}$ of acetous hepatic air, and of that afforded by oil of olives; and of its own bulk of that produced from a mixture of sugar and sulphur. In general, I imagined that which required most heat for its production to be most soluble: though in some instances, particularly that of acetous hepatic air, that circumstance does not take place. But the most remarkable phenomenon attending the union of hepatic air with water is, that it is not permanent. Even water, out of which its own air had been boiled, in a few days after saturation with hepatic air grows

M. Fontana, is evidently erroneous; and indeed, by that determination, common air would not be even 700 times lighter than water, in the temperature of 55°, and the barometer 29.5, which contradicts all barometrical and aerostatic experiments: and I cannot omit mentioning the very near agreement of the weight of common air here found with that resulting from the calculation of Sir George Shuckburgh, it is so great as to differ only by 2 grains in a cubic foot.—Orig.

turbid, and in a few weeks deposits most of it in the form of sulphur, though the bottle be ever so well stopped, or stand inverted in water or mercury. Yet water no way decomposes hepatic air by absorbing it; for the part left unabsorbed by a quantity of water is absorbable by a larger quantity of water, and burns like other hepatic air. Heat does not expel this air from water, till carried to the degree of ebullition.

Of all the tests of hepatic air, the most delicate and sensible is the solution of silver in the nitrous acid. This, according as the nitrous acid is more or less saturated with silver, becomes black, brown, or reddish brown, by contact with hepatic air however mixed with any other air or substance. When the acid is not saturated, or is in large proportion, the brown or black precipitate, which is nothing but sulphurated silver, is re-dissolved.

Of the Action of Hepatic and other Aerial Fluids on each other.—Six cubic inches of common and 6 of hepatic air being mixed with each other, and standing over mercury for 8 days, were not in the least altered in their dimensions or otherwise; though a diminution of a $\frac{1}{120}$ part might be perceived. The mercury was slightly blackened. The event was the same when 3 measures of common and 1 of hepatic air were used. Water took up the hepatic air. No fixed air was found. Five measures of hepatic and 5 of dephlogisticated air, so pure that one measure of it and 2 of nitrous air made only $\frac{3}{10}$ of a measure, remained unaltered for 8 days, the mercury only being blackened. No fixed air was produced, nor the dephlogisticated air phlogisticated. When the mixture was fired, it went off all at once with a loud report. Four measures of phlogisticated and 4 of hepatic air remained unaltered for 16 days: water then took up the hepatic, and left the phlogisticated air. Four measures of inflammable and 4 of hepatic air remained unaltered for 6 days. Two measures of hepatic and 2 of marine acid air suffered no diminution in 3 days. The mercury on which they stood was not blackened. Water took up both, and precipitated the solution of silver black. The same quantity of hepatic and fixed air remained 4 days without any sensible diminution. Four measures of water absorbed the greater part of both, had an hepatic smell, precipitated lime from its solution, and also silver, as usual. The residuum extinguished a candle.

But vitriolic, nitrous, and alkaline airs had very remarkable effects on hepatic air. Two measures of hepatic being introduced to 2 of vitriolic air, a whitish yellow deposition immediately covered both the tops and sides of the jar, and both airs were, without any agitation, reduced to little more than 1 measure; but the opacity of the incrusted glass prevented the ascertaining the diminution with precision. Hence he repeated this experiment more at large, in the following manner. To 5 measures of vitriolic air (each measure containing a cubic inch) he added 1 of hepatic air. In less than a minute, without any agitation, the sides of the glass were covered with a whitish scum, which seemed moist, and a diminution took place of more than 1 measure. In 4 hours after, he introduced a second measure of hepatic air, which was followed by a similar diminution and deposit. The next day he added 3 more measures of this last, at the interval of about 4 hours between each; and still finding a considerable diminution after each, the following day he added another measure; the diminution produced by this last appeared not to exceed 1 measure. He then poured off the residuary air into another jar, and found it not to exceed 3 measures, namely, 5 of vitriolic and 6 of hepatic air, were reduced to 3. Into one measure of this residuary air he introduced a lighted candle: it was immediately quenched. To the 2 remaining measures he added 1 measure of water: by agitation it took up $\frac{4}{10}$ of its bulk. To part the remainder he added nitrous air, which had no effect on it. Another part of it extinguished a candle. It had not a vitriolic smell.

With nitrous air Mr. K. made the following experiments. First, he found that 2 measures or cubic inches of nitrous and 2 of hepatic air were little altered when first mixed, even by agitation; but after 36 hours both were reduced to rather more than $\frac{1}{3}$ of the whole. Yellow particles of sulphur were deposited both on the mercury, and on the sides of the jar, but the mercury was not blackened. The residuary air had still an hepatic smell, and was somewhat further diminished by water; and in the unabsorbed part a candle burned naturally. The water had all the properties of hepatic water.

On the Action of Hepatic Air, and Acid, Alkaline, and Inflammable Liquids, on each other.—One measure of oil of vitriol, whose specific gravity was 1.863, absorbed 2 measures of hepatic air except $\frac{1}{10}$. The acid was whitened by a copious deposition of sulphur. On introducing, over mercury, a measure of red nitrous acid, whose specific gravity was 1.430, to an equal measure of hepatic air; red vapours instantly arose, and only $\frac{1}{10}$ or $\frac{1}{12}$ of a measure remained in an aërial form; but as the acid acted on the mercury, he was obliged to carry the jar into the water tub, by which means the whole was absorbed: no sulphur was here precipitated.

Finding it so difficult to subject hepatic air to the direct action of the concentrated nitrous acid, he diluted it to that precise degree at which it could not act on mercury without the assistance of heat, and then passed through it an equal bulk of the same hepatic air; the acid was whitened, and $\frac{8}{10}$ of the air absorbed, and the residuum detonated. Repeating the same experiment, with hepatic air from liver of sulphur, he found still more of it absorbed by the acid: but the residuum no longer detonated, but burned with a blue and greenish flame, and sulphur was deposited on the sides of the jar.

Distilled vinegar absorbs nearly its own bulk of air, and becomes slightly whitened; but by agitation it may be made to take up about its bulk, and then becomes very turbid. One measure of caustic vegetable alkali, whose specific gravity was 1.043, absorbed nearly 4 measures of alkaline hepatic air. It was at first rendered brown by it; but after some time it got clear, sulphur was deposited, and the surface of the mercury blackened. This shows that alkalis are not dephlogisticated by silver or other metals, as Mr. Baumé imagined, but only cleared of part of the sulphur, which they commonly contain, it being formed by the tartar vitriolate contained in the plant, and coal, during combustion. One measure of caustic volatile alkali, whose specific gravity was 0.9387, absorbed 18 of hepatic air. If the caustic liquor contained more alkali, it would absorb more hepatic air, as 6 measures of hepatic unite to 7 of alkaline air; and thus the strength of alkaline liquors, and their real contents, may be determined better than by any other method. Also the smoking liquor of Boyle, which is difficultly prepared in the usual way, may easily be formed by placing the volatile alkali in the middle glass of Dr. Nooth's apparatus for making artificial mineral waters, and decomposing artificial pyrites, or liver of sulphur, in the lower glass, by marine acid. Oil of olives absorbs nearly its own bulk of this air, and obtains a greenish tinge from it. But new milk scarcely absorbs $\frac{1}{10}$ of its bulk of this air, which is very remarkable, and is not in the least coagulated. Oil of turpentine also absorbs its own bulk of this air, and even more; but then becomes turbid. Water seems also to precipitate this air from it, for when shaken with it a white cloud appears. Spirit of wine, whose specific gravity was 0.835, absorbed nearly 3 times its bulk of this air, and became brown. By this means sulphur may be combined with spirit of wine much more easily than by Count Lauragais's method, the only hitherto known. Water precipitates the sulphur in part.

Of the Properties of Water saturated with Hepatic air.—This water turns tincture of litmus red. It does not affect lime-water. It does not form a cloud in the solution of marine, though it does in that of acetous baro-selenite. The solutions of other earths in the mineral acids are not altered in it. When dropped into a solution of vitriol of iron or marine salt of iron, it produces a white precipitate. In nitrous salt of copper it causes a brown precipitate, and the liquor is changed from blue to green: The precipitate re-dissolves by agitation: in vitriolic of copper it forms a black precipitate. The solution of tin in aqua regia is precipitated by it of a yellowish white colour; that of gold, black; that of regulus of antimony, red and yellow; that of platina, red mixed with white.

The solution of silver in the nitrous acid, and also that of lead, whether in acetous or nitrous acid, are precipitated black. If the solutions are not perfectly saturated with metal, the precipitates will be brown or reddish brown, and may be re-dissolved by agitation. The nitrous solution of mercury is precipitated of a yellowish brown; that of sublimate corrosive, yellow mixed with black; but by agitation it becomes white. The nitrous solution of bismuth becomes, by mixture with this water, reddish brown, and even assumes a metallic appearance; that of cobalt becomes dark; that of zinc, of a dirty white, that of arsenic, in the same acid, yellow mixed with red and white, orpiment and realgar being formed.

If oil of vitriol, whose specific gravity is 1.863, be dropped into hepatised water, it renders it slightly turbid; but if the volatile vitriolic acid be dropped into it, a bluish white and much denser cloud is formed in the water. Strong nitrous acid, whether phlogisticated or not, causes a copious white precipitation; but dilute nitrous acid produces no change. Green nitrous acid, whose specific gravity was 1.328 immediately precipitated sulphur from it. Strong marine acid produced a light cloud; but neither distilled vinegar nor acid of sugar had any effect.

Of the Properties of Alkaline Liquors, impregnated with Hepatic Air.— Colourless fixed alkaline liquors receive a brownish tinge from this air. The residuum they leave is of the same nature as the part they absorb. A caustic fixed alkaline liquor, saturated with this air, precipitates barytes from the acetous acid, of a yellowish white colour. It also decomposes other earthy solutions, and the colour of the precipitates varies according to their purity, and perhaps this test might be so far improved as to supply the place of the Prussian alkali.

It precipitates the solution of vitriol of iron, and also marine salt of iron, black; but this latter generally whitens by agitation. The solutions of silver and lead are also precipitated black with some mixture of white; that of gold is also blackened; but that of platina becomes brown. Solutions of copper let fall a reddish black or brown precipitate. Sublimate corrosive by this test discovers a precipitate partly white and black, and partly orange and greenish.

In the nitrous solution of arsenic it forms a yellow and orange; and in that of regulus of antimony, in aqua regia, an orange precipitate mixed with black. Nitrous solution of zinc, thus treated, shows a dirty white; that of bismuth a brown mixed with white; and that of cobalt a brown and black precipitate. As Prussian alkali always contains some iron, it gives a purple precipitate with this test, which precipitate is easily dissolved. It turns tincture of radishes, which is my test for alkalis, green.

Water saturated with the condensed residuum of alkaline hepatic air, that is, with the purest volatile liver of sulphur, does not precipitate marine selenite, though it forms a slight brown and white cloud in that of marine baro-selenite. It produces a black precipitate in the solution of vitriol of iron, and a black and white in that of marine salt of iron; but by agitation this last becomes wholly white. It precipitates both vitriol of copper, and the nitrous salt of copper, red, and brown. Tin in aqua regia gives a yellowish precipitate; gold a dilute yellow and reddish brown; platina a flesh-coloured precipitate; and regulus of antimony a yellowish red. Silver is precipitated black; and so is lead, both from the nitrous and acetous acids. Sublimate corrosive appeared for an instant red; but soon after its precipitate appeared partly black and partly white. The nitrous solution of bismuth affords also a precipitate, partly black, partly white, and partly reddish brown, and of a metallic appearance; that of cobalt is also black or deep brown. Arsenical solutions give yellow precipitates more or less red; but those of zinc only a dirty white.

Of the Constitution of Hepatic Air.—From an attentive consideration of the above experiments, it is difficult to conclude that hepatic air consists of any thing else than sulphur itself, kept in an aërial state by the matter of heat. Every attempt to extract inflammable air from hepatic air, when drawn from materials that previously contained nothing inflammable, namely, from alkaline or calcareous hepars, proved abortive: on the contrary, when the materials could previously supply inflammable air, as when martial carbonaceous and saccharine compounds were employed, inflammable air, in ever so small a proportion, was detected: nor could hepatic air be procured from the direct union of inflammable air and sulphur, as we have seen.

Of Phosphoric Hepatic Air.—As phosphorus, in respect to its constituent parts, bears a strong resemblance to sulphur, Mr. K. was naturally led to examine its phenomena when placed in the same circumstances: he therefore gently heated about 10 or 12 grains of phosphorus, mixed with about half an ounce of caustic fixed alkaline solution, in a very small phial, furnished with a bent tube, and received the air over mercury. On the first application of heat two small explosions took place, attended with a yellow flame and white smoke, which penetrated through the mercury into the receiver; these were followed by an equable production of air. At last the phosphorus began to swell and froth, and fearing the rupture of the phial, he stopped the tube to prevent the access of atmospheric air, and removed the phial to a water tub, intending to throw it in; but in the mean while the phial burst with a loud explosion, by reason of an obstruction in the tube, and a fierce flame immediately issued from it. However he obtained about 8 cubic inches of air.

This air was diminished very slightly, by agitation with an equal bulk of water, and then became cloudy like white smoke, but soon after recovered its transparency. On turning up the mouth of the tube to examine the water, the unabsorbed air instantly took fire, and burned with a yellow flame without exploding, leaving a reddish deposit on the sides of the tube. Water impregnated with phosphoric air, and over which this air had burned, slightly reddened tincture of litmus. Did not affect Prussian alkali. Had no effect on the nitrous solutions of copper or lead, zinc or cobalt, nor on marine solution of iron or tin, or of tin in aqua regia, nor on the vitriolic solutions of iron, copper, tin, lead, zinc, regulus of antimony, arsenic, or manganese; nor on the marine solutions of iron, copper, lead, zinc, cobalt, arsenic, or manganese. But it precipitated the nitrous solution of silver black, and vitriol of silver brown; also nitrous solution of mercury made without heat brown and black; but vitriol of mercury first became reddish, and afterwards white; and sublimate corrosive yellow and red mixed with white.

Gold dissolved in aqua regia is precipitated purplish black, and from the vitriolic acid brownish red and black; but regulus of antimony in aqua regia is precipitated white by this phosphorated water. The nitrous solution of bismuth showed first a white, and presently after a brown precipitate. Vitriol of bismuth and marine salt of bismuth were also precipitated brown; this latter re-dissolved by agitation. The nitrous solution of arsenic also became brown, but re dissolved by agitation.

Phosphoric air was scarce at all diminished by the addition of an equal measure of alkaline air; and water being put to these, took up in appearance little else than alkaline air; yet on turning up the mouth of the jar, the residuary air smoked without flaming. The water, thus impregnated, had exactly the smell of onions. It turned tincture of radishes green. It precipitated solution of silver black; and that of copper in the nitrous acid brown; but this precipitate was re-dissolved by agitation, and the liquor became green. Sublimate corrosive was precipitated yellow mixed with black. Iron was precipitated white, both from the vitriolic and marine acids; but a pale yellow solution of it in the nitrous acid was not affected; and a red solution of it in the same acid was only congrumated. Regulus of antimony in aqua regia gave a white, cobalt in nitrous acid a very slight reddish, and bismuth in the same acid a brown precipitate. But neither the nitrous solution of lead or zinc, nor that of tin in marine acid or aqua regia, nor that of regulus of antimony in aqua regia, were any way affected. Fixed air, mixed with an equal proportion of phosphoric air, produced a white smoke, some diminution, and a yellow deposit. On agitating the mixture in water, the fixed air was all taken up except $\frac{1}{10}$. The residuum smoked, but did not inflame spontaneously.

From these few experiments, Mr. K. thinks it may be concluded, that phosphoric air is nothing else but phosphorus itself in an aërial state, and differs from sulphur in this, among other points, that it requires much less latent heat to throw it into an aërial form, and hence may be disengaged from fixed alkalis, without the assistance of an acid.

VI. Observations on the Affinities of Substances in Spirit of Wine. By John Elliot, M.D. p. 155.

In Mr. Kirwan's papers on the attractive powers of the mineral acids, it is shown that metallic calces have stronger attractions to those acids, than alkalis and earths. The following experiments not only confirm this doctrine, but also a position that I have lately ventured to advance,* "that certain decompositions will take place in spirit of wine, which will not at all in water, nor in the dry way." I have shown, that if expressed oil be mixed with slaked lime into a paste, so as to form calcareous soap, and mild alkali be added, the latter will not decompose the former, either in water or by fusion. But that if spirit of wine be substituted for water, an alkaline soap and mild calcareous earth will be formed. As sea salt contains the fossil alkali, and as by the table of affinities acids have stronger attraction to metallic calces than to alkalis, I concluded, that if sea salt were added to a metallic soap, a similar double decomposition would take place.

To try this, I took some diachylum, which had been bought at Apothecaries-Hall, and added to it sea salt; then covered them to a sufficient height with spirit of wine, and set the bottle over the fire. Soon after they had boiled, the decomposition of the diachylum began to be apparent. When the boiling had continued some time, I removed the vessel from the fire, and after it had stood a few minutes, decanted the clear liquor while hot; then evaporating it, obtained a true alkaline soap. The residuum of course contained a quantity of calx of lead, combined with marine acid. But much of the diachylum remained either wholly or partly undecomposed: I therefore added more sea salt and spirit of wine, and obtained a further yield of soap. But though much sea salt remained behind, diachylum was still found in the residuum. I found indeed, that if the ingredients were previously freed from their water, the process succeeded to somewhat better advantage.

From 5 oz. of diachylum I did not get quite 3 oz. of soap. This soap was likewise soft, and contained a portion of oil not combined with a sufficient quantity of alkali. The oil I suppose had existed in a similar state in the diachylum: and I remarked, that as the spirit evaporated, it gave out the true soap first, the unsaturated oil not till afterwards; so that the latter might easily be obtained separate from the former. If too much salt was employed, much of it was taken up by the liquid, and communicated to the soap, at least if the ingredients had not been previously deprived of their water. To separate this salt I

* In an Appendix to the 2d edit. of the "Elements of the Branches of Nat. Philos. connected with Medicine."—Orig.

dissolved the soap in hot water. When the liquor was cold, the soap floated at top, the salt remaining in the water underneath. If too little salt was used, this inconvenience did not happen, or not in so great a degree, though then less soap was of course obtained.

As diachylum, though with a greater proportion of litharge, and boiled longer than that I had from the Hall, still contained oil not sufficiently saturated, I made the metallic soap in another way. To a solution of sugar of lead in water I added a solution of alkaline soap in the same liquid. A double decomposition took place, the oil uniting with the calx of lead, the alkali with the acid of salt. Using this metallic soap instead of the other, I obtained an alkaline soap harder and more perfect than in the preceding process; but still found that parts of the oil remained with the calx of lead in the residuum, and adhered so firmly, that repeated quantities of sea salt and spirit of wine did not wholly separate it.

P. S. Since writing the above I have found, that if mild fixed alkali be added to diachylum in hot water, they unite into a gelatinous mass, which is miscible with the water. This may be considered as a kind of hepar. If this substance be put into hot spirit of wine, the decomposition already described takes place. If chalk be substituted for alkali, there is a similar result. I have found that nitre is decomposed by diachylum in spirit of wine. I have also found, that if the compound of diachylum and common salt be put into hot spirit of turpentine, the diachylum is dissolved, but the salt remains at the bottom of the vessel.

VII. An Account of some Minute British Shells, either not duly observed, or totally unnoticed by Authors. By the Rev. John Lightfoot, M. A., F. R. S. p. 160.

The shells which form the subject of this paper were discovered in the neighbourhood of Bullstrode, in Buckinghamshire, by Mr. Agnew, gardiner to the Duchess Dowager of Portland. The drawings of the shells were also made by Mr. Agnew.

The first-mentioned shell, A, pl. 2, named by Mr. Lightfoot *Nautilus lacustris*, is of a flatted spiral figure, umbilicated on one side, convex on the other, but slightly depressed in the centre, and measures about a quarter of an inch in diameter: its volutes or spires are 4 in number: the mouth of the shell is obliquely semioval, the upper edge projecting further than the lower: the substance of the shell is very brittle and pellucid, and when recent is of a reddish brown or chesnut-colour, except 3 or 4 whitish curving streaks, from the centre to the circumference, at nearly equal distances from each other. The internal structure of this shell is extremely curious, the whole cavity being divided, according to the age of the shell, into 3, 4, or 5 chambers, by so many transverse, white, brittle, semipellucid septa, each of which has a triradiated aperture, through which the animal protrudes itself when in motion. As to the real nature of these septa, Mr. Lightfoot does not pretend to guess at the intention of Nature in their formation. If it should be said, that they only point out the different periods of the shell's growth, and are nothing else but the limits or terminations of the animal's periodical increase, Mr. Lightfoot will not dispute the opinion, but, supposing it to be so, he asks whether it is not equally probable that the transverse septa in all the nautili are nothing else? The inhabitant of this curious shell is of the slug kind, but of the aquatic tribe, and has filiform tentacula, the eyes being placed on the head of the animal, near their bases, and not at the tip, as in the land kinds: the colour of the animal is a grey brown. Mr. Lightfoot gives the following specific character of this species, viz. *Nautilus lacustris.* N. testa spirali compressa umbilicata carinata, anfractibus tribus supra convexis contiguis, apertura semiovata, septis triradiato-perforatis. *Fresh-water nautilus.* Nautilus with spiral, compressed, umbilicated, carinated shell, with 3 wreaths convex above and contiguous, semiovate aperture, and triradiate-perforated septa.

In Mr. Walker's publication on minute shells it is described under the name of *Helix lineata*; but the chambered internal structure was unknown to Mr. Walker.

Fig. 1, shell A, pl. 2, shows the shell of its natural size, with the umbilicated side uppermost. Fig. 2, the same with the depressed side uppermost. Fig. 3, the shell magnified, with the depressed side uppermost, and showing the living animal. Fig. 5, the same magnified, with the umbilicated side uppermost. Fig. 4, the same in front, but cut away to the first septum. Fig. 8, the animal's excrement. Fig. 6, 7, horizontal sections of the shell, in order to show the internal structure.

The 2d shell, B, Mr. Lightfoot names *Helix fontana* or *Fountain Helix,* and thus gives its character, viz. Helix testa compressa obtuse carinata, hinc umbilicata, anfractibus tribus utrinque convexis, apertura semiovata. Helix with compressed, obtusely carinated shell, umbilicated on one side, with 3 wreaths convex on both sides, and semiovate aperture.

Fig. 1 shows the shell of the natural size, with the most convex side uppermost. Fig. 2, the same with the umbilicated side uppermost. Fig. 3, the shell magnified, with the most convex side uppermost. Fig. 4, the same with the umbilicated side uppermost.

This species was found in a spring of clear water among rotten leaves, its colour is reddish brown or chesnut.

The 3d shell C, is a very minute, but curious, species of Helix, of a subconical form, consisting of about 5 convex wreaths, gradually diminishing towards the apex. The colour of the whole shell is brown. Mr. Lightfoot names it *Helix spinulosa* or *tender prickly Helix*, and characterises it in the following manner, viz. Helix testa subconica umbilicata, anfractibus quinque convexis, annulis membranaceis acutis cinctis, dorso spinuloso-carinatis, apertura suborbiculari.

The shell is umbilicated at the base, and the wreaths are transversely surrounded with numerous sharp-edged rings, which are produced in the middle or back of each wreath into a kind of spur, formed of compressed and very tender spines. It was found at the foot of pales on old bricks and stones, &c, after rainy weather in June and July.

Fig. 1, 2, the shell of the natural size, in different positions. Fig. 3, 4, 5, the same magnified.

The 4th species D, is a Turbo. It strongly resembles the depressed helices, but its circular mouth forbids its being ranked in that Linnean genus. It is of a brown colour, and consists of 4 cylindric or rounded volutions, which are surrounded transversely with numerous sharp-edged membranaceous rings, which are very fragile and deciduous; the mouth, when perfect, is bordered with a compressed erect margin. Mr. Lightfoot gives the specific character thus, viz. Turbo helicinus. T. testa depresso-plana, hinc umbilicata, anfractibus quatuor torosis, annulis numerosis acutis membranaceis cinctis. Fine ringed turbo. Turbo with depressed-flat shell, umbilicated on one side, with 4 torose wreaths, surrounded by numerous acute membranaceous rings.

Fig. 1, 2, the shell on both sides, of its natural size. Fig. 3, 4, the same magnified.

It was found in spring, near Bullstrode, on base stones, &c.

The 5th and last shell, E, is a species of Patella, and is about a quarter of an inch in length, and a tenth of an inch in diameter, having a pointed vertex nearest to the lower end, turned downwards, and leaning to one side. Mr. Lightfoot names it patella oblonga or oblong fresh-water patella, and thus gives its specific character, viz. Patella testa integerrima oblonga compressa membranacea, vertice mucronato reflexo obliquo. Patella with perfectly entire, oblong, compressed, membranaceous shell, with reflex, oblique, mucronated vertex.

It was found in waters near Beaconsfield, adhering to the leaves of the iris pseudacorus.

Fig. 1, 2, 3, show it in its natural size, in different positions. Fig. 5, magnified, with the vertex upward. Fig. 6, a view of the patella lacustris of Linneus, in order to show the plan of the two different species.

VIII. Observations on the Sulphur Wells at Harrogate, made in July and Aug. 1785. By the Right Rev. Rich. Lord Bishop of Landaff, F. R. S. p. 171.

Reprinted in the 5th vol. of the Bishop of Landaff's (Dr. Watson's) Chemical Essays.

IX. Observations and Remarks on those Stars which the Astronomers of the last Century suspected to be Changeable. By Edw. Pigott, Esq. p. 189.

It is about a century since Hevelius, Montanari, Flamsteed, Maraldi, and Cassini, noticed a certain number of stars which they supposed had either disappeared, changed in brightness, or were new ones; and yet to this day we have acquired no further knowledge of them. This may be attributed to the difficulty of finding out what star is meant, and the not having exact observations of their relative brightness. I therefore have drawn up the following catalogue, and made the necessary observations; so that in future we can examine them without much trouble, and be certain of any change that may take place. To accomplish this, it was requisite to compare with attention many authors and most of the catalogues of stars; in doing which, I have perceived several undoubted errors, and others highly probable.

In order to separate certainty from doubt, I have classed these stars in 2 divisions; the first are undoubtedly changeable; the others remain yet to be better authenticated. Though some of them bear all the appearance of being variable, still no certainty of their being so has come to my knowledge. To those of the first class are subjoined observations made on them within these last 4 years, from which the period and progressive changes of some are deduced, though never settled before; and if already known, are more exactly determined by comparing my observations with former ones. Also, as the position of several are determined only by ancient astronomers, and therefore inaccurately, I have observed them with great exactness, the declinations being taken with a Bird's 18-inch quadrant, and the right ascensions with a 3-feet transit instrument; these last may serve in future to discover their proper motions in right ascension, for which reason I shall specify the stars to which they were compared. The stars of the 2d class have either their relative brightness exactly settled, or their non-existence ascertained. I have also pointed out the probability of a mistake in several, and in general given an account of the appearance they have had within these few years.

Catalogue of variable Stars, reduced to the beginning of 1786.

Class the first.

Names.	R. A. in time.	Declination.	Greatest and least magnit.	From whence reduced.
Nova 1572, in Cassiopeia....	0h 13m 0s —	62° 58′ +″ N	1 — 0	Ricciolus's Almagestum, &c.
o Ceti....................	2 8 33	3 57 25 s.	2 — 0	Bradley.
Algol..	2 54 19	40 6 55 N.	2 — 4	Mayer.
Mayer's 420th in Leo	9 36 5	12 25 0 N.	6 — 0	Mayer.
In Hydra	13 18 4 +	22 9 38 s.	4 — 0	From my observations.
Nova 1604 in Serpentarius ..	17 18 0 —	21 10 ½ s.	1 — 0	Phil. Trans. N° 346.
β Lyræ	18 42 11	33 7 46 N.	3 — 4.5	Bradley.
Near the Swan's head	19 38 58	26 48 ½ N	3 — 0	Phil. Trans. N° 65.
η Antinoi	19 41 34	0 28 14 N.	3.4 — 5	La Caille.
In the Swan's neck	19 42 21 —	32 22 58 N.	5 — 0	From my observations.
In the Swan's breast	20 9 54	37 22 37 N.	3 — 0	From my observations.
δ Cephei	22 21 0	57 20 0 N.	4.3 — 4.5	Flamsteed.

Class the second.

Names.	R. A. in time.	Declination.	Greatest and least magnit.	From whence reduced.
Hevelius's 6 Cassiopeæ......	0 23 16	60 50 0 N.	7 — 0	Hevelius.
46 or ξ Andromedæ	1 9 46	44 24 0 N.	4.5 — 5.6	Flamsteed.
50 or υ Andromedæ	1 24 16	40 20 15 N.	4.5 — 0	Flamsteed.
Hevelius's 41 Andromedæ ..	1 28 40	41 31 ¼ N.	5 — .	Hevelius.
Tycho's 20th Ceti..........	1 39 .	13 20 . s.	5 — 0	Tycho.
55 or Neb. Andromedæ	1 40 30	39 40 3 N.	6 — .	Flamsteed.
Ptol. and Ul. Beigh σ Eridani	2 42 .	9 40 . s.	4 — 0	Ul. Beigh.
41 Tauri	3 53 27	27 0 39 N.	5 — 0	Flamsteed.
47 Eridani................	4 23 54	8 41 40 s.	4 — 0	Flamsteed.
Near 53d Eridani	4 29 0	12 30 ± s.	4 — 0	By estimation.
γ Canis majoris............	6 54 5	15 19 36 s.	3 — 0	La Caille.
β Geminorum	7 32 11 +	28 31 38 N.	1 — 3	Maskelyne.
ξ Leonis..................	9 20 24	12 14 23 N.	4 — 6	Mayer.
ψ Leonis	9 32 3	14 59 36 N.	5.6 — 0	Mayer.
25th Leonis	9 46 8	12 20 36 N.	6.7 — 0	Flamsteed.
Bayer's i Leonis............	9 52 ½	15 30 . N.	6 — 0	Tycho.
δ Ursæ majoris	12 4 45	58 13 24 N.	2 — 4	La Caille.
n Virginis	12 7 43	0 24 16 N.	6 — 0	Mayer.
Bayer's * near g ♍	12 53 0	10 0 . s.	6 — 0	From maps.
In N. thigh of Virgo........	13 29 +	0 30 . s.	6 — 0	From maps.
91 Virginis	13 43 43	2 5 50 N.	6 — 0	Flamsteed.
α Draconis................	13 58 36	65 24 8 N.	2 — 4	Bradley.
In west scales of Libra......	14 53 ½	13 26 . s.	6 — 0	Mém. de l'Acad. des Scien.
Ptol. and Ul. Beigh's 6th unformed in Libra..........	15 29 +	20 30 . s.	4 — 7	Ul. Beigh.
κ Libræ	15 29 39	19 58 27 s.	4 — .	La Caille.
Tycho's 11th Libræ	15 37 ½	19 30 . s.	4 — 0	Tycho.
33 Serpentis	15 38 0	17 14 0 N.	6 — 0	Flamsteed.
Near ε Ursæ minoris........	16 ¼ .	82 ¾ . N.	6 — 0	From maps.
Ptol. 14 Ophiuchi..........	17 2 14	26 15 37 s.	4 — 0	Bradley.
Ptol. 13 Ophiuchi..........	17 18 +	20 35 . s.	4 — 0	Ptol.
Ptol. 18 Ophiuchi..........	17 22 .	24 10 . s	5 — 0	Ptol.
σ Sagittarii................	18 42 0	26 32 34 s.	2 — 4	Mayer.
θ Serpentis................	18 45 35	3 56 26 N.	4 — 5	La Caille.
Tycho's 27th Capricorn......	21 41 .	14 28 . s.	6 — 0	Tycho.
Tycho's 22d Andromedæ	21 43 ½	49 15 . N.	4 — 0	Tycho.
Tycho's 19 Aquarii	22 25 .	15 55 . s.	6 — 0	Tycho.
o Andromedæ	22 52 6	41 10 45 N.	4 — 6	La Caille.
La Caille's 483 Zodi. Cat.....	22 55 40	8 50 45 s.	7 — 0	La Caille.

Now to give a short account of these stars, beginning with those of the first class.

The famous Nova of 1572 *in Cassiopeæ*—Several astronomers are of opinion, that it has a periodical return, which Keill, and others have conjectured to happen every 150 years. This is also my opinion.

o Ceti.—Since the end of 1782 I have observed very exactly the decrease of brightness of this star; but never have seen it of above the 6th magnitude. Oct. 29, 1782, it was of the 7th magnitude, and gradually decreased till Dec. 30, it being then of the 8 . 9th magnitude. 1783, Feb. 16, certainly less than the 9th magnitude. 1783, Aug. 25, of the 6th magnitude, and gradually decreased until Dec. 14, being then of the 10th magnitude, and equal to the little star close to it. I have deduced its period from the times when it was equal to a certain star in the course of its decrease; the results were 320, 337, and 328 days; but M. Cassini determined its mean period with greater exactness to be 334 days. Mr. Goodricke saw it Aug. 9, 1782, of the 2d magnitude, rather brighter than α and less than β Ceti. Sept. 5, it was of the 3d magnitude, being equal to γ Ceti.

Algol.—The period of Algol, discovered by Mr. Goodricke, gave some new light into the nature of the fixed stars. Its degree of brightness, when at its minimum, is different in different periods; and I think, when at its full, it is sometimes brighter than α Persei, and at other times less.

Mayer's N° 420, *lately discovered to be variable by M. Koch.*—A few years before 1782, M. Koch saw the N° 420 undoubtedly less than the N° 419 of Mayer's Catalogue. In February 1782, he found them both exactly of the same brightness, therefore of the 7th magnitude. From an extract of a letter I have lately seen, the variable was of the 9th magnitude in April, 1783, and of the 10th in April, 1784.

Variable in Hydra.—Maraldi, in 1704, having found that this star had a periodical variation, continued to examine it for several years, and concluded its period to be about 2 years, though with considerable variations; in which he was much mistaken, as appears from several observations, which show that its period in all probability is tolerably regular, and only of 494 days. If Maraldi's observations of 1704 and 1708 are exact to a month, and there is no reason to believe otherwise, the period at that time seems to have been a few days longer than it is at present, and therefore the one here deduced may be esteemed as the mean period.

Particulars of the changes it undergoes, are, 1. When at its full brightness it is of the 4th magnitude, and has no perceptible change for about a fortnight. 2. It is about 6 months in increasing from the 10th magnitude, and returning to the same. 3. Therefore it may be considered as invisible also during 6 months. 4. It is considerably quicker in increasing than in decreasing, perhaps by half.

Hevelius's 30th Hydræ is the above star ; he marks it of the 6th magnitude.

The famous Nova of 1604, *in Serpentarius.*—A full account of this star is given by Kepler, and it seems to have had a similar appearance to the Nova in Cassiopea ; therefore the reflections delivered there need not be again repeated.

β *Lyræ.*—Mr. Goodricke discovered the variation and period of this star, and hopes soon to settle its different phases with more exactness. In his last account he mentions having first suspected the period to be only of 6 days 9 hours ; such has always been my opinion.

Nova near the Swan's Head of 1670.—This star was first seen in December 1669 by Don Anthelme ; it soon became of the 3d magnitude, and disappeared in 1672, after having undergone several variations. I have constantly looked for it since November, 1781, without success ; had it increased to only the 10th or 11th magnitude, I should have perceived it, having taken an exact plan of all the surrounding stars.

η *Antinoi.*—The variation and period of this star I discovered last year, and communicated an account of it to the R. S. The period, as settled in my former paper, is $7^d\ 4^h\ 38^m$; but for reasons there alleged, it must be much less precise than the following, viz. $7^d\ 4^h\ 15^m$.

I see no reason to alter materially the other points ; but believe them more exact thus : 40^h at its greatest brightness ; 66^h in decreasing ; 30^h at its least ; 36^h in increasing. It also, in every period, seems to attain the same degree of brightness when at its full, and to be equally decreased.

Variable in the Swan's Neck.—During these 3 years I have observed this star with particular attention, and determined the middle time of its greatest brightness very exactly; whence it is inferred that its period will be found of only 396 days 21 hours.

Particulars of the changes it undergoes are, 1. When at its full brightness it has no perceptible change for about a fortnight. 2. It is about 3½ months in increasing from the 11th magnitude to its full brightness, and the same in decreasing. 3. Therefore it may be considered as invisible during 6 months. 4. It does not attain the same degree of brightness at every period, being sometimes of the 5th, and other times of the 7th magnitude.

Its mean right ascension, computed from my observations, and reduced to Aug. 1, 1783, is $295^\circ\ 33'\ 48\frac{1}{2}''$.

Variable in the Swan's Breast.—This star was first seen by G. Janson in 1600, and afterwards frequently observed by different astronomers, but with intervals of 10 or more years, which is probably the reason why no regularity in its changes has yet been deduced. I have examined minutely the observations made in the last century, and shall venture to give the following results: 1. Continues at its full brightness for about 5 years. 2. Decreases rapidly during 2 years. 3. Invisible to the naked eye for 4 years. 4. Increases slowly during 7 years. 5. All these changes, or its period, are completed in 18 years. 6. It was at its minimum at the end of the year 1663.

It does not always increase to the same degree of brightness, being sometimes of the 3d, and at other times only of the 6th magnitude.

Its mean right ascension, computed from my observations, and reduced to Sept. 1, 1782, is 302° 26′ 45″.

δ Cephei.—This is the last variable star discovered, and again by Mr. Goodricke. Its changes are very difficult to be seen, unless examined when at its minimum and full brightness. I have lately made some good observations on it thus.

It was between its least and greatest brightness August 31, at noon, and Sept. 26, at 21^h: these being compared to my first observations, when also between its least and greatest brightness on Nov. 20, at 3^h and Nov. 30, at 15^h, 1784, give the mean result of its period $5^d\ 8^h\ 37\frac{1}{2}^m$; which corroborates that deduced by Mr. Goodricke of $5^d\ 8^h\ 37\frac{1}{2}^m$.—Those that follow are of the second class.

Hevelius's 6th Cassiopeæ.—In 1782 I first perceived that this star was missing; nor could I find it in 1783 and 1784.

46th or ζ Andromedæ.—This star is said to have diminished in brightness. In 1784 and 1785 I found it, by very exact observations, less than υ, equal to ω, and brighter than *d* and χ.

Flamsteed's 50, 52, τ Andromedæ, and Hevelius's 41 Andromedæ.—The position and characters of these stars differ considerably in different catalogues, and some of them are mentioned by Cassini as having disappeared and re-appeared, Mr. P. gives their brightness as observed in 1783, 1784, and 1785, thus:—Flamsteed's 50th of the 4.5th magnitude, and equal, if not rather less than φ Andromedæ. τ of the 5th magnitude, and equal to 46 and 48 Andromedæ.—49, 52, and Hevelius's 41, of the 5.6th magnitude, and are of the same brightness. A star between Flamsteed's 52 and Hevelius's 41 is of the 6th magnitude, or rather less.

Tycho's 20th Ceti.—This must be the star which Hevelius said had disappeared, being Tycho's 2d in the Whale's belly. There can hardly be any doubt but that it is the χ, misplaced by Tycho. This χ is of the 4.5th magnitude, and of the same brightness as the three ψ Aquarii.

Flamsteed's 55th Andromedæ, marked Neb. in his Catalogue.—It is mentioned in the latest catalogues of Nebulæ that this nebula could not be found. Flamsteed does not mark it nebulous; nor does it appear to be such, but as a star of the 6th magnitude. There are a few small stars near it, which to the naked eye, when the air is very clear, make it appear nebulous, which probably is the reason why Flamsteed marked it thus in his catalogue.

σ or Ptol. and Ul. Beigh's 17th Eridani.—Flamsteed says, he could not see this star in 1691 and 1692. In 1782, 1783, and 1784, I observed one of the 7th magnitude in that place; the relative brightness of which appeared always the same, viz. less than two little stars near and below η Eridani.

Flamsteed's 41 Tauri.—This star was thought by Cassini to be a new one or variable. I see little or no reason to be of that opinion; that it is not new is evident, since it is Ul. Beigh's 26th and Tycho's 43d. In 1784 and 1785 I found it of the 5th magnitude, being equal to φ, and brighter than ψ, P, and χ Tauri.

Star about $2\frac{1}{4}°$ North of 53d Eridani, and 47 Eridani.—The first of these stars Cassini thought a new one, and that it was not visible in 1664. In 1784 I found it was less than ω, and *d*, brighter than A, and seemed equal to ψ Eridani. Cassini mentions another star thereabouts, which he also esteemed a new one: this is probably Flamsteed's 47th. In 1784 it appeared rather less than 46th.

γ Canis Majoris.—Maraldi could not see this star in 1670; but in 1692 and 1693 it appeared of the 4th magnitude. I have very frequently noticed it since 1782, but perceived not the least variation, being constantly of the 4th magnitude, very little brighter than θ, and decidedly brighter than ι.

$\alpha\beta$ Geminorum.—If either of these stars have changed in brightness, it is probably the β. In 1783, 1784, 1785, the β was undoubtedly brighter than α.

ξ Leonis.—Montanari says, this star was hardly visible in 1693. I found it constantly in 1783, 1784, and 1785, of the same brightness, being of the 5th magnitude; less than A, π, and, if any difference, rather brighter than *h* and ω Leonis. Tycho, Flamsteed, Mayer, Bradley, &c. mark it of the 4th magnitude.

ψ Leonis.—This star is said to have disappeared before the year 1667. It is now, and has ever been since 1783, of the 5 . 6th magnitude, being less than ω, and brighter than *i*, Flamsteed's 46th.

25th Leonis.—In 1783 I first perceived this star was missing; nor was it visible in 1784 and 1785, even with the transit-instrument.

Bayer's i Leonis, or Tycho's 16 Leonis.—It was not visible in 1709, nor could I see it in 1785. This is a different star from the *i* Leonis of the other catalogues, though Tycho's description of its place is the same.

δ Ursæ Majoris.—This star is suspected to change in brightness, on account of its being marked by Tycho, prince of Hesse, &c. of the 2d magnitude; while Hevelius, Bradley, and others, have it of the 3d. At present, and for these 3 years past, it appears as a bright 4th magnitude, being rather less than ι, equal to α, and rather brighter than κ Draconis.

n Virginis.—This star is supposed to be variable, because Flamsteed, on the 27th of January, 1680, says he could not see it. He observed it May 12, 1677, and some years afterwards, since it is in his catalogue. I examined it frequently in 1784 and 1785, without perceiving the least change, being of the 6th magnitude, less than *c*, and rather brighter than a star 3° lower in a right line with *c* and *n* Virginis.

Bayer's star of 6th magnitude, 1° *South of g Virginis.*—This star is not in any of the 9 catalogues that I have. Maraldi looked for it in vain; and in May, 1785, I could not see the least appearance of it. It certainly was not of the 8th magnitude.

In the northern thigh of Virgo.—This star, which is marked by Ricciolus of the 6th magnitude, could not be seen by Maraldi in 1709; nor was it of the 9th magnitude, if at all visible, in 1785.

91 or 92 *Virginis.*—In 1785 I found that one of these stars was missing, and which seems to be the 91: the remaining one is of the 6.7th magnitude.

α Draconis.—I am of Mr. Herschel's opinion, that it is highly probable this star is variable. Bradley, Flamsteed, &c. mark it of the 2d magnitude; at present it is only of a bright 4th. I have frequently examined it since October, 1782, without perceiving the least change, being constantly rather less than ι Draconis, equal to δ Ursæ Majoris, and rather brighter than κ Draconis.

Bayer's star in the west scales of Libra.—Maraldi says he could not see this star; nor could I in 1784 and 1785. With a night-glass may be seen thereabouts some small stars of about the 8th magnitude, none of which are near so bright as the 2d ν Libræ.

Ptol. and Ul. Beigh's N° 6 *of the unformed in Libra.*—In examining different catalogues I do not find this star in any other than the above, though it is marked of the 4th magnitude. If Ptolemy had not the κ it might be thought to be that. In 1785 I frequently observed a star of the 7th magnitude very near its place, which appeared rather less than Flamsteed's 41. Flamsteed has not this little star in his catalogue; but he observed it May 9, 1681.

κ Libræ.—This star is thought to be variable. I am not of that opinion; though certainly it is rather singular that Hevelius, whose attention was directed to this part of the heavens, to find Tycho's 11th, did not observe the κ; and the more so, as he has noticed two much smaller stars not far from it. During these 3 years I have found the κ constantly of the 5th magnitude, being less than ψ or θ, equal to λ, and brighter than η.

Tycho's 11th Libræ.—Hevelius says he could not find a star of the 4th magnitude in Libra noticed by Tycho. This must be Tycho's 11th, since he has all the others. It was not visible in 1783, 1784, and 1785, nor probably ever existed; for I think it is evident that this 11th is no other than the κ, with an error of 2° in longitude.

33 *Serpentis.*—In 1784 I perceived that this star was missing; nor was it visible in 1785 with a night-glass.

A star marked by Bayer near ε Ursæ Minoris.—Cassini could not see this star. In 1782 I took, with a night-glass, a plan of all the stars near its place, and near the ε, none of which were brighter than the 7 . 8th magnitude. I have since re-examined the plan, but found no alteration.

The ϱ or Ptol. and Ul. Beigh's 14th Ophiuchi or Flamsteed's 36th.—I have no doubt but that this is the star which is said to have disappeared before 1695. It is also evident, by what Hevelius says in his catalogue on the θ and B, that the ϱ was not seen by him. In 1784 and 1785 I found it of the 4 . 5th magnitude, much brighter than 39, also rather brighter than 51 and 58, and less than 44. On the 30th of June, 1783, I have marked it in my journal equal to 39, and less than 51 and 58; but as the observation was not repeated, I am far from being certain it has undergone any change, particularly as this star has a southern declination of 26°, and therefore great attention must be given to the state of the atmosphere.

Ptol. 13th and 18th Ophiuchi, 4th magnitude.—If there is no error in the catalogue, these two stars have disappeared; but I am confident that Ptolemy's 13th is Flamsteed's 40th, and that Ptolemy's 18th ought to be marked with a north latitude instead of south, which would make it agree nearly with Flamsteed's 58th.

σ Sagittarii.—Mr. Herschel, with great reason, has placed this star among those which probably have changed their magnitudes. I had long since remarked the singular disagreement in all the catalogues, which induced me to observe it frequently, particularly in 1783, 1784, and 1785, when it appeared of the 2 . 3d magnitude, and brighter than π Sagittarii.

θ Serpentis.—Montanari says he saw this star of the 5th magnitude, and that the next year it grew larger. I examined it frequently in 1783, 1784, and 1785, and found it always less than δ Aquilæ, equal to β Aquilæ, and P Ophiuchi; 4th magnitude.

Tycho's 27th Capricorni.—This star was not visible in Hevelius's time; nor could I see it 1778, 1782, 1784, with the transit-instrument.

Tycho's 22d Andromedæ and o Andromedæ.—Cassini remarked, that the star placed by Tycho at the end of the chain of Andromeda as of the 4th magnitude, was become so small that it could scarcely be seen. This is Tycho's N° 22, the longitude and latitude of which places it near the two π Cygni, and where no star was visible in 1784 and 1785. As possibly, by Tycho's description, Cassini took the 22d for the o Andromedæ, I have also examined this star, and in 1783, 1784, and 1785, found its relative brightness thus: less than α Cephei; equal to ζ Cassiopeæ, though, if any difference, rather brighter than λ, κ, or ι Andromedæ.

Tycho's 19th Aquarii.—This is the star that Hevelius says was missing, and that Flamsteed could not see with his naked eye Nov. 18, 1679; nor could I see the least appearance of it in 1782. I am convinced it is the same star as Flamsteed's 56th, marked *f* by Bayer, from which it is only $1\frac{1}{2}°$. Flamsteed's 53d, marked *f* in Ptolemy's catalogue, is a different star.

La Caille's 483 *Aquarii.*—I first discovered that this star was missing in 1778. It was not visible in 1783, 1784.

There are a few other stars suspected by the ancient astronomers to have been new or altered a little in brightness, which I have omitted, not seeing any reason to think them so; and some that are certainly variable, but cannot be observed in these latitudes. I have also, contrary to my first intention, added several which are not mentioned by them; such are those that I lately discovered to be missing.

X. Of a Subsidence of the Ground near Folkstone, on the Coast of Kent. By the Rev. John Lyon, M. A. p. 220.

In our vol. 6, p. 252, was given an account of the sinking of the cliffs or high ground near Folkstone, by the Rev. J. Sackette. The present paper is a further account of the same, taken at the request of Mr. Edw. King in 1785, by Mr. Lyon of Dover; partly confirming, and in part contradicting, some particulars in the former account.

I have been twice to view the place, says Mr. L. and have endeavoured to procure the best information, and have compared my remarks with what Mr. Sackette formerly said on the same subject to the Royal Society. Mr. L. gives an explanation from a large map of the place. It appears that a length of about 130 feet of the cliff had subsided or slipped down about 40 feet lower than the rest.

As I intend, says Mr. L., in explaining the cause of the sinking of the ground, to advance an opinion of my own, and to controvert what Mr. Sackette formerly said on the subject, it may be necessary to explain the nature of the soil, so far as it is open to view, in the neighbourhood of Folkstone. The chalk cliffs, which begin at Dover, form opposite Folkstone town high hills, and leaving the shore, there is a large tract of arable and pasture land between them and the sea. Part of this ground is a kind of marle, which contains pyrites, fragments of the cornu ammonis, and many other fossil bodies. Next to the marle is a loose sandy soil, intermixed with a very large, hard, and coarse kind of stone, in which are often found fossil oyster shells. This sandy soil rests on a marle, which at the cliff is in some places 3 or 4 feet above the beach, and when wet is very slippery. A stratum of this marle extends for many miles on the coast, and where it is not sufficiently covered with sand to bear any weight, it is in many places a quag, and dangerous to pass over.

Through this tract of land there are many drains of water, which may be supplied partly from the falling of the rains in wet seasons, and partly from the springs issuing from the hills; and there is reason to suppose, that in a loose soil these drains form channels in a course of time. At the place where the ground has sunk before, and is now sinking, there is a drain from the marle under the sand; and I am of opinion, that the course of the water is in the same direction as the valley between the hill and the sinking of the ground. That the sinking of the ground is caused by the foundation being undermined, and I think by water, is evident from the appearance of the ground in the valley. The soil is full of fissures, and resembles an arch, which is sunk down, and has left two abutments standing. As the hill more than counter-balances the pressure of the sinking ground on the stratum of wet marle, the consequence is, that the rocks at some yards distance, being only thinly covered with sand, are forced upwards, and become visible, and the wet marle in many places is squeezed through the sand with them. This appears to be the true reason of the sinking of the ground at one place, and the rising of the rocks at another.

That Mr. Sackette's account of the sinking of the ground at Folkstone is founded in error, I have not the least doubt, from the present appearance of some of the objects he describes. I am rather at a loss to follow him exactly, as the oldest man in the town of Folkstone, I am told, never heard of the Mooring-rock he mentions. I think by his description the sinking of the ground must have been in his time at the same place it is now, as Tarlingham-house is not to be seen on the other side of the town. Admitting this to be the case, there will still be a difficulty respecting the relative situation of each place in explaining what he calls a sketch of the country.

If Mr. Sackette, in the description of his sketch of the country, had placed each object according to its real situation; and if the effects he has mentioned had been real ones, they would have been truly wonderful, and worthy the attention of the curious investigator of the hidden operations of nature; but I am apprehensive he had but very little better foundation for what he has said than the vague and inconsistent reports of a few ancient fishermen. Tarlingham-house is by Mr. Sackette's account situated full as far beyond the hill as the width of the plain; but how deep the hill has sunk to render the house visible over the top must depend on the situation of it, viz. how much higher it was than the top of the hill. If the hill has sunk only 10 feet, there must have been some external evidence of it, such as fissures round the base, and a very steep ascent from the top of it, where the separation happened between it and Tarlingham-house; but there are no traces of any sinking of the hills.

There is further proof that Mr. Sackette did not examine into the matter himself, but rested what he said on the report of others; and this is, that Tarlingham-house is not seen over the top of the hill, but considerably to the left of it, and clear even of the base of the hill. Besides, a moment's reflection would have told him, that the sinking of the hills could not produce the effects he mentions; for if the ground in the plain was pushed forward by it, it could not be a partial slipping; not only the church, and the whole town, must have been removed, but every object between the base of the hills and the cliff must have been removed out of their place; but I may venture to affirm, there is no proof of this having been done. I should have been drawn into the same or similar errors myself, if I had rested satisfied with the first accounts I received from an ancient fisherman. He told me the same story of the hills sinking in his time, and Tarlingham-house appearing higher than it did since he could remember. In one part of his relation he was right; for I found, on inquiry, that Tarlingham-house has been taken down, and built on a much larger scale than formerly, since it has been in the hands of the present proprietor.

XI. Particulars relative to the Nature and Customs of the Indians of North-America. By Mr. Richard M'Causland, Surgeon to the 18th Regiment. p. 229.

It has been advanced by several travellers and historians that the Indians of America differed from other males of the human species in the want of one very characteristic mark of the sex, viz. that of a beard. From this general observation, the Esquimaux have been excepted; and hence it has been supposed, that they had an origin different from that of the other natives of America. Inferences have also been drawn, not only with respect to the origin, but even relative to the conformation of Indians, as if this was in its nature more imperfect than that of the rest of mankind. I will not by any means take on me to say that there are not nations of America destitute of beards; but 10 years residence at Niagara, in the midst of the six nations (with frequent opportunities of seeing other nations of Indians) has convinced me, that they do not differ from the rest of men, in this particular, more than one European differs from another: and as this imperfection has been attributed to the Indians of North-America, equally with those of the rest of the continent, I am much inclined to think, that this assertion is as void of foundation in one region as it is in the other.

All the Indians of North-America (except a very small number, who, from living among white people, have adopted their customs) pluck out the hairs of the beard; and as they begin this from its first appearance, it must naturally be supposed, that to a superficial observer their faces will seem smooth and beardless. As further proof that they have beards, we may observe, first, that they all have an instrument for the purpose of plucking them out. Secondly, that when they neglect this for any time, several hairs sprout up, and are seen on the chin and face. Thirdly, that many Indians allow tufts of hair to grow on their chins or upper lips, resembling those we see in different nations of the old world. Fourthly, that several of the Mohocks, Delawares, and others, who live among white people, sometimes shave with razors, and sometimes pluck their beards out. These are facts which are notorious among the army, the Indian-traders, &c.; and which are never doubted in that part of the world by any person in the least conversant with Indians.

The following are a few particulars relative to the Indians of the six-nations, which, as they seem not to be well understood even in America, are probably still less known in Europe. Each nation is divided into 3 or more tribes; the principal of which are called the turtle-tribe, the wolf-tribe, and the bear-tribe. Each tribe has 2, 3, or more chiefs, called Sachems; and this distinction is always hereditary in the family, but descends along the female line: for instance, if a chief dies, one of his sister's sons, or one of his own brothers, will be appointed to succeed him. Among these no preference is given to proximity or primogeniture; but the Sachem, during his life-time, pitches on one whom he supposes to have more abilities than the rest; and in this choice he frequently, though not always, consults the principal men of the tribe. If the successor happens to be a child, the offices of the post are performed by some of his friends till he be of sufficient age to act himself.

Each of these posts of Sachem has a name which is peculiar to it, and which never changes, as it is always adopted by the successor; nor does the order of precedency of each of these names or titles ever vary. Yet, any Sachem, by abilities and activity, may acquire greater power and influence in the nation than those who rank before him in point of precedency; but this is merely temporary, and dies with him. Each tribe has 1 or 2 chief warriors; which dignity is also hereditary, and has a peculiar name attached to it. These are the only titles of distinction which are fixed and permanent in the nation; for though any Indian may, by superior talents, either as a counsellor or as a warrior, acquire influence in the nation, yet it is not in his power to transmit this to his family. The Indians have also their great women as well as their great men, to whose opinions they pay great deference; and this distinction is also hereditary in families. They do not sit in council with the Sachems, but have separate ones of their own. When war is declared, the Sachems and great women generally give up the management of public affairs into the hands of the warriors. It may however so happen, that a Sachem may at the same time be also a chief warrior. Friendships seem to have been instituted with a view towards strengthening the union between the several nations of the confederacy; and hence friends are called the sinews of the six-nations. An Indian has therefore generally one or more friends in each nation. Besides the attachment which subsists during the life-time of the two friends, whenever one of them happens to be killed, it is incumbent on the survivor to replace him, by presenting to his family either a scalp, or a prisoner, or a belt consisting of some thousands of wampum; and this ceremony is performed by every friend of the deceased. The purpose and foundation of war parties therefore, is in general to procure a prisoner or scalp to replace the friend or relation of the Indian who is the head of the party. An Indian who wishes to replace a friend or relation, presents a belt to his acquaintance, and as many as chuse to follow him accept this belt, and become his party. After this, it is of no consequence whether he goes on the expedition or remains at home, as it often happens that he is a child, he is still considered as the head of the party. The belt he presented to his party is returned fixed to the scalp or prisoner, and passes along with them to the friends of the person he replaces. Hence it happens, that a war party, returning with more scalps or prisoners than the original intention of the party required, will often give one of these supernumerary scalps or prisoners to another war party whom they meet going out; on which this party, having fulfilled the purpose of their expedition, will sometimes return without going to war.

XII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, Rutland, 1785. *By Thomas Barker, Esq. Also of the Rain at South Lambeth, Surrey; and at Selbourn and Fyfield, Hampshire.* p. 236.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.						
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	S. Lamb. Surry.	Selbourn Hamp.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29.83	28.59	29.31	45½	34½	39	45	25½	35	1.494	1.78	2.84	2.12½
	Aftern.				46	34½	40	47½	27	38½				
Feb.	Morn.	30.16	28.45	29.34	39	29	34½	38	10	28	0.365	1.20	1.80	1.85
	Aftern.				40	30½	36	42½	23	34				
Mar.	Morn.	29.84	29.16	29.61	44	29½	36	43½	17	30½	0.212	0.35	0.30	0.00½
	Aftern.				45	31½	37½	51	27½	38				
Apr.	Morn.	30.05	28.88	29.71	56	36	48	52½	25	41	0.175	0.34	0.17	0.14½
	Aftern.				58	37	50	67½	37½	54				
May	Morn.	30.09	28.95	29.54	61	50½	55	58½	42	48	0.666	0.81	0.60	0.96
	Aftern.				64	51½	56½	75½	50½	60½				
June	Morn.	29.99	29.32	29.71	66½	53	61	60	48	55	1.567	2.04	1.39	1.19
	Aftern.				69½	55½	63	80½	54½	69				
July	Morn.	29.82	28.97	29.42	68½	60	64	64	53½	58½	3.283	1.73	3.80	1.69
	Aftern.				73	61	65	83	60½	70				
Aug.	Morn.	29.72	28.99	29.36	64½	55½	59½	59½	43½	53½	4.315	3.05	3.21	4.26
	Aftern.				65	56½	61	71	55	64				
Sept.	Morn.	29.89	28.51	29.29	63	50	59	59	36	52	3.314	2.75	5.94	5.30
	Aftern.				64½	51½	60	72	47	63				
Oct.	Morn.	29.99	28.95	29.45	58	41½	50½	55½	28	42½	1.653	4.04	5.21	2.52
	Aftern.				59½	43	51½	62½	37½	52				
Nov.	Morn.	30.02	28.33	29.33	51½	39	44½	49½	28	37	1.125		2.27	1.46¾
	Aftern.				52½	39½	45	56½	34½	44				
Dec.	Morn.	29.79	28.76	29.32	43½	32	39	43	20½	33	2.037	1.53	4.02	3.04
	Aftern.				44½	32½	40	45½	24	37				
Mean of all				29.45			50			47½	20.206	19.62	31.55	24.55

XIII. Account of Experiments made by Mr. John M'Nab, at Henley House, Hudson's Bay, relating to freezing Mixtures. By Henry Cavendish, Esq., F. R. S., and A. S. p. 241.

In my former paper, in the 73d vol. of the Phil. Trans., I said, that the cold produced by mixing spirit of nitre with snow, is owing to the melting of the snow; and that in all probability there is a certain degree of cold, in which spirit of nitre is so far from dissolving snow, that it will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials, before mixing, be equal to this, no additional cold can be produced. A circumstance however, which at first sight seems repugnant to this opinion, occurred in an experiment of Fahrenheit's for producing cold by a mixture of spirit of nitre and ice; namely, that the acid, which had been repeatedly cooled by different frigorific mixtures, was found frozen before it was mixed with the ice; yet cold was produced by the mixture. Professor Braun also found, that cold was produced by mixing frozen spirit of nitre with snow. On consideration however this appeared by no means inconsistent with the opinion there laid down, as there was great reason to think, that the freezing of the acid was of a different kind from that considered in the above-mentioned paper, and that it did not proceed from the watery part separating from the rest and freezing; but that the whole acid, or perhaps the more concentrated part, froze; in which case it would not be extraordinary that the acid should dissolve more snow, and produce cold.

To clear up this point, I sent to Hudson's Bay a bottle of spirit of nitre, of nearly the same strength as Fahrenheit's; and desired Mr. M'Nab to expose it to the cold, and, if it froze, to ascertain the temperature, and decant the fluid part into another bottle, and send both home to be examined, as it would thus be known, whether it was the whole acid, or only the watery part, which froze. For the same purpose also I sent some strong oil of vitriol. I also sent some spirit of nitre and spirit of wine, both diluted with so much water, that it was expected, that with the cold of Hudson's Bay they would suffer the first kind of congelation; that is, their watery part would freeze, and so make the difference between the two kinds of freezing more apparent.

As it seemed likely that, by following the method, pointed out in my former account, a greater degree of cold might be produced than had been done hitherto, I sent 3 other bottles of spirit of nitre and oil of vitriol, all 3 diluted, but not so much so, but that I thought they would require a little further dilution, to reduce them to their properest degree of strength. I also sent a bottle of highly rectified spirit of wine, and a mixture of equal quantities of the above-mentioned common spirit of nitre and oil of vitriol; and desired Mr. M'Nab to find what degree of cold, could be produced by mixing them with snow, after having first reduced them, in the above-mentioned manner, to their best degree of strength. He was also desired to ascertain how much snow he added; for as their strength was determined before they were sent out, it would thus be known what was the best strength of these liquors for frigorific mixtures.

All these bottles were numbered with a diamond; and as I shall sometimes distinguish them by these numbers, and as it may be of use to those who may consult the original, I have added the following list of these bottles, with their contents.

N°	Liquors mentioned in Art. 3.	Weight of marble which they dissolve.	Spec. grav. at 60° of heat.
168	Spirit of nitre	.582	1.4371
27	Dephlogisticated spirit of nitre	.53	1.4040
103	Diluted oil of vitriol	.654	1.5596
28	Equal weights of N° 168 and N° 103		
8	Very highly rectified spirit of wine		.8195
	Liquors mentioned in Art. 2.		
151	Strong oil of vitriol	.98	1.8437
142	Spirit of nitre	.525	1.4043
139	Some of the same diluted with twice its weight of water		
141	Dephlogisticated spirit of nitre	.53	1.4033
143	Some of the same spirit of wine as in N° 8 diluted with $1\frac{1}{2}$ its weight of water		
72	Diluted oil of vitriol for comparing the thermometers	.629	
171	Oil of vitriol of about the usual strength, but the exact strength not known, intended to refresh the former when too weak.		

Professor Braun says, that by mixtures of snow and spirit of nitre he sunk thermometers filled with oil of sassafras, and some other essential oils, to — 100° or — 124°; and that, by the same means, he sunk thermometers filled with the highest rectified spirit of wine to — 148°. Though there seemed great reason to think, from Mr. Hutchins's experiments, that there must be some mistake in this; yet, as it was possible that the essential oils, and even spirit of wine of a strength much different from that with which Mr. Hutchins's thermometers were filled, might follow a considerably different progression in their contraction by great degrees of cold, I sent a thermometer filled with oil of sassafras, and two others with spirits of wine. One of these last was filled with the highest rectified spirits I could procure, its specific gravity at 60° of heat being .8185; the other was intended to be filled with common spirits, though from circumstances I am inclined to suspect that also to have been filled with the best spirits. Besides these, there was sent a mercurial thermometer, accurately adjusted, according to the direction of the committee of the R. S., printed in the 67th vol. of the Transactions; and also the two spirit thermometers used by Mr. Hutchins, which were filled with spirits whose specific gravity was .8247.

These thermometers were compared together by exposing them to the cold, with their balls immersed in a glass vessel filled with diluted oil of vitriol. They were at times also compared in cold more violent than the natural cold of the climate, by adding snow to the acid in which they were tried, in which case care was taken to keep the mixture frequently stirred. Oil of vitriol was recommended for this purpose, as a fluid which would most likely bear any degree of cold without freezing, and whose natural cold might be much increased by the addition of snow. It seems to have answered the purpose very well, and not to have been attended with any inconvenience,

During the first comparison of these thermometers, a whitish globule, such as those which appear in frozen oil, was observed in the tube of the thermometer filled with oil of sassafras. This appearance of congelation did not much increase; but 2 days after a large air bubble was found in its ball, which prevented Mr. M'Nab from making further observations with it. It is well known, that spirit of wine expands more by a given number of degrees of a mercurial thermometer in warm temperatures than in cold ones; and this inequality, as might be expected, was less in the stronger spirit than in the weaker, but the difference was inconsiderable. The oil of sassafras also had some of this inequality, but much less. It however appears to be by no means a proper fluid for filling thermometers with. No appearance was observed which indicates any considerable irregularity in the contraction of spirits of wine in intense cold, or which renders it probable, that thermometers filled with it could be sunk by a mixture of snow and spirit of nitre to a degree near approaching to that mentioned by Professor Braun.

Mr. M'Nab in his experiments sometimes used one thermometer and sometimes another; but in the following pages I have reduced all the observations to the same standard; namely, in degrees of cold less than that of freezing mercury I have set down that degree which would have been shown by the mercurial thermometer in the same circumstances; but as that could not have been done in greater degrees of cold, as the mercurial thermometer then becomes of no use, I found how much lower the mercurial thermometer stood at its freezing point, than each of the spirit thermometers, and increased the cold shown by the latter by that difference.

On the Common and Dephlogisticated Acids of Nitre.—Many experiments show, that both these acids are capable of a kind of congelation, in which the whole, and not merely the watery part, freezes. Their freezing point also differs greatly according to the strength, and varies according to a very unexpected law. Like water too they bear being cooled very much below their freezing point before the congelation begins, and as soon as that takes place, immediately rise up to the freezing point. The white colour of the ice in these experiments seems owing only to its consisting of very slender filaments; for in some cases, where it froze slower, and where, in consequence, it shot into larger solid masses, they were transparent, and of the same colour as the acid itself. By the continuance of a sufficient cold, the acid, which by hasty freezing put on the white appearance, would become hard solid ice, but still retained its white appearance, owing perhaps to the filaments first shot, consisting of an acid differing in strength from that which froze afterwards, and filled up the interstices. In all these experiments, whether the ice was formed into minute filaments or solid masses, still, whenever there was a sufficient quantity of fluid matter to admit of it, they constantly subsided to the bottom; a proof that the frozen part was heavier than the unfrozen. The difference indeed is so great, that in one case where it froze into solid crystals on the surface, these crystals, when detached by agitation, fell with force enough to make a tinkling noise against the bottom of the glass.

These acids contract very much on freezing. Whenever the acid is frozen solid, the surface, instead of being elevated in ridges, like frozen water, is depressed and full of cracks. In one experiment Mr. M'Nab, after a glass almost full of acid was nearly frozen, filled it to the brim with fresh acid; and then, after it was completely frozen, the surface was visibly depressed, with fissures $\frac{1}{8}$ of an inch broad, extending from top to bottom. It is this contraction of the acid in freezing which makes the frozen part subside in the fluid part; as it was found, in the undiluted acid, that the latter consisted of a stronger, and consequently heavier acid than the former. But still the subsidence of the frozen part shows that the ice is not mere water, or even a very dilute acid; which indeed was proved by the examination of the liquors sent home. The experiments show, that though the acids bear being cooled greatly below the freezing point, without any congelation taking place, yet as soon as they begin to freeze they immediately rise up to their freezing point; and this point is always very nearly, if not exactly, the same in the same acid; for those acids were frozen and melted again 3 or 4 times, and were cooled considerably more below the freezing point in one trial than another, and yet as soon as they began to freeze the thermometer immersed in them constantly rose nearly to the same point.

The quantity which these acids will bear being cooled below the freezing point, without freezing, is remarkable. The diluted spirit of nitre, whose freezing point is — $1^{\circ}\frac{1}{2}$, once bore being cooled to near — 39°, without freezing, that is, near 37 degrees below its freezing point. The diluted dephlogisticated spirit of nitre, whose freezing point is — 5°, bore cooling to — 35°; and the dephlogisticated spirit of nitre (141) whose true freezing point is most likely — 19°, bore being cooled to — 49°: perhaps too they might have borne to be cooled considerably lower without freezing, but how much does not appear. It must be observed however, that the same diluted spirit, which at one time bore being cooled to — 39°, at another froze, without any apparent cause, when its cold was certainly less than — 30°, and most likely not much below — 18°.

The freezing point differs remarkably, according to the strength of the acid. In the diluted dephlogisticated and common spirit, the freezing point was — 5° and — 1°$\frac{1}{2}$. In the dephlogisticated and common spirit, the decanted parts of which were stronger than the foregoing in scarcely so great a proportion as that of 4 to 3, it seemed to be — 30° and — 31°$\frac{1}{2}$. It may indeed be suspected, that as this point was determined only by pouring a small quantity of the acid into a glass, at a time when the air and glass were much colder than the acid, these decanted liquors might be cooled by the air and glass, and so make the freezing point appear lower than it really was: but I do not think this could be the case; for as the decanted liquors were full of small filaments of ice, they could hardly be cooled sensibly below their freezing points without freezing; and any cold, communicated to them by the air or glass, would serve only to convert more of them into ice, without sensibly increasing their cold: so that I think this experiment determines the true freezing point of their decanted part; but it must be observed, that as the decanted part was rather stronger than the rest, it is very possible that the freezing point of the undecanted part might be considerably less cold.

A circumstance which might incline one to think, that the way by which the freezing point was determined in this experiment is defective, is, that the freezing point of the dephlogisticated acid N° 27, though nearly of the same strength as the last-mentioned, but rather stronger, was much less low, being only — 19°. But I have little doubt that the true reason of this is, that in the former acid the strength of the decanted part, which is the part whose freezing point was tried, was found to be at least $\frac{1}{20}$ greater than that of the whole mass; whereas in N° 27 the fluid part was in all probability not sensibly stronger than the whole mass; for as N° 27 was cooled only 7° below the freezing point, and its temperature was tried soon after its beginning to freeze, not much of the acid could have frozen; whereas the other was cooled 15° below its freezing point, and was exposed for an hour or two to an air not much less cold, in consequence of which a considerable part of the acid must have frozen; so that in all probability the acid, whose freezing point was found to be — 30°, was in reality $\frac{1}{20}$ part stronger than that whose freezing point was — 19°.

If this reasoning be just, the freezing point of these acids is as follows:

		Freezing point.
Dephlogisticated spirit of nitre, whose strengt=	.56	— 30°
	.53	— 19
	.437	— 4½
Common spirit of nitre, whose strength =	.54	— 31½
	.411	— 1½

On the phenomena observed on mixing snow with these acids.—On Dec. 13, snow was added to the spirit of nitre N° 168. The snow was put in very gradually, and time was taken to find what effect each addition had on the thermometer and mixture, before more was added. The temperature of the acid before the mixture was — 27°, and each addition of snow raised the thermometer a little, till it rose to — 1°$\frac{1}{4}$; after which the next addition made it sink to — 2°; which showed that sufficient snow had then been added. The quantity of snow used was pretty exactly $\frac{4}{10}$ of the weight of the acid, the weight of the acid being 13 oz. so that the strength of the diluted acid was reduced to .411. The acid before the addition of snow had no signs of freezing, its temperature being in all probability much above its freezing point; yet the snow did not appear to dissolve, but formed thin white cakes, which however did not float on the surface, but fell to the bottom, and when broken by the spatula formed a gritty sediment; so that it appears, that these cakes are not simply undissolved snow, but that the adjoining acid absorbed so much of the snow in contact with it, as to become diluted sufficiently to freeze with that degree of cold, and then congealed into these cakes. The quantity of congealed matter seems to have kept increasing till the end of the experiment.

On Dec. 21, an experiment was made in the same manner with the dephlogisticated spirit of nitre N° 27. The acid began to freeze in pouring it into the jar in which the mixture was to be made, and stood stationary there at — 19°; so that the liquor at the beginning of the experiment was white and thick, which made the effect of the addition of the snow less sensible. However the congealed matter constantly subsided to the bottom, and the quantity seems to have continued increasing to the end of the experiment. The heat of the mixture rose to — 4° before cold began to be produced, and the quantity of snow added was $\frac{22}{100}$ of that of the acid, so that the strength of the acid was reduced to .437 by the dilution.

A very remarkable circumstance in this experiment is, that the acid, while the snow was adding, first became of a yellowish, and afterwards of a greenish or bluish hue. This colour did not go off by standing, but continued at least 10 days, during which time the acid constantly kept that colour, except when by hasty freezing it shot into small filaments, in which case it put on the white appearance which these acids always assumed under those circumstances; but once that by gradual freezing it shot into transparent ice, which was of a bluish colour.

It is remarkable, that in both these experiments the addition of snow produced heat, till it arrived pretty exactly at what was found to be the freezing point of the diluted acid; but that as soon as it arrived at that point, the addition of more snow began to produce cold. This can hardly be owing merely to accident, and to both acids having happened to be of that precise degree of heat before the experiment began, that their heat after dilution should coincide with the freezing point answering to their new strength. The true cause seems to be as follows. It will be shown presently that the freezing point of these acids, when diluted as in the foregoing experiments, is much less cold than when they are considerably more diluted; and it was before shown to be much less cold than when not diluted; so that there must be a certain degree of strength, not very different from that to which these acids were reduced by dilution, at which they freeze with a less degree of cold than when they are either stronger or weaker. Now in these experiments, the temperature of the liquors before dilution was below this point of easiest freezing, and a great deal of the acid was in a state of congelation all the time of dilution; the consequence of which is, that when they were diluted to the strength of easiest freezing, they would also be at the heat of easiest freezing; for they could not be below that point, because, if they were, so much of the acid would immediately freeze as would raise them up to it; and they could not be above it, for, if they were, so much of the congealed acid would dissolve as would sink them down to it. After they were arrived at this strength of easiest freezing, the addition of more snow would produce cold, unless this strength be greater than that at which the addition of a small quantity of snow begins to produce cold; but even were this the case, heat would not be produced, but the temperature of the acids would remain stationary till they be so much diluted that the addition of more snow should produce cold. So that, in either case, the heat of the acids, at the time that the addition of fresh snow began to produce cold, must be that of easiest freezing; and consequently, as this heat was found to coincide very nearly with the freezing point of these acids, after dilution, it follows that their strengths at that time could differ very little from the strength of easiest freezing. If the temperature of the liquors at the beginning of the experiment had been above the point of easiest freezing, none of the acid would have congealed during the dilution, and nothing could have been learnt from the experiment relating to the point of easiest freezing; but the heat would have kept increasing, till the acid was diluted to that degree of strength at which the cold produced by the dissolving of the snow was just equal to the heat produced by the union of the melted snow with the acid; after which the addition of more snow would begin to produce cold.

In the dephlogisticated spirit of nitre the freezing points answering to the strength of .434, .53, and .56, were said to be $-4^\circ\frac{1}{2}$, -19°, and -30°; and the differences of -30° and -19° from $-4^\circ\frac{1}{2}$ are to each other very nearly in the duplicate ratio of .126 and .096, the differences of the corresponding strengths from .434; which, as .434 is the strength of easiest freezing, is the proportion that might naturally be expected, and consequently serves in some measure to confirm the foregoing reasoning.

After Mr. M'Nab had diluted these acids as above-mentioned, he divided each of them into 2 parts, and tried what degree of cold could be produced by mixing them with snow. On January 15th, one of these parts of the common spirit of nitre was tried. It was fluid when the experiment began, though its temperature, as well as that of the snow, was $-21^\circ\frac{1}{2}$; but on adding snow it immediately began to freeze, and grew thick, and its heat increased to $-2^\circ\frac{1}{2}$; but by the addition of more snow it quickly sunk again, and at last got to $-43^\circ\frac{1}{4}$. During the addition of the snow, the mixture grew thinner, and by the time it arrived at nearly the greatest degree of cold, consisted visibly of 3 parts: the lowest part, which consisted of frozen acid, was white and felt gritty; the upper part, which occupied about an equal space, was also white, but felt soft, and must have consisted of unmelted snow; the other part, which occupied by much the smallest space, was clear and fluid. The quantity of snow added was about $\frac{9}{13}$ of the weight of the acid, and consequently its strength was reduced to .243.

Though snow was added to the acid in this experiment as long as, and even longer than, it produced any increase of cold, yet some days after, on adding more snow to the mixture, while it was fluid, and of the temperature of $-40^\circ\frac{3}{4}$, the cold was increased to $-44^\circ\frac{1}{4}$, or 1 degree lower than before. Mr. M'Nab did not perceive the snow to melt, though in all probability some must have done so, or no cold would have been produced. The cause of this seems to be, that in the preceding experiment the congealed part of the acid was stronger than the fluid part; so that, though the fluid part was not strong enough to dissolve snow in a cold greater than $-43^\circ\frac{1}{4}$, yet the whole acid together was strong enough to do it in a cold 1° greater. A circumstance occurred in the last experiment which I cannot at all see the reason of; namely, a small part of the acid being poured into a saucer, before the addition of the snow, it was in an hour's time changed into solid ice, though the cold of the air, at the time the acid was poured out, was only $-41^\circ\frac{1}{4}$, and does not seem to have increased during the experiment.

On December 30, the other half of the same acid had been tried in the same manner: at the beginning of the experiment not more than $\frac{1}{9}$ part of the acid was fluid, the rest solid clear ice; its temperature was $-34^\circ\frac{1}{2}$, and that of the snow nearly the same; the greatest degree of cold produced was $-42^\circ\frac{3}{4}$; and the quantity of snow employed was about $\frac{1}{18}$ of the weight of the acid; so that the strength of the mixture was .38. The freezing point of the acid thus diluted appears to be about $-45^\circ\frac{1}{4}$; for by the increase of warmth during the day-time, most of the congealed matter dissolved; but in the evening it began to freeze again, so as to become thicker, its temperature being then — 45°¼ and the next morning it was frozen solid, its cold being 1° greater.

On December 12, the diluted spirit of nitre N° 139, whose strength was .175, was found frozen, its temperature being — 17. The fluid part, which was full of thin flakes of clear ice, and was of the consistence of syrup, was decanted into another bottle, and sent back. Its strength was .21, and was greater than that of the undecanted part in the proportion of .21 to .16; so that, as not much of the undecanted part was really congealed, the frozen part of the acid must have been much weaker than the rest, if not mere water. Accordingly, during the melting of the undecanted part, the frozen particles swam at top. Mr. M'Nab added snow to a little of the decanted liquor, but it did not dissolve; and no increase of cold was produced.

From these experiments it appears, that spirit of nitre is subject to 2 kinds of congelation, which we may call the aqueous and spirituous; as in the first it is chiefly, if not entirely, the watery part which freezes, and in the latter the spirit itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and so produce cold, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation, or even by being actually congealed. When the acid is excessively dilute, the point of aqueous congelation must necessarily be very little below that of freezing water: when the strength is .21, it is at — 17°, and at the strength of .243, it seems to be at — 44°¼. Spirit of nitre, of the foregoing degrees of strength, is liable only to the aqueous congelation, and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold, when the strength is .411, in which case the freezing point is at 1°½. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of .411 than the unfrozen part; it certainly does so when the strength is greater than .411, and there is little doubt but that it does so in the other case. At the strength of .54 the point of spirituous congelation is 31°½, and at .33 probably — 45°¼; at least one kind of congelation takes place at that point, and there is little doubt but that it is of the spirituous kind. In order to present this matter more at one view, I have given the annexed table of the freezing point of common spirit of nitre answering to different strengths.

Strength.	Freezing point.	
.54	—31½°	spirituous congelation.
.411	— 1½	
.38	—45¼	
.243	—44¼	aqueous congelation.
.21	—17	

In trying the first half of the dephlogisticated spirit of nitre, the cold produced was $-44°\frac{1}{2}$. The acid was fluid before the addition of the snow, and of the temperature of $-30°$, but froze on putting in the thermometer, and rose to $-5°$, as related before. In trying the 2d part, the acid was about 0° before the addition of the snow, and therefore had no disposition to freeze. The cold produced was $-42°\frac{1}{2}$. As the quantity of snow added in these experiments was not observed, they do not determine any points of aqueous or spirituous congelation in this acid; but there is reason to think, that these points are nearly the same as those of common spirit of nitre of the same strength, as the cold produced in these experiments was nearly the same as that obtained by the common spirit of nitre.

On the Vitriolic Acid.—On Dec. 12, the strong oil of vitriol N° 151 was found frozen, and was nearly of the colour and consistence of hogs-lard. Its temperature, found by pressing the ball of a thermometer into it, was $-15°$, and that of the air nearly the same; but in the night it had been exposed to a cold of $-33°$. It dissolved but slowly on being brought into a warm room, and was not completely melted before it had risen to $+20°$, and even then was not very fluid, but of a syrupy consistence. During the progress of the melting, the congealed part sunk to the bottom, as in spirit of nitre: and many air bubbles separated from the acid, which, when it was completely melted, formed a little froth on the surface. As soon as it was sufficiently melted to admit of it, which was not till it had risen to the temperature of $+10°$, the fluid part was decanted, and both were sent home to be examined. It appeared by another bottle of oil of vitriol, which also froze by the natural cold of the air, that this acid, as well as the nitrous, contracts in freezing.

Dec. 21, when the weather was at $-30°$, the vitriolic acid N° 103 was diluted with snow, as before directed. The snow dissolved immediately, and no signs of congelation appeared during any part of the process. The temperature of the acid rose only 1° before it began to sink, and the weight of the snow added was only $\frac{10}{122}$ of that of the acid, so that its strength was thus reduced to .605; which is therefore the best degree of strength for producing cold by the addition of snow, when the degree of cold set out with is $-30°$.

The acid thus diluted was divided into 2 parts, and the next day Mr. M'Nab tried what degree of cold could be produced by adding snow to one of them. The temperature of the air at the time was $-39°$, and the mixture sunk by the process to $-55°\frac{1}{2}$. The snow dissolved readily, and the mixture did not lose much of its fluidity till it had acquired nearly its greatest degree of cold, nor did any congealed matter sink to the bottom in any part of the process. The quantity of snow added was about $\frac{86}{100}$ of the weight of the acid, so that the strength of the mixture was about .325.

On Jan. 1, thin crystals of ice were found diffused all through this mixture, the temperature of the air being — 51°½, but that of the liquor was not tried. As this congelation must have been of the aqueous kind, and seems to have taken place at the temperature of — 51°½, it should follow, that this acid had no power of dissolving snow in a cold of 51°½; so that it does not at first appear why a cold 4° greater than that should have been produced in the foregoing experiment. The reason is, that at the time the mixture arrived at — 55°½, it appeared by the diminution of its fluidity to have contained some undissolved snow, and some more was added to it after that time, which before the first of January dissolved and mixed with the acid; so that the acid in the mixture, at the time it sunk to — 55°½, was not quite so much diluted as that which froze on January 1. This is the reverse of what happened in the trial of the nitrous acid; as in that experiment the fluid part, at the time of the greatest cold, was weaker than the whole mixture together; but it must be considered, that that mixture contained much congealed acid, as well as undissolved snow, whereas this contained only the latter.

Jan. 1, snow was added to the other half of the acid diluted on December 21. The cold produced was much greater than before, namely — 68°½; this seems to have proceeded partly from the air and materials having been 12° colder in this than in the former experiment, and partly from the snow having been added faster, so that the mixture arrived at its greatest degree of cold in 20^m, whereas it before took up 46^m. Another reason is, that the former mixture was made in too small a jar, in consequence of which it was poured into a larger before the experiment was completed, by which some cold was lost. The quantity of snow used in this experiment was less than in the former, so that the strength of the acid after the experiment was about .343. The mixture also grew much thicker, and had a degree of elasticity resembling jelly.

Great as the foregoing degree of cold is, Mr. M'Nab, on Feb. 2, produced one much greater. In hopes of obtaining a greater degree of cold by previously cooling the materials, he cooled about 7 ounces of oil of vitriol, whose strength was .629, that is, rather stronger than the foregoing, by placing the jar in which it was contained in a freezing mixture of oil of vitriol and snow; the snow intended to be used was also cooled by placing it under the vessel in which the freezing mixture was made. As soon as the acid in the jar was cooled to the temperature of — 57°½, a little of the snow was added, on which it immediately began to freeze, and rose to — 36°; but in about 40 minutes, as the jar was still kept in the freezing mixture, it sunk to — 48°; by which time it was grown very thick and gritty, especially at bottom. More of the cooled snow was then added, which in a short time made it sink to — 78°½, and at the same time the thickness and tenacity of the mixture diminished; so that by the time it arrived at the greatest degree of cold, very little thickness remained. The

the ball being soldered fast to the tube of the thermometer $7\frac{1}{2}$ lines above its reason of the greater degree of cold produced in this than in the preceding experiment is, that the acid was in a state of congelation: for as the congealed acid united to the snow and became fluid by the union, it is plain, that cold must have been produced both by the melting of the snow and by that of the acid. The day before, Mr. M'Nab, by adding snow to some of the same acid in the usual manner, when the cold of the materials was — 46°, produced a cold of only — 66°.

In these last 4 experiments the acid was reduced, by the addition of the snow, to the strengths of .325, .343, .403, and .334; and the cold produced in them was before said to be — $55°\frac{1}{2}$, — $68°\frac{1}{2}$, — $78°\frac{1}{2}$, and — 66°; whence we may conclude, that these are nearly the points of aqueous congelation answering to the foregoing strengths; only it appears that the strengths here set down are all of them rather too small.

On the mixture of oil of vitriol and spirit of nitre.—This mixture is not so fit for producing cold by the addition of snow, as oil of vitriol alone; for the cold obtained did not exceed — $54°\frac{1}{2}$, in either of the experiments tried with it. The point of spirituous congelation of this mixture, when diluted with somewhat more than $\frac{1}{10}$ of its weight of water, is about — 20°, and is much lower when the acid is considerably more diluted.

On the Spirit of Wine.—The rectified spirits N° 8 were diluted with snow, in the same manner as the other liquors; but were found not to want any, as the first and only addition of snow produced cold. The quantity added was about $\frac{1}{26}$ of the weight of the spirit. The spirit thus diluted was divided, like the other liquors, into 2 parts, and each tried separately. The first was at — 45°, before the addition of the snow, and was sunk by the process to — 56°. The snow, even at the first addition, did not dissolve well, so that the spirit immediately became full of white spots, and grew thick by the time it arrived at its greatest degree of cold. After standing some hours, the mixture rose to the temperature of — 39°, and was got clear, but yet was not limpid, but of the consistence of syrup. No cold was produced by adding snow to it in that state, though it appeared that its point of aqueous congelation was at least 6° lower than its temperature at that time: which seems to show that spirit of wine has scarce any power of dissolving snow when it wants even 6° of its point of aqueous congelation, and therefore is another instance that snow is dissolved much less readily by spirit of wine than by the nitrous and vitriolic acids. In trying the other part of the diluted spirits, the cold produced was only — $47°\frac{1}{2}$, the cold set out with being — 37°.

It appeared by the diluted spirit of wine N° 143, which on Dec. 12 froze by the natural cold of the atmosphere, and was treated in the same manner as the diluted spirit of nitre, that when highly rectified spirit of wine, such as N° 8, is diluted with $1\frac{4}{10}$ its weight of water, its point of aqueous congelation will be at $-21°$. The congealed part of the spirit was white like diluted milk, and even the decanted part, which was full of thin films of ice, had a milky hue. The fluid part was stronger than the rest, and no increase of cold was produced by adding snow to some of it, both of which are marks of aqueous congelation.

The natural cold, when these experiments were made, is remarkable; as there were at least 9 mornings in which the cold was not less than that of freezing mercury; 4 in which it was at least 8° below that point, or $-47°$; and 1 in which it was $-50°$. Whereas out of 9 winters, during which Mr. Hutchins observed the thermometer at Albany Fort, there were only 12 days in which the cold was equal to that of freezing mercury, and the greatest cold seems to have been $-45°$. I cannot learn whether the last winter was more severe than usual at Hudson's Bay; or whether Henley-House is a colder situation than Albany, which may perhaps be the case; for though it is only 130 miles distant from it, yet it stands inland, and to the w. or s. w. of it, which is the quarter from which the coldest winds blow.

XIV. New Experiments on Heat. By Col. Sir B. Thompson, Knt. F. R. S. p. 273.

Examining the conducting power of air, and of various other fluid and solid bodies, with regard to heat, I was led to examine the conducting power of the Torricellian vacuum. From the striking analogy between the electric fluid and heat respecting their conductors and non-conductors (having found that bodies, in general, which are conductors of the electric fluid, are likewise good conductors of heat, and, on the contrary, that electric bodies, or such as are bad conductors of the electric fluid, are likewise bad conductors of heat,) I was led to imagine that the Torricellian vacuum, which is known to afford so ready a passage to the electric fluid, would also have afforded a ready passage to heat. The common experiments of heating and cooling bodies under the receiver of an air-pump I concluded inadequate to determining this question; not only on account of the impossibility of making a perfect void of air by means of the pump; but also on account of the moist vapour which, exhaling from the wet leather and the oil used in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of heat: I had recourse therefore to other contrivances.

I took a thermometer tube, the diameter of whose globular bulb was just half an inch, Paris measure, and fixed it in the centre of a hollow glass ball of the diameter of $1\frac{3}{4}$ Paris inch, in such a manner, that the short neck or opening of bulb, the bulb of the thermometer remained fixed in the centre of the ball, and consequently was cut off from all communication with the external air. In the bottom of the glass ball was fixed a small hollow tube or point, which projecting outwards was soldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the space between the internal surface of the glass ball and the bulb of the thermometer was filled with hot mercury, which had been previously freed of air and moisture by boiling. The ball, and also the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The instrument now became a barometer, and the mercury descending from the ball, which was now uppermost, left the space surrounding the bulb of the thermometer free of air. The mercury having totally quitted the glass ball, and having sunk in the tube to the height of 28 inches, being the height of the mercury in the common barometer at that time, with a lamp and a blow-pipe I melted the tube together, or sealed it hermetically, about $\frac{3}{4}$ of an inch below the ball, and cutting it at this place with a fine file, I separated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now possessed a thermometer whose bulb was confined in the centre of a Torricellian vacuum, and which served at the same time as the body to be heated, and as the instrument for measuring the heat communicated.

Exper. 1.—Having plunged this instrument into a vessel filled with water, warm to the 18th degree of Reaumur's scale, and suffered it to remain there till it had acquired the temperature of the water, that is, till the mercury in the inclosed thermometer stood at 18°, I took it out of this vessel and plunged it suddenly into a vessel of boiling water, and holding it in the water (which was kept constantly boiling) by the end of the tube, in such a manner that the glass ball, in the centre of which was the bulb of the thermometer, was just submerged, I observed the number of degrees to which the mercury in the thermometer had arisen at different periods of time, counted from the moment of its immersion. Thus, after it had remained in the boiling water 1 min. 30 sec. the mercury had risen from 18° to 27°; after 4 minutes had elapsed, it had risen to $44°\frac{9}{10}$; and at the end of 5 minutes it had risen to $48°\frac{2}{10}$.

Exper. 2.—Taking it now out of the boiling water I suffered it to cool gradually in the air, and after it had acquired the temperature of the atmosphere, which was that of 15° R. the weather being perfectly fine, I broke off a little piece from the point of the small tube which remained at the bottom of the glass ball, where it had been hermetically sealed, and of course the atmospheric air rushed immediately into the ball. The ball surrounding the bulb of the thermometer being now filled with air, I re-sealed the end of the small tube at the bottom of the glass ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the instrument so prepared I repeated the experiment before-mentioned; that is, I put it into water warmed to 18°, and when it had acquired the temperature of the water, I plunged it into boiling water, and observed the times of the ascent of the mercury in the thermometer. They were as annexed.

Times elapsed.		Heat acquired.
0m	0s	18° R.
0	45	27
1	0	$34\frac{4}{10}$
2	10	$44\frac{9}{10}$
2	40	$48\frac{2}{10}$
4	0	$56\frac{2}{10}$
5	0	$60\frac{9}{10}$

Hence it appears that the Torricellian vacuum, which affords so ready a passage to the electric fluid, so far from being a good conductor of heat, is a much worse one than common air, which of itself is reckoned among the worst: for in the last experiment, when the bulb of the thermometer was surrounded with air, and the instrument was plunged into boiling water, the mercury rose from 18° to 27° in 45 seconds; but in the former experiment, when it was surrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 seconds = 90 seconds, to acquire that degree of heat. In the vacuum it required 5 minutes to rise to $48°\frac{2}{10}$; but in air it rose to that height in 2 minutes 40 seconds; and the proportion of the times in the other observations is nearly the same.

To remedy some inconveniencies in the instrument, I had recourse to another contrivance much more commodious, and much easier in the execution. At the end of a glass tube or cylinder, 10 or 11 inches in length, and near $\frac{3}{4}$ of an inch in diameter internally, I caused a hollow globe to be blown $1\frac{1}{2}$ inch in diameter, with an opening in the bottom corresponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or short tube, about an inch or $\frac{3}{4}$ of an inch in length. Having a thermometer prepared, whose bulb was just half an inch in diameter, and its freezing point fell at about $2\frac{3}{4}$ inches above its bulb, I graduated its tube according to Reaumur's scale, beginning at 0°, and marking that point, and also every 10th degree above it to 80°, with threads of fine silk bound round it, which being moistened with lac varnish adhered firmly to the tube. This thermometer I introduced into the glass cylinder and globe just described, by the opening in the bottom of the globe, having first choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its sides inwards towards its axis, leaving only an opening sufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in such a manner that the centre of its bulb coincided with the centre of the globe, I marked a place in the cylinder, about $\frac{3}{4}$ of an inch above the 80th degree or boiling point on the tube of the inclosed thermometer, and taking out the thermometer, I choked the cylinder again in this place. Introducing now the thermometer for the last time, I closed the opening at the bottom of the globe at the lamp, taking care, before I brought it to the fire, to turn the cylinder upside down, and to let the bulb of the thermometer fall into the cylinder till it rested on the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now closed, and the bulb of the thermometer being suffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as possible in the centre of the globe, and observing and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upside down, and suffering the bulb of the thermometer to enter the cylinder, and rest on the first or lower choak, the end of the tube was cut off at the mark just mentioned, and a small solid ball of glass, a little larger than the internal diameter or opening of the choak, was soldered to the end of the tube, forming a little button or knob which, resting on the upper choak of the cylinder, served to suspend the thermometer in such a manner that the centre of its bulb coincided with the centre of the globe in which it was shut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the frustrum of a hollow cone, the end of it was soldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically sealed, the barometrical tube was detached from it with a file, and the thermometer was left completely shut up in a Torricellian vacuum, the centre of the bulb of the thermometer being confined in the centre of the glass globe, without touching it in any part, by means of the two choaks in the cylinder, and the button on the end of the tube.

I provided 2 of these instruments, as nearly as possible of the same dimensions; the one called N° 1, being voided of air, in the manner above described; the other, N° 2, being filled with air, and hermetically sealed. With these two instruments I made the following experiments on the 11th of July last, at Manheim, between the hours of 10 and 12, the weather being very fine and clear, the mercury in the barometer standing at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the Academy of Manheim at 47°.

Exper. 3, 4, 5, 6.—Putting both the instruments into melting ice, I let them remain there till the mercury in the inclosed thermometers rested at the point 0°, that is, till they had acquired exactly the temperature of freezing water or melting ice; and then taking them out of the ice I plunged them suddenly into a large vessel of boiling water, and observed the time required for the mercury to rise in the thermometers from 10° to 10°, from 0° to 80°, taking care to keep the water constantly boiling during the whole of this time, and taking care also to keep the instruments immersed to the same depth, that is, just so deep that the point 0° of the inclosed thermometer was even with the surface of the water. These experiments were repeated twice, with the utmost care; and the following table gives the result of them.

Thermometer N° 1.

Time elapsed. Exp. N° 3.	Time elapsed. Exp. N° 4.	Heat acquired.
0^m 51^s	0^m 51^s	10°
0 59	0 59	20
1 1	1 2	30
1 18	1 22	40
1 24	1 23	50
2 0	1 51	60
3 30	3 6	70
11 41	10 27	80
22 44	21 1 = total time of heating from 0° to 80°.	

Total time from 0° to 70°.
In Exp. N° 3 = 11^m 3^s
In Exp. N° 4 = 10 34
Medium = 10 48½

Thermometer N° 2.

Time elapsed. Exp. N° 5.	Time elapsed. Exp. N° 6.	Heat acquired.
0^m 30^s	0^m 30^s	10°
0 35	0 37	20
0 41	0 41	30
0 49	0 53	40
1 1	0 59	50
1 24	1 20	60
2 45	2 25	70
9 10	9 38	80
16 55	17 3 = total time of heating from 0° to 80°.	

Total time from 0° to 70°.
In Exp. N° 5 = 7^m 45^s
In Exp. N° 6 = 7 25
Medium = 7 35

It appears from these experiments, that the conducting power of air to that of the Torricellian vacuum, under the circumstances described, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{2}}{60}$ inversely, or as 1000 to 702 nearly; for the quantities of heat communicated being equal, the intensity of the communication is as the times inversely.

In these experiments the heat passed through the surrounding medium into the bulb of the thermometer: in order to reverse the experiment, and make the heat pass out of the thermometer, I put the instruments into boiling water, and let them remain there till they had acquired the temperature of the water, that is, till the mercury in the inclosed thermometers stood at 80°; and then, taking them out of the boiling water, I plunged them suddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I observed the times employed in cooling as follows.

Thermometer N° 1.

Time elapsed. Exp. N° 7.	Time elapsed. Exp. N° 8.	Heat lost.
——	——	80°
1m 2s	0m 54s	70
0 58	1 2	60
1 17	1 18	50
1 46	1 37	40
2 5	2 16	30
3 14	3 10	20
5 42	5 59	10
Not observed.	Not observed.	0

Total time of cooling from 80° to 10°.
In Exp N° 7. = 16m 4s
In Exp. N° 8. = 16 16
Medium = 16 10

Thermometer N° 2.

Time elapsed. Exp. N° 9.	Time elapsed. Exp. N° 10.	Heat lost.
——	——	80°
0m 33s	0m 33s	70
0 39	0 34	60
0 44	0 44	50
0 55	0 55	40
1 17	1 18	30
1 57	1 57	20
3 44	3 40	10
40 10	Not observed.	0

Total time of cooling from 80° to 10°.
In Exp. N° 9. = 9m 49s
In Exp. N° 10. = 9 41
Medium = 9 45

By these experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{45}{60}$ to $16\frac{10}{60}$ inversely, or as 1000 to 603.

To determine whether the same law would hold good when the heated thermometers, instead of being plunged into freezing water, were suffered to cool in the open air, I made the following experiments. The thermometers N° 1 and N° 2 being again heated in boiling water, as in the last experiments, I took them out of the water, and suspended them in the middle of a large room, where the air (which appeared to be perfectly at rest, the windows and doors being all shut) was warm to the 16th degree of Reaumur's thermometer, and the times of cooling were observed as follows.

Exp. N° 11.
Thermometer N° 1.
Surrounded by a Torricellian vacuum.
Heated to 80°, and suspended in the open air warm to 16°.

Time elapsed.	Heat lost.
	80°
Not observed.	70
1m 24s	60
1 44	50
2 28	40
4 16	30

10 12 = total time employed in cooling from 70° to 30°.

Exp. N° 12.
Thermometer N° 2.
Surrounded by air.
Heated to 80°, and suspended in the open air warm to 16°.

Time elapsed.	Heat lost.
	80°
Not observed.	70
0m 51s	60
1 5	50
1 34	40
2 41	30

6 11 = total time employed in cooling from 70° to 30°.

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be nearly the same as in the foregoing experiments, being as $6\frac{11}{60}$ to $10\frac{12}{60}$ inversely, or as 1000 to 605.

As it might possibly be objected to the conclusions drawn from these experiments that, notwithstanding all the care that was taken in the constructing of the two instruments made use of that they should be perfectly alike, yet they might in reality be so far different, either in shape or size, as to occasion a very sensible error in the result of the experiments; to remove these doubts he made other experiments, with the construction of the instruments varied; and still with the same results.

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gasses, the thermometer N° 3 was constructed with a view to those experiments; and having now provided myself with a stock of those different kinds of airs, I began with fixed air, with which, by means of water, I filled the globe and cylinder containing the thermometer; and stopping up the two holes in the great stopple closing the end of the cylinder, I exposed the instrument in freezing water till the mercury in the inclosed thermometer had descended to 0°; when, taking it out of the freezing water, I plunged it into a large vessel of boiling water, and prepared myself to observe the times of heating, as in the former cases; but an accident happened, which suddenly put a stop to the experiment. Immediately on plunging the instrument into the boiling water, the mercury began to rise in the thermometer with such uncommon celerity, that it had passed the first division on the tube, which marked the 10th degree, according to Reaumur's scale, before I was aware of its being yet in motion; and having thus missed the opportunity of observing the time elapsed when the mercury arrived at that point, I was preparing to observe its passage of the next, when all of a sudden the stopple closing the end of the cylinder was blown up the chimney with a great explosion, and the thermometer, which being cemented to it by its tube, was taken along with it, and was broken to pieces, and destroyed in its fall. This unfortunate experiment, though it put a stop for the time to the inquiries proposed, opened the way to other researches not less interesting. Suspecting that the explosion was occasioned by the rarefaction of the water which remained attached to the inside of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity with which the mercury rose in the thermometer was principally owing to the same cause; I was led to examine the conducting power of moist air, or air saturated with water.

For this experiment I provided myself with a new thermometer N° 4, the bulb of which, being of the same form as those already described (viz. globular) was also of the same size, or half an inch in diameter. To receive this thermometer, a glass cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe $1\frac{1}{2}$ inch in diameter. In the centre of this globe the bulb of the thermometer was confined, by means of the stopple which closed the end of the cylinder; which stopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its centre or axis, and confined the thermometer in its place, without the assistance of any other apparatus. Through this stopple two other small holes were bored, and lined with thin glass tubes, opening a passage into the cylinder, which holes were occasionally stopped up with some stopples of cork; but to prevent accidents, such as I had before experienced from an explosion, great care was taken not to press these stopples into their places with any considerable force, that they might the more easily be blown out by any considerable effort of the confined air.

Does humidity augment the conducting power of air?

To determine this question I made the following experiments, the weather being clear and fine, the mercury in the barometer standing at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N° 17)
Thermometer N° 4.

Surrounded by air dry to the 44th degree of the quill hygrometer of the Manheim Academy.

Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
		80°
0m	34s	10
0	39	20
0	44	30
0	51	40
1	6	50
1	35	60
2	40	70
not observed.		80

8 9 = total time of heating from 0° to 70°.

(Exp N° 18)
The same thermometer (N° 4.)

Surrounded by air rendered as moist as possible by wetting the inside of the cylinder and globe with water.

Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
		0°
0m	6s	10
0	4	20
0	5	30
0	9	40
0	18	50
0	26	60
0	43	70
7	45	80

1 51 = total time of heating from 0° to 70°.

From these experiments it appears, that the conducting power of air is very much increased by humidity. To see if the same result would obtain when the experiment was reversed, I now took the thermometer with the moist air out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I observed the times of cooling, and compared the result of this experiment with those made with dry air.

Though the difference of the whole times of cooling from 80° to 10° in these two experiments appears to have been very small, yet the difference of the times taken up by the first 20 or 30 degrees from the boiling point is very remarkable, and shows with how much greater facility heat passes in moist air than in dry air.

Finding so great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of density. For this experiment I prepared the thermometer N° 4, by stopping up one of the small glass tubes passing through the stopple, and opening a passage into the cylinder, and by fitting a valve to the external overture of the other. The instrument, thus prepared, being put under the receiver of an air-pump, the air passed freely out of the globe and cylinder on working the machine, but the valve above described prevented its return on letting air into the receiver. The gage of the air-pump showed the degree of rarity of the air under the receiver, and consequently of that filling the globe and cylinder, and immediately surrounding the thermometer.

With this instrument, the weather being clear and fine, the mercury in the barometer standing at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following experiments.

(Exp. N° 20.)
Thermometer N° 4.
Surrounded by common air, barometer standing at 27 inches 9 lines.
Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
0^m	31^s	10°
0	40	20
0	41	30
0	47	40
1	4	50
1	25	60
2	28	70
10	17	80

7 36 = total time of heating from 0° to 70°.

(Exp. N° 21.)
Thermometer N° 4.
Surrounded by air rarefied by pumping till the barometer-gage stood at 6 inches 11½ lines.
Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
0^m	31^s	10°
0	38	20
0	44	30
0	51	40
1	7	50
1	19	60
2	27	70
10	21	80

7 37 = total time of heating from 0° to 70°.

(Exp. N° 22.)
Thermometer N° 4.
Surrounded by air rarefied by pumping till the barometer-gage stood at 1 inch 2 lines.
Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
0^m	29^s	10°
0	36	20
0	49	30
1	1	40
1	1	50
1	24	60
2	31	70
not observed.		80

7 51 = total time of heating from 0° to 70°.

The result of these experiments, I confess, surprised me not a little. I hope that further experiments may lead to the discovery of the cause why there is so little difference in the conducting powers of air of such very different degrees of rarity, while there is so great a difference in the conducting powers of air, and of the Torricellian vacuum.

I shall conclude this letter with a short account of some experiments I have made to determine the conducting powers of water and of mercury; and with a table, showing at one view the conducting powers of all the different mediums which I have examined. Having filled the glass globe inclosing the bulb of the thermometer N° 4, first with water, and then with mercury, I made the following experiments, to ascertain the conducting powers of those two fluids.

(Exp. N° 23.)
Thermometer N° 4.
Surrounded by water.
Taken out of freezing water, and plunged into boiling water.

Time elapsed.		Heat acquired.
0m	19s	10°
0	8	20
0	9	30
0	11	40
0	15	50
0	21	60
0	34	70
2	13	80
1	57 = total time of heating from 0° to 70°.	

(Exp. N° 24, 25, and 26.)
Thermometer N° 4.
Surrounded by mercury.
Taken out of freezing water, and plunged into boiling water.

Time elapsed.						
Exp. N° 24.		Exp. N° 25.		Exp. N° 26.		Heat acquired.
0m	5s	0m	5s	0m	5s	10°
0	4	0	2	0	5	20
0	2	0	2	0	4	30
0	4	0	5	0	5	40
0	4	0	4	0	7	50
0	7	0	4	0	8	60
0	15	0	9	0	14	70
Not observed.		0	58	Not observed.		80
0	41	0	31	0	48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the 3 experiments with mercury being 41 seconds, 31 seconds, and 48 seconds, the mean of these times is 40 seconds; and as in the experiment with water the time employed in acquiring the same degree of heat was $1^m\ 57^s = 117$ seconds, it appears from these experiments, that the conducting power of mercury to that of water, under the circumstances described, is as 40 to 117 inversely, or as 1000 to 342. And hence it is plain why mercury appears so much hotter, and so much colder, to the touch than water, when in fact it is of the same temperature: for the force or violence of the sensation of hot or cold depends not entirely on the temperature of the body exciting in us those sensations, or on the degree of heat it actually possesses, but on the quantity of heat it is capable of communicating to us, or receiving from us, in any given short period of time, or as the intensity of the communication; and this depends in a great measure on the conducting powers of the bodies in question. The sensation of hot is the entrance of heat into our bodies; that of cold is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the sensation. And this is another proof that the thermometer cannot be a just measure of the sensible heat or cold existing in bodies; or rather, that the touch does not afford us a just indication of their real temperatures.

A Table of the conducting Powers of the under-mentioned Mediums, as determined by the foregoing Experiments.

Thermom. N° 1. Thermom. N° 4.

Taken out of freezing water, and plunged into boiling water.

Time elapsed.

Torricellian Vacuum (Exp. N° 3, 4, and 13.)	Common air, density = 1, (Exp. N° 20.)	Rarefied air, density = $\frac{1}{4}$ (Exp. N° 21.)	Rarefied air, density = $\frac{1}{24}$ (Exp. N° 22.)	Moist air (Exp. N° 18.)	Water (Exp. N° 23.)	Mercury (Exp. N° 24, 25, and 26.)	Heat acquired.
—	—	—	—	—	—	—	0°
0^m 52^s	0^m 31^s	0^m 31^s	0^m 29^s	0^m 6^s	0^m 19^s	0 5^s	10
0 58	0 40	0 38	0 36	0 4	0 8	0 $3\frac{2}{3}$	20
1 3	0 41	0 44	0 49	0 5	0 9	0 $2\frac{2}{3}$	30
1 18	0 47	0 51	1 1	0 9	0 11	0 $4\frac{2}{3}$	40
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 $6\frac{1}{3}$	60
3 19	2 28	2 27	2 31	0 43	0 34	0 $12\frac{2}{3}$	70
11 57	10 17	10 21	——	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 $36\frac{2}{3}$	

= total times of heating from 0° to 70°.

In determining the relative conducting powers of these mediums, I have compared the times of the heating of the thermometers from 0° to 70° instead of taking the whole times from 0° to 80°, on account of the small variation in the heat of the boiling water arising from the variation of the weight of the atmosphere, and also on account of the very slow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the precise moment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury = 1000, the conducting powers of the other mediums, as determined by these experiments, will be as annexed, viz.

Mercury	1000
Moist air	330
Water	342
Common air, density = 1 . .	$80\frac{2}{5}$
Rarefied air, density = $\frac{1}{4}$ . .	$80\frac{1}{4}$
Rarefied air, density = $\frac{1}{24}$	78
The Torricellian vacuum . .	55

And in these proportions are the quantities of heat which these different mediums are capable of transmitting in any given time; and consequently these numbers express the relative sensible temperatures of the mediums, as well as their conducting powers. How far these decisions will hold good under a variation of cir cumstances, experiment only can determine.

XV. History and Dissection of an Extraordinary Introsusception. By John Coakley Lettsom, M. D. F. R. S. and A. S. p. 305.

A. B. a child 4 years old, was first indisposed about the middle of Sept. 1784. When I was consulted, says Dr. L., which was on the 7th of Oct. the symptoms resembled those of a cholera morbus. At this period however the diarrhœa had ceased; but the patient continued frequently to vomit, especially after taking nourishment. On the 20th a dysentery succeeded, with mucous and bloody stools, which ceased after a few days continuance, when she nearly recovered her former state of health, without even reaching after her usual food. She was now removed into the country; and I did not hear of her again till Dec. when she was brought to town, on account of the return of the dysentery, which was then accompanied with a troublesome tenesmus, and a considerable degree of fever.

By anodyne medicines, and the use of opiate clysters, these complaints were occasionally moderated, and sometimes they totally ceased for a few days, but seldom longer, and the intervals of relief gradually shortened; the attacks became also more violent, commencing with violent rigors, and fever succeeding; the pulse got weaker and weaker, and the patient became extremely extenuated in flesh; and towards the conclusion of this month, after repeated vomitings of a dark-coloured fluid, like coffee grounds, it finished its painful existence. Bleeding, before the debility was become alarming, afforded no material respite. Fomentations to the abdomen, and tepid bathing of the whole body, were equally ineffectual. Anodyne starch clysters afforded some truce, but it could not be durable; the nature of the mischief was too momentous to afford any hope of permanent relief, as the dissection after death will evince.

Examination of the Body after Death, by Mr. Thos. Whately, Surgeon.—On exposing the cavity of the abdomen, the sigmoid flexure of the colon immediately presented itself to view, enlarged to the size of that of an adult, as also a large distended intestine appearing to be at first view a continuation of the transverse arch of this gut; and at the place where this seeming arch joined the sigmoid flexure, there appeared a firm or tight band, as if surrounding the intestine, and here it was strongly bound down. On a nicer inspection this arch was found to be a portion of the ileum, which passing within the band was inclosed in the sigmoid flexure of the colon. All the parts between this portion of the small intestines and the sigmoid flexure, among which were the caput coli, the cæcum with its appendix, and the whole of the great arch of the colon, could no where be seen. The want of these parts, with the enlarged size of the sigmoid flexure, and the hard feel evidently showing that it contained some substance, left no room to doubt, but that all the missing portion of the intestines was contained within the sigmoid flexure. A finger introduced into the anus felt a round substance in the rectum, with an opening in the middle, not unlike the os tincæ. This substance did not adhere, the finger passing round it freely, between it and the internal coat of the rectum. The liver, the urinary bladder, and small intestines, were the remaining parts which first appeared when the parietes of the abdomen were turned back.

Upon looking for the omentum, a portion of it only was found, attached to the stomach, the remaining part evidently passed within the band into the sigmoid flexure. The stomach was tied much closer to the spine than natural, by the displacing of the omentum and great arch of the colon. The gall bladder was as large as that of an adult, and was full of thin bile, but without obstruction to its passage into the duodenum. The general external appearance of all the intestines was natural, except slight inflammation in some places. The cavity of the abdomen also contained more than half an ounce of thin pus; and on the right side were two ligamentous peritoneal substances, very much on the stretch; one formed by an extension of that part of the peritoneum called ligamentum coli dextrum; the other at the place where the colon is connected to the peritoneum over the right kidney.

As the further investigation of this uncommon disease required particular attention, I cut out all the parts connected with it, bringing away the whole sigmoid flexure of the colon, with the rectum, anus, uterus, and bladder; also a part of the arch of the ileum with the omentum, and a portion of the stomach and duodenum.

I made a longitudinal incision through the coats of the sigmoid flexure of the colon, from the anus to the band at its upper part. Within the cavity, which was lined with mucus, appeared a large intestine, taking on the form of the sigmoid flexure, which on examination proved to be the great arch of the colon and the cæcum inverted; so that the villous coat was external, and in contact with the villous coat of the sigmoid flexure, through the whole extent of both; at the extremity of which inverted intestine were two apertures, viz. the large one felt by the finger per anum, and a smaller one. It now plainly appeared, that the band was formed by the upper part of the sigmoid flexure being drawn tight by the inversion of the part of the colon immediately above it, the further progress of which was prevented by the peritoneal connections at that place not giving way; which caused it to appear as a band tying the intestine down. This inclosed intestine was very much diseased, the upper part next the band being highly inflamed, and as it approached the caput coli in the rectum gradually terminated in mortification, so that for 3 inches from its extremity it was perfectly black. No adhesion whatever appeared between the coats of these intestines, as this inverted colon might be lifted out of the sigmoid flexure to the band.

On cutting through the coats of this inverted intestine, it was observed that they were very much thickened and diseased; the enlargement of the gut, which was fully equal to that of an adult, consisting chiefly in a thickening of its various muscular fibres*. The peritoneal coat, now become its internal surface, was every where highly inflamed, but not black as on the outside, the inflammation gradually increasing from the band to the extremity of the cæcum. Through the whole length of its cavity was included a portion of the ileum uninverted, with its connecting mesentery, which communicated with the larger aperture above described at the extremity of the cæcum, and with the arch of the ileum above the band. It was contracted in size, but was nearly free from thickening or inflammation; some adhesions only connected it with the coats of the colon; but the portion above the band was at least 4 times as large, thus resembling in magnitude, as well as occupying the place of the great arch of the colon. Besides this intestine, this cavity contained a portion of the omentum continued from that above, passing within the band, and extending half-way to the rectum; an enlarged cluster of mesenteric glands, of the size of a pigeon's egg, which just emerged from under the band, and were connected with a portion of the mesentery above; and, at the lower part, the appendix vermiformis larger and longer than natural, but likewise uninverted, the mouth of the cavity of which formed the smaller opening in the cæcum before mentioned.

As long as the parts had been in this very uncommon situation, the fæces must have passed through the valve of the colon, directly into the very lowest part of the rectum, without ever coming in contact with any portion of the large intestines. And in the last week of the child's life, when a prolapsus frequently happened, the fæces passed directly from the ileum into the night-stool. The arch of the ileum, in default of that of the colon, formed the reservoir for the fæces; which, with the partial interruption to their passage by the stricture occasioned by the band, probably caused its enlargement. But the fæces contained in it were of a thinner consistence, and wanted the fætor usually met with in the colon.

XVI. New Experiments on the Ocular Spectra of Light and Colours. By Robert Waring Darwin, M.D.; communicated by Erasmus Darwin, M.D., F.R.S. p. 313.

Reprinted in Dr. Darwin's Zoonomia.

* The increased action of these muscles, necessarily attendant on their inverted state, would increase the size of their muscular fibres, as happens in the bladder, when it acts frequently.—Orig.

XVII. Observations on some Causes of the Excess of the Mortality of Males above that of Females. By Joseph Clarke, M. D., Physician to the Lying-in Hospital at Dublin. Communicated by the Rev. Rich. Price, D. D., F. R. S. p. 349.

In the letter accompanying Dr. Clarke's communication, Dr. Price remarks that, the observations which have been made on the laws that govern human mortality prove, that the mortality of males exceeds that of females in almost all the stages of life, and particularly in the earliest stages; and that this excess prevails most in great towns, and all the less natural situations of human life. The facts in these papers throw some light on this subject. Male fœtuses requiring more nutrition than female fœtuses, because larger, and being also for this reason more liable to injury in delivery, are brought into the world less perfect; and this happening more or less in proportion to the vigour and just formation of the mother, it must happen most in those situations where the greatest tenderness of frame and deviations from nature take place. The truth, in short, seems to be, that any debility in either parent must affect most the production of that sex which requires the largest and strongest stamina; and that such debilities prevailing most in great towns and polished societies, the excess of the mortality of males must also be greatest in such situations. And this Dr. P. reckons the principal reason of a circumstance in human mortality, which, before he had received these communications from Dr. Clarke, he did not so well understand.

Dr. Clarke begins his first letter to Dr. Price by remarking that in his (Dr. P.'s) very useful Treatise on Life Annuities, &c. " it has been observed," (vol. 1, p. 373) " that the author of nature has provided, that more males should be born than females, on account of the particular waste of males, occasioned by wars and other causes. That perhaps it might have been observed, with more reason, that this provision had in view that particular weakness or delicacy in the constitution of males which makes them more subject to mortality; and which consequently renders it necessary that more of them should be produced, in order to preserve in the world a due proportion between the sexes." And that he remarks, (vol. 2, p. 247) that " the facts recited at the end of his 4th essay prove, that there is a difference between the mortality of males and females; but that he must however observe, that it may be doubted, whether this difference, so unfavourable to males, be natural; and that there are facts which prove that there is reason for such a doubt." After stating a number of very satisfactory facts of this kind it is remarked, that " the inference from them is very obvious; that they seem to show sufficiently, that human life in males is more brittle than in females, only in consequence of adventitious causes, or of some particular debility which takes place in polished and luxurious societies, and especially in great towns."

What those adventitious causes are, or how this particular debility is produced and operates, are questions which appear highly interesting and curious. Dr. C. had therefore been at considerable pains to examine and arrange a very accurate and extensive registry in such a manner as he hoped would throw some light on these questions. As it is to the accuracy of modern registers that we are originally indebted for our knowledge of the facts in question, he apprehends, it is from the same source only that we shall be enabled satisfactorily to explain them. Of the registry inclosed he observes, that it had been kept from its commencement by a man of uncommon accuracy, one of the under-clerks of our House of Commons; and that as the poor women and their children are obliged to pass through his office, before leaving the hospital, his situation is such that there is no likelihood of his being deceived. It exhibits the occurences of 28 years in above 20,000 instances; a number which he was inclined to think could hardly appear insufficient for establishing some general inferences and conclusions on a tolerably sure foundation. Though his reasoning on these matters should not appear very conclusive, or his calculations perfectly accurate, yet he flattered himself, that the facts would neither be unacceptable nor useless.

He believes it may be safely asserted, that anatomy has not hitherto detected any internal difference between the animal economy of the male and female, which can be supposed to account for their difference of mortality, more especially in early infancy; and this is the period during which the chances are much the greatest against male life. It is a matter of common observation that males, cæteris paribus, grow to a greater size than females, both in utero and every subsequent period of their growth. Consequently they must meet with more difficulty, and endure more hardship and fatigue, in the hour of birth. Accordingly, practitioners in midwifry, taught by experience, know that when any considerable difficulty occurs in the birth of a child, for example in all the different kinds of preternatural labours, they stand a much better chance of saving the life of a female than of a male. It is on this principle we can explain what our registry concurs with others in proving, viz. that near one-half more males than females are still-born. Naturalists are agreed, that the head of the human fœtus is larger in proportion to its body than that of any other animal; and he believes it is certain, that no animal whatever brings forth its young with so much difficulty, pain, and danger, as a woman. Now as we know that the head contains one of the most important organs of the body to life, it is highly reasonable to suppose, that any additional injury which it sustains in delivery may produce very material effects on the whole system. These effects, though often, may not be always immediate. They may operate in weakening the male constitution so as to render it more apt to be affected by any exciting cause of disease soon after birth, and less able to struggle against it. It may be asked, how this will apply to the difference of mortalily in great towns and country situations? The answer evidently is, that in great towns, rickets, scrophula, and other diseases affecting the bones, and producing consequent mal-conformation of the female sex, are more frequent than in healthy country situations.

There is another circumstance which may have some influence in producing that particular debility above-mentioned. It is this: as the stamina of the male are naturally constituted to grow to a larger size, a greater supply of nourishment in utero will be necessary to his growth than to that of a female. Defects in this particular, proceeding from delicacy of constitution or diseases of the mother, must of course be more injurious to the male sex. And though the male children may be so lucky as to escape abortion and the perils of delivery, it is probable that they will be more apt to languish under disease, or die at some future period, from the application of noxious causes to an originally half-starved frame. To a person little accustomed to consider physiological subjects, this reasoning may appear somewhat obscure. It may perhaps be somewhat illustrated by considering that nourishment of the fœtus after birth which nature has provided for. Suppose every mother in a great city obliged to suckle and nurse her own child, without the assistance of spoon-meat; and every mother in the adjacent country to do the same. Of the former there would not perhaps be 1 good nurse in 5; and of the latter perhaps not 1 bad in 10. The difference of mortality that would ensue both to mothers and children thus situated, and the greater sufferings of the male than female sex, may be easily conceived, but not easily calculated. We see that when a woman conceives twins, and has 2 fœtuses in utero to nourish instead of one, it becomes peculiarly fatal both to her and her offspring. The chances are above 4 to 1 greater against her, than against a woman bringing forth 1 child, and about 2 to 1 against her issue.* The facts relating to twins are singular and curious, at the same time that they serve to confirm some of the preceding reasoning. Near $\frac{1}{2}$ more twins die, and near $\frac{1}{3}$ more are still-born, than of single children. And why?—It is not because they meet with greater difficulties in the birth. On the contrary, it is a known fact, that, being much smaller than other children, women bring them forth with more ease. Does it not then proceed from a scanty nutrition, by which they are oftener blighted in utero than single children; and, when born alive, have less strength to support life through the first stages of its existence.

It is further worthy of observation, that though double the numbers of twins die and are still-born, compared to single children, yet the proportion of male twins lost to females is less. Only $\frac{1}{5}$ more of the male sex die than of the female, and only $\frac{1}{3}$ more is still-born. Whereas of single children, whose proportional

* Compare the 7th and 14th, 6th and 13th inferences in the annexed extracts.—Orig.

mortality is $\frac{1}{2}$ less, $\frac{1}{4}$ more of the male sex die, and near double the number is still-born. To what then are we to attribute this lessened mortality in favour of male twins? Probably to their brain and nervous system suffering less during delivery, on account of their heads being much smaller than those of single children. There is one circumstance remaining, relative to the proportion of the sexes, which may be noticed. We see evident wisdom in the creation of a greater number of males than females; but why the proportion they bear to each other differs in different countries and situations, and why there should be a 17th more males born of single children than twins, are questions which he leaves to be decided by those philosophers who understand the theory of generation better than he does. Be this as it may, he is convinced that the majority in favour of the male sex is sooner destroyed than the generality of writers seem to be aware of. Did the limits of this letter permit, he thinks he could prove from Dr. Short's own data,* that the majority of males is destroyed long before the common marriageable period; but he contents himself with an observation or two on the registry before us. If $\frac{1}{2}$ of the whole born in this hospital die before 3 years, which is the established computation for great cities; and if, on the loss of somewhat more than a third of this half, a majority of 1177 be reduced to 483 by a loss of 694, as appears from the registry, it is pretty evident, that by the death of the 2 remaining thirds, a majority will be left in favour of the female sex. It is obvious, that the statement with regard to twins corroborates this supposition: for of them instead of $\frac{1}{5}$, there is near $\frac{1}{2}$ dead and still-born, the consequence of which is, that we send out a majority of females. It may be objected, that their males do not bear so great a proportion to the females; and that therefore it is not to be expected they should keep up their majority so long. But there is only a 17th fewer males produced; whereas it has been already shown, that there is a much greater proportion between the deaths of single and twin males against the former, and in favour of the latter.

In his 2d letter Dr. Clarke states, that with the view of ascertaining how far some of the foregoing conjectures are well founded, and of determining with greater precision the more obvious differences between the male and female sex in infancy, he began in July 1785, by weighing 40 children, 20 of each sex, and by taking the dimensions of their heads. In the months of August and Sept. he repeated the same experiment twice, taking such children as appeared to have arrived at the full period of gestation promiscuously as they happened to be born. He weighed them all a few hours after birth, before they had taken food, and before purgative medicines had time to operate. For this purpose, he made use of a small spring or pocket steelyard, which weighs any thing not hea-

* New Observations, p. 72, et seq.—Orig.

vier than a few pounds, appended to it with sufficient accuracy. To this was attached a flannel bag, into which the children were put, at first naked; but this he soon found very troublesome. The nurses often wanted time sufficient to assist him, and timid mothers were afraid of their infants catching cold; he was therefore obliged to weigh them with their clothes on, and to subtract a certain quantity from the gross weight of each child, according as it was full, middling, or light clothed. Whatever inaccuracy this may have introduced, as to the real weight of the children, it can but little influence their comparative weights, or the differences between the 2 sexes, which it was the object to ascertain.

For measuring their head, he made use of a piece of painted or varnished linen tape, divided into inches, halves, and quarters. The varnish has the good effect of preventing the length of such a measure being readily affected by variations in the humidity of the atmosphere, &c.; and it has little or no elasticity. In this part of the experiment then he could pretend to considerable accuracy. He took first the greatest circumference of the head from the most prominent part of the occiput around over the frontal sinuses; and, 2dly, the transverse dimension from the superior and anterior part of one ear, across the fontanelle, to a similar part of the opposite ear. These dimensions appeared the most likely to afford data for determining the respective sizes of the brain in the different sexes. The result was as follows:

	Twenty males.			*Twenty females.*		
	Weight.	Circumference of heads.	Dimensions from ear to ear.	Weight.	Circumference of heads.	Dimen. from ear to ear.
	lbs. &c.	Inches.	Inches.	lbs. &c.	Inches.	Inches
Exp. 1	149½	282	152	137¼	272	143
Exp. 2	144½	277	146¼	135	272	147
Exp. 3	148	280	147½	132	273	143¼
Totals	442	839	445¾	404¼	817	433¼
Average	7 lb. 5 oz. 7 dr.	14	7¼	6 lb. 11 oz. 6 dr.	13⅝	7²⁄₉

Having found the relative proportions between the sexes to turn out thrice with so much uniformity, and observing them to correspond pretty nearly with some experiments, made for very different purposes by the late Professor Roederer, of Gottingen, he did not think it necessary to prosecute the subject further.

On the whole, it may be observed, that the difference of weight between the male and female at birth may be rated at about 9 oz., or nearly $\frac{1}{12}$ part of the original weight. In the circumference of their heads there is a difference of near $\frac{1}{2}$ an inch, or about a 28th or 30th part; and the same proportion of a 28th is pretty nearly preserved in the transverse dimension. It is evident, as the bony passage through which infants pass is of a certain determined capacity, that were their heads equally incompressible with those of adults, the difference of half an inch in their size would often prove fatal to them. By the compressibility of their heads, however, in well formed women, this difficulty is by time surmounted.

The effects that such a compression on the brain may produce, have not hitherto been well attended to. In reckoning children, weighing from $5\frac{1}{2}$ to $6\frac{1}{2}$, 6 lb. weight, and from $6\frac{1}{2}$ to $7\frac{1}{2}$, 7, and so forth, in order to avoid fractions, the numbers of males and females, arranged according to their weight, he found to stand as follows.

Males.								Females.							
lbs.	4	5	6	7	8	9	10	lbs.	4	5	6	7	8	9	10
N°	0	3	6	32	16	2	1	N°	2	9	14	25	8	2	9

Hence it appears, that the majority of males runs thus; 7, 8, 6, 5; while that of the females is 7, 6, 5, 8. Hence also appears the merciful dispensations of Providence towards the female sex; for when deviations from the medium standard occur, it is remarkable, that they are much more frequently below than above this standard. In 120 instances there are only 5 children exceeding $8\frac{1}{2}$ lb. in weight. The same may be observed with regard to the size of their heads. Only 6 measured above $14\frac{1}{2}$ inches in circumference, and these all of the male sex; 5 measured $14\frac{3}{4}$, and one 15. In transverse dimensions only 4 exceeded $7\frac{3}{4}$, the largest of which was $8\frac{1}{2}$; whereas deviations under the standard in these particulars were very numerous, never however under 12 around and $6\frac{1}{4}$ across.

In the year 1753, Dr. Roederer published a paper, De Pondere et Longitudine Infantum recens naturum, in the Commentaries of the R. S. of Gottingen, of which the celebrated Haller was the principal institutor, and long the president. In this paper he proves, in the clearest manner, by incontestible experiments, the absurdity of the ideas of obstetric writers with regard to the progress of the ovum during gestation, and the weight of the fœtus after birth. He shows, though they state the weight of the fœtus, come to the full time, to be from 12 to 14 or 16 lb., that it is more generally 6 or 7, and very rarely exceeds 8. This deserves particular notice for 2 reasons; 1st, because it serves to show how little dependence is to be placed on the assertions of authors who copy each other servilely, without having recourse to experiment even in the most obvious cases; and, 2dly, because this paper has been overlooked by some of the most celebrated writers and teachers of midwifry now living. What idea are we to form of the accuracy of one of our latest systematic writers, who (telling us that he has been a practitioner of midwifry, in a capital city, for 20 years, and a teacher for more than 12) states, in one page of his work, that the weight of a fœtus at 8 months is about 7 lb.; and on the opposite page, that at full time it weighs from 12 to 14 lb.?*

Of 27 children, carried to the full period of gestation, weighed and measured in length by Roederer, without any attention to the difference of sex; Dr. C.

* See a Treatise of Midwifry, p. 88 and 89, divested of technical terms and abstruse theories, by A. Hamilton, M. D. 8° edit. London, 1781.—Orig.

found that 18 were of the male and 9 of the female sex; and that the average weight of the former was about 6 lb. 9 oz., that of the latter about 6 lb. 2 oz. 2 dr. Whether he, says Dr. Clarke, used the same weights, I cannot exactly say. He observes, that he used the civil pound of Gottingen, which I can easily perceive consisted of 16 oz. as mine did; but whether a German oz. be the same with ours, I have not data to determine. The average length of the males measured by him was about $20\frac{1}{3}$ inches, and of the females about $19\frac{17}{18}$. He weighed also the placentæ of 21 lying-in women, 16 of whom had borne male children, and 5 female. The average weight of the former was 1 lb. $2\frac{1}{2}$ oz.; that of the latter 1 lb. 2 oz. Hence it appears, that in other circumstances, besides those I have taken notice of, the male and female sex differ. So far I thought it necessary to take extracts from Dr. Roederer's paper, as his observations and mine throw light on each other, and add confirmation to both.

There is one circumstance or two so intimately connected with his former letter, that Dr. C. cannot pass them over in silence. Having found that males suffer more in the birth than females, he was desirous of knowing whether the chance of the mother's recovery was thereby in any degree affected; and to determine this he was once more at the pains of turning over the registry with care. He found, that of 214 women, dead of single children, 50 were delivered of still-born males, and 15 of still-born females; 76 of living males, and 73 of living females. Of the 15 dead of twins, 6 had twins one of each sex; 6 others had twins both of the male sex; and 3 had twins both of the female sex. All of which twins (2 or 3 excepted) it is very remarkable, survived the death of their mothers. It would appear then, that the life of the mother is principally endangered in those cases where the bulk of the male's head precludes the possibility of his being brought into the world alive, either by the efforts of nature or art. The conception of twins we have observed to be more fatal to the mother than that of single children. The average weight of 12 twins, which had occurred to him of late, he found to be 11 lb. a pair. The largest pair weighed 13 lb. and the least $8\frac{1}{2}$. From some rude attempts made to ascertain the weight of the contents of the gravid uterus in cases of twin and single children, he was inclined to think, that they are to each other as about 15 to 10, or perhaps $14\frac{1}{2}$ to $9\frac{1}{4}$.

An Abstract of the Registry kept at the Lying-in Hospital, Dublin, from Dec. 8, 1757, *to Dec.* 31, 1784.

Anno.	Number of Patients admitted.	Wentout not delivered.	Delivered in the Hospital.	Boys born.	Girls born.	Total numb. of children.	Women having twins.	Children dead.	Children still-born	Women dead.
1757	55	-	55	30	25	55	—	6	3	1
1758	455	1	454	255	207	462	8	54	21	8
1759	413	7	406	228	192	420	13	95	22	5
							1 had 3			
1760	571	15	556	300	260	560	4	116	36	4
1761	537	16	521	283	249	532	11	104	29	9
1762	550	17	533	279	266	545	12	106	33	6
1763	519	31	488	274	224	498	12	94	29	9
1764	610	22	588	287	308	595	7	83	28	12
1765	559	26	533	288	251	539	6	94	25	6
1766	611	30	581	324	261	585	4	111	18	3
1767	695	31	664	373	301	674	10	125	29	11
1768	689	34	655	362	302	664	9	154	47	16
1769	675	33	642	350	301	651	9	152	38	8
1770	705	35	670	372	305	677	7	107	37	8
1771	724	29	695	370	341	711	16	102	44	5
1772	725	21	704	368	344	712	8	116	32	4
1773	727	33	694	367	344	711	17	136	31	13
1774	709	28	681	357	334	691	10	154	29	21
1775	752	24	728	364	378	742	14	122	27	5
1776	883	31	802	418	407	825	22	132	39	7
							1 had 3			
1777	872	37	835	452	395	847	12	145	35	7
1778	961	34	927	476	460	936	9	127	39	10
1779	1064	53	1011	550	476	1026	15	146	59	8
1780	967	48	919	499	441	1940	21	115	41	5
1781	1079	52	1027	598	447	1045	18	121	38	6
1782	1021	31	990	549	458	1007	17	127	57	6
1783	1230	63	1167	632	553	1185	17	91	72	15
							1 had 3			
1784	1317	57	1260	642	640	1282	23	76	68	11
Totals	20625	839	19786	10647	9470	20117	331	3111	1006	229

Proportion of males and females born, about 9 males to 8 females.
children dying under 16 days old, as 1 to about $6\frac{1}{2}$.
children still-born, as 1 to about 20.
women having twins, as 1 to about 60.
women dying in child-bed, as 1 to about 87.

Extracts from the Registry kept at the Lying-in Hospital, Dublin, from 1757 to 1784.

Uniparous.

Women. Delivered.	Women. Dead.	Sex.	Children. Dead.	Still-born.
19455	214	M. 10305	M. 1656	M. 602
		F. 9150	F. 1247	F. 351
		Total 19455	2903	Total 953
			953	
			Total 3856 dead and still-born.	

Multiparous, Twins, Triplets, &c.

Women. Delivered.	Women. Dead.	Sex.	Children. Dead.	Still-born.
331	15	M. 342	M. 116	M. 29
		F. 320	F. 91	F. 20
		Total 662	207	Total 49
			49	
			Total 256 dead and still-born.	

Inferences.

1. Proportion of males to females born nearly as........ 17 to 15
2. ——— children dying under 16 days .... 1 .. $6\frac{2}{3}$
3. ——— children still-born.................. 1 .. $20\frac{2}{5}$
4. ——— males dying to females .... 4 .. 3
5. ——— —— still-born to ditto .... 12 .. 7
6. ——— still-born and dead of each sex to the whole .... 1 .. 5
7. ——— women dying in child-bed .... 1 .. 92

Inferences.

8. Proportion of male twins to females born .... 17 to 16
9. ——— twins dying under 16 days .... 1 .. $3\frac{1}{5}$
10. ——— twins still-born .... 1 .. $13\frac{1}{2}$
11. ——— male twins dying to females .... 5 .. 4
12. ——— ——— still-born to ditto .... 3 .. 2
13. ——— still-born and dead of each sex to the whole........................ 1 .. $2\frac{3}{5}$
14. ——— women dying........................ 1 .. 22

Totals of dead and still-born.

	Males.	Females.
	1656	1247
	602	351
	2258	1598
Born in hospital	10305	9150
Dead and still-born	2258	1598
Sent out living	8047	7552
	7552	
Balance in favour of	495 the male sex.	

Totals of dead and still-born whether uniparous or multiparous.

	Males.	Females.
	1656	1247
	116	91
	602	351
	29	20
	2403	1709
Born	10647	9470
	2403	1709
	8244	7761
	7761	

Totals of twins, &c. dead and still-born.

	Males.	Females.
	116	91
	29	20
	145	111
Born	342	320
Dead and still-born	145	111
Sent out living	197	209
		197
Balance in favour of the female sex		12

Of 23117 children born, at the end of a fortnight, there is only a balance of................ 483 in favour of the male sex, though originally 1177; greater loss of males 694.

XVIII. Some Particulars of the present State of Mount Vesuvius; with the Account of a Journey into the Province of Abruzzo, and a Voyage to the Island of Ponza. By Sir William Hamilton, K. B., F. R. S., and A. S. Dated, Naples, January 24, 1786.

The eruption of Mount Vesuvius, which began in November, 1784, continued in some degree till about the 20th of last month. The lava either overflowed the rim of the crater, or issued from small fissures on its borders, on that side which faces the mountain of Somma, and ran more or less in 1, and at times in 3 or 4 channels, regularly formed, down the flanks of the conical part of the volcano; sometimes descending and spreading itself in the valley between the two mountains; and once, when the eruption was in its greatest force, in the month of November last, the lava descended still lower, and did some damage to the vineyards, and cultivated parts at the foot of Vesuvius, towards the village of St. Sebastiano; but generally the lava not being abundant, stopped and cooled before it was able to reach the valley. By the accumulation of these lavas on the flanks of Vesuvius, its form has been greatly altered; and by the frequent explosion of scoriæ and ashes, a considerable mountain has been formed within the crater, which now rising much above its rim has also given that part of the mountain a new appearance.

Sir W. having never had an opportunity of examining the islands of Ponza, Palmarole, Zannone, and other small islands, or rather rocks, situated between the island of Ventotiene and Monte Circello, near Terracina, on the Continent; and thinking that by a tour of these islands, he should be enabled to render his former observations more complete, he determined to take a favourable opportunity to visit these islands. But before putting this plan in execution, he made a long excursion in the province of Abruzzo, as far as the Lake of Celano, anciently called Fucinus, and where the famous Emissary of the Emperor Claudius, a most stupendous work for draining that lake, remains nearly entire, though filled up with rubbish and earth in many parts, and of course useless. The water of this lake, which is more than 30 miles in circumference, increases daily, and is destroying the rich and cultivated plains on its borders. It is surrounded by very high mountains, many of them covered with snow, and at the foot of them are many villages, and rich and well cultivated farms. He went with torches into the Emissary of Claudius as far as he could. It is a covered under-ground canal, 3 miles long, and great part of it cut through a hard rock; the other parts supported by masonry, with wells sunk to give air and light. According to Suetonius, Claudius employed 30 thousand men eleven years on this great work, intended to convey the superfluous water of the lake into the bed of the river Liris, now called Garigliano; and no doubt, if it was cleared and repaired, it would again answer that purpose. In its present state it is a most magnificent monument of antiquity.

The whole country from Arpino, the native place of Marius,* by Isola, Sora, Civitella, and Capistrello, to the Lake of Celano, Sir W. thinks infinitely more beautiful and picturesque than any spot he had seen on the Alps, in Savoy, Switzerland, or the Tyrol. The road is not passable for carriages, and indeed is scarcely so, even in summer, for horses or mules, and is often infested with banditti; a party of which, consisting of 22, had quartered themselves in a village which he passed through, and left it but a week before his arrival. There are many wolves and some bears in the adjacent mountains, which also commit their depredations in the winter. The tyger-cat, gatto pardo, or lynx, is sometimes found in the woods of this part of Abruzzo. The road follows the windings of the Garigliano, which is here a beautiful clear trout stream, with a great variety of cascades and water-falls, particularly a double one at Isola, near which place Cicero had a villa, and there are still some remains of it, though converted to a chapel. The valley is extensive, and rich with fruit trees, corn, vines, and olives. Large tracts of land are here and there covered with woods of oak and chestnut, all timber trees of the largest size. The mountains nearest the valley rise gently, and are adorned with either modern castles, towns, and villages, or the ruins of ancient ones. The next range of mountains, rising behind these, are covered with pines, larches, and such trees and shrubs as usually abound in a like situation: and above them a third range of mountains and rocks, being the most elevated part of the Appennine, rise much higher, and, being covered with eternal snow, make a beautiful contrast with the rich valley above-mentioned; and the snow is at so great a distance, as not to give that uncomfortable chill to the air, which is always found in the narrow vallies of the Alps and the Tyrol.

On the 15th of August last, Sir W. went in a felucca to the island of Ischia. He had nothing to add to his former observations on this island, already communicated to the R. S.; except that about 60 yards from the shore, at a place called St. Angelo, situated between the towns of Ischia and Furia, a column of boiling water bubbles on the surface of the sea with great force, and communicates its heat to the water of the sea near it; but as the wind was very high, and the surf considerable, he was not able then to examine this curious spot as he could have wished. The inhabitants of the neighbourhood told him, that

* Marius had a large villa, about twelve miles distant from Arpino. I went to visit the spot, on which now stands the only convent of the austere order of La Trappe in Italy. It is in the Pope's state, and has been evidently built of the ruins of Marius's house, and its present name is Casa Mari.—Orig.

it always boiled up in the same manner, winter and summer; and that it was of great use to them in bending their planks for ship-building; and that the fishermen also frequently made use of this natural cauldron to boil their fish. In his description of the island of Ischia mention is made of several spots where, near the shore, he had found, when bathing in the sea, the sand under his feet so hot as to oblige him to retire hastily. Sir W. visited most of the small islands in the group near the city of Naples: as Ischia, Ventotiene, Stefano, Ponza, Palmaroll, Zannone, &c. in which the chief curiosities are the stupendous rocks and other volcanic remains, especially the perfect basaltes which are found in several of them, and which he ascribes to the cooling of lava. To confirm this idea he says, when I was last in England, I inquired of many of the manufacturers of glass, whether it had ever happened, that the glass cooling in their furnaces had taken any distinct forms like prisms or crystallizations; but I got no satisfactory answer till I applied to the ingenious Mr. Parker, of Fleet-street, who not only informed me, that some years ago, a quantity of his flint glass had been rendered unserviceable by taking such a form in cooling; but also gave me several curious specimens of the glass itself: some of them are in laminæ, which may be easily separated; and others resemble basaltic columns in miniature, having regular faces. I was much pleased with this discovery, proving beyond a doubt the volcanic origin of most basaltes. Many of the rocks of lava of the island of Ponza are, with respect to their configurations, strikingly like the specimens of Mr. Parker's above-mentioned glass, none being very regularly formed basaltes, but all having a tendency towards it. Mr. Parker could not account for the accident that occasioned his glass to take the basaltic forms; but I have remarked, both in Sicily and at Naples, that such lavas as have run into the sea, are either formed into regular basaltes, or have a great tendency towards such a form. The lavas of Mount Etna, which ran into the sea near Iacci, are perfect basaltes; and a lava that ran into the sea from Mount Vesuvius, near Torre del Greco, in 1631, has an evident tendency to the basaltic forms.

It is probable, that all these islands and rocks may in time be levelled by the action of the sea. Ponza, in its present state, is the mere skeleton of a volcanic island, as little more than its harder vitrified parts remain, and they seem to be slowly and gradually mouldering away. Other new volcanic islands may likewise be produced in these parts. The gulphs of Gaeta and Terracina may, in the course of time, become another Campo Felice; for, as has been mentioned in one of my former communications on this subject, the rich and fertile plain so called, which extends from the bay of Naples to the Appennines, behind Caserta and Capua, has evidently been entirely formed by a succession of such volcanic eruptions. Vesuvius, the Solfaterra, and the high volcanic ground, on which great part of this city is built, were once probably islands; and we may conceive the islands of Procita, Ischia, Ventotiene, Palmarole, Ponza, and Zannone, to be the outline of a new portion of land, intended by nature to be added to the neighbouring continent; and the Lipari islands, all of which are volcanic, may be considered in the same light with respect to a future intended addition of territory to the island of Sicily. The more opportunities I have of examining this volcanic country, the more I am convinced of the truth of what I have already ventured to advance, which is, that volcanos should be considered in a creative rather than a destructive light. Many new discoveries have been made of late years, particularly in the South-Seas, of islands which owe their birth to volcanic explosions; and some indeed where the volcanic fire still operates. I am led to believe, that on further examination, most of the elevated islands at a considerable distance from the continent would be found to have a volcanic origin; as the low and flat islands appear in general to have been formed of the spoils of sea productions, such as corals, madrepores, &c.

Postscript.—The earth is not yet so perfectly quiet in Calabria and at Messina, as to encourage the inhabitants to begin to re-build their houses, and they continue to live in wooden barracks. There has however been no earthquake of consequence during these last 3 months. My conjecture, that the volcanic matter, which was supposed to have occasioned the late earthquakes, had vented itself at the bottom of the sea between Calabria and Sicily, seems to have been verified; for the pilot of one of his Sicilian Majesty's sciabecques, having some time after the earthquakes cast anchor off the point of Palizzi, where he had often anchored in 25 fathom water, found no bottom till he came to 65 fathom, and having sounded for 2 miles out at sea towards the point of Spartivento in Calabria, he still found the same considerable alteration in the depth of the sea. The inhabitants of Palizzi also declare, that during the great earthquake of the 5th of February, 1783, the sea had frothed and boiled up tremendously off their point.

XIX. Of a new Electrical Fish. By Lieutenant William Paterson.* p. 382.

While at the island of Johanna, one of the Comora islands, in his way to the East-Indies, Mr. P. met with an electrical fish, which has hitherto escaped the observation of naturalists, and seems in many respects to differ from the electrical fishes already described. The fish is 7 inches long, 2 inches and a half broad, has a long projecting mouth, and seems to be of the genus tetrodon. The back of the fish is a dark brown colour, the belly part of sea-green, the sides yellow, and the fins and tail of a sandy green. The body is interspersed

* This fish is the Tetrodon Electricus of the Gmelinian edition of the Systema Naturæ of Linnæus, viz. Tetrodon maculis rubris, viridibus et albis, supra fuscus, subtus thalassinus, ad latera flavus, pinnis viridibus.

with red, green, and white spots, the white ones particularly bright; the eyes large, the iris red, its outer edge tinged with yellow.

The island of Johanna is situated in latitude 12° 13′ south. The coast is wholly composed of coral rocks, which are in many places hollowed by the sea. In these cavities Mr. P. found several of the electrical fishes. The water is about 56° or 60° of heat of Fahrenheit's thermometer. He caught 2 of them in a linen bag, closed up at one end, and open at the other. In attempting to take one of them in his hand, it gave him so severe an electrical shock, that he was obliged to quit his hold. He however secured them both in the linen bag, and carried them to the camp, which was about 2 miles distant. On his arrival there, one of them was found to be dead, and the other in a very weak state, which made him anxious to prove, by the evidence of others, that it possessed the powers of electricity, while it was yet alive. He had it put into a tub of water, and desired the surgeon of the regiment to lay hold of it between his hands; on doing which he received an evident electrical stroke. Afterwards the adjutant touched it with his finger on the back, and felt a very slight shock, but sufficiently strong to ascertain the fact.

XX. Observation of the Transit of Mercury over the Sun's Disc, made at Louvain, in the Netherlands, May 3, 1786. *By Nathaniel Pigot, Esq., F. R. S.* p. 384.

About 6 o'clock, when Mr. P. attended for the observation, there being a great number of solar spots, Mercury might easily have been mistaken for one; but his motion soon removed every doubt in that respect. Flying clouds obscured the sun at intervals; but during the last half hour, the weather was fine, the sky clear, the limb of the sun well defined; Mercury round and very black. There seems to have been some mistake, in respect of this phenomenon, either in the calculation or the printing of the Connoissance des Temps of this year: the emersion of the centre of Mercury is there set down at $19^h\ 45^m$ apparent time at Paris; whereas, by his observation, the egress of the centre at Louvain was at $20^h\ 47^m\ 28^s$ or 29^s apparent time. Taking here no other equation into consideration, besides the difference of meridians between Paris and Louvain, which by a great number of observations, he determined in 1775 to be $9^m\ 37^s$ in time, the emersion of the centre at Paris must have been at $20^h\ 37^m\ 51^s$ or 52^s, which differs nearly 53^m from the computed time.

By observation, the internal contact at the egress $20^h\ 45^m\ 41^s$, and the external contact at $20^h\ 49^m\ 16^s$.

XXI. Observation of the late Transit of Mercury over the Sun, observed by Edward Pigott, Esq., at Louvain, in the Netherlands. p. 389.

We have been fortunate here in seeing Mercury's egress. I observed it thus:

Mercury's limb in contact with the sun's limb........	$20^h\ 45^m\ 37^s$
Mercury bisected by the sun's limb................	20 47 17
Mercury quite out, or last contact	20 49 22

XXII. Additional Observations on making a Thermometer for Measuring the Higher Degrees of Heat. By Mr. Josiah Wedgwood, F. R. S., and Potter to her Majesty. p. 390.

In my first paper I communicated every thing that experience had then taught me, respecting both the construction and use of this thermometer; but more extensive practice has since convinced me, that other managements and precautions are necessary, in order to bring it to the perfection it is capable of receiving: for pieces made of the same clay, and exactly of the same dimensions, have been found to differ in the degree of their diminution by fire, in consequence of some circumstances in the mode of their formation, at that time unheeded, and very difficult to be developed.

Of the 2 ways proposed for forming them, the mould and the press, the former was made choice of, as being, for general use, the most commodious. The soft clay was pressed into a square mould with the fingers; and the pieces, when dry, were pared down on two opposite sides, by means of a paring gage made for that purpose, so as to pass exactly to 0° at the entrance of the converging canal of the measuring gage. But the pieces thus formed have been found liable, in passing through strong fire, to receive a little alteration in their figure, which produces an uncertainty with respect to their subsequent measurement: the two sides, instead of continuing flat, become concave; the edges, both at top and bottom, projecting beyond the middle part, sometimes very considerably, as at a and b, fig. 7, pl. 1, where AB represents a perpendicular section of an unburnt piece, and ab a like section of the same piece after it has undergone a heat of 160 degrees. This irregularity in the form, which is sensible only after passing through the high degrees of fire, was observed in some of the early experiments, but was not then considered as productive of any error.

On more attentively examining this matter, it appeared, that when the clay is pressed into a mould, the surface in contact with the mould acquires a more compact texture than the inner part of the mass; that this compactness restrains, in some degree, its diminution in the fire; and therefore, that when this surface, or less diminishable crust, is pared off from the two sides only, the piece may be considered as having its upper and lower strata, AA and BB, composed of a less diminishable matter than the intermediate part, the necessary consequence of which structure will be such a figure as we find the pieces to assume; for if any stratum in the mass shrinks less than the rest, the extremities of that stratum must be left proportionably prominent. That this was the true cause of the inequality, I was convinced by firing some pieces unadjusted, with all their surfaces entire, as they came from the mould; for these pieces, after passing through the same strong fires with the preceding, continued flat, with the angles regularly sharp, and without the least sensible prominence in any part.

Some of the moulds, employed for this use, were made of plaster, a material more convenient for the workman than metal, as the pieces part more freely from it, but which contributed greatly to increase the above-mentioned irregularity: for the plaster, by absorbing a portion of the water from the clay contiguous to it, renders the surface at the same time, even at the instant of contact, much more consistent, and consequently more difficult to press into the angles of the mould; so that the outsides of these pieces were not only more compressed, but formed of clay of a different temper from the inner parts, being much drier or firmer, a circumstance which restrains still more their diminution in the fire.

The moulds were therefore laid aside, and the press adopted in their stead; for as the soft clay, pressed in a cylindrical vessel, gives way and escapes through an aperture made for that purpose (by which means it is formed into long rods), the sides of the piece cannot be supposed to receive so great a degree of compressure against the sides of the aperture through which it is delivered in this operation, as it does against the sides of the mould, by which it is confined till every part has born a pressure sufficient to force the clay into every angle, which is much greater than even a workman would imagine till he comes to try the experiment himself. But with this change some new difficulties arose; for pieces pressed through the same aperture, and from the same press-ful of clay, and adjusted, when dry, to the same point in the gage, were found, after passing together through the same strong fires, to differ in their dimensions from each other, in some instances more than any of the preceding.

Having hitherto paid no particular attention myself to the mere manual labour of pressing the clay, I determined on this event, to go through that and every other operation, however simple and seemingly insignificant, with my own hands. In doing this I observed, that the power necessary for forcing the clay through an aperture which bore but a small proportion to the diameter of the mass of clay in the press, was so great as to squeeze out, along with the clay that first passed through, a considerable portion of the water that belonged to the rest. From this over proportion of water in the composition of the first pieces they were soft and spongy, and the succeeding ones more and more compact, till at length the clay proved so stiff as scarcely to be forced through at all.

Clay, containing different proportions of water, is well known to diminish differently in drying; but it was not imagined that, when dry, there would be any difference in its subsequent diminutions by fire. Experiments however, multiplied in a variety of circumstances, showed decisively, what the pieces formed in the mould had given grounds to suspect, that those formed of the softest clay, and which had undergone the least pressure, diminished most in burning; and that the diminution is uniformly less and less, in proportion to the greater degree of pressure or compactness. The knowledge of the cause of the irregularity suggested a remedy. I lessened the width of the press very much, so as to bring the diameter of the mass of clay, and that of the aperture through which it is delivered, to a nearer proportion with each other. A much less degree of force being now sufficient, the pieces, or rods, were proportionably more uniform, though there was still a sensible difference, in consistence, between those which were first and last pressed out from the same mass of clay. The intermediate ones, within a certain distance from the two extremes, corresponded very nearly with each other; so that by rejecting a sufficient number of the first and last, and using the intermediate ones only, the inequality may be considered as almost annihilated.

Yet I still found that, in strong fire, the edges became a little prominent, though not so much as before. I was aware that these pieces must partake, in some degree, of the imperfection of those made in the mould; having their surfaces rendered, by their friction against the sides of the aperture, more compact than the inner part. But I suspected that something might depend also on the form, and accordingly made many trials for ascertaining the form that might be least liable to this irregularity: the angles only were bevilled off, the sides were rounded, the pieces were rounded all over, made of ovals and other curves, and both the longest and shortest dimensions were used as the extent to be measured: the general result was, that the nearer they came to a circular figure, the less inequality they contracted in the fire, and by making them entirely circular, the imperfection appeared to be obviated altogether; cylindric pieces bearing the strongest fires without the least appearance of prominence or inequality in any part of their surface. I have therefore chosen this last form, leaving only one narrow flat side (ab, fig. 8) as a bottom for the pieces to rest on, and to distinguish the position in which they are to be measured in the gage.

I have endeavoured at the same time to obviate whatever inaccuracy the inequality of compactness may be capable of producing, by so adjusting the aperture through which the rods are pressed, and on which their figure and dimensions depend, as to supersede the use of the paring gage altogether; that the whole surface may remain of the same uniform compactness which it received in the press. And as it is scarcely practicable, in any mode of forming soft clay, to have all the pieces precisely of the same dimensions after drying, I do not reject those which come within 2 or 3 degrees of the standard, but, instead of injuring the surface by paring or rubbing, I mark on the ends the degrees which they respectively exceed or fall short; which degrees are accordingly to be subtracted, or added, in all observations of heat made with those pieces. It may be proper to take notice of an irregularity in the apparent diminutions of the pieces, which was sometimes observed to happen from another cause, their bending a little in strong fire, so as to be prevented from going so far in the gage as they would have done if they had continued perfectly straight. But as this takes place only in pieces of considerable length, and as they derive no advantage of any kind from that length, the remedy is too obvious to need being here mentioned.

Another fallacious appearance arose, not from any imperfection in the pieces themselves, but from a deception with respect to the heat in which the comparison of them had been made. In one period of the course of my experiments I employed, for firing them, a small, shallow, cylindrical vessel, setting the pieces on end, close together, on its bottom, and placing it in the middle of the fuel, in a common air furnace, with care to keep the fire as equal all round it as possible. It was expected, that all the pieces would receive an equal heat; and as they were found, after the operation, to differ in their dimensions, sometimes considerably, from each other, these differences proved a source of much perplexity, till it was discovered that the pieces had really undergone unequal degrees of heat.

In the paper on the comparison of this thermometer with Fahrenheit's, I have taken notice of the great difficulty of obtaining, in small furnaces, a perfectly equal heat, even through the extent occupied by a few of these little pieces: and how different the heat may be in different parts of one vessel, we may be satisfied by an easy experiment, viz. setting a cylindrical rod of clay, of the length of 8 or 10 inches, upright in the middle of a crucible, and urging it with strong fire in a common small furnace; the rod will be found very differently diminished at different parts of its height; and if its height be sufficient to reach some way above the fuel, nearly the whole range of the thermometric scale may be produced in one rod; an ocular proof not only of the diversity of heat within a small compass, but likewise of the peculiar sensibility of this thermometer, every part of the mass expressing distinctly the degree of heat which it has itself felt. It will be proper, in this experiment, to have a tube fixed in the bottom of the crucible, for keeping the rod steady. By this means the heat of my air-furnace renders a rod of the thermometric clay tapering, from about 4 parts in diameter at top to 3 at bottom, which are nearly the proportions between the width of the piece when unburnt, or but just ignited, and when it has suffered a heat of 160 degrees. By due attention to the circumstances above stated, any single quantity of clay may be made up into thermometer-pieces, that shall differ very little, if any thing at all, from each other.

But a new difficulty now arose, more embarrassing than any of the former; that of procuring fresh supplies of clay, of the same thermometric quality with the first. The quantity of the clay which, after trial of many others, I had made choice of, was small; but the particular spot it was taken from being known, and having purchased the little estate in which it was raised, I had not a doubt of obtaining more of the same when it should be wanted: for clays in general, when raised from an equal depth, in the same stratum, and near the same place, are found to possess the same properties, with respect to ductility in the hands of the workman, a disposition to assume by fire a porcelain or vitreous texture, singly, or in composition, and all other qualities relative to their use in pottery. In this, however, I was deceived; for when a fresh supply was wanted, to complete my experiments, though I had some taken from a pit joining to the first, and at the same depth, it was found to diminish differently from the former parcel. I then had some raised from different parts of the same field and bed, and at different depths; and in various other places in Cornwall, from the spot where this species of clay is first met with to the Land's End; but all these clays differed so much from the first in the quantity of their diminution by fire, and most of them also from each other, that I despaired of being ever able to find one that would correspond with it, or any natural clays that could be obtained, twice of exactly the same thermometric properties, how similar soever in other respects.

On a review of the numerous comparisons made of these new clays, in different degrees of heat, from the commencement of redness up to intense fire, the most striking differences of the greatest part of them from the old seemed to originate in the lower stages of heat; and of those which were got from the neighbourhood of the old, the variations from it in the higher stages seemed, for the most part, to be only consequences of those differences in the lower ones. I have mentioned, in the first paper, that the original thermometer pieces had their bulk enlarged a little on the approach of ignition; but that by the time they became visibly red-hot throughout, they are reduced to their former dimensions again; and at this moment the thermometric diminution begins. The new clays had their bulk enlarged in a much greater proportion, and the enlargement was of much longer continuance: some of them required a heat of 15 degrees to destroy the increase which ignition had produced in their bulk, and bring them back to their original dimensions: after this period, most of them diminished pretty regularly, and uniformly with the old, being nearly so many degrees behind it, in all the succeeding stages of heat, as they required to bring them back from the enlarged state.

I have mentioned also, in my former paper, that a quantity of air is extricated from the clay, most rapidly at the period in which the augmentation of bulk takes place; and that the augmentation was probably owing to this air forcing the particles of the clay a little asunder, previous to the instant of its escape. It was therefore presumed, that the greater extension of these new clays might be owing, either to a greater quantity, or stronger adhesion, of this combined air: and as clay, kept moist for a length of time, in certain circumstances, undergoes a process seemingly analogous to fermentation, it was hoped that, by such a process, part of its combined air might be detached. But experiments made on this idea have proved, that these clays, kept moist for a twelvemonth,—kept for a considerable length of time in a heat just below visible redness,—boiled in water for many hours,—alternately, and repeatedly, moistened and dried,—suffer no alteration in their thermometric properties, and continue to differ from the standard clay just as much as they did at first.

Some of these new clays differed from the old in a property still more essential, and by which I was much more disconcerted; for though they continued diminishing with tolerable regularity, keeping only some degrees behind it, up to a certain period of heat, about that in which cast iron melts; yet many of the pieces, urged with a heat known to be greater than that, were found not to be diminished so much as those which had suffered only that lower heat. Further experiments showed, that after diminishing to a certain point, they begin, on an increase of the heat beyond that point, to swell again: and as this effect is constant in certain clays, and begins earliest in those which are most vitrescible, and as clays are found to swell on the approach of vitrification, I consider this second enlargement of bulk, however inconsiderable, as a sure indication of the clay or composition having gone beyond the true porcelain state, and of a disposition taking place towards vitrification; which stage is always, so far as my experience reaches, attended with a new extrication of air; and in some instances, this air being unable to make its escape from the tenacious mass that envelopes it, the burnt clay is thereby so much increased in bulk as to swim on water like very light wood. The degree of heat therefore, at which this enlargement begins, may be considered as a criterion of the degree of vitrescibility of the composition; which points out a new use of this thermometer, enabling us to ascertain the degree of vitrescibility of bodies that cannot actually be vitrified by any fires which our furnaces are capable of producing.

All my researches among the natural clays proving fruitless, and the experiments having shown that all those, which could sufficiently resist vitrification, diminished too little in the fire, I endeavoured to find a body possessed of the opposite property, that is, diminishing too much, and, by a mixture of these two, to produce the medium diminution required. As I could not find any natural substance possessed of that property, which would not at the same time render the compound too vitrescible, I was obliged to have recourse to some artificial preparation; and as the earth of alum is the pure argillaceous earth, to which all clays owe their property of diminution in the fire, possessing that property in a greater or less degree according to the quantity of alum earth in their composition, I mixed some of this earth with the clay, and found it to answer my wishes completely, both in procuring the necessary degree of diminution, and increasing its unvitrescibility. So little is this compound disposed to vitrification, that the greatest heat I could give it, that of 160°, did not even bring it to a porcelain texture, but left it still bibulous; and as it does not arrive at the porcelain state in this fire, there can be no danger of its approaching too near to the vitrescent in any heat that we can produce in a furnace.

In order to obtain the exact medium required, I took one of the best of the clays I had procured from Cornwall, and mixed it with different proportions of the alum earth, till the composition was found, on repeated trials, to agree with the original in all degrees of heat. This coincidence was not indeed essential; but as many degrees of heat were already before the public, measured by thermometer pieces made of the first clay, and as the correspondence of the first with Fahrenheit's scale had likewise been in some measure ascertained, it was desirable that the same degrees of heat should continue to be expressed by the same numbers.

The alum earth is prepared for this purpose by dissolving the alum in water, precipitating with a solution of fixed alkali, and washing the earth repeatedly with large quantities of boiling water: when the earth has settled, the water above it is let off by cocks in the sides of the hogsheads; and when the vessels are filled up with fresh water, care is taken to stir up the earth from the bottom, and mix it thoroughly with the liquor. I find it most convenient to use the earth undried, in its gelatinous state, as in this state it unites easily and perfectly with the clay; whereas, when the alum earth has concreted into dry masses, great labour is necessary to mix them uniformly together.

I have tried several different parcels of English alum, from the same and from different manufactories, and found no material difference in the quantity of earth it contains. Nor indeed would it be of any consequence if there was a difference in this respect, as the proportion of alum earth necessary for different clays, and even for different parcels of the same clay, can only be ascertained by repeated trials, adding successive quantities of the earth till the desired effect is found to be produced. Ten hundred weight of the Cornwall porcelain clay, which I have now in use, required all the earth that was afforded by five hundred weight of alum.

It is material in this place to observe, that the earth of alum is extremely tenacious of water, insomuch that, though apparently dry, the water and air amount to near as much as the pure earth, and are not to be completely driven out without a full red heat. When divided by the admixture of other earthy bodies, it parts with its water easier indeed than before; but a mixture containing so much of it as the thermometric composition does, is far more retentive of water than common clay, and requires to be kept for some time in a heat equal to that of boiling water, before it is to be considered as dry, that is, before the adjustment of the pieces in the gage. If they are adjusted when only apparently dry, or of such a degree of dryness as they can be brought to by a heat that the hand can bear, the heat of boiling water will diminish them 2 or 3 degrees; and the greatest part of what they have thus been deprived of, they gradually recover again on being exposed to the atmosphere, so that the adjustment must be made immediately after the boiling heat.

By the same expedient to which I have thus been obliged to have recourse for procuring to the porcelain clay of Cornwall the standard degree of diminution, and resistance to fire, the same qualities may probably be communicated to any other clay that is tolerably pure from calcareous earth and iron; so that the thermometer clay is no longer to be considered as the produce of any particular spot (which was the principal inconvenience originally imagined to attend it,) but may be procured and prepared in all parts of the world where good common clay, and alum, are to be found; and corresponding thermometers may consequently be constructed, without any standard to copy from. For, if a converging canal be formed, of any convenient length, with the widths at the two ends in the proportion of 5 to 3, with the sides perfectly straight, and divided into 240 equal parts, numbering the divisions from the wider end;*—and if a clay be obtained of such quality, that when formed, in the manner already mentioned, into pieces of such size as to enter to 0 in the gage or canal, these pieces shall just begin to diminish, or go a little further in the canal, by a heat visibly red;—go to 27, by the heat in which copper melts;—about 90 by the welding heat of iron; about 160, by the greatest heat that can be produced with coked pit-coal in a well-constructed common air-furnace, about 8 inches square, still continuing bibulous, so as to stick to the tongue: such gages, and pieces of such clay, so adjusted, will always compose correspondent thermometers.

Having mentioned occasionally several alternate periods of dilatation and contraction in clay, it may be proper to state, and bring into one view, the whole succession of changes which I have observed in this curious material; as otherwise they might create some confusion in the minds of those who have not had

* Or the divisions on the side may be continued to 300; and in that case, instead of the widths of the two ends being in proportion of the odd numbers 5 and 3, the one will be just double to the other.—Orig.

occasion to think attentively on this subject, and lead them to ask how a body so variable, and liable to such opposite changes from different degrees of heat, can yet be a just measure of those degrees. The changes which take place in all the natural clays that have come under my examination are 6.

1. The first is, the shrinking of the moist clay in drying, from the mere loss of its water. The purer the clay is, the more water it requires to soften it, and the more it diminishes in bulk by the loss of that water. 2. The dry clay, gradually heated, preserves its bulk unvaried up to the approach of ignition. At this period it is enlarged a little; probably, as already observed, from its combined air endeavouring to escape. 3. When this air has made its escape, the clay begins to diminish, or to lose the bulk it had before acquired; and returns back, sooner or later, to the same dimensions which it was of when dry. It is at this point that the thermometric diminution commences. 4. From this point the clay continues to diminish more and more in proportion as the heat is increased. This I call the thermometric stage of diminution: it is of greater or less extent, terminating at different periods of heat, according to the nature of the clay: in the standard thermometer clay, it commences with visible ignition, and continues so doubtless far beyond the extreme heats of our furnaces, an interval consisting of 160 degrees of the scale: in others, it begins 4, 6, and in some even 15 of those degrees later, and terminates also much sooner: and in some its whole extent is not above 20 of the same degrees. Throughout the greatest part of this stage, the clays are found to retain their property of sticking to the tongue and imbibing water: between this bibulous state and the vitrescent there is an intermediate one, distinguished by the name of porcelain; and to the higher term of this porcelain state the stage of thermometric diminution seems to continue. 5. When the clay has passed the porcelain state, it begins to be enlarged again, a symptom of the vitrescent stage being commenced; and in this period it swells more or less, according to the nature of its composition. 6. By further heat the swelled mass, becoming fluid, subsides, is converted into glass or slag, and contracted into less volume than the clay occupied in any of its preceding states.

It is plain therefore, that clay can be a measure of heat no further than from ignition, or that point beyond ignition where the 3d stage terminates, to the beginning of the vitrescent stage; and that, as the first 3 changes are completely passed before the clay is applied to thermometric purposes, being strictly no other than preparatory processes, the thermometer pieces, whatever clay they may be made of, provided it is sufficiently unvitrescible, are to be considered as possessing only the 4th stage. But a singular property of the composition of clay and alum earth remains to be mentioned, viz. that it has really no other than this one stage: it suffers no enlargement of its bulk at ignition, or in any other period; but proceeds in one uninterrupted course of diminution, from the soft state in which the pieces are formed, up to the extreme fires of our furnaces. Though the diminution however is uninterrupted, it is at the same time so inconsiderable at the beginning, from the heat of boiling water, at which the pieces are adjusted, up to ignition, that the same point of visible redness is taken for the commencement of the scale, in this as in the original clay, without any sensible error or variation in their progress.

I am inclined to believe, though experiments have not yet enabled me to speak with certainty on this point, that the same cause which enlarges the natural clays on their first exposure to the fire, operates also in this composition, but in a much lower degree; that while the natural clays have their whole mass distended by the efforts of the air in forcing its passage, the composition is only restrained in its diminution, or prevented from diminishing so fast as it otherwise would do, and as it is found to do in the subsequent part of its course, after the air has escaped from it.

As the composition of clay and alum earth is far more tenacious of water than the clay itself, and was found, after being dried by the heat of boiling water, to yield, by distillation in a retort, above 3 times as much aqueous fluid as the original thermometric clay did; it seems probable, that a part of this water, retained to the approach of ignition, and in a state of chemical combination, may facilitate the passage of the air, serving as a vehicle to convey it off through interstices not permeable to air alone, and consequently enabling it to escape without doing that violence to the mass, which the natural clays sustain from the expulsion of their air after the water has been detached from it; for the experiments of Dr. Priestley have shown, that vessels even of burnt clay are permeable to air when they have imbibed water into their substance, though not at all so in a dry state.

XXIII. The Latitude and Longitude of York determined from a Variety of Astronomical Observations; together with a Recommendation of the Method of determining the Longitude of Places by Observations of the Moon's Transit over the Meridian. By Edward Pigott, Esq. p. 409.

The difference of meridians between Greenwich and York was found by the following methods: viz. 1st. by occultations of stars by the moon; 2dly, by observed meridian right-ascensions of the moon's limb; 3dly, by observations of Jupiter's 1st satellite; 4thly, by a lunar eclipse. By the 1st method, viz. occultations of stars, the difference of meridians between York and Greenwich was, by a medium of the observations, $4^m\ 27^s$; in like manner the medium by the right ascensions gave $4^m\ 24^s\frac{1}{4}$; that by Jupiter's satellites $4^m\ 31^s$; and that by the lunar eclipse $4^m\ 16^s$. The place of observation was in Botham, about 400 or 500 yards N. W. of York Minster.

Part of the Eclipse of the Moon, Sept. 10, 1783.

The two last Columns show the difference of Meridians between Greenwich and York. The observations marked with an asterisk were made by Mr. Goodricke.

Spots observed.	York, by Mr. Goodricke and me. App. time.	Paris, by M. Mechain. App. time.	Paris, by M. Messier. App. time.	Diff. of meridians by M. Mechain.	Diff. of merid. by M. Messier.
	h. ′ ″	h. ′ ″	h. ′ ″	′ ″	′ ″
Galileus bisected	9 45 32	9 58 33	....	3 38	....
Aristarchus covered....	9 49 13*	10 2 38	....	4 2	....
Copernicus touches .... {	9 57 3*	10 11 18	10 11 9	4 52	4 48
	9 57 20	10 11 18	10 11 9	4 35	4 31
Copernicus bisected .... {	9 58 33*	10 12 5	....	4 9	....
	9 58 55	10 12 5	....	3 47	....
Copernicus covered	9 59 9*	10 12 57	10 12 41	4 25	4 14
Plato touches	10 5 7	10 18 37	10 18 40	4 7	4 15
Plato covered	10 6 18	10 19 52	10 19 28	4 11	3 52
Manilius touches ...... {	10 11 26*	10 25 34	10 25 24	4 45	4 40
	10 11 33	10 25 34	10 25 24	4 38	4 33
Tycho touches........ {	10 11 57*	10 25 34	10 25 24	4 14	4 9
	10 11 57	10 25 34	10 25 24	4 14	4 9
Manilius covered	10 12 47*	10 26 29	10 26 53	4 19	4 48
Tycho covered.. ..... {	10 13 8*	10 27 19	10 27 8	4 48	4 42
	10 13 32	10 27 19	10 27 8	4 24	4 18
Menelaus bisected	10 15 41	10 29 19	....	4 15	....
Prom. Acut. Cen. covered	10 25 26*	10 39 5	....	4 16	....
Proclus bisected	10 29 00	10 42 28	....	4 5	....
Mare Crisium touches.. {	10 30 18*	10 43 56	10 44 00	4 15	4 24
	10 30 18	10 43 56	10 44 00	4 15	4 24
Mare Crisium bisected..	10 32 43*	10 46 34	10 46 10	4 28	4 9
Mare Crisium covered..	10 35 38*	10 49 11	....	4 10	....
Grimaldus emerges....	12 23 30	....	12 37 5	....	4 17
Grimaldus bisected	12 23 44	12 36 48	....	3 41	....
Grimaldus emerged.... {	12 23 55*	12 37 25	....	4 7	....
	12 23 59	12 37 25	....	4 3	....
Galileus emerges	12 25 50*	....	12 39 1	....	3 53
Galileus bisected	12 25 56*	12 39 16	....	3 57	....
Aristarchus bisected. ..	12 28 59	12 43 8	....	4 46	....

Difference of meridians on a mean 4′ 16″

M. Mechain's Observatory was 9′ 23″, and M. Messier's 9′ 18″ east of Greenwich.

Mr. P. thinks the meridian observations of the moon's limb the best method for determining the difference of meridians. The rule he adopted is this: The increase of the moon's R.A. in 12 hours, or any given time, found by computation, is to 12 hours, as the increase of the moon's R.A. between two places, found by observations, is to the difference of meridians. Instead of computing the moon's R.A. for 12 hours, he has constantly taken it from the Nautical Almanacs, which give it sufficiently exact, provided some attention be paid to the increase or decrease of the moon's motion.

Were the following circumstances attended to, the results, he doubts not, would be much more exact. 1st, Compare the observations to the same made in several other places. 2dly, Let several and the same stars be observed at these places. 3dly, Such stars as are nearest in R.A. and declination to the moon are most preferable. 4thly, To get as near as possible an equal number of observations of each limb, to take a mean of each set, and then a mean of both means. 5thly, To adjust the telescopes to the eye of the observer before the observation.

The latitude of York, by a medium of many observations, was, 53° 57′ 45″. And the mean declination of the magnetic needle was 23° 40′ west.

Sir H. Englefield, when at Scarborough, in Aug. and Sept. 1781, was so kind as to observe, at noon, the height of his barometer and thermometer. Mr. P. also made similar observations in the Observatory at York; from which, by 8 comparisons, none disagreeing above 0.018 of an inch from the mean, he found that the quicksilver at the sea stood 0.063 of an inch higher than at York. The barometers were made by Ramsden, and they agreed together to 0.005 part of an inch.

XXIV. Advertisement of the Expected Return of the Comet of 1532 *and* 1661 *in the Year* 1788. *By the Rev. Nevil Maskelyne, D.D., F.R.S. &c.* p. 426.

The comet of 1531, 1607, and 1682, having returned in the year 1759, according to Dr. Halley's prediction in his Synopsis Astronomiæ Cometicæ, first published in the Philos. Trans. in 1705, and re-published with his Astronomical Tables in 1749, there is no reason to doubt that all the other comets will return after their proper periods, according to the remark of the same author. In the first edition of the Synopsis he supposed the comets of 1532 and 1661, from the similarity of the elements of their orbits, to be one and the same; but in the 2d edition he has seemed to lessen the weight of his first conjecture by not repeating it. Probably he thought it best to establish this new point in astronomy, the doctrine of the revolution of comets in elliptic orbits, as all philosophical matters in the beginning should be, on the most certain grounds; and feared that the vague observations of the comet, made by Apian in 1532, might rather detract from, than add to, the evidence arising from more certain data. Astronomers however have generally acquiesced in his first conjecture of the comets of 1532 and 1661 being one and the same, and to expect its return to its perihelium accordingly in 1789. The interval between the passages of the comet by the perihelium in 1532 and 1661, is 128 years, 89 days, 1 hour, 29 minutes (32 of the years being bissextile), which added to the time of the perihelium in 1661, together with 11 days to reduce it from the Julian to the Gregorian stile, which we now use, brings out the expected time of the next perihelium to be April 27th, 1^h 10^m in the year 1789.

The periodic times of the comet, which appeared in 1531, 1607, and 1682, having been of 76 and 75 years alternately, Dr. Halley supposed, that the subsequent period would be of 76 years, and that it would return in the year 1758; but, on considering its near approach to Jupiter, in its descent towards the sun in the summer of 1681, he found, that the action of Jupiter on the comet was, for several months together, equal to a 50th part of the sun upon it, tending to increase the inclination of the orbit to the plane of the ecliptic, and lengthen the periodic time. Accordingly, the inclination of the orbit was found by the observations made in the following year 1682, to be 22′ greater than in the year 1607. The effect of the augmentation of the periodic time could not be seen till the next return, which he supposed would be protracted by Jupiter's action to the latter end of the year 1758, or the beginning of 1759. M. Clairaut, previous to its return, took the pains to calculate the actions both of Jupiter and Saturn on it during the whole periods from 1607 to 1682, and from 1682 to 1759, and thence predicted its return to its perihelium by the middle of April. It came about the middle of March, only a month sooner, which was a sufficient approximation to the truth in so delicate a matter, and did honour to this great mathematician, and his laborious calculations.

The comet in question is also, from the position of its orbit, liable to be much disturbed both by Jupiter and Saturn, particularly in its ascent from the sun after passing its perihelium, if they should happen to be near it, when it approaches to or crosses their orbits; because it is very near the plane of them at that time. When it passed the orbit of Jupiter in the beginning of February 1682, O. S. it was 50° in consequentia of that planet; and when it passed the orbit of Saturn in the beginning of October 1663, it was 17° in consequentia of it. Hence its motion would be accelerated while it was approaching towards the orbit of either planet by its separate action, and retarded when it had passed its orbit; but, as it would be subjected to the effect of retardation through a greater part of its orbit than to that of acceleration, the former would exceed the latter, and consequently the periodic time would be shortened; but probably not much, on account of the considerable distance of the comet from the planets when it passed by them; and therefore we may still expect it to return to its perihelium in the beginning of the year 1789, or the latter end of the year 1788, and certainly some time before the 27th of April 1789. But of this we shall be better informed after the end of this year, from the answers to the prize question proposed by the Royal Academy of Sciences at Paris, to compute the disturbances of the comet of 1532 and 1661, and thence to predict its return.*

If it should come to its perihelium on the 1st of January 1789, it might probably be visible, with a good achromatic telescope, in its descent to the sun, the middle of September 1788, and sooner or later, according as its perihelium should be sooner or later. It will approach us from the southern parts of its orbit, and therefore will first appear with considerable south latitude and south declination; so that persons residing nearer the equator than we do, or in south latitude, will have an opportunity of discovering it before us. It is to be wished that it may be first seen by some astronomer in such a situation, and furnished with proper instruments for settling its place in the heavens, the earliest good observations being most valuable for determining its elliptic orbit, and proving its identity with the comets of 1532 and 1661. The Cape of Good Hope would be an excellent situation for this purpose. In order to assist astronomers in looking out for this comet, I have here given its heliocentric and geocentric longitudes and latitudes and correspondent distances from the sun and earth, on supposition that it shall come to its perihelium on Jan. 1, 1789. But if that should happen sooner or later, the heliocentric longitudes and latitudes and distances from the sun will stand good, if applied to days as much earlier or later, as the time of the perihelium may happen sooner or later; and the geocentric longitudes and latitudes and distances from the earth must be recomputed accordingly. The calculations are made for a parabolic orbit from the elements determined by Dr. Halley from Hevelius's observations in 1661, only allowing for the precession of the equinoxes. The elements made use of were as follow:

Time of perihelium January 1, 1789, at noon.

Perihelium distance 0.44851.

Place of ascending node $2^s\ 24^\circ\ 18'$.

Inclination of orbit to the ecliptic $32^\circ\ 36'$.

Perihelium forwarder in orbit than the ascending node $33^\circ\ 28'$. Its motion is direct.

* Since this was written, I received the unwelcome news, in a letter from M. Mechain, of the Royal Acad. of Sciences at Paris, that the Academy has not received satisfactory answers concerning the disturbances of the comet between 1532 and 1661, and 1661 and the approaching return, and that the prize is referred to be adjudged of at Easter 1788, and that it will be 6000 livres. N. M. —Orig.

Computed Places of the Comet, on supposition that it shall return to its Perihelium January 1, 1789, at noon.

Times.	Distance from ☉.	Distance from the earth.	Heliocentric longitude.	Heliocentric latitude.	Geocentric longitude.	Geocentric latitude.	Product of distances from ☉ and earth.
1788.			S. D. M.	D. M.	S. D. M.	D. M.	
April 23, 7	4. 0	4.52	11 3 54	30 56 S	11 16 30	27 5 S	18.07
June 4, 1	3. 5	3.54	11 7 6	31 25	11 26 31	31 4	12.38
July 14, 5	3.	2.57	11 11 16	31 55	0 3 21	38 11	7.70
Aug. 2,46	2.75	2.15	11 13 47	32 10	0 4 8	42 59	5.90
.... 20,43	2. 5	1.79	11 16 39	32 22	0 2 0	48 16	4.48
Sept. 7, 3	2.25	1.51	11 20 9	32 32	11 25 6	53 28	3.39
.... 24, 0	2.	1.29	11 24 16	32 36	11 13 12	56 45	2.58
Oct. 10,26	1.75	1.13	11 29 24	32 30	10 28 22	56 36	1.75
.... 26,64	1.50	1.01	0 5 51	32 4	10 15 50	52 6	1.51
Nov. 9,34	1.25	0.88	0 14 19	31 0	10 8 36	46 47	1.10
.... 23,39	1. 0	0.76	0 26 4	28 32	10 4 10	39 0	0.76
Dec. 7,21	0.75	0.62	1 13 58	22 29	9 29 18	27 45	0.46
.... 23,32	0.50	0.50	2 20 58	2 8	9 14 31	2 7 S	0.25
.... 24,35	0.49	0.51	2 24 18	0 0	9 12 58	0 0	0.25
1789.							
Jan. 1, 0	0.45	0.59	3 23 25	17 17 N	9 2 50	13 8 N	0.26

The last observation made by Hevelius on the comet in 1661, was when its distance from the earth was 0.986, and from the sun 1.37, with what he calls a very long and good telescope; at which time it appeared faint and small with it, though still sufficiently visible. Let us suppose this to have been a telescope of 9-feet focal length, with an aperture of 1.65 inch; then, because the diameter of the aperture of a telescope sufficient to render the comet equally visible should be as the product of its distances from the sun and earth, and the product of the numbers above-mentioned 0.986 and 1.37 is 1.35, we shall have the following analogy to find the aperture of a refracting telescope sufficient to show the comet as it appeared to Hevelius: as 1.35 : 1.65 inch :: 9 : 11 inches, so is the product of distances from the sun and earth to the diameter of the aperture required in inches.

XXV. A new Method of finding Fluents by Continuation. By the Rev. S. Vince, A.M., F.R.S. p. 432.

The utility of finding fluents by continuation was manifest to Sir Isaac Newton, who first proposed it; and since his time some of the most eminent mathematicians have employed much of their attention on it. The method which I have investigated and exemplified in this paper I offer as being entirely new; and at the same time it not only exhibits, at once, the general law up to the required fluent, but also appears, from some of the instances here given, to be more extensive and convenient in its application than any method hitherto offered. The general resolution of the given fluxion into a series of fluxions of the same kind, where the index of the unknown quantity without the vinculum keeps decreasing or increasing either by the index under or by half the index, has not, that I know of, before been given; which furnishes us at once not only with a very easy method of continuing fluents, but also points out a very simple method of investigating the fluent of the given fluxion without continuation. For if $\int \dot{A} = p + b\int \dot{B} + c\int \dot{C} + d\int \dot{D} + \text{\&c.}$ $\int \dot{B} = p' + c'\int \dot{C} + d'\int \dot{D} + \text{\&c.}$ $\int \dot{C} = p'' + d''\int \dot{D} + \text{\&c. \&c. \&c.}$ then if for $\int \dot{B}$, $\int \dot{C}$, &c. &c. we substitute their respective values, we shall get a general series for $\int \dot{A}$ without continuation. The extent of any new method is, at first, seldom obvious; and how far that which is here proposed may be successfully employed in other cases will best appear from its application. Different methods will always be found to have their uses in particular cases; for where one becomes impracticable, another will often be found to succeed; and I hope that which is here offered will contribute something towards facilitating the investigation of fluents.

Here, Mr. V. finds the fluent of $\frac{x^m\dot{x}}{(a^n + x^n)^r}$ from that of $\frac{x^m\dot{x}}{a^n + x^n}$ being given.

From the fluent of $\frac{x^n\dot{x}}{a + bx^m + x^{2m}}$, he finds that of $\frac{x^n\dot{x}}{(a + bx^m + x^{2m})^r}$.

From the fluent of $\frac{x^n\dot{x}}{1 - x}$, is found that of $\frac{x^n\dot{x}}{\sqrt{1 - x^2}}$.

From the fluent of $\frac{x^n\dot{x}}{x^m - b}$, is found that of $x^n\dot{x}\sqrt{\frac{x^m - a}{x^m - b}}$.

All these are determined by very ingenious contrivances, and illustrated by many neat examples of particular cases. A variety of other useful and kindred methods and forms may be profitably consulted in the author's treatise on Fluxions since published in 1 vol. 8vo.

XXVI. Conjectures relative to the Petrifactions found in St. Peter's Mountain, near Maestricht. By Petrus Camper, M. D., F. R. S. p. 443.

The discovery of a great number of petrified bones about the year 1770, in the mountain of St. Peter at Maestricht, and particularly of large jaw-bones with their teeth, suggested to the late M. Hoffman, first Surgeon to the Military Hospital at Maestricht, a worthy member of several learned Societies, and a great admirer of natural history, the idea that these maxillæ belonged to crocodiles. This notion was spread by himself and his literary correspondents through all Europe. He did me the favour to send me, not only the history of those petrifactions, but also several figures of the jaw-bones in question, and of other bones, which were all entirely new to me, except some fragments of the bones of turtles. I discovered however, at the very first sight, the characteristical differences which distinguished these bones from those of crocodiles, of which I had at that time several in my collection. His intention was to write on this subject, and to send his essay, containing his reasons for supposing these bones to belong to crocodiles, to the R. S.; but I dissuaded him, as a friend, from doing this, lest he should afterwards be under a necessity of retracting his opinion: and I sent him a figure of the lower jaw of a crocodile, accurately done by my own hand, and soon after the skull and under jaw of a pretty large crocodile; which induced him to defer his design of writing about these antiquities of the old world, till he should be better informed on the subject of cetaceous fishes.

Major Drouin, of Maestricht, who made, about the same time, a collection of an infinite variety of corals, madrepores, alcyoniums, echinites, belemnites, shells, and petrified wood, from the same mountain and its environs, likewise procured a beautiful specimen of two maxillary bones of the same incognitum, but with the insides turned outwards; and this gentleman also supposed them to belong to the crocodile. A sketch of this specimen is to be found in M. Buchoz's Dons de la Nature, tab. 68. But the specimen itself is now in Teyler's Museum, at Haarlem, with the whole of Major Drouin's collection.

Another still more valuable and perfect specimen is to be seen at the house of the Rev. Dean Godding, of which there is also a rough sketch in M. Buchoz's Dons de la Nature, pl. 66. In this the greater part of both the upper and under maxillary bones is entire, and a bone, with small teeth, belonging to the palate; by which it appears that the animal had not only teeth in the jaw-bones, but also in the throat, as several fishes have, but which are never found in the mouth of crocodiles.

Notwithstanding all my endeavours to convince my friends, and afterwards M. Drouin, and particularly the dean, whose valuable and truly beautiful specimens I saw in the year 1782, I never could prevail on them to adopt my opinion, that these bones belonged to physeteres or respiring fishes. M. Hoffman, adhering closely to the Linnæan system, objected, that the physeteres had teeth only in the lower jaw-bone, whereas this fossil monster had them in both upper and lower maxilla. He did not seem to recollect, that φυσητὴρ signifies something respiring, or breathing, and applied to fishes, breathing fishes; nor that the physeteres, according to the Linnean system, have small teeth in the upper jaw-bone, though larger ones in the lower jaw, according to the observations of Dr. Otho Fabricius, in his Fauna Groenlandica, p. 42, where he mentions the macrocephalus, and p. 45, where he speaks of the microps.

In August 1782, I sent M. Godding, who had favoured me with a copy of his valuable specimen, a full demonstration of its being the head of a physeter, or breathing fish, Delphinus, or Orca, or under whatever genus it may be ranked, as having large teeth of the same size in both the maxillæ. But in vain; for he continued still to call it a crocodile, as if its value depended on the species of the animal. The analogy of all the other marine bodies seems to make it still more probable, that these large bones belong to the inhabitants of the sea, and not of rivers. The large turtles, the numberless echinites, madrepores, shells, alcyoniums, belemnites, orthoceratites, and so on, are all sea animals; and the crocodile would, in that case, be the only inhabitant of the rivers mixed with them. The pretended crocodile found near Whitby, in Yorkshire, Phil. Trans. vol. 50, p. 688 and 786,* is undoubtedly the skeleton of a balæna.

§ 2. After the decease of M. Hoffman, his family having offered the whole collection for sale, I went in August 1782 to Maestricht on purpose to examine it; and I could not but greatly admire the richness and beauty of the collection, especially that of the fossil bones from St. Peter's mountain; but as the heirs did not consider the expenses necessary to transport the collection down the Maese, where each sovereign puts an enormous duty on every thing that passes through his territories, nor the small number of persons who were likely to purchase it, they rated the price so high that nobody chose to bid for it. The eldest daughter, having at length become possessed of the whole, offered me the principal specimens at a price I agreed to. Among them were the duplicates I have already sent to the British Museum, and with which the honourable trustees are perfectly satisfied. These specimens may serve also to ascertain what I have said about them, as being real fragments of physeteres, some of turtles, and the like, but not a single one of any species of crocodile.

§ 3. The arguments for their being jaw-bones and vertebræ of fishes seem to be, first, the smoothness of these bones; and, 2dly, the many holes by which the nerves go out at the side, and under each tooth, as is very evident in that beautiful specimen now in the British Museum, on the outside of which eleven holes are visible, in the same manner as they are in the delphini, and more particularly in the lower jaw-bone of the cete, the physeter macrocephalus, or potfish, cachalot, &c. 3dly, the form of the teeth, which have solid roots, as in pl. 3, fig. 6, BCEF, and the teeth of fig. 8. 4thly, because there are small teeth in the palate, as in Dean Godding's specimen. 5thly, because the vertebræ have the appearance of true cetaceous vertebræ, as in fig. 5, and in several beautiful and large specimens now in the Museum. Several of these vertebræ were besides entirely unknown to me, and not at all analogous to the vertebræ of the crocodile, described and represented by Dr. N. Grew.

§ 4. As I intended to visit London in 1785, I flattered myself I should still find the skeleton of the great crocodile formerly at Gresham College, and be able to find out such characteristic distinctions as should be necessary to decide

* Abridg. vol. x, page 259, 289.

the question. Dr. Gray was so kind as to go with me to the lower apartments of the British Museum, where we found, though not without difficulty, the skeleton much neglected, spoiled, and deprived of several interesting parts. I admired however the remains of it, being infinitely pleased with the transverse sutures, fig. 1, 2 abcfδζ, by which not only those of the neck and thorax, but those of the loins also, are divided, and which I made a drawing of, as large as the life, the 20th of October, 1785, of which fig. 1 and 2 are very accurate copies.

I confess I had not observed that particular division or suture in the skeleton of a small crocodile, of 13 inches, made by my youngest son; but after being apprized of it by the large skeleton in the Museum, of 12 feet 4 inches, Paris measure, on looking at my own when I returned home, I found them both alike, and that those parts were not epiphyses; of which however the transverse processes of the neck, fig. 1, deqonp, have all the appearance, though there is no other epiphysis to be observed in the rest of the bones of that large skeleton. When we compare the fossil vertebra, fig. 5, with those now in the Museum, we shall find the epiphyses ABCD analogous to abcd, fig. 4, being the real epiphyses in the vertebra of a young porpoise.

I procured, in London, the largest vertebræ of the neck of a turtle I could get, and prepared two of them as in fig. 3, in which, as along the back of that singular creature, I found the transverse divisions acdf: of all which I have not seen a single instance among the dorsal spinæ from St. Peter's mountain, one of which consists of 7, another of 12, and a third of 14 vertebræ. Some of the vertebræ have, I acknowledge, an inferior process, as in the crocodile, lm, fig. 1. Of these I have sent also 2 to the Museum. The ostrich and the turtle mydas have such processes, but no quadruped I know of.

The articulation of the vertebræ with each other, by the surfaces of the bodies themselves, is entirely different, not only from that of the crocodile, but from that of all the cetaceous fishes I have ever seen: and I dare venture to assert, I have seen a great many, exclusive of those in my collection. The anterior part of the Maestricht vertebræ is more or less triangular and hollow. as in fig. 5, CDL. The posterior AB is convex. Both these surfaces are very smooth, as if they had been covered with a very thin cartilage, and moved on each other, without being united by an elastic lamella, as in all quadrupeds and cetaceous fishes; in which the vertebræ have on both the surfaces a round brim, or circular edge, ahib, by means of which the ligaments are connected, and a flat hollow surface within, as hi, fig. 4, for the elastic pulp that is between them.

§ 5. The dentition is so singular in these fossil jaw-bones that it deserves a particular description. In all quadrupeds, as in man, the teeth which appear first are all shed at a certain period of life, and in the mean time new ones are formed above, under, or at the sides of the primordial or temporary teeth, but in different sockets. The grinders are not all renewed, but in general 3 when there are 6, and 2 when there are 5. Nature however is not always uniform in this operation. Mr. John Hunter has given a very interesting and complete natural history of the teeth, in which these observations are stated. In the crocodile the succeeding or secondary teeth appear even when the animal's head is equal to 2 feet; that is, when it has acquired $\frac{1}{3}$ of its usual growth. When they grow too fast, before the temporary tooth is shed, they perforate the side of the bone, at the part where they meet with the least resistance. Instances of this variety occur in the large crocodile's head in my collection.

In all quadrupeds, the enamel is, of the solid part of the teeth, the first formed, making a cavity, in which the other bony substance is deposited, and formed by lamellæ placed one within another, as is observed by Mr. John Hunter, in the work already mentioned, p. 92. To this the root is added, which is filled in the same manner till the tooth is long enough to pierce through the gums. But in the fossil jaw-bones of St. Peter's mountain, a small secondary tooth is formed, with its enamel and solid root at once, within the bony substance of the primordial or temporary tooth itself, as is to be seen in the small fragment now in the British Museum, and in fig. 8, ABCDE; which, by continuing to grow, seem to make by degrees sufficient cavities in the bony roots of the primary teeth: but what becomes of them at last, and how they are shed, I am not able to guess. I have one in my collection, where the succeeding tooth is entirely formed within the centre and substance of the primordial tooth. In fig. 6, a little oval cavity is observable, which has been the seat of a new or secondary tooth.

§ 6. The maxilla inferior of the incognitum, sent by me to the British Museum, is a most magnificent specimen, having 14 teeth. A similar one, somewhat longer, as it measures $3\frac{1}{2}$ feet, in my own collection, has also 14. Another fragment of the left side, 2 feet long, and 8 inches broad, shows the primordial and succeeding teeth in the clearest manner. The specimen, of which I sent a drawing, fig. 8, to the illustrious President of our Society, Sir Joseph Banks, is still more useful to confirm the mode of dentition than any other I have in my Museum.

§ 7. Several ribs and the phalanges of the toes of the fore-feet, a specimen of which I sent in a fragment from the same rock, of about a foot long and 8 inches broad, may serve as another proof of the difference between these and the crocodile's toes, when compared with the still valuable, though neglected, skeleton in the British Museum; which I am sorry I could not make a drawing of, having been too much employed on other objects. All these characteristic differences cannot fail to convince the learned Society of the truth of what I have asserted, about the animal these bones belonged to; for though we cannot determine exactly the species itself, yet I flatter myself the preceding observations evidently prove that they did not belong to any animal of the crocodile kind.

§ 8. Another very beautiful specimen, a foot and a half long, and about 10 inches broad, I have been induced to add, because it contains the anterior part of the scutum of a very large turtle. Of this Mr. John Hunter has a similar bone from the same mountain in his valuable collection, but sent to him under another name. I am convinced it belonged formerly to a turtle, first, because I have from the same mountain the entire back of a turtle, 4 feet long and 16 inches broad, a little damaged at the sides, and a pretty large fragment of another turtle, in my possession. 2dly, Because I have a similar one, but so placed within the matrix as to show the inside, which is perfectly similar to the inside of that piece in the back of a large turtle I got in London, by the favour of Mr. Sheldon. 3dly, Because I have among these bones the lower jaw-bone of a very large turtle, of which the crura, though not entire, are 7 inches long, and distant from each other 6 inches; the thickness is equal to 1¼ inch. All these fragments prove the frequency of turtle bones among the other fossil bones found in the mountain near Maestricht.

Dr. Michaelis wrote to me some time ago, that the above-mentioned fragment, in Mr. J. Hunter's Collection, belonged to a bird; which I could hardly believe, as I never had seen in any collection whatever, either in London, Paris, Brussels, Gottingen, Cassel, Brunswick, Hanover, or Berlin, nor in my own country, any fossil bone belonging to a bird. I know there is a small one described in the Abbé Rozier's Journal de Physique, for March 1782, which is at present in the collection of M. D'Arcet, at Paris. I expect also from Montmartre a small leg of a petrified bird; but these are the only ones I have ever heard of, those of Stonefield, near Woodstock, being most undoubtedly of fishes. I think it is a circumstance worthy the attention of the curious, that no human bones, and of birds but very few, have been hitherto found in a petrified state, and belonging to the old world.

Explanation of the plates.—Fig. 1, 2, pl. 3, are vertebræ taken from the skeleton of the crocodile described by Dr. Neh. Grew, in his Catalogue of the Natural Rarities at Gresham College, p. 42 and 43.

abcf$\delta\zeta$, the bodies of the vertebræ; ab of the 4th; cf of the first vertebra of the neck; βzt, and xyw, the spinous processes; $\gamma\beta$ and s the ascending; t and uv the descending processes; ghcidenpoq, the transverse, united by cartilages to the bodies of the vertebræ. Grew calls them ossa mucronata. The transverse processes of the 4th vertebra being lost, the roots of the mucronated processes are very evident at ghik. On the under part of these vertebræ are (l and m) processes, similar to those we find in the vertebræ of the neck in turtles and birds. Not only the 6 posterior but the 5 anterior vertebræ of the back are provided with such processes; of these however Dr. Grew makes no mention.

Fig. 2 represents the 7th vertebra of the back; A and C are the ascending and descending processes, forming the articulations with the adjacent vertebræ; B the transverse process, to which is united the rib FB in B; DE the spinous process; HHI the body of the same vertebra. These figures abridged, were all made from the same skeleton, now in the British Museum. The whole length is equal to $12\frac{1}{3}$ feet, Paris measure; the head equal to 2 feet; the neck equal to 1 foot; the trunk equal to 3 feet 8 inches; the tail equal to 5 feet 8 inches. The measurement given by Dr. Grew does not agree with mine; but he seems not to have taken it with great attention (p. 42), for he makes use of the words about, almost, &c.

Observation.—What struck me was, the transverse suture, abcfδζ, which divided the bodies of all the vertebræ of the neck, back, and loins. This division ended with the os sacrum, which was entire, as were also the vertebræ of the tail. Dr. Grew seems only to have taken notice of the sutures belonging to the transverse processes. I have a small skeleton of a crocodile equal to 13 inches, in which the 7 vertebræ of the neck, 12 of the back, and the 5 of the loins, are divided in the same manner as in the large skeleton in the British Museum. Those of the os sacrum and tail are without, and have no mark of an epiphysis.

Conclusion.—The transverse division of the vertebræ above-mentioned is also peculiar to this animal; and there is no epiphysis, as in other animals. To be sure of this, I dissected and made a skeleton of the Lacerta Iguana, Linn. sp. 26, perfectly well described by Maregraf, Hist. Bras., p. 236, cap. 11; but I found no such divisions, though the animal was young, and though it had still epiphyses on the legs, &c. The neck consists of 4 vertebræ, the back of 11, the loins of 9, the os sacrum of 2, as in the crocodile; the tail of more than 60. The dissection of tortoises seemed of consequence, at least a more accurate inspection of the vertebræ, particularly those of the neck, as being analogous in some respects to those of the crocodile, especially in the structure of the inferior processes D and E, with lm, fig. 1.

Fig. 3. Represents 2 vertebræ of the neck of a pretty large turtle, reduced. ABBC the bodies; L and I the ascending, H and T the descending processes; RK the spinous, abde the transverse, and DE the inferior processes. abcdef, the transverse division of these, similar to that in the crocodile.

Fig. 4, a vertebra from the tail of a young phocæna or porpoise; in which ab is an orbicular plate, united by means of cartilage to the body of the vertebra ad, which is provided with such a one on both sides, ab and cd. Those bony lamellæ are the epiphyses of the vertebræ, and are alike in all quadrupedes, to which class all the cetaceous fishes belong. When we consider the structure in general of these last, we find the hind legs only are wanting, and of course the ossa innominata; but the ossa pubis are very remarkable in all of them.

Fig. 5 is a fossil vertebra of the unknown animal, whose bones are so often met with in St. Peter's Mountain at Maestricht. ABCD is the body; CIKEF the spinous processes; CKI the medullary canal, running under KEF, in a direction parallel to IF, and coming out again at F. The remaining marks of the lamellated epiphyses, ID and AB, are evident proofs of the analogy between these and the vertebræ of the cetaceous fishes; and also of their want of resemblance to the vertebræ of the crocodile, as will appear by comparing the 1st and 2d figures with the 5th.

Fig. 6 is a very accurate drawing of one of the fossil teeth belonging to the same incognitum. ABC is its point, of a lanceolated figure, whose edges, BA and AC, are dentated; BC is the root, uneven, bony, fixed within the socket with DGF; DGBC is covered with the gums; HI is an oval sinuosity, in which generally the secondary teeth are generated, as is seen in fig. 8, representing a fragment of the upper jaw-bone of the same incognitum, ABCDE. The teeth in all the physeteres and delphini have solid roots, except in the young ones, in which they often have cavities to receive the blood-vessels and nerves. But the crocodile has the teeth entirely hollow, as appears in

Fig. 7, in which the cavity ΠΔΘ shows the difference between the crocodile's teeth and those of the cetaceous and other fishes. This tooth is the anterior one of a large head of a crocodile, 2 feet long, and of the same size as that in the British Museum. A hollow tooth may however belong to a physeter, as Dr. Otho Fabricius observes in his Fauna Groenlandica, p. 44, when speaking of the physeter microps: of which he says, "Habet in maxilla inferiori dentes 22, utrinque 11 arcuatos, falciformes, intus ad apicem usque cavos," within they are hollow to the very end.

Fig. 8, Fragmentum maxillæ superioris, lateris dextri capitis Physeteris incogniti, ex Monte St. Petri, Traj. ad Mosam. Origo dentium serotinorum ex ipsis radicibus solidis primo enatorum in quinque manifesta est. Quæ ad dentitionem hanc singularem pertinent, ex fig. 2, Tab. Fragm. similis sed Maxil. inf. 12 Aug. 1784. peti debent.

XXVII. Catalogue of One Thousand New Nebulæ and Clusters of Stars. By William Herschel, LL. D., F. R. S. p. 457.

The following catalogue, which contains 1000 new nebulæ and clusters of stars, is extracted from a series of observations, or sweeps of the heavens, which was begun in the year 1783, and which I am still continuing till the whole be completed. As I may perhaps find an opportunity hereafter to publish these observations at full length, I shall now only mention such circumstances, relating to the instrument and apparatus with which they were made, as will be necessary to show what degree of accuracy may be expected in the determination of the places of these nebulæ and clusters of stars; and also to serve any astronomer, who wishes to review them, to form a judgment what instrument will suffice for this purpose.

The telescope I have used, as has been observed on a former occasion, is a Newtonian reflector of 20-feet focal length, and $18\frac{7}{10}$ inches aperture. The sweeping power has been 157, except where another is expressly mentioned. The field of view 15′ 4″. My eye-glass is mounted on that side of an octagon tube, which, in the horizontal position of the instrument, makes an angle of 45° with the vertical; having found, by experience, that this position, resembling the situation of a reading desk, is preferable to the perpendicular one commonly used in the Newtonian construction. In the present improved state of the apparatus this telescope will, in general, give the relative place of an object by a single observation true to within $1\frac{1}{2}$ or 2 minutes of polar distance, and 4 or 6 seconds of time in right ascension. But when there is an opportunity of repeating the observation, it will hardly differ a single minute in the former, and seldom so much as 3 or 4^s in the latter. My apparatus however has not been equally perfect from the beginning; for, being from time to time adapted to the different views I had in sweeping, it could only arrive to its present degree of perfection by many experiments and gradual improvements.

To begin a short history of this 20-feet telescope. In the month of October of the already mentioned year I began to use it, being then mounted on its present stand, but with a lateral motion under the point of support of the great speculum, by which its direction could be changed about 15 degrees. It had also a kind of moveable gallery in front, about 9 feet long, which permitted me to follow a celestial object near 15 degrees more; by which means I obtained a range of 30 degrees without moving the stand. The Newtonian form has the capital advantage of rendering observations equally commodious in all altitudes; I had therefore placed the instrument in the meridian, that I might view the stars in their most favourable situation.

When I had seen most of the objects I wished to examine, I proceeded to the work of a general review of the heavens. The first method that occurred was, to suffer the telescope to hang freely in the centre; then, walking backwards and forwards on the moveable gallery, I drew the instrument from that position by a handle fastened to a place near the eye-glass, so as to make it follow me, and perform a kind of very slow oscillations of 12 or 14 degrees in breadth, each taking up generally from 4 to 5 minutes of time. At the end of each oscillation

I made a short memorandum of the objects I chanced to see; and when a new nebula or cluster of stars came in my way, I made a delineation of the stars in the field of view, both of the finder and of the telescope, that it might serve to find them again. This being done, the instrument was, by means of a fine motion under my hands, either lowered or raised about 8 or 10 minutes, and another oscillation was then performed like the first. Thus I continued generally for about 10, 20, or 30 oscillations, according as circumstances would permit; and the whole of it was then called a sweep, and as such numbered and registered in my journal.

When I had completed 41 sweeps, the disadvantages of this method were too evident to proceed any longer. By going into the light so often as was necessary to write down my observations, the eye could never return soon enough to that full dilatation of the iris which is absolutely required for delicate observations. The difficulty also of keeping a proper memorandum of the parts of the heavens which had been examined in so irregular a manner, intermixed with many short and long stops while I was writing, as well as the fatigue attending the motion, on a not very convenient gallery, with a telescope in my hands of no little weight, especially at the extremes of the oscillations, where it made a considerable arch upwards, were sufficient motives to induce me to look out for another method of sweeping. And it is evident, that the places of nebulæ hitherto determined, which was till the 13th of December, 1783, must be liable to great inaccuracy. I therefore began now to sweep with a vertical motion; and as this increased the labour of continually elevating and depressing the telescope by hand, I called in the assistance of a workman to do that part of the business, by which means I could observe very commodiously, and for a much longer time than before. Soon after I removed also the only then remaining obstacle to seeing well, by having recourse to an assistant, whose care it was to write down, and at the same time loudly to repeat after me, every thing I required to be written down. In this manner all the descriptions of nebulæ and other observations were recorded; by which I obtained the singular advantage that the descriptions were actually writing and repeating to me while I had the object before my eye, and could at pleasure correct them, whenever they disagreed with the picture before me without looking from it.

In about half a dozen sweeps, done according to this new way, I found that the stars of Flamsteed's catalogue entered nearly at the time when they were expected; this suggested the possibility of converting my telescope into a transit instrument. By way of trial, Dec. 18, 1783, I began to use a watch, and noted the times of the transits of stars and nebulæ to the nearest minute; and, this succeeding, Dec. 24, a sidereal time-piece was introduced. I found also that, by the turns of the handle which gave motion to the telescope, it was practicable, in a coarse way, to ascertain the difference of altitude between any two objects that passed the field of view; on which account, Dec. 30, I began to use an index-board, divided into inches, and marked with numbers, which, being placed behind the rope that moved the telescope, would point out at what altitude a certain index, affixed to the rope, was situated. My tackle of ropes and pulleys was such that, while the telescope traversed an arch of 2°, the mark on the rope passed over about 24 inches of the index-board: but the exact measure was always to be determined experimentally, as it varied according to the situation of the instrument. I perceived immediately that the quantity of rope used in the motion of the telescope would be much better observed by the assistant, if the index were brought within doors near the writing desk: to effect this I used a small cord, which, being led off from the great one, was carried over a pulley into the observatory, so as to pass over a set of numbers, which I now divided into such parts as, in an equatorial situation of the instrument, would give nearly each equal to one minute.

It would exceed the limits of this paper to enumerate the various trials I made to bring the right ascension to greater perfection; such as causing the tube sometimes to hang inclining or rubbing against a perpendicular plane; at others, drawing it against the same by a small weight, fastened to a cord, passing over a side pulley, &c. I shall also pass over the several changes in the form of the machine showing the polar distance, which, for convenience sake, was soon brought to an index moving over a dial, in the manner of a clock. By way of directing the person who gives motion to the telescope, a small machinery was added, which strikes a bell at each extreme of the breadth of the sweep, and is adjustable to any required number of turns of the handle.

In June, 1784, I introduced a small quadrant of altitude, the use of which became soon after of the greatest consequence in determining the value of the numbers of the polar distance piece. Hitherto I had settled this value by causing a star to pass vertically through the field of the finder, which was very accurately limited to 2°; but now I found, by many comparisons between the degree determined by the quadrant and by the finder, that I had generally under-rated the value of the numbers. Fortunately so many stars of Flamsteed's catalogue had been taken, that the numbers between their different polar distances were sufficient to recover the value of the degree; but this occasioned a laborious re-calculation of the places of all objects taken in near 300 sweeps. The quadrant being once introduced, I carried the refinements of the determination, in high sweeps where the ropes acted very unequally, so far as to ascertain by it separately the value of every 20 or 30 minutes throughout the whole breadth of a sweep of 2°, and the numbers were then accordingly cast up by so many different tables calculated on purpose.

Being still disappointed in many instances, when, on a review of a nebula whose place I had before determined, I perceived a difference of 4 or 5 minutes in polar distance, I began at last entirely to new model the machinery of the polar distance piece, and on Sept. 24, 1785, completed one with the following capital improvements. My former piece showed a set of numbers whose value differed in every situation of the telescope, and therefore required different and very extensive tables to cast them up in degrees and minutes. This shows at once both the degree and minute of the polar distance of every celestial object, without requiring any tables to cast up numbers. In the next place, the considerable inaccuracy arising from the unequal tension of the great ropes, and their expansion or contraction by moisture or dryness, is entirely taken away; for now my index cord is contrived so as to go off from the front of the telescope itself, in the direction of a tangent to the arch it describes when moving; by which means this cord will even serve as an hygrometer to show the variations of the ropes that suspend the telescope. If a shower of rain, for instance, should shorten them so as to elevate the telescope 2, 4, or 6 minutes, which has happened sometimes, notwithstanding they have all been well saturated with oil, the index cord will immediately make the polar distance clock show this effect of the rain, by pointing out an equal change of the dial. As to the variations of the cord itself, they are in the first place very trifling, since it consists merely of a few threads of hemp, very loosely twisted, well oiled, and always equally stretched; but especially these variations are of no consequence, as they are so easily to be discovered by the check of the quadrant of altitude affixed to the telescope, or the successive transits of known stars, and may either be immediately corrected by the adjustable hand of the polar distance dial, or be left to be accounted for afterwards.

The improvement of the right ascension has not been less attended to; and the R. S. having kindly intrusted me with an excellent time-piece, I succeeded at last by means of the addition of the following apparatus. Against the side of the tube is fixed a vertical iron plate, and the point of suspension of the telescope is disposed so as to permit this plate to be just in contact with a roller which remains fixed during the time of a sweep. There is also a considerable spring applied on the opposite side, in such a manner as, by always exerting a pressure nearly uniform, to cause the iron plate to rub against the fixed roller as the telescope sweeps up and down. By this means I have frequently, in very stormy weather, observed many hours without finding my time materially affected, and the corrections will seldom, in accurate observations, exceed a few seconds.

To those who are accustomed to the accuracy of transit instruments in regular observatories, this telescope, notwithstanding the above-mentioned improvements, may perhaps appear far from being brought to perfection; but they should recollect the size of the instrument as well as its extensive use, since I can not only follow any object for near a quarter of an hour, without disturbing the situation of the apparatus, but can at pleasure, in a few minutes, turn it to any part of the heavens, and view a celestial object wherever it may chance to be situated, even the zenith not excepted.

From this account it will be understood, that the places of a few of the nebulæ and clusters of stars, determined before the 13th of December, 1783, may be faulty in right ascension as far as 1^m of time, and in polar distance to 8 or 10′ of space. Afterwards the errors will be found to become gradually less considerable till the latter end of the year 1784, when I suppose they will seldom exceed half that quantity. From that period to Sept. 24, 1785, they will diminish, and probably not often amount to so much as 3 or 4′ in polar distance, and 10 or 12^s in right ascension. And now I flatter myself that all places, determined since the last mentioned time, will generally be true to a very small quantity; such as 4 or 6^s in right ascension, and $1\frac{1}{2}$ or 2′ in polar distance, and often much nearer.

Some of the nebulæ in that part of the heavens which, in a former paper, I have called the stratum of Coma Berenices, are indeed so crouded, that there was no possibility of taking them all in the centre of the field of view, and a somewhat less degree of accuracy may therefore be expected; but having used myself by very frequent estimations of the parts of the field of view to judge of their value in time as well as in space, I corrected this defect at the moment of observation by affixing to the transits of these excentric nebulæ, such proper marks of plus or minus in right ascension and polar distance, as I judged would bring them to a central observation. A similar method, well known to good astronomers in estimating their 10ths of seconds by the proportional space over which the stars move in their meridian passage, makes it unnecessary to expatiate on the degree of accuracy that long practice enables us herein to obtain.

If however I had been willing to delay giving this catalogue till, by a repeated review of the heavens, the places had been more accurately determined, the work would undoubtedly have been more perfect; but whoever considers that it requires years to go through such observations will perhaps think with me, that it is the best way to give them in their present state, if it were but to announce the existence of such objects by way of inducing other astronomers also to look out for them. Another motive for not delaying this communication, is to show that my late endeavours to delineate the construction of the heavens have been guided by a careful inspection of them; and probably a catalogue which points out no less than 1000 instances of such systems as those are into which I have shown the heavens to be divided, will considerably support what has been said on this subject in my two last papers.

When the diurnal motion of the earth was first maintained, it could not but greatly add to the reception of this opinion when the telescope exposed to our view Jupiter, Mars, and Venus, revolving on their axes;* and if these instances of the similar condition of other planets support the doctrine of the diurnal motion, the view of so many sidereal systems, some of which we may discern to be of a most surprising extent and grandeur, will in like manner add credit to what I have proposed with regard to the condition of our situation within a system of stars; for, to the inhabitants of the nebulæ of the present catalogue, our sidereal system must appear either as a small nebulous patch; an extended streak of milky light; a large resolvable nebula; a very compressed cluster of minute stars hardly discernible; or as an immense collection of large scattered stars of various sizes. And either of these appearances will take place with them according as their own situation is more or less remote from ours.

In the distribution of the nebulæ and clusters of stars into classes, I have partly considered the convenience of other observers: thus, in the first class, the degree of brightness of the nebulæ has been the leading feature, as most likely to point out those which their several instruments may give them expectation to reach. The 1st class therefore contains the brightest of them; the 2d, those that shine but with a feeble light; and in the 3d are placed all the very faint ones. Besides this general division, I have added a 4th and a 5th class, which contain nebulæ that, on different accounts, seemed to deserve a more particular description than I had allotted to the 3 former divisions. The clusters of stars are sorted by their apparent compression, in the manner of my former catalogues of double, treble, and multiple stars; so that the closest and richest clusters take up the 1st class; the brightest, largest, and pretty much compressed ones, the 2d; and those which consist only of scattered and less collected large stars, are put into the last.

In every class, the order of time when the nebulæ and clusters of stars were discovered, or first observed with my 20-feet telescope, has been followed; and that I might describe all these objects in as small a compass as could well be done, I have used single letters to express whole words, an explanation of which, with an example of the manner of reading those letters, is given. It should be observed, that all estimations of brightness and size must be referred to the instrument with which the nebulæ and clusters of stars were seen; the clearness and transparency of the atmosphere, the degree of attention, and many more particular circumstances, should also be taken into consideration; so that probably some of the nebulæ which I have called very bright, and very large, may only be just perceivable, as very small faint patches, in many of our best common telescopes.

* To these may now also be added Saturn, on whose body I have, in the year 1780, seen several belts, with spots that changed their situation in the course of a few nights.—Orig.

The identity of each nebula in this catalogue has been well ascertained by a projection on a proper map, made on purpose, which pointed out all other nebulæ near its place, and thus afforded the means of a rigorous examination. When therefore several nebulæ are found within the limits of the accuracy with which my telescope can discriminate them, in different nights, it may be concluded, that they were seen either at once in the same field of view, or otherwise in immediate succession during the same sweep. In the same manner these nebulæ have been compared with those that are contained in the 2 volumes of the Connoissance des Temps, for the years 1783 and 1784, of which none have been inserted in this catalogue. It was indeed easy enough to distinguish the nebulæ of that excellent collection from those of mine which in several places are very near them: the quantity of good light in my telescope having enabled me, even in bright moon-light nights, to see occasionally some of the most feeble of the former, when the latter could not by any means be perceived.

Dr. H. adds the catalogue of the 1000 new nebulæ and clusters of stars, with an explanation of the marks and columns in which they are disposed; all which it is unnecessary to repeat in this place.

XXVIII. Investigation of the Cause of that Indistinctness of Vision which has been ascribed to the Smallness of the Optic Pencil. By William Herschel, LL.D., F.R.S. p. 500.

Soon after my first essays of using high powers with the Newtonian telescope, I began to doubt whether an opinion, which has been entertained by several eminent authors, "that vision will grow indistinct, when the optic pencils are less than the 40th or 50th part of an inch," would hold good in all cases. To judge according to so rigid a criterion, I perceived that I was not entitled to see distinctly with a power much more than about 320, in a 7-feet telescope which bore an aperture of 6.4 inches; whereas in many experiments on double stars I found myself very well pleased with magnifiers that far exceeded such narrow limits. This induced me, as it were, by way of apology to myself, for seeing well where I ought to have seen less distinctly, to make a few experiments on the subject of the diameter of optic pencils. It occurred to me, that an opinion which limits them to any given size cannot be supported by theory, which does not determine on subjects of this nature, but must be decided, like many other physical questions relating to matters of fact, by careful experiments made on the subject. The way therefore to come at truth, in a case which seemed of considerable importance, lay still open to me, as it had done to former observers; and I thought myself authorized, according to a Cartesian maxim, Dubia etiam pro falsis habenda, to suppose, for a while, the size of optic pencils, requisite for distinct vision, entirely undecided.

The first opportunity I had of making the proposed experiments was in the year 1778, and the result of them proved so decisive that I have never since resumed the subject; and had it not been for a late conversation with some of my highly esteemed and learned friends, I might probably have left the papers on which these experiments were recorded, among the rest of those that are laid aside when they have afforded me the information I want. But a doubt seeming still to be entertained on the subject of the smallness of the optic pencils, it may now be proper to communicate these experiments, that it may appear how far the conclusions I have drawn from them are warranted by the facts on which I suppose them to rest.

Experiments with the naked eye.—Exper. 1. Through a very thin plate of brass I made a minute hole with the fine point of a needle; its magnified diameter, very accurately measured under a double microscope, I found to be .465 of an inch, while under the same apparatus a line of .05 in length gave a magnified image of 3.545 inches. Hence I concluded, that the real diameter of the perforation was about the 152d part of an inch. Through this small opening, held close to the eye, I could very distinctly read any printed letters on which I made the trial. Proper allowance must be made for the very inconvenient situation of the eye, which by the unusual closeness to the paper cannot be expected to see with its common facility. Besides, the continual motion of the letters, which is required on account of the smallness of the field of view, must needs take up a considerable time.

Exper. 2. In some other pieces of brass I made smaller holes; and among many, that were measured with the same accuracy as in the former experiment, I found one whose magnified diameter was .29: hence the real diameter could not exceed the 244th part of an inch. Through this opening I could also read the same letters; but the difficulty of managing so as not to intercept all the incident light, as well as the very uneasy situation of the eye, were sufficient reasons for not carrying the intended experiments any further under this form. Besides, I should hardly have allowed them to be fair, if, on a further contraction of the hole in the brass plate, an indistinctness had come on; as we might well have suspected at least 2 other causes, besides the smallness of the pencils, to contribute to such an imperfection, viz. want of light, and a deflection of it on the contracted edges of the hole.

Microscopic experiments.—Exper. 3. I had now recourse to a double microscope, consisting, for simplicity's sake, of only 2 lenses. The focal length of the eye-glass, carefully ascertained by an object half a mile off, being .9; the distance of the object-glass from the eye-glass 9.36; and the aperture of the object-glass .0405. Hence we compute that the diameter of the optic pencil, when it entered the eye, could not exceed the 232d part of an inch; yet with this construction I saw very distinctly every object I placed under the microscope.

Exper. 4. I reduced the aperture of the object-glass to .013; hence the pencil was found to be the 724th part of an inch; and yet I saw with this construction very distinctly every object that was placed under the magnifier.

Exper. 5. I made a 2d reduction of the aperture of the object-glass, so that now it was no more than .0052; and therefore the optic pencil less than the 1800th part of an inch; and yet I could very well count the bristles on the edge of the wing of a fly, and distinguish their length and thickness.

Exper. 6. Changing the construction of the microscope, I now reduced the pencils by an increase of power. Solar focus of the eye-glass .52; distance between the object-glass and eye-glass 7.6; aperture the same as in the 3d experiment. This gave me a pencil of the 336th part of an inch, with which I saw very distinctly.

Exper. 7. Applying now the reduced aperture of the 4th experiment, I had a pencil of the 1139th part of an inch, with which I saw very well.

Exper. 8. I changed the eye lens for another of .171 focal length; the object-glass and distance between the two lenses remaining as in the two last experiments; aperture .02. This gave a pencil of the 2173d part of an inch, with which I could count, or rather successively see, the bristles before-mentioned very well; the field, on account of the great power, not taking in more than 2 large and a small one at a time.

Exper. 9. I was now convinced, that we may see distinctly with pencils incomparably less than the 40th or 50th part of an inch; and indeed so far from expecting any obstruction to distinct vision from the smallness of the pencils, it appeared now as if their size might in future be entirely left out of the account. With a view however of seeing what other cause might bring on that indistinctness which had been ascribed to the smallness of the optic pencils, I continued these experiments with a variation in the apparatus, and used now an object lens of a different focal length; the aperture and other particulars being as in the 4th experiment. By this construction, which gave me a pencil of the 724th part of an inch, I could see objects very well; but though they appeared distinctly, they were not so sharp on the edges as one would wish to see them. This being compared with the 4th experiment, it appeared that, with equal pencils, unequal degrees of distinctness may take place; and a pretty striking circumstance, which served to lead me in the following experiments, was, that the smallest power gave me the least distinct image; notwithstanding, from former trials, the goodness of the lenses I employed could not be doubted.

Exper. 10. On an examination of circumstances it occurred to me, as indeed I had already before surmised, that a certain proportion of aperture might be necessary to a given focal length of an object-lens or speculum; and that a failure in this point might probably bring on that indistinctness which had been ascribed to the smallness of the pencils. In order therefore to put this to a trial, I used now an object-lens of 1.25 focal length, with an aperture confined to .01; the rest of the apparatus being as in the 3d, 4th, and 5th experiments. The pencil in this case was about the 1000dth part of an inch; and though by a different construction I had already seen very well with a pencil of not half that diameter, I found this to give me, as now I had reason to expect, a very indistinct picture, so much so indeed, that it could hardly be called a representation of the object.

Exper. 11. Increasing the aperture of the object-lens to .0124, I had a pencil of the 758th part of an inch, but could see no better with it.

Exper. 12. Proceeding in the track now pointed out to me, I admitted an aperture of .017, which gave a pencil of the 550th part of an inch, but could see not much better with it than before.

Exper. 13. On a further increase of the aperture to .0231, and a pencil of the 406th part of an inch, I saw a little better; but still had not distinctness enough even to see the bristles before-mentioned at all. Hence we may conclude that, in such constructions as the present one, the aperture of the object-glass must bear a considerable proportion to its focal length; since the 54th part (for .0231 : 1.25 :: 1 : 54) is here not nearly sufficient.

Exper. 14. To the same apparatus I applied a higher power, by an exchange of the eye-glass; but the indistinctness remained as before.

Exper. 15. Returning again to the former construction, I admitted an aperture of about .037; and having now a pencil of nearly the 250th part of an inch, I could but just perceive some of the large bristles; which shows that even the 34th part (for .037 : 1.25 :: 1 : 34) of the focal length is not a sufficient aperture for object-lenses that act under such circumstances as the present.

So far I have only related experiments that were made in the year 1778; and my opinion that the smallness of the optic pencils could be no objection to seeing well being thus supported by evident facts, I hesitated not, in a paper on the Parallax of the Fixed Stars (Phil. Trans. vol. 72, p. 96) to affirm, that we might see distinctly with pencils much smaller than the 40th or 50th part of an inch. It did not appear to be necessary, nor would the subject of that paper permit me to enter into a detail of experiments; but baving, in the course of my reading about that time, met with an account of some very small globules made for microscopic uses, I contented myself with an instance of small pencils taken from them. I shall however now proceed just to hint at a few inferences that may be drawn from these related experiments; as, on a mature consideration, we may find reason to believe they point out a cause of indistinctness of vision hitherto never noticed by optical writers; and which, when properly investigated, cannot but influence, and in some respects contribute to the improvement of, our theories in optics. For, admitting that every object-glass or speculum, whose aperture bears less than a certain ratio to its focal length, will begin to give an indistinct picture, it will follow, that while former opticians have been endeavouring to diminish the aberrations arising from the spherical figure, and the different refrangibility of rays, by increasing the focal length, they have been unaware of exposing themselves to the consequences of the cause of indistinctness here pointed out. And till its influence shall be well ascertained and brought to a proper theory, we must suspect that such tables as those which are given in our best authors of optics, pointing out an aperture of less than 6 inches for a glass of 120 feet focal length, or a ratio of 1 to 240, must be far from having that degree of perfection which may yet be obtained. No wonder that telescopes, made according to theories or tables, where one of the causes of indistinctness is unsuspected, and therefore left out of the account, can bear no smaller pencil than the 40th or 50th part of an inch! If then, on one hand, by increasing our apertures we certainly run into great imperfections, we ought nevertheless also to consider what dangers, on the other, we may incur by lessening them too much.

As soon as convenient, I intend experimentally to pursue this subject, in order to obtain proper data for submitting this cause of optical imperfection to theory; at present my engagement with the work of a 40-feet reflector will hardly permit so much leisure; and till I shall have repeated, extended, and varied these experimental investigations, I would wish them to be considered as mere hints that may afford matter for future disquisitions to the theoretical optician.

END OF THE SEVENTY-SIXTH VOLUME OF THE ORIGINAL.

I. An Account of a New Comet. By Miss Caroline Herschel. Vol. LXXVII. *Anno* 1787. p. 1.

The employment of writing down the observations, when my brother uses the 20-feet reflector, does not often allow me time to look at the heavens; but as he is now on a visit to Germany, I have taken the opportunity of his absence to *sweep* in the neighbourhood of the sun, in search of comets; and last night, Aug. 1, 1786, about 10 o'clock, I found an object very much resembling in colour and brightness the 27th nebula of the *Connoissance des Temps*, with the difference however of being round. I suspected it to be a comet; but a haziness coming on, it was not possible entirely to satisfy myself as to its motion till this evening, Aug. 2. I made several drawings of the stars in the field of view with it, and have inclosed a copy of them, with my observations annexed, that you may compare them together. By the naked eye, the comet is between the

54th and 53d Ursæ majoris, and the 14th, 15th and 16th Comæ Berenices; and makes an oblique triangle with them, the vertex of which is turned towards the south.

Miss Herschel made several observations of this comet, with a Newtonian sweeper of 27 inches focal length, and a power of about 20, the field of view being 2° 12′; which evinced the comet's motion, by its change of position to certain stars.

II. Remarks on the New Comet. By William Herschel, LL. D., F. R. S. p. 4.

As my sister's letter of the 2d of August, relative to the comet discovered by her, has had the honour of being communicated to the R. S., I beg leave to add the following remarks on it. The track of the parallel not being taken at the time of her observations, I have endeavoured to recover it by means of directing the same instrument which was used on this occasion towards that part of the heavens where it was placed the 1st and 2d of August. Hence, from fig. 9, pl. 1, in which AB represents a parallel of declination, we may conclude, that the comet was nearly in the same meridian with the star a; but more north than it by an interval equal to the distance of the small star b from a. This will consequently give us a pretty good opportunity to ascertain the comet's place with some accuracy.

P. S. The first view I had of the comet, after my return from Germany, was the 19th of August, when with a 10-feet reflector it appeared not much unlike the 3d nebula of the Connoissance des Temps, with which it might be very conveniently compared on account of its proximity. It was however considerably brighter, and seemed to have a very imperfect and confused kind of gathered light about the middle, which could hardly deserve the name of a nucleus. It had also, besides a diffused coma, a very faint, scattered light towards the north following part, extending to about 3 or 4′, and losing itself insensibly.

III. Magnetical Experiments and Observations. Being the Lecture founded by the late Henry Baker, Esq., F. R S. By Mr. T. Cavallo, F. R. S. p. 6.

The Bakerian Lecture, which last year I had the honour to deliver to the R. S., contained the account of some magnetical experiments, particularly concerning the magnetism of brass, from which it appeared, that most brass becomes magnetic, so far as to attract the magnetic needle, by being hammered, and loses its magnetism by annealing or softening in the fire; but that there is some brass, which possesses no magnetism naturally, nor acquires any by hammering. Several experiments, made since the reading of that paper, having shown a few particulars, which tend to correct what was advanced in it, I shall in the present lecture mention, first, those particulars, and shall then proceed to notice other experiments and observations relating to other branches of the same subject of magnetism.

In performing the experiments on the magnetism of brass, I generally used a magnetic needle suspended in a peculiar manner, as it is described in my last lecture; viz. a common sewing needle, or a piece of steel wire rendered magnetic, and suspended by means of a chain of hair; which sort of suspension I find not only from the experiments then made, but also by several subsequent trials, to be the nimblest hitherto contrived; because some substances which seem to be quite destitute of magnetism, by not attracting any of the magnetic needles otherwise suspended, will sensibly affect this. However, notwithstanding the nicety of this method for discovering a very low degree of magnetic attraction, it was found still inferior to that of exploring substances floating on the surface of quicksilver, as used by M. Brugman. It seemed therefore necessary to repeat some of those experiments on brass, and also on platina, by examining their magnetism by this means, viz. by putting the pieces of brass or grains of platina on the surface of quicksilver, and then presenting a strong magnet near them. The result of those experiments was, that very seldom a piece of brass, or grain of platina, occurred, which was not affected by the magnet; and even when they were not affected by it, their indifference, as it may be expressed, was not very clear and decisive; and indeed there are very few substances in nature which, when examined by this means, are not in some degree attracted by the magnet, so general is the dispersion of iron, or such is the tendency which most bodies have towards the magnet.

Such brass which in the former experiments appeared to have no magnetism naturally, nor to accquire any by hammering, was now found to be mostly magnetic, though in so very small a degree as to be discoverable only when floating on quicksilver. The same was the case with the grains of platina before they were hammered; but after hammering, their attraction towards the magnet became more evident; whereas those pieces of brass, which naturally had not any degree of magnetism sufficient to affect the needle, nor acquired any by hammering, but yet showed some tendency towards the magnet when floating on quicksilver, never, or very seldom, had that tendency increased by hammering.

As in the course of those experiments it naturally occurred to observe several particulars, which may be of use to those persons who wish to repeat these experiments, I shall now subjoin the principal of them. It is necessary first of all to observe, that the vessel in which the quicksilver is put, for the purpose of examining the magnetism of divers bodies, must be at least 6 inches in diameter; otherwise the substances that are set to float on the mercury, will be continually running towards the sides of the vessel, on account of the curvature of the surface of that metal, which in narrow vessels begins at a greater distance from the edge, than in vessels of a larger diameter.

It is also necessary to keep the quicksilver very clean and very pure; the want of which precautions will render the event of the experiments precarious. I have observed a very remarkable phenomenon, with respect to the surface of the mercury that is used for this purpose. It is, that though substances will move on it with great nimbleness, when the mercury is first poured out of the bottle into the open vessel, yet a short time after, viz. after having remained for an hour or two, and some imes for a shorter time, exposed to the atmosphere, a piece of brass or other substance will by no means move on it with equal facility; so that some pieces of brass, or grains of platina, which, after first pouring the quicksilver into the open vessel, were evidently attracted by the magnet, about an hour after were not in the least attracted by it. The method which, when the surface of the quicksilver is rendered thus sluggish, will effectually purify it, is to pass the quicksilver through a funnel of paper, viz. a piece of clean writing paper rolled up conically, and having at its apex an aperture of about a 50th part of an inch in diameter; which operation is sometimes necessary even on first pouring out the quicksilver, and which I have often been obliged to repeat 3 or 4 times in the course of an hour. There seems to be formed a kind of crust on the surface of the mercury when exposed, which, though invisible by mere inspection, may be perceived by moving the floating body; for if it be tried immediately after having passed the quicksilver through the paper funnel, the floating substance will seem to proceed by itself; whereas, some time after, the same body, when moved, seems to communicate that motion to the adjacent quicksilver, and to drag it along with itself, somewhat like when one moves a body, which floats on the surface of a liquor that begins to coagulate.

The formation of this crust seems to be mostly owing to the imperfect metals, which in various quantities are usually amalgamated with the common sort of quicksilver; because that amalgamation tends to dephlogisticate those metals, and the semi-calcined part floats at the top, and it is not unlikely that the dephlogistication goes on much quicker in the open air. The reality of this supposition is corroborated by observing, that the purer the quicksilver is, the smaller is the crust formed, or opposition made to the floating bodies. However, I have observed it in some measure even in the purest quicksilver that can be procured; and am inclined to think, that it must be partly owing to moisture, or to very fine dust that adheres to the quicksilver when exposed to the atmosphere.

In performing such experiments, care should be had to keep the air and the quicksilver as much undisturbed as possible, and also to present the magnet in a proper manner, viz. so as not to touch the surface of the mercury, nor to strike against the floating body, especially when that is in motion; for after the impulse, though that may be very slight, the floating body will be impelled backwards, which may be often mistaken for magnetic repulsion. The least exceptionable method is the following: first, if the floating body be in motion let it stop, then hold a strong artificial magnet nearly in a perpendicular direction, and with one pole just over one side of the floating body, or rather so that the perpendicular let fall from the pole of the magnet to the surface of the quicksilver, may be about $\frac{1}{10}$ of an inch distant from the body to be tried. The height of the magnet above the quicksilver should be just sufficient to let the floating body pass under it without touching. In this situation the magnet must be held steady; and if the floating body has any magnetism, it will be soon drawn directly under the magnet.

In these experiments it will be generally found, that one part of the floating body is more magnetic than the rest, which appears from that particlar part being constantly drawn directly under the pole of the magnet; whereas, when the magnetism is diffused equably, the centre of gravity of the body, provided its shape be not very irregular, becomes stationary just under the pole of the magnet. It is not every magnet that will discover the very weak magnetism of certain substances; for sometimes a powerful magnet will evidently attract what a weaker one will not move in the least. I shall lastly observe, with respect to the experiments of last year's lecture, that though then I thought to have fused and incorporated together brass and iron, yet some subsequent trials gave reason to believe, that the iron is concealed in some part or other of the melted brass, rather than equably diffused through the substance of the latter; and the principal reason for this suspicion is, that when those pieces of mixed metal are tried on the quicksilver, some points in their surfaces are generally attracted by the magnet in preference to others.

The remainder of this paper may be consulted in the author's Treatise on Magnetism, in 8vo. p. 283, &c. published in 1787.

IV. Description of a New Electrometer. By the Rev. Abraham Bennet, M. A. Dated Wirksworth, Sept. 14, 1786. p. 26.

This electrometer consists of two slips of leaf gold, suspended in a glass. The foot may be made of wood or metal; the cap of metal. The cap is made flat on the top, that plates, books, evaporating water, or other things to be electrified, may be conveniently placed on it. The cap is about an inch wider in diameter than the glass, and its rim about $\frac{3}{4}$ of an inch broad, which hangs parallel to the glass, to turn off the rain and keep it sufficiently insulated. Within this is another circular rim, about half as broad as the other, which is lined with silk or velvet, and fits close on the outside of the glass; thus the cap fits well, and may be easily taken off to repair any accident happening to the leaf gold. Within this rim is a tin tube, hanging from the centre of the cap, somewhat longer than the depth of the inner rim. In the tube a small peg is placed, and may be occasionally taken out. To the peg, which is made round at one end and flat at the other, 2 slips of leaf gold are fastened with paste, gum-water, or varnish. These slips, suspended by the peg, and that in the tube fast to the centre of the cap, hang in the middle of the glass, about 3 inches long, and a quarter of an inch broad. In one side of the cap there is a small tube, to place wires in. It is evident, that without the glass the leaf gold would be so agitated by the least motion of the air, that it would be useless; and if the electricity should be communicated to the surface of the glass, it would interfere with the repulsion of the leaf gold; therefore two long pieces of tin-foil are fastened with varnish on opposite sides of the internal surface of the glass, where the leaf gold may be expected to strike, and in connexion with the foot. The upper end of the glass is covered and lined with sealing-wax as low as the outermost rim, to make its insulation more perfect.

The following experiments will show the sensibility of this instrument.

1st. Powdered chalk was put into a pair of bellows, and blown on the cap, which electrified it positively when the cap was about the distance of 6 inches from the nozzle of the bellows; but the same stream of powdered chalk electrified it negatively at the distance of 3 feet. In this experiment there is a change of electricity from positive to negative, by the dispersion or wider diffusion of the powder in the air. It is also changed by placing a bunch of fine wire, silk, or feathers, in the nozzle of the bellows, and is wholly negative when blown from a pair of bellows without their iron pipe, so as to come out in a larger stream; this last experiment did not answer in dry weather so well as in wet. The positive electricity of the chalk, thus blown, is communicated because part of the powder sticks to the cap; but the negative is not communicated, the leaf gold collapsing as soon as the cloud of chalk is dispersed.

2dly, A piece of chalk drawn over a brush, or powdered chalk put into the brush, and projected on the cap, electrifies it negatively; but its electricity is not communicated.

3dly, Powdered chalk blown with the mouth or bellows from a metal plate placed on the cap, electrifies it permanently positive. Or if the chalk is blown from the plate, either insulated or not, so that the powder may pass over the cap, if not too far off, it is also positive. Or if a brush is placed on the cap, and a piece of chalk drawn over it, when the hand is withdrawn the leaf gold gradually opens with positive electricity as the cloud of chalk disperses.

4thly, Powdered chalk falling, from one plate to another, placed on the instrument, electrifies it negatively.

Other methods of producing electricity with chalk and other powders have been tried; as projecting chalk from a goose wing, chalking the edges of books and clapping the book suddenly together, also sifting the powder on the cap; all which electrified it negatively: but the instrument being placed in a dusty road, and the dust struck up with a stick near it, electrified it positively. Breaking the glass tear on a book electrified it negatively, probably by friction in the act of shivering, for when broken in water it did not electrify it. Wheat flour, and red-lead, are strongly negative in all cases where the chalk is positive. The following powders were like chalk: red and yellow ochre, rosin, coal ashes, powdered crocus metallorum, aurum mosaicum, black-lead, lampblack, (which was only sensible in the first two methods), powdered quick-lime, umber, lapis calaminaris, Spanish brown, powdered sulphur, flowers of sulphur, iron filings, rust of iron, sand. Rosin and chalk, separately alike, were changed by mixture; this was often tried in dry weather, but did not succeed in damp: white-lead also sometimes produced positive, and sometimes negative, when blown from a plate.

If a metal cup be placed on the cap, with a red-hot coal in it, a spoonful of water thrown in electrifies the cup negatively; and if a bent wire be placed in the cap, with a piece of paper fastened to it to increase its surface, the positive electricity of the ascending vapour may be tried by introducing the paper into it. Perhaps the electrification of fogs and rain is well illustrated by pouring water through an insulated cullender, containing hot coals, where the ascending vapour is positive, and falling drops negative.

The sensibility of this electrometer may be considerably increased by placing a candle on the cap. By this means a cloud of chalk, which only just opens the leaf gold, will cause it to strike the sides for a long time together; and the electricity, which was not before communicated, now passes into the electrometer, causing the leaf gold to repel, after it is carried away. Even sealing-wax by this means communicates its fire at the distance of 12 inches at least, which it would scarcely otherwise do by rubbing on the cap. A cloud of chalk or wheat flour may be made in one room, and the electrometer, with its candle, be afterwards leisurely brought from another room, and the cloud will electrify it before it comes very near. The air of a room adjoining to that in which the electrical machine was used, was very sensibly electrified, which was perceived by carrying the instrument through it with its candle.

In very clear weather, when no clouds were visible, the electrometer has been often applied to the insulated string of kites without metal, and their positive electricity caused the leaf gold to strike the sides; but when a kite was raised in cloudy weather, with a wire in the string, and when it gave sparks about a quarter of an inch long, the electricity was sensible by the electrometer at the distance of 10 yards or more from the string; but when placed at the distance of 6 feet, the leaf gold continued to strike the sides of the electrometer, for more than an hour together, with a velocity increasing and decreasing with the density or distance of the unequal clouds which passed over. Sometimes the electricity of an approaching cloud has been sensible without a kite, though in a very unfavourable situation for it, being in a town surrounded with hills, and where buildings encompassed the wall on which the electrometer was placed. A thunder cloud passing over, caused the leaf gold to strike the sides of the glass very quick at each flash of lightning. No sensible electricity is produced by blowing pure air, projecting water, by smoke, flame, or explosions of gunpowder. The quantity of electricity necessary to cause a repulsion of the leaf gold is so small, that the sharpest point or edges do not draw it off without touching: hence it is unnecessary to avoid points or edges in the construction of this instrument.

V. Appendix to the Description of a New Electrometer. By the Rev. Abraham Bennet, M. A. p. 32.

This is a somewhat more particular account of the description of Mr. Bennet's electrometer; which however, with an account of experiments, may be profitably consulted in the author's "New Experiments on Electricity," printed at Derby, in 8vo. 1789.

To the experiments on blowing powders from a pair of bellows Mr. B. adds, that if the powder is blown at about the distance of 3 inches on a plate which is moistened or oiled, its electricity is contrary to that produced by blowing on a dry plate. This shows that the electricity of the streams of powder issuing out of the bellows is only contrary to the more expanded part, because it is within the influence of its atmosphere; for when this is destroyed by the adhesion of the powder to the moistened plate, it is negative when the bellows are positive, as it was before positive when the more expanded cloud was negative. He also adds, that the experiments on evaporation of water may be tried with more ease and certainty of success by heating the small end of a tobacco-pipe, and pouring water into the head, which, running down to the heated part, is suddenly expanded, and will show its electricity when projected on the cap of the electrometer, more sensibly than any other way he had tried. If the pipe be fixed in a cloven stick, and placed in the cap of one electrometer, while the steam is projected on another, it produces both electricities at once. Spirit of wine and ether are electrified like water. Oil and vitriolic acid produced smoke without any change of electricity. In these experiments a long pipe is better than a short one.

VI. Some Account of an Earthquake felt in the Northern Part of England. By Samuel More, Esq. Dated Castle-Head, Lancashire, Aug. 22, 1786. p. 35.

This shock of an earthquake was felt in Cumberland and the neighbouring counties, on Friday, Aug. 11, this year, about 2 o'clock in the morning.

On Mr. M.'s arrival at Penrith in the evening, every one there spoke of it as having been sensibly felt in that town. The next day, pursuing his journey, he was informed it had been felt along the banks of Ulswater, in Patterdale, at Ambleside, along the side of Winander Meer, and particularly at the house in the island on that lake, the property of Mr. Christian. At Castle-Head, the lady of the house, and some of the servants, were awakened by it, and describe it as shaking violently the beds, the chairs in the rooms, and the sashes of the windows. At Cartmeal, a town about 5 miles from hence, it was also felt very severely; and at the village of Carke, 2 miles from Cartmeal, a gentleman states that he was awake some time before the shock; that he first heard a rumbling noise, like a carriage at a distance, and was considering what carriage could be moving at that hour, when he felt the shock. The noise continued some time after the shock was over; and he thinks the whole might last about 4 or 5 seconds, and it seemed to travel from the east to the westward. Almost every body in the neighbourhood of Carke and Cartmeal were awakened by it, and some persons much alarmed. At Lancaster, about 10 miles east of Cartmeal, it was very plainly felt, particularly in the great tower of the castle. It appears to have extended as far as Manchester, where it was slightly perceived.

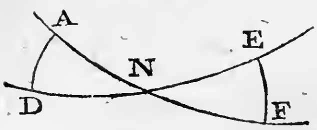
VII. Determination of the Heliocentric Longitude of the Descending Node of Saturn. By Thomas Bugge. p. 37.

The culmination of Saturn was observed with a 6-feet achromatic transit-instrument, and the planet compared with o and π of Sagittarius, whose apparent right-ascensions in the middle of August 1784 were 282° 56′ 54″ and 284° 14′ 33″. The meridian altitude was observed with a 6-feet mural quadrant. From the observations are calculated the right-ascension and declination, the geocentric longitude and latitude, of Saturn, which are to be depended on to 4 or 6 seconds. Those observed longitudes and latitudes are compared with the tables of Dr. Halley and of M. de la Lande. In the errors of the tables + signifies that the longitude of the tables is less than the observed longitude; and the meaning of — is, that the calculated longitude is greater than the observed. It ought to be observed, that the heliocentric longitudes of Dr. Halley's tables have been corrected for the perturbations after the principles of M. Lambert (Memoires de Berlin, pour 1783, p. 216, and Collection des Tables Astronomiques de Berlin, tom. 2, p. 269.)

1784.	♄ culmination, mean time at Copen.	♄ observed longitude.	♄ observed latitude.	The error of Halley.		The error of M. de la Lande.	
				in long.	in lat.	in long.	in lat.
	h. m. s.	s. ° ′ ″	° ′ ″	′ ″	″	′ ″	″
July 12	12 3 1	9 20 34 48	0 3 35 B	+2 22	+32	−9 40	+33
20	11 29 9	9 19 59 39	0 2 59	+2 14	+38		
Aug. 1	10 38 25	9 19 9 22	0 1 44	+1 45	+27	−9 49	+28
8	10 9 0	9 18 42 56	0 1 2	+1 48	+21		
21	9 14 59	9 18 1 23	0 0 2	+1 35	+28		
27	8 50 19	9 17 46 19	0 0 29 A	+1 26	−26	−9 35	−30
31	8 33 47	9 17 38 7	0 0 53	+1 19	−23		
Sept. 5	8 13 45	9 17 29 36	0 1 26	+1 20	−25	−9 25	−27
15	7 33 45	9 17 19 39	0 2 4	+1 18	−25		
Oct. 8	6 4 23	9 17 34 6	0 3 57	+1 22	−26	−8 48	−32

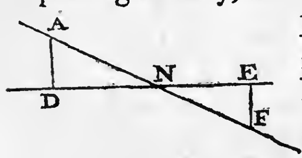
In order to reduce the observed geocentric longitude to the sun, or by observation to find the heliocentric longitude, it is required to know the angle at the planet $= p$. If this angle be calculated in the common way only by the tables, there will arise some difference, according to the different elements and the different constructions of the tables. Thus, at the time of Saturn's culmination, this angle is found the 12th of July, by the tables of Dr. Halley $= 0° 3' 13''$, and by the tables of M. de la Lande $= 0° 2' 0''$; the 8th of August by Halley. $= 2° 43' 35''$, and by M. de la Lande $= 2° 42' 34''$; the 27th of August after Dr. Halley $= 4° 14' 15''$, and after M. de la Lande $= 4° 13' 47''$. To avoid those differences, which often may alter the heliocentric longitude more than 1 or 2′, the following method may be useful. The heliocentric longitude of the earth, calculated after the tables of M. Mayer, is to be depended on to 8 or 10″. From the heliocentric longitude of the earth, and from the observed geocentric longitude of the planet, corrected for the aberration and nutation, is deduced the angle at the earth $= t$, or the distance between the sun and the planet seen from the earth. The dimensions of the elliptic orbit of the planet are so far ascertained, that the logarithms of the distance from the sun have not any material difference in the different tables. From the angle t, the distance of the earth from the sun, and the distance of the planet from the sun, the angle p is calculated to a sufficient degree of accuracy. Thus, the 12th of July, by the distances of Dr. Halley, $p = 0° 2' 59''$, and by the distances of M. de la Lande $= 0° 2' 59''$; the 8th of August after Dr. Halley $p = 2° 43' 25''$, and after M. de la Lande $p = 2° 43' 36''$; the 27th of August after Dr. Halley $p = 4° 14' 10''$, and after M. de la Lande $p = 4° 14' 28''$. The difference very seldom will amount to 20 seconds, and is of no consequence in this matter. From the observed geocentric latitude of the angle at the sun $= s$, and the angle at the earth $= t$, the heliocentric latitude of the planet is found $= \frac{\text{tang. geoc. lat.} \times \text{sin. } s}{\text{sin. } t}$.

1784.	Mean time at Copenhagen.			♄ observed heliocentric long.				♄ observed heliocentric lat.			
	h.	m.	s.	s.	°	′	″	°	′	″	
July 12	12	3	1	9	20	37	29	0	3	13	B
20	11	29	9	9	20	51	53	0	2	41	
Aug. 1	10	38	25	9	21	13	17	0	1	34	
8	10	9	0	9	21	26	2	0	0	56	
21	9	14	59	9	21	49	27	0	0	2	
27	8	50	19	9	22	0	12	0	0	27	A
31	8	33	47	9	22	7	32	0	0	50	
Sept. 5	8	13	45	9	22	16	28	0	1	21	
15	7	33	45	9	22	34	32	0	1	59	
Oct. 8	6	4	23	9	23	16	15	0	3	35	

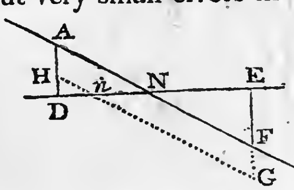


When two heliocentric longitudes, and the corresponding northern and southern latitude are given, the distance of the node from one of the longitudes or places may be found. Let DE be the ecliptic, AF the orbit of the planet, N the node, DE the difference between the two observed heliocentric longitudes $= a$, EF the southern latitude $= \beta$, AD the northern latitude $= b$, NE the distance of the node from the heliocentric place at E, and corresponding to the southern latitude $= x$. In the spherical triangles ADN and FEN, $\frac{\text{sin. } (a - x)}{\text{tang. } b} = \text{cot. N} = \frac{\text{sin. } x}{\text{tang. } \beta}$. By placing the value of sin. $(a-x)$ in the equation $\frac{\text{sin. } a. \text{ cos. } x - \text{sin. } x \text{ . cos. } a}{\text{tang. } b} = \frac{\text{sin. } x}{\text{tang. } \beta}$. By resolving this equation $\frac{\text{sin. } x}{\text{cos. } x} = \text{tang. } x = \frac{\text{sin. } a \text{ . tang. } \beta}{\text{tang. } b + \text{cos. } a \text{ . tang. } \beta}$.

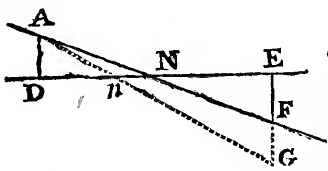
If a, b, and β, be very small arcs, which commonly is the case with the planets, then sin. $a = a$, tang. $\beta = \beta$, tang. $b = b$, and cos. $a = 1$. Hence the spherical formula will be transformed into another $x = \frac{a\beta}{b + \beta}$. This formula belongs to plane geometry, and may besides be thus demonstrated. DN : NE = AD : EF.



Hence DN + NE : NE = AD + EF : EF; and NE $= \frac{\text{DE} \times \text{EF}}{\text{AD} + \text{EF}}$. If the difference of the longitudes do not exceed 1°, and the latitudes not greater than 10′, the spherical and the rectilineal formula will agree to a very few seconds. Small faults in the longitude will not very much alter the true place of the node; but very small errors in the latitude are of great consequence. Let the error in



the southern heliocentric latitude be FG $= + d$. The error in the northern latitude AH $= - d$. Hence DH : Dn = GE : En, and En $= \frac{a\,(\beta + d)}{b + \beta}$. By subtracting EN $= \frac{a\beta}{b + \beta}$, the error in the heliocentric longitude of the node, $Nn = \frac{ad}{b+\beta}$. If the fault in the southern latitude $= -d$, in the northern latitude $= +d$, the same formula is still true; but then $EN > En$, and the place of the erroneous node will be between E and N. In both cases the errors in the place of the node are directly as the errors in the latitudes.



Let us now suppose, that only the one latitude is erroneous $\pm d$. Then $Nn = \pm En \mp EN = a \times \left(\pm \frac{\beta \pm d}{b+\beta \pm d} \mp \frac{\beta}{b+\beta}\right) = \frac{abd}{(b+\beta)^2 \pm d(b+\beta)}$. In the case when the error in both latitudes is positive $= +d$, and $\beta > b$, or $\beta < b$, the resulting error in the place of the node $= \frac{ad(\mp b \pm \beta)}{(b+\beta)^2 + 2d(b+\beta)}$. In the case when the error in both latitudes is negative $-d$, and $\beta > b$, or $\beta < b$, then the error in the node $= \frac{ad(\mp b \pm \beta)}{(b+\beta)^2 - 2d(b+\beta)}$. In those two cases the error is less than in any of the former, and quite nothing when $b = \beta$. If the radius of the instrument, with which the meridian altitudes are observed, be given, the quantity of d is also given. In a mural quadrant of 6 or 8 feet, $d = 5$ or 3 seconds. Take $a = 34' \, 3''$, $b = 56''$, $\beta = 27''$, $d = 5''$, and the error in the southern latitude $+ d$, in the northern $= -d$; then $Nn = \frac{10215''}{83} = 2' \, 12''$. Take now the error only in the southern latitude $= +d$; then $Nn = \frac{572040''}{7304} = 1' \, 18''$; in the case of $-d$; $Nn = \frac{572040''}{6474} = 1' \, 28''$. Hence it appears, that in comparing two single observations, it will hardly be possible to avoid a fault of $\pm$ 2 minutes in the place of the node.

If the instrument be of a less force than a mural quadrant of 6 feet, and the possible faults in the altitudes greater, for example, 10 or 15″, the resulting error in the place of the node may very easily be calculated; but the error in the node will be enormous, and the observations of no use for a nice astronomer.

Compared Observations 1784.	♄ heliocentric longitude on the last day.				♄ distance from the ☊.			Heliocentric long. of ♄ ☊.			
	s.	°	′	″	°	′	″	s.	°	′	″
July 12 with Sept. 15	9	22	34	32	0	44	38	9	21	49	54
July 12 — Oct. 8	9	23	16	15	1	23	41	9	21	52	36
July 20 — Sept. 15	9	22	34	32	0	43	27	9	21	50	55
July 20 — Oct. 8	9	23	16	15	1	22	33	9	21	53	42
Aug. 1 — Sept. 5	9	22	16	28	0	29	15	9	21	47	13
Aug. 1 — Sept. 15	9	22	34	32	0	45	23	9	21	49	9
Aug. 1 — Oct. 8	9	23	16	15	1	25	34	9	21	50	41
Aug. 8 — Aug. 27	9	22	0	12	0	11	7	9	21	49	5
Aug. 8 — Aug. 31	9	22	7	32	0	19	34	9	21	47	58
Aug. 21 — Aug. 27	9	22	0	12	0	10	0	9	21	50	12
Aug. 21 — Aug. 31	9	22	7	32	0	17	23	9	21	50	9
					Mean			9	21	50	8.5

This mean agrees pretty well with the observations on the 21st, 27th, and 31st of August, which are nearest the node, and most to be depended on. Aug. 21, at $9^h\ 12^m\ 26^s$ true time at Copenhagen, the heliocentric longitude of Saturn $= 9^s\ 21^\circ\ 49'\ 27''$, and the distance from the node $= 41''$. Aug. 27, at $8^h\ 49^m\ 23^s$, the heliocentric longitude $= 9^s\ 22^\circ\ 0'\ 12''$; therefore, in $5^d\ 23^h\ 36^m\ 57^s$ Saturn has described an arc of $10'\ 45''$, and $10'\ 45'' : 5^d\ 23^h\ 36^m\ 57^s = 41'' : x$. Hence Saturn has spent $9^h\ 7^m\ 44^s$ in going through those $41''$; and Saturn's passage through the node happened August 21, 1784, at $18^h\ 20^m\ 10^s$, and the heliocentric longitude of his descending node $= 9^s\ 21^\circ\ 50'\ 8''.5$. The errors in the place of the node are relative to the tables of Dr. Halley $+ 19'\ 39''$, to the tables of M. Cassini $+ 16'\ 4''$, and to the tables of M. de la Lande $+ 1'\ 31''$.

In the foregoing computation of Saturn's heliocentric longitude from the tables of Dr. Halley, this longitude has been corrected for the perturbation after the principles of M. Lambert. Though the geocentric places, calculated in this manner, will agree still better with the observations than without those perturbations, yet they are only empiric, and not founded on the theory and principles of gravitation; I shall therefore conclude this paper, by adding the faults in the heliocentric places of Saturn, calculated only and directly from the tables of Dr. Halley, which may be of some use to improve those valuable tables,

1784.		♄ heliocentric longitude from Dr. Halley's tables.				Error in longitude.		♄ heliocentric latitude from Dr. Halley's tables.				Error in latitude.
		s.	°	′	″	′	″	°	′	″		″
July	12	9	20	27	40	+9	49	0	2	45	N	
	20	9	20	42	3	+9	50	0	2	17		+28
Aug.	1	9	21	3	41	+9	36	0	1	10		+24
	8	9	21	16	19	+9	43	0	0	37		+21
	21	9	21	39	45	+9	42	0	0	24	S	−22
	27	9	21	50	34	+9	38	0	0	53		−26
	31	9	21	57	47	+9	45	0	1	11		−21
Sept.	5	9	22	6	48	+9	40	0	1	45		−24
	15	9	22	24	50	+9	42	0	2	22		−23
Oct.	8	9	23	6	33	+9	44	0	4	1		−26

VIII. Description of a Set of Halos and Parhelia, seen in the Year 1771, *in North America. By Alex. Baxter, Esq.* p. 44.

Extract from a journal kept in the upper countries of North-America. At Fort Gloucester, on the river of Lake Superior, six miles above the falls of St. Mary's, and as much from the head of the river, where it issues from the Lake. "January 22, 1771. Last night and to-day the frost has been more severe than at any time this winter: I was hardly able, at mid-day, to keep my face to the wind uncovered, though the sun shone very bright, and the sky clear. In the morning the wind was easterly, which went about with the sun to the south and westward, returning to the east in the evening; a very small breeze. A little before 2 o'clock P. M. observed as follows. There was a very large circle or halo round the sun, within which the sky was thick and dusky, the rest of the hemisphere being clear; and, a little more than $\frac{1}{3}$ way from the horizon to the zenith, was a beautifully enlightened circle, parallel to the horizon, which went quite round, till the two ends of it terminated in the circle that surrounded the sun; where, at the points of intersection, they each formed a luminous appearance about the size of the sun, and so like him when seen through a thick hazy sky, that they might very easily have been taken for him. Directly opposite to the sun was a luminous cross, in the shape of a St. Andrew's Cross, cutting at the point of intersection the horizontal circle, where was formed another mock sun, like the other two mentioned above. The two lower limbs of the cross appeared but faintly a little way below the circle, the two higher reached a good way above the circle towards the zenith very clear and bright. In this horizontal circle, directly half-way between the sun of the cross and those at the ends of the same circle, were other two mock suns, of the same kind and size, one on each side; so that in this horizontal circle were 5 mock suns, at equal distances from each other, and in the same line the real sun, all at equal heights from the horizon. Besides these meteors, there was, very near the zenith, but a little more towards the circle of the real sun, a rainbow of very bright and beautiful colours, not an entire semi-circle, with the middle of the convex side turned towards the sun, which lowered as the sun descended. This phenomenon continued in all its beauty and lustre till about half after 2. The cross went gradually off first; then the horizontal circle began to disappear in parts, while in others it was visible; then the 3 mock suns farthest from the sun, the 2 in the sun's circle continuing longest; the rainbow began to decrease after these; and last of all the sun's circle, but it was observable at 3 o'clock, or after it."

IX. Observations of the Transit of Mercury, May 4, 1786, at Dresden. By M. Kohler, Inspector of the Mathematical Repository of the Elector of Saxony. p. 47.

Apparent time.
9^h 21^m 54^s beginning of the planet's egress, doubtful.
9 25 23 complete egress, or last contact, very certain.

The telescope with which Mr. Köhler observed, was a 9-feet refractor of Dollond's, magnifying 104 times. He made a comparison of his observation with that of Alexander Aubert, Esq. from which he inferred the longitude of Dresden to be 54^m $30^s.9$ east of Greenwich.

X. Observations of the Transit of Mercury at St. Petersburg. By M. Rumovski, Astronomer in the Imperial Academy. From the French. p. 48.

The first internal contact $17^h\ 2^m\ 19^s$ true time.
The last internal contact 22 26 55 very exact.

During the passage, Mr. R. measured, with an objective micrometer, some distances of the limbs, and the diameter of Mercury, which was sometimes $7''.56$, and sometimes $8''.64$, so that he takes the mean at $8''.2$ or $8''.4$. Supposing the sun's semidiameter $15'\ 52''$, and his parallax $8''.5$, by 28 combinations he found the

Time of the middle	19^h	44^m	37^s.	
Time of the conjunction	19	13	33.	
Geocentric longitude..................	1^s	$13°$	$50'$	$1''$.
Longitude of the node................	1	15	53	56.
Geocentric latitude	0	0	11	43.
Least central distance	0	0	11	32.

XI. On the Strata observed in Sinking for Water at Boston, Lincolnshire. By Mr. James Limbird, Surveyor to the Corporation. p. 50.

May 7, 1783, George Naylor, of Louth, well-borer, began to bore at the well in the Market-Place, Boston; which had been sunk and bored to the depth of 186 feet from the surface, in 1747, by Thomas Partridge. The well was made about 6 feet in diameter at the top, 5 feet at the bottom, and 27 feet deep, and the earth prevented from falling in by a circular frame of wood, which goes from the surface of the earth to the depth of 21 feet 6 inches, and is there supported by brick-work, laid on a bed of light-coloured blue clay, which continues to the depth of 36 feet from the surface, where is a bed of sand and gravel about 18 inches thick, and under it the same sort of blue clay as before, which continues to the depth of 48 feet from the surface. Below this is a bed of dark-coloured stone, like rag stone, about 6 inches thick, from under which George Naylor says, that a salt spring issues. Beneath this layer of stone, is a bed of dark-blue clay, which continues to the depth of 75 feet from the surface, where is a bed of stone, of a lightish colour, about 6 inches thick, and under it a bed of dark-blue clay, which continues to the depth of 114 feet from the surface, where is a bed of stone, of a brightish colour, about 8 inches thick, and under it a bed of gravel, about 6 inches thick, where George Naylor says there is another salt spring. Under the gravel is a bed of dark-coloured clay, resembling black-lead, which continues to the depth of 174 feet from the surface, when it changes to a chalky clay, intermixed with small pebbles and flints, which continues about 3 inches, and then changes to the same kind of dark-coloured clay as before; in which, after boring to the depth of 186 feet from the surface, he came to the solid earth bored to, in 1747, by the above-mentioned Thomas Partridge. After boring in the same kind of clay to the depth of 210 feet from the surface, it changes to a lighter-coloured one, which continues about 6 inches, and then changes dark again, and continues so to the depth of 342 feet from the surface, where is a bed of shells and white coloured earth, about half an inch thick, and under it a light-coloured earth like that at 210 feet from the surface, and under it a bed of dark-coloured clay. After continuing in that clay to the depth of 444 feet from the surface, George Naylor put down a tin pipe, 50 yards in length, and 2¼ inches in diameter within, to prevent the gravel and stones from falling down and obstructing the rods; but being too weak for that purpose, it separated into different lengths, and entirely prevented his boring, so that he was obliged to get the said pipes up again, which took him 48 days; having got them up, and cleared the hole pretty well, he left off boring till he could procure some stronger pipes.

July, 1784, he put down 21 pipes of cast iron, each pipe being 2¼ inches in diameter within, half an inch thick, and on an average 6 feet 1 inch in length; they were fixed together with boxes and screws, and with a piece of soft leather between the top of each box and screw, to prevent them from breaking; the uppermost pipe is fastened to a plank, which lies on the top of the brick-work.

At the distance of 447 feet from the surface there is a bed of dark-coloured earth mixed with chalk and gravel, which continues to the depth of 449 feet 10 inches from the surface, where is a bed of dark-coloured earth without any chalk and with very little gravel, which continues to the depth of 454 feet 7 inches from the surface; there it changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 456 feet 8 inches from the surface, and then changes to a dark-coloured earth without any chalk, and with very little gravel, which continues to the depth of 457 feet from the surface, and then changes to a lighter colour; and this continues to the depth of 462 feet and 4 inches from the surface, where it changes to a darker colour, and so continues to the depth of 470 feet 3 inches from the surface. Here the ground changes to a dark-coloured earth, mixed with chalk and gravel, which continues to the depth of 470 feet 7 inches from the surface, where he came to a bed of stone, like rag-stone, about 13 inches thick, which ground into powder with the wimble, and mixed with the earth. Under this bed of stone is a dark-coloured earth, without any chalk, and with but little gravel, which continues to the depth of 472 feet from the surface, when it changes something lighter, and continues so about 2 inches, where the earth appears to be mixed with chalk and gravel, and continues so for about 1 inch, when it changes to a black silt, having a great deal of light-coloured sand in it.

Sept. 6, 1785, George Naylor broke one of the screws belonging to his rods just above the top of the box, at the distance of between 92 and 93 yards from the surface; when the upper rod, having a circular head or ring 2 inches in diameter at the top, dropped down 40 yards through the iron pipes; which rods were got up again Sept. 15, by a spring. After trying several instruments to get up the lower part of the rods, to no effect, on Oct. 3 following was contrived a spiral instrument, about 2 feet long, with a catch at the top of it, to take the bottom of the uppermost box of the rods that were down; but the top of the rods having fallen several inches from the perpendicular, prevented the instrument from taking them between the 1st and 2d boxes: therefore the surveyor to the corporation and George Naylor, on Oct. 7, contrived a spiral instrument, about 2 feet long, without any catch at the top, which George Naylor put down about 10 yards below the upper box, and there taking hold of the rods, raked them up to the top, and by that means brought them perpendicular, when he left them, and on Oct 8 put down the instrument invented before; by which he got hold of the rods a little below the top box, and brought them up. When the rods broke, George Naylor was boring in a dark-coloured silt, intermixed with chalk and gravel, at the distance of 474 feet from the surface, which continued to the depth of 475 feet 5 inches, when it changed to dark-coloured wet silt without any chalk, in which George Naylor bored to the depth of 478 feet $8\frac{1}{2}$ inches from the surface. Here he imagined, by the easy turning of the wimble, that he had got into a spring of water, and gave over boring, to see if the water would rise in the pipes; when, after keeping the water in the well below the top of the pipes for several days by pumping, the water in the pipes was found to rise about 5 feet per day on an average; which only producing about 7 pints, it was supposed there was no spring of water bored into, but that the rise of water in the pipes was occasioned by the soccage only. On Monday, Nov. 28, an iron bucket was made and affixed to the bottom of the rods, and let down the pipes, and filled with water at the depth of 85 yards from the surface; which water was salt, and of a reddish colour. The bucket was again let down, and filled at the depth of 156 yards from the surface; that water was more salt than the first, and much of the same colour.

The committee appointed by the corporation for superintending the business of sinking for water, having taken the whole of these circumstances into their consideration, and examined George Naylor, who did not account, in a manner satisfactory to them, for the slow progress he had lately made in boring, were of opinion, that it would be proper for the present to discontinue all operations in the well; they therefore directed the stage to be taken up, the mouth of the iron pipes to be carefully plugged up, the well to be covered with oak plank, and the ground over it to be paved as before; all which was accordingly done.

Dated Boston, Nov. 28, 1786. JAMES LIMBIRD,

Surveyor to the Corporation.

XII. Observations of Miss Herschel's Comet, made at Chislehurst, in August and September, 1786. *By the Rev. Francis Wollaston, LL. B., F. R. S.* p. 55.

The comet of August last, having afforded Mr. W. an opportunity of putting to some test the system of wires, a description of which he had laid before the R. S.,* he thought it might not be improper, as a sequel to that paper, to give an account of the observations made with it on this occasion; which will serve to show what dependence may be had on observations made with such an instrument. The telescope to which he applied it was an achromatic object-glass of Dollond, of 16 inches focal length, and 2 inches aperture, with a Ramsden's eye-glass, magnifying about 25 times, mounted on a very firm equatorial stand: with this, which takes in 2° of a great circle, he compared the times of the comet and such stars as lay convenient, as they severally passed the centre wire and other adjoining wires; making occasionally a diagram, or drawing of their appearance in that telescope.

In this list of observations are set down the times, with the neighbouring stars, and the differences of right ascension and declination. During the whole time, the comet was invisible to the naked eye, and without any tail. Its appearance was so very similar to the nebula N° 3 in Messier's Catalogue inserted in the Connoissance des Temps for 1784, and some other years, as scarcely to be distinguished from it when in the telescope together; though it certainly had a brighter spot in the centre.

XIII. Of a Thunder-Storm in Scotland; with some Meteorological Observations. By Patrick Brydone, Esq., F. R. S. Dated Lennel-House, near Coldstream, December 20, 1786. p. 61.

Tuesday, July 19, 1785, was a fine soft morning, thermometer at ten, 68°; about 11, clouds began to form in the south-east; and between 12 and 1 there were several flashes of lightning, followed by rolling claps of thunder, at a considerable distance. Soon after however Mr. B. was suddenly alarmed by a loud report, for which he was not prepared by any preceding flash: it resembled the firing of several muskets, so close together, that the ear could hardly separate the sounds; and was followed by no rumbling noise like the other claps. Soon after he was told that a man and 2 horses had been struck dead by the thunder, at a small distance from his house. Mr. B. immediately set out, and arrived on the spot in less than half an hour after the accident. The horses were still yoked to the cart, and lying in the same position in which they had been struck down; but the body of the young man had been already carried off

* See vol. 75, p. 346.

by his companion, who soon returned to the place; and described to him how every thing had passed.

They were both servants to Mr. Turnbull, a tenant of the Earl of Home; and were returning home with 2 carts loaded with coals. James Lauder had the charge of the first cart, and was sitting on the fore-part of it. They had crossed the Tweed a few minutes before, at a deep ford, and had almost gained the highest part of an ascent above 65 or 70 feet above the bed of the river. At that instant he was stunned by a loud report, and saw his companion, his horses and cart, fall to the ground. He immediately ran to his assistance, but found him quite dead. His face, he said, was of a livid colour, his clothes were torn to pieces, and he had a strong smell of burning. He immediately emptied his own cart, and carried home Lauder's body to his friends; so that I had not an opportunity of examining it: but Mr. Bell, minister of Coldstream, a gentleman of the most perfect candour and veracity, said, that he had been sent for, to announce the fatal event to the young man's parents, and had examined the body; that he found the skin of the right thigh much burnt and shrivelled, and many marks of the same kind over the whole body; but none on the legs, which he imputed to their hanging over the fore-part of the cart at the time of the explosion, and not being in contact with any part of it. His clothes, and particularly his shirt, was very much torn, and emitted a strong smell of burning. The body was buried 2 days after, without having discovered any symptoms of putrefaction.

Lauder's companion showed the distance between the 2 carts, which was exactly marked; for his horses had turned round at the time of the explosion, and broke their harness; it was about 24 yards, and Lauder's cart was a few feet higher on the bank, but had not yet reached the summit. Mr. B. now examined the cart, and the spot around it. The horses were black, and of a strong make; they had fallen on the left side, and their legs had made a deep impression in the dust, which, on lifting them up, showed the exact form of each leg; so that no kind of struggle or convulsive motion had succeeded the fall, but every principle of life seems to have been extinguished in an instant. The hair was much singed over the greatest part of their bodies; but was most perceptible on the belly and legs. Their eyes were already become dull and opaque, and looked like the eyes of an animal which had been long dead. The joints were all supple; and he could not perceive that any of the bones were either softened or dissolved, as it has been alledged sometimes happens to animals killed by lightning. The left shaft of the cart was broken; and the splinters had been thrown off in many places, particularly where the timber of the cart was connected by nails, or cramps of iron. Many pieces of the coal were likewise thrown out to a considerable distance, all round the cart; and some of them had the appearance of coal which had lain some time on a fire. He also gathered up the fragments of Lauder's hat, which had been torn to innumerable small pieces; as well as part of his hair, which was strongly united to some of the fragments which had composed the crown of the hat. About $4\frac{1}{2}$ feet behind each wheel of the cart, was an odd appearance in the ground; a circular hole of about 20 inches in diameter the centre of which was exactly in the tract of each wheel. The earth was torn up, as if by violent blows of a pick-axe, and the small stones and dust were scattered on each side of the road. The tracks of the wheels were strongly marked in the dust, both behind and before these holes, but were completely obliterated for upwards of a foot and a half on these spots. This led Mr. B. to suspect, that the force which had formed them must likewise have acted strongly on the wheels; and, on examination, he found evident marks of fusion on each of them. The surface of the iron, to the length of about 3 inches, and the whole breadth of the wheel, had become of a bluish colour, had entirely lost its polish and smoothness, and had the appearance of drops incompletely formed on its surface; these were of a roundish form, and had a sensible projection. To ascertain whether these marks were occasioned by the explosion which had turned up the ground, he pushed back the cart on the same tracks which it had described on the road; and found, that the marks of fusion answered exactly to the centre of each of the holes; and that, at the instant of the explosion, the iron of the wheels had been sunk deep in the dust. They had made almost half a revolution after the explosion, which might be occasioned by the falling down of the horses, which pulled the cart a little forward. On examining the opposite part of the wheels, or that part which was at the greatest distance from the earth no mark of any kind was observable. The broken earth still emitted a smell something like that of ether. The ground was remarkably dry, and of a gravelly soil.

It would appear, that this great explosion had, in an instant, pervaded every substance connected with the cart, the wheels of which had probably conducted it from the ground. They had been completely wetted but a few minutes before, as well as the legs and bellies of the horses, and might perhaps be the reason why the hair on these parts was much more burnt than on the rest of their bodies. However, the two horses had already walked over this electrical mine, without having produced any effect; and had not the cart followed them might have escaped without hurt. He examined all their shoes, but could not perceive the least mark on any of them, nor was the earth broken where they had trodden. But the cart was deeply laden, and the wheels had penetrated much farther into the ground.

The equilibrium between the earth and the atmosphere seems at this instant to have been completely restored; for no further appearance of thunder or lightning was observed within the hemisphere; the clouds dispelled, and the air resumed the most perfect tranquillity; but how this vast quantity of electric matter could be discharged from the one element into the other without exhibiting any appearance of fire, he pretends not to examine. The fact however appears certain; and when he was mentioning it as a singular one, a gentleman told him, that the shepherd of St. Cuthbert's farm, on the opposite bank of the Tweed, had been an eye-witness of the event, and gave a different account of it. Mr. B. immediately went to the farm, found the shepherd, and made him conduct him to the spot whence he had observed it, and desired him to give an account of what had happened. He was looking, he said, at the two carts going up the bank, when he was stunned by a loud report, and at the same instant saw the first of the carts fall to the ground, and observed that the man and horses lay still, as if dead. He said, he saw no lightning, nor appearance of fire whatever; but observed the dust to rise at the place; that there had been several flashes of lightning some time before from the south-east, whereas the accident happened to the north-west of where he stood. The distance, in a right line across the river, might be between 2 or 3 hundred yards. He was sensible of no shock, nor uncommon sensation of any kind.

Several other phenomena happened on that day, probably all proceeding from the same cause; some of which Mr. B. mentions. The shepherd belonging to the farm of Lennel-hill was in a neighbouring field, tending his flock, when he observed a lamb drop down; and said, he felt at the same time as if fire had passed over his face though the lightning and claps of thunder were then at a great distance from him. He ran up immediately, but found the lamb quite dead; nor did he perceive the least convulsive motion, nor symptom of life remaining, though the moment before it appeared to be in perfect health. He bled it with his knife, and the blood flowed freely. This happened about a quarter of an hour before the explosion which killed Lauder; and it was not above 300 yards distant from the spot. He was only a few yards from the lamb when it fell down. The earth was not torn up, nor did he observe any dust rise.

Thomas Foster, a celebrated fisher in Coldstream, and another man, were standing in the middle of the Tweed, fishing for salmon with the rod, when they suddenly heard a loud noise; and, turning round to see from whence it came, they found themselves caught in a violent whirlwind, which felt sultry and hot, and almost prevented them from breathing. It was not without much difficulty they could reach the bank, where they sat down, exhausted with fatigue, and greatly alarmed: however it lasted but a very short time, and was succeeded by a perfect calm. This happened about an hour before the explosion.

A woman, making hay near the banks of the river, fell suddenly to the ground, and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came. Mr. Bell, our minister, nephew of Thompson the poet, and possessed of all the candour and ingenuity of his uncle, said, that, walking in his garden, a little before Lauder's accident, he several times felt a sensible tremor in the ground. He also said, that he had observed on Lauder's body a zig-zag line, of about an inch and a quarter broad, which extended from his chin down to his right thigh, and had followed nearly the line of the buttons of his waistcoat. The skin was burnt white and hard.

These are all the circumstances, says Mr. B., I have been able to collect that are well authenticated, and I shall not trouble you with reports that are not. From the whole it would appear, that the earth had acquired a great superabundance of electrical matter, which was every where endeavouring to fly off into the atmosphere. Perhaps it might be accounted for from the excessive dryness of the ground; and for many months, the almost total want of rain, which is probably the agent that nature employs in preserving, or in restoring, the equilibrium between the other two elements. But I shall not pretend to investigate the causes: all I wanted, was to give some account of the effects.

P. S. I cannot send away this letter without adding, in a postscript, that on Friday the 11th of August last, early in the morning, we had a pretty smart shock of an earthquake. I was awaked by it, and felt the motion most distinctly for 4 or 5 seconds at least. It appeared as if the bed had been pulled gently from side to side several times. The motion was nearly north north-west and south-east, as far as I could judge from the motion of the bed. The windows were violently shaken, and made a great noise, which I believe was mistaken by many people for a noise accompanying the earthquake. I immediately rose to look at my watch, and found it 20 minutes after 2. It was a dead calm, the morning close and warm, with a small drizzling rain, and though the moon was but 2 days past the full, so dark that I could not perceive the hour without striking a light. It was felt in almost every house in this neighbourhood, and all the way from this country to the west coast of the island, where it seems to have been more violent than here: but to the east of this place it was very little felt.

Perhaps it may not be improper to mention the state of the weather for some time before and after this event, as it may possibly have had some influence on it. The drought was very great till the 22d of July, when it rained a little; and this was repeated, though in small quantities, and generally accompanied by high winds, till Thursday the 27th, when it blew the most violent tempest I ever remember in this country. The young crop of turnips, in many fields, were blown out of the ground, and almost entirely destroyed. The pease became brown as if withered, and so did the leaves of the forest trees on that side which was opposed to the blast. Vast clouds of dust were raised from the dry fields and roads, which looked like smoke, and had the appearance at a distance as if many villages had been on fire all over the country. The water too was raised from the surface of the river, and carried quite away by the violence of the hurricane, forming small clouds in the air, which we traced to a great distance. The great violence of this tempest lasted but a few hours, and at night it fell calm. The barometer was little affected, and stood at $29\frac{1}{2}$ inches. The wind was nearly west, veering sometimes a little to the north. From this time we had a course of very fine weather, the wind constantly in the west points, till the time of the earthquake (which happened on what is called the last of the dog days), when it changed to the south-east, and brought us 5 of the worst days I ever remember to have seen at that season; it rained almost incessantly, with a cold easterly wind, and the sun did not once appear till the morning of Wednesday the 16th, after which we had again a course of fine weather. I examined the barometer at the time of the earthquake, but did not find that it had been sensibly affected. It rose a little on that morning; but this I imputed to the wind having changed into the east.

XIV. On finding the Values of Algebraic Quantities by Converging Serieses, and Demonstrating and Extending Propositions given by Pappus and others. By Edw. Waring, F.R.S. p. 71.

Suppose the roots of the equation $x^b \pm 1 = 0$ to be given, where h denotes any whole number or fraction; to find the roots or values of any given algebraical qnantity, by converging infinite serieses.

1. Let the algebraical quantity be $\sqrt[n]{(\pm A)}$, then the roots of the algebraical quantity will be $A^{\frac{1}{n}} \times (\alpha + \lambda\sqrt{-1})$, $A^{\frac{1}{n}} \times (\beta + \mu\sqrt{-1})$, $A^{\frac{1}{n}} \times (\gamma + \nu\sqrt{-1})$, &c. where $\alpha + \lambda\sqrt{-1}$, $\beta + \mu\sqrt{-1}$, $\gamma + \nu\sqrt{-1}$, &c. are the roots of the equation $x^n \pm 1 = 0$. It will be $+ 1$ if it was $-$ A, and $- 1$ if $+$ A.

2. Let the given algebraical quantity be $\sqrt[r]{(\pm \sqrt[n]{(\pm A)} \pm \sqrt[m]{(\pm B)} \pm \sqrt[p]{\pm C} \pm \text{\&c.})}$, and $\alpha + \lambda\sqrt{-1}$, $\alpha' + \lambda'\sqrt{-1}$, $\alpha'' + \lambda''\sqrt{-1}$, &c. and $\Gamma + \Delta\sqrt{-1}$, be respectively one of the roots of the equations $x^n \mp 1 = 0$, $x^m \mp 1 = 0$, $x^p \mp 1 = 0$, &c. and $x \mp 1 = 0$. Substitute $\pm P = \pm A^{\frac{1}{n}}\alpha \pm B^{\frac{1}{n}}\alpha' \pm C^{\frac{1}{n}}\alpha''$ $\pm$ &c. and $\pm Q = \pm A^{\frac{1}{n}}\lambda \pm B^{\frac{1}{m}}\lambda' \pm C^{\frac{1}{m}}\lambda'' \pm$ &c. In the first place let P be greater Q, and $\pm$ P be $+$ P, then will $(P \pm Q\sqrt{-1})^{\frac{1}{r}} = (P^{\frac{1}{r}} - \frac{1}{r} \cdot \frac{1-r}{2r} \times \frac{Q^2}{P^{\frac{2r-1}{r}}} + \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \cdot \frac{1-3r}{4r} \times \frac{Q^4}{P^{\frac{4r-1}{r}}} - \text{\&c.} = \pm L) \pm (\frac{1}{r} \cdot \frac{Q}{P^{\frac{r-1}{r}}} - \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \cdot \frac{Q^3}{P^{\frac{3r-1}{r}}} + \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \cdot \frac{1-3r}{4r} \cdot \frac{1-4r}{5r} \times \frac{Q^5}{P^{\frac{5r-1}{r}}} - \text{\&c.} = \pm M)$

$\times \sqrt{-1} = \pm L \pm M \sqrt{-1}$, in which case the two serieses $\pm L$ and $\pm M$ converge, and $(\Gamma + \Delta \sqrt{-1}) \times (\pm L \pm M \sqrt{-1})$ will be a value or root of the given quantity.

In the same manner the remaining roots may be deduced.

2. Let $\pm P$ be $-P$, multiply $-P \pm Q \sqrt{-1}$ into -1, and it becomes $P \mp Q \sqrt{-1}$, a quantity of the same formula as the preceding; let $\Gamma + \Delta' \sqrt{-1}$ be a root of the equation $x + 1 = 0$, then will $(\Gamma' - \Delta' \sqrt{-1})(\pm L \pm M \sqrt{-1}) = \pm H' \pm K' \sqrt{-1}$, be a root of the given quantity. Otherwise; the root may be deduced from the above-mentioned series by substituting in it for $-(P)^{\frac{1}{r}}$ its value $P^{\frac{1}{r}} \times (-1)^{\frac{1}{r}}$, and it will become the same as the preceding.

3. Let P be less than Q, and the value of $(\pm P \pm Q \sqrt{-1})^{\frac{1}{r}}$ may be deduced from the preceding series by substituting in it $\pm Q \sqrt{-1}$ for P, and $\pm P$ for Q. Otherwise, since $(\pm P \pm Q \sqrt{-1})^{\frac{1}{r}} = \pm \sqrt[2r]{-1} \times (Q \mp P \sqrt{-1})^{\frac{1}{r}}$, and the root of $(Q \mp P \sqrt{-1})^{\frac{1}{r}}$ can be deduced by the preceding method, which suppose $L' + M' \sqrt{-1}$; multiply this root into $H \pm \Theta \sqrt{-1}$, where $H + \Theta \sqrt{-1}$ denotes a value of the root $\pm \sqrt[2r]{-1}$, and the quantity resulting will be one value of the given quantity. The remaining values can be deduced by the same method. In this case the given quantity is resolved into a series ascending according to the dimensions of P, and descending according to the dimensions of Q; in the former case it was resolved into a series ascending according to the dimensions of Q, and descending according to the dimensions of P; both the serieses affording the possible or impossible parts will always converge.

4. If $P = \pm Q$, then will $(\pm P \pm P \sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times (\pm 1 \pm \sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} (\pm \sqrt{\tfrac{1}{2}} \pm \sqrt{-\tfrac{1}{2}})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4r]{-1}$; for $\sqrt[4]{-1} = \pm \sqrt{\tfrac{1}{2}} \pm \sqrt{-\tfrac{1}{2}}$.

4. 2. When $P = 0$, or $Q = 0$, then it becomes the first case $\sqrt[n]{\pm A}$.

5. Let $P = Q \mp \alpha$, where α has a very small ratio to Q; then will $(P \pm Q \sqrt{-1})^{\frac{1}{r}} = (P \pm (P \pm \alpha) \sqrt{-1})^{\frac{1}{r}} = (P \times 2^{\frac{1}{2}} \times (-1)^{\frac{1}{4}} \pm \alpha \sqrt{-1})^{\frac{1}{r}} = P^{\frac{1}{r}} \times 2^{\frac{1}{2r}} \times \sqrt[4r]{(-1)} \pm \frac{1}{r} \times P^{\frac{1-r}{r}} \times 2^{\frac{1-r}{2r}} \times \sqrt[\frac{4r}{1-r}]{(-1)} \times \sqrt{(-1)}\, \alpha - \frac{1}{r} \times \frac{1-r}{2r} \times P^{\frac{1-2r}{r}} \times 2^{\frac{1-2r}{2r}} \times \sqrt[\frac{4r}{1-2r}]{(-1)} \times \alpha^2 \pm \frac{1}{r} \cdot \frac{1-r}{2r} \cdot \frac{1-2r}{3r} \times P^{\frac{1-3r}{r}} \times 2^{\frac{1-3r}{2r}} \times \sqrt[\frac{4r}{1-3r}]{(-1)} \times \sqrt{(-1)}\alpha^3 + \&c.$ In this series the same root of the quantity $\sqrt[4r]{-1}$ is always to be used.

6. If in the given quantity are contained more quantities of the above-mentioned kind or their roots; then, by repeating the same operation, can be deduced the roots or values of the given quantity. In some cases the impossible part may vanish, which may be the case in a quantity of the following formula,

viz. $\sqrt[n]{}(a + \alpha\sqrt[2m]{} - b) + \sqrt[n]{}(a + \beta\sqrt[2m]{} - b) + \sqrt[n]{}(a + \gamma\sqrt[2m]{} - b)$ + &c. where α, β, γ, &c. denote the $2m$ roots of $\sqrt[2m]{} - 1$. The general principles of discovering the cases in which this happens, have been given in the Meditationes Algebraicæ.

The roots of the equation $x^{h} \pm 1 = 0$ will be found from common algebra and these principles, if h is not greater than 10; or, more generally, if $h = 2^{l} \times 3^{l'} \times 4^{l''} \ldots 10^{l^{8}}$, where $l, l', l'' \ldots l'^{8}$ denote any whole numbers: or, in general, the roots of the above-mentioned equation, or even of the equation $x = \sqrt[r]{}(\pm L \pm M\sqrt{-1})$, can be found from tables of sines. The same principles may be applied to the discovery of the values of exponential irrational quantities.

In the Miscel. Analy. was given, from a substitution invented by me, and not similar to any before given, a resolution of equations, which contains the resolutions of all equations before given, and from which the resolutions of some equations, not before delivered, have been added.

PART II.—1. Let an equation $A = 0$, involving r unknown independent quantities, be predicated of another equation containing the same quantities, and the demonstration of it be required. 1st. Reduce both the equations to equations involving independent quantities only; then reduce the two equations to one, so that one of the above-mentioned quantities may be exterminated, and if there results a self-evident equation, viz. $A = A$, or $A - A = 0$, in which the correspondent terms destroy each other respectively; then the first equation is justly predicated of the second; that is, if the above-mentioned equations afford the same value of the quantity exterminated, the proposition is true; otherwise not.

Corol. From these principles can be demonstrated many propositions given by Pappus and others.

A C D E B

Exam. Let $AD = 2AC = 2x$, $DE = a$, and $EB = b$, where AD, DE, and EB are independent quantities; if $AB \times BE = (2x + a + b) \times b = CB \times BD = (x + a + b)(a + b)$, then will $CB = x + a + b : BD = a + b :: AC \times CE = x \times (x + a) : AD \times DE = 2x \times a$. Hence can be deduced the two equations $(b - a)x = a^2 + ab$ and $2a \times (x + a + b) = (a + b) \times (x + a)$; reduce these two equations to one, so as to exterminate x, and there results the self-evident equation $(a - b) \times \frac{a^2 + ab}{b - a}(-a^2 - ab) + a^2 + ab = 0$, and consequently the proposition is true.

2. If s equations, involving $t + r$ unknown and independent quantities, be predicated of t equations involving the above-mentioned quantities; reduce the t equations and one of the above-mentioned s equations to one, so that t unknown quantities may be exterminated; and if there results a self-evident equation, then the above-mentioned equation is justly predicated of the t equations. And in the same manner we may reason concerning the remaining $s - 1$ equations.

3. 1. If one equation is justly predicated of another, and in both the un-

known quantity exterminated has only one dimension; then the latter equation can be predicated of the former; for in this case both equations have only one and the same value of the unknown quantity exterminated.

3. 2. If the quantity exterminated has more dimensions than one in the equations, then the proposition may not generally be true; for the equations may have some roots the same, but not all. These observations may be applied to more equations.

4. From n given equations $a = 0$, $b = 0$, $c = 0$, &c. can easily be deduced others dependent on them, by finding any direct algebraical functions of the above-mentioned equations, that is, $\varphi\ (a, b, c, \&c.)$, which will always $= 0$; and in like manner, from the relation between any lines being given, can be deduced innumerable relations between the above-mentioned lines, and other lines dependent on them.

PART III.—1. Ratios, which are supposed greater or less than others, can easily be transformed into equations, which contain affirmative and negative quantities. For example, let the ratio $a : b$ be greater than the ratio $c : d$, then will $\frac{b}{a} = \frac{c}{d} - k$; if it be less, then will $\frac{b}{a} = \frac{c}{d} + k$, where k denotes an affirmative quantity; and, vice versâ, if $\frac{b}{a} = \frac{c}{d} - k$, then will the ratio of $a : b$ be greater than the ratio of $c : d$, &c.

2. If one quantity a is affirmed to be greater than another b, for a in the given equations substitute its value $b + k$; if less, for a write $b - k$, where k denotes an affirmative quantity.

3. Reduce the equations, so as to take away their denominators, and the demonstration of the proposition will often very easily follow.

4. Let $k = \frac{P}{Q}$ and $k' = \frac{P'}{Q'}$; and if P and Q be affirmative, let P′ and Q′ be affirmative; and, vice versâ, if negative, negative; then, if k be affirmative, will k' also be affirmative; the same also may be affirmed, if P and Q have both contrary signs to P′ and Q′; but if one has the same, and the other contrary, then will k and k' have contrary signs.

5. Let some affirmative quantities be less than others, then any direct affirmative function of the former, viz. function in which no negative or impossible quantities or indexes are contained, will be less than the same function of the latter. The contrary happens when the indexes are all negative, and the quantities affirmative as before: for example, let 2 quantities be less than 2 others, then the product of the former 2 will be less than the product of the latter.

Corol. Hence some quantities may often be known to be greater or less than others, from their direct function being greater or less than the same functions of the others: for example, let $a^2 - b^2$ be an affirmative quantity, then will a be greater than b.

6. If one equation or ratio is affirmed on the supposition that another given one is true, reduce both the equations by the methods given above, and from the principles before delivered the proposition will often be evident. Hence may be deduced demonstrations to propositions of this sort given by Pappus and others.

Exam.—Let the ratio $a + b : b$ be greater than $c + d : d$, then the ratio $b : a - b$ will be less than $d : c - d$.

For, since the ratio $a + b : b$ is greater than $c + d : d$, the ratio $b : a + b$ will be less than $d : c + d$, and consequently $\frac{a+b}{b}(\frac{a}{b} + 1) = \frac{c+d}{d}(\frac{c}{d} + 1) + k$, whence $\frac{a}{b} - 1 = \frac{c}{d} - 1 + k$, and $\frac{a-b}{b} = \frac{c-d}{d} + k$, and the ratio $b : a - b$ less than $d : c - d$.

Exam. 2.—Let the ratio of $a + b : c + d$ be greater than the ratio of $a : c$, then will the ratio of $b : d$ be greater than the ratio of $a + b : c + d$. By the preceding method convert these ratios into equations, and there result $\frac{c+d}{a+b} + k = \frac{c}{a}$ and $\frac{d}{b} + k' = \frac{c+d}{a+b}$; and the proposition asserts, that if k be an affirmative quantity, k' will also be an affirmative quantity. Reduce these two equations, so as to take away their denominators, and the resulting equations will be $ac + ad + a \times (a + b) \times k = ac + bc$, and $ad + bd + (a + b) \,.\, bk' = bc + bd$, whence $k = \frac{bc - ad}{a(a+b)}$, and $k' = \frac{bc - ad}{b(a+b)}$, and the proposition is evident.

Exam. 3.—Let a be greater than c, and b, and $(a + b) \times (a - b) = (c + d) \times (c - d)$, that is, $a^2 - b^2 = c^2 - d^2$; then will b be greater than d: for a in the equation $a^2 - b^2 = c^2 - d^2$ write $c + k$, and there results $2ck + k^2 = b^2 - d^2$, whence $b^2 - d^2$ is an affirmative quantity, and consequently b greater than d.

Exam. 4.—Let, as in Ex. 1, the ratio $a + b : b$ be greater than $c + d : d$, then will $b : a - b$ be less than the ratio $d : c - d$. By the preceding method translate these ratios into the two equations $\frac{b}{a+b} + k = \frac{d}{c+d}$ and $\frac{a-b}{b} = \frac{c-d}{d} + k'$; reduce these equations, so as to take away their denominators, and there result $bc + bd + (a + b) \times (c + dk) = ad + bd$ and $da - db = bc - bd + bdk'$, and consequently $k = \frac{ad - bc}{(a+b)(c+d)}$, and $k' = \frac{ad - bc}{bd}$; but these two fractions, which express the values of k and k', have the same numerators, and their denominators both affirmative; therefore if one k be affirmative, the other k' will also be affirmative.

Corol. From these principles can easily be deduced innumerable propositions of this sort. Assume 2 or more ratios, of which let some be supposed greater than others; then, from the above-mentioned transformation, by addition, subtraction, multiplication, division, &c. can be found such functions of the above-mentioned quantities, that some may become greater than others, and thence may be deduced the propositions above-mentioned.

7. It may not be improper in this place to adjoin a few observations on finding the limits of some quantities in which others contained in given equations become negative or affirmative.

1. Given an equation involving 2 unknown quantities, x and y; the limits of the quantity y, between which the quantity x will become affirmative or negative, may be deduced from the following principles. The quantity x passes from affirmative to negative or from negative to affirmative, either through nothing or infinite; or from 2 impossible roots it passes to affirmative or negative through 2 or more equal roots; and, vice versâ, from affirmative or negative to 2 or more impossible roots through 2 or more equal roots. Find therefore the values of y, when x becomes $= 0$, or infinite; and also all the cases in which 2, &c. values of x become equal, that is, when its roots become impossible; and thence can be deduced the limits of the quantity y, between which x becomes affirmative or negative.

2. If $x = \frac{P}{Q}$ be an affirmative quantity, then P will be affirmative or negative, according as Q is an affirmative or negative quantity, &c. Assume therefore $P = 0$ and $Q = 0$, and from the roots of the resulting equation can be deduced the cases in which x becomes an affirmative quantity.

3. If more n unknown quantities, x, y, z, v, &c. be contained in a given equation; then, by the preceding method, find the limits of z, v, &c., between which x becomes an affirmative or negative quantity, and let the quantities denoting the limits contain not more than $n - 1$ unknown quantities: from the above-mentioned quantities or equations expressing the limits, find others denoting their limits, which do not contain more than $n - 2$ above-mentioned quantities, and so on.

4. Often from the substitution of the limits of given quantities can be acquired the limits of the remaining one x. Find all the greatest values of the quantity x contained between the above-mentioned limits, and thence can be deduced the limits sought.

5. If there are given m equations involving $m + 1$ or more unknown quantities; then sometimes with, and sometimes without, reducing them to others involving fewer unknown quantities, can be found by the preceding method limits; and from comparing the limits so acquired can sometimes be deduced the limits sought.

6. If a given function of the unknown quantities x, y, z, &c., is asserted to be contained between given limits, when other functions of the above-mentioned

quantities are contained between given limits, and the demonstration of it is required; from the given equations and the given functions find limits of the unknown quantities respectively, and if the latter limits are contained between the former, the proposition is generally true, otherwise not.

7. From the above-mentioned principles can be found the cases in which an unknown quantity x admits of one or more affirmative values.

8. It appears from the principles before delivered, that the finding the number of affirmative and negative roots of a given equation, necessarily includes the finding the number of its impossible roots; and therefore it may not be improper to subjoin somewhat on what has been done on this subject.

1. Descartes gave a method of finding the number of affirmative and negative roots of a given equation, when all its roots are possible; but all the roots in equations of superior dimensions are very seldom possible, unless when the equation is purposely made.

2. It has been demonstrated by others and myself, that the equation will at least have so many changes of signs from + to −, and − to +, as there are affirmative roots, and so many continued progresses from + to + and − to −, as there are negative roots.

3. A rule for finding in general the number of affirmative or negative roots in a biquadratic, and in the equation $x^n + \text{A}x^m + \text{B} = 0$, was first published in the Medit. Algebr.

4. Harriot demonstrated a method of finding the number of impossible roots contained in a cubic equation. In the year 1757 I sent to the Royal Society a method of finding the number of impossible roots contained in a biquadratic and quadrato-cubic equations, and in the equation $x^n \pm \text{A}x^m \pm \text{B} = 0$.

5. Schooten gave a method of finding the number of impossible roots which can be concluded from the deficient terms of an equation. Newton gave a rule which often discovers the number of impossible roots contained in a given equation. Campbell discovered a new rule on the same subject. Mr. Maclaurin has added somewhat more general on these subjects: these rules may be rendered more general by a principle first given in the Miscell. Analyt. viz. multiplying the given equation into a quantity $x - a$ or $(x - a) \times (x - b)$, &c. and finding from the rule the number of impossible roots contained in the given equation. Similar and more general rules and principles have been added in the Medit. Algebr. These rules, in equations of superior dimensions, seldom discover the true number of impossible roots. I believe also, that I first gave a rule in the Miscell. Analyt. for finding the number of impossible roots from finding an equation, whose roots are the squares &c. of the roots of a given equation, which rule in equations of superior dimensions sometimes finds impossible roots, when Newton's, Campbell's, &c. rules fail, and fails when they find them; and also a

rule for finding impossible roots from an equation, whose roots are the squares of the differences of the roots of the given equation; this rule (as has been observed by me in the Miscell. Analyt. and Philos. Trans.) always discovers whether all the roots of the given equation are possible or not; and the last term of the resulting equation discovers also, whether 0, 4, 8, 12, &c. or 2, 6, 10, 14, &c. impossible roots, are contained in the given equation; to which may be subjoined, if the given equation has r possible and $n - r = 2t$ impossible roots, that the number of changes of signs from + to − and − to + in the resulting equation will not be less than $r \cdot \frac{r-1}{2}$, and the number of continued progresses from + to + and − to − will not be less than t: whence, if the number of continued progresses be t', the number of impossible roots will not be greater than $2t'$, and the number of possible roots not less than $n - 2t'$. If the number of changes of signs be h', the number of possible roots will not be greater than r', where $r' \times \frac{r'-1}{2}$ is the greatest possible number which does not exceed h', and the number of impossible roots not less than $n - r'$. Another rule was, I believe, first given by me in the Miscell. Analyt. 1762, for finding impossible roots, by finding an equation whose roots are z, where $x^n - px^{n-1} + qx^{n-2} -$ &c. $= z$, and $nx^{n-1} - (n-1)px^{n-2} + (n-2)qx^{n-2} -$ &c. $= 0$.

In the Medit. Algebr. somewhat has been added concerning impossible, affirmative, and negative values of the unknown quantities, in an equation which involves 2 or more unknown quantities; and also was first delivered a rule from the number of affirmative, negative, and impossible roots of an equation being known, to find the number of impossible, negative, and affirmative roots of an equation, whose roots have a given algebraical relation to the roots of a given equation; on which two last subjects little, I believe, had been before published.

XV. Experiments on the Production of Dephlogisticated Air from Water with various Substances. By Sir B. Thompson, Knt., F. R. S. p. 84. *Dated Munich, Sept.* 1, 1786.

Various opinions having been entertained with respect to the origin of the dephlogisticated air, produced by exposing healthy vegetables in water to the action of the sun's rays, according to the method of Dr. Ingenhousz; and not being myself thoroughly satisfied with any of the theories proposed, I made the following experiments, with a view of throwing some new light on that subject.

Having found that raw silk possesses a power of attracting and separating air from water in great abundance, when exposed in it to the action of light, it occurred to me to examine the properties of this air, and to consider more attentively the circumstances attending its production.

Exper. 1. My first object was to collect a sufficient quantity of the air separated from water by silk, to determine its goodness by the test of nitrous air; and to this end, having filled with clear spring water a globe of thin, white, and very transparent glass, 4¼ inches in diameter, with a cylindrical neck ¾ of an inch in diameter, and about 12 inches long, I introduced into it 30 grains of raw silk, which had been previously washed in water, to free it of air; and inverting the globe under water, and placing its neck in a glass jar, containing a quantity of the same water with which the globe was filled, I exposed it in a window to the action of the sun's rays, and prepared myself to examine the progress of the generation or production of the air.

In 10 minutes an infinite number of exceedingly small air-bubbles began to make their appearance on the surface of the silk; and these bubbles continuing to increase in number, and in size, at the end of about 2 hours the silk appearing to be entirely covered with them, rose to the upper part of the globe. These bubbles going on to increase in size, and running into each other, at length began to detach themselves from the silk, and to form a collection of air at the upper part of the globe; but the measure of my eudiometer being rather large, it was not till after the globe had been exposed in the sun near 4 days, that a sufficient quantity of air was collected to make the experiment with nitrous air, in order to ascertain its goodness by that test.

Having at length collected a sufficient quantity of this air for that purpose, I carefully removed it from the globe, and mixing with 1 measure of it 3 measures of nitrous air, they were reduced to 1.24 measures; which shows, that it was actually dephlogisticated air, and that of a considerable degree of purity. Common air, tried at the same time, 1 measure of it with 1 measure of nitrous air, were reduced to 1.08 measure.

Having again exposed the globe with the same water and silk in the window, where the sun shone the greatest part of the day, at the end of 3 days I had collected 3¾ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1.18$; that is, 1 measure of this air, added to 3 measures of nitrous air, were reduced to 1.18 measure. A small wax-taper, which had been just blown out, a small part only of the wick remaining red-hot, on being plunged into a phial filled with this air, immediately took fire, and burnt with a very bright and enlarged flame. The water in the globe appeared to have lost something of its transparency, and had changed its colour to a very faint greenish cast, having also acquired the odour or fragrance proper to raw silk.

This experiment was repeated several times with fresh water (retaining the same silk) and always with nearly the same result: with this difference however, that when the sun shone very bright, the quantity of air produced was not only greater, but its quality also much superior to that yielded when the sun's rays were more feeble, or when they were frequently intercepted by flying clouds. The air however was always not only much better than common air, but better than the air in general produced by the fresh leaves of plants exposed in water to the sun's rays in the experiments of Dr. Ingenhousz; and under the most favourable circumstances it was so good, that 1 measure of it required 4 measures of nitrous air to saturate it, and 3.65 measures of the two airs were destroyed; or, proved with nitrous air, it gave $1a + 4n = 1.35$, which, I believe, is better than any air that has yet been produced in the experiments with vegetables.

The method here adopted of using algebraic characters in noting the result of the experiments made to determine the goodness of air, though not strictly mathematical, is very convenient; and for that reason, I shall continue to make use of it. a represents the air which is proved: n nitrous air; and the numbers joined to these letters show the quantities, or the number of measures, of the different airs made use of in the experiment. The other number, which stands alone, or without any letter attached to it, on the other side of the equation, shows the volume, or the number of measures and parts of a measure to which the two airs are reduced after they are mixed. I shall sometimes add a 4th number, showing the quantity of the two airs destroyed, as this more immediately shows the goodness of the air which is proved. Thus, in the experiment last-mentioned, 1 measure of the air proved, mixed with 4 measures of nitrous air, were reduced to 1.35 measure, consequently 3.65 measures of the two airs were destroyed; for it is $1 + 4 = 5 - 1.35 = 3.65$, and the result of this trial I should write thus, $1a + 4n = 1.35$, or 3.65.

Or, for still greater convenience in practice, as this last number 3.65, or $3\frac{65}{100}$, shows more immediately the goodness of the air in question, by supposing with Dr. Ingenhousz, the measure of the eudiometer to be divided into 100 equal parts, it will be $100\,a + 400n = 135$, and 365, expressing the volume of the two airs destroyed, will become a whole number. But instead of writing $100\,a + 400\,n = 135$, &c., I shall continue to write $1a + 4n = 135$, and shall express the last number (3.65) as a whole number notwithstanding; and I shall sometimes (following the example of Dr. Ingenhousz) write this number only, in noting the goodness of any air in question.

I would just observe, with respect to the process of proving the goodness of any kind of air, by the test of nitrous air, that I mix the two airs in a phial, about 1 inch in diameter and 4 inches long, putting the air to be proved into the phial first, and then introducing the nitrous air, one measure after another, till the volume of the 2 airs after the diminution has taken place, amounts to more than 1 measure, and is less than 2 measures. Immediately after the introduction of each measure of nitrous air, I give the phial a couple of shakes;

after which I suffer it to stand at rest, while I prepare another measure of nitrous air, which commonly takes up about 20 seconds.

The measure of the eudiometer being filled with air, I suffer it to remain quiet under water 15 seconds, or while I can leisurely count 30, in order that the air may have time to acquire the temperature of the water in the trough, and that the water in the measure may have time to run down from the sides of the glass tube; and in shutting the slider I take care to bring it to be exactly even with the surface of the water in the trough. Similar precautions are also made use of, in measuring the volume of the two airs in the tube of the eudiometer, after they have been mixed and diminished in the phial. In order to know when I have added nitrous air enough to the air in the phial, so that the volume of the two airs may amount to 1 measure, and may not be greater than 2 measures, there are 2 marks on the phial, made with the point of a diamond, the one showing 1 measure of the eudiometer, the other showing 2 measures. The tube of my eudiometer is half an inch in diameter internally, and 1 measure occupies 3¼ inches in length on it, and the measure itself is made of a piece of the tame tube. Both the one and the other are ground with fine emery on the inside, in order to take off the polish of the glass, and by that means facilitate the running down of the water, which might otherwise hang in drops on the inside of the tube on the introduction of air. The nitrous air was always fresh made, and of the same materials, viz. fine copper wire dissolved in smoking spirits of nitre, diluted with 5 times its volume of water, and all possible attention was paid to every other circumstance that could contribute to the accuracy of the experiments.

Exper. 2. Finding that the quantity and the quality of the air produced depended, in a great measure, on the intensity of the light by which the water and the silk were illuminated, I was desirous of seeing whether, by depriving them entirely of all light, they would not at the same time be deprived of the power of furnishing air. To ascertain this fact, I took a globe A, similar to that used in the foregoing experiment, and having filled it with fresh spring water, I introduced into it 30 grains of raw silk, and placing it with its cylindrical neck inverted in a jar filled with the same water, I covered the whole with a large inverted earthen vessel, and exposed it, so covered up, for several days in my window, by the side of another globe B, containing a like quantity of water and silk, which I left naked, and consequently exposed to the direct rays of the sun. The result of this experiment was, that the water and silk in the globe exposed to the sun's rays furnished air in great abundance, as in the experiment before-mentioned; while that in the globe covered up in darkness, produced only a few very inconsiderable air-bubbles, which remained attached to the silk.

Exper. 3. To see if heat would not facilitate the production of air in the globe sheltered from the light, I now removed it from the window, and placed it near a German stove, where I kept it warmed to about 90° of Fahrenheit's thermometer for more than 24 hours; but this was all to no purpose. The air produced was so exceedingly small in quantity, that it could neither be proved, nor measured, there being only a few detached air-bubbles, which had collected themselves near the top of the globe. The medium heat of the water in the globe exposed in the sun's rays, at the time when it furnished air in the greatest abundance, was about 90° Fahrenheit. It was sometimes as high as 96°; but air was frequently produced in considerable quantities when the heat did not exceed 65° and 70°.

Exper. 4. Finding by the last experiment that heat alone, without light, was not sufficient to enable silk in water to produce air, I was desirous of seeing the effect of light, without heat on them. To this end, I took the globe B, with its contents, and plunging it into a mixture of ice and water brought it to the temperature of about 50° F. and taking it out of this mixture, and exposing it immediately in the sun's rays, which were very piercing at the time, I kept it in this temperature above 2 hours by the occasional application of cloths, wet in ice water, to the lower part of the globe. Notwithstanding this degree of cold, a considerable quantity of air was produced; though it was not furnished in so great abundance as when the globe was suffered to become hot in the sun's rays.

Having thus ascertained the great effect of the sun's rays in the production of the air furnished on exposing silk in water to their influence, my next attempt was to determine, whether this arose from any peculiar quality in the sun's light; or whether other light would not produce the same effect. With a view to ascertaining this point, I made the following interesting experiment.

Exper. 5. Having removed all the air from the globe B, and having supplied its place with a quantity of fresh water, so as to render it quite full, I replaced it inverted in its jar, and removing it into a dark room, surrounded it with 6 lamps with reflectors, and 6 wax candles, placed at different distances from 3 to 6 inches from it, and so disposed as to throw the greatest quantity of light possible on the silk in the water, taking care at the same time that the water should not acquire a greater heat than that of about 90° F. After about 10 minutes, the air-bubbles began to make their appearance on the surface of the silk; and at the end of 6 hours, there was collected at the upper part of the globe a quantity of air sufficient to make a proof of its goodness with nitrous air; and on trial it was found to be dephlogisticated, and of such a degree of purity, that 1 measure of it with 3 measures of nitrous air occupied no more than 1.68 measure.

I afterwards exposed, to the same light, in small inverted glass jars, filled with water, a fresh-gathered healthy leaf of the peach tree, and a stem of the pea plant with 3 leaves on it; both which vegetables furnished air in the same manner as they are known to furnish it when exposed, under similar circumstances, to the action of the sun's direct rays, but in less quantities, which I attribute to the greater intensity of the sun's light above that of my lamps.

After describing some additional contrivances, to render the globes, &c. more convenient, Sir B. says, finding that raw silk, exposed in water to the action of light, causes the water to yield pure air in so great abundance, I was desirous of finding out whether this arose from any peculiar quality possessed by the silk; or whether other bodies might not be made to produce the same effect: to this end, having provided 6 globes, each about $4\frac{1}{2}$ inches in diameter, and having filled them with fresh spring water, I introduced into them the following substances, and exposed them all, at the same time, to the action of the sun's rays.

In the globe N° 1..... I put 15 grains of sheep's wool,
N° 2.......... 15 grains of Eider down,
N° 3.......... 15 grains of the fine fur of a Russian hare,
N° 4.......... 15 grains of cotton wool,
N° 5.......... 15 grains of lint, or the ravellings of fine linen,
N° 6.......... 15 grains of human hair; these substances being all well washed, and being thoroughly freed of air, by being wet before they were put into the globes.

The results of these experiments were as follows:

Exper. 6. The globe N° 1, which contained the sheep's wool, did not begin to furnish air in any considerable quantity till the 3d day of its being exposed to the action of the sun's rays; and several days of cloudy weather intervening, I did not remove the air till the 8th day, when I collected $1\frac{3}{4}$ cubic inch, which proved with nitrous air, gave $1a + 3n = 1.28$, or 272 degrees. The wool at no time furnished more than $\frac{1}{3}$ part of the air, which an equal quantity of silk would have furnished under the same circumstances.

Exper. 7. The water in the globe N° 2, with the Eider down, began almost immediately to furnish air, and continued to yield it during the whole time of the experiment, nearly in as large quantities as the water with silk had done in the former experiments, and nearly of the same quality. $1\frac{3}{4}$ cubic inches of this air, furnished the 8th day from the beginning of the experiment, or the 3d of sunshine, proved with nitrous air, gave $1a + 3n = 1.34$, or 266 degrees of purity.

Exper. 8. The globe N° 3, with the hair's fur, which was white, furnished more air than the sheep's wool, but not so much as the Eider down. After 4 days of sunshine, I collected 2 cubic inches of this air, which, proved with nitrous air, gave $1a + 3n = 1.44$, or 256.

Exper. 9. The globe N° 4, with cotton wool, furnished a considerable quantity of air, which appeared to be better than that furnished by any of the 5 other globes. Proved with nitrous air, it turned out $1a + 3n = 1.07$, or 293; and, what was particular, the water did not appear to have altered its colour in the least, or to have lost any thing of its transparency.

Exper. 10. The globe, N° 5, with ravelings of linen, was very tardy in furnishing air, and produced but a small quantity; at the end of a fortnight, however, I collected about 2 cubic inches, which, proved with nitrous air, gave $1a + 3n = 1.51$, or 249.

Exper. 11. The globe N° 6, with human hair, furnished still less air than with ravelings of linen in the last mentioned experiment; but, notwithstanding the smallness of the quantity, it was considerably superior in quality to atmospheric air; for, proved with nitrous air, it gave $1a + 2n = 1.45$, or 155; whereas common air, proved at the time, gave $1a + 1n = 1.08$, or 92.

Exper. 12. To ascertain the relative goodness of the air furnished by the water in these experiments, and of that produced by exposing fresh healthy vegetables in water to the action of the sun's light, according to the method of Dr. Ingenhousz, I collected a small quantity of air from a stem of a pea plant, which had 4 healthy leaves on it, and found it to be much inferior to that furnished in the experiments with silk, and the various other substances I made use of. Proved with nitrous air, it gave $1a + 2n = 1.05$, or 195. And similarly with other plants.

With a view of determining, with greater precision, the quantity and the quality of the air produced by a given quantity of water and silk, exposed for a given time to the action of the sun's rays, I made the following experiment.

Exper. 13. A globe of fine, clear, white glass, about $8\frac{3}{10}$ inches in diameter, and containing 296 cubic inches, being filled with fresh spring water, and 30 grains of raw silk, was exposed in my window three days, being for the most part cold and cloudy, with short intervals of sunshine. Air produced $9\frac{1}{2}$ cubic inches; quality $1a + 3n = 1.61$, or 239.

Similar experiments being repeated on several successive days, the quantities and qualities of the airs furnished on the different days were as follow:

	Quantity.		Quality.
On the 12th, 13th, and 14th of May	$9\frac{1}{2}$	cubic inches	$1a + 3n = 1.61$, or 239
15th.	$8\frac{46}{100}$	....	$1a + 4n = 1.74$, or 326
16th.	9	....	$1a + 4n = 1.44$, or 356
17th.	6	....	$1a + 4n = 1.35$, or 365
18th.	$\frac{3}{4}$	....	$1a + 4n = 1.56$, or 344
19th.	$\frac{1}{4}$	....	$1a + 4n = 1.74$, or 326
Total quantity	$33\frac{96}{100}$	Mean quality	$1a + 4n = 1.84$, or 316

As in this experiment the air furnished each day was removed at night, and the place it occupied in the globe supplied with fresh water, I was desirous of seeing what variation it would occasion in the result of the experiment, if, instead of removing the air from time to time, I suffered it to remain in the globe till the end of the experiment: to this end I made

Exper. 14. In which the globe being filled with fresh water, and the silk used in the last experiment, being first well washed, the whole was exposed 4 days to the action of the sun's rays, the weather being remarkably fine, and very hot. On removing the air produced, I found it amounted to $30\frac{1}{10}$ cubic inches; and its quality, proved with nitrous air, was $1a + 3n = 1.02$, or 298.

Exp. 15. The silk employed in the last experiment having been frequently used in the foregoing ones, I was desirous of seeing the effect of making use of fresh silk; and also of varying the proportion between the quantity of silk, the quantity of water, and the size of the globe; accordingly at 6 o'clock, P. M. June 13, I filled a small globe, about 3 inches in diameter, which contained just 20 cubic inches, with fresh spring water, and 17 grains of raw silk, wound in a single thread, which had never been put into water, or otherwise used, since it came out of the hands of the silk-winder. At the end of 4 days, this globe had only furnished $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a + 1n = 1.32$, or 68; consequently was much worse than common air. On the 18th, it began to produce good air, and during 6 hours of sunshine it furnished $1\frac{15}{100}$ cubic inches, which, proved with nitrous air, gave $1a + 3n = 1.15$, or 285. The two following days (viz. the 19th and 20th of June) it furnished $1\frac{27}{100}$ cubic inch of air, which, proved with nitrous air, gave $1a + 3n = 1.37$, or 263; after which it totally ceased to yield air, though exposed for several days in the sun's rays. Total quantity of air produced $2\frac{67}{100}$ cubic inches; mean quality $1a + 3n = 1.46$, or 254.

By this experiment it appears, that raw silk, when used for the first time, does not immediately dispose the water to yield pure air; on the contrary, that it phlogisticates the air yielded by water to a very considerable degree; and this I afterwards found to be the case with several other substances.

Though the quality at a medium, of the air furnished in this experiment was not quite so good as that furnished in the two experiments last-mentioned (viz. N° 13 and N° 14), yet its quantity, in proportion to the quantity of water made use of, was greater than in either of them: it amounted to something more than $\frac{1}{8}$ of the volume of the water.

Exper. 16. The great globe, contents 296 cubic inches, being filled with fresh spring water, and 120 grains of poplar cotton, on the evening of June 9, and being the next day exposed to the sun about 4 hours, on the morning of the 11th the air produced was removed, and its quantity was found to be $1\frac{3}{4}$ cubic inch. Its quality was very bad, viz. $1a + 1n = .165$, or 35 degrees only better than thoroughly phlogisticated air. On the 11th, 12th, and 13th, 1 cubic inch of air only was produced, and this appeared to be as bad as possible; for, proved with nitrous air, it gave $1a + 1n = 2$, or 0. On the 14th a few air-bubbles only were furnished; but, notwithstanding these unfavourable appearances, I still continued the experiment, and my patience was amply rewarded; for the next day, the 15th, the sun being very powerful, and the weather very hot, the water changing suddenly to a greenish colour, began all at once to give good air in great abundance. In the course of the day $10\frac{42}{100}$ cubic inches were produced, which, proved with nitrous air, gave $1a + 3n = 1.43$, or 257. June 16th, a very warm clear day, the globe exposed in the sun, from 8 o'clock in the morning till 5 o'clock in the afternoon, furnished $14\frac{34}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1.34$, or 266. June 17th, cloudy, with intervals of sunshine. The globe with about 4 hours sun gave $7\frac{34}{100}$ cubic inches of air, of a very eminent quality, viz. $1a + 4n = 1.40$, or 360.

The water having by degrees lost its transparency, and having acquired a deep green colour, it broke up this day, and deposited a green sediment; after which it recovered its transparency, and became almost colourless. It continued, notwithstanding, to furnish air in considerable quantities. June 18th, being exposed in the sun's rays from 8 o'clock in the morning till 2 o'clock in the afternoon, when the heavens became overcast, the globe yielded $6\frac{27}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1.44$, or 356. June the 19th and 20th, the globe furnished no more than $3\frac{13}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1.06$, or 294; after which it ceased totally to furnish air, and the colour of the water changed to a dead yellowish cast, and the cotton assumed the same hue.

The following are the quantities and qualities of the different parcels of air furnished in the course of this experiment.

	Quantity.		Quality.	
On the 10th of June	$1\frac{3}{4}$	cubic inches	$1a + 1n = 1.65$,	or 35
11th, 12th, and 13th.......	1	....	$1a + 1n = 2.$	or 0
14th.......	0	....	——	——
15th.......	$10\frac{42}{100}$	....	$1a + 3n = 1.43$,	or 257
16th.......	$14\frac{34}{100}$	....	$1a + 3n = 1.34$,	or 266
17th.......	$7\frac{34}{100}$	....	$1a + 4n = 1.40$,	or 360
18th.......	$6\frac{27}{100}$	....	$1a + 4n = 1.44$,	or 356
19th and 20th.......	$3\frac{13}{100}$	....	$1a + 3n = 1.06$,	or 294
Total quantity.	$44\frac{1}{4}$	Mean quality	$1a + 3n = 1.23$,	or 277

To ascertain, with greater precision, the qualities of the air furnished at different periods of the experiment, or rather the period when the water begins to give good air; and also to determine the relative quantities and qualities of the airs produced in the experiments with raw silk, and in those with poplar cotton, Sir B. made the following experiments, viz. *Exper.* 17, 18-25, with nearly similar results. On which he then remarks, that the water in several of these experiments had acquired a faint greenish cast; but the colour of that with the cotton was rather the deeper. On examining the water of the cotton under a microscope, I found it contained a great number of animalcules, exceedingly small, and of nearly a round figure. That with the silk contained the same kind of animalcules also, but not in so great abundance. I never failed to find them in every case in which the water used in an experiment had acquired a greenish hue; and from their presence alone, I think it more than probable, that the colour of the water, in the first instance, arose in all cases. I was yet by no means satisfied with respect to the part which the silk and other bodies, exposed in water in the foregoing experiments, acted in the purifying or dephlogisticating of the air produced. Dr. Priestley has long since discovered, that many animal and vegetable substances putrifying, or rather dissolving, in water, in the sun, cause the water to yield large quantities of dephlogisticated air; but I could hardly conceive, that the small quantity of silk which was used in my experiments, and which had been constantly in water for more than 3 months, and had so often been washed, and even boiled in water, should yet retain a power of communicating any thing to the large quantities of fresh water in which it was successively placed; at least any thing in sufficient quantities to impregnate those bodies of water, and to cause them to yield the great abundance of air which they produced.

It was still more difficult to account for the purification of the air in the experiments with wool and fur, and human hair; especially, as in some of these experiments the water had not sensibly changed colour, nor did it appear to have lost any thing of its transparency. It is true, in these cases, the quantities of air produced were very small; but yet its quality was better than that of common air, and considerably superior to that of the air existing in the water, previous to its being exposed to the action of the sun's light. In short, it was dephlogisticated in the experiment; but the manner in which this was done is very difficult to ascertain. With a view to throw some new light on this intricate subject, I made the following experiments.

Exper. 26. Concluding that if silk and other bodies, used in the foregoing experiments, actually did not contribute any thing, considered as chemical substances, in the process of the production of pure air yielded by water; but if, on the contrary, they acted merely as a mechanical aid in the separation of the air from the water, by affording a convenient surface for the air to attach itself to; in this case, any other body, having a large surface, and attracting air in water, might be used instead of silk in the experiment, and pure air would be furnished, though the body so employed should be totally incapable of communicating any thing whatever to the water.

To ascertain this fact, washing the great globe (containing 296 cubic inches) perfectly clean, and filling it with fresh spring water, I introduced into it a quantity of the fine flexible thread of glass, commonly called spun glass, such as is used for making brushes for cleaning jewels, and for making a kind of artificial feather frequently sold by the Jew pedlars. This spun glass is no other than common glass drawn out, when hot, into an exceeding fine thread; which thread, in consequence of its extreme fineness, retains its flexibility after it is cold. I made choice of this substance not only on account of its great surface, but also on account of the strong attraction which is known to subsist between glass and air, and the impossibility of its communicating any thing to the water.

The result of the experiment was, that the globe being exposed in the sun, air-bubbles began almost immediately to make their appearance on the surface of the spun glass, and in 4 hours $\frac{77}{100}$ of a cubic inch of air was collected, which, proved with nitrous air, gave $1a + 1n = 1.12$, or 88; after which, not a single air-bubble more was produced, though the globe was exposed a whole week in the window, during which time there were several very warm, fine, sunshiny days.

This experiment shows evidently, that something more is wanting to the production of pure air by water, exposed in the sun, than merely a surface to which the air dissolved in the water can attach itself, in order to its making its escape. The air furnished in this experiment was doubtless merely that with which the water issuing from the earth was overcharged, and which would have made its escape from the water, had this, instead of being exposed with the spun glass in the sun, been simply left for some time exposed to the free air of the atmosphere.

It appears that this air, naturally existing in spring water, instead of being dephlogisticated is something worse than common air; and this agrees with the observations of Dr. Priestley, and seems to justify his opinion with respect to the cause of the fertility of lands washed by waters issuing from the earth. If the above experiment shows that something is wanted to be mixed with water, in order to enable it to yield pure, when exposed to the action of the sun's light, the following show, that this something, whatever it may be, is frequently to be found in the water itself, in its natural state.

Exper. 27. A large jar of clear white glass, containing 455 cubic inches, being washed very clean, was filled with fresh spring water, and inverted in a glass basin of the same, and placed in the middle of the garden of the Elector's palace, where it was left exposed to the weather 28 days. At the same time another like jar was filled with water, taken from a pond in the garden, in which many aquatic plants were growing, and was exposed in the same place, and during the same period. This water had a very faint greenish cast. The pond from which it was taken is fed by a large river, the Isar, which runs by the town.

The 2d day after these waters had been exposed in the sun, a small quantity of air had collected itself at the upper part of each of the jars. The 3d, 4th, and 5th days, the pond water furnished air in pretty large quantities; and it went on to yield it without intermission, when the sun shone on it, till the 14th day, when it seemed to be nearly exhausted. I continued the experiment however till the 28th day, though during the last fortnight the quantity of air in the jar did not appear sensibly to be increased.

The spring water, during the first 5 or 6 days, furnished very little air: and it was not till the 14th day that it began to yield it in any considerable quantities. From this time it went on to furnish it, though but very slowly, till about the 22d day, when it ceased, appearing to be quite exhausted. On the 28th day I removed the airs from the jars, when I found their quantities and qualities to be as follow:

	Quantity.		Quality.
Air furnished by the spring water	14	cubic inches	$1a + 2n = 1.62$, or 138
by the pond water	$31\frac{1}{2}$	....	$1a + 3n = 1.48$, or 252

Neither the colour of the spring water, nor that of the pond water, appeared to be sensibly changed; but both had deposited a considerable quantity of earth, which was found adhering to the surfaces of the glass basins in which the jars were inverted. As these basins were rather deep, and as they were very thick in glass, and consequently not very transparent, their bottoms, where the sediment of the water was collected, were in a great measure obscured or deprived of the direct rays of the sun. Suspecting that this circumstance might have had some effect, so as to have hindered the water from furnishing so much air as otherwise it might have yielded, to satisfy myself respecting this matter I repeated the experiment, disposing the apparatus in such a manner, that the sediment of the water, which attached itself to the bottom of the vessel in which the jar was inverted, had the advantage of being perfectly illuminated.

Exper. 28. In a large cylindrical jar, of very fine transparent glass, 10 inches in diameter, and 12 inches high, filled with spring water, I inverted a conical glass jar, $9\frac{3}{4}$ inches in diameter at the bottom, and containing 344 cubic inches, filled with the same water; and exposed the whole 21 days, in a window fronting the south. The quantity of air produced amounted to 40 cubic inches; and its quality, proved by the test of nitrous air, gave $1a + 3n = 1.87$, or 213.

The water in this experiment furnished very little air till the 7th day; but after that time, having assumed a faint greenish cast, and a fine greenish slimy sediment (the green matter of Dr. Priestley) beginning to be formed on the bottom of the jar, it began to yield air in abundance, and continued to furnish it in pretty large quantities till about the 18th day, when it appeared to be exhausted. Why the water should turn green in this experiment, and not in the last, I know not; unless it was in consequence of the large surface of water in the cylindrical jar, which was exposed to the air in this experiment; or in consequence of the sun's shining directly on the bottom of the vessel where the sediment was formed.

P. S. Since writing the above, an interval of fine weather, and a moment of leisure, have given me an opportunity of making a few more experiments, of which I have thought it right to give a short account. And I must begin by observing, that having never been thoroughly satisfied with respect to the origin of the dephlogisticated air produced on exposing fresh vegetables in water to the action of the sun's rays, according to the method of Dr. Ingenhousz, my doubts, with respect to the opinion generally entertained of its being elaborated in the vessels of the plant, instead of being removed, were rather confirmed by the result of the foregoing experiments; and however disposed I was to adopt the beautiful theory of the purification of the atmosphere by the vegetable kingdom, I was not willing to admit a fact which has been brought in support of it, till it should appear to have been demonstrated by the most decisive experiments.

That the fresh leaves of certain vegetables, exposed in water to the action of the sun's rays, cause a certain quantity of pure air to be produced, is a fact which has been put beyond all doubt; but it does not appear to be by any means so clearly proved, that this air is "elaborated in the plant by the powers of vegetation;"—"phlogisticated or fixed air being first absorbed or imbibed by the plant as food, and the dephlogisticated air being rejected as an excrement:" for besides that many other substances, and in which no elaboration, or circulation, can possibly be suspected to take place, cause the water in which they are exposed to the action of light to yield dephlogisticated air as well as plants, and even in much greater quantities, and of a more eminent quality, the circumstances of the leaves of a vegetable, which, accustomed to grow in air, are separated from its stem, and confined in water, are so unnatural, that I cannot conceive, that they can perform the same functions in such different situations.

Among many facts which have been brought in support of the received opinion of the elaboration of the air in the vessels of the plants in the experiments in question, there is one on which great stress has been laid, which I think requires further examination. The fresh healthy leaves of vegetables, separated from the plant, and exposed in water to the action of the sun's rays, appear, by all the experiments which have hitherto been made, to furnish air only for a short time; after a day or two, the leaves changing colour, cease to yield air:

and this has been conceived to arise from the powers of vegetation being destroyed; or in other words, the death of the plant; and from hence it has been inferred, with some degree of plausibility, not only that the leaves actually retained their vegetative powers for some time after they were separated from their stock, but that it was in consequence of the exertion of these powers, that the air, yielded in the experiment, was produced.

But I have found, that though the leaves, exposed in water to the action of light, actually do cease to furnish air after a certain time, yet that they regain this power after a short interval, when they furnish (or rather cause the water to furnish) more and better air than at first, which can hardly be accounted for on the supposition that the air is elaborated in the vessels of the plant.

Exper. 29. A globe, containing 46 cubic inches, filled with fresh spring water and 2 peach leaves, was exposed in the window to the action of the sun's rays, 10 days successively, the weather being in general fine, when the following appearances took place. The 1st and 2d day, a certain quantity of air was produced, about as much as in former like experiments. The 3d day very little was produced; and the 4th day none at all, the globe to all appearance being quite exhausted. Continuing the experiment however, on the 5th day, the water having acquired a faint greenish hue, air was again produced pretty plentifully, making its appearance on the surface of the leaves in the form of air-bubbles, as at the beginning of the experiment; at the end of the 6th day the air was removed, and it was found to amount to $\frac{54}{100}$ of a cubic inch, its quality being 232 degrees, or $1a + 3n = 1.68$. On the 7th day $\frac{9}{10}$ of a cubic inch of air was produced of 297 degrees, or $1a + 3n = 1.03$; and during the 8th, 9th, and 10th days, $1\frac{3}{4}$ cubic inch of air, of 307 degrees (or $1a + 4n = 1.93$), was furnished; after which an end was put to the experiment. The total quantity of air produced $3\frac{19}{100}$ cubic inches; mean quality 291 degrees, or $1a + 3n = 1.09$.

Finding that leaves which were dead, or in which all the powers of vegetation were evidently destroyed, continued notwithstanding to separate air from water, and that in so great abundance, I was desirous of seeing the effect of exposing fresh healthy leaves in water which I knew to be previously saturated with, and disposed to yield dephlogisticated air. I conceived, that if the plants exposed in water actually imbibed fixed or phlogisticated air as food, and after digesting it, "discharged the dephlogisticated air as an excrement;" in that case, as there is no instance of any plant, or animal, being able to nourish itself with its own excrement, the leaves exposed in water saturated with dephlogisticated air, instead of imbibing and elaborating it, would immediately die. The experiments which I made to ascertain this fact, without any comment, were as follow.

Exper. 30. Having provided a quantity of water, which, by being exposed with a few green leaves in the sun, had acquired a greenish cast, and which I found was disposed to yield dephlogisticated air in great abundance, I filled a globe, containg 46 cubic inches, with this water, and putting to it two healthy peach leaves, exposed the globe in the sun on September 7, from 11 o'clock in the morning till 2 o'clock in the afternoon (3 hours), when $\frac{7}{10}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1.52$, or 248 degrees. A like globe, with fresh spring water and two peach leaves, exposed at the same time, furnished only $\frac{1}{9}$ of a cubic inch of air, which, on account of the smallness of its quantity, I did not submit to the test of nitrous air.

Exper. 31. Sept. 8, very fine clear weather, but rather cold for the season. Three equal globes, A, B, and C, containing each 46 cubic inches, were filled as follows, and exposed in the sun from 9 o'clock in the morning till half an hour past 4 in the afternoon, when they were found to have produced air as undermentioned.

The globe A, filled with water, which, by being previously exposed in the sun for several days, with potatoes cut in thin slices, had turned green, furnished $\frac{9}{10}$ of a cubic inch of air of 299 degrees, or $1a + 3n = 1.01$. N. B. This water, before it was put into the globe, was strained through two thicknesses of very fine Irish linen. The globe B, filled with the same green potatoe water, strained as before, to which were added 4 middling-sized peach leaves, furnished $2\frac{1}{2}$ cubic inches of air of 320 degrees, or $1a + 4n = 1.80$. The globe C, filled with fresh spring water, with 4 peach leaves, furnished $\frac{52}{100}$ of a cubic inch of air of 151 degrees, or which, proved with nitrous air, gave $1a + 2n = 1.49$.

To ascertain the quantities and qualities of the airs remaining in the different waters used in this experiment, putting the globes separately over a chafing-dish of live coals, and making the water boil, taking care to hold the globe in such an inclined position as that the air separated from the water might be collected in the upper part of the globe, the airs produced were as follow.

	Quantity.		Quality.
By the green water in the globe A	$\frac{85}{100}$	of a cubic inch	280 degrees.
By the green water in the globe B	$\frac{31}{100}$	....	241
By the spring water in the globe C	$\frac{11}{100}$	....	68

The waters in these experiments were made to boil but for a moment; otherwise, it is probable, more air might have been separated from them. Finding that fresh leaves, exposed to the action of the sun's rays, in water which had already turned green, caused pure air to be separated from the water in so great abundance, I repeated the experiment, only, instead of leaves, I now made use of a small quantity of conferva rivularis; when I had nearly the same result as with the leaves. To ascertain the relative quantities and qualities of the airs yielded by the green water, when exposed with fresh leaves, and when exposed with raw silk; and also to ascertain, at the same time, how long leaves, exposed in green water, retain their power of separating from it, I made,

Exper. 32. Two equal globes, A and B, containing 46 cubic inches, the former A, filled with green potatoe water, strained through linen, and 4 peach leaves; the latter B filled with the same potatoe water, strained in like manner, and 17 grains of raw silk, were exposed from Sunday noon, Sept. 10th, till Monday evening, the weather being cold, with many flying clouds, in all about 6 or 7 hours sun. The airs produced were as follow.

	Quantity.		Quality.
By the globe A, with green water and 4 peach leaves	$2\frac{7}{10}$	cubic inches	292 deg.
By the globe B, with green water and 17 grs. of raw silk	$2\frac{7}{10}$	....	307

Another globe C, filled with green water alone, was exposed at the same time, but it was broken by an accident before the experiment was completed. The two globes, A and B, with their contents, being again exposed from Tuesday noon till Thursday evening, yielded air as follows.

	Quantity.		Quality.
The globe A, with the peach leaves	$4\frac{47}{100}$	cubic inches	344 degrees.
The globe B, with raw silk	$4\frac{3}{10}$	....	350

The weather on Tuesday and Wednesday was cold, with very little sunshine; but Thursday was a very fine warm day, when the greatest part of the air was produced. This air was removed and proved on Friday morning Sept. 15.

Perhaps all the appearances above described might be satisfactorily accounted for, by supposing the air produced in the different experiments to have been generated in the mass of water by the green matter; and that the leaves, the silk, &c. did no more than assist it in making its escape, by affording it a convenient surface to which it could attach itself, in order to its collecting itself together, and taking on itself its elastic form. The phenomena might also be accounted for by supposing the green matter to be a vegetable substance, agreeable to the hypothesis of Dr. Priestley, and that attaching itself to the surfaces of the bodies exposed in the water, as a plant is attached to its soil, it grows; and, in consequence of the exertion of its vegetative powers, the air yielded in the experiment is produced.

I should most readily have adopted this opinion, had not a most careful and attentive examination of the green water, under a most excellent microscope, at the time when it appeared to be most disposed to yield pure air in abundance, convinced me, that, at that period, it contains nothing that can possibly be supposed to be of a vegetable nature. The colouring matter of the water is evidently of an animal nature, being nothing more than the assemblage of an infinite number of very small, active, oval-formed animalcules, without any thing resembling tremella, or that kind of green matter, or water moss, which forms on the bottom and sides of the vessel when this water is suffered to remain in it for a considerable time, and into which Dr. Ingenhousz supposes the animalcules above-mentioned to be actually transformed.

XVI. Discovery of Two Satellites revolving round the Georgian Planet. By Wm. Herschel, LL. D., F. R. S. p. 125.

In the beginning of last month (Jan. 1787) I was often surprised when I reviewed nebulæ that had been seen in former sweeps, to find how much brighter they appeared, and with how much greater facility I saw them. The cause of it could be no other than the quantity of light that was gained by laying aside the small speculum, and introducing the front view. It would not have been pardonable to neglect such an advantage, when there was a particular object in view, where an accession of light was of the utmost consequence. The 11th of January therefore, in the course of my general review of the heavens, I selected a sweep which led to the Georgian planet; and while it passed the meridian I perceived near its disc, and within a few of its diameters, some very faint stars whose places I noted down with great care.

The next day, when the planet returned to the meridian, I looked with a most scrutinizing eye for my small stars, and perceived that two of them were missing. Had I been less acquainted with optical deceptions, I should immediately have announced the existence of one or more satellites to our new planet; but it was necessary that I should have no doubts. The least haziness, otherwise imperceptible, may often obscure small stars; and I judged therefore that nothing less than a series of observations ought to satisfy me, in a case of this importance. To this end I noticed all the small stars that were near the planet the 14th, 17th, 18th, and 24th of January, and the 4th and 5th of February; and though at the end of this time I had no longer any doubt of the existence of at least one satellite, I thought it right to defer this communication till I could have an opportunity of seeing it actually in motion. Accordingly I began to pursue this satellite on Feb. the 7th, about 6 o'clock in the evening, and kept it in view till 3 in the morning on Feb. the 8th; at which time, on account of the situation of my house, which intercepts a view of part of the ecliptic, I was obliged to give over the chace: and during those 9 hours I saw this satellite faithfully attend its primary planet, and at the same time keep on, in its own course, by describing a considerable arch of its proper orbit.

While chiefly attending to the motion of this satellite, I did not forget to follow another small star, which I was pretty well assured was also a satellite, especially as I had, on the night of the 14th of January, observed 2 small stars which were wanting on the 17th, and again missed other 2 the 24th which had been noticed the 18th; but, whether owing to my great attention to the former satellite, or to the closeness of this latter, which was nearly hidden in the rays of the planet, I could not be well assured of its motion. The first moment that offered for continuing these observations was on Feb. the 9th, when I saw my first discovered satellite nearly in the place where I expected to find it. I perceived also, that the next supposed satellite was not in the situation where I had left it on the 7th, and could now distinguish very plainly that it had advanced in its orbit, since that day, in the same direction with the other satellite, but at a quicker rate. Hence it is evident, that it moves in a more contracted orbit; and I shall therefore call it in future the first satellite, though last discovered, or rather last ascertained; since I do not doubt but that I saw them both, for the first time, on the same day, Jan. 11th, 1787.

I now directed all my attention to the first satellite, and had an opportunity to see it for about $3\frac{1}{4}$ hours; during which time, as far as one might judge, it preserved its course. The interval which the cloudy weather had afforded was however rather too short for seeing its motion sufficiently, so that I deferred a final judgment till the 10th; and, in order to put my theory of these 2 satellites to a trial, I made a sketch on paper, to point out before-hand their situation with respect to the planet, and its parallel of declination. The long expected evening came on, and, notwithstanding the most unfavourable appearance of dark weather, it cleared up at last. And the heavens now displayed the original of my drawing, by showing, in the situation I had delineated them, the Georgian planet attended by 2 satellites. For upwards of 5 hours I saw them go on together, each pursuing its own track; and I left them situated, about 2 o'clock in the morning on February the 11th, as represented in a figure then drawn of their appearance.

I have not seen them long enough to assign their periodical times with great accuracy; but suppose that the first performs a synodical revolution in about $8\frac{3}{4}$ days, and the 2d in nearly $13\frac{1}{2}$ days. Their orbits make a considerable angle with the ecliptic; but to assign the real quantity of this inclination, with many other particulars, will require a great deal of attention, and much contrivance: for, as estimations by the eye cannot but be extremely fallacious, I do not expect to give a good account of their orbits till I can bring some of my micrometers to bear on them; which, these last nights, I have in vain attempted, their light being so feeble as not to suffer the least illumination, and that of the planet not being strong enough to render the small silk-worm's threads of my delicate micrometers visible. I have however several resources in view, and do not despair of succeeding pretty well in the end.

XVII. Remarks on Mr. Brydone's Account of a Remarkable Thunder Storm in Scotland. By the Rt. Hon. Charles Earl Stanhope, F. R. S. p. 130.

No storm of lightning has ever produced effects more curious to contemplate than those related by Mr. Brydone, in his letter to the president of this Society. That account contains facts of such consequence, and so perfectly inexplicable by the commonly received principles of electricity, that it undoubtedly deserves particular attention. It appears, that a man, James Lauder, sitting on the fore part of a cart drawn by 2 horses, was suddenly struck dead, as also the horses that he was driving, and that the cart itself was much injured by electrical fire, though no lightning fell at or near the place where this accident happened.

Now few facts of this kind have ever been better authenticated than this is. It appears first, that a man, who was sitting on the fore-part of another cart, only 24 yards behind the cart that was struck, "had Lauder, his cart and horses, full in view when they fell; he was stunned by a loud report, and saw his companion, his horses and cart, fall to the ground; he immediately ran to his assistance, but found him quite dead; he perceived no flash or appearance of fire." It also appears, that another man, a shepherd of St. Cuthbert's farm, was also a witness of this event. He was distant from Lauder "between 2 and 300 yards, and was looking at the 2 carts, when he was stunned by a loud report, and at the same instant saw the first of the carts fall to the ground. He saw no lightning, nor appearance of fire whatever." The concurrent testimony of these two men is confirmed by Patrick Brydone, Esq. who lives at a small distance from the spot where Lauder was killed; and Mr. Brydone relates, that a storm appeared far off; and that he, and some company in his house, were "suddenly alarmed by a loud report, for which they were not prepared by any preceding flash." There is the greater weight to be given to this account of Mr. Brydone, as it so happened, that he was just then "observing the progress of the storm, at an open window, in the 2d story of his house," and making the company "observe, by a stop-watch, the time that the sound took to reach them."

That the death of Lauder and of the horses was not occasioned by any direct main stroke of explosion from a thunder-cloud, either positively or negatively electrified, Lord S. thinks is evident; since no lightning whatever passed from the clouds to the earth, or from the earth to the clouds, at the place where they were killed. It is equally evident, and for the very same reason, that they were not deprived of life by any transmitted main stroke of explosion, either positive or negative. It is also obvious, he adds, that the lateral explosion was not the cause of this mischief; for the lateral explosion does always proceed immediately from the main stroke itself; and therefore there can exist no lateral explosion, in the case when there is no main stroke whatever.

Lord S. thinks, from the different circumstances of this case, that the effects produced proceeded from electricity; and that no electrical fire did pass immediately either from the clouds into the cart, &c. or from the cart, &c. into the clouds. From the circular holes in the ground, of about 20 inches in diameter, the respective centres of which were exactly in the track of each wheel, and the corresponding marks of fusion on the iron of the wheels, which marks answered exactly to the centre of each of the holes; it is evident, he says, that the electrical fire did pass, from the earth to the cart, or from the cart to the earth, through that part of the iron of the wheels which was in contact with the ground. From the splinters that had been thrown off, in many places, particularly where the timber of the cart was connected by nails or cramps of iron, and from the various other effects mentioned in Mr. Brydone's paper, it is further evident, that there was a violent motion of the electrical fluid in all, or at least in different parts of the cart, and of the bodies of the man and horses, though there was no lightning.

Wonderful as these combined facts may appear, and uncommon as they certainly are in this country, they are however easy to be explained by means of that particular species of electrical shock, which I have distinguished in my Principles of Electricity, published in 1779, by the appellation of the "electrical returning stroke:" and though at the time I wrote that Treatise, I had it not in my power to produce any instance of persons or animals having been killed in the very peculiar manner since related in Mr. Brydone's paper; I did however, from my experiments mentioned in that book, venture to assert, with confidence, that, "if persons be strongly superinduced by the electrical atmosphere of a cloud, they may, under circumstances similar to those explained in that treatise, receive a very strong shock, be knocked down, or be even killed, at the instant that the cloud discharges, with an explosion, its electricity, whether the lightning falls near the very place where those persons are, or at a very considerable distance from that place, or whether the cloud be positively or negatively electrified."

And I further stated that, "whether the distance between the person so circumstanced, and the place where the lightning falls, be 50 or 100 yards, or 1 mile, or 2 miles, or 3 miles, or more, the truth of the general proposition there laid down would not be anywise affected." I have also explained in that treatise how a still more singular effect might be produced, namely, how "an explosion, which happens in one place, may cause in a 2d place, at a very considerable distance from the first place, a sudden returning stroke, which may knock down, or even kill, persons and animals at that 2d place; at the same time that other persons, or other animals, situated in a 3d place, that is even immediately between the first place where the lightning falls, and the 2d place, just mentioned, where the shock of the returning stroke happens, shall receive no detriment whatever."

But, before speaking of the accident of Lauder, which appears to have been occasioned by a returning stroke, proceeding from an assemblage of clouds, I will say a few words on one or two other facts, mentioned in Mr. Brydone's account. Mr. Brydone informs us, that "the shepherd belonging to the farm of Lennel-hill was in a neighbouring field, when he observed a lamb, only a few yards from him, drop down, though the lightning and claps of thunder were then at a great distance from him. He ran up immediately, but found the lamb quite dead; nor did he perceive the least convulsive motion, or symptom of life remaining, though the moment before it appeared to be in perfect health." This effect is so precisely similar to those explained in my Principles of Electricity, and particularly to that mentioned in section 328, that it is quite unnecessary to enlarge on it. I shall only observe, that such an electrical returning stroke as that by which this lamb was destroyed, namely, a returning stroke which happens at a place where there is neither lightning nor thunder near, belongs to the most simple class of returning strokes; and that it may be produced by the sudden removal of the elastic electrical pressure of the electrical atmosphere of a single main cloud, as well as by that of an assemblage of clouds. It appears by Mr. Brydone's account, that the shepherd, who saw the lamb fall, was near enough to it to feel, in a small degree, the electrical returning stroke at the same time that the lamb dropped down.

Mr. Brydone further relates, that "a woman making hay near the banks of the river fell suddenly to the ground; and called out to her companions, that she had received a violent blow on the foot, and could not imagine from whence it came." This blow was, unquestionably, the electrical returning stroke. When a person, walking or standing out of doors, is knocked down or killed by the returning stroke, the electrical fire must rush in, or rush out, as the case may be, through that person's feet, and through them only; which would not be the case, were the person to be knocked down or killed by any main stroke of explosion, either positive or negative.

Lord S. then proceeds to explain, from the returning stroke, described in his Principles of Electricity, how the chief effects mentioned in Mr. Brydone's account may probably have been produced; viz. the death of the man and horses, with the dispersion of parts of the cart, and the marks on the wheels, &c.

XVIII. Concerning the Latitude and Longitude of the Royal Observatory at Greenwich; with Remarks on a Memorial of the late M. Cassini de Thury. By the Rev. Nevil Maskelyne, D. D., F. R. S., &c. p. 151.

Memoire sur la jonction de Douvres à Londres. Par M. Cassini de Thury, Directeur de l'Observatoire Royal; de la Société Royale de Londres, &c.

Il est intéressant pour le progrès de l'astronomie que l'on connaisse exactement la différence de longitude et de latitude entre les deux plus fameux observatories de l'Europe; et quoique les observations astronomiques faites depuis un siècle offrent un moyen assez exact pour parvenir à cette recherche, il parait cependant que l'on n'est point d'accord sur la longitude de Greenwich à onze secondes près, et sur sa latitude à quinze secondes. L'on a reconnu par les opérations trigonométriques exécutées en France, au Nord, et au Perou, que sur l'étendue d'un degré du méridien ou de 57 mille toises, l'on se trompait à peine de dix toises, ce qui a été prouvé par des bases mesurées à l'extrémité des suites de triangles; ainsi sur la distance de Douvres à Londres, qui est de 49800 toises ou environ, on ne pourrait se tromper de 120 toises, qui repondent à onze secondes en longitude. M. Cassini a déjà publié, dans le livre de la Méridienne Vérifiée, les opérations par lesquelles l'on a déterminé la distance de Calais à la grosse tour de Douvres de 18241 toises par un premier triangle, et de 18243 toises par un second triangle; on aurait cette distance avec une plus grande exactitude en observant les angles conclus à Douvres, qui sont fort aigus. M. Cassini a découvert des côtes de France plusieurs objets sur les côtes d'Angleterre, qni seront visibles de la tour de Douvres; et sur cette première base on établirait une suite de quelques triangles jusqu'à Londres, dont le nombre et la grandeur dépendent de l'exposition des objets compris dans la direction de Douvres à Londres. M. Cassini ne doute point que ce projet ne soit agréé d'un Souverain qui aime les sciences, qui non content des découvertes du celèbre Cook vient d'ordonner un second voyage autour du monde, et que la Société Royale ne charge un de ses membres de l'exécution; et dans le cas où ses occupations l'empêcheraient de s'y livrer, qu'elle ne permît à M. Cassini de s'en charger. L'honneur qu'elle lui a fait de l'associer à un corps aussi respectable serait un titre pour lui accorder sa confiance. M. Cassini a profité du voyage du Roi en Flandres en 1748 pour joindre les triangles de la méridienne à ceux de Snellius en Hollande; en 1762 il a prolongé la perpendiculaire de Paris jusqu'à Vienne en Autriche. La branche qui s'étendra jusqu'à Londres sera la troisième, et formera la jonction des deux plus belles villes de l'Europe.

The preceding memorial of the late M. Cassini de Thury was put into my hands by Sir Joseph Banks, our president, on the 28th of April, 1785, desiring me at the same time to give an answer to it. Happy if I can solve the doubts entertained by the late royal astronomer of France concerning the latitude and longitude of this Royal Observatory, and at the same time do justice to the memories of my learned predecessors, and to myself, I shall give an account of the principal operations that have been performed here for ascertaining those points, and then add my own remarks to elucidate the subject and reconcile the difficulties in question.

Had Dr. Bradley lived longer, for the benefit of astronomy, to publish his valuable observations; or had they been since published by another hand, which unfortunately they hitherto have not; these remarks might have been unnecessary, and perhaps even the occasion for them might never have occurred; as it would have then appeared on what foundation the latitude of this observatory had been established, and what differences of meridians between Greenwich and the other principal observatories of Europe, resulted from the observed eclipses of Jupiter's satellites and other celestial phenomena. However, having formerly been apprised by Dr. Bradley himself, of several particulars of moment relative to his observations, and particularly of the method which he used for settling his latitude and refractions, after he became possessed of the new instruments in 1750, and being assisted with some of his manuscript calculations, with the addition of my own observations, I flatter myself I can throw the light wanted on the question, and obviate the principal difficulty, that relative to the difference of latitude of Greenwich and Paris, and reduce the difference of meridians within smaller limits, notwithstanding Dr. Bradley's original observations had been removed from this observatory, in which they were made, before I came here, and have not yet been restored to it.

Dr. Bradley having been furnished by government, in the year 1750, with a brass mural quadrant of 8 feet radius, constructed by that excellent artist Mr. John Bird, an instrument far superior to any before used in the practice of astronomy, assiduously observed with it the pole star, and other stars lying to the north of the zenith, for upwards of 3 years; and then removed it to the opposite side of the wall, making it change place with the iron quadrant of the same radius constructed by Mr. Graham, also an excellent instrument, though inferior to this, and commenced a regular series of observations of the sun, planets, and fixed stars, which have been ever since continued in the same manner. Moreover, the temperature of the air, shown by the barometer and thermometer, is affixed to each observation; and the zenith point of the quadrant settled from time to time, by the help of a zenith sector of $12\frac{1}{2}$ feet radius, turned alternately contrary ways, the same with which Dr. Bradley had before made his two useful and admirable discoveries of the aberration of light and the nutation of the earth's axis.

By the observations of the pole star and other circumpolar stars, above and below the pole, Dr. Bradley got the apparent zenith distance of the pole; by the apparent and equal zenith distances of the sun at the two equinoxes, having at the same time opposite right ascensions, as found from comparing his observed transits over the meridian with those of fixed stars, after the manner used by Mr. Flamsteed for deducing the right ascensions of the fixed stars, he found the apparent zenith distance of the equator, which lessened by parallax and added to the apparent zenith distance of the pole, gave a sum less than 90° by the sum of the two refractions belonging to the pole and meridian zenith distance of the equator. But he remarked, that the difference of refractions, belonging to these zenith distances, would come out the same within 2 or 3″ by any of the best tables then extant, whether deduced solely from observations, or partly from observations and partly from theory. The sum and difference of refractions answering to the pole and equator being thus given, the refractions themselves are given, the greater of which added to the apparent zenith distance of the equator, gives the latitude of the place, and the less refraction added to the apparent zenith distance of the pole, gives the co-latitude.

He afterwards, from the consideration that the refractions at the pole and equator may be taken without sensible error as the tangents of the zenith distances, according to Mr. Thomas Simpson's theory of refractions in his Mathematical Dissertations, divided more accurately the sum of the refractions at the pole and the equator into the just parts answering to each zenith distance, and thereby found the latitude with more exactness. In this manner he found the latitude of the Royal Observatory to be 51° 28′ 39½″, and the mean refraction at 45° 3′ to be 57″, the barometer standing at 29.6 inches, and the thermometer of Fahrenheit's scale at 50°. But, not to let a matter of so much consequence rest on my assertion or memory, when further proof can be given of it, I have by me, in the hand-writing of Dr. Bradley, among other particulars, his calculations of the latitude of the observatory from his observations, according to the manner above explained; in which he first states it at 51° 28′ 38″, and finally more correctly in these words. "The apparent zenith distance of the equator, by the mean of 20 observations in 1746-47, 51° 27′ 28″. The mean apparent distance of the pole, by the observations made between 1750-52, 38° 30′ 35″. Sum 89° 58′ 3″. Sum of refractions 1′ 57″. Polar refraction 45½″. Equatorial refraction 1′ 11½″. Latitude 51° 28′ 39½″. Co-latitude 38° 31′ 20½″."

The latitude of the observatory being thus settled, as well as the quantity of refractions for all stars passing the meridian between the pole and the equator, Dr. Bradley readily inferred from his observations the true distance of all such stars from the north pole, which, compared with their zenith distances observed below the pole, gave the refractions at those lower altitudes. Finally, by comparing the refractions together observed in extreme degrees of heat and cold, he deduced the law of their variation as affected by heat and cold; and thus at length he inferred his elegant rule for determining the refraction in all circumstances, that it is to 57″, in the direct compound ratio of the tangent of the apparent zenith distance lessened by 3 times the refraction, to the radius, and of the height of the barometer in inches to 29.6 inches, and in the inverse ratio of the degree of height of Fahrenheit's thermometer increased by 350, to 400.

But it may be proper to confirm this rule for refractions also from the same manuscript of Dr. Bradley, which I before cited for confirming the latitude, by the following passage, which immediately follows the other. "Suppose the mean refraction at $45° 3' = 57''$, and $y = 350$; then $y + t$: bar. :: $77''$: refr. at $45° 3'$. . . . . Rad. : tan. ZD :: $3'$: m . . . . . rad. : tan. ZD $- m$ :: refr. at $45° 3'$: r the refraction required." It is easy to see that this rule agrees with the other; for putting $t = 50$, and barometer $= 29.6$, the first analogy, putting the barometer down in tenths of an inch, is $350 + 50 = 400 : 296 :: 77'' : 56''.98$ for the refraction at $45° 3'$, or $57''$ within $\frac{1}{500}$ of a second. The second analogy serves to give the treble refraction nearly, called m. Whence it is evident that the last analogy coincides with the rule above given.

This valuable rule was first communicated by myself to the public in vol. 54 of the Philos. Trans., p. 265, and in p. 49 and 129 of the first edition of tables requisite to be used with the Nautical Almanac, together with a table of the mean refractions deduced from it, with the first Nautical Almanac, that of 1767, published by order of the commissioners of longitude in 1766; and again, at page the 5th of the Explanation and Use of the Astronomical Tables, annexed to the first volume of my Observations made at the Royal Observatory from 1765 to 1774, published by order of the president and council of the R. S., with two tables in that work, containing the mean refractions and decimal multipliers for reducing them to any given temperature of the air indicated by the barometer and thermometer. The words in page the 5th of the said preface are as follow. "The astronomical refractions and latitude of the observatory were settled with the greatest accuracy by Dr. Bradley, from his observations of the circumpolar stars, with the brass mural quadrant, during the 3 years that it was turned to the north, and of the sun and stars in the subsequent years after it was removed to point to the south. The following elegant rule was the result of his observations, that the refraction at any altitude is to 57 seconds, in the direct compound ratio of the tangent of the apparent zenith distance lessened by 3 times the refraction, to the radius, and of the altitude of the barometer in inches to 29.6 inches, and in the reciprocal ratio of the height of Fahrenheit's thermometer increased by the number 350, to the number 400. Tables 22 and 23 were adapted to this rule; the first containing the mean refractions answering to 29.6 inches height of the barometer and 50 degrees height of the thermometer; and the 2d table containing decimals for multiplying the mean refraction in order to find the correction, which applied to it will give the actual refraction, the same as would have been produced by the rule with somewhat more trouble. Dr. Bradley supposed the horizontal parallax of the sun $10\frac{1}{3}$ seconds, in the calculations from which he inferred the refractions; and I have been informed, that he determined the latitude of the Observatory $51° 28' 39\frac{1}{2}''$. But, had he made use of the true parallax 8″8 or 8¾″, as found by the two late transits of Venus over the sun, he would have made the refraction at the altitude of 45° to be 56½″ instead of 57″, and the latitude of the observatory exactly 51° 28′ 40″ instead of 51° 28′ 39½″. But his rule for refractions cannot be corrected for all altitudes, without examining his observations of refractions made at various times."

On comparing this extract with M. Cassini's memoir, I cannot but express my surprise, that he should not have adverted to a passage containing so direct an application to the grounds of his memorial, in a publication of such notoriety, and of so old a date as 1776; had he done so, I cannot but think he would never have hazarded such an opinion as that advanced by him in his memoir, of an uncertainty of 15″ in the latitude of Greenwich; but he might have been induced to believe, that the latitude of this place had been well determined.

For further confirmation of the certainty of the astronomical refractions, and latitude of the observatory, as settled by Dr. Bradley, it may be proper to add, that the Greenwich brass mural quadrant underwent a trial, which all astronomical instruments ought to be submitted to, but which very few ever have been, on account of the difficulty and nicety of the operation, namely, an examination of the total arc; when it was found by Dr. Bradley to be an accurate quadrant, the arc appearing at one trial to differ only a fraction of a second from 90°, and another time, after an interval of above 6 years, to be a perfect quadrant. See p. 24 of Bird's Method of constructing Mural Quadrants, published by the Board of Longitude in 1768. In like manner he had before examined the total arc of the iron quadrant, first put up by Mr. Graham, for the use of Dr. Halley, in the year 1725, by means of a level, and found it to be 16″ less than a quadrant. See Bird's Method of constructing Mural Quadrants, p. 7, and Memoires of the Royal Academy of Sciences at Paris, for 1752, p. 424. But this quadrant was, in the year 1753, re-divided by Mr. Bird, and in this respect probably rendered as accurate as the other. See Bird's Method of constructing Mural Quadrants, p. 24.

Dr. Bradley made a curious use of the new set of divisions, soon after they were laid on the quadrant, to re-examine the error of the total arc laid down originally by Mr. Graham (which by the plumb-line and level he had found to be 16″ less than a quadrant in 1745) according to the following passage contained in the manuscript before cited. "August 12, 1753, I measured with the screw of my micrometer the difference of the arcs (of $\frac{64}{96}$) as set off by Mr. Graham originally, and by Mr. Bird when he put on a new set of divisions on the old quadrant, and I found that Mr. Graham's arc was less than Mr. Bird's by $\frac{8}{40}$ divisions of my micrometer, which to a radius of 96 inches answers to 10″.6; so that the whole arc of 96 differs from a true quadrant 15″.9, which is the same difference that I formerly found by means of the level, &c."

Let me further add, that Dr. Bradley had informed me, that he had found the same refractions, and latitude of the Observatory, and obliquity of the ecliptic, by both quadrants, making a proportionable allowance, in the use of the iron quadrant, for the error of 16″ in the total arc in proportion to the zenith distance of the object before it was new divided. The Rev. Dr. Hornsby, F. R. S. Savilian Professor of Astronomy at Oxford, to whose care Dr. Bradley's original observations have been committed, in order to their being printed and published, having favoured me with calculations of the latitude of the Royal Observatory from observations of the pole star made with both quadrants, from a manuscript of Dr. Bradley, I think it proper to give it a place here, not only as a very curious paper, but also as strongly confirming the latitude of this place before stated.

Transcribed from a loose Paper of Dr. Bradley.—" The mean zenith distances of the pole star above and below the pole, corrected by refraction, aberration, &c. and reduced to January 1751, O. S. as collected from the observations made after the new quadrant was balanced, Nov. 24, 1750."

Number of observations.	Above the pole.	Number of observations.	Below the pole.	to
23	36° 29′ 46.66″	8	40° 33′ 2.95″	Jan. 15, 1751
22	45.42	26	3.37	Aug. 27, 1751
23	45.13	27	2.14	May 4, 1752
19	44.63	28	1.67	Nov. 10, 1752
28	45.00	23	1.74	May 31, 1753
9	44.59	8	1.54	July 26, 1753
124	36 29 45.24	120	40 33 2.24	
			36 29 45.24	
			77 2 47.48	
			38 31 23.74	
		Error of collimation	1.74	
		Co-latitude	38 31 22. 0	

	77° 1′ 15.8″			
	38 30 37.9	Refraction.		
Collimation	1.4	42″ +	36° 29′ 2½″	4° 3′ 10.8″
Refraction	+ 45.5	49 −	40 32 13½	+ 6.6
	38 31 22.0	6½	4 3 11	4 3 17.4
By new quad.	51 28 38.0	Refraction	+ 6½	
			4 3 17½	
			2 1 39 — the mean distance from mean pole Jan. 1751.	

The apparent zenith distance of the pole, by the mean of 310 observations, is

	°	′	″			°	′	″	
	38	30	36	allowing — 2″ for the error of the line of collimation.					
Refraction		+	45½						
	38	31	21½		Co-lat.	38	31	21½	by the new quadrant.
Latitude	51	28	38½			38	31	18½	by the old quad. new divisions.

Apparent Zenith distances of the Pole observed with the Iron Quadrant.

1753.		Apparent zenith distance of the pole.			Barom.	Thermometer. in.	Thermometer. out.
		°	′	″	Inches.	°	°
Sept.	13	38	30	40.1	30.10	64	65
	23			41.5	29.88	61	60
Oct.	2			42.5	29.67	56	53
	5			41.0	30.02	57	57
	11			41.1	29.48	61	57
	19			40.2	29.82	50	43
	31			40.8	29.65	42	34
Nov.	5			42.1	29.69	45	39
	16			40.5	29.39	42	35
	19			42.2	29.59	40	35
	20			40.9	29.84	40	33
	24			40.0	30.00	43	35
	29			40.0	29.80	38	30
Dec	3			39.3	30.16	39	33
	8			37.4	30.06	35	27
	17			41.5	29.50	50	49
	30			38.0	30.11	33	22
Mean		38	30	40.5	29.81	47	41½
Refraction			+	46.4			
Col.			—	8.4			
Pol. corr.		38	31	18.5			

So far the manuscript.

Thus the latitude by the brass quadrant being 51° 28′ 38½″, and by the iron quadrant with new divisions 51° 28′ 41½″, the mean by both quadrants is 51° 28′ 40″, or only half a second greater than settled in another manner, according to the manuscript of Dr. Bradley in my possession. Also the apparent zenith distance of the pole with the mean refraction 45″.4 being 38° 30′ 36″.1 by the brass quadrant, and 38° 30′ 33″.1, by the iron quadrant, the mean by both is 38° 30′ 34″.6, or only $\frac{4}{10}$ths of a second less than by Dr. Bradley's manuscript cited before.

On my promotion to the Royal Observatory in 1765, finding its latitude to have been so accurately settled by Dr. Bradley before me, I might have thought myself dispensed from making any particular or very laborious observations for that purpose; however, I confirmed it by my own observations to great nearness, viz. within 1 or 2″, at the same time that I was establishing a new catalogue of the principal fixed stars, continually observed here for settling the right ascensions of all other celestial objects with the transit instrument. The result in brief is as follows: I first settled the relative right ascensions of about 30 of the brightest fixed stars, and lying nearest the equator, by a great number of observations with the transit instrument, referring them to α Aquilæ as the fundamental star, whose right ascension I assumed from Dr. Bradley's determination. Hence, by observed transits of the sun and the same stars, in the spring and autumn, when his daily motion in declination was at least 16′ or $\frac{2}{3}$ of the greatest, I inferred the sun's right ascensions relative to the right ascensions of those stars settled in the manner just mentioned. Also from the sun's observed zenith distances taken with the brass mural quadrant on the same days, and corrected by refraction, parallax, and error of line of collimation, with Dr. Bradley's obliquity of the ecliptic, and latitude of the observatory, I computed the sun's declinations, and thence the right ascensions corresponding to them.

Now, if the assumed right ascension of α Aquilæ, and thence those of the other stars were affected with some small error, as might be supposed, the sun's right ascensions deduced from the observed transits would differ the same way from the truth at both seasons of the year, viz. by the unknown error of the assumed right ascension of α Aquilæ; but his right ascensions inferred from his observed zenith distances would be affected contrary ways at the two opposite seasons of the year, by the unknown errors in the refractions, parallaxes, latitude of the place, and obliquity of the ecliptic. Hence, the mean of the two corrections of the sun's right ascension, found from the observed declinations about the vernal and autumnal equinox, would be the true correction of the assumed right ascension of α Aquilæ; and the difference of the same corrections would, by an easy calculation, show how much the computed declinations were too great or too little for the truth, and consequently what the true declinations were, and what the true zenith distance of the sun was, when in the equator, or the latitude of the place, on supposition that Dr. Bradley's refractions were truly stated; for any small uncertainty in the obliquity of the ecliptic, as stated by him, could not affect this result, which was deduced equally from observations of the sun in north and south declination, when the same error of the obliquity would affect the sun's right ascensions deduced from the observed declinations contrary ways. I took the sun's parallax from the 24th of my tables annexed to my observations, constructed on a horizontal parallax 8″.84, which I had deduced from the observations of the first transit of Venus, that in 1761, and differing insensibly from $8\frac{3}{4}''$, which I deduced from the observations of the total durations of the transit between the internal contacts observed at Wardhus and Otaheite in 1769, consequently more correct than the horizontal parallax of $10\frac{1}{3}''$ used by Dr. Bradley. It is also evident, that the true zenith distance of the equator thus found, diminished by Dr. Bradley's mean refraction, will be the apparent zenith distance of the equator, affected only by the mean refraction.

I shall now give the apparent zenith distance of the equator, and the true latitude of the observatory, resulting in this manner from my observations of 6 years, from the autumnal equinox of 1765 to that of 1771, in which I allowed 0″.9 for the correction of the error of the line of collimation, additive to the observed zenith distances, as I found from a revision of my calculations of the zenith distances of stars taken with the mural quadrant, compared with the like taken with the zenith sector in 1768.

Years of the observations.	N° of days of observations.	Apparent zenith distance of equator, taking the sun's horizontal parallax 8″.8.			True latitude of the Observatory, according to Dr. Bradley's refractions and the sun's horizontal parallax 8″.8.		
		°	′	″	°	′	″
Autumnal equinox of 1765 and vernal of 1766	52	51	27	28.6	51	28	40.1
Autumnal equinox of 1766 and vernal of 1767	38			30.4			41.9
Both equinoxes of 1768	46			32.2			43.7
Both equinoxes of 1769	48			28.7			40.3
Both equinoxes of 1770	20			29.3			40.8
Both equinoxes of 1771	42			29.7			41.3
Mean from 6 years observations		51	27	29.8	51	28	41.3
Mean found by Dr. Bradley, with ⊙'s horizontal parallax $10\frac{1}{3}$″		51	27	28	51	28	39.5
But if 1″.2 be added to reduce them to the ⊙'s horizontal parallax 8″.8, Dr. Bradley's result will be changed to		51	27	29.2	51	28	40.7
Differing from my determination above only				0.6			0.6

In further confirmation of the latitude of the Observatory, I shall now adduce 8 years observed zenith distances of the sun in the solstices, being deduced from a number of observations taken at and near the solstices, and corrected for line of collimation, refraction, parallax, and nutation.

Years of observations.	Summer solstitial zenith distance reduced.			N° of days of observations.	Winter solstitial zenith distance.			N° of days of observations.	Half sum or latitude of the place.			Half difference or obliquity of the ecliptic.		
	°	′	″		°	′	″		°	′	″	°	′	″
1765	28	0	30.2	6	74	50	46.1	5	51	28	38.2	23	28	8.0
1766			33.6	7			48.8	2			41.2			7.6
1767			32.0	6	uncertain from an accident.			..	..	..	..	..	..	..
1768			29.7	6			44.9	7			37.3			7.6
1769			31.8	8			46.2	5			39.0			7.2
1770			30.7	7			44.1	6			37.4			6.7
1771			32.0	8			42.2	9			37.1			5.1
1772			31.8	12	Line of collimation altered by applying an achromatic object-glass to the telescope.									
Mean	28	. 0	31.5		74	56	45.4		51	28	38.4	23	28	7.0
Latitude	51	28	40									On Jan. 1, 1769.		

23 28 8.5 mean obliquity of ecliptic on Jan. 1, 1769.

This obliquity 23° 28′ 8″.5 is deduced in the manner used by Dr. Bradley, and is more to be depended on than the other 23° 28′ 7″.0 deduced from both solstices, on account of the less certainty of the lower refractions, from which however it only differs a second and a half. Thus all the observations of the sun and circumpolar stars accord to 1″ or 2″ with the latitude of the Observatory settled by Dr. Bradley, making use of his refractions.

I shall now determine the latitude independent of Dr. Bradley's refractions, and infer the higher refractions at the same time, from a comparison of my observations of the apparent zenith distance of the equator before set down, with Dr. Bradley's observations of the apparent zenith distance of the pole, both taken with the same excellent brass mural quadrant, in the same manner as Dr. Bradley deduced them from the apparent zenith distance of the equator observed with the iron quadrant compared with the apparent zenith distance of the pole observed with the brass quadrant, according to the extract from the manuscript in my possession before cited.

The mean apparent zenith distance of the equator, by my observations of 6 years from 1765 to 1771, was related before 51° 27′ 29″.8. The mean apparent zenith distance of the pole was found by Dr. Bradley from 1750 to 1752 to be 38° 30′ 35″. Their sum 89° 58′ 4″.8 taken from 90° leaves 1′ 55″.2, the sum of the refractions at the two zenith distances. Saying then, as 1′ 56″.7 the sum of the refractions by Dr. Bradley's rule, to 1′ 55″.2 the sum by observation, so are 1′ 11″.4 and 45″.3 the respective refractions at the two apparent zenith distances of the equator and pole by Dr. Bradley's rule, to 1′ 10″.5 and 44″.7 the two refractions at those zenith distances, which added to them give the co-latitude 38° 31′ 19″.7, and the latitude 51° 28′ 40″.3. And as 1′ 56″.7 : 1′ 55″.8 :: so is 57″ the refraction at the apparent zenith distance 45° 3′ by Dr. Bradley: 56″.27 the true refraction at that zenith distance, or not half a second differing from Dr. Bradley's, but more to be depended on as deduced from observations made with the brass quadrant only, and calculated from a parallax of the sun nearer to the truth.

But if the apparent zenith distance of the pole be made use of, resulting from a mean of 310 observations made with the brass quadrant, according to Dr. Bradley's manuscript, communicated by Dr. Hornsby, from the whole of his observations from 1750 to 1753, viz. 38° 30′ 36″, the sum of this and 51° 27 29″.8, the apparent zenith distance of the equator found by myself with the same instrument, or 89° 58′ 5″8 taken from 90° leaves 1′ 54″.2, the sum of the two refractions at the pole and equator. Whence the refraction at the pole will be found in like manner as before 44″.3, and that at the equator 1′ 9″.9, and the latitude 51° 28′ 39″.7, and the refraction at the apparent zenith distance of 45° 3′ = 55″.8, which is 1″.2 less than Dr. Bradley's determination, and 1″.2 greater than deduced from Mr. Hawksbee's experiment of the refraction of the air hereafter cited. It will be shown in the sequel, that the latitude thus found does not at all depend on the truth of the total arc, but only supposes the instrument proportionally divided at the points answering to the pole and the equator. From the whole then I conclude, that the latitude of the Royal Observatory at Greenwich is firmly established from Dr. Bradley's observations and my own at 51° 28′ 40″, probably without the error of a single second.

Let us now inquire into the latitude of the Royal Observatory at Paris. M. le Monnier, in the Memoires of the Royal Academy of Sciences for 1738, and in his Histoire Céleste, has examined into the latitude of the Royal Observatory at Paris, resulting from the observations of the principal French astronomers, and assuming the refraction at the height of the pole at Paris to be 50″, which is 2″ less than Dominico Cassini's table gives, and the same which Dr. Bradley's rule gives, he finds the latitude of their Royal Observatory as follows:

	°	′	″
From the observations of M. Picard	48	50	10
.... M. de la Hire	48	50	12
.... Le Chev. de Louville	48	50	8
.... M. Maraldi	48	50	14
His own observations in 1738, after examining and making an allowance for the error of the total arc of his quadrant	48	50	14
His further observations in 1740, making allowance for the error of the total arc of his quadrant, and considering the effect of the state of the air indicated by the thermometer on the refractions	48	50	15
In the Memoires of the Royal Academy of Sciences for 1744, M. Cassini de Thury (the author of the memoir) finds from his own observations, with the same refractions	48	50	12
In the Memoires of 1755, the Abbé de la Caille, from a nice and accurate calculation of his observations made at the College of Mazarine, at Paris, and the Cape of Good Hope, deduces new tables of refraction suitable to each place, and states their respective latitudes, and thence that of the Royal Observatory at Paris	48	50	14
Hence the ancient observations of M. Picard, M. de la Hire, and the Chevalier de Louville give	48	50	10
The modern and more accurate observations of M. Maraldi, M. le Monnier, M. Cassini de Thury, and the Abbé de la Caille, give	48	50	14

which is now generally made use of by the French astronomers as the true latitude of their Royal Observatory; and, from the near agreement of so many diligent observers and able astronomers, cannot be supposed to differ above 2 or 3″ from the truth. The difference of this and 51° 28′ 40″, the latitude of the Royal Observatory at Greenwich above stated, is 2° 38′ 26″, the true difference of latitude of the two observatories, which, from what has been said of the observations on which the respective latitudes were founded, cannot be supposed to differ above 3 or 4″ from the truth. What then becomes of the uncertainty of 15″ supposed by the late M. Cassini?

The same difference of latitude I find nearly from a comparison of my own observations of γ and β Draconis, taken with the zenith sector in 1768, with those of the Abbé de la Caille in 1750 and 1756, given in his Fundamenta Astronomiæ, after making the proper allowances for aberration, precession, and nutation, and correcting my observations by Dr. Bradley's refraction, and the Abbé de la Caille's by his table, and making allowance for the distance of the Abbé de la Caille's Observatory from their Royal Observatory; viz. 2° 38′ 25″.4 from γ Draconis, and 2° 38′ 26″.1 from β Draconis; the mean being 2° 38′ 25″.7, differing only 0″.3 from that stated above; but from Dr. Bradley's observations 2° 38′ 24″9, and 2° 38′ 27″.2, mean 2° 38′ 26″.0. It is too well known to astronomers to need my pointing out, that the best method of determining the difference of latitude of places, differing but little in latitude, is by such differences of zenith distances of stars passing near the zeniths, as the two above cited, observed at both places, in the same manner as the amplitude of the celestial arc is observed for finding the length of a degree of the meridian by comparison with geometrical measures.

The question now will be, on what foundation was the late M. Cassini's supposition of an uncertainty of 15″ in the latitude of Greenwich built? This appears evidently to have been on a passage in the Abbé de la Caille's researches into the astronomical refractions and latitude of Paris, contained in the Memoires of the Royal Academy of Sciences for 1755, p. 578, 579, where M. de la Caille takes the differences of zenith distances of 14 stars observed by Dr. Bradley (in correspondence to the same observed by himself at the Cape of Good Hope, for determining the moon's parallax in declination) published in the Memoires of the Royal Academy of Sciences for 1752, and the same observed by himself at Paris, after his return from the Cape, and correcting them for the difference of the refractions at the respective zenith distances, according to his own table of refractions, and the known apparent motions of the stars, finds the mean 2° 37′ 23″.9, which added to 48° 51′ 29″.3, his latitude at the College of Mazarine, gave him 51° 28′ 53″.2 for the latitude of Greenwich, exceeding Dr. Bradley's latitude by 13 or 14″.

Now the legitimacy of this conclusion depends on a supposition that both instruments measured the true angle, or that their total arcs were justly laid off, and that the Abbé de la Caille's table of refractions is just. The first indeed has been proved with respect to Dr. Bradley's quadrant, but never has been attempted with respect to the Abbé de la Caille's sextant; for the examination which the Abbé made of his instrument by parts for every $7\frac{1}{2}°$ (see Memoires of the Royal Academy of Sciences for 1751, p. 405), could not determine the error of the whole arc, as the difference from the truth might be insensible on such small arcs, and the examination seems to have been intended to find the differences of these small arcs from each other, rather than from the true arc which they represent. We may therefore be allowed to doubt of the truth of this circumstance. This doubt will be further strengthened by several particulars which I shall adduce.

1. The apparent altitude of the pole at the Royal Observatory 48° 51′ 12″, resulting from the Abbé de la Caille's observations, exceeds 48° 51′ 4″ the mean of the observations of Mess. Maraldi, le Monnier, and Cassini de Thury, by 8″, and at the same time his refractions for that altitude exceed what they adopt by the same quantity. 2. His refractions are greater than all other tables give, Dominico Cassini's, Flamsteed's, Newton's, Bradley's, Mayer's, Simpson's, and Lord Macclesfield's. The latter I have by me in a manuscript of Dr. Bradley, being what he used to correct his observations by, before he had been enabled to determine the refractions with the new mural arc. They were deduced from a brass quadrant of 5-feet radius made by Mr. Sisson, still remaining in the Observatory at Sherburn-Castle, and are the more to be esteemed because the divisions of the instrument had been submitted to the strictest re-examination, by which, in the opinion of Dr. Bradley, it was probably rendered as perfect in its kind as any extant, or as human skill could at that time produce. See Dr. Bradley's Letter to Lord Macclesfield, Phil. Trans. vol. 45, p. 5. The refractions in this table are less than Dr. Bradley's by 2″.4 at the altitude of 45°, and 4″ at the altitude of 20°. Mayer's refractions agree almost exactly with Dr. Bradley's, and are entitled to much weight, having been determined by a 6-feet mural arc constructed by Mr. Bird. 3. The refractions were found by the French Academicians at the polar circle, according to M. Maupertuis's Book on the Figure of the Earth, to agree nearly with Dominico Cassini's table. Hence it may be inferred, that the refractions in a warmer climate, as France, should be less than according to the same table, and therefore much less than according to M. de la Caille's, and approaching to Dr. Bradley's, which are a little less than M. Cassini's. 4. M. le Monnier, after his return from the polar circle, with a quadrant examined at the zenith and horizon, and after making allowance for the error thence inferred in the total arc, observed a great many refractions of stars under the pole, with the state of the thermometer, and sometimes of the barometer also, as recorded in his Histoire Céleste. These I calculated formerly, and found the refractions observed in very hot and very cold weather, compared together, to follow the same rate of increase and decrease, according to the changes of temperature, as Dr. Bradley has assigned; and, reducing the observed refractions to the mean temperature, I found them agree nearly with Dr. Bradley's. 5. The refractive power of the air

about its mean temperature was carefully observed by Mr. Hawksbee, as related in his Physico-Mechanical Experiments, and the ratio of the sine of incidence to that of refraction, out of air into a vacuum, found to be as 999736 to 1000000. Hence the astronomical refraction at the altitude of 45° should be 54″.6, only 2″.4 less than Dr. Bradley's, and 2″ less than the same when his higher refractions are new calculated with the true parallax of the sun, and 1″.2 less than I have before shown to result from my observations of the apparent zenith distance of the equator compared with Dr. Bradley's of the apparent zenith distance of the pole, both taken with the same brass mural quadrant, but 12″ less than the Abbé de la Caille's.

From all these facts, I think I may be allowed to conclude, that the Abbé de la Caille's refractions are not just, but considerably too large; and consequently, as there can be no doubt of the care or diligence used by this astronomer in his observations and calculations, that the total arc of his instrument is too large for the radius, and, as I shall show presently, gives the measures of the zenith distances too small.

But it may be asked, are then all the observations of this great astronomer, with their results, the fruit of so much labour and pains, to be considered as uncertain, or lowered in their value, in proportion to the error of his instrument? I am happy to answer, that the very ingenious method which he used of getting his refractions, from the comparison of the sum of the apparent altitudes of the poles at Paris and the Cape, with the sum of the apparent zenith distances of stars passing the meridian between the two places, has fortunately, without his being aware of it, given him the refractions affected with the error of the arc of the instrument, and consequently proper for correcting his observations; for if the instrument be supposed ill divided, any error in the divisions will naturally be thrown on the refractions; and if the total arc is too large for the radius, the stars will appear to approach the zenith by the error of the divisions as well as the refractions, and the refractions in the table will come out too large, but still suitable to the instrument because a correction is necessary to be added to the observed zenith distance, on account of the error of the instrument, as well as of the true refractions, and the table deduced from the instrument gives the sum of the two corrections together, without determining them separately.

Hence his table of refractions, though well adapted to his instrument, may be very unfit to be applied to any other. His latitude of his observatories and his declinations of the stars will not lose any of their certainty, at least within the limits of the zenith distances measured by his sector, viz. 60°. And this accounts for a circumstance, at first sight rather extraordinary, that his declinations of stars should agree so nearly (generally within 5″ of Dr. Bradley's, as

Dr. Bradley himself remarked) though his refractions made use of were so very different.

Having now shown that the Abbé de la Caille's refractions are too great, and only fit to be applied to his own instrument, it will be easy, by a just calculation, to reconcile the before-mentioned zenith distances of 14 stars observed at Greenwich by Dr. Bradley and by the Abbé de la Caille at the College of Mazarine at Paris, with the established latitudes of the two observatories, nearly; in doing which I shall claim the same right to correct Dr. Bradley's observations by his table of refractions, as I have allowed the Abbé de la Caille to be entitled to correct his observations by his table of refractions; which I think will be allowed me, after what I have said of the manner in which the Greenwich refractions were deduced and the instruments used. The difference of latitude of the College of Mazarine, and the Royal Observatory at Greenwich will then come out by the several stars, as follows: 2° 37′ 12″.7; 16″.0; 13″.8; 13″.7; 18″.4; 17″.7; 19″.7; 23″.2; 17″.7; 17″.0; 13″.9; 12″.4; 5″.6; 11″.9. The mean is 2° 37′ 15″.2 (or 8″.7 less than the Abbé de la Caille's result in his method of calculation, which I have shown to be inadmissible) and added to 48° 51′ 29″.3, the latitude of the Abbé de la Caille's Observatory, gives 51° 28′ 44″.5 for the latitude of the Royal Observatory at Greenwich, only $4\frac{1}{2}$″ more than established by Dr. Bradley's observations and my own; a sufficient agreement, especially considering that many of the stars were at great distances from the zenith, and that no account has been made of the temperature of the air at the times of the observations. The proper method however, of settling the difference of latitude of two observatories, is by stars near the zenith, as I observed before; and the difference of the latitudes of the two observatories of the College of Mazarine and Greenwich, by the Abbé de la Caille's observations of β and γ Draconis compared with mine, was 2° 37′ 10″.4, and compared with Dr. Bradley's 2° 37′ 10″.7; the first of which added to the Abbé de la Caille's latitude, gives 51° 28′ 39″.7, and the other 51° 28′ 40″, for the latitude of the Royal Observatory at Greenwich, exactly agreeing with that deduced immediately from the observations made at this place.

The same result nearly follows from M. Cassini de Thury's own observations of the zenith distance of the sun at the summer solstice of 1755, contained in the Memoires of the Royal Academy of Sciences for that year, compared with Dr. Bradley's, which latter was communicated to me by the late John Howe, Esq.; for, by M. Cassini's observations, the solstitial altitude of the sun's upper limb, corrected by the difference of refraction and parallax, according to Dominico Cassini's table, which happens to agree with the same difference by my tables at this height, was 64° 53′ 36″; from which 15′ 47″ being subtracted for the semi-diameter of the sun according to Mayer's tables, there remains 64° 37′ 49″, the true altitude of the sun's centre; and consequently the sun's true zenith distance 25° 22′ 11″. But the same was found from Dr. Bradley's observations, by my tables of refractions and the sun's parallax, 28° 0′ 32″.8. The difference 2° 38′ 21″.8, or 2° 38′ 22″, is the difference of latitude of the two observatories, which added to 48° 50′ 14″, the latitude of the Royal Observatory at Paris, gives 51° 28′ 36″ for the latitude of the Royal Observatory at Greenwich, or only 4″ less than before stated from the Greenwich observations, the difference lying the contrary way to that which the Abbé de la Caille carried the latitude of Greenwich, by improperly applying his own table of refractions to the Greenwich observations as well as to his own.

The Abbé de la Caille having, in the sequel of his memoir, inferred the difference of latitude of Gottingen and the College of Mazarine, from 22 stars observed by M. Mayer with a 6-feet mural quadrant of Bird's construction, correspondent to the same observed by himself, I shall make some remarks on this comparison, because it appears to afford a strong argument to show that the Abbé de la Caille's refractions are too great; and that Mayer's, which agree with Dr. Bradley's, are just. The Abbé, after correcting the zenith distances of 22 stars observed at both places by his own table of refractions, finds the difference of latitude of Gottingen and Paris, by a mean, to be 2° 40′ 35″.1; which added to 48° 51′ 29″.3, the latitude of the College of Mazarine, gives him the latitude of M. Mayer's Observatory 51° 32′ 4″.4. He adds, that some observations of the pole star sent to him by M. Mayer would give the latitude of Gottingen 19″ less than he has established it, as just mentioned. Now I find, that if M. Mayer's observations of the pole star, as well as of the stars to the south of the zenith, be corrected by M. Mayer's table of refractions, and the Abbé de la Caille's observations by his table of refractions, the latitude resulting from M. Mayer's observations of the pole star will agree to 2″ with that resulting from the difference of latitude by the stars to the south; for subtracting 19″ from 51° 32′ 4″.4, the latitude which the Abbé de la Caille has assigned to Gottingen in the manner above-mentioned, there remains 51° 31′ 45″.4 the latitude which he found by the pole star; to which adding 52″.8, the refraction at the mean height of the pole star according to the Abbé de la Caille, the sum 51° 32′ 38″.2, must be the apparent height of the pole by M. Mayer's observations; which diminished by 45″.6, M. Mayer's refraction, gives the true latitude of M. Mayer's Observatory 51° 31′ 52″.6. But by the difference of the Abbé de la Caille's and M. Mayer's zenith distances of the 22 stars to the south, corrected each by their own table of refractions, I find the difference of latitude 2° 40′ 32″.0; 28″.9; 25″.1; 27″.9; 22″.6; 28″.3; 32″.0; 26″.7; 27″.2; 24″.2; 25″.4; 31″.3; 23″.2; 23.6; 28″.9; 18″.4; 16″.7; 22″.0; 23″.8; 21″.1; 27″.3; 27″.3; the mean of which is 2° 40′ 25″.6; which added to 48° 51′ 29″.3, the latitude of the College of Mazarine, gives the latitude of Gottingen 51° 31′ 54″.9, or only 2″.3 more than I have deduced above from M. Mayer's observations of the pole star rightly corrected, and only 0″.9 less than is set down in M. Mayer's tables, which he expressly says, p. 48 of the precepts to his solar and lunar tables, published by myself for the Commissioners of Longitude in 1770, was deduced from his own observations.

I have before, when I showed the Abbé de la Caille's refractions to be considerably too great, at the same time vindicated them as fit for his instrument, because he deduced them in a manner which gave him the apparent elevation of objects above their true place by the sum of refraction and the error of his instrument, if his instrument measured the zenith distances too small, as I had concluded it did. The like remark may be applied to Dr. Bradley's table; for his refractions at the pole and equator, having been determined with one and the same quadrant, at one time turned to the north to observe the apparent zenith distance of the pole by means of the polar star and other circumpolar stars, and afterwards to the south to observe the apparent zenith distance of the equator, in the manner before explained, must necessarily be the true refractions, if the instrument measured the true angle; and the sum or difference of the true refractions and the errors of the instrument for these zenith distances, in case the instrument did not measure the true angle; and therefore equally proper to correct his observations, whether the total arc was just or not.

Further, from the two refractions thus found at the equator and pole, the refractions of the circumpolar stars at their passing the meridian above the pole were computed by Dr. Bradley, from the hypothesis that the refractions at considerable altitudes are as the tangent of the zenith distances; which rule is pretty accurately true with respect to the real refractions, and would vary but little from the truth for the apparent refractions, which would be the sum or difference of the true refractions and the errors of the arc, in case the total arc erred from the truth by a very small quantity, not exceeding 10 or at most 20 seconds. The observed zenith distances of the stars above the pole being corrected by the refractions thus computed, and subtracted from the known co-latitude, gave their true distances from the north pole; which added to the co-latitude gave their true zenith distances under the pole; and this diminished by the observed zenith distance would give the refraction under the pole, or the sum or difference of the refractions and the errors of the instrument belonging to their respective zenith distances; and thus his whole table would exhibit the sum or difference of the true refraction and error of the instrument. Hence the latitude of Greenwich established by Dr. Bradley, with his quadrant, as well as the latitudes of the observatories at the College of Mazarine and the Cape of Good Hope, settled by the Abbé de la Caille with his sextant, and the declinations of the stars and the obliquity of the ecliptic found by both, will be very near the truth, independent of the justness of the total arcs, though their respective refractions may be suitable only to their own particular instruments. But, for the reasons before given, I apprehend the Abbé de la Caille's refractions to be much too large, and Dr. Bradley's to be very near the truth.

I shall now close my inquiry into the latitudes of Greenwich and Paris, and Dr. Bradley's and the Abbé de la Caille's refractions, by a remark naturally arising from my comparison of, and endeavours to reconcile, their observations, which I desire to submit to the consideration of astronomers, it not having, that I know of, been made before; that a table of refractions should be made for every vertical instrument from observations made with itself turned alternately north and south; and that the table, so made, applied to observations made with it, will give the true zenith distances, whether the total arc of the instrument be accurately just, or affected with a small error, or however unequally it be divided below the pole, provided the divisions are equal between themselves in the part of the instrument lying between the equator, the zenith, and the pole.

It remains to give some account of the longitude of Greenwich, or rather of the difference of meridians of Greenwich and Paris, in reply to the late M. Cassini's doubts on the subject. This had been settled by Dr. Bradley at $9^{m}\ 20^{s}$, as he informed me himself, and that he had deduced it from eclipses of Jupiter's first satellite observed at both places, and that he had found it come out the same both from the immersions and emersions. This quantity had been inserted in the table of latitudes and longitudes of places, prefixed to Dr. Halley's tables, on the authority of Dr. Bradley, so long ago as the year 1749, the date of the publication of those tables, and was generally admitted by astronomers till the year 1763, when the late Mr. James Short, F. R. S., computed it from the 4 transits of Mercury over the sun in 1723, 1736, 1743, and 1753, observed at Paris, London, and Greenwich, to be $9^{m}\ 16^{s}$. See Philos. Trans., vol. 53, p. 158. In the year 1776, I requested the late Mr. Wargentin, the learned secretary of the Royal Academy of Sciences at Stockholm, and author of the improved tables for computing the eclipses of Jupiter's satellites, who collected observations of them from the principal observatories of Europe, in order to the further improvement of the tables, to inform me what difference of meridians of Greenwich and Paris resulted from my last 10 years observations of the eclipses of the first satellite of Jupiter, compared with those made by M. Messier at Paris. In the answer which he favoured me with, inserted in the Philos. Trans., vol. 67, p. 162, he set down the result of the comparison of 8 corresponding immersions and 9 emersions observed on both parts, by myself and M. Messier, from which he deduced the difference of meridians of the Royal Observatories of Greenwich and Paris $9^{m}\ 35^{s}$. By 2 corresponding immersions and 9 corresponding emersions, observed at both Royal Observatories, he found $9^m\ 21^s$. From the observations made between 1761 and 1764 he found $9^m\ 28^s$. By the observations made before 1700, $9^m\ 21^s$. And, from a comparison of mine and the Parisian observations, with the intermediate help of his own made at Stockholm, $9^m\ 26^s$: and from the whole he inferred the difference of meridians to be $9^m\ 25^s$.

Twelve years having elapsed since Mr. Wargentin's comparison, I was desirous to see what would result from the further observations made during that time, and applied to the Comte Cassini, the respectable heir of the late M. Cassini de Thury, and his successor at the Royal Observatory, and to the celebrated M. Messier, to favour me with such of their observations of the eclipses of the first satellite of Jupiter as had been made correspondent to mine. These they immediately sent me in the most obliging manner, by which I am enabled to make further inferences concerning the difference of our meridians, as exhibited in the two following tables; in reference to which it is to be understood, that all the observations at Greenwich were made with a 46-inch achromatic telescope of 3.6 inches aperture, except a few otherwise noted as observed with a 6-feet Newtonian reflector, whose aperture is 9.4 inches; and once with a 2-feet Gregorian reflector, whose aperture is 4.5 inches; and once with an 18-inch Gregorian reflector, with an aperture of 4.4 inches, furnished with new metals of Mr. Edwards's brilliant composition, described in the appendix to the Nautical Almanac of the present year, which reflects as much of the incident light as an achromatic telescope transmits; and that in making out the columns, intitled difference of meridians corrected, I have subtracted 7^s from the immersions, and added as much to the emersions, observed with the 6-feet reflector, and added 13^s to the immersions, and subtracted as much from the emersions observed with the 2-feet reflector, to reduce them to what they should have been probably observed at with the 46-inch achromatic telescope, and added 5^s to the time of the emersion observed on Sept. 5, 1784, at the Royal Observatory at Paris, with a 5-feet reflector of Dollond, to reduce it to the $3\frac{1}{2}$ feet achromatic telescope.

Difference of meridians of the Royal Observatories of Greenwich and Paris, by observations of eclipses of Jupiter's first satellite, observed at both places.

By Immersions.

	Diff. of meridians.	Diff. of mer. corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
1779, Jan. 11	$9^m\ 22^s$	$9^m\ 29^s$	6 F.	Limbs undulating.
18	9 38	9 45	6 F.; air a little hazy.	Air very clear.
1780, Jan. 14	9 5	9 5	Air very clear.	
Mar. 18	9 16	9 16	Air very clear.	Air hazy.
25	9 8	9 8	— — —	Air a little hazy.
1785, Oct. 1	9 11	9 11	In contact with Jupiter's body.	A little hazy.
Mean of 6 imm.	9 17	9 19		

By Emersions.

	Diff. of meridians.	Diff. of mer. corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Royal Observatory at Paris.
1773, Nov. 1	9m 10s	9m 3s	6 F. air a little hazy.	
1775, Feb. 15	10 44	10 37	6 F. twilight.	Air very clear.
Mar. 17	9 43	9 36	6 F. air a little hazy.	Air very clear.
1778, Mar. 13	10 46	10 46	— Bright moonshine.	
Apr. 12	9 46	9 46	Air hazy.	
1779, Mar. 30	10 43	10 43	— — —	Air hazy.
Apr. 1	9 56	10 9	2 F. reflector, air very clear.	
1781, May 24	9 49	9 49	Air very clear.	
31	9 8	9 8	— — —	Air very clear.
1782, Aug. 29	9 27	9 27	Air very clear.	
1783, Aug. 2	8 54	8 54	Air very clear.	The sat. very near Jupit. disc.
Oct. 26	9 45	9 45	Air very clear, but Jupiter low.	Limb's undulating much.
1784, Sept. 5	9 4	9 9	Air very clear.	5-feet reflector by Dollond, magnifying 450 times.
1785, Nov. 9	9 26	9 26	18-inch reflector, new metals.	
16	9 45	9 45	— — —	Hazy.
Mean of 15 em.	9 44.4	9 42		
Mean of 6 imm.	9 17	9 19		
Mean of both means	9 31	9 30½		

Before the 24th of May, 1781, a 3½-feet achromatic telescope of Dollond, of 42 lines aperture, that was but an indifferent one, was made use of at the Royal Observatory at Paris. From that time a very good one was employed of the same size and aperture.

Difference of meridians of the Royal Observatory of Greenwich and the Hôtel de Clugny at Paris, 2s of time east of the Royal Observatory, deduced from observations of eclipses of Jupiter's first satellite observed at both places.

By Immersions.

	Diff. of meridians.	Diff. of mer. corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Hôtel de Clugny.
1775, July 15	9m 55s	9m 55s	— — —	Air very clear.
Aug. 7	9 6	9 13	6 F.	Air very clear.
1776, Sept. 10	9 22	9 22	Jupiter a little hazy.	
1777, Sept. 6	9 33	9 33	— — —	Air very clear.
Nov. 7	9 23	9 23	Air hazy.	Air very clear.
1779, Jan. 11	10 18	10 25	6 F.	Air very clear.
18	9 23	9 30	6 F. air a little hazy.	Air very clear.
Dec. 22	8 10	8 10	Air very clear.	A little hazy.
1780, Jan. 7	9 39	9 39	Air very clear.	Air very clear.
14	9 40	9 40	Air very clear.	Air very clear.
Mar. 18	9 2	9 2	Air very clear.	Air very clear.
25	9 24	9 24		
1781, Feb. 26	8 48	8 48	♃'s limb undulates.	
Mar. 5	8 59	8 59	— — —	Air very clear.
Apr. 13	9 16	9 16	— — —	Jupiter ill defined.
1783, July 8	9 58	9 58	— — —	Air very clear.
1786, Sept. 4	9 19	9 19	Air very clear.	
Dec. 30	9 31	9 31	Air very clear.	Very hazy.
Mean of 18 im.	9 22.5	9 23.7		

By Emersions.

	Diff. of meridians.	Diff. of mer. corrected.	Circumstances of the observations at Greenwich.	Circumstances of the observations at the Hôtel de Clugny.
1775, Feb. 15	9m 31s	9m 24s	6 F. twilight.	Air very clear.
22	9 1	8 54	6 F.	Air very clear.
Mar. 17	9 13	9 6	6 F. air a little hazy.	Hazy.
1776, Jan. 26	9 15	9 15	— — —	
1777, Feb. 4	8 51	8 51	Air very clear.	Air very clear.
Mar. 24	9 13	9 13	Air very clear.	Air very clear.
31	9 22	9 22	Air very clear.	
1778, Feb. 25	8 48	8 48	— — —	Air hazy.
Mar. 13	9 15	9 15	— — —	A little hazy.
Apr. 5	9 17	9 17	Air hazy.	Air very clear.
12	10 11	10 11	— — —	Air very clear.
May 21	9 29	9 22	6 F.	Air very clear.
1780, Apr. 19	9 28	9 28	Air very clear.	
May 28	9 37	9 37	— — —	Air very clear.
1781, May 31	9 10	9 10	— — —	Air a little hazy.
June 16	9 25	9 25	— — —	Air a little hazy.
1783, Aug. 2	9 23	9 23	Air very clear.	Air very clear.
25	9 40	9 40	Air very clear.	
Oct. 3	9 30	9 30	Air very clear.	Air hazy.
1785, Nov. 9	9 14	9 14	18-inch reflector, new metals.	
18	9 12	9 12	Air very clear.	
1786, Jan. 3	9 58	9 58	Air a little hazy.	Air hazy.
Mean of 22 em.	9 22	9 20.7	M. Messier's achromatic telescope is of 3½ feet focus, 40 lines aperture, with magnifying powers of 70 and 140.	
Mean of 18 im.	9 22.5	9 23.7		
Mean by both	9 22	9 22		
Royal Observ. west of Hôtel de Clugny.	−2	−2		
Diff. of merid. of rhe Royal Observatories.	9 20	9 20		

Hence the difference of meridians of the two Royal Observatories, by the observations made in the Royal Observatories themselves, is $9^m\ 30^s$; and by the observations made by M. Messier, at the Hôtel de Clugny, and reduced to the Royal Observatory, is $9^m\ 20^s$. The mean of both results is $9^m\ 25^s$. But if greater weight be given to the latter determination than to the former in the ratio of 2 to 1, on account of the series of M. Messier's observations being the more complete, the difference of meridians will be $9^m\ 23^s$.

M. du Sejour, in the Memoires of the Royal Academy of Sciences for 1771, found $9^m\ 20^s$, as well from the beginning as end of the solar eclipse of 1769. M. Mechain, the learned editor of the Connoissance des Temps, informs me, that from the immersions of Celeno and Maia at the moon's limb, on March 5th last year, he has found by calculation from M. Messier's observations compared with mine, $9^m\ 19^s.9$, and $9^m\ 17^s.9$, or by a mean $9^m\ 18^s.9$; but by his own observations compared in like manner, he makes it a little more than $9^m\ 20^s$. He regulated his clock by correspondent altitudes; but M. Messier corrected his by a transit instrument, which however has no meridian mark. For the present I infer, that we may take the difference of meridians $9^m\ 20^s$, as being within a very few seconds of the truth, till some more occultations of fixed stars by the moon, already observed, or hereafter to be observed, in favourable circumstances, and carefully calculated, shall enable us to establish it with the last exactness. To collect and calculate such observations I have not leisure at present; but the field of calculation is equally open to the celebrated astronomers of Paris, the observations made at this place being now published annually.

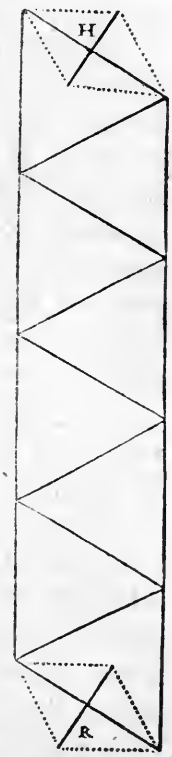
The extensive geometrical operations recommended by the late M. Cassini de Thury, and commenced under the direction of Major-general Roy, F. R. S., by his exact measure of a base on Hounslow-heath, may also, when completed, determine the difference of meridians of Greenwich and Paris to great exactness. But they do not seem to me likely to throw any new light on the difference of latitude of the two Observatories, because the uncertainty we are still under about the true figure and dimensions of the earth, and the irregular attractions arising from the irregular external figure, and unequal density of the internal parts of the earth, would prevent us from drawing any accurate conclusions, or such as we could confide in, from those geometrical measures, with respect to so large a quantity as $2^\circ\ 38'\ 26''$ the difference of latitude; and, at all events, it must be less exact, as it is less direct, to determine the difference of latitude of two places from the measured distance of the two parallels compared with the length of a degree in the intermediate latitude, inferred from former measures of degrees, which were themselves determined with the help of astronomical observations, than to infer it from the immediate astronomical observations made at the two Observatories, in the manner I have already deduced it.

Greenwich, Feb. 21, 1787. NEVIL MASKELYNE, Astron. Royal.

XIX. An Account of the Mode proposed to be followed in Determining the Relative Situation of the Royal Observatories of Greenwich and Paris. By Major-general Wm. Roy, F. R. S., and A. S. p. 188.

Two years have nearly elapsed since an account of the measurement of a base on Hounslow-heath was laid before the R. S., being the first part of an operation ordered by his Majesty to be executed for the immediate purpose of ascertaining the relative situations of the Royal Observatories of Greenwich and Paris; but whose chief and ultimate object has always been considered of a still more important nature, namely, the laying the foundation of a general survey of the British islands.

When the operation commenced in 1784, it was not doubted, that in 1786, at latest, we should have been able to have proceeded with the series of triangles from Hounslow-heath to the neighbourhood of Dover; but the contrivance and construction of an instrument, new of its kind, proposed to be made use of, and more particularly the nicety of its division, by which it is hoped the angles may be determined to a degree of precision hitherto unexampled, have required much more time than Mr. Ramsden himself at first imagined. Without meaning to disappoint, this ingenious artist was perhaps in the outset too remiss and dilatory, and accidents having happened when the workmanship was already far advanced, which he could not foresee or prevent, the execution has thus been greatly retarded. However, since the instrument may at present be considered as nearly finished, such parts as yet remain to be perfected being only of the smaller kind, we may fairly conclude, that early in the ensuing summer, or as soon as the weather in this country will permit, the trigonometrical operation may be begun. In this state of things, I have therefore judged that it might be proper to lay before the Society a short sketch of the mode proposed to be followed in fulfilling his Majesty's commands, accompanied by a very slight general map of the country, only collected from the common surveys, but still sufficient to show nearly the disposition of the triangles that will be used in forming the junctions between the meridians of the two Observatories.



In every series of triangles, where each angle is to be actually observed with the same instrument, they should, as near as the circumstances will permit, be equilateral: for were it possible to choose the stations in such a manner as that each angle should be exactly 60 degrees, the half number of triangles in the series, multiplied by the length of one side, would, as in the annexed figure, give at once the total distance; not only the sides of the scale or ladder would be perfectly parallel, but the diagonal steps, marking the progress from one extremity to the other, would be alternately so throughout the whole length. The first side is supposed to be found by the measurement of a base H, of about half its length; and the last side to be verified by such another base R at the opposite extremity. In any particular case, where only 2 angles of a triangle can be actually observed, these should be as near as possible each 45°. At any rate their sum should not differ much from 90°; for the less the computed angle differs from 90°, the less chance there will be of any considerable error in the intersection.

Romney-marsh, from its levelness, as well as other advantageous circumstances attending its situation, seeming to afford the best base of verification for the last triangle, I have given the series the shortest direction from Hounslow-heath to that part of Kent. The right hand stations occupy in general the heights which extend across the Wealds. Those on the left are placed on the great range of chalk hills, which end on our side of the Channel, between Folkstone and Walmer Castle, and recommence on the opposite side between Cape Blancnez and Calais. I do not mean to make St. Paul's a station in the suite; because in that case Harrow and Hampstead must also have been used, all 3 extremely inconvenient for the reception of the great instrument. Besides, Greenwich Observatory being hidden from the country to the south-west by the Norwood heights, and from that to the south-east by Shooter's-hill, after having made the detour of Harrow and Hampstead, and come across the smoke of the Capital, we should still have been obliged to make use of the 2 stations of Norwood and Shooter's-hill, without procuring so good an intersection of Bottle-hill, or Botley-hill, as is obtained by means of the station at the Hundred-acre-house. But though none of the stations of the series actually fall within London, yet from those in its vicinity, viz. the Pagoda, Norwood, Greenwich Observatory, and Shooter's-hill, we shall equally have it in our power to determine accurately the situations of Harrow, Hampstead, and St. Paul's, as well as many other chief steeples within the limits of the Capital. Another principle I have endeavoured to adhere to, in the disposition of the triangles, is this, that, after having obtained sides in length from 12 to 18 miles, I continue them at that length as much as the circumstances will permit; for, if they came to be reduced considerably below that extent, the obvious advantages of a long base at the outset would be lost, by the subsequent contraction towards the close of the operation.

The tower of Tenterden church, being a very conspicuous object, may be seen every where from the summit of the chalk hills, as far west as the river Medway. It may also be seen from the eastern extremity of the second base, by which the last triangle, Tenterden, Lid, Allington Knoll, is proposed to be verified. This knoll is itself a very remarkable object, more accessible, and in other respects more proper too for the purpose of a station, than Lymne church steeple, which I had at one time thoughts of occupying. The high ground which separates Romney-marsh from the Wealds of Kent, passes immediately behind Ruckinge, that is, to the north-westward of it, and may therefore probably prevent the top of the chalk hills from being seen from the west end of the base of verification; but if Tatterlees-Barn, or any other point on the range near it, can be seen from Ruckinge, then the station on the knoll, as well as that at Lymne, will become equally unnecessary, and the triangle of verification will become Tenterden, Lid, Tatterlees.

I propose to have a station on Fairlight Head, a land of considerable height, whence there is a good view of the coast of France near Boulogne. From this point and Tatterlees, with the help of the Indian lights, I have no doubt of obtaining a fine intersection of the signal of the Boulemberg, a hill of some note behind the town of Boulogne, and one of the stations used by the French Academicians in the execution of their triangles. The advantages of obtaining a triangle of this magnitude, whose sides are respectively in length about 45, 36, and 25 miles, are too obvious to require any comment. The high chalk cliffs near Folkstone prevent Dover Castle from being seen from Lid, or any where in the plain of Romney-marsh. Hence it will become necessary to form 2 small triangles to the northward of Tatterlees, in order to obtain an intersection of one of the turrets of the keep of that castle. Of the centre of the keep, it will be perceived, by the strong dotted lines, that the French Academicians have procured, from their stations at Calais, Blancnez, and Audinghen, an acute intersection at Dover Tower, making in the whole an angle of 28° 16′ 20″.

The points which are obviously the best for connecting our triangles with those of our neighbours, are the Boulemberg, Blancnez, and Calais, provided we could by any means obtain as good an intersection of the last as we are certain of getting of the 2 first; but the breadth of the range of chalk hills being little different on our side from what it is on theirs, by confining ourselves to such a base as they will afford us, we cannot any way obtain an intersected angle at Calais greater than about 29° or 30°.

Thinking that possibly from St. Peter's church in the isle of Thanet the tower of Notre Dame at Calais may be seen, I have extended dotted triangles into that part of Kent; because, if the united heights should not be sufficient to raise the top of the tower above the curvature of the sea, which is the only thing to be doubted, we are always certain, that the signal of Blancnez may, by means of the Indian lights, be easily seen, since the whole range of chalk hills behind Calais are discovered with the naked eye from the isle of Thanet, when the weather is tolerably clear.

Having in this manner ascertained the relative situations, with regard to the coast of England, of 3 points on the coast of France, forming a triangle whose sides and angles we already know from their trigonometrical operations, we shall in like manner be enabled to determine the situation of the point called M near Dunkirk, where the meridian of the Royal Observatory of Paris intersects a line drawn from the great tower of Dunkirk to that of N. D. at Calais. The distance M. P. on the meridian of Paris, between that point M and the parallel of Greenwich, will then be had; and that being added to 133417 fathoms, the distance northward from the Paris Observatory, we shall have the total terrestrial arc, comprehended between the parallels of the 2 Observatories, answering to an arc in the heavens of 2° 38′ 26″, or a difference of latitude between 51° 28′ 40″ and 48° 50′ 14″. In like manner the distance from Greenwich, on the parallel of

Greenwich, to its intersection with the Paris meridian, will then be readily computed, answering to the difference of longitude between the two Observatories; which, as far as can be judged from the map of Kent, corrected for the error in the direction of its meridian, amounts to about 2° 20′ 20″, supposing always that no uncertainty remains with regard to the position of the point M near Dunkirk. But here some remarks become necessary, which may probably suggest to the Academy of Sciences, that a further investigation of this matter may be needful on their part.

By referring to the 57th page of the first part of M. Cassini's book (La Méridienne vérifiée,) it will be seen, that Dunkirk, by one series of triangles, is eastward from the meridian of Paris 1426.53, and by another 1414.29 toises, the mean of which is 1420.41, equal to 1514 fathoms. This difference of 6½ fathoms, or little more than half a second of longitude between the mean and extreme places of M, is certainly very inconsiderable. But in the 60th page, where, in verifying the meridian of Paris, by the comparison of the angle that Broulezele makes with the meridian of Dunkirk, and the angle of convergence of one meridian to the other, a difference of 21 seconds between 10° 16′ 13″ and 10° 16′ 34″, is alledged to be almost insensible, we do not think to be a conclusion so unexceptionable. This however is not the only cause of uncertainty with regard to the just position of the point M: one of more importance arises, from the difference that is found by two sets of triangles in the angle of intersection of the meridian of Paris, with a line drawn through M from the tower of Dunkirk to that of Calais.

Thus, by p. 53 and 56 of the first part of M. Cassini's book, Dunkirk being the station, Broulezele makes an angle with the meridian of 10° 18′ 25″ towards the south-west: and the angle between Broulezele and Hondscote being 78° 11′ 42″, their difference 67° 53′ 17″ is the angle that Hondscote is south-east from the meridian; therefore the complement of this last angle to 180°, viz. 112° 6′ 43″, is the angle that Hondscote makes with the meridian of Dunkirk produced northward. By p. 166 of the 2d part, Dunkirk being the station, the angle between Hondscote and Mont-Cassel is shown to be 51° 7′ 15″; that between Mont-Cassel and Watten 42° 6′ 35″; and by p. 167 that between Watten and Calais is 51° 40′ 20″. The sum of these 3 angles is 144° 54′ 10″; from which deducting 67° 53′ 17″, the angle that Hondscote is south-eastward from the meridian, there remain 77° 0′ 53″ for the angle of Calais south-westward from it; and the complement of this angle to 180°, viz. 102° 59′ 7″, becomes the angle that the meridian of Dunkirk produced northward makes with a line drawn through M to Calais: to which last adding the angle of convergence of one meridian to the other 1′ 50″½, corresponding to the distance of 1514 fathoms, equal to 1′ 29″½ of a great circle, we shall have 103° 0′ 57″½ for the angle which the meridian of Paris produced northward from M makes with the line joining Dunkirk and Calais.

Again, by p. 63 of the 3d part of M. Cassini's book, Dunkirk being the station, the angle that Gravelines makes with the meridian south-westward is 72° 11′ 48″; and by p. 12 of the said 3d part, the angle between Watten and N. D. Calais is 51° 39′ 50″: also that between Watten and Gravelines is 46° 52′ 0″. Now the difference between these last two, 4° 47′ 50″, being added to 72° 11′ 48″, we have 76° 59′ 38″, and its complement 103° 0′ 22″, for the angles that the meridian of Dunkirk makes with the line drawn from thence through the point M to Calais: to which last angle adding the former convergence 1′ 50″½, we have 103° 2′ 12″½ for the angle that the meridian of Paris produced northward from M makes with the said line; but by the former set of angles it was found to be only 103° 0′ 57″½, the difference being 1′ 15″.

From M. Cassini's book it appears, that Dunkirk is north from Paris 125515.25 toises, which make 133768 fathoms; and the point M being south from the tower of Dunkirk 351 fathoms, there remains for the distance of M northward from the Royal Observatory 133417 fathoms. Now, with this distance as radius, the value of an angle of 1′ 15″ is 48½ fathoms, equal to 4″ 34‴ of longitude. Thus the point M, instead of being westward from Dunkirk 1514 fathoms, will, by the last set of angles, only be removed from it 1465½ fathoms: therefore the difference between the mean and extreme places of M, in this way of considering it, will amount to 24¼ fathoms, about 4 times as much as that resulting from the comparison stated in the 57th page. In the parallel of Greenwich the extreme difference will amount to 58.4 fathoms, or about 5½ seconds of longitude, not much more than one third part of a second of time. In this sort of uncertainty, with regard to the precise point M of intersection of the meridian of the Royal Observatory of Paris with the line joining Dunkirk and Calais, the only thing that can be done on our part, is to consider the mean position of M as just, that is, to suppose it to be 1514 fathoms westward from the great tower of Dunkirk, and having connected it with the British triangles, to show then what angle its meridian will make with the line drawn from Dunkirk to Calais.

General Roy then institutes a comparison of the celestial arc of the meridian, comprehended between the parallels of Greenwich and Perpignan, with the corresponding portions, measured and computed, of the terrestrial arc of the said meridian, between the point M and Perpignan. In the consideration of this matter it is to be observed, that M. Cassini has divided the celestial arc between the parallel of Dunkirk, or, which is the same thing, between the parallel of M and Perpignan, into 4 principal sections; viz. that from M to Paris, from Paris to Bourges, from Bourges to Rodés, and from Rodés to Perpignan; assigning to each section the measured portion of the corresponding terrestrial arc resulting from the triangles of the meridian. Having made the necessary calculations for these stations, he then infers that from these data, with the latitude of the Royal Observatory at Greenwich 51° 28′ 40″, and that of Paris 48° 50′ 14″, we shall have the latitudes of the several stations between Greenwich and Perpignan, with their differences, or the celestial arcs comprehended between them, as below.

Stations.	Latitudes.	Diff. or celestial arcs.
Greenwich Royal Observatory........	51° 28′ 40″ 0‴	
		0° 26′ 50″ 52‴
Point M near Dunkirk..............	51° 1 49 8	
		2 11 35 8
Paris Royal Observatory	48 50 14 0	
		1 45 9 19
Bourges	47 5 4 41	
		2 43 51 5
Rodés	44 21 13 36	
		1 39 11 28
Perpignan (St. Jaumes)	42 42 2 8	

This latitude of Perpignan 42° 42′ 2″ 8‴, is what results from the immediate comparison of the lengths of the celestial arcs, as determined by the zenith distances of stars, taken with a sector of 6 feet radius, and where the observations are so nearly consistent among themselves, as to leave little doubt of their accuracy; but in the 290th page of M. Cassini's book, so often quoted, as well as in the 170th page of his Description Géographique de la France, published in 1783, the latitude of Perpignan is given 42° 41′ 55″, which is 7″ 8‴ less than that deduced from the observations, without any reason that we can perceive being assigned for the reduction.

Perpignan, the southernmost station of the meridian line extending from Dunkirk through the whole kingdom of France, is situated at no great distance from the bottom of the Pyrenean mountains, where that lofty range ends at the Mediterranean sea. M. De La Caille was of opinion, that the plummet of the sector must have been affected by the attraction which it would suffer from that cause; a supposition which however has been doubted, since the observations made in this country on the attraction of Schehallien: for by these it appeared that the effect, though sensible, was but small, even when the sector was placed as near as possible to the opposite sides of the mountain. It is indeed true, that the Canigou, the highest of the Pyrenean range, being situated obliquely to the meridian, and at a considerable distance from Perpignan, would not probably occasion much deviation in the plummet; yet, on the other hand, when we compare the very trifling quantity of matter in Schehallien with the immensity of the mass in the Pyrenees, in the direction of the meridian, I cannot help being of M. De La Caille's opinion, that the plummet of the sector would be sensibly affected, that is, it would be drawn to the southward out of its perpendicular direction, and would thus give the zenith distance of the pole, or any other northern star, too little, and consequently a latitude too great. Until triangles shall have been extended beyond the Pyrenees, and the sector placed on the south side of the range, the quantity of this attraction, by its double or counter-effect, cannot possibly be ascertained. I will however only suppose it to have been 10″ 8‴, to be deducted from the latitude of Perpignan, which will then become 42° 41′ 52″, only 3″ less than that assigned to it in M. Cassini's two books before mentioned. Thus the arc between Rodés and Perpignan will be 1° 39′ 21″ 36‴, and the total celestial arc between Greenwich and Perpignan will be 8° 46′ 48″.

With regard to the corresponding terrestrial arc, it is to be observed, that various measurements have at different times been made, in different latitudes, of the lengths of the degrees of the meridian, for the purpose of obtaining, within certain limits at least, the true figure and dimensions of the earth. The most essential operations of this sort, are those in Peru under the equator, in middle latitudes in France and Italy, and in Lapland near the polar circle. The attraction of mountains, and unavoidable errors in the execution, will ever prevent just conclusions from being drawn from the comparison of measurements made too near each other. These last will always be found to differ more or less among themselves. Sometimes even the results may become absurd or contradictory. In cases of this sort, a mean of several should doubtless be taken for a mean latitude. Hence it is, that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has no regular figure, that is, not such as would be generated by the revolution of a curve around its axis. Others have supposed it to be an ellipsoid; regular, if both polar sides should have the same degree of flatness; but irregular, if one should be flatter than the other. And lastly, some suppose it to be a spheroid differing from the ellipsoid, but yet such as would be formed by the revolution of a curve around its axis.

In order therefore to set this matter in its true light, and to enable every one to judge, by simple inspection only, which of the theories agrees best with actual measurement, Gen. R. has computed on 10 different hypotheses, and arranged in their order, the lengths of the arc between Greenwich and Perpignan; as also some other chief properties of each figure. The first of the 11 columns, or that which comes next to the celestial arc, contains the measured portions of the corresponding terrestrial arc, as far as they have yet been executed. In the 2d column are arranged the computed dimensions appertaining to the earth as a sphere, supposing its semi-diameter to be a mean between the longest and shortest of M. Bouguer's 2d spheroid. After the sphere follow 7 ellipsoids of different degrees of oblateness, from the first, whose semi-diameters have to each other the ratio of 179.047 to 178.047, to the 7th, where it is only that of 540 to 539.

With regard to the 1st ellipsoid, supposing the earth to be homogeneous, it is well known, that the ratio of its semi-diameters may be found, by comparing with each other the lengths of the pendulums that vibrate seconds in different latitudes; which lengths are deduced from the seconds of acceleration, that the pendulum, so adjusted, and unalterably fixed as to length, at the equator, would perform in 24 hours, on being successively transported to different latitudes, as far as the pole, where the force of gravity being the greatest, the acceleration would likewise be the greatest. The calculations for this purpose were first made soon after Lord Mulgrave's return from his voyage towards the north pole in 1773.

And it appears, that the arithmetical mean of 75 comparisons between Spitzbergen and the equator gives the ratio of the semi-diameters 179.047 to 178.047. On this hypothesis the arc MP should contain 27350 fathoms. The error on the total arc M Perpignan amounts to 2078 fathoms. M. Bouguer's degree at the equator being adhered to, the 45th of latitude will exceed the truth 216, and that at the equator 148 fathoms.

The ratio of the semi-diameters of the 2d ellipsoid, has been obtained by the comparison of such measured lengths of the degrees of the meridian in different latitudes, as have been found to be most consistent with each other. Our countryman, Mr. Norwood, was the first, of late times, who made any attempt of this sort. But the measurement, executed by him in the year 1633, between London and York, has no pretence to exactness, since he himself tells us, that when he did not measure, he paced! Besides, his degree is as great, or even greater, than that in Lapland;* and these are surely sufficient reasons for rejecting it from the comparison. The degree measured by M. Liesganig in latitude 45° 57′, in that part of Poland lately fallen to the share of the Emperor and annexed to Hungary, being so much shorter than degrees to the southward of it, gives grounds to suspect, that some error had crept into that operation, or that the plummet had been affected by the attraction of neighbouring mountains, and therefore is not used on the present occasion. M. De La Caille's degree at the Cape of Good Hope, being in south latitude, and so much greater than those of the same height in northern latitudes, is also improper to be brought into the comparison, lest the difference may have arisen from a dissimilarity in the two polar sides of the ellipsoid. The degree measured in the north of France, compared with that in Austria, coming out absurd, it has been judged best to take a mean between them for a mean latitude. In like manner the latitudes of the two Italian degrees differing but little from each other, a mean length has been taken between them for a mean latitude. Accordingly

* That in Lapland however has since been found to be very erroneous.

the latitudes and the measured lengths of the degrees which, in the 2d ellipsoid, have been compared together, will appear as below:

Observers names.	Countries.	Latitudes.		Measured lengths.	
Bouguer,	Peru,		0° 0′		60484.5
Mason and Dixon,	Maryland,		39 12		60628.5
Boscovich,	Italy,	43° 0′	43 52	60725.5	60773.4
Beccaria,	Piedmont,	44 44		60821.3	
Cassini, &c.	Middle of France,		45 0		60777.6
Liesganig,	Austria,	48 43	49 3	60839.4	60833.0
Cassini, &c.	North of France,	49 23		60826.6	
Maupertuis, &c.	Lapland,		66 20		61194.3

Now these 6 degrees, being successively compared with each other, 15 results are thence obtained, the arithmetical mean of which gives for the ratio of the semi-diameters of the ellipsoid that of 192.483 to 191.483. Hence the arc MP should be in length 27331 fathoms. The arc M Perpignan exceeds the truth 1758, the 45th of latitude 180, and that at the polar circle near 88 fathoms.

The ratio of the semi-diameters of the 3d ellipsoid 216.06 to 215.06 is obtained by adhering to the measured lengths of the degrees at the equator and polar circle. According to this hypothesis the arc MP should contain 27301 fathoms. The arc M Perpignan exceeds the truth 1288, and that at the 45th of latitude more than 128 fathoms.

The ratio of the semi-diameters of the 4th ellipsoid 222.55 to 221.55 is the same as that assigned by M. Bouguer to his first spheroid, where the increments to the degrees of the meridian above that at the equator are as the 2d power or squares of the sines of the latitudes. The arc MP should contain 27294 fathoms. The arc M Perpignan errs in excess 1177 fathoms. The 45th degree exceeds the truth 116 fathoms; and that at the polar circle falls short of the measured length 21 fathoms: M. Bouguer's degree at the equator being adhered to as the standard.

The ratio of the semi-diameters of the 5th ellipsoid, 230 to 229, is that assigned to the earth by Sir Isaac Newton. On this hypothesis the arc MP should contain 27241 fathoms. The arc M Perpignan only exceeds the truth 202 fathoms, because the 45th degree of the meridian is here adhered to as the standard length. But then the degree at the equator falls short of the measurement 102 fathoms, and that at the polar circle $146\frac{1}{2}$; therefore an arc of $8^{\circ}\frac{1}{3}$, in the first case, would be defective 850, and in the last 1220 fathoms.

The ratio of the semi-diameters of the 6th ellipsoid, 310.3 to 309.3, is obtained by adhering to the measured lengths of the degrees at the equator and 45th of latitude. The arc MP should contain 27230 fathoms. The arc M Perpignan only exceeds the truth 131 fathoms; but on this hypothesis, the degree at the polar circle would be defective near 217 fathoms, and consequently on $8^\circ\frac{1}{3}$ the error would be 1807 fathoms.

The 7th or last ellipsoid, being that of the least flattening, has for the ratio of its semi-diameters 540 to 539. The arc MP should contain 27206 fathoms. The 45th degree of latitude being adhered to as the standard, the arc M Perpignan would only exceed the truth by 46 fathoms; but, on the other hand, the degree at the equator erring in excess $124\frac{1}{2}$ fathoms, and that at the polar circle being defective near 303; therefore, in the first case, the error on $8^\circ\frac{1}{3}$ would be 1037, and in the last 2524 fathoms. Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not any way correspond with the measured portions of the surface of the earth: for if we retain the length of M. Bouguer's degree at the equator as the standard, and make the ellipsoid extremely flat, as in N° 1, the figure will become too prominent in middle latitudes, that is, the curve will rise above the real surface of the earth, and in proportion to the excess of the radius, will always give degrees that exceed the measured length. On the contrary, if we give the ellipsoid a small degree of flatness, as in N° 7, and adopt the measured length of the 45th degree as the standard, the measured and computed arcs will nearly agree in middle latitudes; but at the equator the curve will rise very considerably above the surface, and will there give degrees that are too great; while at the polar circle it will fall below it, and give degrees that are too little in the proportion of about $2\frac{1}{2}$ to 1 compared with the error at the equator. From all which we may conclude, that the earth is not an ellipsoid.

The 2 columns towards the right-hand of the table, contain the arcs of 2 spheroids differing from the ellipsoid. The first is that adopted by M. Bouguer as his first hypothesis, where the increments to the degrees of the meridian above that at the equator follow the ratio of the 2d power or squares of the sines of the latitudes, and to which he has suited his first table of degrees, N° 32, p. 298. This spheroid differs but insensibly from the 4th ellipsoid. They have both the same semi-diameters; but the arcs of the spheroid being somewhat longer than those of the ellipsoid, the former thus becomes, in a trifling degree, more prominent in middle latitudes. On this hypothesis the arc MP should be in length 27295 fathoms; M Perpignan exceeds the measurement 1196 fathoms; and the degree at the equator being adhered to as the standard, the 45th errs in excess 118, while that at the polar circle is defective only 20 fathoms.

The 2d spheroid is that on which M. Bouguer founded his 2d hypothesis, which supposes the increments to the degrees of the meridian, above that at the equator, to follow the ratio of the 4th power or squared squares of the sines of the latitudes, and to which he has adapted his 2d table of degrees N° 38, p. 305. The ratio of the semi-diameters of this spheroid, viz. 179.4 to 178.4 differs little from that appertaining to the 1st ellipsoid; but here the curve falling considerably within, being less prominent than the ellipsoid in middle latitudes, the arcs are thus contracted in such a manner as to agree within 5 fathoms with the measured length of the meridian of France, in an extent of about $8°\frac{1}{3}$, comprehended between M near Dunkirk, and Perpignan situated at the bottom of the Pyrenean mountains. Also the errors in the several sections of this arc are not only small, but they are sometimes plus and sometimes minus, a never failing proof that, as far as our present data will enable us to judge, the figure here assigned to the earth, notwithstanding what has been alleged to the contrary, is exceedingly near the truth. According to this hypothesis, the distance MP on the meridian of Paris, which is yet to be determined by our trigonometrical operations, should contain 27243 fathoms, being only 35 fathoms less than what is given by the mean of the 7 different ellipsoids, a space not amounting quite to 2″ of latitude. The result of the measurement of this space, answering to an arc in the heavens of 26′ 50″ 52‴ of latitude, will be a further confirmation, or otherwise, of the justness of the theory. The degree at the equator being adhered to as the standard, the 45th is defective 37.6, while that at the polar circle errs in excess 9.4 fathoms.

Differences of longitude.—Hitherto there has been no particular reference to the computed lengths of degrees of longitude on each hypothesis, in 3 different latitudes, namely, the equator, and 43° 32′ and 51° 28′ 40″. No measurements of degrees of longitude have ever been executed with sufficient care and accuracy, except that in the south of France, as mentioned in the 105th and 106th pages of M. Cassini's book, which was determined by the repeated explosions of gunpowder in the open air, and found to contain 41618 toises, equal to 44354.4 fathoms. Hence the error in excess of M. Bouguer's theory, on the length of this degree trigonometrically measured, amounts only to 19 fathoms, which is little more than $\frac{1}{10}$ part of a second of time.

In fixed Observatories, where able astronomers have been for many years employed in repeating their observations of the heavenly bodies, it seems surprising that any doubt should remain with regard to what is called the astronomical difference of longitude, or, in other words, the difference of time between them; yet it has been alleged, that an uncertainty of this sort exists, even with regard to the situation of Greenwich and Paris, which, reckoned by its extremes, extends to about 10 or 11 seconds, answering in the latitude of Greenwich to the enormous difference in space of between 1600 and 1700 fathoms! But it will be considered as still more wonderful, if between 2 British Observatories, Greenwich and Oxford, which have been long supplied with great and costly instruments of the very best kinds, there should remain an uncertainty in this respect of 2 or 3 seconds of time: for in the latitude of Greenwich 3 seconds correspond to 477, and in that of Oxford to $474\frac{1}{2}$ fathoms. These however are points which must be left to the respective astronomers to settle in the best way they can; and it is not to be doubted that the Astronomer Royal will throw a new and very satisfactory light on the matter, in the paper which he proposes about this time to lay before the R. S., along with M. Cassini's Memoir, which, for that purpose, has now been nearly 2 years in his possession.

With regard to the trigonometrical operation (which may be considered as infallible, because, by means of the base of verification, it will prove itself, and if small errors unavoidably arise in the course of a long suite of triangles, the maximum of these may be always ascertained,) Gen. R. has no doubt that the distance between Greenwich and the point P in his map may thence be determined to a very small number of fathoms, perhaps to 15 or 16 on a difference of longitude of about 2° 20′ 20″, and therefore to about $\frac{1}{20}$th part of a second of time on each degree. This, for any useful purpose, will certainly be admitted to be sufficiently near the truth, and is probably considerably nearer than it will be brought for many years to come, by a mean of the best observations of the heavenly bodies, if these should be found in the present state of the matter to leave it yet doubtful to 2 or 3 seconds.

The astronomical difference of time may also be obtained by experiments on the instantaneous explosion of light; but these he would propose to be made subsequently to the trigonometrical operations. The station of Tatterlees, towards the eastern extremity of our range of chalk hills, or some point near it, would seem to be the most proper for the place of explosion, because it can be seen from Bottle-hill, on the same range, and nearly in the meridian of Greenwich Observatory. It is not to be doubted, that Tatterlees may be seen from Fienne Windmill, or even perhaps from that of the Brunemberg; since they are both situations on the continuation of the same range in France, the distance being shorter too, and little land, but chiefly sea, intervening. Let us then suppose, that the two astronomers with their clocks and transit-instruments are posted, one at Bottle-hill, and the other at the Brunemberg, while gunpowder is repeatedly exploded at Tatterlees, or while the Indian lights are alternately exhibited, and again covered by an extinguisher prepared for the purpose, which operation may be repeated several times the same evening; it is certain, that a just mean being taken between the instants so marked by the respective clocks, well regulated before-hand, the difference of time between the two extreme stations will thus be obtained to a very considerable degree of accuracy, and probably more to be relied on than that resulting from the comparison of the observations of the heavenly bodies.

But whatever might be the mode adopted as the best for conducting experiments of this nature, the observers must not only be very attentive and diligent, but also quick-sighted, have their clocks nicely regulated indeed, and the trials must be many times repeated before the uncertainty, even in this way, which seems to be the best mode, could be reduced to less than $\frac{1}{20}$th part of a second of time, to which it may infallibly be brought by trigonometry.

Having in this manner shown what probable degree of exactness may be expected in the various, but usual, ways of ascertaining the difference of longitude between the Observatories of Greenwich and Paris, and compared the results with the uncertainty that seems yet to exist in this matter from the state of astronomical observations; let us next see how Mr. Ramsden's instrument is likely to perform, when actually applied to the determination in question, by the observed angle between the pole star in its eastern or western azimuth, and a very remote station, whose distance from the instrument is known by the series of triangles, and distinguishable by the Indian lights at night, for the purpose of this particular observation.

With an instrument, carrying telescopes so good that the pole star may be seen in daylight, it is obvious, that the bisected angle between the star in its eastern and western azimuths will give at once the polar distance of the star, and the true meridian of the place, as referred to any known stations visible at the time of observation. But as cloudy weather may often prevent a complete observation of this sort from being obtained, and since much time might be lost in attempting it, therefore the declination of the star settled for any particular period being accurately known, its apparent distance from the pole may, by the established rules, be readily computed for any proposed day, as well as the precise times of its greatest elongations, twice in 24 hours, when in its eastern and western azimuths, at which times it will, for several minutes, appear, as to sense, stationary or without motion, except in altitude. These are therefore the best times for taking the angle between the star and any particular station, since the observations may be repeated frequently in the space of a few minutes, or until it shall be perceived that the star has again approached towards the pole. Now suppose the station of the instrument to be at Tatterlees, whose distance from the perpendicular to the meridian of Greenwich, and consequently from its parallel, is known by the trigonometrical operation. The latitude of the station becomes known also; and let the co-latitude be 38° 54′ 20″. Let us next suppose the distance of Bottle-hill on one side to be 44100 fathoms, equal to 43′ 28″.6 of a great circle; and that of the Brunemberg on the other to be 38250 fathoms, equal to 37′ 42″.6 of a great circle; and further, that on these two stations the Indian lights are exhibited for the time proposed. Now, let the angle between the meridian and Bottle-hill, and that between it and the Brunemberg, be observed by means of the pole star corrected for its distance for the day; and suppose the first to be 75° 10′, and the last 125° 5′; thus we shall have 2 spherical triangles to compute, in each of which, 2 sides and the contained angle are known, and one side, viz. the co-latitude, is common to both. Now from these data, making use of the half sum and half difference of the sides, we shall have the angles in these 2 triangles, and the angle of longitude between Bottle-hill and the Brunemberg, equal to that at the pole, will be found to be 1° 55′ 56″.1. If from this angle we deduct about 30″ or 35″, for the space that the Bottle-hill seems to be to the westward of the meridian of Greenwich, there will then remain 1° 55′ 21″ for the east longitude of the Brunemberg.

As far as we are enabled to judge at present, from the examination of the divisions of Mr. Ramsden's instrument, there is every reason to believe, that in taking angles around the horizon, the mean of several repetitions of the same angle, as referred to different parts of the circumference of the circle, will differ very little from the truth, so little indeed, that in many cases the error will totally vanish. But in elevating the telescope towards the pole, let us suppose that an error of 5″ on each of the contained angles at Tatterlees has been committed; and further, that even an error of 5″ of latitude, equal to about 84½ fathoms on the meridian, may have been fallen into, in estimating the co-latitude (which never can happen, but is only here admitted, to place the example in the most disadvantageous circumstances possible;) then whoever will recompute the 2 triangles with these new data, will find the result in longitude not to be varied, in the first case above $\frac{1}{5}$th part of a second, or $\frac{1}{75}$th part of a second in time; and in the last not quite 1″, or $\frac{1}{15}$th part of a second in time. Hence I conclude, that the best mode of determining the differences of longitude will be by the instrument itself, applied in this way, in taking the angles between the pole star and very remote stations, distinguishable at night by the help of the Indian lights, and whose distance is accurately known. This method will, it is true, be liable, as well as astronomical observations, to the imperfections of the instrument, particularly those of the telescope, and the unavoidable error in its application; but, on the other hand, it will be entirely free from the irregularities of clocks, and the imperfections of vision in marking the instantaneous explosion of light. When both methods have been repeated a sufficient number of times, with all imaginable care, we shall then, and not till then, be able to judge to which the preference may be due. Thus 5 or 6 long stations, in or nearly in the parallel of Greenwich, such, for instance, as that of Shooter's-hill Tower, would reach from the east quite to the west of the island: and as a very considerable degree of consistency might be expected among the results for equal portions of the parallel, this method seems to be as likely as any to furnish data for determining the nature of the spheroid or figure of the earth.

I embrace this opportunity of mentioning a circumstance, wholly unknown to me at the time my paper was composed. From what has been said, in the foregoing pages, it will probably be inferred, that I considered the proposed mode of determining the differences of longitude by the observations of the pole star, made with a very accurate instrument, rather as new, not knowing that the same idea, or one nearly the same, had before occurred to the Rev. Mr. Michell, and been treated on by him in his very ingenious paper in the Philosophical Transactions, vol. 56, for the year 1766. That I must have read that valuable performance about the time of its publication is not to be doubted; but in the lapse of so many years, every trace of it had gone from my remembrance, otherwise I would have most certainly referred to it in the proper place, and with the attention that it so well deserves. However, without entering here into particulars, it will obviously appear, that the one has not been borrowed from the other.

XX. Of three Volcanos in the Moon. By Wm. Herschel. LL. D., F. R. S. p. 229.

April 19, 1787, $10^h\ 36^m$ sidereal time, I perceive 3 volcanos in different places of the dark part of the new moon. Two of them are either already nearly extinct, or otherwise in a state of going to break out; which perhaps may be decided next lunation. The 3d shows an actual eruption of fire, or luminous matter. I measured the distance of the crater from the northern limb of the moon, and found it 3′ 57″.3. Its light is much brighter than the nucleus of the comet which M. Méchain discovered at Paris the 10th of this month.

April 20, 1787, $10^h\ 2^m$ sidereal time, the volcano burns with greater violence than last night. I believe its diameter cannot be less than 3″, by comparing it with that of the Georgian planet; as Jupiter was near at hand, I turned the telescope to his 3d satellite, and estimated the diameter of the burning part of the volcano to be equal to at least twice that of the satellite. Hence we may compute that the shining or burning matter must be above 3 miles in diameter. It is of an irregular round figure, and very sharply defined on the edges. The other 2 volcanos are much farther towards the centre of the moon, and resemble large, pretty faint nebulæ, that are gradually much brighter in the middle; but no well defined luminous spot can be discerned in them. These 3 spots are plainly to be distinguished from the rest of the marks on the moon; for the reflection of the sun's rays from the earth is, in its present situation, sufficiently bright, with a ten-feet reflector, to show the moon's spots, even the darkest of them: nor did I perceive any similar phænomena last lunation, though I then viewed the same places with the same instrument.

The appearance of what I have called the actual fire or eruption of a volcano, exactly resembled a small piece of burning charcoal, when it is covered by a very thin coat of white ashes, which frequently adhere to it when it has been some time ignited; and it had a degree of brightness, about as strong as that with which such a coal would be seen to glow in faint daylight. All the adjacent parts of the volcanic mountain seemed to be faintly illuminated by the eruption, and were gradually more obscure as they lay at a greater distance from the crater. This eruption resembled much that which I saw on the 4th of May, in the year 1783; an account of which, with many remarkable particulars relating to volcanic mountains in the moon, I shall take an early opportunity of communicating to this society. It differed however considerably in magnitude and brightness; for the volcano of the year 1783, though much brighter than that which is now burning, was not nearly so large in the dimensions of its eruption: The former seen in the telescope resembled a star of the 4th magnitude as it appears to the natural eye; this, on the contrary, shows a visible disk of luminous matter, very different from the sparkling brightness of star-light.

P. S. M. Méchain having favoured me with an account of the discovery of his comet, I looked for it among the Pleiades, supposing its track since the 10th of this month to lie that way; and saw it April 19th, at 10^h 10^m sidereal time, when it preceded Fl. *d* Pleiadum about 54^s in time, with nearly the same declination as that star; but no great accuracy was attempted in the determination of its place. As I have mentioned the comet in a foregoing paragraph of this paper, I thought it proper here to add my observation of it. "The comet is nearly round, with a small tail towards the north following part; the chevelure extends to about 4 or 5′; and it has a central, very small, ill-defined nucleus, of no great brightness."

XXI. An Experiment to determine the Effect of Extirpating one Ovarium, on the number of young produced. By John Hunter, Esq., F. R. S. p. 233.

In all animals of distinct sex (says Mr. H.) the females, those of the bird kind excepted, have, I believe, 2 ovaria, and of course the oviducts are in pairs. By distinct sex I mean when the parts destined to the purposes of generation are of 2 kinds, each kind appropriated to an individual of each species, distinguished by the appellation of male and female, and equally necessary to the propagation of the animal; the testicles, with their appendages, constituting the male; the ovaria, and their appendages, the female sex.

As the ovaria are the organs which, on the part of the female, furnish what is necessary towards the production of the 3d, or young animal; and as females appear to have a limited portion of the middle stage of life allotted for that purpose; it becomes a question, whether those organs are worn out by repeated acts of propagation; or whether there is not a natural and constitutional period to that power on their part, even if such power has never been exerted? If we consider this subject in every view, taking the human species as an example, we shall discover that circumstances, either local or constitutional, may be capable of extinguishing in the female the faculty of propagation. Thus we may observe when a woman begins to breed at an early period, as at 15, and has her children fast, that she seldom breeds longer than the age of 30 or 35; therefore we may suppose, either that the parts are then worn out, or that the breeding constitution is over. If a woman begins later, as at 20 or 25, she may continue to breed to the age of 40 or more; and there are, now and then, instances of women, who, not having conceived before, have had children as late in life as at 50 years or upwards. After that, few women breed, even if they should not have bred before: therefore there must be a natural period to the power of conception. A similar stop to propagation may likewise take place in many other classes of animals, probably in the female of every class, the period varying according to circumstances; but still we are not enabled to determine how far it depends on any particular property of the constitution, or of the ovarium alone.

As the female of most classes of animals has 2 ovaria, I imagined, that by removing one it might be possible to determine how far their actions were reciprocally influenced by each other, from the changes which by comparison might be observed to take place, either by the breeding period being shortened, or perhaps, in those animals whose nature it is to bring forth more than one at a time, by the number produced at each birth being diminished.

There are 2 views in which this subject may be considered. The first, that the ovarium, when properly employed, may be a body determined and unalterable respecting the number of young to be produced. In that case we can readily imagine, that when one ovarium is removed, the other may produce its determined number in 2 different ways; one when the remaining ovarium, not influenced by the loss of the other, will produce its allotted number, and in the same time; the other, when it is affected by the loss, yet the constitution demands the same number of young each time of breeding, as if there were still 2 ovaria; consequently it furnishes double the number it would have been required to supply had both been allowed to remain, but must cease from the performance of its function in half the time. The 2d view of the subject is by supposing that there is not originally any fixed number which the ovarium must produce; but that the number is increased or diminished according to circumstances; that it is rather the constitution at large that determines the number;

and that, if one ovarium is removed, the other will be called on by the constitution to perform the operations of both; by which means the animal should produce, with 1 ovarium, the same number of young as would have been produced if both had remained.

With an intention to ascertain these points, as far as I could, I was led to make the following experiment; and for that purpose chose pigs in preference to any other animal, because they are easily managed, and breed perfectly well under the confinement necessary for experiments. Having selected 2 female ones of the same colour and size, and likewise a boar pig, all of the same farrow; after having removed one ovarium only from one of the females, and cut a slit in one of its ears, that there might be no mistake between it and the other, I had them well fed and kept warm, that there might be no impediment to their breeding; and whenever they farrowed, their pigs were taken away exactly at the same age.

About the beginning of the year 1779, they both took the boar; but the one which had been spayed earlier than the perfect female. The distance of time however was not great, and they continued breeding at nearly the same times. The spayed animal continued to breed till Sept. 1783, when she was 6 years old, which was a space of more than 4 years. In that time she had 8 farrows; but did not take the boar afterwards, and had in all 76 pigs. The perfect one continued breeding till December 1785, when she was about 8 years old, a period of almost 6 years, in which time she had 13 farrows, and had in all 162 pigs; after this time she did not breed: I kept her till Nov. 1786.

I have here annexed a table of the different times of each farrow, with the number of pigs produced.

Spayed Sow.

Farrows.	Number of young.	Time.
1	6	Dec. 1779
2	8	July 1780
3	6	Jan. 1781
4	10	Aug. 1781
5	10	Mar. 1782
6	9	Sept. 1782
7	14	May 1783
8	13	Sept. 1783
	76	

Perfect Sow.

Farrows.	Number of young.	Time.
1	9	
2	6	
3	8	
4	13	Dec. 1781
5	10	June 1782
6	16	Dec. 1782
7	13	June 1783
8	12	Oct. 1783
	87	

November following she was put to the boar, but brought no pigs. April 1784, she was again put to the boar, without effect, and never was observed to take the boar afterwards, though often with him. November 1784, she was killed.

Eleven pigs more than were produced by the spayed sow in her eight farrows. The remainder of her farrows as follow:

Farrows.	Number of young.	Time.
9	12	Feb. 1784
10	16	June 1784
11	12	Dec. 1784
12	16	May 1785
13	19	Dec. 1785
	75	

After which she bred no more. The first 8 farrows were 87, the last 5 farrows were 75, total 162, the number from the spayed one 76, being 86 less than farrowed by the imperfect animal.

It is observable, that both sows rather increased in their number each time the older they grew, though not uniformly; the difference between the first and last in both animals being considerable. From the above table we find, that the sow with only one ovarium bred till she was 6 years old, from the latter end of 1779 till Sept. 1783, about 4 years, and in that time brought forth 76 pigs. The perfect animal bred till she was 8 years of age. In the last, if conception depended on the ovaria, it was to be expected that she would bring forth double the number at each birth; or, if she did not, that she would continue breeding for double the time. We indeed find her producing 10 more than double the number of the imperfect animal, and continuing to breed much longer.

From a circumstance mentioned in the course of this experiment it appears, that the desire for the male continues after the power of breeding is exhausted in the female; and therefore does not altogether depend on the powers of the ovaria to propagate, though we must at the same time allow, that it may be influenced by the existence of such parts. If these observations should be considered as depending on a single experiment, from which alone it is not justifiable to draw conclusions, I have only to add, that the difference in the number of pigs produced by each was greater than can be justly imputed to accident, and is a circumstance certainly in favour of the universality of the principle I wished to ascertain.*

From this experiment it seems most probable, that the ovaria are from the beginning destined to produce a fixed number, beyond which they cannot go, though circumstances may tend to diminish that number; that the constitution at large has no power of giving to 1 ovarium the power of propagating equal to 2; for, in the present experiment, the animal with 1 ovarium produced 10 pigs less than half the number brought forth by the pig with both ovaria. But that the constitution has so far a power of influencing one ovarium, as to make it

* It may be thought by some, that I should have repeated this experiment; but an annual expense of twenty pounds for ten years, and the necessary attention to make the experiment complete, will be a sufficient reason for my not having done it.—Orig.

produce its number in a less time than would probably have been the case if both ovaria had been preserved, is evident from the above recited experiment.

XXII. Experiments made to determine the Positive and Relative Quantities of Moisture Absorbed from the Atmosphere by Various Substances, under similar Circumstances. By Sir Benj. Thompson, Knt., F. R. S. p. 240.

Having provided a quantity of each of the under-mentioned substances, in a state of perfect cleanness and purity, says Sir B. T., I exposed them, spread out on clean China-plates, 24 hours in the dry air of a very warm room, the last 6 hours the heat being kept up to 85° of Fahrenheit's thermometer; after which I entered the room with a very accurate balance, and weighed equal quantities of them, as expressed in the following table. Then each substance being equally spread out on a clean China plate, they were removed into a very large uninhabited room on the 2d floor, where they were exposed 48 hours, on a table placed in the middle of the room, the air of the room being at the temperature of 45° F.; after which they were carefully weighed in the room, and were found to weigh as under-mentioned.

They were then removed into a very damp cellar, and placed on a table, in the middle of a vault, where the air, which appeared by the hygrometer to be completely saturated with moisture, was at the temperature of 45° F.; and in this situation they were suffered to remain 3 days and 3 nights, the vault being hung round, during all this time, with wet linen cloths, to render the air as damp as possible, and the door of the vault being shut. At the end of the 3 days I entered the vault, with the balance, and weighed the various substances on the spot, when they were found to weigh as is expressed in the 3d column of the following table.

The various substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold uninhabited room.	Weight after being exposed 72 hours in a damp cellar.
	Pts.	Pts.	Pts.
Sheep's wool	1000	1084	1163
Beaver's fur	1000	1072	1125
The fur of a Russian hare	1000	1065	1115
Eider down	1000	1067	1112
Silk { Raw, single thread	1000	1057	1107
Silk { Ravelings of white taffety	1000	1054	1103
Linen { Fine lint	1000	1046	1102
Linen { Ravelings of fine linen	1000	1044	1082
Cotton wool	1000	1043	1089
Silver wire, very fine, gilt, and flatted, being the ravelings of gold lace	1000	1000	1000

The weight used in these experiments was that of Cologne, the parts or least divisions being $= \frac{1}{65536}$ part of a mark, consequently 1000 of these parts make about $52\frac{3}{4}$ grains troy. It seems that those bodies which are the most easily wet, or which receive water, in its unelastic form, with the greatest ease, are not those which in all cases attract the watery vapour dissolved in the air with the greatest force. Perhaps the apparent dampness of linen, to the touch, arises more from the ease with which that substance parts with the water it contains, than from the quantity of water it actually holds: in the same manner as a body appears hot to the touch, in consequence of its parting freely with its heat, while another body, which is actually at the same temperature, but which withholds its heat with greater obstinacy, affects the sense of feeling much less violently.

It is well-known, that woollen-clothes, such as flannels, &c. worn next the skin, greatly promote insensible perspiration. May not this arise principally from the strong attraction which subsists between wool and the watery vapour which is continually issuing from the human body? That it does not depend entirely on the warmth of that covering, is clear; for the same degree of warmth, produced by wearing more clothing of a different kind, does not produce the same effect. The perspiration of the human body being absorbed by a covering of flannel, it is immediately distributed through the whole thickness of that substance, and by that means exposed by a very large surface to be carried off by the atmosphere; and the loss of this watery vapour, which the flannel sustains on the one side, by evaporation, being immediately restored from the other, in consequence of the strong attraction between the flannel and this vapour, the pores of the skin are disencumbered, and they are continually surrounded by a dry, warm, and salubrious atmosphere.

I am astonished, that the custom of wearing flannel next the skin should not have prevailed more universally. I am confident it would prevent a multitude of diseases; and I know of no greater luxury than the comfortable sensation which arises from wearing it, especially after one is a little accustomed to it. It is a mistaken notion, that it is too warm a clothing for summer. I have worn it in the hottest climates, and in all seasons of the year, and never found the least inconvenience from it. It is the warm bath of a perspiration confined by a linen shirt, wet with sweat, which renders the summer heats of southern climates so insupportable; but flannel promotes perspiration, and favours its evaporation; and evaporation, as is well known, produces positive cold. I first began to wear flannel, not from any knowledge which I had of its properties, but merely on the recommendation of a very able physician, Sir Richard Jebb; and when I began the experiments of which I have here given an account, I little thought of discovering the physical cause of the good effects which I had experienced from it; nor had I the most distant idea of mentioning the circumstance.

With regard to the original object of these experiments, the discovery of the relation which I thought might possibly subsist between the warmth of the substances in question, when made use of as clothing, and their powers of attracting moisture from the atmosphere; or, in other words, between the quantities of water they contain, and their conducting powers with regard to heat; I could not find that these properties depended in any manner upon, or were in any way connected with, each other.

XXIII. The Principles and Illustration of an Advantageous Method of Arranging the Differences of Logarithms, on Lines Graduated for the Purpose of Computation. By Mr. W. Nicholson. p. 246.

1. If two geometrical series of numbers, having the same common ratio, be placed in order with the terms opposite each other; the ratio, between any term in one series and its opposite in the other, will be constant. 2. And likewise the ratio of a term in one series to any term in the other, will be the same as obtains between any other two terms having the same relative position and distance. 3. In all such pairs of geometrical series, as have the same common ratio, the last-mentioned property obtains, though the first antecedent and consequent be taken in one pair, and the second in any other pair.

4. If the differences of the logarithms of numbers be laid in order on an arrangement of equi-distant parallel right lines, in such a manner as that a right line, drawn across the whole, shall intersect it at divisions which denote numbers in geometrical progression; then, from the condition of the arrangement and the property of this logarithmic line, it follows, first, that every right line, so drawn, will, by its intersections, indicate a geometrical series of numbers; secondly, that such series, as are so indicated by parallel right lines, will have the same common ratio; and, thirdly, that the series thus indicated by two parallel right lines, supposed to move laterally without changing either their mutual distance, or parallelism to themselves, will have each the same common ratio; and in all pairs of series indicated by such two lines, the ratio between an antecedent on one parallel and the opposite term on the other, taken as a consequent, will be constant.

All these properties Mr. N. demonstrates or illustrates by examples. 5. Thus far the logarithmic line has been considered as unlimited. If therefore an antecedent and consequent be given, it will be possible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then supposed to move, without changing either their distance or absolute direction, so that the line, which before marked an antecedent, may now mark a new antecedent; the other (by art. 2 and 3) will mark a number, at the same relative position and distance, which shall be the consequent to this last antecedent after the same ratio. 6. Suppose a logarithmic line to contain no more than a single range of numbers from 1 to 10, it will not be necessary, for the purposes of computation, to repeat it; for if a slider or beam have 2 fixed points at the distance of the interval between 1 and 10, and a moveable point be made to range between these (always to indicate the antecedent); then, if the consequent fixed point fall without the rule, the other fixed point will show the division it would have fallen on, if the rule had been prolonged. This may be easily applied to the arrangement described, N° 4. 7. If the arrangement consist only of the logarithms from 1 to 10, and the parallel cross lines intersect that geometrical series whose successive ratios altogether, with that of the last term to 10 times the first, make by composition of the ratio $\frac{1}{10}$, the contrivance, N° 6, may be applied to show such consequents as fall laterally without the rule. 8. It is convenient that the arrangement of the lines be disposed so as to occupy a rectangular parallelogram; or, in other words, that the cross line, cutting the series last-mentioned, may be at right angles to the length of the rule.

The construction of an instrument on the foregoing principles admits of various dispositions of the graduated lines and apparatus for measuring intervals on them. Mr. N. here gives examples of several different forms: one is a rule consisting of 10 parallel lines, equivalent to a double line of numbers upwards of 20 feet in length. Another is a beam compass for measuring intervals.

A 3d is a Gunter's scale, equivalent to that of $28\frac{1}{2}$ inches in length, published by the late Mr. Robertson. It is however but $\frac{1}{4}$ of the length, and contains only $\frac{1}{4}$ of the quantity of division. In the slider GH, fig. 10, pl. 1, is a moveable piece AB, across which a fine line is drawn; and there are also lines CD, EF, drawn across the slider, at a distance from each other equal to the length of the rule. The line CD or EF is to be placed at the consequent, and the line in the piece AB at the antecedent: then, if the piece AB be placed at any other antecedent, the same line CD or EF will indicate its consequent in the same ratio taken the same way; that is, if the antecedent and the consequent lie on the same side of the slider, all other antecedents and consequents in that ratio will lie in the same manner, and the contrary if they do not, &c. But if the consequent line fall without the rule, the other fixed line on the slider will show the consequent; but on the contrary side of the slider to that where it would else have been seen by means of the first consequent line.

Fig. 11 is an instrument equivalent to the same rule of $28\frac{1}{2}$ inches long. It consists of 3 concentric circles engraved and graduated on a plate of about $1\frac{1}{2}$ inch in diameter. From the centre proceed two legs A, B, having right-lined edges in the direction of radii. They are moveable either singly or together. To use this instrument, place one of the edges at the antecedent, and the other at the consequent, and fix them to that angle. The 2 legs being then moved together, and the antecedent leg placed at any other number, the other leg will give its consequent in the like position or situation on the lines. If the line CD happen to lie between the legs, and B be the consequent leg, the number sought will be found one line farther from the centre than it would otherwise have been; and, on the contrary, it will be found one line nearer in the like case, if A be the consequent leg. This instrument, differing from the first only in its circular form, and the advantages resulting from that form, the lines must be taken to succeed each other in the same manner laterally; so that numbers, which fall either without or within the arrangement of circles, will be found on such lines of the arrangement as would have occupied the vacant places if the succession of lines had been indefinitely repeated sideways.

Mr. N. thinks this construction superior to every other which has yet occurred, not only in point of convenience, but likewise in the probability of being better executed, because small arcs may be graduated with very great accuracy, by divisions transferred from a larger original. The circular instrument is a combination of the Gunter's line and the sector, with the improvements here pointed out. The property of the sector may be useful in magnifying the differences of the logarithms in the upper part of the line of sines, the middle of the tangents, or the beginning of the versed sines. It is even possible, as mathematicians will easily conceive, to draw spirals on which graduations of parts, every where equal to each other, will show the ratios of those lines by means of moveable radii similar to those in this instrument.

XXIV. Observations tending to show that the Wolf, Jackal, and Dog, are all of the same Species. By John Hunter, Esq., F. R. S. p. 253.

The true distinction between different species of animals must ultimately be gathered from their incapacity of propagating with each other an offspring capable again of continuing itself by subsequent propagations: thus the horse and ass beget a mule capable of copulation, but incapable of begetting or producing offspring. If it be true, that the mule has been known to breed, which must be allowed to be an extraordinary fact, it will by no means be sufficient to determine the horse and ass to be of the same species; indeed, from the copulation of mules being very frequent, and the circumstance of their breeding very rare, I should rather attribute it to a degree of monstrosity in the organs of the mule which conceived, not being those of a mixed animal, but those of the mare or female ass. This is not so far-fetched an idea, when we consider that some true species produce monsters, which are a mixture of both sexes, and that many animals of distinct sex are incapable of breeding at all. If then we find nature in its greatest perfection deviating from general principles, why may not it happen likewise in the production of mules, so that sometimes a mule shall breed from the circumstance of its being a monster respecting mules?

The times of uterine gestation being the same in all the varieties of every species of animals, this circumstance becomes necessary to determine a species. The affinity between the fox, wolf, jackal, and several varieties of the dog, in their external form and several of their properties, is so striking, that they appear to be only varieties of the same species. The fox would seem to be a greater remove from the dog than either the jackal or wolf, at least in disposition, not being either so sociable respecting its own species or man, but naturally a solitary animal: from all which I should suspect it is only allied to the dog by being of the same genus. It is confidently asserted by many that the fox breeds with the dog, but this has not been accurately ascertained; but if it had, it would probably have been carried further, and once breeding, according to what we have said, does not constitute a species; this however is a part I mean to investigate. Wolves and jackals are found in herds; and the jackal is so little afraid of the human species, that, like a dog, it comes into houses in search of food, more like a variety of the dog in consequence of cultivation than chance. It is by much the most familiar of the two; for we shall find hereafter, that in its readiness to copulate with the dog, and its familiarity with the dog afterwards, it is somewhat different from the wolf. The wolf then being an animal better known in Europe, where inquiries of this kind are made, some pains has been taken to ascertain, whether or not it was of the same species with the dog; but I believe it has been hitherto considered as only belonging to the same genus.

Accident often does as much for natural history as premeditated plans, especially when nature is left to itself. The first instance of the dog and wolf breeding in this country seems to have been about the year 1766. A Pomeranian bitch of Mr. Brookes's, in the New Road, was lined only once by a wolf, and brought forth a litter of 9 healthy puppies. The veracity of Mr. Brookes is not to be doubted, respecting the bitch being lined by a wolf; yet, as it was possible she might have been lined by some common dog without his knowledge, the fact was not clearly made out; but it has been since ascertained that the dog and wolf will breed. Several noblemen and gentlemen bought some of the puppies, as I was informed by Mr. Brookes. My Lord Clanbrassil purchased a bitch-puppy; and Mr. Brookes presented one to me, which I kept for observations and experiment. Its actions were not truly those of a dog; it had more quickness in attending to things, was more easily startled, as if particularly apprehensive of danger, quicker in transitions from one action to another, not so ready to the call, being less docile; and from these peculiarities it lost its life, being stoned to death in the streets for a mad dog.*

* Hearing that Lord Clanbrassil's bitch had bred, Sir Joseph Banks was so obliging as, at my request, to write to his Lordship, who sent the following account:

On the supposition that Mr. Brookes's bitch was lined by no dog but the wolf, which I think we have no reason to doubt, the species of the wolf is ascertained; but I chose to trace this breed still further; and hearing that Lord Pembroke's bitch had also bred, I was anxious to know the truth of it; and, finding his lordship was in France, I took the liberty of writing to Lord Herbert, and received in answer the following letter:*

Buffon, whose remarks in natural history are well known, made experiments to ascertain how far the wolf and dog were of the same species, but without success. He says, " A she-wolf, which I kept 3 years, though shut up very young, and along with a greyhound of the same age, in a spacious yard, could not be brought to agree with it, nor endure it, even when she was in heat. She

Sir,—About 17 or 18 years ago, the late Lord Monthermer and I happened to see a dog-wolf at Mr. Brookes's, who deals in animals, and lives in the New Road. The animal was remarkably tame; and it struck us, for that reason, that a breed might be procured between him and a bitch. We promised Mr. Brooke's a good price for puppies, if he succeeded. In about a year a bitch produced 9; of which Lord Monthermer bought one; and I had another, which was a bitch. Lord Monthermer's died of fits in about 2 years: mine lived longer, and had puppies only once. One I gave to Lord Pembroke; but what became of it I do not remember. It was grand-daughter of the wolf by the dam, and got by a large pointer of mine. It might be considered, that Mr. Brookes's word was not sufficient proof that the puppies were really got by the wolf; but the appearance of the animals, so totally different from all others of the canine species, did not leave a doubt on our minds; and I remember Hans Stanley, who had adopted Buffon's opinion, was thoroughly convinced on seeing mine. The animals had the shape of the wolf refined: the fur long, but almost as fine as that of the black fox. I am, Sir, &c. Clanbrassil.

Jan. 7, 1787.

* Sir, *Wilton-house, Dec.* 20, 1786.

The half-bred wolf-bitch you allude to was given, as I have always understood, to Lord Pembroke by Lord Clanbrassil. She might perhaps have been bought at Brookes's by him. She had 4 litters, one of ten puppies, by a dog between a mastiff and a bull-dog. One of these was given to Dr. Eyre, at Wells, in Somersetshire, and one to Mr. Buckett, at Stockbridge. The 2d litter was of 9 puppies, some of which were sent to Ireland, but to whom I know not. This litter was by a different dog, but of the same breed as the first. The 3d litter was of 8 puppies, by a large mastiff. Two of these were, I believe, sent to the present Duke of Queensberry. The 4th litter consisted of 7 puppies; 2 of which were sent to M. Cerjat, a gentleman who now resides at Lausanne, in Switzerland, and is famous for breaking dogs remarkably well. These 2 puppies were however naturally so wild and unruly, that he found it impossible to break them. She died 4 years ago, and the following inscription was put over the place where she is buried in this garden, by Lord Pembroke's orders.

Here lies Lupa,
whose grandmother was a wolf,
whose father and grandfather were dogs, and whose
mother was half wolf and half dog. She died
on the 16th of Oct., 1782, aged 12 years.

I am sorry it is not in my power to give you any better account; but if you think proper to write to Lord Pembroke, who is at Paris, I am convinced he will be very happy to give you any further information. I am, &c. Herbert.

was the weaker, yet the more mischievous; provoking, attacking, and biting the dog, which at first only defended himself, but at last killed her." And in another part of his work, he makes the following observation: "The dog, the wolf, the fox, and the jackal, form a genus, of which the different species are really so nearly allied to each other, and of which the individuals resemble each other so much, particularly by the internal structure and parts of generation, that it is difficult to conceive why they do not breed together."*

This part of natural history lay dormant till Mr. Gough, who sells birds and has a collection of animals on Holborn Hill, repeated the experiment on a wolf-bitch, which was very tame, and had all the actions of a dog under confinement. A dog is the most proper subject for comparison, as we have opportunities of being acquainted with its dispositions and modes of expressing its sensations, which are most distinguishable in the motion of the ears and tail; such as pricking up the ears when anxious, wishing, or in expectation; depressing them when supplicant, or in fear; raising the tail in anger or love, depressing it in fear, and moving it laterally in friendship; and also in raising the hair on the back from many affections of the mind. This animal became in heat in the month of Dec. 1785; and as Mr. Gough had some idea of breeding from wild animals, as monkeys, leopards, &c. he was anxious to have the wolf lined by some dog; but she would not allow any dog to come near her, probably from her not being accustomed to be with dogs, and being always chained. She was held however while a greyhound lined her, and they were fastened together exactly as the dog and bitch. While in conjunction she was pretty quiet; but when at liberty, she endeavoured to fly at the dog. In this way she was twice

* In the supplement to his works, he gives the following account which had been sent to him. "A very young she-wolf, brought up at the Marquis of Spontin's, at Namur, had a dog, of nearly the same age, kept with it as a companion. For 2 years they were at liberty, coming and going about the apartments, the kitchen, the stables, &c. lying under the table, and on the feet of those who sat round it. They lived in the greatest familiarity. The dog was a strong greyhound. The wolf was fed on milk for 6 months; after that, raw meat was given her, which she preferred to that which was dressed. When she ate no one durst approach her; but at other times people might do as they pleased, provided they did not use her ill. At first she made much of all the dogs which were brought to her; but afterward she gave the preference to her old companion, and from that time she became very fierce if any strange dog approached her. She was lined for the first time on the 25th of March; this was frequently repeated while her heat continued, which was 16 days; and she littered the 6th of June, at 8 o'clock in the morning; the period of gestation was therefore 73 days at the most.* She brought forth 4 young ones of a blackish colour, some of whose feet, and a part of the breast, were white; in this respect taking after the dog, who was black and white. From the time she littered she became surly, and set up her back at those who came near her; did not know her masters, and would even have killed the dog, had it been in her power."—Orig.

* This is a longer period than in the bitch by at least 10 days; but as the account was made from the first time of her being lined, and she was in heat for a fortnight, and lined in that time, it is very probable, if the time was known when she conceived, that it would prove to be the same period as in the dog.—Orig.

lined. She conceived, and brought forth 4 young ones. The time she went with young was not exactly known; but it was believed to be the same as in the bitch. Two of the puppies were like the dog in colour, who had large black spots on a white ground; one was of a black colour, and the 4th of a kind of dun, and would probably have been like the mother. She took great care of them, yet did not seem very anxious when one was taken from her by the keeper; nor did she seem afraid when strangers came into the room. Unfortunately these experiments were carried no further; one being sold to a gentleman, who carried it to the East-Indies; and the other 3 were killed by a leopard, one of which I was to have had. The same wolf was in heat in Dec. 1786, and was lined several times by a dog. She pupped on the 24th of Feb. 1787, and had 6 puppies, which may afford opportunities, if they are thought necessary, of repeating experiments on this subject.

While pursuing this subject, I was informed that Captain Mears of the Royal Bishop East-Indiaman, had brought home a bitch jackal with young, which had brought forth soon after his arrival; and that he had given the bitch jackal and one puppy to Mr. Bailey, bird-merchant, in Piccadilly. I went to see them, and purchased the puppy, the subject of the following experiment, which had dispositions very similar to the half bred wolf which I had from Mr. Brookes before-mentioned. To have a true history of this animal, I took the liberty of writing to Mr. Mears, who politely called on me, and, at my request, set down the particulars in the form of a letter to me, of which the following is a copy.*

I took this puppy into the country, and chained it up near a mastiff dog, and they were very familiar, and seemingly fond of each other. When the bitch became first in heat, I could not get a proper dog for her; but the latter end of Sept. being again in the same situation, several dogs were procured, and left with her. They appeared indifferent about her, probably from being in a strange place; and she did not seem inclined to be familar with them; whether the great dog might be able to line her I do not know; she was however twice tied by a tarrier on the 3d of Oct. In a few weeks she was evidently become larger; and on the 30th of November in all 59 days, she brought forth 5 puppies. Some days before this period she dug a hole under ground, by the side of her

* SIR,—I had the honour of yours the 15th inst.; and with regard to the female jackal, I can assure you, that she took a small spaniel dog of mine on board my ship, the Royal Bishop. I had her, when a cub, at Bombay; and a very short time before I arrived in England she got to heat, and enticed this small dog into the long boat, where I saw them repeatedly fast together. I brought her to my house in the country, where she pupped 6 puppies, one of which you have seen. Mr. Plaw, at N° 90, Tottenham-Court-Road, has a dog-puppy, which will be at your service at any time you chuse to send for him, to make any further experiments: I called on Mr. Plaw, and got his promise to let you have the dog. I have the honour to be, Sir, &c.

N° 107, Hatton-street, Jan. 16, 1786. WM. MEARS.

P. S. I had the bitch on board 14 months.

kennel, in which she brought forth, and it was some time before she would allow the puppies to stay in the kennel when put there. In about 8 days some, and 9 days others of them began to open their eyelids.

Here then is an absolute proof of the jackal being a dog; and it appears to me, that the wolf is equally made out to be of the same species. It now then becomes a question, whether the wolf is from the jackal, or the jackal from the wolf, supposing they had but one origin? From the supposition, that varieties become more tame in their nature, we should be led to believe, the wolf to be the original, and that the jackal was a step towards civilization in that species of animal. There are wolves of various kinds, each country having a wolf peculiar to itself; but the jackals that I have seen have been more uniformly the same, both those from Africa, and those from the East-Indies. I am informed, however, that they vary in size. Whether all the wolves of different countries are of one species, or some of them only of the same genus, I do not know; but I should rather suppose them to be all of one species. What is with me an argument in favour of this supposition is, that if there were wolves of distinct species, we should have had by this time a great variety of that species of wolves, with the various dispositions arising from variation in other respects; and those varieties now turned to very useful purposes, as has been the case with the dog; for all the wolves we are yet acquainted with, have naturally the principle of cultivation in them, as much probably as any animal, or as much at least as those wolves we now know to be dogs. The not having a civilized species of wolf is indeed with me a proof that they are all of the same species with the dog. If they are all of the same species with the dog, then the first variety that took place was still in the character of a wolf, differing only in colour, or some trivial circumstance, which could only take place from a difference in climate; civilization or cultivation in a state of nature being the same in them all. Where they became jackal, or what we now call dog, is difficult to say; or what dog we can call the first remove, as many dogs differ very much from each other; or whether the jackal is the intermediate link between the wolf and the dog. In either case we have 3 great varieties in this species, wolf, jackal, and dog, with the varieties in each. If the dog is proved to be the wolf tamed, the jackal may probably be the dog returned to his wild state.

To ascertain the original animal of a species, it is proper to examine all the varieties of that species, and see how far they have the character of the genus, and what resemblance they bear to the other species of the genus; for it is natural to suppose, that the original, or the animal which is nearest to it, will have more of the true character of the genus, and will have a stronger resemblance to the species nearest allied to it, than any of the other varieties of its own species. If we apply this to the dog, and consider the fox as a distinct species, which thee is great reason to believe it is, that variety which has the strongest resemblance to the fox, is to be considered as the original of all the others; which will prove to be the wolf.

Another mode of considering this subject, which is however secondary to the above, is, supposing that all animals were at first wild; and therefore that those animals which remain wild, are the original stock; and that the further we find animals removed from their originals in appearance, they are really further removed in consequence of variation taking place from cultivation, so that we may still be able to trace the gradation. What gives some force to this idea is, that where the dogs have been least cultivated, there they still retain most of their original character, or similarity to the wolf or the jackal, both in shape and disposition. Thus the shepherd's dog, all over the world, has strongly the character of the wolf or jackal; so that but little difference is to be observed, except in size and hair. Size is perhaps a variety taking place under a variety of circumstances; but difference in hair is in general influenced by climate, though perhaps not always so. Thus the wolf has longer and softer hair than the jackal, because he is a more northern animal; and the jackal and shepherd's dog in Portugal and Spain have shorter and stronger hair than those of Germany or Kamchatka, from inhabiting warmer climates. But when we consider their general shape, the character of countenance, the quick manner with the pricked and erect ears, we must suppose them varieties of the same species. The smelling at the tail has been described as characteristic of the dog; but I believe it is common to most animals, and only marks the male; for it is the most certain way the male has of knowing the female, and also discloses another scent, which is the final intention, whether the female is disposed to receive the male.

The Esquimaux dog, and that found among the Indians as far south as the Cherokees; the shepherd's dog in Germany, called Pomeranian; the shepherd's dog in Portugal and Spain; have all a strong similarity to the wolf and jackal. Buffon, on the origin of dogs, seems to have possessed nearly the same idea; for he says the shepherd's dog is the original stock from which the different races of dogs have sprung.

As the wolf turns out to be a dog, it seems astonishing, that there was no account of dogs being found in America. But this I consider as a defect in the first history of that country, for there are wolves; and I think, in spite of all that has been said to the contrary, the Esquimaux and Indian dog is only a variety from a wolf in that country, which had been tamed. Mr. Cameron, of Titchfield-street, who was many years among the Cherokees, and considerably to the westward of that country, observes, that the dog found there is very similar to the wolf; and that the natives consider it to be a species of tame wolf; but as we come more among the Europeans who have settled there, the dogs are more of a mixed breed; for why they should only have had this kind of dog transported among them, while every other part of America has the varieties of Europe, is not easily solved.

The voice of animals is commonly characteristic of the species; but I should suppose it is only characteristic of the original species, and not always of the variety, and this supposition holds good in the dog species. It would appear, that the voice of the wolf and the jackal is very similar, and is principally conveyed through the nose, and exactly resembles that noise in dogs, which is a mark of longing or melancholy, and also of fondness; but has no resemblance to the bark of the dog, which they do not perform. Barking is peculiar to certain varieties of the dog kind, and even some that do bark, do it less than others. The dogs in the South-Sea islands do not bark: our greyhound barks but little; while the mastiff, and many of the smaller tribe, as spaniels, are particularly noisy in this way. It would appear as if the frequency of this noise arose from imitation; for the dogs in the South-Seas learn to bark; and others, as the hound, have a peculiar howl, which by huntsmen is called the tongue. This noise, as also the bark, is made by opening the mouth. A variety in the voice, or some parts of the voice, in the varieties of the same species, is not peculiar to the dog.

XXV. Experiments on the Congelation of the Vitriolic Acid. By James Keir, Esq., F. R. S. p. 267.

That the vitriolic acid sometimes assumes a solid, crystalline state, has been observed by Basil Valentine, and by many later chemists; but their relations of this appearance are neither sufficiently explicit, with regard to the essential and concomitant circumstances, nor do they seem very consistent with each other. It appears however, that two very distinct species of congelation of this acid have been noticed. That which is described by the older chemists, and also by some modern authors, requires no greater degree of cold than the common temperature of the air, even in summer, and is peculiar to that acid which is obtained by distillation from martial vitriol, and which is possessed of a smoking quality in a high degree: for not only the authors, by whom this congelation has been observed, have given this description of the acid employed, but also the late experiments of M. Dollfuss (Crell Annalen 1785) seem to show, that the smoking quality is essential to the phenomenon; for neither the acid obtained from vitriol, when deprived by rectification of its smoking quality, nor the English oil of vitriol (which is known to be obtained by burning sulphur, and which does not smoke), were found, by his trials, to be susceptible of this species of congelation. The acid thus congealed has been called glacial, or icy oil of vitriol. The other kind of congelation has been little noticed till lately. To this congelation every kind of vitriolic acid is subject, whether it smokes or not, and whether it has been prepared from martial vitriol, or from sulphur, provided the cold to which it is exposed be sufficiently intense: for the cold requisite for this species of congelation, is considerably greater than what is sufficient for the former.

Mr. Macquer relates, in the 2d edition of his Dict. of Chemistry (article, Vitriolic Acid), that the Duke d'Ayen had observed the congelation of concentrated vitriolic acid, which had been exposed to a cold expressed by 13 or 14 degrees below 0 of Reaumur's scale; but that mixtures, consisting of 1 part of the above-mentioned concentrated acid, with 2 or more parts of water, could not be frozen by the cold to which he exposed them, till he had diluted the acid so much, that its density was to that of water as $104\frac{1}{4}$ to 96; in which latter case of congelation, it is probable, that the water only did freeze, as it does in dilute solutions of neutral salts. M. de Morveau (Mem. de l'Acad. de Dijon, pour 1782) made similar experiments, with a view to verify those of the Duke d'Ayen, and with similar success. By means of an intense cold, produced by adding spirit of nitre to pounded ice, he congealed a part of some vitriolic acid, which he had previously concentrated. He observed, that though a very intense cold had been employed to freeze the concentrated acid, it nevertheless remained congealed in much less degrees of cold, and that it thawed very slowly. Lastly, some experiments have lately been made by Mr. M'Nab, at Hudson's Bay, on the congelation of acids by intense cold; an account of which experiments is given in the Phil. Trans. for 1786, by Mr. Cavendish, at whose desire they had been made. These experiments are the more valuable, as the density of the acids employed, and the temperature, and other concomitant circumstances, have been distinctly noted; and they are rendered still more interesting, by the very judicious remarks made on them by Mr. Cavendish. It is there related, that a vitriolic acid, whose specific gravity was to that of water as 1843.7 to 1000, froze when exposed to a cold of — 15° of Fahrenheit's scale; that another more dilute vitriolic acid, consisting of 629 parts of the former concentrated acid, and 351 parts of water, congealed in a temperature of — 36°; and that when the acid was further diluted, it was found capable of sustaining a much greater cold without freezing. In these experiments, as also in those of M. de Morveau, it appeared that the whole of the acid did not congeal, but that part of it retained its fluidity. Mr. Cavendish found, on examining the part which had congealed, and that which had remained fluid, that they were nearly of the same strength; and he is thence led to think, that the difference between them, by which the one is more disposed to congeal than the other, does not depend on their different strengths, but on some quality less obvious, and the same which constitutes the difference between glacial and common oil of vitriol. In all the experiments which had been made by the Duke d'Ayen, M. de Morveau, and Mr. M'Nab, the vitriolic acid, when strong, had frozen with less cold than when diluted; but these experiments did not enable Mr. Cavendish to determine, whether this acid has any determinate strength or point of easiest freezing (such as he had discovered to be possessed by spirit of nitre), or whether the cold requisite for congelation does not continually diminish, as the strength of the acid increases, without limitation. This latter opinion he thinks the most probable, from the circumstance of the Duke d'Ayen's and M. de Morveau's acids having frozen with a considerably less intense cold than those of Mr. M'Nab, which, he supposes, were weaker, as the former acids had been concentrated purposely.

The observations which I have made, (says Mr. K.) and am going to relate, apply solely to the latter kind of congelation of the vitriolic acid, as the acid which I employed was of the kind that is prepared by burning sulphur, and is commonly sold in England under the name of oil of vitriol, and was perfectly free from colour, smell, or smoking quality. After a severe frost at the end of the year 1784, and beginning of 1785, I observed that some vitriolic acid contained in a corked phial, had congealed; while other parcels of the same acid, some stronger and some weaker, equally exposed to the cold, had remained fluid. As I imputed the congelation to the great intensity of the cold, I was afterwards much surprized, when the frost ceased, to find that the acid remained frozen during many days, when the temperature of the air was sometimes above 40° of Fahrenheit's scale; and when the congealed acid was brought into a warm room, purposely to thaw it, a thermometer, placed in contact with it during its thawing, continued stationary at 45°. From these circumstances I concluded, that the freezing and thawing point of this acid was very near the last mentioned degree; and accordingly, on exposing the liquor which had been thawed to the air, at the temperature of 30°, the congelation again took place in a few hours. From the circumstance of other parcels of the same acid, but of different strengths, remaining fluid, though they had been exposed to a much greater cold than was necessary for the congelation of that acid liquor which had frozen, I was led to believe, that there must be some certain strength at which the vitriolic acid was more disposed to freeze than at any other, greater or less. I knew that the specific gravity of the acid which had frozen was nearly to that of water as 1800 to 1000, and that of the stronger acid, which had not frozen, was as 1846 to 1000; which last is the usual density of the oil of vitriol commonly sold in England. I knew also, that the acid which had frozen was in no respect but in strength different from the stronger acid which had retained its fluidity; having myself, some weeks before, taken the former acid from the bottle containing the latter, and diluted it with water till it was reduced to the specific gravity of 1800.

Though from the above observations I was convinced of the proposition generally, that the vitriolic acid is most disposed to freeze when at a certain strength, and then it is susceptible of congelation by means of much less cold than has been hitherto imagined; yet, as only part of my acid had frozen, I could not with certainty know the strength of the frozen part, and I therefore was not able to state, with any accuracy, the degree of strength most favourable to congelation, nor the limits of strength within which the acid may be congealed by such moderate cold. In the following winter I had not leisure to pursue the subject; but since the commencement of the present year, I have verified my former observation with more attention to the exact densities of the acids; and I have found, that the point of strength most favourable to congelation is very determinate, and that a very small variation above or below that point renders the acid incapable of freezing without a considerable augmentation of cold. As the acid, when brought to the proper strength, was capable of freezing with less cold than water does, I immersed several acids of different strengths in melting snow, instead of exposing them to the air, the temperature of which was variable, whereas that of melting snow was constant and determinate. Those acids which would not freeze in melting snow, were afterwards immersed in a mixture of snow, water, and common salt, the temperature of which was not so constant and determinate as that of melting snow; but it generally remained for several hours at about 18°, and was sometimes several degrees lower. The intention of adding water to the snow and salt was to lessen the intensity of the cold of this mixture, and to render it more permanent than if the snow and salt alone were mixed.

The acids which had frozen in melting snow, and which were five in number, having been thawed and brought to the temperature of 60°, were found on examination to have the annexed specific gravities.

1786
1784
1780
1778
1775

Those acids which would not freeze in melting snow, but which froze when immersed in snow, water, and salt, having been exposed to a greater cold, were of a greater latitude of density. Their specific gravities, when brought to the temperature of 60°, were found to be expressed by the annexed numbers.

1814
1810
1804
1794
1790
1770
1759
1750

The acids which remained, and which would not freeze either in melting snow, or in the mixture of snow, salt, and water, were found on examination to have the annexed specific gravities.

1846
1839
1815
1745
1720
1700
1610
1551

It appears, from the 1st table of specific gravities, that the medium density of the acids which did freeze with the cold of melting snow was 1780; and from the 2d table it appears that, at the densities of 1790 and 1770, the acid had been incapable of freezing with that degree of cold. Hence it follows, that 1780 is nearly the strength or density of easiest freezing; and that an increase or diminution of that density, equal to $\frac{1}{178}$th part, renders the acid incapable of freezing with the cold of melting snow, notwithstanding this cold is some degrees above the freezing point of the most congelable acid. From the 2d table of specific gravities it appears, that by applying a more intense cold, namely, that produced by a mixture of snow, salt, and water, the limits of the density of the acids capable of congelation were extended to about $\frac{3}{178}$ above or below the point of easiest freezing: and there seems little reason to doubt that, by greater augmentations of cold, these limits may be further extended; but in what ratio these augmentations and extensions proceed cannot be determined without many observations made in different temperatures.

Though it is probable that the most concentrated acids may be frozen, provided the cold be sufficiently intense, yet there seems reason to believe, that some of the congelations which have been observed in highly concentrated acids have been effected in consequence of the density of these acids having been reduced nearly to the point of easy freezing by their having absorbed moisture from the air: for the Duke d'Ayen and M. de Morveau exposed their acids to the air, in cups or open vessels; and the latter author even acquaints us, that on examining the specific gravity of the acid which had frozen, he found it to be to that of water as 129 to 74; which density being less than the point of easiest freezing, proves that the acid which he employed, and which he had previously concentrated, had actually been weakened during the experiment. I have several times exposed concentrated oil of vitriol in open vessels in frosty weather; and I have sometimes, but not always, observed a congelation take place. On separating the fluid from the congealed part, and on examining the specific gravity of the latter, after it had thawed, I found that it had been reduced to the standard of easiest freezing. When the congealed acid was kept longer exposed, it gradually thawed, even when the cold of the air increased; the reason of which is not to be imputed to the heat produced by the moisture of the air mixing with the acid, for this cause operated during the congelation, but principally to the diminution of density below the point of easy freezing, which was occasioned by the continued absorption of moisture from the air, and which rendered the acid incapable of continuing frozen without a great increase of cold.

It appears then, that the concentration of M. de Morveau's acid, at the time of its congelation, from which circumstance Mr. Cavendish infers generally, that the vitriolic acid freezes more easily as it is more dense, is not a true premise; and that therefore the inference, though justly deduced, is invalid. On the contrary, there seems every reason to believe, from the analogy of my experiments abovementioned, that as the density of the acid increases beyond the point of easiest freezing, the facility of the congelation diminishes; at least to as great density as we have been ever able to obtain the vitriolic acid; for if it were possible to divest it entirely of water, it would probably assume a solid state in any temperature of the air.

The crystallization of the frozen vitriolic acid is more or less distinct, according to the slowness of its formation, and other favourable circumstances. Sometimes the crystals are very distinctly shaped, large, and very hard. Their form is the same as the common form of mineral alkali and of selenitic spar, but with angles different in dimensions from either of these. They are solids consisting of 10 faces, of which the 2 largest are equal, parallel, and opposite to each other, and are oblique-angled parallelograms or rhomboids, whose angles are, as near as I could measure them, of 105 and 75 degrees. Between these 2 rhomboidal faces are placed 8 faces of the form of trapeziums. Thus each crystal may be supposed to be composed of 2 equal and similar frustums of pyramids joined together by their rhomboidal bases. I observed, that the crystals always sunk in the fluid acid to the bottom of the vessel, which showed that their density was increased by congelation. I thought of ascertaining their specific gravity by adding gradually to this fluid part some concentrated vitriolic acid, till the crystals should float in the liquor, the examination of whose specific gravity would determine that of the floating crystals. But I was surprized to find that the crystals sunk even in the concentrated acid, and consequently were denser. I then poured some of the congelable acid, previously brought to the freezing temperature, into a graduated narrow cylindrical glass, up to a certain mark, which indicated a space equal to that occupied by 200 grains of water. The glass was placed in a mixture of snow, salt, and water, and when the acid was frozen, a mark was made on the part of the glass to which the acid had sunk. Having thawed the acid, and emptied the glass, I filled it with water to the mark to which the acid had sunk by freezing, and I found, that 15 grains more of water were required to raise it to the mark expressing 200 grains; which shows that the diminution of bulk, sustained by the acid in freezing, had been equal to $\frac{1}{13.3}$ of the whole.

Computing from this datum, we should estimate the specific gravity of the congealed acid to have been 1924; but as it contained evidently a great number of bubbles, its real specific gravity must be considerably greater than the above determination, and cannot easily be ascertained on account of these bubbles. By way of comparison, I observed the alteration of bulk which water contained in the same cylindrical glass would suffer by freezing; and I found that its expansion was equal to about $\frac{1}{10}$th part of its bulk. The water had been previously boiled; but yet it contained numberless bubbles. In this respect then there is a remarkable difference between the congelations of water and of vitriolic acid; but perhaps the difference arises principally from the bubbles of elastic fluid, which may be in greater quantity, and may add more to the bulk of the water than of the acid.

Greater cold is produced by mixing snow or pounded ice with the congealed than with the fluid acid, but the quantity I have not determined. There is reason to believe it may be considerable. In the experiments made at Hudson's Bay, by Mr. M'Nab, the greatest cold which he had produced by mixing acids with snow, was effected by a vitriolic acid which had previously congealed; and to this circumstance of the congelation of the acid, Mr. Cavendish justly imputes the intensity of the cold, as the liquefaction of both the frozen acid and the snow had concurred towards this effect; whereas, in mixing fluid acids with snow, the thawing of the snow is probably the sole productive cause.

I was desirous of comparing the times required for the liquefaction of ice and of congealed acid, when both were exposed to the same temperature. For this purpose I filled 2 equal and similar cylindrical glasses; one with the congelable vitriolic acid, and the other with water; and, after having immersed them in a freezing mixture till both fluids were frozen, and reduced to the temperature of 28°, I withdrew the glasses from the freezing mixture, wiped them dry, and placed them together in a room, where the thermometer stood at 62°. In 40 minutes the ice was thawed, and in 95 minutes the acid was liquefied, at the end of which time the thermometer, which stood near the glasses, had risen to 64°. It appears then, that the congealed acid requires more than twice the time for its liquefaction, when exposed to that temperature, that ice does; but I do not think that we can infer, that the heats absorbed and rendered latent, as some late philosophers express themselves; or, in other words, that the cold generated by the liquefaction of ice and of congealed acid, are in the above proportion of the times, from the following consideration; that, as during the liquefaction of the ice, its temperature remains stationary at 32°, and during the liquefaction of the acid, its temperature remains about 44° or 45°, the ice, being considerably colder than the acid, will take the heat from the contiguous air much faster.

The experiment does however show, that a considerable quantity of cold is generated by the liquefaction of this acid; and hence it appears probable, that in making experiments of producing cold artificially, by mixing snow with acids in very cold temperatures, it would probably be useful to employ a vitriolic acid of the proper density for congelation, and to freeze it previously to its mixture with snow. It must not however be imagined, that the cold generated by the mixture of these 2 frozen substances, is nearly equal to the sums of the colds generated by the separate liquefactions of the congealed acid and ice, when singly exposed to a thawing temperature: for the mixture resulting from the liquefaction, consisting of the vitriolic acid and the water of the snow, appears, from the generation of heat which occurs in the mixture of these ingredients in a fluid state, to be subject to different laws relatively to heat, than either of the ingredients separately. And the heat thus generated, as soon as the congealed acid and ice are brought to a fluid state, must counteract in some measure, the cold produced by the liquefaction.

The vitriolic acid, like water and other fluids, is capable of retaining its fluidity when cooled considerably below its freezing point. I placed a phial, containing some congelable vitriolic acid, in a mixture of salt, snow, and water; and soon afterwards, while the acid was yet fluid, I immersed in it a thermometer, the mercury of which quickly sunk from 50° to 29°. While I was moving the thermometer in the fluid, in order to make it acquire the exact temperature, I saw the mercury suddenly rise, and on looking at the acid, I observed numberless small crystals floating in it, which had been suddenly formed. The degree to which the mercury then rose was 46½°. Another time, while the acid was freezing, the thermometer placed in it stood at 45°.

From the above observations, the following inferences may be drawn. 1st, That the vitriolic acid has a point of easiest freezing; that is, there is a certain strength or density, at which this acid freezes with considerably less cold than at any other strength, greater or less; and that this density is nearly to that of water as 1780 is to 1000. 2dly, That the greater or less disposition of congelation of the vitriolic acid, which is free from the smoking quality peculiar to the acid obtained by distilling martial vitriol, does not depend on any other quality or circumstance than its strength or density. 3dly, That the freezing and thawing degree of the most congelable acid, is about 45° of Fahrenheit's scale. It is however to be observed, that this degree is inferred from the temperature indicated by the thermometers immersed in the freezing and thawing acids; but that I never effected the congelation of the fluid acid, without exposing it to a greater cold, namely, either that of melting snow, or of the external air in frosty weather. Like water, this acid possesses the property of retaining its fluidity when cooled several degrees below its freezing point; and of rising suddenly to this point, when its congelation is promoted by agitation, or by contact with even a warmer thermometer. 4thly, That, like water and other congelable fluids, the vitriolic acid generates cold during its liquefaction, and heat during its congelation; the quantity of which heat and cold, so generated, remains to be determined by future experiments. 5thly, That the acid, by congelation, when the circumstances for distinct crystallization are favourable, assumes a regular crystalline form, a considerable solidity and hardness, and a density much greater than it possessed in a fluid state.

With respect to the first mentioned species of congelation, which is peculiar to the smoking vitriolic acid that is procured from martial vitriol, though I have had no opportunity of seeing it, as all the vitriolic acid, that is used in this country, is obtained by burning sulphur, yet I will beg leave to suggest, that it may be worth the attention of those chemists to whom it occurs, to observe more accurately than has been done, the freezing temperature and the density of the congelable acids; and to examine whether the density of this smoking acid also is connected with the glacial property. It seems further to be deserving of investigation, whether there is not some analogy between the congelation of the smoking oil of vitriol, and the very curious crystallization which Dr. Priestley observed in a concentrated vitriolic acid, saturated with nitrous acid vapours*; and whether this smoking quality does not proceed from some marine or other volatile acid, which may be contained in the martial vitriol, whence the vitriolic acid is obtained.

XXVI. Of some New Experiments on the Production of Artificial Cold.† *By Thomas Beddoes, M. D. Dated Oxford, May* 2, 1787. p. 282.

Mr. Walker, apothecary to the Radcliffe Infirmary, has been engaged upwards of a year in a series of experiments on the means of producing artificial cold, several of which seem to be very remarkable, and such as, considering their novelty, and the attention which has lately been paid to this subject, I flatter myself, says Dr. B., will be found to deserve a place among the Transactions of the R. S. Mr. Walker, in his first experiments, found, as Boerhaave had done before him, that sal ammoniac, as well as nitre, well dried in a crucible, and reduced to a fine powder, will produce a greater degree of cold than if they had not received this treatment. But Boerhaave, by sal ammoniac, lowered the temperature of water only by 28°; whereas Mr. Walker observed his thermometer to fall 32°, and when he used nitre 19°. It occurred to him, that the combination of these substances would produce a greater effect than either separately: and he found that this was really the case. A proposal for freezing water in summer, mentioned by Dr. Watson (Essays 3, 139) determined him to attempt the same thing in this way. Accordingly, April 28, 1786, the thermometer standing at 47°, he made a solution of a powder, consisting of equal parts of sal ammoniac and nitre, in a basin, by means of which he cooled some water, contained in a glass tumbler, to 22°. To this he added some of the same powder, and immersed 2 very small phials in it; one containing boiled, the other unboiled water; when he soon found the water in the phials to be frozen, the unboiled freezing first.

Having observed that Glauber's salt, when it retains its water of crystallization, produces cold during its solution, he thought of adding this to his other powers, and July 18, 1786, reduced the thermometer 46°. In this experiment the fol-

* Experiments and Observations relating to various Branches of Natural Philosophy, vol. i. p. 26 and 450. M. Cornette has also effected the crystallization of vitriolic acid by distilling it with nitrous acid and charcoal. Memoir. de l'Acad. des Scienc. Paris, pour 1779.—Orig.

† See a further account of these experiments by Mr. Walker in the 78th, 79th, 86th, and 93d vols. of the Phil. Trans. The greatest degree of artificial cold was produced without pounded ice or snow, by a mixture of 9 parts phosphate of soda, 6 parts nitrate of ammonia, and 4 parts diluted nitric acid. With pounded ice or snow, the greatest degrees of cold were produced by mixing 3 parts muriate of lime with 1 part snow; or by adding 10 parts diluted sulphuric acid to 8 parts snow.

lowing proportions were used: the temperature of the air being 65°, to water 4 oz., at 63°, were added,

Of sal ammoniac ʒ xi thermometer sunk to 32°,	that is,	31°
Of nitre ʒ x. . . . . 24°,	that is,	8°
Of Glauber's salts ℥ ij . . . . 17°,	that is,	7°
		46°

In this way he froze water on a day so hot that the thermometer in the shade stood at 70°. By first cooling the salts and water in one mixture, and then making another of these cooled materials, he sunk the thermometer 64°.

August 28. The temperature of the air being 65°, $\frac{1}{2}$ an oz. of rectified spirit of wine was diluted with $3\frac{1}{2}$ oz. of water, and immersed in the same frigorific mixture. When cooled to 24°, it began to freeze. A quantity of the the neutral salts, likewise cooled in the mixture, were put into the diluted spirit, when the thermometer fell to — 4°, so that the liquor was cooled 69 degrees. Spirit of nitre, diluted in the manner described by Mr. Cavendish (Phil. Trans., vol. 76), having reduced the thermometer to — 3°, sal ammoniac was added, on which it fell to — 15°. Nitrated volatile alkali, during its solution in water, reduced the thermometer to 35° (from 50° to 15°); but the cold was not increased by sal ammoniac or nitre.

Mr. Walker's most remarkable experiment was made on the 21st of March, 1787, when he found that nitrous acid, when poured on Glauber's salt, produced effects nearly the same as when it is poured on pounded ice; and that the cold thus produced, is rendered still more intense by the addition of sal ammoniac in powder. Mr. Walker, by many trials, discovered that the best proportion of these ingredients is the following: of concentrated nitrous acid, 2 parts by weight, of water 1 part; of this mixture cooled to the temperature of the atmosphere 18 oz., of Glauber's salt $1\frac{1}{2}$ lb. (avoirdupois), and of sal ammoniac 12 oz. On adding the Glauber's salt to the nitrous acid, thus diluted, the thermometer fell from + 51° to — 1°, or 52 degrees; and on adding the sal ammoniac it fell to — 9°, that is full 60 degrees. Nitrated volatile alkali, employed instead of sal ammoniac, produced a cold rather more intense. By means of this mixture, in a very few minutes, in the elaboratory before the class, I froze some spirits above proof, diluted with an equal bulk of water; and another gentleman this day sunk the thermometer 68°.

On April 20, 1787, Mr. Walker effected the congelation of quicksilver by a combination of these mixtures, without a particle of snow or ice. When he began his experiment the temperature of the mercury was 45°; so that, the freezing point of that metal being — 39°, there were produced 84° of cold. This experiment was performed as follows. Four pans, of sizes progressively diminishing, so that one might be placed within the other, were procured. The largest of these pans was placed in another vessel still larger, in which the materials for the 2d frigorific mixture were thinly spread, in order to be cooled. The 2d pan, containing the liquor (viz. vitriolic acid properly diluted) was placed in the largest pan. The 3d pan, containing the salts for the 3d mixture, was immersed in the liquor of the 2d pan; and the liquor for the 3d mixture was put into wide-mouthed phials, which were immersed in the 2d pan likewise, and floated round the 3d pan. The 4th pan, which was the smallest of all, containing its cooling materials, was placed in the midst of the salts of the 3d pan. Of the materials for the mixtures to be made in these 4 pans, the 1st and 2d consisted of diluted vitriolic acid and Glauber's salt, the 3d and 4th of diluted nitrous acid, Glauber's salt and sal ammoniac, in the proportions assigned.

The pans being adjusted in the manner above described, the materials of the first and largest pan were mixed: this mixture reduced the thermometer to + 10, and cooled the liquor in the 2d pan to 20; and the salts for the 2d mixture, which were placed underneath in the large vessel, nearly as much. The 2d mixture was then made with the materials thus cooled, and it reduced the thermometer to 3°. The ingredients of the 3d mixture, by immersion in this, were cooled to + 10°, and when mixed reduced the thermometer to — 15°. The materials for the 4th mixture were cooled by immersion in this 3d mixture to about — 12°. On mixing they made the mercury in the thermometer sink rapidly, and as it appeared to Mr. Walker, below — 40°. Its thread seemed to be divided below that point; but the froth occasioned by the ebullition of the materials prevented his making so accurate an observation as he could have wished.

The reason why this last mixture reduced the thermometer more than the 3d, though both were of the same materials, and the last at a lower temperature, Mr. Walker imagines to have been partly because the 4th pan had not another immersed in it to give it heat, and partly because the materials were reduced to a finer powder. I should imagine, that mercury reduced to its freezing point will freeze more quickly than water reduced to its freezing point, because it appears, from experiments on their capacity for heat, that the latter of these bodies has so much more latent heat in its liquid state; which greater quantity of latent heat must, as it becomes sensible, more retard the congelation.

I forbear to enumerate many variations of these experiments which Mr. Walker has among his notes; but there is one mixture which, though its power is not equal to that last described, may prove very serviceable in experiments of this nature, on account of its cheapness. It consists of oil of vitriol diluted with an equal weight of water: added to Glauber's salt, it produces about 46 degrees of cold. The addition of sal ammoniac renders it more intense by a few degrees. One remarkable circumstance occurred to Mr. Walker, as he was endeavouring to ascertain the best strength of the vitriolic acid: he happened to be trying a mixture of 2 parts of oil of vitriol and one of water, when he observed that, at the temperature of 35°, the mixture coagulated as if frozen, and the thermometer became stationary; but, on adding more Glauber's salt, it fell again, after some little time, but so great a cold was not produced as when this circumstance did not occur, and when the acid was weaker. The same appearance of congelation took place with other proportions of acid and water, at other temperatures.

Mineral alkali, when it retained its water of crystallization, added to some of these mixtures heightened their effects. But when it had lost this water, it rather produced heat than cold; and the same thing is also true of Glauber's salt. This circumstance leads us, in some measure, to the theory of these phenomena. Water undoubtedly exists in a solid state in crystals; it must therefore, as in other cases, absorb a determinate quantity of fire, before it can return to its liquid state. On this depends the difference between Glauber's salt and fossil alkali in their different states of crystallization and efflorescence. The same circumstance too enables us to understand the great effect of Glauber's salt, which, as far as I recollect, has the greatest quantity of water of crystallization. Those therefore who shall chuse to pursue the path which Mr. Walker has opened to them, would do well to try combinations of salts containing much water of crystallization; but they must take care lest the effect should be diminished or destroyed by the formation of compounds that fix a smaller quantity of fire. It is however but justice to Mr. Walker to observe, that he has carried his experiments in this way very far, and with great ingenuity.

XXVII. An Account of a Doubler of Electricity, or a Machine by which the least conceivable Quantity of Positive or Negative Electricity may be Continually Doubled, till it becomes Perceptible by Common Electrometers, or Visible in Sparks. By the Rev. Abraham Bennet, M. A. p. 288.

This paper, with improvements, may be consulted in the author's electrical papers, collected and printed at Derby in 1789.

XXVIII. Some Particulars relative to the Production of Borax. In a Letter from William Blane, Esq., to Gilbert Blane, M. D., F. R. S. Dated Lucknow, Aug. 28, 1786. p. 297.

My journey to the northern mountains in Jan. last, says Mr. B., in attendance on the Vizier, gave me an opportunity of satisfying, in some degree, my curiosity on the subject you are so desirous of being informed of, the production and manufacture of borax. The place which his excellency visited is called

Betowle, and is a small principality in the first of the northern mountains, where they rise from the plains of Hindostan, and is distant from Lucknow about 200 miles N. E. The town is a principal mart, where the commodities of the mountains are exchanged for those of the plain. The raja, or prince of the country, holds his possessions in the hills as an independent sovereign; but for those on the plain he owes fealty, and pays tribute to the vizier. He therefore embraced this opportunity of paying homage in person to his lord. During his stay at court, I had an opportunity of making the inquiries I wished from his people, and particularly from his dewan or minister, who had with him some of the inhabitants of the place where the borax is made.

This saline substance, called in the language of this country swagah, is brought into Hindostan from the mountains of Thibet. The place where it is produced is in the kingdom of Jumlate, distant from Betowle about 30 days journey north. Jumlate is the largest of the kingdoms in that part of the Thibet mountains, and is considered as holding a superiority over all the rest. The place where the borax is produced is described to be in a small valley, surrounded with snowy mountains in which is a lake, about 6 miles in circumference, the water of which is constantly so hot, that the hand cannot be held in it for any time. The ground round the banks of the lake is perfectly barren, not producing even a blade of grass; and the earth is full of a saline matter in such plenty that, after falls of rain or snow, it concretes in white flakes on the surface, like the natron in Hindostan. On the banks of this lake, in the winter season, when the falls of snow begin, the earth is formed into small reservoirs, by raising it into banks about 6 inches high; when these are filled with snow, the hot water from the lake is thrown on it, which, together with the water from the melted snow, remains in the reservoir, to be partly absorbed by the earth, and partly evaporated by the sun; after which, there remains at the bottom a cake, of sometimes half an inch thick, of crude borax, which is taken up and reserved for use. It can only be made in the winter season, because the falls of snow are indispensably requisite, and also because the saline appearances on the earth are strongest at that season. When once it has been made on any spot, in the manner above described, it cannot be made again on the same place, till the snow shall have fallen on it and dissolved 3 or 4 times; after which the saline efflorescence re-appears, and it is again fit for the operation.

The borax, in the state above described, is transported from hill to hill on goats, and passes through many different hands before it reaches the plains, which increases the difficulty of obtaining authentic information on the original manufacture. When brought down from the hills, it is refined from the earth and gross impurities by boiling and crystallization. I could obtain no answers to any questions on the quality of the water, and the mineral productions of the soil. All they could say of the former was, that it was very hot, very foul, and as it were greasy; that it boils up in many places, and has a very offensive smell: and the latter remarkable only for the saline appearances above described. That country however in general produces considerable quantities of iron, copper, and sulphur. After being purified, it sells in the market here for about 15 rupees per maund; and I am assured, by many of the natives, that all the borax in India comes only from the place above-mentioned.

I am afraid you will think this at best but a very unsatisfactory and unphilosophical account of the matter: but what can be done, where the only mode of information is through some of the wild and unsettled mountaineers? for the place is inaccessible even to the inhabitants of Hindostan, and has never been visited by any of them, except a few wandering Faquires, who have been sometimes led that way, either to do penance, or to visit some of the temples in the mountains. The cold in winter is described to be so intense that every thing is frozen up, and that life can only be preserved by loads of blankets and skins. In the summer again, the reflection from the sides of the mountains, which are steep and close to each other, there being little or no plain ground between them, renders the heats insufferable. I shall conclude with a few observations regarding the credibility of the relation; and first that it is really brought from the mountains is certain, as I have myself often had occasion to see large quantities of it brought down, and have purchased from the Tartar mountaineers, who brought it to market; 2dly, I have never heard of its being either produced or brought into this country from any other quarter; and 3dly, if it was made on the Coromandel coast, as some books mention, I think there can be little doubt, but that the whole process would have been fully inquired into, and given to the public long before this time.

XXIX. A Letter from the Father Prefect of the Mission in Thibet, F. Joseph da Rovato, containing some Observations relative to Borax. From the Italian. p. 301.

The father prefect of the mission in Thibet has the pleasure to acquaint the R. S., that, residing at Patna, he has frequently been desired by M. Vogles, an able naturalist from Germany, to obtain some circumstantial account of the places where, and the manner in which, the borax procured from the kingdom of Thibet is obtained; no one else, as he said, having any communication with those almost impenetrable parts. Though our mission have long since forsaken that kingdom, yet the Father prefect being somewhat connected with the Bahadur Shah, brother to the king of Nepal, whose kingdom extends northward as far as Kuti on the frontiers of Thibet, he wrote to him, and requested all the information that could be obtained on the subject. The Bahadur Shah, in order to give the best satisfaction in his power, was pleased to send to the prefect, as far as Patna, a man in his service, who, being a native of the country where the borax is prepared, could give the most ample intelligence concerning that substance.

This man, partly in the Nepalese and partly in the Hindoo language, both which are understood by the prefect, gave the following account. In the province or territory of Marmé, 28 days journey to the north of Nepal, and 25 to the west of Lassa, the capital of Thibet, there is a vale about 8 miles broad. In a part of this vale there are two villages or castles, the one named Scierugh, and the other Kanglé, the inhabitants of which are wholly employed in digging the borax, which they sell into Thibet and Nepal, having no other means of subsistence, the soil being so barren as to produce nothing but a few rushes. Near the two above-mentioned castles there is a pool of a moderate size, and some smaller ones, where the ground is hollow, in which the rain-water collects. In these pools, after the water has been some time detained in them, the borax is formed naturally; the men, wading into the water, feel a kind of a pavement under their feet, which is a sure indication that borax is there formed, and there they accordingly dig it.

Where there is a little water, the layer of borax is thin; and where it is deep, it is thicker, and over the latter there is always an inch or two of soft mud, which is probably a deposit of the water, after it has been agitated by rain or wind. Thus is the borax produced merely by nature, without either boiling or distillation. The water in which it is formed is so bad, that the drinking a small quantity of it will occasion a swelling of the abdomen, and in a short time death itself. The earth that yields the borax is of a whitish colour; and in the same valley, about 4 miles from the pools, there are mines of salt, which is there dug in great abundance for the use of all the inhabitants of these mountains who live at a distance from the sea. The natives, who have no other subsistence on account of the sterility of the soil, pay nothing for digging borax; but strangers must pay a certain retribution, and usually agree at so much a workman. This is paid to a Lama, named Pema Tupkan, who owns the pits in Marmé. Ten days journey farther north, there is another valley named Tapré, where they dig borax, and another still farther, called Cioga; but of this latter I have not marked the situation. Borax in the Hindoo and Nepalese languages is called Soaga. If it be not purified, it will easily deliquesce; and in order to preserve it any time, till they have an opportunity of selling it, the people often mix it with earth and butter. In the territory of Mungdan, 16 days journey to the north of Nepal, there are rich mines of arsenic; and in various other places are found mines of sulphur, as also of gold and silver, whose produce is much purer than those of the mines of Pegu.

XXX. On Hepatic Airs or Gases; by Mons. Hassenfratz. From the French. p. 305.

Having made some experiments on the different species of hepar sulphuris, in 1785, Mons. H. resolved to investigate the nature of the inflammable hepatic air, which is let loose when those hepars are decompounded by the nitrous acid. The sulphur which is seen to be precipitated after every combustion of inflammable hepatic gas, led him to believe that this substance (viz. sulphur) might be one of its constituent parts; but it taught him nothing more; and this was almost the only experiment which had at that time been made relative to its analysis. He therefore determined to have recourse to synthesis, and the more so, as he knew that M. Monges, by causing some fixed air to pass over sulphur in fusion, had obtained, as the result, fixed air which had a sulphureous smell, and which actually held some sulphur in solution. Mr. H. repeated this experiment, and he moreover instituted the following:

He first caused some fixed air to pass over sulphur in fusion, whereby he obtained a sulphureous fixed air, which threw down a precipitate from lime-water, uniting with the lime so as to form a [mild] calcareous earth, and disengaging the sulphur, part of which swam on the surface of the water, and part settled at the bottom. He made a similar experiment with nitrous gas, and obtained a sulphureous nitrous gas, which, during its combination with vital air [oxygen gas] so as to form nitrous acid, let go the sulphur.

Azotic gas [la mofête atmospherique] yielded, by a similar treatment, a sulphureous azotic gas, which, after it had stood some time over water, deposited its sulphur.

By the same treatment vital air [oxygen gas] was converted into sulphureous vital air, mixed with some volatile sulphureous acid, which the water absorbed. Nitrous gas being mixed with this sulphureous vital air, a redness ensued, together with a disengagement of sulphur. Also, when the sulphureous vital air was detonated with inflammable air, sulphur was precipitated.

Atmospheric air gave nearly the same result as vital air, except that the obtained air (as might easily be inferred from the previous experiments) was a mixture of sulphureous vital air and sulphureous azotic gas [mofête sulfureuse.] When nitrous gas was combined with this vital air* or when it was detonated with inflammable air, in both cases a precipitation of sulphur took place.

Lastly, when inflammable gas was made to pass over sulphur in fusion, a sulphureous inflammable air was obtained, exactly resembling the hepatic gas which is procured by pouring nitrous acid on liver of sulphur. Hence it may be inferred, that the inflammable air which is obtained by pouring nitrous acid

* That is, with the vital air of the atmospheric air so treated.

upon the different species of liver of sulphur, and which has been denominated hepatic gas, is nothing more than a sulphureous inflammable air, which may be formed synthetically, as well as all the other species of sulphureous gas.

XXXI. Botanical Description of the Benjamin Tree of Sumatra. By Jonas Dryander, M. A. Libr. R. S. and Member of the Royal Academy of Sciences at Stockholm. p. 307.

Though Garcias ab Horto, Grim, and Sylvius, were acquainted with the real tree from which Benjamin or Benzoin is collected, their descriptions of it are so imperfect and insufficient for its botanical determination, that succeeding botanists have fallen into many errors concerning it; and it is remarkable, that though this drug was always imported from the East-Indies, most of the later writers on the Materia Medica have conceived it to be collected from a species of Laurus, native of Virginia, to which, from this erroneous supposition, they have given the trivial name of Benzoin. This mistake seems to have originated with Mr. Ray, who in his Historia Plantarum, vol. 2, p. 1845, at the end of his account of the Arbor Benjoifera of Garcias, says: "Ad nos scripsit D. Tancredus Robinson Arborem resiniferam odoratam foliis citrinis prædictæ haud absimilem transmissam fuisse e Virginia a D. Banister, ad illustrissimum Præsulem D. Henr. Compton, in cujus instructissimo horto culta est.——Arbor ista Virginiana Citri, vel Limonii foliis Benzoinum fundens, in horto reverendissimi Episcopi culta."

This error was detected by Linnæus, but another was substituted by him in its place; for in his Mantissa Plantarum Altera, he tells us, that Benjamin is furnished by a shrub described there under the name of Croton Benzoe, and afterwards in the Supplementum Plantarum, describes again the same plant, under the name of Terminalia Benzoin. M. Jacquin, who had been informed that this shrub was called by the French Bienjoint, supposes, with reason, that the similar sound of that word with Benjoin, the French name for Benjamin, may have occasioned this mistake.*

Since that period Dr. Houttuyn has described the Benjamin tree of Sumatra; but for want of good specimens has been so unfortunate as to mistake the genus to which it belongs. It is hoped therefore, that the following descriptions may not be unworthy a place in the Philos. Trans.; they are made from dried specimens procured from Sumatra by Mr. Marsden, F. R. S. at the request of Sir Joseph Banks, Bart. P. R. S. and clearly prove that this tree agrees in the parts of fructification with the Styrax of Linnæus.

Styrax Benzoin. S. with oblong, acuminated leaves, downy beneath, and compound racemes of the length of the leaves.

* Hort. Vindob. vol. 3, p. 51.

Benjus. *Garcias ab Horto* in *Clusius's Exotics.*

Arbor Benzoini. *Grim* in *Ephem. Acad. Nat. Curios. Dec.* 2. *Ann.* 1. *p.* 370. *f.* 31. *Sylvius* in *Valentini Historia Simplicium. p.* 487.

Benzuin. *Radermacher* in *Act. Soc. Batav. vol.* 3. *p.* 44.

Benjamin or Benzoin. *Marsden's Hist of Sumatra, p.* 123.

Laurus Benzoin. *Houttuyn* in *Act. Harl. vol.* 21. *p.* 265. *t.* 7.

It is a native of Sumatra.

Description.—Branches round, tomentose.

Leaves alternate, footstalked, oblong, perfectly entire, acuminated, being, above smooth, beneath tomentose, a palm long. *Footstalks* round, striated, channelled, tomentose, very short.

Racemes axillary, compound, nearly the length of the leaves: *Footstalks common* tomentose; *partial* alternate, spreading, tomentose: *Pedicels* or Stalklets very short. *Flowers* on one side.

Calyx campanulated, very obscurely five-toothed, outwardly tomentose; above a line in length.

Petals five, (perhaps connate at the base,) linear, obtuse, outwardly grey with very fine down, four times longer than the calyx.

Filaments ten, inserted into the receptacle, rather shorter than the petals, beneath connate into a cylinder of the length of the calyx, ciliated on the upper part below the anthers.

Anthers linear, longitudinally adnate to the petals, and shorter by half than they.

Germ superior, ovate, tomentose. *Style* filiform, longer than the stamens. *Stigma* simple.

XXXII. *Of an Experiment on Heat. By George Fordyce, M. D., F. R. S.* p. 310.

Heat changes the qualities and appearances of matter in various ways. It is also a powerful agent in many of the operations which mankind employ to fit matter for their use. Though the ancients performed many of these operations with a considerable degree of accuracy, yet there are many which they were totally unacquainted with, and others they brought to little perfection. One principal cause was their having no means of measuring heat accurately. Van Helmont was the first who found the mode of measuring heat by expansion. His measure was an air thermometer, which is described in his Dissertation, named "Aer," cap. 12. Since his time, various improvements have been made on thermometers; many are still wanted. This instrument is however the foundation of modern discoveries on this subject. The ancients were acquainted with the manner of heating bodies by communication, by friction, by burning fuel, by the sun, by fermentation, and the taking place of chemical combinations in other cases. Boyle found, that melting a solid body produced cold (Experimental History of Cold, title 1, chap. 18): Dr. Cullen, that cold was also produced by converting bodies into vapour. It has been since that time found that the opposite condensations, viz. of vapours into fluids, or fluids into solids, generate heat. Cramer was the first who took notice of the different conducting powers of different bodies, in his "Ars Docimastica," p. 1, § 274, Scholium.

The power of animal bodies, of resisting the cold of the medium they are in, has been long known. Mr. Ellis took notice of their being also able to resist the heat. Dr. Cullen ascribed this power to a peculiar quality in animals different from the powers of inanimate matter. We saw a confirmation of this power being very great when we kept a dog of no large size (he might weigh about 25 lb.) in air heated to 160 degrees of Fahrenheit's thermometer for half an hour. We took him out with only the addition of a few degrees of heat; not from any uneasiness of the animal, but from being satisfied with the experiment. This power has been shown by Mr. Hunter to extend to vegetables. The degree of heat one body is capable of impregnating another with, was hardly touched on by any author before Dr. Crawford, who has done a great deal in this branch, and is still pursuing it.

The subject of the present inquiry is different from all these. The proposition is, supposing we can make an application to a cold body, so as to produce heat in it, and this application be made with the same force to the same body, whether by this means an equal quantity of heat will always be produced in an equal quantity of matter? That is, for instance, whether an equal quantity of the rays of the sun being thrown on an equal surface of the same matter, so that they shall be equally lost, bent, or reflected, an equal mass of matter below shall be equally heated according to its capacity; whether equal vibrations excited shall always produce the same quantity of heat; whether a chemical attraction taking place between an equal quantity of two substances shall always produce an equal quantity of heat?

The importance of this inquiry is sufficiently evident, since, if the same quantity of fuel being burnt, the same quantity of heat be always produced, our whole attention will be to take care that no part of the heat shall be lost; but if burning the fuel under one set of circumstances will actually produce a greater quantity of heat than burning it in other circumstances; or if burning it, will produce a great heat in one place, which cannot be carried to another place, but will be again annihilated, a very different attention must be paid. I was first led into this train of thinking by observing reverberatory furnaces. Formerly I had no doubt but that it was obvious, that the same quantity of fuel burnt would produce the same quantity of heat; but having occasion to try some experiments in reverberatory furnaces, where great heat and cleanness were required, I tried to heat the furnace with charcoal and coak, or pit-coal charred, that is, burnt till no smoke arises, but could never produce the heat required, though I could do it easily with coal. I insulated my furnace, so that after 24 hours strongest fire, it did not feel in the least warm on the outside. I heightened the chimney; but all to no effect: in the fire-place the heat was sufficient to melt malleable iron, but in the laboratory, in the horizontal part of the chimney, the heat was trifling. Since that time I have made various experiments to ascertain the proposition laid down. The following one, which has been varied and repeated with the same result, may perhaps draw the attention of chemists to this point.

I formed a cylinder of thin pasteboard, 6 inches diameter and 16 inches long. The inside I lined with rabbit skin, laying the fur smooth; a thin ring of pasteboard was placed in the middle. One end was closed with a bottom of the same pasteboard; the other was open. This cylinder was placed in the centre of another wider cylinder, also of pasteboard, which had likewise a bottom of pasteboard. It was so placed, as that the outer cylinder was distant from the inner $1\frac{1}{2}$ inch; at the bottom and sides the space between was filled with eider down, suffered to rise to as great a bulk as it would from its own elasticity. The two cylinders were even at top, and the space between them shut by a cover of pasteboard. In the side of the machine, a little below the middle of the inner cylinder, a pasteboard tube was made to pass through the outer and open into the inner, half an inch wide, for the insertion of a thermometer. A similar tube was placed a little from the middle, towards the other end of the smaller cylinder. A circular plate of pasteboard, 6 inches diameter, and about $\frac{1}{8}$ thick, weighing 1 oz. 102 gr. was pushed down the inner cylinder, till it was stopped by the ring. A circle of flint-glass, ground flat and parallel on both sides, was fixed over the mouth of the inner cylinder so as not to obstruct any part of it. A similar apparatus, as exactly as possible, was formed, excepting that the circular plate in the middle of the inner cylinder was iron, of the same dimensions with the pasteboard one, and weighing 12 oz. 62 gr. These apparatuses were set in a warm exposure for several months, to dry. The circular plates, destined to receive the direct rays of the sun, were placed as nearly perpendicular to the inner cylinder as possible. They were both covered with a black paint, sufficient to prevent the rays of the sun from penetrating either to the iron or pasteboard.

On July 28, 1786, the sun shining on a room facing about s. w. the air not cloudy, but not very bright; the air in the room 71°; at a quarter after 12, thermometers being passed through the tubes below the plates of iron and pasteboard, after standing a quarter of an hour, showed the heat 67° in both apparatuses. Both were now exposed to the sun, so that the rays fell perpendicular on the paint covering the plates, in equal quantity on each as nearly as possible. If there was any difference, rather more were thrown on the pasteboard diaphragm. In 5 minutes the thermometer below the pasteboard diaphragm showed 72°; the thermometer under the iron had hardly risen half a degree.

Progress of the rising of the thermometers.

Under paste-board diaphragm.	Under iron diaphragm.
72°	$67\frac{1}{2}$
75	70
80	76 +
85	83
90	88 +
95	94
100	100*
105	107
110	115
After 20 minutes	
110	121

* At this time thermometers were put through tubes into the chambers of the apparatus, between the glasses and diaphragms. The apparatus with the iron diaphragm raised this thermometer to 121°; that with the pasteboard to 120°.—Orig.

The apparatus with the pasteboard diaphragm was exposed still to the sun; that with the iron was removed, and suffered to cool till its thermometer showed 107°; it was then exposed again to the sun till it had acquired the heat of 110°, to which degree the apparatus with the pasteboard hardly reached. The windows were now shut. The heat of the room had arisen to 80°. Both the apparatuses were placed on a table; the doors were shut, so that there was no current of air.

Pasteboard apparatus, after 30 minutes.		Iron apparatus.
96		104
After 75 minutes, or 1^h 45′ from the beginning.		
83		89
After 2^h or 3^h 45′ from removal from the sun.		
78	room 75	80

A similar result arose when there were no glasses to exclude the external air. Likewise when the diaphragms were changed from one apparatus to the other.

The first thing to be noted in this experiment is, that the rays of the sun acted on the same black paint only; for it was so thick, that the rays could not penetrate to the iron or pasteboard below. The colour was the same, and there was the same gloss; if any thing, that on the iron, in the experiment related, was rather more glossy, that it might not be favoured, as in former experiments the results had been in favour of the iron apparatus acquiring the greatest heat. Every thing therefore was the same, except that the iron and pasteboard were of different weights, of different capacities of heat, and of different degrees of readiness to acquire heat, and communicate it.

It is evident, that a greater quantity of heat was actually produced in the apparatus with the iron diaphragm: for though in the first 2 or 3 minutes the pasteboard became hotter than the iron, yet as soon as the iron began to be sensibly heated, it became hot faster than the pasteboard, and actually became hotter, and even continued to do so, when the pasteboard no longer could produce more heat than was dissipated from the surface of the apparatus into the air. When they were set in an air equally cold the apparatus with the iron diaphragm was longer in cooling, though they were both of the same degree of heat when set by. This greater quantity of heat I ascribe to the iron's taking the heat from the black paint faster than the pasteboard, as being a better conductor. Just as if a plate of glass was placed on a plate of steel, and another, perfectly similar, was placed on a plate of clay, and both were placed equally among equal vibrating bodies. In this case it is clear, that much greater vibration would take place if the same means of exciting it were applied to that plate of glass attached to the plate of steel, than if they were applied to that attached to the clay. I do not mean to say, that heat is vibration; but merely to illustrate my idea of heat being only a quality, and not a substance. I am led to this not only by this experiment now related, but by various other considerations, which I shall not now insist on, as they are not sufficiently finished to be laid before this Society. I shall only add that, among other things which may be illustrated by it, one is, that all the planets may possibly be of the same heat; since, if the matter of which Mercury consists was averse to the generation of heat in proportion to the greater number of the rays of the sun it receives more than the Georgium Sidus, they would be both of the same heat, notwithstanding their different distances from the sun. I have already said, that I was led to an inquiry into the subject by the effect it has on chemical operations.

XXXIII. On an Observation of the Right Ascension and Declination of Mercury out of the Meridian, near his Greatest Elongation, Sept. 1786, *made by Mr. John Smeaton, F. R. S., with an Equatorial Micrometer, of his own Invention and Workmanship; and an Investigation of a Method of Allowing for Refraction in such kind of Observations.* p. 318.

After noticing several contrivances in telescopes, &c. as preparatory to his observations, Mr. S. says, the morning of Sept. 23, about a quarter past 5 o'clock, the air being clear and perfectly serene, it being then about an hour after Mercury's rising, and near $\frac{3}{4}$ of an hour before the rising of the sun, I very readily found Mercury with the telescope, and when found could easily see him with an opera-glass; and Mercury being then in a state of very little alteration of declination, I adjusted one of the declination wires to his apparent run, by making him traverse the whole field. The observations were then taken as in the first table; and in the evening I was lucky enough to get those of λ Ceti and ο Tauri, intending to repeat the whole the next morning and evening. The next morning proved cloudy, and so continued, that I saw the planet no more; but in the evening of the 26th, I found the stars come again so near the same declination, that I was encouraged to continue the observation to see what change would happen. It then came on bad rainy weather till the 30th, when I again repeated the observation, and found the stars to come so near in declination that I was fully satisfied of the stability of the instrument, so far at least as could regard 24 hours: but as I was then appointed to go a journey, and could have no other use for it, I locked the door of the observatory, leaving the instrument in its position, that I might see what change would happen by the time of my return; and was quite astonished to find, on the 13th of October, that it had remained in a manner unmoved; for it had suffered no more apparent alteration than what might occur by the errors of observing, and alterations of the clocks and transit.

Mr. S. then states his observations in several tables, not now necessary to be reprinted; from all which he made the following deductions:

Deduction of the position of Mercury from the preceding observations.

1*st*. *In right ascension.*

ο Tauri followed λ Ceti Sept. 23		24^m	59^s
——— 26		24	59.2
——— 30		24	59.1
——— Oct. 13		24	58.7
——— On a mean of the four		24	59
α Orionis followed ο Tauri Sept. 30	2^h	29	49.9
——— Oct. 13	2	29	50.1
——— at a mean	2	29	50
Now Mercury preceded λ Ceti Sept. 23	15	48	53.4
λ Ceti preceded ο Tauri by mean of four		24	59
ο Tauri preceded α Orionis by mean of two	2	29	50
Mercury therefore preceded α Orionis by	18	43	42.4

2*d*. *In declination.*

Sept. 23, A.M. Mercury passed the middle horary wire, south of its centre				1′	8″
Same evening ο Tauri passed the middle horary wire, north of it				30	26
Therefore Mercury passed the middle horary wire more s. than ο Tauri by				31	34
But Sept. 26, λ Ceti passed N. of centre	17′	11″	Diff.	13	7
——— ο Tauri ———	30	18			
——— 30, λ Ceti ———	17	11	———	13	6
——— ο Tauri ———	30	17			
——— Oct. 13, λ Ceti ———	17	11	———	13	4
——— ο Tauri ———	30	15			

From the smallness of the above differences we may infer, that very little uncertainty in declination had attended the passage of ο Tauri on Sept. 23.

On Sept. 30, ο Tauri passed N.	30′	17″	Sum	54′	37″
——— α Orionis ——— S.	24	20			
On Oct. 13, ο Tauri ——— N.	30	15	———	54	35
——— α Orionis ——— S.	24	20			

α Orionis then at a mean passed more south than ο Tauri	54′	36″
Mercury therefore on the 23d passed with more N. declination than α Orionis	23	2

The preceding deductions and remarks show the consistency of the observations with themselves; yet, from the position of the telescope, it being only elevated $11\frac{1}{4}^{\circ}$ above the horizon, it is necessary to examine how far the deductions above specified were capable of being affected by refraction. And in this respect it will appear, that if it be supposed, there is no difference in the quantity of refraction of such objects as appear within the limits of the field of view of this instrument, which is 1° 17′, then their relative positions to each other will not be affected by it. On a calculation Mr. S. finds that the two stars, so altered by refraction, will arrive together at the horary circle at the same time, and with the same difference of declination, as if no refraction had taken place. It is therefore only the difference of refraction which takes place in objects at different heights in the same field, that can alter their relative situations: however, it appears necessary to examine what this may amount to. Accordingly, on a further calculation he finds a correction of 6″ in declination, and $1^s.1$ in right ascension.

As therefore Mercury passed with 23′ 2″ more north declination than α Orionis, and passed through a part of the medium that lifted him up less; it therefore gave him less north declination than it did to α, and therefore apparently diminished the real difference; hence 6″ must be added to the apparent difference 23′ 2″, making it 23′ 8″ difference of declination: and as Mercury was lifted up less than α, he would not so soon come to the middle wire by $1^s.1$ as he should have done, he therefore came too late by $1^s.1$, which must be subtracted from the time of Mercury's passage the 23d of Sept. which will increase the time in which he preceded α Orionis; that is, $18^h\ 43^m\ 42^s.4$ increased by $1^s.1$ will become $18^h\ 43^m\ 43^s.5$ difference of right ascension.

Having thus determined that the 23d Sept. 1786, A. M. at $5^h\ 22^m\ 34^s.9$ mean time, Mercury preceded α Orionis $18^h\ 43^m\ 43^s.5$, and had then a more northern declination by 23′ 8″. But according to Dr. Maskelyne's Catalogue of 34 stars, the right ascension of α Orionis reduced to the time when he was observed is 85° 54′ 12″. Now as the whole circle of the sphere makes a revolution in the time that α Orionis makes one turn, which is $23^h\ 56^m\ 4^s.1$, then from this deduct $18^h\ 43^m\ 43^s.5$, remains $5^h\ 12^m\ 20^s.6$, for the time that α Orionis preceded Mercury in right ascension; but if α ran the whole rotation = 360° in $23^h\ 56^m\ 4^s.1$, what portion of it will be run in $5^h\ 12^m\ 20^s.6 = 18740^s.6$?

But 24^h = 86400 seconds, and 360 = 1296000 seconds:

Say then, as $86400^s : 18740^s.6 :: 1296000'' : 281109''$ =	78°	5′	9″
But, according to Dr. Maskelyne's select Catalogue, the right ascension of α Orionis for Sept. 30, 1786, was, (which add)	85	54	12
The r. asc. of Mercury at the time of observation was therefore	163	59	21

According to Dr. Maskelyne's select Catalogue α Orionis had declination north, corrected for precession	7°	21′	8″.8
The sum of aberration and nutation from Connoissance des Temps		+	8 .4
Gives the correct declination north of α Orionis	7	21	17 .2
To which add that Mercury passed more north		23	8
Mercury's declination therefore was	7	44	25 .2

The result.

1786, Sept. 23, A. M. at $5^h\ 22^m\ 35^s$ M. T.	Mercury's	right ascension	163	59	21
		declination north	7	44	25

XXXIV. A Remarkable Case of numerous Births, with Observations. By Maxwell Garthshore, M. D., F. R. S., and A. S. p. 344.

Copy of a Letter from Dr. Blane, Physician to his Majesty's Navy, and to St. Thomas's Hospital, F. R. S., to Dr. Garthshore, Physician to the British Lying-in Hospital. Dated Sackville-street, June 22, 1786.

A few days ago, I received from the country an account of a woman who was delivered of 5 children at a birth in April last. As your extensive experience and reading in this line of practice enable you to judge how far this fact is rare or interesting, I submit it to you whether it deserves to be communicated to the R. S. Mr. Hull, the gentleman who sent me the case, is a very sensible and ingenious practitioner of physic at Blackburn, in Lancashire. He attended the labour himself from beginning to end, and his character for fidelity and accuracy is well known to me, as he was formerly a pupil at the hospital to which I am physician; so that no fact can be better authenticated. He mentions also, that he has preserved all those 5 children in spirits; and, if desired, he will send them for the inspection of the Society.* GILBERT BLANE.

Margaret Waddington, aged 21, a poor woman of the township of Lower Darwin, near Blackburn in Lancashire, formerly delivered of one child at the full term of pregnancy, conceived a 2d time about the beginning of Dec. 1785, and from that period became affected with the usual symptoms that attend breeding. At the end of the first month, she became lame, complained of considerable pains in her loins, and the enlargement of her body was so remarkably rapid, that she was then judged by her neighbours to be almost half gone with child. At the end of the 2d month she found herself somewhat larger, and her breeding complaints continued to increase. When the 3d month was completed, she thought herself fully as large as she had formerly been in her 9th month, and to her former symptoms of nausea, vomiting, lameness, and pain of the loins, she had now added a distressing shortness of breath. She continued to increase so rapidly in size, that she thought she could perceive herself growing larger every day, and she was under the frequent necessity of widening her clothes. When she reckoned herself 18 weeks gone, she first perceived somewhat indistinctly the motion of a child. By the 20th of April, 1786, all her complaints were become much more distressing; she had much tension and pain over all the abdomen, her vomiting was incessant, and she now could not make water but with the utmost difficulty. The symptoms being palliated by Mr. Lancaster, she advanced in her pregnancy to Monday the 24th of April, when being supposed to have arrived at the 20th week, she was seized with labour pains. These continued gradually to increase till the next day, about 2 in the afternoon; at which time I was sent for, Mr. Lancaster being absent, and she was soon delivered of a small, dead, but not putrid, female child. The pains continuing, this was soon followed by a 2d less child; to this very soon succeeded a 3d, larger than the first, which was alive; to these a 4th soon followed, somewhat larger than the first, and very putrid; last of all, there soon

* They were accordingly sent; and having been exhibited to the Society when this paper was read, were afterwards deposited in the Museum of Mr. John Hunter.—Orig.

succeeded a 5th child, larger than any of the former, and born alive. These 5 children were all females; 2 were born alive; and the whole operation was performed in the space of 50 minutes. The first made its appearance at 2 in the afternoon, and the last at 10 minutes before 3. Each child presented naturally was preceded by a separate burst of water, and was delivered by the natural pains only. In a short time after the birth of the last, the placenta was expelled by nature without any hæmorrhage, was uncommonly large, and in some places beginning to be putrid. It consisted of one uniform continued cake, and was not divided into distinct placentulæ, the lobulated appearance being nearly equal all over. Each funis was contained in a separate cell, within which each child had been lodged; and it was easy to perceive, by the state of the funis, and that part of the placenta to which it adhered, in which sac the dead, and in which the living children had been contained. I examined the septa of the cells very carefully, but could not divide them as usual into distinct laminæ, nor determine which was chorion or which amnios. I could not prevail on the good women to allow me to carry it home, to be more narrowly inspected; and I submitted more readily to their prejudice for its being burned, as its very soft texture seemed to render it hardly capable to bear injection. The 2 living children having survived their birth but a short time, I was allowed to carry them home; and I have preserved the whole 5 in spirits, and have since weighed and measured them, and find their proportions to be as follows in Avoirdupois weight, inches and parts.

	Oz.	Dr.		Inches.
The 1st born dead	6	12	Length	9
The 2d .... putrid	4	6	......	$8\frac{1}{2}$
The 3d .... alive	8	12	......	$9\frac{1}{8}$
The 4th.... putrid	6	12	......	$9\frac{1}{8}$
The 5th.... alive	9	—	......	$9\frac{1}{8}$

The mother, in spite of the crowds with which her chamber was continually filled, continued to recover, and was able to be out of bed on the 27th and 28th, her 3d and 4th days; but finding herself then weak, by my advice, kept her bed till the 11th of May, when she went out of doors, and on the 21st walked to Blackburn, 2 miles distant. This was the 27th day from her delivery, she having entirely recovered her strength without any accident. It may not be improper to add, that the husband of this woman has been in an infirm state of health for 3 years past, and is now labouring under a confirmed phthisis.

Signed, JOHN HULL.

Blackburn, Lancashire, June 9, 1786.

Observations on Numerous Births.—Though the females of the human species produce most commonly but 1 child at a birth; and though their formation with only 2 breasts, and 1 nipple to each, renders it probable they were not originally intended to produce in general more than 2; yet, from what we know of the womb and its appendages, and what from the latest experiments we are led to conjecture as to the mode of conception, we cannot presume à priori to set limits to the fertility of nature, nor determine decisively what number of fœtuses may be conceived and nourished to a certain period in the human uterus at the same time.

The present singular and well-attested case assures us, that 5 have certainly been born at once, and we have no title absolutely to reject all the testimonies of even more numerous births, or to say that, in some rare instances, this number has never been exceeded. What has tended to render relations of this sort ridiculous, and to throw a degree of discredit on the whole, is the many marvellous, and evidently absurd and incredible histories, which not only the retailers of prodigies, but even the credulous writers of medical observations, have collected. I need only refer those, who wish to amuse themselves with surprising relations of this kind, to the curious collections of Schenkius, Schurigius, Ambrose Parey, and others.

But, in order to show how very uncommon births of this kind are, and how truly singular the case communicated by Mr. Hull to Dr. Blane is, I take the liberty to subjoin a short view of the usual course of nature in this matter among our own country-women, where we are least likely to be deceived. Though female fertility certainly varies according to the climate, situation, and manner of life; yet, I believe, it may be taken for a general rule, that where people live in the most simple and natural state, if they are the best nourished, and if they enjoy the firmest health and strength, they will there be the most fertile in healthy children; but we have no data to determine that they will there have the greatest number at one birth. At the British Lying-in Hospital, where we have had 18,300 delivered, the proportion of twins born has been only 1 in 91 births. In the Westminster Dispensary, of 1897 women delivered, the proportion of twins has been once in 80 births; but in the Dublin Lying-in Hospital, where above 21,000 have been delivered, they have had twins born once every 62d time. The average of which is once in 78 births nearly, in these kingdoms.

The calculations made in Germany from great numbers, in various situations, state twins as happening in a varied proportion from once every 65th to once every 70th time. But in a more accurate and later calculation made at Paris, by M. Tenon, Surgeon to the Salpêtriere, we learn, that in 104,591 births the proportion of twins was only 1 in 96, which is only a small degree less than we have calculated at the British Lying-in Hospital. It would be easy to add other calculations, all differing from these and from each other, more or less; but these are sufficient to show that nature observes no certain rule in this matter; and that even twins, the most usual variation, is not a very common occurrence.

When we advance to triplets, or 3 born at once, we find comparatively very few instances in this or any other country; and though every one has heard of such events as now and then happening, yet very few have seen them. In all those 18,300 women delivered at the British Lying-in Hospital, there has not been 1 such case. In the London Lying-in Hospital, where, being instituted later, much fewer have been delivered, they have had 2 such recorded as prodigies. In the Westminster Dispensary, in 1897 women delivered, there has been but 1 such event. In the Dublin Hospital, in 21,000 births, they have had triplets born thrice, or once in 7000 times, but have never exceeded that proportion or number, born at one time.

In a pretty extensive practice of above 30 years, both in the county of Rutland and in London, I have attended but 1 labour where 3 children were born; am personally acquainted with but 1 lady who, at Dumfries, in Scotland, after bearing twins twice, was delivered of 3 children at once; and I was never acquainted with any one who produced a greater number. Yet so much does this matter vary at Edinburgh, that Dr. Hamilton, Professor of Midwifry, writes, he had seen triplets born there, 5 or 6 times in less than 25 years. Mauriceau, in a long life of very extensive practice at Paris, with opportunities of knowing most things extraordinary that happened in his time in France, tells us, he had seen triplets born but a few times; had heard of 4 in that city but once, and mentions no greater number. One circumstance which he relates is so far worthy of attention, as it accords with one somewhat similar subjoined to Mr. Hull's case now read, viz. "That the husband of one of those women who bore 3 children was by trade a painter, and had been, for 2 years preceding this birth, paralytic over one half of his body, and yet had no reason to doubt the fidelity of his wife."

These facts, as far as they are to be depended on, may show us, that the capacity of procreation in the male may remain under very infirm health; and that we ought to judge with candour of such wives as are fruitful when living with very ailing husbands, and who produce healthy children in the 8th, or even 9th month after their death; as we can never say determinately under what degree of disease the male is totally incapable of procreation: more especially as we are very certain, that the female is not, when labouring under very desperate, and certainly fatal diseases, provided the principal organs of generation be sound. Nay, in cases of pulmonary phthisis, the life of the female seems to be protracted by pregnancy; and I have attended a lady, who, after being pronounced irrecoverably hectic, lived long enough to be twice delivered naturally of healthy children at the full time. But what particular circumstances of constitution, or state of health, can capacitate the male to become the father of more than one child at a birth, or how this could be effected, should it be wished, remains among those secrets of nature which our want of facts and observations renders us utterly incapable to speculate on. It seems probable, and these 2 observations, as well as Spallanzani's, and other late experiments, would rather incline us to suppose, that these numerous births depend most on the structure and state of the female organs; but nothing, that I know of, has ever been discovered in this obscure matter.

The occurrence of 4 born at once we find to be much more uncommon; and, I think, Haller's conjecture, rather than calculation, of its happening once in 20,000 births, very much under-rated, as it appears that once in 100,000 would be much nearer the truth. Of this however we have several well authenticated cases which have happened in this island. In the year 1674, there was published in London a 4to pamphlet, intitled, "The Fruitful Wonder, or a strange Relation, from Kingston-upon-Thames, of a Woman who, on Thursday and Friday, the 5th and 6th days of this instant March, 1673-4, was delivered of 4 children at 1 birth, viz. 3 sons and 1 daughter, all born alive, lusty children, and perfect in every part, which lived 24 hours, and then died, all much about the same time, with several other Examples of numerous Births, from credible Historians, with the Physical and Astrological Reasons for the same. By J. P. Student in Physic." Dr. Plott, in his History of Staffordshire, p. 194, mentions Eleanor, the wife of Henry Diven, of Watlington, who was delivered of 4 children at a birth in the year 1675.—Sir Robert Sibbald, in his Scotia Illustrata, after mentioning a case of 3 born at once, adds, "Imo in variis regni locis repertæ sunt mulieres quæ 4 fœtus uno partu ediderunt;" but makes no mention of more.

In the Gentleman's Magazine, which is reckoned a pretty authentic record of the times, we have the following accounts of numerous births. Ann Boynton, of Hensbridge, in Somersetshire, was this day, June 1, 1736, delivered of 3 daughters and 1 son; 1 of the daughters died, the rest are likely to live. The mother has been married but 4 years, and has had twice twins before, which completes the number of 8 children at 3 births.—October 3, 1743, at Rate, in Berkshire, Joan Galloway was delivered of 2 boys and 2 girls, 3 of whom were alive.—In January, 1746, the wife of Plumer, a labouring man, at Mill-Wimley, near Hitchin, Hertfordshire, was delivered of 3 living boys, and 1 dead.—August 22, 1746, the wife of Williams, of Coventry-street, Piccadilly, was delivered of 2 boys and 2 girls, all likely to live.—June, 1752, a woman in the parish of Tillicultrie, near Stirling, in Scotland, was delivered of 4 children, which were all immediately baptised, and all died at the same time next morning.—In September, 1757, a poor woman, of Burton Ferry, Glamorganshire, was delivered of 3 boys and 1 girl.

Dr. Hamilton before-mentioned writes, that not many years ago a woman was delivered of 4 children, at Pennycuick, the seat of Sir John Clark, Bart. near Edinburgh, when she was advanced to the middle of her last month of pregnancy, and that some of these children lived 2 or 3 years. He further says, that, 5 years ago, he attended a woman at Edinburgh, who, in the 7th month of her pregnancy, after a journey of 30 miles, was suddenly delivered of 4 children, all perfect and well grown for the time, of which 1 was born dead, and 3 alive; but those 3 died next day. He further adds, that these are the only cases of quadruplets, or any larger number, he had ever heard of, as born in Scotland, in his memory.

Though cases similar to the present of 5 children born at once, are still much more uncommon; and though Haller's assertion of their not happening above once in a million of births, may be reckoned a very moderate calculation, yet we are not altogether without such instances in this country. From the Gent. Mag. we learn, that on the 5th of Oct. 1736, a woman at a milk-cellar, in the Strand, was delivered of 3 boys and 2 girls at 1 birth; and that in March, 1739, at Wells, in Somersetshire, a woman was delivered of 4 sons and 1 daughter, all alive, all christened, and all then seemingly likely to live.—In the Commercium Literarium Norimbergense for the year 1731, we have 2 such cases; one happening in Upper Saxony, the other near Prague, in Bohemia; in each of which 5 children were born and christened, all of whom were arrived to that equal degree of maturity, which rendered it probable, they were all conceived about the same time.—I learned from 2 foreign Professors, when in London last winter, that they had each heard of a case of 5 children born near Paris, and near Ghent in Flanders; but the particulars not being sent as promised, I presume they may have been misinformed.

When we advance further we get into the region of tradition and improbability; and it would ill become me to trouble a Society, whose professed object is truth and science, with the numerous and wonderful relations which many grave and learned authors have recorded as facts they themselves believed; yet I still think we have no authority to reject absolutely every relation of this kind, when Ambrose Parey, a very honest, though credulous man, tells, that in his time, in the parish of Sceaux, near Chambellay between Sarte and Maine, the mother of the then living lord of the noble house of Maldemeure had, in the first year of her marriage, brought forth twins, in the 2d triplets, in the 3d four, in the 4th five, and in the 5th year 6 children at 1 birth, of which labour she died; and when he adds, that of these last 6 one is yet alive, and is now Lord of Maldemeure, how can we disbelieve this circumstance? This story may very possibly be inaccurately stated, yet the whole cannot be a fiction, as it was published among the very people, and in the age when it happened, and never has been since contradicted so far as we know. Though the wonderful regularity of the progress gives an appearance of fable to the whole, yet we must believe the thing to be possible: and that this then existing lord might be the only 1 of the six who lived long enough to be born at the full time, in a mature state; the whole, or most of the other 5, as we have sometimes seen in cases of twins, having been born as dead abortions, which had never arrived to a bulk sufficient to interfere with his growth.

I leave the learned to pay what degree of credit they please to the wonderful relations we read of the extreme fertility of the women of Egypt, Arabia, and other warm countries, as recorded by Aristotle, by Pliny, and by Albucasis, where 3, 4, 5, and 6 children are said to have been frequently born at once, and the greatest part of these reared to maturity; and will only say, that though a late traveller, M. Savary, gives ample testimony of the extreme general fertility of Egypt in all vegetable and animal productions, and particularly of its abundant population, he mentions nothing of the numerous births recorded by the ancient naturalists and historians.

Of still more fruitful births I will pass over a number of instances which I could adduce from Johannes Rhodius, Lucas Schroeckius, Caspar Bauhin, Johannes Helvigius, Bianchi, and others, and finish with 1 case more, recorded by Petrus Borelli in his Second Century of Observations, published at Paris in the year 1656; a collection indeed filled with many wonderful stories, though by a man of equal integrity and ingenuity: he tells us, that in the year 1650, just 5 years before, the lady of the then present Lord Darre produced at 1 birth 8 perfect children, which he owns was a very unusual event in that country.

I think it totally unnecessary to pursue this inquiry further; but must observe, that the present is the only case I have found, where the children were all females; that the males have in all the other cases been at least equal, and generally the more numerous; that in many of them, at least a part was dead born; and that most commonly the rest died in a short time. It is thence clear, that those numerous births are certainly unfavourable to population, as very few indeed of those children can be carried to near the full term of pregnancy, and fewer still to that degree of strength that admits of their being reared, where more than 2 are born at one time.

As from Mr. John Hunter's very curious experiments and observations, read lately to this Society, on the procreation of swine, we are led to believe, that a certain determined number of ova, capable of receiving male impregnation, are originally formed in each ovarium; and which number, when exhausted, the female constitution has no power to renew; if this be the true account of the economy of nature in this particular, which has every appearance of probability, those numerous births must occasion a very fruitless profusion and waste of the human race, and become every way detrimental to its increase. From the united testimony of all the foregoing cases, it is undeniably clear, that the females of the human species, though most commonly uniparous, are, in certain circumstances to us unknown, every now and then capable of very far exceeding their usual number; and I must again repeat, that it does not appear that we can set any bounds to the powers of nature in that respect; or pretend, as some have done, with certainty to say, what may be the utmost limits of human fertility.

XXXV. Chloranthus, a New Genus of Plants, described by Olof Swartz, M. D. p. 359.

Among the numberless vegetable productions that have appeared in the Royal Garden at Kew, is the present. It is already long since this curious plant has been introduced there as a native of China, where, we are told, the same is cultivated in the Chinese gardens, though it seems not to have any qualities either palatable or odoriferous nor a beautiful appearance. At the first sight of the plant, there is some likeness of viscum or loranthus; and considering the inflorescentia, and the insertion of the antheræ, we find no less analogy; though on a nearer examination it is greatly different, and of a very intricate construction. The pains I have taken to enucleate the family relation of this hitherto unknown vegetable, have induced me, for the sake of its singularity, to present it as a new genus, of which I think the following natural character may be the most proper:

Calyx none; but there is an ovate, concave, pointed scale, on which the germen is seated.

Corol. Monopetalous, dimidiated, or a single petal, roundish, three-lobed, convex, inserted into the outside of the germen, staminiferous, deciduous. The exterior lobe is larger than the others.

Stamens. Filaments none. Anthers four, longitudinally growing to the margins of the petal, bivalve.

Pistil. Germ oblong, or difform obovate, nearly covered by the scale, projecting in front, petaligerous. Style oblique, thick, very short, cornered. Stigmas three, very small, upright.

Pericarp. Berry oblong, single-seeded.

Seed oblong.

Essential Character.—*Calyx* none.

Corol. Petal trilobate, seated on the side of the germen. Anthers growing to the petal.

Berry single-seeded.

I have not been able to find any description or figure answering to this plant in the works of the East-Indian naturalists. I have only met with one Chinese drawing, in the library of Sir Joseph Banks, Bart. P. R. S. among some others of their garden plants, that seems to represent the present. It is said to be called Chu-Lan by the Chinese; but it ought not to be confounded with the Tsjiulang or Camunium Chinense of Rumphius (Herb. Amboin. l. 7, cap. 15, and Auctuarii ejusd. cap. 47,) the description of which seems to correspond in some parts with the Chloranthus: the first figure however, on the 18th plate, shows the plant of Rumphius to be the Vitex pinnata of Linnæus.

XXXVI. On the Precession of the Equinoxes. By the Rev. Sam. Vince, M. A., F. R. S. p. 363.

1. The true cause of the precession of the equinoctial points was first assigned by Sir Isaac Newton; but it is confessed, that he has fallen into an error in his investigation of the effect. Without however entering into any inquiry relative to the circumstances in which he has erred, I propose to show how we may obtain a true solution from his own principles, by means of which alone the whole calculation may be rendered extremely simple and evident; and though very satisfactory solutions have been already given, yet the importance of the problem will sufficiently apologize for offering any thing further on the subject that may at all tend to elucidate it.

2. Let S (pl. 4, fig. 1) be the sun, ABDC the earth, T its centre, EQ the equator, P, p, the poles; draw CTB perpendicular to SAD, and join SE, which produce to meet CB in K. Call the radius ET unity, and let the force of the sun on a particle at T be $\frac{1}{ST^2}$, then the force on a particle at E $= \frac{1}{SE^2}$; hence, if we resolve this latter force into 2 others, one in the direction ET, and the other in the direction parallel to TS, we have SE : ST :: $\frac{1}{SE^2}$: the force in the direction parallel to TS $= \frac{ST}{SE^3} = \frac{ST}{(ST - EK)^3} = \frac{1}{ST^2} + \frac{3EK}{ST^3}$, omitting the other terms of the series on account of their smallness. Hence the force with which a particle at E is drawn from CB is equal to $\frac{3EK}{ST^3}$; consequently the effect of this force in a direction perpendicular to ET will be $\frac{3EK \times KT}{ST^3}$; hence this force : the force of the sun on a particle at T :: $\frac{3EK \times KT}{ST^3}$: $\frac{1}{ST^2}$ :: 3EK $\times$ KT : ST. Now if P = the periodic time of the earth, p = the periodic time of a body revolving at the earth's surface; then the force of the earth to the sun : the force of the body to the earth, or the force of gravity, :: $\frac{ST}{P^2} : \frac{1}{p^2}$; and hence the force of the sun on a particle at E perpendicular to ET : the force of gravity :: $\frac{3EK \times KT \times p^2}{P^2}$: 1.

3. Let v be the centre of gyration, and put M = the quantity of matter in the earth: then the effect of the inertia of M placed at v, to oppose the communication of motion, is the same as the effect of the inertia of the earth; and hence, by the property of that centre, $ET^2 : Tv^2$ ($= \frac{2}{5}ET^2$) :: M : $\frac{2}{5}$M, which is the quantity of matter to be placed at E to have the same effect.

4. Put m = the excess of the quantity of matter in the earth above that of its inscribed sphere. Now by Sir Isaac Newton's first 2 lemmas, it appears, that the action of the sun on the shell m of matter, to generate an angular velocity about an axis perpendicular to CABD, is just the same as it would be to generate an angular velocity in a quantity of matter equal to $\frac{1}{5}n$ placed at E. Let us therefore suppose the sun's attraction, perpendicular to ET, to be exerted on a quantity of matter at E equal to $\frac{1}{5}m$, and at the same time to have a quantity of matter to move equal to $\frac{2}{5}m$; and then from this and art. 3 it appears, that the effect will be the same as the accelerative force of the sun to turn about the earth. Hence that accelerative force is, from art. 2, equal to $\frac{3EK \times KT \times p^2 \times \frac{1}{5}m}{\frac{2}{5}M \times P^2} = \frac{3EK \times KT \times p^2 \times m}{2M \times P^2}$, gravity being unity. Now, if TE : TP :: 1 : 1 − r, then M : M − m :: 1 : 1 − $2r$, therefore, M : m :: 1 : $2r$, hence $\frac{m}{2M} = r$, consequently the accelerative force $= \frac{3EK \times KT \times p^2 \times r}{P^2}$.

5. Let $\dot{z}$ = the arc described by a point of the equator about its axis in an indefinitely small given time, which may therefore represent its velocity; and let $a\dot{z}$ represent the arc described in the same time by a body revolving about the earth at its surface; then $\frac{a^2\dot{z}^2}{2}$ = the sagitta of the arc described by the body in the same time, and consequently $a^2\dot{z}^2$ = the velocity generated by gravity while a point of the equator describes $\dot{z}$. Hence, by art. 4, we have $1 : \frac{3EK \times KT \times p^2 \times r}{P^2} :: a^2\dot{z}^2 : \frac{3EK \times KT \times p^2 \times r \times a^2\dot{z}^2}{P^2}$ the velocity of the point E of the equator generated by the action of the sun, while the equator describes $\dot{z}$ about its axis; consequently the ratio of these velocities is as $\frac{3EK \times KT \times p^2 \times a^2\dot{z}}{P^2} : 1$.

6. Let $\dot{y}$ be an arc described by the sun in the ecliptic to a radius equal to unity, while a point of the equator describes $\dot{z}$ about its axis; then (as ap = the time of the eath's rotation, and the arcs described in equal times to equal radii are inversely as the periodic times) $\frac{1}{P} : \frac{1}{ap} :: \dot{y} : \dot{z} = \frac{P\dot{y}}{ap}$; hence, if v and w be put for the sine and cosine of the sun's declination, the ratio of the velocities in the last article becomes $\frac{3aprvw\dot{y}}{P} : 1$.

7. Hence if TSL (fig. 2) be the ecliptic to the radius unity, P the plane of the sun, SER the equator, PE the sun's declination, and we take EC : cd (cd being perpendicular to EC) :: $1 : \frac{3aprvw\dot{y}}{P}$, and through d, E, describe the great circle

TEM, then will ST be the precession of the equinox during the time the sun describes $\dot{y}$ in the ecliptic. Now Ed (or EC, as the angle at E is indefinitely small) : dc :: rad. $= 1$: sine angle E $= \frac{3aprvw\dot{y}}{P}$; hence (if SV be drawn perpendicular to TE) 1 : sine SE :: $\frac{3aprvw\dot{y}}{P}$: SV $= \frac{3aprvw \times \text{sin. SE} \times \dot{y}}{P}$; therefore, sin. STV or ESP : 1 :: SV : ST $= \frac{3aprvw \times \text{sin. SE} \times \dot{y}}{P \times \text{sin. ESP}}$.

8. Now $\frac{v}{\text{sin. ESP}} = \text{sin. SP}$, and $w = \frac{\text{cos. SP}}{\text{cos. ES}}$, hence $\frac{vw}{\text{sin. ESP}} = \frac{\text{sin. SP} \times \text{cos. SP}}{\text{cos. ES}}$; but $\frac{\text{cos. ESP}}{\text{tan. ES} \times \text{cot. SP}} = 1$, hence $\frac{vw}{\text{sin. ESP}} = \frac{\text{sin. SP} \times \text{cos. SP} \times \text{cos. ESP}}{\text{cos. ES} \times \text{tan. ES} \times \text{cot. SP}} = \frac{\text{sin. 2SP}^2 \times \text{cos. ESP}}{\text{sin. ES}}$ consequently, ST $= \frac{3apr \times \text{sin. SP}^2 \times \text{cos. ESP} \times \dot{y}}{P} =$ (if $x = \text{sin. SP}$) $\frac{3apr \times \text{cos. ESP} \times x^2\dot{x}}{P \times \sqrt{(1 - x^2)}}$, whose fluent, when $x = 1$, is $\frac{3apr \times \text{cos. ESP} \times y}{2P}$ (y being now $=$ to a quadrant) the arc of precession while the sun describes 90° of the ecliptic; and to find the degrees say, $4y : 360° :: \frac{3apr \times \text{cos. ESP} \times y}{2P} : 360° \times \frac{3apr \times \text{cos. ESP}}{8P}$, consequently the precession in a year $= 360° \times \frac{3apr \times \text{cos. ESP}}{2P} = 21'' \, 6'''$. This would be the precession of the equinox arising from the attraction of the sun, if the earth were of a uniform density, and the ratio of the diameters as 229 : 230; but if the greatest nutation of the earth's axis be rightly ascertained, the precession is only about $14\frac{2}{3}''$; which difference between the theory and what is deduced from observation, must arise either from the fluidity of the earth's surface, or an increase of density towards the centre, or the ratio of the diameters being different from what is here assumed, or probably from all the causes conjointly. But as the best observations must be liable to some small degree of inaccuracy, and an error of 1 or 2 seconds in the nutation will, in this case, make a very considerable alteration in the conclusion, the estimation of the precession arising from the action of the sun seems to be subject to a very considerable degree of uncertainty.

XXXVII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon, Rutland, 1786. By Thomas Barker, Esq. Also of the Rain at South Lambeth, Surrey; and at Selbourn and Fyfield, Hampshire.* p. 368.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.						
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	S. Lamb. Surry.	Selbourn Hamp.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29.87	28.33	29.18	49	25	38	48½	11½	34	3.467	2.48	6.58	4.93
	Aftern.				50	25	39	53	19	39				
Feb.	Morn.	29.96	28.84	29.50	48	32½	40	45½	24	34	0.665	1.08	1.27	4.78
	Aftern.				46½	33	41	49	27½	39½				
Mar.	Morn.	29.78	28.76	29.29	46	28	37	46	18	30	0.832	1.11	1.53	1.64
	Aftern.				47½	29	38	50½	22½	39				
Apr.	Morn.	29.96	29.02	29.46	55½	38½	47	52	29	41	1.252	1.22	1.63	1.41
	Aftern.				57	40	48	68	37	51				
May	Morn.	29.90	28.75	29.46	63½	45	53	59½	32	48½	2.383	0.97	2.16	2.79
	Aftern.				65½	46½	55	73	49	59½				
June	Morn.	29.90	29.32	29.53	66	58½	62	65	49	56½	1.583	2.24	1.05	1.51
	Aftern.				69	59	64	80½	60	68				
July	Morn.	30.00	29.05	29.56	65	56½	61½	62½	50	56	1.799	0.86	1.81	1.42
	Aftern.				66½	58½	63	74	51	66				
Aug.	Morn.	29.84	29.01	29.43	67	57	61	65	48	55	2.632	1.19	4.00	3.57
	Aftern.				68	58½	62	75½	57	65½				
Sept.	Morn.	29.98	28.40	29.35	61	49	55	58	38	47	2.840		4.50	1.62
	Aftern.				61	49	56½	67	47	57				
Oct.	Morn.	30.00	28.29	29.55	54½	45	49	50	31	41	4.762	8.22	5.04	4.18
	Aftern.				55½	45½	50	62½	42	49				
Nov.	Morn.	29.81	28.55	29.36	45	36½	41	45	27	35	2.938		4.38	1.22
	Aftern.				45½	36½	41½	47½	30	39				
Dec.	Morn.	30.05	28.44	29.15	46	32	39½	45½	Thermom.		2.136	3.06	5.62	4.13
	Aftern.				46	33	40½	46	broken.					
Mean of all				29.40			49½				27.289	22.43	39.57	29.60

XXXVIII. *Observations on the Structure and Economy of Whales. By John Hunter, Esq., F. R. S.* p. 371.

The animals which inhabit the sea are much less known to us than those found on land; and the economy of those with which we are best acquainted is much less understood: we are therefore too often obliged to reason from analogy where information fails; which must probably ever continue to be the case, from our unfitness to pursue our researches in the unfathomable waters. This unfitness does not arise from that part of our economy on which life and its functions depend; for the tribe of animals which is to be the subject of this paper, has in that respect the same economy as man, but from a difference in the mechanism by which our progressive motion is produced. The anatomy of the larger marine animals, when they are procured in a proper state, can be as well ascertained as that of any others; dead structure being readily investigated. But even such opportunities too seldom occur, because those animals are only to be found in distant seas, which no one explores in pursuit of natural history; neither can they be brought to us alive from thence, which prevents our receiving their bodies in a state fit for dissection. As they cannot live in air, we are unable to procure them alive.

Some of these aquatic animals yielding substances which have become articles of traffic, and in quantity sufficient to render them valuable as objects of profit, are sought after for that purpose; but gain being the primary view, the researches of the naturalist are only considered as secondary points, if considered at all. At the best, our opportunities of examining such animals do not often occur till the parts are in such a state as to defeat the purposes of accurate inquiry, and even these occasions are so rare as to prevent our being able to supply, by a 2d dissection, what was deficient in a first. The parts of such animals being formed on so large a scale, is another cause which prevents any great degree of accuracy in their examination; more especially when it is considered, how very inconvenient for accurate dissections are barges, open fields, and such places as are fit to receive animals or parts of such vast bulk.

As the opportunities of ascertaining the anatomical structure of large marine animals are generally accidental, I have availed myself, as much as possible, of all that have occurred; and, anxious to get more extensive information, engaged a surgeon, at a considerable expense, to make a voyage to Greenland, in one of the ships employed in the whale fishery, and furnished him with such necessaries as I thought might be requisite for examining and preserving the more interesting parts, and with instructions for making general observations; but the only return I received for this expense was a piece of whale's skin, with some small animals sticking upon it. From the opportunities I have had of examining different animals of this order, I have gained a tolerably accurate idea of the anatomical structure of some genera, and such a knowledge of the structure of particular parts of some others, as to enable me to ascertain the principles of their economy.

Those which I have had opportunities of examining were the following: Of the delphinus phocæna, or porpoise, I have had several, both male and female. Of the grampus I have had 2; one of them 24 feet long, the belly of a white colour, which terminated at once, the sides and back being black; the other about 18 feet long, the belly white, but less so than in the former, and shaded off into the dark colour of the back. Of the delphinus delphis, or bottle-nose whale, I had one sent to me by Mr. Jenner*, surgeon, at Berkeley. It was about 11 feet long. I have also had one 21 feet long, resembling this last in the shape of the head, but of a different genus, having only 2 teeth in the lower jaw; the belly

* Now Dr. Jenner, the discoverer of vaccination.

was white, shaded off into the dark colour of the back. This species is described by Dale, in his Antiquities of Harwich. The one which I examined must have been young; for I have a skull of the same kind nearly 3 times as large, which must have belonged to an animal 30 or 40 feet long. Of the balæna rostrata of Fabricius, I had one, 17 feet long. The balæna mysticetus, or large whalebone whale, the physeter macrocephalus, or spermaceti whale, and the monodon monoceros, or narwhale, have also fallen under my inspection. Some of these I have had opportunities of examining with accuracy; while others I have only examined in part, the animals having been too long kept before I procured them, to admit of more than a very superficial inspection.

From these circumstances it will be readily supposed, that an accurate description of all the different species is not to be expected; but having acquired a general knowledge of the whole tribe, from the different species which have come under my examination, I have been enabled to form a tolerable idea, even of parts which I have only had the opportunity of seeing in a very cursory way. General observation would lead us to believe, that the whole of this tribe constitutes one order of animals, which naturalists have subdivided into genera and species; but a deficiency in the knowledge of their economy has prevented them from making these divisions with sufficient accuracy; and this is not surprising, since the genera and species are still in some measure undetermined even in animals with which we are better acquainted.

The animals of this order are in size the largest known, and probably therefore the fewest in number of all that live in water. Size, I believe, in those animals who feed on others, is in some proportion to the number of the smaller; but I believe this tribe varies more in that respect than any we know, viewing it from the whalebone whale, which is 70 or 80 feet long, to the porpoise that is 5 or 6: however, if they differ as much among themselves as the salmon does from the sprat, there is not that comparative difference in size that would at first appear. The whalebone whale is, I believe, the largest; the spermaceti whale the next in size (the one which I examined, though not full grown, was about 60 feet long); the Grampus, which is an extensive genus, is probably from 20 to 50 feet long; under this denomination there is a number of species.

From my want of knowledge of the different genera of this tribe of animals, an incorrectness in the application of the anatomical account to the proper genus may be the consequence; for when they are of a certain size, they are brought to us as porpoises; when larger, they are called grampus, or fin-fish. A tolerably correct anatomical description of each species, with an accurate drawing of the external form, would lead us to a knowledge of the different genera, and the species in each; and, in order to forward so useful a work, I propose, at some future period, to lay before the Society descriptions and drawings of those which have come under my own observation. From some circumstances in their digestive organs we should be led to suppose, that they were nearly allied to each other; and that there was not the same variety, in this respect, as in land animals.

In the description of this order of animals, I shall always keep in view their analogy to land animals, and to such as occasionally inhabit the water, as white bears, seals, manatees, &c. with the differences that occur. This mode of referring them to other animals better known, will assist the mind in understanding the present subject. It is not however intended in this paper to give a particular account of the structure of all the animals of this order, which I have had an opportunity of examining: I propose at present chiefly to confine myself to general principles, giving the great outlines as far as I am acquainted with them, minuteness being only necessary in the investigation of particular parts. In my account I shall pay some attention to the relations of men who have given facts without knowing their causes, whenever I find that such facts can be explained on true principles of the animal economy, but no further.

This order of animals has nothing peculiar to fish, except living in the same element, and being endowed with the same powers of progressive motion as those fish that are intended to move with a considerable velocity: for I believe, that all that come to the surface of the water, which this order of animals must do, have considerable progressive motion; and this reasoning we may apply to birds; for those which soar very high have the greatest progressive motion. Though inhabitants of the waters, they belong to the same class as quadrupeds, breathing air, being furnished with lungs, and all the other parts peculiar to the economy of that class, and having warm blood; for we may make this general remark, that in the different classes of animals there is never any mixture of those parts which are essential to life, nor in their different modes of sensation.

I shall divide what is called the economy of an animal, first, into those parts and actions which respect its internal functions, and on which life immediately depends, as growth, waste, repair, shifting or changing of parts, &c. the organs of respiration and secretion, in which we may include the powers of propagating the species. 2dly, Into those parts and actions which respect external objects, and which are variously constructed, according to the kind of matter with which they are to be connected, whence they vary more than those of the first division. These are the parts for progressive motion, the organs of sense and the organs of digestion; all which either act, or are acted on, by external matter.

This variation from external causes in many instances influences the shape of the whole, or particular parts, even giving a peculiar form to some which belong to the first order of actions, as the heart, which in this tribe, in the seal, otter, &c. is flattened, because the chest is flattened for the purpose of swimming.

The contents of the abdomen are not only adapted to the external form; but their direction in the cavity is, in some instances, regulated by it. The anterior extremity, or fin, though formed of distinct parts, in some degree similar to the anterior extremities of some quadrupeds, being composed of similar bones placed nearly in the same manner, yet are so formed and arranged as to fit them for progressive motion in the water only. The external form of this order of animals is such as fits them for dividing the water in progressive motion, and gives them power to produce that motion in the same manner as those fish which move with a considerable velocity. On account of their inhabiting the water, their external form is more uniform than in animals of the same class which live on land; the surface of the earth on which the progressive motion of the quadruped is to be performed being various and irregular, while the water is always the same.

The form of the head, or anterior part of this order of animals, is commonly a cone, or an inclined plane, except in the spermaceti whale, in which it terminates in a blunt surface. This form of head increases the surface of contact to the same volume of water which it removes, lessens the pressure, and is better calculated to bear resistance of the water through which the animal is to pass; probably, on this account, the head is larger than in quadrupeds, having more the proportion observed in fish, and swelling out laterally at the articulation of the lower jaw: this may probably be for the better catching their prey, as they have no motion of the head on the body; and this distance between the articulations of the jaw is somewhat similar to the swallow, goat-sucker, bat, &c. which may also be accounted for, from their catching their food in the same manner as fish; and this is rendered still more probable, since the form of the mouth varies according as they have or have not teeth. There is however in the whale tribe more variety in the form of the head than of any other part, as in the whalebone, bottle-nose, and spermaceti whales; though in this last it appears to owe its shape, in some sort, to the vast quantity of spermaceti lodged there, and not to be formed merely for the catching of its prey. From the mode of their progressive motion, they have not the connection between the head and body, that is called the neck, as that would have produced an inequality inconvenient to progressive motion.

The body behind the fins or shoulders diminishes gradually to the spreading of the tail; but the part beyond the opening of the anus is to be considered as tail, though to appearance it is a continuation of the body. The body itself is flattened laterally; and I believe the back is much sharper than the belly. The projecting part, or tail, contains the power that produces progressive motion, and moves the broad termination, the motion of which is similar to that of an oar in sculling a boat; it supersedes the necessity of posterior extremities, and allows of the proper shape for swimming; that the form may be preserved as much as possible, we find that all the projecting parts, found in land animals of the same class, are either entirely wanting, as the external ear; or are placed internally, as the testicles; or are spread along under the skin, as the udder. The tail is flattened horizontally, which is contrary to that of fish, this position of tail giving the direction to the animal in the progressive motion of the body. I shall not pursue this circumstance further than to apply it to those purposes in the animal economy, for which this particular direction is intended.

The 2 lateral fins, which are analogous to the anterior extremities in the quadruped, are commonly small, varying however in size, and seem to serve as a kind of oars. To ascertain the use of the fin on the back is probably not so easy, as the large whalebone and spermaceti whales have it not; one should otherwise conceive it intended to preserve the animal from turning. I believe, like most animals, they are of a lighter colour on their belly than on their back: in some they are entirely white on the belly; and this white colour begins by a regular determined line, as in the grampus, piked whale, &c.: in others, the white on the belly is gradually shaded into the dark colour of the back, as in the porpoise. I have been informed, that some of them are pied upwards and downwards, or have the divisions of colour in a contrary direction.

The element in which they live renders certain parts which are of importance in other animals useless in them, gives to some parts a different action, and renders others of less account. The puncta lachrymalia with the appendages, as the sac and duct, are in them unnecessary; and the secretion from the lachrymal gland is not water, but mucus, as it also is in the turtle; and we may suppose only in small quantity, the gland itself being small. The urinary bladder is smaller than in quadrupeds; and indeed there is not any apparent reason why whales should have one at all. The tongue is flat, and but little projecting, as they neither have voice, nor require much action of this part, in applying the food between the teeth for the purpose of mastication, or deglutition, being nearly similar to fish in this respect, as well as in their progressive motion.

In some particulars they differ as much from each other as any 2 genera of quadrupeds I am acquainted with. The larynx, the size of trachea, and the number of ribs, differ exceedingly. The cæcum is only found in some of them. The teeth in some are wanting. The blow-holes are 2 in number in many, in others only 1. The whalebone and spermaceti are peculiar to particular genera: all which constitute great variations. In other respects we find a uniformity, which would appear to be independent of their living and moving only in the water, as in the stomach, liver, parts of generation of both sexes, and in the kidneys; in these last however I believe it depends in some degree on their situation, though it is extended to other animals, the cause of which I do not understand.

All animals have, I believe, a smell peculiar to themselves: how far this is connected with the other distinctions, I do not know, our organs not being able to distinguish with sufficient accuracy. The smell of animals of this tribe is the same with that of the seal, but not so strong, a kind of sour smell, which the seal has while alive; the oil has the same smell with that of the salmon, herring, sprat, &c.

The observations respecting the weight of the flesh of animals that swim, which I published in my observations on the economy of certain parts of animals, are applicable to these also; for the flesh in this tribe is rather heavier than beef; 2 portions of muscle of the same shape, one from the psoas muscle of the whale, the other of an ox, when weighed in air, were both exactly 502 grs.; but weighed in water, the portion of the whale was 4 grs. heavier than the other. It is probable therefore, that the necessary equilibrium between the water and the animal is produced by the oil, in addition to which the principal action of the tail is such as tends either to raise them, or keep them suspended in the water, according to the degree of force with which it acts. From the tail being horizontal, the motion of the animal, when impelled by it, is up and down: 2 advantages are gained by this, it gives the necessary opportunity of breathing, and elevates them in the water; for every motion of the tail tends to raise the animal: and that this may be effected, the greatest motion of the tail is downwards, those muscles being very large, making 2 ridges in the abdomen; this motion of the tail raises the anterior extremity, which always tends to keep the body suspended in the water.

Of the Bones.—The bones alone, in many animals, when properly united into what is called the skeleton, give the general shape and character of the animal. Thus a quadruped is distinguished from a bird, and even one quadruped from another, it only requiring a skin to be thrown over the skeleton to make the species known; but this is not so decidedly the case with this order of animals, for the skeleton in them does not give us the true shape. An immense head, a small neck, few ribs, and in many a short sternum, and no pelvis, with a long spine, terminating in a point, require more than a skin being laid over them to give the regular and characteristic form of the animal. The bones of the anterior extremity give no idea of the shape of a fin, the form of which depends wholly on its covering. The different parts of the skeleton are so enclosed, and the spaces between the projecting parts are so filled up, as to be altogether concealed, giving the animal externally a uniform and elegant form, resembling an insect enveloped in its chrysalis coat.

The bones of the head are in general so large, as to render the cavity which contains the brain but a small part of the whole; while, in the human species, and in birds, this cavity constitutes the principal bulk of the head. This is perhaps most remarkable in the spermaceti whale; for on a general view of the bones of the head, it is impossible to determine where the cavity of the skull lies, till led to it by the foramen magnum occipitale. The same remark is applicable to the large whalebone and bottle-nose whale; but in the porpoise, where the brain is larger in proportion to the size of the animal, the skull makes the principal part of the head.

Some of the bones in one genus differ from those of another. The lower jaw is an instance of this. In the spermaceti and bottle-nose whales, the grampus, and the porpoise, the lower jaws, especially at the posterior ends, resemble each other; but in both the large and small whalebone whales, the shape differs considerably. The number of some particular bones varies also very much. The pike whale has 7 vertebræ in the neck, 12 which may be reckoned to the back, and 27 to the tail, making 46 in the whole. In the porpoise there are 5 cervical vertebræ, and 1 common to the neck and back, 14 proper to the back, and 30 to the tail, making in the whole 51. The small bottle-nose whale, caught near Berkeley, in the number of cervical vertebræ resembled the porpoise; it had 17 in the back, and 37 in the tail, in all 60. In the porpoise, 4 of the vertebræ of the neck are anchylosed; and in every animal of this order, which I have examined, the atlas is by much the thickest, and seems to be made up of 2 joined together, for the 2d cervical nerve passes through a foramen in this vertebra. There is no articulation for rotatory motion between the 1st and 2d vertebræ of the neck.

The small bottle-nose whale had 18 ribs on each side, the porpoise 16. The ends of the ribs that have 2 articulations, in the whole of this tribe, I believe are articulated with the body of the vertebræ above, and with the transverse processes below, by the angles; so that there is one vertebra common to the neck and back. In the large whalebone whale the first rib is bifurcated, and consequently articulated to 2 vertebræ. The sternum is very flat in the piked whale; it is only one very short bone; and in the porpoise it is a good deal longer. In the small bottle-nose it is composed of 3 bones, and is of some length. In the piked whale the first rib, and in the porpoise the first 3, are articulated with the sternum. As a contraction, corresponding to the neck in quadrupeds, would have been improper in this order of animals, the vertebræ of the neck are thin, to make the distance between the head and shoulders as short as possible, and in the small bottle-nose whale are only 6 in number.

The structure of the bones is similar to that of the bones of quadrupeds; they are composed of an animal substance, and an earth that is not animal: these seem only to be mechanically mixed, or rather the earth thrown into the interstices of the animal part. In the bones of fishes this does not seem to be the case, the earth in many fish being so united with the animal part, as to render the whole transparent, which is not the case when the animal part is removed by steeping the bone in caustic alkali: nor is the animal part so transparent when deprived of the earth The bones are less compact than those of quadrupeds that are similar to them.

Their form somewhat resembles what takes place in the quadruped, at least in those whose uses are similar, as the vertebræ, ribs, and bones of the anterior extremities have their articulations in part alike, though not in all of them. The articulation of the lower jaw, of the carpus, metacarpus, and fingers, are exceptions. The articulation of the lower jaw is not by simple contact either single or double, joined by a capsular ligament, as in the quadruped; but by a very thick intermediate substance of the ligamentous kind, so interwoven that its parts move on each other, in the interstices of which is an oil. This thick matted substance may answer the same purpose as the double joint in the quadruped.

The 2 fins are analogous to the anterior extremities of the quadruped, and are also somewhat similar in construction. A fin is composed of a scapula, os humeri, ulna, radius, carpus, and metacarpus, in which last may be included the fingers, because the number of bones are those which might be called fingers, though they are not separated, but included in one general covering with the metacarpus. They have nothing analogous to the thumb, and the number of bones in each is different; in the fore-finger there are 5 bones, in the middle and ring-finger 7, and in the little finger 4. The articulation of the carpus, metacarpus, and fingers, is different from that of the quadruped, not being by capsular ligament, but by intermediate cartilages connected to each bone. These cartilages between the different bones of the fingers are of considerable length, being nearly equal to $\frac{1}{2}$ of that of the bone; and this construction of the parts gives firmness, with some degree of pliability, to the whole.

As this order of animals cannot be said to have a pelvis, they of course have no os sacrum, and therefore the vertebræ are continued on to the end of the tail; but with no distinction between those of the loins and tail. But as those vertebræ alone would not have had sufficient surface to give rise to the muscles requisite to the motion of the tail, there are bones added to the fore-part of some of the first vertebræ of the tail, similar to the spinal processes on the posterior surface. From all these observations we may infer, that the structure, formation, arrangement, and the union of the bones, which compose the forms of parts in this order of animals, are much on the same principle as in quadrupeds.

The flesh or muscles of this order of animals is red, resembling that of most quadrupeds, perhaps more like that of the bull or horse than any other animal: some of it is very firm; and about the breast and belly it is mixed with tendon.

Though the body and tail is composed of a series of bones connected together and moved as in fish, yet it has its movements produced by long muscles, with long tendons, which renders the body thicker, while the tail at its stem is smaller than that of any other swimmer, whose principal motion is the same. Why this mode of applying the moving powers should not have been used in fish, is probably not so easily answered; but in fish the muscles of the body are of nearly the same length as the vertebræ.

The depressor muscles of the tail, which are similar in situation to the psoæ, make 2 very large ridges on the lower part of the cavity of the belly, rising much higher than the spine, and the lower part of the aorta passes between them. These 2 large muscles, instead of being inserted into 2 extremities as in the quadruped, go to the tail, which may be considered in this order of animals as the 2 posterior extremities united into one. Their muscles, a very short time after death, lose their fibrous structure, become as uniform in texture as clay or dough, and even softer. This change is not from putrefaction, as they continue to be free from any offensive smell, and is most remarkable in the psoæ muscles, and those of the back.

Of the construction of the tail.—The mode in which the tail is constructed is perhaps as beautiful, as to the mechanism, as any part of the animal. It is wholly composed of 3 layers of tendinous fibres, covered by the common cutis and cuticle: 2 of these layers are external, the other internal. The direction of the fibres of the external layers is the same as in the tail, forming a stratum about $\frac{1}{3}$ of an inch thick; but varying, in this respect, as the tail is thicker or thinner. The middle layer is composed entirely of tendinous fibres, passing directly across, between the 2 external ones above described, their length being in proportion to the thickness of the tail; a structure which gives amazing strength to this part. The substance of the tail is so firm and compact, that the vessels retain their dilated state, even when cut across; and this section consists of a large vessel surrounded by as many small ones as can come in contact with its external surface; which of these are arteries, and which veins, I do not know. The fins are merely covered with a strong condensed adipose membrane.

Of the fat.—The fat of this order of animals, except the spermaceti, is what we generally term oil. It does not coagulate in our atmosphere, and is probably the most fluid of animal fats; but the fat of every different order of animals has not a peculiar degree of solidity, some having it in the same state, as the horse and bird. What I believe approaches nearest to spermaceti, is the fat of ruminating animals, called tallow. The fat is differently situated in different orders of animals; probably for particular purposes, at least in some we can assign a final intention. In the animals which are the subject of the present paper it is found principally on the outside of the muscles, immediately under the skin, and is in considerable quantity. It is rarely to be met with in the interstices of the muscles, or in any of the cavities, such as the abdomen or about the heart. In animals of the same class living on land, the fat is more diffused: it is situated, more especially when old, in the interstices of muscles, even between the fasciculi of muscular fibres, and is attached to many of the viscera; but many part, are free from fat, unless when diseased, as the penis, scrotum, testicle, eyelid, liver, lungs, brain, spleen, &c.

In fish its situation is rather particular, and is most commonly in 2 modes; in the one, diffused through the whole body of the fish, as in the salmon, herrings pilchard, sprat, &c.; in the other, it is found in the liver only, as in all of the ray kind, cod, and in all those called white-fish, there being none in any other part of the body *. The fat of fish appears to be diffused through the substance of the parts which contain it, but is probably in distinct cells. In some of these fish, where it is diffused over the whole body, it is more in some parts than others, as on the belly of the salmon, where it is in larger quantity.

The fat is differently inclosed in different orders of animals. In the quadruped, those of the seal kind excepted, in the bird, amphibia, and in some fish, it is contained in loose cellular membrane, as if in bags, composed of smaller ones, by which means the larger admit of motion on one another, and on their connecting parts; which motion is in a greater or less degree, as is proper or useful. Where motion could answer no purpose, as in the bones, it is confined in still smaller cells. The fat is in a less degree in the soles of the feet, palms of the hands, and in the breasts of many animals. In this order of animals and the seal kind, as far as I yet know, it is disposed of in 2 ways; the small quantity found in the cavities of the body, and interstices of parts, is in general disposed in the same way as in quadrupeds; but the external, which includes the principal part, is inclosed in a reticular membrane, apparently composed of fibres passing in all directions, which seem to confine its extent, allowing it little or no motion on itself, the whole, when distended, forming almost a solid body. This however is not always the case in every part of animals of this order; for under the head, or what may be rather called neck, of the bootle-nose, the fat is confined in larger cells, admitting of motion. This reticular membrane is very fine in some, and very strong and coarse in others, and even varies in different parts of the same animal. It is fine in the porpoise, spermaceti, and large whalebone whale; coarse in the grampus and small whalebone whale †: in all of them it is finest on the body, becoming coarser towards the tail, which is composed of

* The sturgeon is however an exception, having its fat in particular situations, and in the interstices of parts, as in other animals.—Orig.

† Where it is fine, it yields the largest quantity of oil, and requires the least boiling.—Orig.

fibres without any fat: which is also the case in the covering of the fins. This reticular net-work in the seal is very coarse; and in those which are not fat, when it collapses, it looks almost like a fine net with small meshes. This structure confines the animal to a determined shape, whereas in quadrupeds fat when in great quantity destroys all shape.

The fat differs in consistence in different animals, and in different parts of the same animal, in which its situation is various. In quadrupeds, some have the external fat softer than the internal; and that inclosed in bones is softest nearer to their extremities. Ruminating animals have that species of fat called tallow, and in their bones they have either hard fat or marrow, or fluid fat called neat's-foot oil. In this order of animals, the internal fat is the least fluid, and is nearly of the consistence of hog's-lard; the external is common train oil; but the spermaceti whale differs from every other animal I have examined, having the 2 kinds of fat just mentioned, and another which is totally different, called spermaceti, of which I shall give a particular account.

What is called spermaceti is found every where in the body in small quantity, mixed with the common fat of the animal, bearing a very small proportion to the other fat. In the head it is the reverse, for there the quantity of spermaceti is large when compared to that of the oil, though they are mixed, as in the other parts of the body. As the spermaceti is found in the largest quantity in the head, and in what would appear on a slight view to be the cavity of the skull, from a peculiarity in the shape of that bone, it has been imagined by some to be the brain. These 2 kinds of fat in the head are contained in cells, or cellular membrane, in the same manner as the fat in other animals; but besides the common cells there are larger ones, or ligamentous partitions going across, the better to support the vast load of oil, of which the bulk of the head is principally made up. There are 2 places in the head where this oil lies; these are situated along its upper and lower part: between them pass the nostrils, and a vast number of tendons going to the nose and different parts of the head. The purest spermaceti is contained in the smallest and least ligamentous cells: it lies above the nostril, all along the upper part of the head, immediately under the skin, and common adipose membrane. These cells resemble those which contain the common fat in the other parts of the body nearest the skin. That which lies above the roof of of the mouth, or between it and the nostril, is more intermixed with a ligamentous cellular membrane, and lies in chambers whose partitions are perpendicular. These chambers are smaller the nearer to the nose, becoming larger and larger towards the back part of the head, where the spermaceti is more pure. This spermaceti, when extracted cold, has a good deal the appearance of the internal structure of a water melon, and is found in rather solid lumps.

About the nose, or anterior part of the nostril, I discovered a great many vessels having the appearance of a plexus of veins, some as large as a finger. On examining them, I found they were loaded with the spermaceti and oil; and that some had corresponding arteries. They were most probably lymphatics; therefore I should suppose, that their contents had been absorbed from the cells of the head. We may the more readily suppose this, from finding many of the cells, or chambers, almost empty; and as we may reasonably believe that this animal had been some time out of the seas in which it could procure proper food, it had perhaps lived on the superabundance of oil.

The solid masses are what are brought home in casks for spermaceti. I found, by boiling this substance, that I could easily extract the spermaceti and oil which floated on the top from the cellular membrane. When I skimmed off the oily part, and let it stand to cool, I found that the spermaceti crystallized, and the whole became solid; and by laying this cake on any spongy substance, as chalk, or on a hollow body, the oil drained all off, leaving the spermaceti pure and white. These crystals were only attached to each other by edges, forming a spongy mass; and by melting this pure spermaceti, and allowing it to crystallize, it was reduced in appearance to half its bulk, the crystals being smaller, and more blended, consequently less distinct.

The spermaceti mixes readily with other oils, while it is in a fluid state, but separates or crystallizes whenever it is cooled to a certain degree; like 2 different salts being dissolved in water, one of which will crystallize with a less degree of evaporation than the other; or, if the water is warm, and fully saturated, one of the salts will crystallize sooner than the other, while the solution is cooling. I wanted to see whether spermaceti mixed equally well with the expressed oils of vegetables when warm, and likewise separated and crystallized when cold, and on trial there seemed to be no difference. When very much diluted with the oil, it is dissolved or melted by a much smaller degree of heat than when alone; and this is the reason perhaps that it is in a fluid state in the living body.

If the quantity of spermaceti be small in proportion to the other oil, it is perhaps nearly in that proportion longer in crystallizing; and when it does crystallize, the crystals are much smaller than those that are formed where the proportion of spermaceti is greater. From the slowness with which the spermaceti crystallizes when much diluted with its oil, from a considerable quantity being to be obtained in that way, and from its continuing for years to crystallize, one would be induced to think that perhaps the oil itself is converted into spermaceti. It is most likely, that if we could discover the exact form of the different crystals of oils, we should thence be able to ascertain both the different sorts of vegetable oils, expressed and essential, and the different sorts of animal oils, much better than by any other means; in the same manner as we know salts by the forms into which they shoot.

The spermaceti does not become rancid, or putrid, nearly so soon as the other animal oils; which is most probably owing to the spermaceti being for the most part in a solid state; and I should suppose that few oils would become so soon rancid as they do, if they were always preserved in that degree of cold which rendered them solid: neither does this oil become so soon putrid as the flesh of the animal; and therefore, though the oil in the cells appeared to be putrid before boiling, it was sweet when deprived of the cellular substance. The spermaceti is rather heavier than the other oil.

In this animal we find 2 sorts of oil, besides the deeper seated fat, common to all of this class; one of which crystallizes with a much less degree of cold than the other, and of course requires a greater degree of heat to melt it, and forms perhaps the largest crystals of any expressed oil we know: yet the fluid oil of this animal will crystallize in an extreme hard frost, much sooner than most essential oils, though not so soon as the expressed oils of vegetables. Camphire however is an exception, since it crystallizes in our warmest weather, and when melted with expressed oil of vegetables, if the oil is too much saturated for that particular degree of cold, crystallizes exactly like spermaceti. In the ox the tallow, and what is called neat's-foot oil, crystallize in different degrees of cold. The tallow congeals with rather less cold than the spermaceti; but the other oil is similar to what is called the train oil in the whale. I have endeavoured to discover the form of the crystals of different sorts of oil; but could never determine exactly what that was, because I could never find any of the crystals single, and by being always united, the natural form was not distinct.

It is the adipose covering from all of the whale kind that is brought home in square pieces, called flitches, and which, by being boiled, yields the oil on expression, leaving the cellular membrane. When these flitches have become in some degree putrid, there issues 2 sorts of oil; the first is pure, the last seems incorporated with part of the animal substance, which has become easy of solution from its putridity, forming a kind of butter. It is unctuous to the touch, ropy, coagulates or becomes harder by cold, floats on water, not being soluble in it; and the pure oil, separating in the same manner from this, floats above all. What remains, after all the oil is extracted, retains a good deal of its form, is almost wholly convertible into glue, and is sold for that purpose. The cellular, or rather what should be called the uniting membrane in this order of animals, is similar to that in the quadruped; we find it uniting muscle to muscle, and muscle to bone, for their easy motion on one another. The cellular membrane, which is the receptacle for the oil near the surface of the body, is in general very different from that in the quadruped, as has been already observed.

Of the skin.—The covering of this order of animals consists of a cuticle and cutis. The cuticle is somewhat similar to that on the sole of the foot in the human species, and appears to be made up of a number of layers, which separate by slight putrefaction; but this I suspect arises in some degree from there being a succession of cuticles formed. It has no degree of elasticity or toughness, but tears easily; nor do its fibres appear to have any particular direction. The internal stratum is tough and thick, and in the spermaceti whale its internal surface, when separated from the cutis, is just like coarse velvet, each pile standing firm in its place; but this is not so distinguishable in some of the others, though it appears rough from the innumerable perforations. It is the cuticle that gives the colour to the animal; and in parts that are dark I think I have seen a dirty coloured substance, washed away in the separation of the cuticle from the cutis, which must be a kind of rete mucosum.

The cutis in this tribe is extremely villous on its external surface, answering to the rough surface of the cuticle, and forming in some parts small ridges, similar to those on the human fingers and toes. These villi are soft and pliable; they float in water, and each is longer or shorter according to the size of the animal. In the spermaceti whale they were about a quarter of an inch long; in the grampus, bottle-nose and piked whales, much shorter; in all, they are extremely vascular. The cutis seems to be the termination of the cellular membrane of the body more closely united, having smaller interstices, and becoming more compact. This alteration in the texture is so sudden as to make an evident distinction between what is solely connecting membrane, and skin, and is most evident in lean animals; for in the change from fat to lean, the skin does not undergo an alteration equal to what takes place in the adipose membrane, though it may be observed, that the skin itself is diminished in thickness. In fat animals the distinction between skin and cellular membrane is much less, the gradation from the one to the other seeming to be slower; for the cells of both membrane and skin being loaded with fat, the whole has more the appearance of one uniform substance. This uniformity of the adipose membrane and skin is most observable in the whale, seal, hog, and the human species; and is not only visible in the raw but in the dressed hides; for in dressed skins the external is much more compact in texture than in the inner surface, and is in common very tough.

In some animals the cutis is extremely thick, and in some parts much more so than others: where very thick, it appears to be intended as a defence against the violence of their own species or other animals. In most quadrupeds it is muscular, contracting by cold, and relaxing by heat. Many other stimulating substances make it contract; but cold is probably that stimulus by which it was intended to be generally affected.

The skin is extremely elastic in the greatest number of quadrupeds, and in its contracted state may be said to be rather too small for the body; by this elasticity it adapts itself to the changes which are constantly taking place in the parts; and it is for the want of it that it becomes too large in some old animals. In all animals it is more elastic in some parts than others, especially in those where there is the greatest motion. How far these variations take place in the whale I do not exactly know; but a loose elastic skin in this tribe would appear to be improper as universal covering, considering the progressive motion of the animal, and the medium in which it moves; it therefore appears to be kept always on the stretch, by the adipose membrane being loaded with fat, which does not allow the skin to recede when cut. It is however more elastic at the setting on of the eyelids, round the opening of the prepuce, the nipples, the setting on of the fins, and under the jaw, to allow of motion in those parts; and here there is more reticular, and less adipose membrane. But in the piked whale there is probably one of the most striking instances of an elastic cuticular contraction: for though the whole skin of the fore part of the neck and breast of the animal, as far down as the middle of the belly, be extremely elastic; yet to render it still more so, it is ribbed longitudinally like a ribbed stocking, which gives an increased lateral elasticity. These ribs are, when contracted, about $\frac{5}{8}$ of an inch broad, covered with the common skin of the animal; but in the hollow part of the rib, it is of a softer texture, with a thinner cuticle. This part is possessed of the greatest elasticity: but why it should be so elastic is difficult to say, as it covers the thorax, which can never be increased in size; yet there must be some peculiar circumstance in the economy of the species requiring this structure, which we as yet know nothing of. The skin is intended for various purposes. It is the universal covering given for the defence of all kinds of animals; and that it might answer this purpose well, it is the seat of one of the senses.

Of the mode of catching their food.—The mouths of animals are the first parts to be considered respecting nourishment or food, and are so much connected with every thing relative to it, as not only to give good hints whether the food is vegetable or animal, but also respecting the particular kind of either, especially of animal food. The mouth not only receives the food, but is the immediate instrument for catching it. As it is a compound instrument in many animals, having parts of various constructions belonging to it, I shall at present consider it in this tribe no further than as connected with their mode of catching the food, and adapting and disposing it for being swallowed. It is probable that these animals do not require either a division of the food, or a mastication of it in the mouth, but swallow whatever they catch, whole; for we do not find any of them furnished with parts capable of producing either effect. The mouth in most of this tribe is well adapted for catching the food; the jaws spread as they go back, making the mouth proportionally wider than in many other animals. There is a very great variety in the formation of the mouths of this tribe of animals, which we have many opportunities of knowing, from the head being often brought home when the other parts of the animal are rejected; a circumstance which frequently leaves us ignorant of the particular species to which it belonged.

Some catch their food by means of teeth, which are in both jaws, as the porpoise and grampus; in others, they are only in one jaw, as in the spermaceti whale; and in the large bottle-nose whale, described by Dale, there are only 2 small teeth in the anterior part of the lower jaw. In the Narwhale only 2 tusks in the fore part of the upper jaw;* while in some others there are none at all. In those which have teeth in both jaws, the number in each varies considerably; the small bottle-nose had 46 in the upper, and 50 in the lower; and in the jaws of others there are only 5 or 6 in each.

The teeth are not divisible into different classes, as in quadrupeds; but are all pointed teeth, and are commonly a good deal similar. Each tooth is a double cone, one point being fastened in the gum, the other projecting: they are however not all exactly of this shape. In some species of porpoise the fang is flattened, and thin at its extremity; in the spermaceti whale the body of the tooth is a little curved towards the back part of the mouth; this is also the case in some others. The teeth are composed of animal substance and earth, similar to the bony part of the teeth in quadrupeds. The upper teeth are commonly worn down on the inside, the lower on the outside; this arises from the upper jaw being in general the larger.

The situation of the teeth when first formed, and their progress afterwards, as far as I have been able to observe, is very different in common from those of the quadruped. In the quadruped the teeth are formed in the jaw, almost surrounded by the alveoli, or sockets, and rise in the jaw as they increase in length; the covering of the alveoli being absorbed, the alveoli afterwards rise with the teeth, covering the whole fang; but in this tribe the teeth appear to form in the gum, on the edge of the jaw, and they either sink in the jaw as they lengthen, or the alveoli rise to inclose them: this last is most probable, since the depth of the jaw is also increased, so that the teeth appear to sink deeper and deeper in the jaw. This formation is readily discovered in jaws not full grown; for the teeth increase in number as the jaw lengthens, as in other animals. The pos-

* I call these tusks to distinguish them from common teeth. A tusk is that kind of tooth which has no bounds set to its growth, excepting by abrasion, as the tusk of the elephant, boar, sea-horse, manatee, &c.—Orig.

terior part of the jaw becoming longer, the number of teeth in that part increases, the sockets becoming shallower and shallower, and at last being only a slight depression.

It would appear that they do not shed their teeth, nor have they new ones formed similar to the old, as is the case with most other quadrupeds, and also with the alligator. I have never been able to detect young teeth under the roots of the old ones; and indeed the situation in which they are first formed makes it in some degree impossible, if the young teeth follow the same rule in growing with the original ones, as they probably do in most animals. If it be true, that the whale tribe do not shed their teeth, in what way are they supplied with new ones, corresponding in size with the increased size of the jaw? It would appear that the jaw, as it increases posteriorly, decays at the symphysis, and while the growth is going on, there is a constant succession of new teeth, by which means the new-formed teeth are proportioned to the jaw. The same mode of growth is evident in the elephant, and in some degree in many fish; but in these last the absorption of the jaw is from the whole of the outside along where the teeth are placed. The depth of the alveoli seems to prove this, being shallow at the back part of the jaw, and becoming deeper towards the middle, where they are the deepest, the teeth there having come to the full size. From this forwards they are again becoming shallower, the teeth being smaller, the sockets wasting, and at the symphysis there are hardly any sockets at all. This will make the exact number of teeth in any species uncertain.

Some genera of this tribe have another mode of catching their food, and retaining it till swallowed, which is by means of the substance called whalebone. Of this there are 2 kinds known; one very large, probably from the largest whale yet discovered; the other from a smaller species. This whalebone, which is placed on the inside of the mouth, and attached to the upper jaw, is one of the most singular circumstances belonging to this species, as they have most other parts in common with quadrupeds. It is a substance, I believe, peculiar to the whale, and of the same nature as horn, which I shall use as a term to express what constitutes hair, nails, claws, feathers, &c. it is wholly composed of animal substance, and extremely elastic.*

Whalebone consists of thin plates of some breadth, and in some of very considerable length, their breadth and length in some degree corresponding to each other; and when longest they are commonly the broadest, but not always so. These plates are very different in size in different parts of the same mouth, more especially in the large whalebone whale, whose upper jaw does not pass parallel on the under, but makes an arch, the semidiameter of which is about $\frac{1}{4}$

* From this it must appear that the term bone is an improper one.—Orig.

of the length of the jaw. The head in my possession is 19 feet long, the semi-diameter not quite 5 feet: if this proportion is preserved, those whales which have whalebone 15 feet long must be of an immense size. These plates are placed in several rows, encompassing the outer skirts of the upper jaw, similar to teeth in other animals. They stand parallel to each other, having one edge towards the circumference of the mouth, the other towards the centre or cavity. They are placed near together in the piked whale, not being $\frac{1}{4}$ of an inch asunder where at the greatest distance, yet differing in this respect in different parts of the same mouth; but in the great whale the distances are more considerable.

The outer row is composed of the longest plates; and these are in proportion to the different distances between the 2 jaws, some being 14 or 15 feet long, and 12 or 15 inches broad; but towards the anterior and posterior part of the mouth, they are very short: they rise for half a foot or more, nearly of equal breadths, and afterwards shelve off from their inner side, till they come near to a point at the outer: the exterior of the inner rows are the longest, corresponding to the termination of the declivity of the outer, and become shorter and shorter till they hardly rise above the gum. The inner rows are closer than the outer, and rise almost perpendicularly from the gum, being longitudinally straight, and have less of the declivity than the outer. The plates of the outer row laterally are not quite flat, but make a serpentine line, more especially in the piked whale the outer edge is thicker than the inner. All round the line made by their outer edges, runs a small white bead, which is formed along with the whalebone, and wears down with it. The smaller plates are nearly of an equal thickness on both edges. In all of them, the termination is in a kind of hair, as if the plate was split into innumerable small parts, the exterior being the longest and strongest.

The 2 sides of the mouth composed of these 2 rows meet nearly in a point at the tip of the jaw, and spread or recede laterally from each other, as they pass back; and at their posterior ends, in the piked whale, they make a sweep inwards, and come very near each other, just before the opening œsophagus. In the piked whale there were above 300 in the outer rows on each side of the mouth. Each layer terminates in an oblique surface, which obliquity inclines to the roof of the mouth, answering to the gradual diminution of their length; so that the whole surface, composed of these terminations, forms one plane rising gradually from the roof of the mouth; from this obliquity of the edge of the outer row, we may in some measure judge of the extent of the whole base, but not exactly, as it makes a hollow curve, which increases the base. The whole surface resembles the skin of an animal covered with strong hair, under which surface the tongue must immediately lie, when the mouth is shut; it is of a light brown colour in the piked whale, and is darker in the large whale.

In the piked whale, when the mouth is shut, the projecting whalebone remains entirely on the inside of the lower jaw, the two jaws meeting every where along their surface; but how this is effected in the large whale I do not certainly know, the horizontal plane made by the lower jaw being straight, as in the piked whale; but the upper jaw, being an arch, cannot be hid by the lower. I suppose therefore that a broad upper lip, meeting as low as the lower jaw, covers the whole of the outer edges of the exterior rows. The whalebone is continually wearing down, and renewing in the same proportion, except that when the animal is growing it is renewed faster, and in proportion to the growth.

The formation of the whalebone is extremely curious, being in one respect similar to that of the hair, horns, spurs, &c.; but it has besides another mode of growth and decay, equally singular. These plates form a thin vascular substance, not immediately adhering to the jaw-bone; but having a more dense substance between, which is also vascular. This substance, which may be called the nidus of the whalebone, sends out the above thin broad processes, answering to each plate, on which the plate is formed, as the cock's spur or the bull's horn, on the bony core, or a tooth on its pulp; so that each plate is necessarily hollow at its growing end, the first part of the growth taking place on the inside of this hollow. Besides this mode of growth, which is common to all such substances, it receives additional layers on the outside, which are formed on the above-mentioned vascular substance extended along the surface of the jaw. This part also forms on it a semi-horny substance between each plate, which is very white, rises with the whalebone, and becomes even with the outer edge of the jaw, and the termination of its outer part forms the bead above-mentioned. This intermediate substance fills up the spaces between the plates as high as the jaw, acts as abutments to the whalebone, or is similar to the alveolar processes of the teeth, keeping them firm in their places.

As both the whalebone and intermediate substance are constantly growing, and as we must suppose a determined length necessary, a regular mode of decay must be established, not depending entirely on chance, or the use it is put to. In its growth, 3 parts appear to be formed; one from the rising core, which is the centre, a 2d on the outside, and a 3d being the intermediate substance. These appear to have 3 stages of duration; for that which forms on the core I believe makes the hair, and that on the outside makes principally the plate of whalebone; this, when got a certain length, breaks off, leaving the hair projecting, becoming at the termination very brittle; and the 3d, or intermediate substance, by the time it rises as high as the edge of the skin of the jaw, decays and softens away like the old cuticle of the sole of the foot when steeped in water. The use of the whalebone, I should believe, is principally for the retention of the food till swallowed; and do suppose the fish they catch are small, when compared with the size of the mouth.

The œsophagus, as in other animals, begins at the fauces, or posterior part of the mouth; and, though circular at this part, is soon divided into 2 passages by the epiglottis passing across it, as will be described hereafter. Below its attachment to the trachea, it passes down in the posterior mediastinum, at some distance from the spine, to which it is attached by a broad part of the same membrane, and its anterior surface makes the posterior part of a cavity behind the pericardium. Passing through the diaphragm it enters the stomach, and is lined with a very thick, soft, and white cuticle, which is continued into the first cavity of the stomach. The inner, or true coat, is white, of a considerable density, and not muscular; but thrown into large longitudinal folds by the contraction of the muscular fibres of the œsophagus, which are very strong. It is very glandular; for on its inner surface, especially near the fauces, orifices of a vast number of glands are visible. The œsophagus is larger in proportion to the bulk of the animal than in the quadruped, though not so much so as it usually is in fish, which we may suppose swallow their food much in the same way. In the piked whale it was 3½ inches wide.

The stomach, as in other animals, lies on the left side of the body, and terminates in the pylorus towards the right. The duodenum passes down on the right side, very much as in the human subject, excepting that it is more exposed from the colon not crossing it. It lies on the right kidney, and then passes to the left side behind the ascending part of the colon and root of the mesentery, comes out on the left side, and getting on the edge of the mesentery becomes a loose intestine, forming the jejunum. In this course behind the mesentery it is exposed, as in most quadrupeds, not being covered by it, as in the human. The jejunum and ilium pass along the edge of the mesentery downwards to the lower part of the abdomen. The ilium near the lower end makes a turn towards the right side, and then mounting upwards, round the edge of the mesentery, passes a little way on the right, as high as the kidney, and there enters the colon, or cæcum. The cæcum lies on the lower end of the kidney, considerably higher than in the human body, which renders the ascending part of the colon short. The cæcum is about 7 inches long, and more like that of the lion or seal than of any other animal I know.

The colon passes obliquely up the right side, a little towards the middle of the abdomen; and when as high as the stomach, crosses to the left, and acquires a broad mesocolon: at this part it lies on the left kidney, and in its passage down gets more and more to the middle line of the body. When it has reached the lower part of the abdomen, it passes behind the uterus, and along with the vagina in the female; between the 2 testicles, and behind the bladder and root of the penis, in the male, bending down to open on what is called the belly of the animal; and in its whole course it is gently convoluted. In those which have no cæcum, and therefore can hardly be said to have a colon, the intestine before its termination in the rectum makes the same kind of sweep round the other intestines, as the colon does where there is a cæcum.

The intestines are not large for the size of the animal, not being larger in those of 18 or 24 feet long than in the horse, the colon not much more capacious than the jejunum and ilium, and very short; a circumstance common to carnivorous animals. In the piked whale, the length from the stomach to the cæcum is $28\frac{1}{3}$ yards, length of cæcum 7 inches, of the colon to the anus $2\frac{3}{4}$ yards. The small intestines are just 5 times the length of the animal, the colon with the cæcum a little more than half its length.

Those parts that respect the nourishment of this tribe do not all so exactly correspond as in land animals; for in these one in some degree leads to the other. Thus the teeth in the ruminating tribe point out the kind of stomach, cæcum, and colon; while in others, as the horse, hare, lion, &c. the appearances of the teeth only give us the kind of colon and cæcum; but in this tribe, whether teeth or no teeth, the stomachs do not vary much, nor does the circumstance of cæcum seem to depend on either teeth or stomach. The circumstances by which, from the form of one part we judge what others are, fail us here; but this may arise from not knowing all the circumstances. The stomach, in all that I have examined, consists of several bags, continued from the first on the left towards the right, where the last terminates in duodenum. The number is not the same in all; for in the porpoise, grampus, and piked whale, there are 5; in the bottle-nose 7. Their size respecting each other differs very considerably; so that the largest in one species may in another be only the 2d. The 2 first in the porpoise, bottle-nose, and piked whale, are by much the largest; the others are smaller, though irregularly so.

The first stomach has, I believe, in all very much the shape of an egg, with the small end downwards. It is lined every where with a continuation of the cuticle from the œsophagus. In the porpoise the œsophagus enters the superior end of the stomach. In the piked whale its entrance is a little way on the posterior part of the upper end, and is oblique.

The 2d stomach in the piked whale is very large, and rather longer than the first. It is of the shape of the Italic *S*, passing out from the upper end of the first on its right side, by nearly as large a beginning as the body of the bag. In the porpoise it by no means bears the same proportion to the first, and opens by a narrower orifice; then passing down along the right sight of the first stomach, it bends a little outwards at the lower end, and terminates in the 3d. Where this 2d stomach begins, the cuticle of the 1st ends. The whole of the inside of this stomach is thrown into unequal rugæ, appearing like a large irregular honeycomb. In the piked whale the rugæ are longitudinal, and in many places very deep, some of them being united by cross bands; and in the porpoise the folds are very thick, massy, and indented into each other. This stomach opens into the 3d by a round contracted orifice, which does not seem to be valvular.

The 3d stomach is by much the smallest, and appears to be only a passage between the 2d and 4th. It has no peculiar structure on the inside, but terminates in the 4th by nearly as large an opening as its beginning. In the porpoise it is not above 1, and in the bottle-nose about 5 inches long. The 4th stomach is of a considerable size; but a good deal less than either the 1st or 2d. In the piked whale it is not round, but seems flattened between the 2d and 5th. In the porpoise it is long, passing in a serpentine course almost like an intestine. The internal surface is regular, but villous, and opens on its right side into the 5th, by a round opening smaller than the entrance from the 3d. The 5th stomach is in the piked whale round, and in the porpoise oval; it is small, and terminates in the pylorus, which has little of a valvular appearance. Its coats are thinner than those of the 4th, having an even inner surface, which is commonly tinged with bile.

The piked whale and, I believe, the large whalebone whale, have a cæcum; but it is wanting in the porpoise, grampus, and bottle-nose whale. The structure of the inner surface of the intestine is in some very singular, and different from that of the others. The inner surface of the duodenum in the piked whale is thrown into longitudinal rugæ, or valves, which are at some distance from each other, and these receive lateral folds. The duodenum in the bottle-nose swells out into a large cavity, and might almost be reckoned an 8th stomach; but as the gall ducts enter it I shall call it duodenum.

The inner coat of the jejunum, and ilium, appears in irregular folds, which may vary according as the muscular coat of the intestine acts: yet I do not believe, that their form depends entirely on that circumstance, as they run longitudinally, and take a serpentine course when the gut is shortened by the contraction of the longitudinal muscular fibres. The intestinal canal of the porpoise has several longitudinal folds of the inner coat passing along it, through the whole of its length. In the bottle-nose the inner coat, through nearly the whole track of the intestine, is thrown into large cells, and these again subdivided into smaller; the axis of which cells is not perpendicular to a transverse section of the intestine, but oblique, forming pouches with the mouths downwards, and acting almost like valves, when any thing is attempted to be passed in a contrary direction: they begin faintly in the duodenum, before it makes its quick turn, and terminate near the anus. The colon and rectum have the rugæ very flat, which seems to depend entirely on the contraction of the gut.

The rectum near the anus appears, for 4 or 5 inches, much contracted, is glandular, covered by a soft cuticle, and the anus small. I never found any air in the intestines of this tribe; nor indeed in any of the aquatic animals. The mesenteric artery anastomoses by large branches.

There is a considerable degree of uniformity in the liver of this tribe of animals. In shape it nearly resembles the human, but is not so thick at the base, nor so sharp at the lower edge, and is probably not so firm in its texture. The right lobe is the larger and thicker, its falciform ligament broad, and there is a large fissure between the 2 lobes, in which the round ligament passes. The liver towards the left is very much attached to the stomach, the little epiploon being a thick substance. There is no gall-bladder; the hepatic duct is large, and enters the duodenum about 7 inches beyond the pylorus.

The pancreas is a very long, flat body, having its left end attached to the right side of the first cavity of the stomach: it passes across the spine at the root of the mesentery, and near to the pylorus joins the hollow curve of the duodenum, along which it is continued, and adheres to that intestine, its duct entering that of the liver near the termination in the gut.

Though this tribe cannot be said to ruminate, yet in the number of stomachs they come nearest to that order; but here I suspect that the order of digestion is in some degree inverted. In both the ruminants, and this tribe, I think it must be allowed that the 1st stomach is a reservoir. In the ruminants the precise use of the 2d and 3d stomachs is perhaps not known; but digestion is certainly carried on in the 4th; while in this tribe I imagine digestion is performed in the 2d, and the use of the 3d and 4th is not exactly ascertained.

The cæcum and colon do not assist in pointing out the nature of the food and mode of digestion in this tribe. The porpoise, which has teeth, and 4 cavities to the stomach, has no cæcum, similar to some land animals, as the bear, badger, racoon, ferret, polecat, &c.; neither has the bottle-nose a cæcum, which has only 2 small teeth in the lower jaw; and the piked whale, which has no teeth, has a cæcum, almost exactly like the lion, which has teeth and a very different kind of stomach.

The food of the whole of this tribe I believe is fish; probably each may have a particular kind, of which it it fondest, yet does not refuse a variety. In the stomach of the large bottle-nose, I found the beaks of some hundreds of cuttle-fish. In the grampus I found the tail of a porpoise; so that they eat their own genus. In the stomach of the piked whale, I found the bones of different fish, but particularly those of the dog-fish. From the size of the œsophagus we may conclude, that they do not swallow fish so large in proportion to their size as many fish do, that we have reason to believe take their food in the same way: for fish often attempt to swallow what is larger than their stomachs can at one time contain, and part remains in the œsophagus till the rest is digested.

The epiploon on the whole is a thin membrane; on the right side it is rather a thin net-work, though on the left it is a complete membrane, and near to the stomach of the same side becomes of a considerable thickness, especially between the first 2 bags of the stomach. It has little or no fat, except what slightly covers the vessels in particular parts. It is attached forwards, all along to the lower part of the different bags constituting the stomach, and on the right to the root of the mesentery, between the stomach and transverse arch of the colon, first behind to the transverse arch of the colon and root of the mesentery, then to the posterior surface of the left or first bag of the stomach, behind the anterior attachment. In some of this tribe there is the usual passage behind the vessels going to the liver, common to all quadrupeds I am acquainted with; but in others, as the small bottle-nose, there is no such passage, which by the cavity behind the stomach in the epiploon of this animal becomes a circumscribed cavity.

The spleen is involved in the epiploon, and is very small for the size of the animal. There are in some, as the porpoise, 1 or 2 small ones, about the size of a nutmeg, often smaller, placed in the epiploon behind the other. These are sometimes met with likewise in the human body.

The kidneys in the whole of this tribe of animals are conglomerated, being made up of smaller parts, which are only connected by cellular membrane, blood-vessels, and ducts, or infundibula; but not partially connected by continuity of substance, as in the human body, the ox, &c.: every portion is of a conical figure, whose apex is placed towards the centre of the kidney, the base making the external surface; and each is composed of a cortical and tubular substance, the tubular terminating in the apex, which apex makes the mamilla. Each mamilla has an infundibulum, which is long, and at its beginning wide, embracing the base of the mamilla, and becoming smaller. These infundibula unite at last, and form the ureter. The whole kidney is an oblong flat body, broader and thicker at the upper end than the lower, and has the appearance of being made up of different parts placed close together, almost like the pavement of a street. The ureter comes out at the lower end, and passes along to the bladder, which it enters very near the urethra. The bladder is oblong, and small for the size of the animal. In the female the urethra passes along to the external fulcus or vulva, and opens just under the clitoris, much as in the human subject.

Whether being inhabitants of the water makes such a construction of kidney necessary I cannot say; yet one must suppose it to have some connection with such situation, since we find it almost uniformly take place in animals inhabiting the water, whether wholly, as this tribe, or occasionally, as the manatee, seal, and white bear: there is however the same structure in the black bear, which I believe never inhabits the water. This perhaps should be considered in another light, as nature keeping up to a certain uniformity in the structure of similar animals; for the black bear in construction of parts is, in every other respect as well as this, like the white bear.

The capsulæ renales are small for the size of the animal, when compared to the human, as indeed they are in most animals. They are flat, and of an oval figure; the right lies on the lower and posterior part of the diaphragm somewhat higher than the kidney; the left is situated lower down, by the side of the aorta, between it and the left kidney. They are composed of 2 substances; the external having the direction of its fibres or parts towards the centre; the internal seeming more uniform, and not having so much of the fibrous appearance.

The blood of animals of this order is, I believe, similar to that of quadrupeds; but I have an idea that the red globules are in larger proportion. I will not pretend to determine how far this may assist in keeping up the animal heat; but as these animals may be said to live in a very cold climate or atmosphere, and such as readily carries off heat from the body, they may want some help of this kind. It is certain that the quantity of blood in this tribe, and in the seal, is comparatively larger than in the quadruped, and therefore probably amounts to more than that of any other known animal. This tribe differs from fish in having the red blood carried to the extreme parts of the body, as similar to the quadruped.

The cavity of the thorax is composed of nearly the same parts as in the quadruped; but there appears to be some difference, and the varieties in the different genera are greater. The general cavity is divided into 2, as in the quadruped, by the heart and mediastinum. The heart in this tribe, and in the seal, is probably larger in proportion to their size than in the quadruped, also the blood-vessels, more especially the veins.

The heart is inclosed in its pericardium, which is attached by a broad surface to the diaphragm, as in the human body. It is composed of 4 cavities,* 2 auricles, and 2 ventricles: it is more flat than in the quadruped, and adapted to the shape of the chest. The auricles have more fasciculæ, and these pass more across the cavity from side to side than in many other animals; besides, being

* As the circulation is a permanent part of the constitution respecting the class to which the animal belongs, and as the kind of heart corresponds with the circulation, these should be considered in the classing of animals. Thus we have animals whose hearts have only 1 cavity, others with 2, 3, and 4 cavities.—Orig.

very muscular, they are very elastic, for being stretched they contract again very considerably. There is nothing uncommon or particular in the structure of the ventricles, in the valves of the ventricles, or in that of the arteries. The general structure of the arteries resembles that of other animals; and where parts are nearly similar, the distribution is likewise similar. The aorta forms its usual curve, and sends off the carotid and subclavian arteries.

Animals of this tribe, as has been observed, have a greater proportion of blood than any other known, and there are many arteries apparently intended as reservoirs, where a larger quantity of arterial blood seemed to be required in a part, and vascularity could not be the only object. Thus we find, that the intercostal arteries divide into a vast number of branches, which run in a serpentine course between the pleura, ribs, and their muscles, making a thick substance somewhat similar to that formed by the spermatic artery in the bull. Those vessels, every where lining the sides of the thorax, pass in between the ribs near their articulation, and also behind the ligamentous attachment of the ribs, and anastomose with each other. The medulla spinalis is surrounded with a net-work of arteries in the same manner more especially where it comes out from the brain, where a thick substance is formed by their ramifications and convolutions; and these vessels most probably anastomose with those of the thorax.

The subclavian artery in the piked whale, before it passes over the first rib, sends down into the chest arteries which assist in forming the plexus on the inside of the ribs; I am not certain but the internal mammary arteries contribute to form the anterior part of this plexus. The motion of the blood in such must be very slow; the use of which we do not readily see. The descending aorta sends off the intercostals, which are very large, and give branches to this plexus; and when it has reached the abdomen, it sends off, as in the quadruped, the different branches to the viscera, and the lumbar arteries, which are likewise very large for the supply of that vast mass of muscles which moves the tail.

In our examination of particular parts, the size of which is generally regulated by that of the whole animal, if we have only been accustomed to see them in those which are small or middle-sized, we behold them with astonishment in animals so far exceeding the common bulk as the whale. Thus the heart and aorta of the spermaceti whale appeared prodigious, being too large to be contained in a wide tub, the aorta measuring a foot in diameter. When we consider these as applied to the circulation, and figure to ourselves, that probably 10 or 15 gallons of blood are thrown out at one stroke, and moved with an immense velocity through a tube of a foot diameter, the whole idea fills the mind with wonder. The veins I believe have nothing particular in their structure, excepting in parts requiring a peculiarity, as in the folds of the skin on the breast in the piked whale, where their elasticity was to be increased.

Of the larynx.—The larynx in most animals living on land is a compound organ, adapted for respiration, deglutition, and sound, which last is produced in the actions of respiration; but in this tribe the larynx I suppose is only adapted to respiration, as we do not know that they have any mode of producing sound. It is composed of os hyoides, thyroid, cricoid, and 2 arytenoid cartilages, with the epiglottis. It varies very much in structure and size, when compared in animals of different genera. These cartilages were much smaller in the bottle-nose of 24 feet long, than in the piked whale of 17 feet, while the os hyoides was much larger.

In the bottle-nose, the os hyoides is composed of 3 bones, besides 2 whose ends are attached to it, being placed above the os hyoides, making 5 in all. In the porpoise, piked whale, &c. it is but one bone, slightly bent, having a broad thin process passing up, which is a little forked; it has no attachment to the head by means of other bones, as in many quadrupeds. The thyroid cartilage in the piked whale is broad from side to side, but not from the upper to the lower part: it has 2 lateral processes, which are long, and pass down the outside of the cricoid, near to its lower end, and are joined to it much as in the human subject. These differ in shape in different animals of this tribe. The cricoid cartilage is broad and flat, making the posterior and lateral part of the larynx, and is much deeper behind, and laterally, than before. It is extremely thick and strong, flattened on the posterior surface, and hollowed from the upper edge to the lower. It terminates by a thick edge on the posterior part above, but irregularly at the lower edge, in the cartilages of the larynx.

The 2 arytenoid cartilages are extremely projecting, and united to each other till near their ends; are articulated on the upper edge of the cricoid, but send down a process, which passes on the inside of the cricoid, being attached to a bag in the piked whale, which is formed below the thyroid and before the cricoid cartilages; they cross the cavity of the larynx obliquely, making the passage, at the upper part, a groove between them: the cavity at this place swells out laterally, but is very narrow between the anterior and posterior surfaces. The passage above, between the arytenoid and thyroid cartilages, is wide from side to side, and is continued down on the outside of the processes of the arytenoid cartilage, as well as between them, ending below the thyroid, which is folliculated on its inner surface on the fore part of the cricoid cartilage.

The epiglottis makes a 3d part of the passage, and completes the glottis by forming it into a canal, in several of this tribe; but in the piked whale it was not attached to the arytenoid cartilages, but only in contact, or inclosing them at their base, so as to make them form a complete canal. I could not observe any thing like a thyroid gland. From the glottis and epiglottis being so connected as to make but one canal, and from the thyroid and cricoid cartilages being so flattened in some between the anterior and posterior surface, the passage through these parts is very small or contracted; but the trachea swells out again into a very considerable size. Its larger branches are in proportion to the trunk, and enter the lungs at the upper end along with the blood-vessels.

Of the lungs.—The lungs are 2 oblong bodies, one on each side of the chest, and are not divided into smaller lobes, as in the human subject. They are of considerable length, but not so deep between the fore and back part, as in the quadruped, from the heart being broad, flat, and of itself filling up the fore part of the chest. They pass farther down on the back part than in the quadruped, by which their size is increased, and rise higher up in the chest than the entrance of the vessels, coming to a point at the upper end. From the entrance of the vessels they are connected downwards, along their whole inner edge, by a strong attachment (in which there are in some lymphatic glands) to the posterior mediastinum. The lungs are extremely elastic in their substance, even so much so as to squeeze out any air that may be thrown into them, and to become almost at once a solid mass, having a good deal the appearance, consistence, and feel of an ox's spleen. The branches of the bronchiæ which ramify into the lungs have not the cartilages flat, but rather rounded; a construction which admits of greater motion between each. The pulmonary cells are smaller than in quadrupeds, which may make less air necessary, and they communicate with each other, which those of the quadruped do not; for by blowing into one branch of the trachea, not only the part to which it immediately goes, but the whole lungs are filled.

As the ribs in this tribe do not completely make the cavity of the thorax, the diaphragm has not the same attachments as in the quadruped, but is connected forwards to the abdominal muscles, which are very strong, being a mixture of muscular and tendinous fibres. The position of the diaphragm is less transverse than in the quadruped, passing more obliquely backwards, and coming very low on the spine, and higher up before; which makes the chest longest in the direction of the animal at the back, and gives room for the lungs to be continued along the spine.

The parts immediately concerned in inspiration are extremely strong; the diaphragm remarkably so. The reason of this must at once appear; it necessarily requiring great force to expand in a dense medium like water, especially too when the vacuity is to be filled with one which is rarer, and is to water a species of vacuum, the pressure being much greater on the external surface than the counter-pressure from within. But expiration on the other hand must be much more easily performed; the natural elasticity of the parts themselves, with the pressure of the water on the external surface of the body, being greater than the resistance of the air from within, will both tend to produce expiration without any immediate action of muscles. The diaphragm in these animals appears to be the principal agent in inspiration; and the cavity of the thorax not being entirely surrounded by bony parts, is of course less easily expanded, and the apparatus for its expansion in all directions, as in the quadruped, does not exist here.

The blow-hole, or passage for the air.—As the nose in every animal that breathes air is a common passage for the air, and is also the organ of smelling; I shall describe it in this tribe as instrumental to both these purposes. There is a variety in some species of this animal which is, I believe, peculiar to this order; that is, the want of the sense of smelling; none of those which I have yet examined having that sense, except the 2 kinds of whalebone whale: such of course have neither the olfactory nerves, nor the organ; therefore in them the nostrils are intended merely for respiration; but others have the organ placed in this passage as in other animals.

The membranous portion of the posterior nostrils is one canal; but when in the bony part, in most of them, it is divided into 2; the spermaceti whale however is an exception. In those which have it divided, it is in some continued double through the anterior soft parts, opening by 2 orifices, as in the piked whale; but in others it unites again in the membranous part, making externally only one orifice, as in the porpoise, grampus, and bottle-nose. At its beginning in the fauces, it is a roundish hole, surrounded by a strong sphincter muscle, for grasping the epiglottis; beyond this, the canal becomes larger, and opens into the 2 passages into the bones of the head. This part is very glandular, being full of follicles, whose ducts ramify in the surrounding substance, which appears fatty and muscular like the root of the tongue, and these ramifications communicate with each other, and contain a viscid slime. In the spermaceti whale, which has a single canal, it is thrown a little to the left side. After these canals emerge from the bones near the external opening, they become irregular and have several sulci passing out laterally, of irregular forms, with corresponding eminences. The structure of these eminences is muscular and fatty, but less muscular than the tongue of a quadruped.

In the porpoise there are 2 sulci on each side; 2 large and 2 small, with corresponding eminences of different shapes, the large ones being thrown into folds. The spermaceti whale has the least of this structure; the external opening in it comes farther forwards toward the anterior part of the head, and is consequently longer than in others of this order. Near to its opening externally it forms a large sulcus, and on each side of this canal is a cartilage, which runs nearly its whole length. In all that I have examined, this canal, forwards from the bones, is entirely lined with a thick cuticle of a dark colour.

In those which have only 1 external opening, it is transverse, as in the porpoise, grampus, bottle-nose, and spermaceti whale, &c.; where double, they are longitudinal as in the piked whale, and the large whalebone whale. These openings form a passage for the air in respiration to and from the lungs; for it would be impossible for these animals to breathe air through the mouth; indeed I believe the human species alone breathe by the mouth, and in them it is mostly from habit; for in quadrupeds the epiglottis conducts the air into the nose. In the whole of this tribe, the situation of the opening on the upper surface of the head is well adapted for this purpose, being the first part that comes to the surface of the water in the natural progressive motion of the animal; therefore it is to be considered principally as a respiratory organ, and where it contains the organ of smell, that is only secondary.

As the animals of this order do not live in the medium which they inspire, the organs conducting the air to the lungs are in some sort particularly constructed, that the water in which they live may not interfere with the air they breathe. The projecting glottis, which has been described, passes into the posterior nostrils, by which means it crosses the fauces, dividing them into 2 passages. The enlargement at the termination of the glottis, observed in some of them, would seem to be intended to prevent its retraction; but, as it seems confined to the porpoise and grampus, it may perhaps in them answer some other purpose.

The beginning of the posterior nostrils, which answers to the palatum molle in the quadruped, having a sphincter, the glottis is grasped by it, which renders its situation still more secure, and the passages though the head, across the fauces and along the trachea, are rendered one continued canal; this union of glottis and epiglottis with the posterior nostril, making only a kind of joint, admits of motion, and of dilatation and contraction of the fauces in deglutition, from the epiglottis moving more in or out of the posterior nostril. This construction of parts answers a purpose similar to that of the epiglottis in the quadruped; it may be considered as the epiglottis and the arytenoid cartilages joining, to make a tubular or cylindrical epiglottis, instead of a valvular one. The reasons why there should be so peculiar a construction of parts do not at first appear; but we certainly see by it an absolute guard placed on the lungs, that no water should get into them. This tribe being without the projecting tongue of the quadruped, and wanting its extensive motion, and the power of sucking things into the mouth, may probably require the construction between the air and lungs to be more perfect; but how far it is so, I will not pretend to say.

The size of the brain differs much in different genera of this tribe, and likewise in the proportion it bears to the bulk of the animal. In the porpoise I believe it is largest, and perhaps in that respect comes nearest to the human. The size of the cerebellum, in proportion to that of the cerebrum, is smaller in the human subject than in any animal with which I am acquainted. In many quadrupeds, as the horse, cow, &c. the disproportion in size between cerebellum and cerebrum is not great, and in this tribe it is still less; yet not so small as in the bird, &c.

The whole brain in this tribe is compact, the anterior part of the cerebrum not projecting so far forwards as in either the quadruped or in the human subject; neither is the medulla oblongata so prominent, but flat, lying in a kind of hollow made by the 2 lobes of the cerebellum. The brain is composed of cortical and medullary substances, very distinctly marked; the cortical being in colour, like the tubular substance of a kidney; the medullary very white. These substances are nearly in the same proportion as in the human brain. The 2 lateral ventricles are large, and in those that have olfactory nerves are not continued into them as in many quadrupeds; nor do they wind so much outwards as in the human subject, but pass close round the posterior ends of the thalami nervorum opticorum. The thalami themselves are large; the corpora striata small; the crura of the fornix are continued along the windings of the ventricles, much as in the human subject. The plexus choroides is attached to a strong membrane, which covers the thalami nervorum opticorum, and passes through the whole course of the ventricle, much as in the human subject. The substance of the brain is more visibly fibrous than I ever saw it in any other animal, the fibres passing from the ventricles as from a centre to the circumference, which fibrous texture is also continued through the cortical substance. The whole brain in the piked whale weighed 4 lb. 10 oz. The nerves going out from the brain, I believe, are similar to those of the quadruped, except in the want of the olfactory nerves in the genus of the porpoise.

The medulla spinalis is much smaller in proportion to the size of the body than in the human species, but still bears some proportion to the quantity of brain; for in the porpoise, where the brain is largest, the medulla spinalis is largest; yet this did not hold good in the spermaceti whale, the size of the medulla spinalis appearing to be proportionally larger than the brain, which was small when compared to the size of the animal. It has a cortical part in the centre, and terminates about the 25th vertebra, beyond which is the cauda equina, the dura mater going no lower. The nerves which go off from the medulla spinalis are more uniform in size than in the quadruped, there being no such inequality of parts, nor any extremities to be supplied, except the fins. The medulla spinalis is more fibrous in its structure than in other animals; and when an attempt is made to break it longitudinally, it tears with a fibrous appearance, but transversely it breaks irregularly.

The dura mater lines the skull, and forms in some the 3 processes answerable to the divisions of the brain, as in the human subject; but in others, this is bone. Where it covers the medulla spinalis, it differs from all the quadrupeds I am acquainted with, inclosing the medulla closely, and the nerves immediately passing out through it at the lower part, as they do at the upper, so that the cauda equina, as it forms, is on the outside of the dura mater.

As the organs of sense are variously formed in different animals, fitted for the various modes of impression; and as the modes are either increased or varied, according to circumstances which make no part of the sense itself, but which are necessary for the economy of the animal, we find the senses in this tribe varied in their construction, and in some a sense is even wholly wanting. The organs of sense, which appear to be adapted to every mode of life, are those of touch and taste; but those of smell, sight, and hearing, probably require to be varied according to circumstances. Thus smell may be increased by a mode of impregnation, hearing by the vibration of different mediums, and sight by the different powers of refraction of different mediums; therefore, as animals are intended by nature to be differently circumstanced, so are the senses formed.

Of the sense of touch.—The cutis in this tribe appears, in general, particularly well calculated for sensation; the whole surface being covered with villi, which are so many vessels, and we must suppose nerves. Whether this structure is only necessary for acute sensation, or whether it is necessary for common sensation, where the cuticle is thick, and consisting of many layers, I do not know. We may observe, that where it is necessary the sense of touch should be accurate, the villi are usually thick and long, which probably is necessary, because in most parts of the body, where the more acute sensations of touch are required, such parts are covered by a thick cuticle. Of this the ends of our fingers, toes, and the foot of the hoofed animals, are remarkable examples. Whether this sense is more acute in water, I am not certain, but should imagine it is.

Of the sense of taste.—The tongue, which is the organ of taste, is also endowed with the sense of touch. It is also to be considered, in the greatest number of animals, as an instrument for mechanical purposes; but probably less so in this tribe than any other. However, even in these it must have been formed with this view, since, merely as an organ of taste, it would only have required surface, yet is a projecting body endowed with motion. In some it is better adapted for motion than in others; and I should suppose this to be requisite, on account of the difference in the mode of catching the food, and in the act of swallowing. It is most projecting in those with teeth, probably for the better conducting the food, step by step, to the œsophagus; whereas it does not seem so necessary to have such management of the tongue in those which have no teeth, and catch their food by merely opening the mouth, and swimming upon it, or by having their prey carried in by the water. In the porpoise and grampus it is firm in texture, composed of muscle and fat, being pointed and serrated on its edges, like that of the hog.

In the spermaceti whale the tongue was almost like a feather-bed. In the piked whale it was but gently raised, hardly having any lateral edges, and its tip projecting but little, yet, like every other tongue, composed of muscle and fat. The extent between the 2 jaw bones in this whale was very considerable, taking in the whole width of the head or upper jaw, and of course including the whalebone. This extent of surface, between jaw and jaw, having but little projection of tongue, is almost flat from side to side, is extremely elastic when contracted, and throws the inner membrane into a vast number of very small folds, that run parallel to one another, but which are again thrown into a close serpentine course by the elasticity of the part in a contrary direction. From the tongue being capable of but little motion, there is only a small mass of muscle required; and from the thinness of the jaw bones, the distance between the lower surface of the mouth and external surface of the skin is but small; and this skin being ribbed, and very elastic, is capable of considerable distention, by which the cavity of the mouth can be enlarged. The tongue of the large whalebone whale, I should suppose, rose in the mouth considerably; the 2 jaws at the middle being kept at such a distance on account of the whalebone, so that the space between, when the mouth is shut, must be filled up by the tongue.

Of the sense of smelling.—In this tribe of animals there is something very remarkable in what relates to the sense of smelling; nor have I been able to discover the particular mode by which it is performed. When we consider these animals as quadrupeds, and only constructed differently in external form for progressive motion through water, we must see that it was necessary that all the senses should correspond with this medium: we must therefore be at a loss to conceive how they smell, since we may observe that the organ for smelling water, as in fish, is very different from that formed to smell air; and as we must suppose this tribe are only to smell water, being the medium in which such odoriferous particles can be diffused, we should expect their organ to be similar to that of fish; but in that case nature would have been obliged to have attached the nose of a fish to an animal constructed like a quadruped; and it is contrary to the laws which are established in the animal creation to mix parts of different animals together.

In many of this tribe there is no organ of smell at all; and in those which have such an organ, it is not that of a fish, therefore probably not calculated to smell water. It becomes difficult therefore to account for the manner in which such animals smell the water; and why the others should not have had such an organ*, which I believe is peculiar to the large and small whalebone whales. Though it is not the external air which they inspire that produces smell, I believe

* Is the mode of smelling in fish similar to tasting in other animals? Or is the air contained in the water impregnated with the odoriferous parts, and this air the fish smells? If so, it is somewhat

it is the air retained in the nostril out of the current of respiration, which by being impregnated with the odoriferous particles contained in the water during the act of blowing, is applied to the organ of smell. It might be supposed that they could smell the air on the surface of the water by every inspiration, as animals do on land; and probably they may: but this will not give them the power to smell the odoriferous particles of their prey in the water at any depth; and as their organ is not fitted to be affected by the application of water, and as they cannot suck water into the nostril, without the danger of its passing into the lungs, it cannot be by its application to this organ that they are enabled to smell.

Some have the power of throwing the water from the mouth through the nostril, and with such force as to raise it 30 feet high: this must answer some important purpose, though not immediately evident to us. As the organ appears to be formed to smell air only, and as I conceive the smelling of the external air could not be of use as a sense, I therefore believe that they do not smell in inspiration; yet let us consider how they may be supposed to smell the odoriferous particles of the water. The organ of smell is out of the direct road of the current of air in inspiration; it is also out of the current of water when they spout; may we not suppose then, that this sinus contains air, and as the water passes in the act of throwing it out, that it impregnates this reservoir of air, which immediately affects the sense of smell? This operation is probably performed in the time of expiration, because it is said that this water is sometimes very offensive; but all this I only give as conjecture. If the above solution is just, then only those which have the organ of smell can spout, a fact worthy of inquiry. The organ of smell would appear to be less necessary in these animals than in those which live in air, since some are wholly deprived of it; and the organ in those which have it is extremely small, when compared with that of other animals; as well as the nerve which is to receive the impression, as was observed above.

Of the sense of hearing.—The ear is constructed much on the same principles as in the quadruped; but as it differs in several respects, which it is necessary to particularize, to convey a perfect idea of it the whole should be described. As this would exceed the limits of this paper, I shall content myself with a general description, taking notice of those material points in which it differs from that of the quadruped. This organ consists of the same parts as in the quadruped; an external opening, with a membrana tympani, an eustachian tube, a tympanum with its processes, and the small bones. There is no external projection forming a funnel, but merely an external opening. We can

similar to the breathing of fish, it not being the water which produces the effect there, but the air ontained in it. This I proved by experiments, and is mentioned by Dr. Priestley.—Orig.

easily assign a reason why there should be no projecting ear, as it would interfere with progressive motion; but the reason why it is not formed as in birds, is not so evident; whether the percussions of water could be collected into one point as air, I cannot say. The tympanum is constructed with irregularities, so much like those of an external ear, that I could suppose it to have a similar effect.

The external opening begins by a small hole, scarcely perceptible, situated on the side of the head a little behind the eye. It is much longer than in other animals, in consequence of the size of the head being so much increased beyond the cavity that contains the brain. It passes in a serpentine course, at first horizontally, then downwards, and afterwards horizontally again, to the membrana tympani, where it terminates. In its whole length it is composed of different cartilages, which are irregular and united together by cellular membrane, so as to admit of motion, and probably of lengthening or shortening, as the animal is more or less fat. The bony part of the organ is not so much inclosed in the bones of the skull as in the quadruped, consisting commonly of a distinct bone or bones, closely attached to the skull, but in general readily to be separated from it; yet in some it sends off, from the posterior part, processes which unite with the skull. It varies in its shape, and is composed of the immediate organ and the tympanum.

The immediate organ is, in point of situation to that of the tympanum, superior and internal, as in the quadruped. The tympanum is open at the anterior end, where the eustachian tube begins. The eustachian tube opens on the outside of the upper part of the fauces: in some higher in the nose than others; highest I believe in the porpoise. From the cavity of the tympanum, where it is rather largest, it passes forwards and inwards, and near its termination appears very much fasciculated, as if glandular. The eustachian tube and tympanum communicate with several sinuses, which passing in various directions surround the bone of the ear. Some of these are cellular, similar to the cells of the mastoid process in the human subject, though not bony. There is a portion of this cellular structure of a particular kind, being white, ligamentous, and each part rather rounded than having flat sides*. One of the sinuses passing out of the tympanum close to the membrana tympani, goes a little way in the same direction, and communicates with a number of cells. The whole function of the eustachian tube is perhaps not known; but it is evidently a duct from the cavity of the ear, or a passage for the mucus of these parts;

* These communications with the eustachian tube may be compared to a large bag on the bases of the skull of the horse and ass, which is a lateral swell of the membranous part of the tube, and when distended will contain nearly a quart.—Orig.

the external opening having a particular form would incline us to believe that something was conveyed to the tympanum.

The bony part of the organ is very hard and brittle, rendering it even difficult to be cut with a saw, without its chipping into pieces. That part which contains the immediate organ is by much the hardest, and has a very small portion of animal substance in it; for when steeped in an acid, what remains is very soft, almost like a jelly, and laminated. The bone is not only harder in its substance, but there is on the whole more solid bone than in the corresponding parts of quadrupeds, it being thick and massy. The part containing the tympanum is a thin bone, coiled on itself, attached by one end to the portion which contains the organ; and this attachment in some is by close contact only, as in the narwhale; in others the bones run into one another, as in the bottle-nose and piked whales. The concave side of the tympanum is turned towards the organ, its 2 edges being close to it; the outer is irregular, and in many only in contact, as in the porpoise: while in others the union is by bony continuity, as in the bottle-nose whale, leaving a passage on which the membrana tympani is stretched, and another opening which is the communication with the sinuses.

The surface of the bone containing the immediate organ opposite to the mouth of the tympanum is very irregular, having a number of eminences and cavities. The cavity of the tympanum is lined with a membrane, which also covers the small bones with their muscles, and appears to have a thin cuticle. This membrane renders the bones, muscles, tendons, &c. very obscure, which are seen distinctly when that is removed. It appears to be a continuation of the periosteum, and the only uniting substance between the small bones. Besides the general lining, there is a plexus of vessels, which is thin and rather broad, and attached by one edge, the rest being loose in the cavity of the tympanum, somewhat like the plexus choroides in the ventricles of the brain. The cavity we may suppose intended to increase sound, probably by the vibration of the bone; and from its particular formation we can easily conceive that the vibrations are conducted, or reflected, towards the immediate organ, it being in some degree a substitute for the external ear.

The external opening being smaller than in any animals of the same size, the membrana tympani is nearly in the same proportion. In the bottle-nose whale, the grampus, and porpoise, it is smooth and concave externally, but of a particular construction on the inner surface; for a tendinous process passes from it towards the malleus, converging as it proceeds from the membrane, and becoming thinner till its insertion into that bone. I could not discover whether it had any muscular fibres which could affect the action of the malleus. In the piked whale, the termination of the external opening, instead of being smooth and concave, is projecting, and returns back into the meatus for above an inch in length, is firm in texture, with thick coats, is hollow on its inside, and its mouth communicating with the tympanum; one side being fixed to the malleus similar to the tendinous process which goes from the inside of the membrana tympani in the others.

A little way within the membrana tympani, are placed the small bones, which are 3 in number, as in the quadruped, malleus, incus, and stapes; but in the bottle-nose whale there is a 4th, placed on the tendon of the stapedæus muscle. These bones are as it were suspended between the bone of the tympanum and that of the immediate organ. The malleus has 2 attachments, besides that with the incus; one close to the bone of the tympanum, which in the porpoise is only by contact, but in others by a bony union; the other attachment is formed by the tendon, above described, being united to the inner surface of the membrana tympani. Its base articulates with the incus. The incus is attached by a small process to the tympanum, and is suspended between the malleus and stapes. The process by which it articulates with the stapes is bent towards that bone. The stapes stands on the vestibulum, by a broad oval base. In many of this tribe, the opening from side to side of the stapes is so small as hardly to give the idea of a stirrup.

The muscles which move these bones are 2 in number, and tolerably strong. One arises from that projecting part of the tympanum which goes to form the eustachian tube, and running backwards is inserted into a small depression on the anterior part of the malleus. The use of this muscle seems to be to tighten the membrana tympani; but in those which have the malleus anchylosed with the tympanum, we can hardly conjecture its use. The other has its origin from the inner surface of the tympanum, and passing backwards is inserted into the stapes by a tendon, in which I found a bone in the large bottle-nose. This muscle gives the stapes a lateral motion. What particular use in hearing may be produced by the action of these muscles, I will not pretend to say; but we must suppose whatever motion is given to the bones must terminate in the movement of the stapes.

The immediate organ of hearing is contained in a round, bony process, and consists of the cochlea and semicircular canals, which somewhat resemble the quadruped; but, besides the 2 spiral turns of the cochlea, there is a 3d, which makes a ridge within that continued from the foramen rotundum, and follows the turns of the canal. The cochlea is much larger, when compared with the semicircular canals, than in the human species and quadruped.

We may reckon 2 passages into the immediate organ of hearing, the foramen rotundum, and foramen ovale. They are at a greater distance than in the quadruped. The foramen rotundum is placed much more on the outer surface of the bone, and not in the cavity of the bony tympanum; but may be said to communicate with the surrounding cellular part of the tympanum. The foramen rotundum, which is the beginning of one of these turns, appears to be only one end of a transverse groove, which is afterwards closed in the middle, forming a canal with the 2 ends open; so that this foramen appears to have 2 beginnings; but the other opening is probably only a passage for blood-vessels going to the cochlea. From this foramen begins the inner turn of the cochlea, which is the largest, especially at its beginning; the other begins from the vestibulum. The cochlea is a spiral canal coiled within itself, and divided into 2 by a thin spiral bony plate, which is completed in the recent subject, and forms 2 perfect canals. In the recent subject, the foramen rotundum is lined with the membrane of the tympanum, which terminates in a blind end, forming a kind of membrana cochleæ. The other opening, in the recent subject, communicates with the spiral turn, beyond the membranous termination of the foramen rotundum.

The foramen ovale has a little projection inwards all round, on which the stapes stands: within this is the vestibulum, which is common to the other spiral turn of the cochleæ, and the semicircular canals; this canal of the cochlea passes out first in a direction contrary to its general course, but soon makes a turn into the spiral. It is round, and not merely a division of the cochlea into 2 by a septum, but has a membrane of its own, which is attached to the thin bony plate, and lines that part of the cochlea in such a manner as to retain its structure when the bone is removed. The cochlea in some completes one turn and a half; in others, more. It is not a spiral on a plane, or cylinder, but on a cone.

I have already observed, that by looking in at the foramen rotundum, we see 2 small ridges; the uppermost is the swell of the canal from the vestibulum just described; the lower ridge, which is also a canal, may be observed just to pass along the foramen belonging to this canal, close to the septum between the 2; a circumstance I believe peculiar to this tribe. Its beginning is close to the vestibulum, but does not open from it, and passes along the first described spiral turn to its apex: when opened it appears to be a canal full of small perforations, probably the passages of the branches from the auditory nerve. This bony process has several perforations in it; one of them large, for the passage of the 7th pair of nerves. The size of the portio mollis, before its entrance into the organ, is very large, and bears no proportion to that which enters. The passage for this nerve is very wide, and seems to have an irregular blind conical, and somewhat spiral, termination; its being spiral arises from the closeness to the point of the cochlea. In the terminating part there are a number of perforations into the cochlea, and one into the semicircular canals, which afford a passage to the different divisions of the auditory nerve. There is a considerable foramen in its anterior side near the bottom, for the passage of the portio dura, and which is continued backward to the cavity of the tympanum near the stapes, and emerges near the posterior and upper part of this bone.

Of the organ of seeing.—The eye in this tribe of animals is constructed on nearly the same principle as that of quadrupeds, differing however in some circumstances; by which it is probably better adapted to see in the medium through which the light is to pass. It is on the whole small for the size of the animal, which would lead to the supposition, that their locomotion is not great; for I believe animals that swim are in this respect similar to those that fly; and as this tribe come to the surface of the medium in which they live, they may be considered in the same view with birds which soar; and we find birds that fly to great heights, and move through a considerable space, in search of food, have their eyes larger in proportion to their size.

The eyelids have but little motion, and do not consist of loose cellular membrane, as in quadrupeds, but rather of the common adipose membrane of the body; the connection however of their circumference with the common integuments is loose, the cellular membrane being less loaded with oil, which allows of a slight fold being made on the surrounding parts in opening the eyelids. This is not to an equal degree in them all, being less so in the porpoise than in the piked whale. The tunica conjunctiva, where it is reflected from the eyelid to the eyeball, is perforated all round by small orifices of the ducts of a circle of glandular bodies lying behind it. The lachrymal gland is small; its use being supplied by those above-mentioned; and the secretion from them all I believe to be a mucus similar to what is found in the turtle and crocodile. There are neither puncta nor lachrymal duct, so that the secretion, whatever it be, is washed off into the water.

The muscles which open the eyelids are very strong: they take their origin from the head, round the optic nerve, which in some requires their being very long, and are so broad as almost to make one circular muscle round the whole of the interior straight muscles of the eye itself. They may be divided into 4; a superior, an inferior, and one at each angle: as they pass outwards to the eyelids, they diverge and become broader, and are inserted into the inside of the eyelids almost equally all round. They may be termed the dilatores of the eyelids; and, before they reach their insertion, give off the external straight muscles, which are small, and inserted into the sclerotic coat before the transverse axis of the eye: these may be named the elevator, depressor, adductor, and abductor, and may be dissected away from the others as distinct muscles. Besides these 4 going from the muscles of the eyelid to the eye itself, there are 2 which are larger, and inclose the optic nerve with the plexus. As these pass outwards they become broad, may in some be divided into 4, and are inserted into the sclerotic coat, almost all round the eye, rather behind its transverse axis.

The two oblique muscles are very long; they pass through the muscles of the eyelids, are continued on to the globe of the eye, between the 2 sets of straight muscles, and at their insertions are very broad; a circumstance which gives great variation to the motion of the eye. The sclerotic coat gives shape to the eye, both externally and internally, as in other animals; but the external shape and that of the internal cavity are very dissimilar, arising from the great difference in the thickness of this coat in different parts. The external figure is round, except that it is a little flattened forwards; but that of the cavity is far otherwise, being made up of sections of various circles, being a little lengthened from the inner side to the outer, a transverse section making a short ellipsis. In the piked whale the long axis is 2 and $\frac{3}{4}$ inches, the short axis 2 and $\frac{1}{8}$ inches.

The posterior part of the cavity is a tolerably regular curve, answering to the difference in the 2 axes; but forwards, near the cornea, the sclerotic coat turns quickly in, to meet the cornea, which makes this part of the cavity extremely flat, and renders the distance between the anterior part of the sclerotic coat and the bottom of the eye not above an inch and a quarter. In the piked whale the sclerotic coat, at its posterior part, is very thick: near the extreme of the short axis it was $\frac{1}{2}$ an inch, and at the long axis $\frac{1}{8}$th of an inch thick. In the bottle-nose whale, the extreme of the short axis was $\frac{1}{2}$ an inch thick, and the extremes of the long axis about a $\frac{1}{4}$ of an inch, or half the other. The sclerotic coat becomes thinner as it approaches to its union with the cornea, where it is thin and soft. It is extremely firm in its texture where thick, and from a transverse section would seem to be composed of tendinous fibres, intermixed with something like cartilage; in this section 4 passages for vessels remain open. This firmness of texture precludes all effect of the straight muscles on the globe of the eye, by altering its shape, and adapting its focus to different distances of objects, as has been supposed to be the case in the human eye. The cornea makes rather a longer ellipsis than the ball of the eye; the sides of which are not equally curved, the upper being most considerably so. It is a segment of a circle somewhat smaller than that of the eyeball, is soft and very flaccid. The tunica choroides resembles that of the quadruped; and its inner surface is of a silver hue, without any nigrum pigmentum. The nigrum pigmentum only covers the ciliary processes, and lines the inside of the iris. The retina appears to be nearly similar to that of the quadruped. The arteries going to the coats of the eye form a plexus passing round the optic nerve, resembling in its appearance that of the spermatic artery in the bull and some other animals.

The crystalline humor resembles that of the quadruped; but whether it is very convex or flattened, I cannot determine, those I have examined having been kept too long to preserve their exact shape and size. The vitreous humor adhered to the retina at the entrance of the optic nerve. The optic nerve is very long in some species, owing to the vast width of the head. I shall not at present consider the eye in animals of this tribe, as it respects the power of vision, that being performed on a general principle common to every animal inhabiting the water; more especially as I am only master of the construction and formation of the eye, and not of the size, shape, and densities of the humors; yet from reasoning we must suppose them to correspond with the shape of the eye, and the medium through which the light is to pass.

Of the parts of generation.—The parts of generation in both sexes of this order of animals come nearer in form to those of the ruminating than of any others; and this similarity is perhaps more remarkable in the female than in the male; for their situation in the male must vary on account of external form, as was before observed. The testicles retain the situation in which they were formed, as in those quadrupeds in which they never come down into the scrotum. They are situated near the lower part of the abdomen, one on each side, on the 2 great depressors of the tail. At this part of the abdomen, the testicles come in contact with the abdominal muscles anteriorly. The vasa deferentia pass directly from the epididymis behind the bladder, or between it and the rectum, into the urethra; and there are no bags similar to those called vesiculæ seminales in certain other animals.

The structure of the penis is nearly the same in them all, and formed much on the principle of the quadruped. It is made up of 2 crura, uniting into one corpus cavernosum, and the corpus spongiosum seems first to enter the corpus cavernosum. In the porpoise at least the urethra is found nearly in the centre of the corpus cavernosum; but towards the glans seems to separate or emerge from it, and becoming a distinct spongy body, runs along its under surface, as in quadrupeds. The corpus cavernosum in some is broader from the upper part to the lower than from side to side; but in the porpoise has the appearance of being round, becoming smaller forwards, so as to terminate almost in a point some distance from the end of the penis. The glans does not spread out as in many quadrupeds, but seems to be merely a plexus of veins covering the anterior end of the penis, yet is extended a good way farther on, and is in some no more than one vein deep.

The cura penis are attached to 2 bones, which are nearly in the same situation, and in the same part of the pelvis, as those to which the penis is attached in quadrupeds; but these bones are only for the insertion of the crura, and not for the support of any other part, like the pelvis in those animals which have posterior extremities, neither do they meet at the fore part, or join the vertebræ of the back. The erectores penis are very strong muscles, having an origin and insertion similar to those of the human subject. The acceleratores muscles are also very strong; and there is a strong and long muscle, arising from the anus, and passing forwards to the bulb of the penis, that runs along the under surface of the urethra, and is at last lost or inserted in the corpus spongiosum. This muscle draws the penis into the prepuce, and throws that part of the penis that is behind its insertion into a serpentine form. It is common to most animals that draw back the penis into what is called the sheath, and may be called the retractor penis.

In all the females that I have examined, the parts of generation are very uniformly the same; consisting of the external opening, the vagina, the 2 horns of the uterus, Fallopian tubes, fimbriæ, and ovaria. The external opening is a longitudinal slit, or oblong opening, whose edges meet in 2 opposite points, and the sides are rounded off, so as to form a kind of sulcus. The skin and parts on each side of this sulcus are of a looser texture than on the common surface of the animal, not being loaded with oil, and allowing of such motion of one part on another as admits of dilatation and contraction. The vagina passes upwards and backwards towards the loins, so that its direction is diagonal respecting the cavity of the abdomen, and then divides into the 2 horns, one on each side of the loins; these afterwards terminating in the Fallopian tubes, to which the ovaria are attached. From each ovarium there is a small fold of the peritoneum, which passes up towards the kidney of the same side, as in most quadrupeds.

The inside of the vagina is smooth for about $\frac{1}{2}$ of its length, and then begins to form something similar to valves projecting towards the mouth of the vagina, each like an os tincæ; these are about 6, 7, 8, or 9 in number. Where they begin to form, they hardly go quite round, but the last are complete circles. At this part too the vagina becomes smaller, and gradually decreases in width to its termination. From the last projecting part, the passage is continued up to the opening of the 2 horns, and the inner surface of this last part is thrown into longitudinal rugæ, which are continued into the horns. Whether this last part is to be reckoned common uterus or vagina, and that the last valvular part is to be considered as os tincæ, I do not know; but from its having the longitudinal rugæ, I am inclined to think it is uterus, this structure appearing to be intended for distinction. The horns are an equal division of this part; they make a gentle turn outwards, and are of considerable length. Their inner surface is thrown into longitudinal rugæ, without any small protuberances for the cotyledons to form on, as in those of ruminating animals; and where they terminate the Fallopian tubes begin.

In the bottle-nose whale, where the Fallopian tubes opened into the horns of the uterus, they were surrounded by pendulous bodies hanging loose in the horns. The Fallopian tubes, at their termination in the uterus, are remarkably small for some inches, and then begin to dilate rather suddenly; and the nearer to the mouth the more this dilatation increases, like the mouth of a French horn, the termination of which is 5 or 6 inches in diameter. They are very full of longitudinal rugæ through their whole length. The ovaria are oblong bodies, about 5 inches in length; one end attached to the mouth of the Fallopian tube, and the other near to the horn of the uterus. They are irregular on their external surface, resembling a capsula renalis or pancreas. They have no capsula, but what is formed by the long Fallopian tube.

How the male and female copulate, I do not know; but it is alleged that their position in the water is erect at that time, which I can readily suppose may be true; for otherwise, if the connection is long, it would interfere with the act of respiration, as in any other position the upper surface of the heads of both could not be at the surface of the water at the same time. However, as in the parts of generation they most resemble those of the ruminating kind, it is possible they may likewise resemble them in the duration of the act of copulation; for I believe all the ruminants are quick in this act. Of their uterine gestation, I as yet know nothing; but it is very probable that they have only a single young one at a time, there being only 2 nipples. This seemed to be the case with the bottle-nose whale caught near Berkeley, which had been seen for some days with a young one following it, and they were both caught together.

The glands for the secretion of milk are 2; one on each side of the middle line of the belly at its lower part. The posterior ends, from which go out the nipples, are on each side of the opening of the vagina in small sulci. They are flat bodies lying between the external layer of fat and abdominal muscles, and are of considerable length, but only $\frac{1}{4}$ of that in breadth. They are thin, that they may not vary the external shape of the animal, and have a principal duct running in the middle through the whole length of the gland, and collecting the smaller lateral ducts, which are made up of those still smaller. Some of these lateral branches enter the common trunk in the direction of the milk's passage, others in the contrary direction, especially those nearest to the termination of the trunk in the nipple. The trunk is large, and appears to serve as a reservoir for the milk, and terminates externally in a projection, which is the nipple. The lateral portions of the sulcus which incloses the nipple, are composed of parts looser in texture than the common adipose membrane, which is probably to admit of the elongation or projection of the nipple. On the outside of this there is another small fissure, which I imagine is likewise intended to give greater facility to the movements of all these parts.

The milk is probably very rich; for in that caught near Berkeley with its young one, the milk, which was tasted by Mr. Jenner, and Mr. Ludlow, Surgeon at Sodbury, was rich like cow's milk to which cream had been added.

The mode in which these animals must suck would appear to be very inconvenient for respiration, as either the mother or young one will be prevented from breathing at the time, their nostrils being in opposite directions, therefore the nose of one must be under water, and the time of sucking can only be between each respiration. The act of sucking must likewise be different from that of land animals; as in them it is performed by the lungs drawing the air from the mouth backwards into themselves, which the fluid follows, by being forced into the mouth from the pressure of the external air on its surface; but in this tribe, the lungs having no connection with the mouth, sucking must be performed by some action of the mouth itself, and by its having the power of expansion.

Explanation of the Plates.—Pl. 5, fig. 1. This fish, called a grampus, was 24 feet long: it was caught in the mouth of the river Thames, in the year 1759, and brought up to Westminster Bridge in a barge.

Fig. 2, another species of grampus, 18 feet long, which was caught in the river Thames, 15 years ago.

Fig. 3, a species of bottle-nose whale; the Delphinus Delphis of Linnæus. It was caught on the sea-coast, near Berkeley, where it had been seen for several days, following its mother, and was taken along with the old one, and sent up to me whole, for examination, by Mr. Jenner, surgeon, at Berkeley. The old one was 11 feet long. Fig. 6, the head of the same whale as fig. 3, to show the shape of the blow-hole, which is transverse, and almost semi-circular.

Fig. 4, the bottle-nose whale described by Dale, and 21 feet long. It is similar to that of fig. 3, in its general form, but has only 2 small pointed teeth in the fore part of the upper jaw, and is rather lighter coloured on the belly. It was caught above London Bridge in the year 1783, and became the property of the late Mr. Alderman Pugh, who very politely allowed me to examine its structure, and to take away the bones.

Fig. 5, the balæna rostrata of Fabricius, or piked whale. It was caught on the Dogger-Bank. It had met with some accident between the 2 lower jaws under the tongue, in which part a considerable collection of air had taken place, so as to raise up the tongue and its attachments into a round body in the mouth, projecting even beyond the jaws. This rendered the head specifically lighter than the water, so that it could not sink, and therefore was easily caught. It was 17 feet long, and was brought to St. George's Fields, where I purchased it. The dorsal fin having been cut off close to the back, is therefore only marked by a dotted line.

Fig. 7, includes the external parts of generation, with the relative situation of the anus and the nipples, of the balæna rostrata. A A, the labia pudendi spread open, exposing the meatus urinarius, vagina, and anus, which in a natural state are all concealed, there only appearing a long slit, of 15 inches length, the 2 edges of which are in contact. B, the clitoris; C, the meatus urinarius; D, the opening of the vagina; E, the anus; F, the sulcus, in which the left nipple lies, spread open, and the nipple itself exposed to view; G, the sulcus of the right nipple, in a natural state, only appearing like a line; H, a sulcus near to the nipple, which is spread open to show the inside. This sulcus I conceive gives a freedom to the motion of the skin of these parts, so as to allow the nipple to be more freely exposed; I, the same sulcus on the opposite side, closed up.

Fig. 8, a side view of one of the plates of whalebone of the balæna rostrata. A, The part of the plate which projects beyond the gum; B, the portion which is sunk into the gum; CC, a white substance, which surrounds the whalebone, forming there a projecting head, and also passing between the plates, to form their external lamellæ; DD, the part analogous to the gum; E, a fleshy substance, covering the jaw-bone, and on which the inner lamella of the plate is formed; F, the termination of the plate of whalebone in a kind of hair.

Fig. 9, a perpendicular section of several plates of whalebone in their natural situation in the gum; their inner edges, or shortest terminations, are removed, and the cut edges of the plates seen from the inside of the mouth. The upper part shows the rough surface formed by the hairy termination of each plate of whalebone. The middle part shows the distance the plates of whalebone are from each other. The lower part shows the white substance in which they grow, and also the basis on which they stand.

Fig. 10, an outline considerably magnified, to show the mode of growth of the plates, and of the white intermediate substance. A, the middle layer of the plate, which is formed on a pulp or cone that passes up in the centre of the plate. The termination of this layer forms the hair; B, one of the outer layers, which grows, or is formed, from the intermediate white substance. CCCC, the intermediate white substance, laminæ of which are continued along the middle layer, and form the substance of the plate of whalebone; D, the outline of another plate of whalebone; E, the basis on which the plates are formed, which adheres to the jaw-bone.

XXXIX. *Some Observations on Ancient Inks, with the Proposal of a New Method of Recovering the Legibility of Decayed Writings. By Charles Blagden, M.D., Sec. R.S. and F.A.S.* p. 451.

In a conversation some time ago with Thomas Astle, Esq., relative to the legibility of ancient MSS. a question arose, whether the inks in use 8 or 10 centuries ago, and which are often found to have preserved their colour remarkably well, were made of different materials from those employed in later times, of which many are already become so pale as scarcely to be read. With a view to the decision of this question, Mr. Astle obligingly furnished several MSS. on parchment and vellum, from the 9th to the 15th centuries inclusively; some of which were still very black, and others of different shades of colour, from a deep yellowish brown to a very pale yellow, in some parts so faint as to be scarcely visible. On all of these I made experiments with the chemical re-agents which appeared best adapted to the purpose; namely, alkalis both simple and phlogisticated, the mineral acids, and infusion of galls.

It would be tedious and superfluous to enter into a detail of the particular experiments; as all of them, one instance only excepted, agreed in the general result, to show, that the ink employed anciently, as far as the above-mentioned MSS. extended, was of the same nature as the present: for the letters turned of a reddish or yellowish brown with alkalis, became pale, and were at length obliterated, with the dilute mineral acids, and the drop of acid liquor which had extracted a letter, changed to a deep blue or green on the addition of a drop of phlogisticated alkali; the letters also acquired a deeper tinge with the infusion of galls, in some cases more, in others less. Hence it is evident that one of the ingredients was iron, which there is no reason to doubt was joined with the vitriolic acid; and the colour of the more perfect MSS. which in some was a deep black, and in others a purplish black, together with the restitution of that colour, in those which had lost it, by the infusion of galls, sufficiently proved that another of the ingredients was astringent matter, which from history appears to have been that of galls. No trace of a black pigment of any sort was discovered, the drop of acid, which had completely extracted a letter, appearing of a uniform pale ferruginous colour, without an atom of black powder, or other extraneous matter, floating in it.

As to the greater durability of the more ancient inks, it seemed, from what occurred in these experiments, to depend very much on a better preparation of the material on which the writing was made, namely, the parchment or vellum; the blackest letters being generally those which had sunk into it the deepest. Some degree of effervescence was commonly to be perceived when the acids came in contact with the surface of these old vellums. I was led however to suspect that the ancient inks contained a rather less proportion of iron than the more modern; for in general the tinge of colour, produced by the phlogisticated alkali in the acid laid on them, seemed less deep; which however might depend in part on the length of time they had been kept: and perhaps more gum was used in them, or possibly they were washed over with some kind of varnish, though not such as gave any gloss.

One of the specimens sent by Mr. Astle proved very different from the rest. It was said to be a MS. of the 15th century; and the letters were those of a full engrossing hand, angular, without any fine strokes, broad, and very black. On this none of the above-mentioned re-agents produced any considerable effect; most of them rather seemed to make the letters blacker, probably by cleaning the surface; and the acids, after having been rubbed strongly on the letters, did not strike any deeper tinge with the phlogisticated alkali. Nothing had a sensible effect toward obliterating these letters but what took off part of the surface of the vellum; when small rolls, as of a dirty matter, were to be perceived. It is therefore unquestionable, that no iron was used in this ink; and from its resistance to the chemical solvents, as well as a certain clotted appearance in the letters when examined closely, and in some places a slight degree of gloss, I have little doubt but they were formed with a composition of a black sooty or carbonaceous powder and oil, probably something like our present printer's ink, and am not without suspicion that they were actually printed.*

While I was considering of the experiments to be made, in order to ascertain the composition of ancient inks, it occurred, that perhaps one of the best methods of restoring legibility to decayed writing might be, to join phlogisticated alkali with the remaining calx of iron; because, as the quantity of precipitate

* A subsequent examination of a larger portion of this supposed MS. has shown that it is really part of a very ancient printed book.—Orig.

formed by these two substances very much exceeds that of the iron alone, the bulk of colouring matter would thus be greatly augmented. M. Bergman was of opinion, that the blue precipitate contains only between a 5th and a 6th part of its weight of iron; and though subsequent experiments tend to show that, in some cases at least, the proportion of iron is much greater, yet on the whole it is certainly true, that if the iron left by the stroke of a pen were joined to the colouring matter of phlogisticated alkali, the quantity of Prussian blue thence resulting would be much greater than the quantity of black matter originally contained in the ink deposited by the pen; though perhaps the body of colour might not be equally augmented. To bring this idea to the test, I made a few experiments as follows.

The phlogisticated alkali was rubbed on the bare writing, in different quantities; but in general with little effect. In a few instances however it gave a bluish tinge to the letters, and increased their intensity, probably where something of an acid nature had contributed to the diminution of their colour.

Reflecting that when the phlogisticated alkali forms its blue precipitate with iron, the metal is usually first dissolved in an acid, I was next induced to try the effect of adding a dilute mineral acid to writing, besides the alkali. This answered fully to my expectations; the letters changing very speedily to a deep blue colour, of great beauty and intensity. It seems of little consequence as to the strength of colour obtained, whether the writing be first wetted with the acid, and then the phlogisticated alkali be touched on it, or whether the process be inverted, beginning with the alkali; but on another account I think the latter way preferable. For the principal inconvenience which occurs in the proposed method of restoring MSS. is, that the colour frequently spreads, and so much blots the parchment as to detract greatly from the legibility; now this appears to happen in a less degree when the alkali is put on first, and the dilute acid is added on it. The method I have hitherto found to answer best has been, to spread the alkali thin with a feather over the traces of the letters, and then to touch it gently, as nearly on or over the letters as can be done, with the diluted acid, by means of a feather, or a bit of stick cut to a blunt point. Though the alkali has occasioned no sensible change of colour, yet the moment that the acid comes on it, every trace of a letter turns at once to a fine blue,* which

* The phlogisticated alkali (which is to be considered simply as a name) appears to consist of a peculiar acid, in the present extensive acceptation of that term, joined to the alkali. Now the theory of the above-mentioned process I take to be, that the mineral acid, by its stronger attraction for the alkali, dislodges the colouring (Prussian) acid, which then immediately seizes on the calx of iron, and converts it into Prussian blue, without moving it from its place. But if the mineral acid be put on the writing first, the calx of iron is partly dissolved and diffused by that liquor before the Prussian acid combines with it; whence the edges of the letters are rendered more indistinct, and the parchment is more tinged. The sudden evolution of so fine a colour, on the mere traces of letters, affords an amusing spectacle.—Orig.

soon acquires its full intensity, and is beyond comparison stronger than the colour of the original trace had been. If now the corner of a bit of blotting paper be carefully and dexterously applied near the letters, so as to suck up the superfluous liquor, the staining of the parchment may be in great measure avoided: for it is this superfluous liquor which, absorbing part of the colouring matter from the letters, becomes a dye to whatever it touches. Care must be taken not to bring the blotting paper in contact with the letters, because the colouring matter is soft while wet, and may be easily rubbed off. The acid I have chiefly employed has been the marine; but both the vitriolic and nitrous succeed very well. They should be so far diluted as not to be in danger of corroding the parchment, after which the degree of strength does not seem to be a matter of much nicety.

The method now commonly practised to restore old writings, is by wetting them with an infusion of galls in white wine. This certainly has a great effect; but it is subject, in some degree, to the same inconvenience as the phlogisticated alkali, of staining the substance on which the writing was made. Perhaps if, instead of galls themselves, the peculiar acid or other matter which strikes the black with iron, were separated from the simple astringent matter, for which purpose 2 different processes are given by Piepenbring and by Scheele, this inconvenience might be avoided. It is not improbable also that a phlogisticated alkali might be prepared, better suited to this object than the common; as by rendering it as free as possible from iron, diluting it to a certain degree, or substituting the volatile alkali for the fixed. Experiment would most likely point out many other means of improving the process described above; but in its present state I hope it may be of some use, as it not only brings out a prodigious body of colour on letters which were before so pale as to be almost invisible, but has the further advantages over the infusion of galls, that it produces its effect immediately, and can be confined to those letters only for which such assistance is wanted.

END OF THE SEVENTY-SEVENTH VOLUME OF THE ORIGINAL.

I. Of the Methods of Manifesting the Presence, and Ascertaining the Quality, of Small Quantities of Natural or Artificial Electricity. By Mr. Tib. Cavallo, F. R. S. Being the Lecture founded by the late Henry Baker, Esq. F. R. S. Anno 1788, *Vol.* 78, p. 1.

After some appropriate introductory observations, Mr. C. says that a great deal still remains to be done, before we can attain to the knowledge of the object in view, viz. of the real nature, of the first origin, and of the general use of electricity. The instruments hitherto invented are still inadequate to the purpose, and the known methods of operating are not free from considerable objections. To examine the peculiar constructions, intended uses, properties, and defects, of those instruments, as well as methods of performing the experiments, is the principal object of the present lecture. The late Mr. John Canton first constructed an electrometer, or instrument capable of showing the presence of what was then considered as a small quantity of electricity. This instrument consisted of 2 small balls of pith of elder or of cork, fastened to the 2 extremities of a linen thread, the middle of which was fastened to an oblong wooden box, in which the thread and balls were kept, when not actually in use. It was with such an instrument that Mr. Canton himself, Father Beccaria, and others, ascertained the electricity generally existing in the air; and that Mr. Ronayne discovered the constant electricity of fogs. But in the course of my experiments, having made frequent use of such electrometers, it naturally occurred to me, that in several cases, when the electrometer gave no signs of electricity, or at least not sufficient to ascertain its quality, the cause of it was the relatively large size of the instrument; for a small quantity of electricity being diffused through the box, thread, and balls of the electrometer, had not power sufficient to separate the balls, and of course to show its presence. In consequence of this, I contracted the size of the electrometers to such a degree as could be affected by less than the 10th part of that quantity of electricity which was necessary to affect Mr. Canton's electrometer. But in making the electrometers very short, the stiffness of the threads, which had been insignificant in a great extent, became now very considerable; hence, instead of fastening the balls to the 2 extremities of 1 piece of thread, I found it necessary to suspend each ball by a separate piece of thread, the upper part of which was formed into a loop, which moved in a ring of brass wire.

The electrometers, thus improved, were still subject to a great imperfection, which was the twisting of the threads; to avoid which I substituted fine silver wire, instead of linen threads, which answered very well. However, in observing the electricity of the atmosphere, these electrometers appeared to labour under a considerable inconvenience, which was their being disturbed by the wind. To remove this imperfection, I inclosed the electrometer in a bottle, which construction has been found to answer remarkably well. This bottle electrometer has been since altered by various persons; though those alterations do not tend to improve it altogether. M. de Saussure, by altering the shape of the bottle, and depriving it of a neck, has rendered it capable of retaining the communicated electricity only for a very short time; whereas, some of those electrometers, constructed on my original plan, have retained the communicated electricity for more than 4 hours. Besides other alterations for the worse.

Another alteration of the bottle electrometer was lately made by the Rev. Mr. Bennet, and is described in the Philos. Trans. vol. 77. It consists principally in substituting 2 slips of gold-leaf for the corks suspended by wires. This alteration has some peculiar advantages and disadvantages. Its advantages are in general a greater degree of sensibility, and a more easy construction. Its disadvantages are, first, that the instrument is not portable; and, 2dly, that even when not carried about, it is apt to be spoiled very easily. However, in some cases it is very useful, so that on the whole it may be considered as a very good improvement.

Besides the way of ascertaining small quantities of electricity by means of very delicate electrometers, 2 methods have been communicated to the philosophical world, by which such quantities of electricity may be rendered manifest, as could not be perceived by other means. The first of these methods is an invention of M. Volta, the apparatus for it being called the condenser of electricity, and is described in the Phil. Trans. vol. 72. The second is a contrivance of the above-mentioned Mr. Bennet, who calls the apparatus the doubler of electricity; a description of which is inserted in the Phil. Trans. vol. 77.

M. Volta's condenser consists of a flat and smooth metal plate, furnished with an insulating handle, and a semi-conducting, or imperfectly insulating plane. When we wish to examine a weak electricity with this apparatus, as that of the air in calm and hot weather, which is not generally sensible to an electrometer, we must place the plate on the semi-conducting plane, and a wire, or some other conducting substance, must be connected with the metal plate, and must be extended in the open air, so as to absorb its electricity; then, after a certain time, the metal plate must be separated from the semi-conducting plane, and being presented to an electrometer will electrify it much more than if it had not been placed on the above-mentioned plane. The principle on which the action of this apparatus depends is, that the metal plate, while standing contiguous to the semi-conducting plane, will both absorb and retain a much greater quantity of electricity than it can either absorb or retain when separate, its capacity being increased in the former, and diminished in the latter case. Hence we find, that its office is not to manifest a small quantity of electricity, but to condense an expanded quantity into a small space: so that, if by means of this apparatus a person expected to render more manifest than it generally is, when communicated immediately to an electrometer, the electricity of a small tourmalin, or of a hair when rubbed, he would find himself mistaken.

It is Mr. Bennet's doubler that was intended to answer this end; viz. to multiply; by repeated doubling, a small, and otherwise unperceivable, quantity of electricity, till it became sufficient to affect an electrometer, to give sparks, &c. The merit of this invention is certainly considerable; but the use of it is far from precise and certain. After repeating Mr. Bennet's description of his doubler, Mr. C. adds, as soon as I understood the principle of this contrivance, I hastened to construct such an apparatus, to try several experiments of a very delicate nature, especially on animal bodies and vegetables, which could not have been attempted before, for want of a method of ascertaining exceedingly small quantities of electricity; but, after a great deal of trouble, and many experiments, I was at last forced to conclude, that the doubler of electricity is not an instrument to be depended on, for this principal reason; viz. because it multiplies not only the electricity which is willingly communicated to it from the substance in question; but it multiplies also that electricity which in the course of the operation is almost unavoidably produced by accidental friction; or that quantity of electricity, however small it may be, which adheres to the plates in spite of every care and precaution.

Having found, that with a doubler constructed in the above manner, after doubling or multiplying 20 or 30 times, it always became strongly electrified, though no electricity had been communicated to it before the operation, and though every endeavour of depriving it of any adhering electricity had been practised; I naturally attributed that electricity which appeared after repeatedly doubling, to some friction given to the varnish of the plates in the course of the operation. In order to avoid entirely this source of mistake, or at least of suspicion, I constructed 3 plates without the least varnish, and which of course could not touch each other, but were to stand only within about $\frac{1}{8}$ of an inch of each other. To effect this, each plate stood vertical, and was supported by 2 glass sticks, which were covered with sealing-wax. I need not describe the manner of doubling or of multiplying with those plates; the operation being essentially the same as when the plates are constructed according to Mr. Bennet's original plan, excepting that, instead of placing them one on the other, mine are placed facing each other. Sometimes, instead of touching the plates themselves with the finger, I have fixed a piece of thin wire to the back of the plate, and have then applied the finger to the extremity of the wire, suspecting that some friction and some electricity might possibly be produced when the finger was applied in full contact to the plate itself.

Having constructed those plates, Mr. C. thought he might proceed to perform the intended experiments without any further obstruction; but in this he found himself quite mistaken: for, on trying to multiply with those new plates, and when no electricity had been previously communicated to any of them, he found, that after doubling 10, 15, or at most 20 times, they became so full of electricity as to afford even sparks. All his endeavours to deprive them of electricity proved ineffectual. Neither exposing them, and especially the glass sticks, to the flame of burning paper, nor breathing on them repeatedly, nor leaving them untouched for several days, and even for a whole month, during which time the plates remained connected with the ground by means of good conductors, nor any other precaution he could think of, was found capable of depriving them of every vestige of electricity; so that they might show none after doubling 10, 15, or at most 20 times. The electricity produced by them was not always of the same sort; for sometimes it was negative for 2 or 3 days together; at other times it was positive for 2 or 3 days more; and often it changed in every operation. This made him suspect, that possibly the beginning of that electricity was derived from his body, and being communicated by the finger to the plate that was first touched, was afterwards multiplied. In order to clear this suspicion, he actually tried those plates at different times, viz. before and after having walked a great deal, before and after dinner, &c. noting very accurately the quality of the electricity produced each time: but the effects seemed to be quite unconnected with the above-mentioned concomitant circumstances; which independence was further confirmed by observing that the electricity produced by the plates was of a fluctuating nature, even when, instead of touching the plates with the finger, they had been touched with a wire, which was connected with the ground, and which he managed by means of an insulating handle.

At last, after a great variety of experiments, Mr. C. became fully convinced that those plates did always retain a small quantity of electricity, perhaps of that sort with which they had been last electrified, and of which it was almost impossible to deprive them. The various quality of the electricity produced was owing to this, viz. that as one of those plates was possessed of a small quantity of positive electricity, and another was possessed of the negative electricity, that plate which happened to be the most powerful, occasioned a contrary electricity in the other plate, and finally produced an accumulation of that particular sort of electricity. These observations evidently show, that no precise result can be obtained from the use of those plates, and of course that when constructed according to the original plan, they are still more equivocal, because they admit of more sources of mistake.

As those plates, after doubling or multiplying only 4 or 5 times, show no signs of electricity, none having been communicated to them before, he imagined that they might be useful so far only, viz. that when a small quantity of electricity is communicated to any of them in the course of some experiment, one might multiply it with safety 4 or 5 times, which would even be of advantage in various cases, but in this also his expectations were disappointed. Having tried them in various experiments, Mr. C. then adds, after all the above-mentioned experiments made with those doubling or multiplying plates, we may come to the following conclusion, viz. that the invention is very ingenious, but their use is by no means to be depended on. It is to be wished, that they may be improved, so as to obviate the weighty objections that have been mentioned in the preceding pages, the first desideratum being to construct a set of such plates as, when no electricity is communicated, they will produce none after having performed the operation of doubling for a certain number of times. On the whole, the methods by which small quantities of electricity may be ascertained with precision are, he says, only 3. If the absolute quantity of electricity be small and pretty well condensed, as that produced by a small tourmalin when heated, or by a hair when rubbed, the only effectual method of manifesting its presence, and ascertaining its quality, is to communicate it immediately to a very delicate electrometer, viz. a very light one, that has no great extent of metallic or of other conducting substance; because if the small quantity of electricity that is communicated to it be expanded throughout a proportionably great surface, its elasticity, and of course its power of separating the corks of an electrometer, will be diminished in the same proportion. The other case is, when we want to ascertain the presence of a considerable quantity of electricity, which is dispersed or expanded into a great space, and is little condensed, like the constant electricity of the atmosphere in clear weather, or like the electricity which remains in a large Leyden phial after the first or 2d discharge.

To effect this, he uses an apparatus, which in principle is nothing more than Mr. Volta's condenser; but with certain alterations, which render it less efficacious than in the original plan, but at the same time render it much less subject to equivocal results. He places 2 of the above described tin plates on a table, facing each other, and about $\frac{1}{8}$ of an inch asunder. One of these plates, for instance A, is connected with the floor by means of a wire, and the other plate B is made to communicate, by any convenient means, with the electricity required to be collected. In this disposition the plate B, on account of the proximity of the other plate, will imbibe more electricity than if it stood far from it, the plate A in this case acting like the semi-conducting plane of M. Volta's condenser, though not with quite an equal effect, because the other plate B does not touch it; but yet, for the very same reason, this method is incomparably less subject to any equivocal result. When the plates have remained in that situation for the time that may be judged necessary, the communication between the plate B, and the conducting substance which conveyed the electricity to it, must be discontinued by means of a glass stick, or other insulating body; then the plate A is removed, and the plate B is presented to an electrometer, to ascertain the quality of the electricity; but if the electrometer be not affected by it, then the plate B is brought with its edge into contact with another very small plate, which stands on a semi-conducting plane, after the manner of M. Volta's condenser; which done, the small plate, being held by its insulating handle, is removed from the inferior plane, and is presented to the electrometer: and it frequently happens, that the small plate will affect the electrometer very sensibly, and be quite sufficient for the purpose; whereas the large plate itself showed no clear signs of electricity.

The 3d and last case is when the electricity to be ascertained is neither very considerable in quantity, nor much condensed; such is the electricity of the hair of certain animals, of the surface of chocolate when cooling, &c. In this case the best method is to apply a metal plate, furnished with an insulating handle, like an electrophorus plate, to the electrified body, and to touch this plate with a finger for a short time while standing in that situation; which done, the plate is removed, and is brought near an electrometer; or its electricity may be communicated to the plate of a small condenser, as directed in the preceding case, which will render the electricity more conspicuous.

Having thus far described the surest methods of ascertaining the presence and quality of electricity, when its quantity or degree of condensation is small, Mr. C. now adds some further remarks on the subject of electricity in general, and which have been principally suggested by what has been mentioned in the preceding pages. From which he says it may be inferred, that the air, or in general any substance, is a more or less perfect conductor of electricity, according as the electricity which is to pass through it is more or less condensed; so that if a given quantity of electric fluid be communicated to a small brass ball, we may take it away by simply touching the ball with a finger; but if the same quantity of electric fluid be communicated to a surface of about 100 or 1000 square feet, the touching with the finger will hardly take away any part of it. If it be asked, what power communicates the electricity, or originally disturbs the equilibrium of the natural quantity of electric fluid in the various bodies of the universe; we may answer, that the fluctuating electric state of the air, the passage of electrified clouds, the evaporation and condensation of fluids, and the friction arising from divers causes, are perpetually acting on the electric fluid of all bodies, so as either to increase or diminish it, and that to a more considerable degree than is generally imagined.

Mr. C. lastly concludes with briefly proposing an explanation of the production of electricity by friction, which is dependent on the above stated proposition, viz. that bodies are always electrified in some degree; and also on the well known principle of the capacity of bodies for holding electric fluid being increased by the proximity of other bodies in certain circumstances. It seems to me, he says, that the cylinder of an electrical machine must always retain some electricity of the positive kind, though not equally dense in every part of its surface; therefore, when the part of it A is set contiguous to the rubber, it must induce a negative electricity in the rubber. Now, when by turning the cylinder, another part of it B, which suppose to have a less quantity of positive electricity than the preceding part A, comes quickly against the rubber; the rubber being already negative, and not being capable of losing that electricity very quickly, must induce a stronger positive electricity in the part B, which is now opposite to it; but this part B cannot become more positively electrified, unless it receives the electric fluid from some other body, and therefore some quantity of electric fluid passes from the lowest part of the rubber to the part B of the glass, which additional quantity of electric fluid is retained by the part B only while it remains in contact with the rubber; for after that, its capacity being diminished, the electric fluid endeavours to escape from it. Thus we may conceive how every other part of the glass acquires the electric fluid, &c. and what is said of the cylinder of an electrical machine may, with proper changes, be applied to any other electric and its rubber.

It appears therefore, that according to this theory, a part of the rubber, viz. that which the surface of the glass cylinder enters in turning round, must serve to furnish the electric fluid to the glass, and the upper part must be possessed of a negative electricity capable of inducing a positive electricity in the glass contiguous to it. In fact, this seems to be confirmed by the general practice and experience; for that rubber answers best for a common electrical machine, which can easily conduct the electric fluid with its under part, and the upper part of which is more ready to acquire, and to retain, the negative electricity; hence the rubbers are generally furnished with amalgam below, and with a piece of silk above; hence also, if the cylinder of the machine be turned the contrary way, it will produce little or no electricity. It often happens, that the part which conducts the fluid, and that which acquires the electricity contrary to that of the electric, are not so disposed in a rubber as is above described; but it remains always true, that the rubber must be possessed of those two properties, viz. to conduct the electric fluid very readily in one or more parts, and to acquire, as well as retain, on other parts, an electricity contrary to that acquired by the electric that is to be rubbed with it.

II. The Croonian Lecture on Muscular Motion. By George Fordyce, M. D., F. R. S. p. 23.

In considering muscular motion, I must begin with some observations on motion in general, and with that well known and self-evident axiom, that one particle of matter, considered by itself, will remain at rest if it be at rest, and will continue in motion if it be in motion, and in the same direction. This has been called the vis insita, or vis inertiæ, of matter. It may be said in other words, that a single particle of matter being at rest, would therefore always continue at rest, if it were not for some external impulse made on it. This impulse may be from some other particle of matter in motion acting on it, and communicating part of its motion to it, while it communicates an equal quantity of its rest to the matter so acting on it, so that the quantity of motion and rest shall be the same after the impact, in both bodies, as they were before: or, in other words, a simple particle of matter in motion would always continue in motion, in the same direction, if it did not meet with another, on which it acted; and after the impact, there would be the same quantity of motion and rest in both bodies taken together, as was before. If we consider equal motion, in direct contrary direction, as rest; motion, or rest, produced in a body by the above means, I shall call communicated.

If 2 simple particles of matter, of any species, not farther distant from one another than the sun is from the earth, were both at perfect rest, these 2 particles would instantly begin to move toward each other, if no other particle of matter whatever existed. There would therefore be an impulse, producing motion between these bodies, without any contact. Motions produced in this way, I call original motions. The first consideration with regard to any particular motion is therefore, whether it be an original or communicated motion. If it be an original motion, it will follow the laws of that particular species of original motion; if it be a communicated motion, it will follow the laws of communicated motion. Many observations shew, that muscular motion is not a communicated motion, and therefore an original one.

In any system of bodies, or particles of matter, affecting one another only by the motions already existing in them being communicated to each other, they may diminish their motion, or bring one another to rest; but they never can increase the motion existing in the whole. It happens frequently, that the motions in the animal body are increased, without any alteration of external applications to it; the cases are so numerous, that it is hardly worth bringing an example: we might mention the increase at times of the circulation, and all the motions of the fluids without the least new motion in the surrounding bodies, or interference, or even knowledge of the mind. This motion must therefore be original, and not communicated.

In communicated motion, if one body be at rest, and a motion be communicated to it by another, the power of the whole motion shall not be greater than that in the communicating body at the time of the communication. If I take out the heart of an animal, and cut off the auricles, it will in many cases continue to contract and dilate for some time. If it be left to come to rest, and if soon after a needle be introduced into the ventricle, placed transversely, and if the interior surface of the ventricle be pricked gently by the needle, the ventricle will contract with such power as to force the needle deep into it: in this case, the force of the contraction of the ventricle is much greater than the power with which it was pricked by the needle; this contraction was therefore not communicated to it by the moving needle, but was generated, and therefore an original motion.

In all communicated motion, by which two bodies at a distance are brought near to one another, there must subsist some other matter, by which they may be drawn, or forced, nearer to one another; but in original motion, it is not at all necessary that any other matter should exist at all. Two particles, placed at as great a distance from one another as the sun is from the earth, as before observed, though at perfect rest, would begin to move nearer one another, by the attraction of gravitation, if all other matter whatever were annihilated.

Most authors who have treated on muscular motion have supposed that it was a communicated motion; and that it was produced by something passing by the nerves, from the brain to the moving part. Three doctrines have been set forth; one, that there is a fluid passing along the nerves; a 2d, that there is a vibration; and a 3d, that the nerves are surrounded with something like electric matter, in which motion runs from the brain to the moving parts. Those who have considered this subject must be tired of the arguments which have been brought to refute each of these; for no argument from fact has been employed to prove any one of them: I shall therefore leave them as mere chimeras of the brain. I have taken notice, that it is not requisite for any motion to pass between two bodies, exciting in each other an original motion, through or by any other matter: I have also shown, that muscular motion is an original motion: it follows, that it is not necessary for any motion, or communication, to pass through any other matter, in order to bring the muscular fibres into action.

One case of muscular motion is, when a stimulus is applied to some part of the body, and a muscle at a distance immediately contracts. It has been supposed in this case, that some influence was communicated to the nerve of the part where the stimulus was applied, and through it to the brain, and from the brain through the nerves of the contracting muscle. Granting, for a little, that some motion may pass along the nerves, and therefore that the end of the nerve, where the application was made, may be the part in which the original motion began, the stimulus frequently does not touch the end of the nerve; for if vapour of volatile alkali be applied to the nostrils, a universal glow of heat, and increased circulation, will constantly take place; but the vapour of the volatile alkali could not touch the nerves of the nostrils, the membrane being constantly covered with mucus, which the vapour could not penetrate without dissolving it, which it had not time to do, and, if it had, would have united with it, so as to form a soap, void of any stimulating power.

If therefore the original motion began in the end of the nerves of the nostril, it must be excited by a substance at a distance from the end of that nerve, without any communication of motion between the stimulus, that is, application producing the motion, and the end of the nerve in which it is excited; therefore, on any supposition, a stimulus is capable of exciting a motion, in a part at a distance, without any communication of motion; and it is therefore not necessary that the nerves should be at all employed in the motions of the body excited by a stimulus, as it can act at a distance without their intervening. Further, that the nerves are not employed in the motions excited by stimuli, is evident from this experiment: take the heart out of a living animal, cut all the nerves off as close as possible, lay it in nearly the heat of the body of the animal, it will continue to contract for some time. As soon as it has ceased contracting, prick a fibre in one of the ventricles; both ventricles, and all their fibres, will contract instantly, though there be now no communication by the nerves, between many of the contracting fibres, and the fibre stimulated. It might be suspected that the motion of the fibre stimulated might affect the others: in this case the contractions would be progressive; but, on the contrary, the whole contract at once.

I cannot help bringing another instance, where stimuli produce action in parts at a distance, without any communication of motion by the nerves. When infusion of cantharides is applied to the skin, as we say vulgarly, it is not applied immediately to the skin; but in the first instance to the mucous and sebaceous matter, which every where covers the scarf skin; under this lies the scarf skin, which the infusion can hardly be conceived to come at; if it did, the scarf skin we know is perfectly impenetrable to such a fluid; it can therefore never touch the skin, in which it excites inflammation, and on which it therefore acts at a distance, and excites motion, which no one can suspect to come through the nerves: nor is there any motion through the mucus and scarf skin, of any other kind than would arise if an infusion of any other insect had been applied, which had no power of exciting inflammation. From what has been said we may conclude, that when a stimulus has been applied so as to excite motion in a distant part, no motion whatever takes place in the nerves, or is communicated by them from the part to which the stimulus is applied to the moving part.

I need not draw your attention to another proposition, viz. that when a stimulus is applied to a distant part, so as to produce motion, it often happens, that the stimulating matter is not carried by the blood-vessels, or otherwise, to the moving part. This proposition has often been demonstrated, and is well known. All the original power exerted by any of the moving parts consists in a power of particles coming nearer to one another; for every muscle or fibre becomes shorter when it acts; or in other words contracts; and every other moving part in like manner contracts when in action. It is true that there are many contrivances to make the contraction have great effect in producing motion, force being never spared for conveniency, as Mr. Hunter has I believe already set forth: yet it is clear from every experiment made on the subject, that all motion arises from particles coming near one another in some direction. It would be superfluous to point out these experiments; I shall mention one only, and an obvious one. Lay bare a muscle, and prick any of its fibres, it immediately becomes shorter.

The original power of coming nearer to one another of 2 or more particles of matter, has been called attraction. There have been several original powers of coming nearer one another of particles of matter, which have been considered as different attractions, such as the attraction of gravitation, of magnetism, of electricity, &c. The attraction which is my present object, I call the attraction of life. This attraction is either of 2 species, or is exerted variously; for all the moving parts have their particles nearer one another in the living than in the dead body. The proof of this is as necessary as it is obvious. Take the body of any animal, when the life is entirely gone from it, and the effects of it are entirely lost, but before any putrefaction, or any change in its chemical qualities, has taken place; and lay bare, and dissect out, any muscle, especially one which has long fibres, and no middle tendon, such as the sartorius, for example, and afterwards lay it in its place, leaving it of the length it naturally takes; it will reach farther than from its origin to its insertion; but lay bare, and dissect out, the same muscle in the living body, and it will always be shorter than from its origin to its insertion. If it should be said, that the dissection stimulated the muscle, and brought it into action, let it not be dissected out, but its tendon cut through, as the tendo Achilles, for instance, the same thing will happen. And we now are all convinced, from various experiments, that a tendon in a sound state is not capable of being stimulated by being wounded, cut through, or broken.

I apprehend then that we may conclude, that all the moving parts are constantly contracted, that is, their particles are nearer one another when the body is alive, than when dead, and totally left to their elasticity. This species of action I call the tone. The 2d species, or variety, which occurs in the attraction of life, is when a moving part, for a short time, has its particles brought nearer one another than they are from their tone, and which very rarely continues for many seconds of time without intermediate relaxation. I call it their action; when it continues for a longer time, it is called spasm; which however is so vague a term, that I could wish totally to reject it, at least to confine it only to this sense, viz. a greater contraction, or coming nearer one another of the particles, of a moving part, than that which would happen from their tone, remaining without any intermediate relaxation.

For the present I do not mean to say any thing further with regard to the tone, or spasm of parts; but only to consider the action as excited by applications to some part of the body at a distance from the moving part. I have already rejected all communication by the application, or stimulus, being carried by the blood-vessels, or any other way whatever, to the part. I have also rejected any motion, or communication of any kind whatever by the brain and nerves to the part. I conceive that when any stimulus or application whatever is made in any part, so as to produce any action in a distant part, that that medicine or application, without having any operation whatever on the intermediate parts, gives a power to the particles of the moving part of greater attraction. I shall illustrate this idea by supposing that there is a machine moving by various powers, either original or communicated; and that in this machine there are 2 magnets, which by their attractive power have come to a given distance from one another, but have been prevented from coming nearer by some power endeavouring to draw them back. A much stronger magnet applied to a part of the machine, in a certain manner, so as not to touch either of the 2 already there, nor to affect any other part, may increase their power of attraction, so as to make them overcome the resistance, and come nearer one another*. In the same manner I apprehend that an application made to the skin of the abdomen may, and often does, occasion the action of the intestines to take place, without any effect whatever on the intermediate parts; but that it simply excites the attraction of the particles of the moving parts of the intestines; certainly a part of the matter through which the influence is to pass, viz. the mucus and scarf skin, is actually inanimate matter.

In certain cases of original motion there is attraction, or the coming nearer of particles only. In others there is not only attraction, but opposite repulsion; the cases of which are unnecessary to enumerate to this society. In the attraction of life there is no opposite repulsion; all the motions of the body are produced entirely by the force of particles coming nearer one another. When this force diminishes, or the action goes off, and leaves the part entirely to its tone, the particles of the moving part are by no means repulsed from one another, but are left to be drawn asunder by their elasticity, weight, or the weight of the surrounding parts, or any other accidental power: yet there are applications which may be made to distant parts of the body, which may, and do, take off the attraction which occasions the action of the moving part; and all those reasonings which I have already applied to applications which excite action, and which are called stimuli, are equally applicable to those applications which make action cease, and which we call sedatives. The great ground on which I have attempted to make these observations, is the foundation of certain maxims in the practice of medicine, which I shall now proceed to sketch out to the society,

* I do not mean to insinuate, in the smallest degree, that the powers of the body at all depend on, or have any thing to do with, magnetism.—Orig.

whose institution, to me, has always seemed to include the philosophy, but not the actual practice, of medicine.

Medicine is a science of long cultivation in that channel in which all the sciences have flowed, and had early attained great perfection, I believe, from the testimony of various writers of antiquity, and other circumstances: for though Celsus observes well, that there could be no physicians among the Greeks at the time of the Trojan war, inasmuch as Homer never mentions one medicine, but only application to the gods for the cure of fevers, and other internal diseases; yet the Egyptians, from whom the Greeks received a great part of their knowledge in all science, as well as in medicine, had certainly not only regular physicians for internal diseases, but also stone-cutters, oculists, aurists, &c. long before the Trojan war; and Hippocrates, by his own testimony, took much of his knowledge from what he calls the ancients. In the progress therefore of the science of medicine, it came into my mind to inquire how far, and on what ground, the modern increase of science in anatomy, chemistry, mathematics, &c. had forwarded the knowledge of medicine. In the first place, it is well known that medicine was in the hands of Greek physicians from the time of Hippocrates, or rather from the destruction of the Egyptian monarchy by Cambyses, down to the time of the Crusades; in all this time there was hardly a dissection of the human body, from an opinion about manes; but when it came into Europe again, where this opinion remained indeed, but in a much less degree, anatomy began again to flourish; and by other means all the other sciences shone forth with a greater lustre than they had ever done in any period handed down to us by the history of any nation. It was obvious therefore to conceive, that the knowledge of the structure of the body, and the investigation of the powers of matter, made in a more accurate manner, and on a more extensive scale, would elucidate the doctrine of the human body, and its diseases, and their treatment, in a new and more perfect manner: to this opinion I mean now to apply the reasoning I have before laid down.

In the structure and physiology of the body, 2 great discoveries have been made by the moderns; the circulation of the blood; and the lymphatics and absorption of the lymph. These at once overthrow the ideas of the ancients with regard to most of the functions of the interior parts of the body; which was now conceived by many to be an hydraulic machine, and subject to all those disorders which were incidental to such a machine; and particularly, from various fluids flowing with great rapidity, through tubes, many of them of infinite fineness, that stoppages must often take place, which were to be removed by dissolving out the obstructing matter; that the blood was mixed so perfectly through all the body, and so constantly, that the same blood must be taken away, whatever blood-vessel was opened; and when we contemplate the numerous openings and communications of the vessels with one another, that blood flowing out of any one will empty them all equally, and that therefore it can be of no consequence from what part of the body blood is evacuated. In a pleurisy, for instance, where can be the difference, whether blood be taken from the right or left arm, or from the vessels of the skin of the breast? But there is a difference, and a great one too; since taking a much less quantity of blood from the skin of the breast, is actually known, in certain cases, from experience, to cure a pleurisy, than would have had that effect if taken from the vessels in the arm, and will even carry off the disease, when it could not be carried off at all by evacuation from the arm: yet it is undoubtedly the very same blood in all its qualities; and in both cases the vessels of the pleura are equally emptied. The act of flowing out of the blood from the vessels of the skin of the breast then has an immediate action on the action of the moving parts in the pleura, and carries off the inflammation independent of the circulation, or any of its laws; and so far has the knowledge of the circulation been of any advantage in this case, that it had nearly thrown out topical bleeding in inflammation, which is one of our most powerful remedies in the disease. In like manner, when the moving fibres of the stomach do not contract, so as to expel any vapour that may get into it, a spice applied to the skin over the stomach will, in many cases, occasion these fibres to contract. Now it is well known from anatomy, that there is no communication between the skin of the abdomen and the stomach; and if the spice were to act by touching these fibres, it would be the same, whether it was applied to the skin over the stomach, or to the skin of the arm; for in both cases it must be absorbed by the lymphatics, and carried to the left side of the heart, and there and in the lungs be blended universally with the whole blood, and carried by the arteries to the moving fibres of the stomach. Nay more, that it would be equal, whether it was applied to the inner surface of the stomach itself, or to the external skin, or any other membrane, of any other cavity: for the stomach is covered with mucus, and lined with a membrane which is perfectly impervious, and totally prevents any thing contained in the stomach from being any way applied to the moving fibres; it must in this case therefore be likewise taken up by the lymphatics, or lacteals, and carried to the heart before it could touch the moving fibres of the stomach. The maxim then, arising from our knowledge of the lymphatics, would be, that it was of no consequence where we applied a spice in cases of flatulency; which is not true. By similar reasons it might be easily shown, that all the knowledge of the properties of the fluids, which has been acquired by modern and accurate experiments, hardly contributes any thing to the knowledge of applying medicines for the cure of diseases; and that the study of the laws of the attraction of life, or what has been called muscular motion, is of considerable importance.

One more observation I have only to make, viz. that all original motions are by their nature perfectly unintelligible as to their cause; who can tell the cause of gravity, chemical attraction, &c.? and so undoubtedly the attraction of life, in its cause, can never be investigated, being, like all other attraction, a power which 2 or more particles of matter have of coming near each other. But though the study of causes of original powers be totally absurd and futile, yet the laws of their action are capable of investigation by experiment, and applicable to the evolving much useful knowledge. Need I remind this Society, that the investigation of the laws of gravitation, by Sir Isaac Newton, has rendered it immortal? The investigation of the laws of the attraction of life has also been greatly forwarded by one of its present members; but it is an investigation which will probably require ages to render perfect.

III. Of a Mass of Native Iron, found in South America. By Don Michael Rubin de Celis. From the Spanish. p. 37.

About 30 years ago, the various barbarous nations who inhabited the provinces of the great Chaco Gualamba, expelled the Spaniards from thence; and since that time the countries on the southern part of the river Vermejo, and western of the great river Paranà, have been almost totally deserted. The only employment of the few Indians who dwell within the jurisdiction of Santiago del Estero is to gather the honey and wax with which the woods abound. These Indians discovered, in the midst of a wide-extended plain, a large mass of metal, which they called pure iron; part of which projected above the ground about a foot, and almost the whole of its upper surface was visible. Intelligence of this discovery was immediately communicated to the Viceroys of Peru. That such a mass of iron should be found in a country where there are no mountains, nor even the smallest stone within a circumference of 100 leagues, could not fail to appear extraordinary, though we know there are mines of pure iron in Europe. Some private persons, at the great risk of their lives, both from the uncertainty of procuring food or even water (of which none is to be found but what rain happens to be preserved in some natural cavities of the earth,) from the danger of meeting the roving Indians, from the various wild beasts found in those plains, such as tygers, leopards, tapirs, from the swarms of poisonous reptiles, and finally from the endless thickets, led on by hopes of enriching themselves, boldly undertook the journey, to obtain some of the metal. They transmitted a part of it to Lima and Madrid, by which no other advantage was gained than to ascertain it to be very soft and very pure iron. As it is forbidden by law, for political reasons, to manufacture iron in that country, though different parts of it abound with iron mines; and as it was asserted, that the vein of iron extended many leagues, the visible part being only its crest projecting above the ground, which, when dug round, was found to measure 3 yards from N. to S., $2\frac{1}{4}$ yards from E. to W., and about $\frac{1}{3}$ of a yard in thickness; the viceroy of the river Plata sent me with orders to examine this discovery with accuracy; and, in case I found it a beneficial mine, that I should establish a colony there. I accordingly set off, well escorted, in the beginning of Feb. 1783, from Rio Salado, an ancient civilized hamlet of the Indians whom we call Vilelas, and pursued my journey in the direction E. $\frac{1}{4}$ N. E. though on further examination I found that I ought to have taken my direction E. $\frac{1}{4}$ S. E. both corrected.

The aspect of the country between the river and the mine, distant 70 leagues from the settlement, is curious: it consists of an immense plain, alternately intermixed with thick woods and fertile fields, forming most pleasing landscapes. The latitude of the mine I found, by observation, to be 27° 28′ S. There is not one fixed place of habitation throughout the whole country, owing to a scarcity of running water. That which is drank by the honey-gatherers, who reside there in small bodies the greatest part of the year, collecting honey in the woods, is rain water, as I have already observed. These, and a few roving tribes of barbarous Indians, who resemble the Tartars in their way of life, and come hither, at a certain season of the year, from the borders of the river Vermejo, in quest of a wild root, which they call Koruu, and which they constantly chew, as a remedy against the pestilential air of their native country, and also as a preservative against the bite of poisonous reptiles, are the only people ever seen in those pleasant and extensive plains.

I arrived the 15th of Feb. at the place called Otumpa, where the mass was found almost buried in pure clay and ashes. The exterior appearance of it was that of perfectly compact iron; but on cutting off pieces of it, I found the internal part full of cavities, as if the whole had been formerly in a liquid state. I was confirmed in this idea, by observing on the surface of it the impressions as of human feet and hands of a large size, as well as of the feet of large birds, which are common in this country. Though these impressions seem very perfect, yet I am persuaded that they are either a lusus naturæ, or that impressions of this nature were previously on the ground, and that the liquid mass of iron falling on it received them. It resembled nothing so much as a mass of dough, which, having been stamped with impressions of hands and feet, and marked with a finger, was afterwards converted into iron.

I began to cut off part of it with chissels; and in separating from the mass 25 or 30 pounds, I spoiled all the chissels I had, to the number of 70. I ordered my men to dig round it, and found the under surface covered with a coat of scoriæ from 4 to 6 inches thick; undoubtedly occasioned by the moisture of the earth, because the upper surface was clean. Having moved it half round, by means of handspikes, I ordered the ground under its bed to be dug to a considerable depth, and even blew it up in 2 places with gunpowder; after which, examining the deepest part of the earth, I found it exactly like the upper part, and of the same nature as the earth of all the country, as likewise of 2 pits, which I had dug at the distance of 70 or 100 paces E. and W. of the mass. Finding here no root or trace of generation, I reasoned in the following manner.

Either this mass was produced in the spot where it lies, or it was conveyed hither by human art, or cast hither by some operation of nature. It could not be generated here, according to any known process of nature. And whence, by whom, or how, could it be conveyed hither, as there are no iron mines within hundreds of leagues, nor remembrance that any have been worked in the kingdom? It could be of no value, since it could not be used; and why bring it into a country the most uninhabitable of all the Chaco, from the want of water? Besides, how could so heavy a mass be conveyed, the Indians never having known the use of wheel carriages? This mass therefore must have been the effect of some volcanic explosion. Many circumstances induce me to think so. Volcanos frequently leave behind them, after explosion, pits of water, either hot or cold; and at the distance of about 2 leagues to the east of this mass, I discovered a brackish mineral spring, the only one to be found in all this country. In the whole district of the Chaco I travelled over, I observed no difference in the level of the ground, except the very spot where I made this discovery; and here only I found a gentle ascent running from N. to S. and which attracted my notice before I had seen the mass of iron. This ascent is between 4 and 6 feet above the rest of the country. The earth in every part around this mass, as well as about the brackish spring, is a very light, loose earth, like ashes, even in colour. The grass produced immediately contiguous, called Ahivi, is short, small, and extremely unpalatable to cattle; whereas the grass found in the rest of the country, at a little distance from the mass and salt spring, is long and very grateful to them. At a little depth in the earth are found stones of quartz, of a beautiful red colour, which the honey-gatherers make use of as flints to light their fires. They had formerly carried some of them away, on account of their peculiar beauty, being spotted and studded as it were with gold. One of these that weighed about 1 oz. came into the hands of the Governor of Santiago del Estero, who told me, that he ground it, and showed me more than 1 dr. of gold that he had extracted from it.

It is an undoubted fact, that in these immense forests there exists a mass of pure iron, in the shape of a tree, with its branches. Many of the Indians have seen it; and the inhabitants of the colony of the Avipones are acquainted with the spot where it lies. A distinguished European of the city of Salta touched it. The body of this tree extends on the ground in the direction from E. to W.

having left behind to the E. a part which lies in the direction from N. to S. and it is from this that the pieces of metal may have been taken with a chissel. From this account one may venture to form the following hypothesis. I will suppose, that the volcanic explosion happened in the spot where I discovered the brackish spring; that by the explosion a great quantity of earth was raised, and formed the elevation of this part of the plain above the rest; that it was originally much higher, but the continual rains of the Chaco, which is overflowed a 3d part of the year, are always acting to bring it to a level with the rest of the country. The direction of the greatest portion of the mass of iron ejected was from E. to W. and being heavier reached to the distance where it is found. Another portion of the matter smaller, and perhaps more fluid, separated and took another direction, running into several streams, as when water is thrown out of a pail. This small portion of matter having cooled, and the earth which supported it being washed away gradually by the water, must I apprehend have formed what is now called the tree of iron.

The saline and antimonial matters, which usually accompany all minerals, must have been scattered round about in a similar manner, and rendered the ground barren. In the kingdom of Santa Fe de Bogotà, there is found dust of platina mixed with gold. Almost every body knows the great affinity there is between these two metals, so that it is not surprising, considering all these reasons, that the fire of the volcano should have melted the platina which lay above the gold, and thrown it up. This principle of volcanos is the most natural to account for the formation of those famous masses of silver found separate at Guantajaia, about which so many extravagant and ridiculous stories have been told. The mass of iron, which is the subject of this letter, according to its cubic measure, and allowing it a little more specific gravity than iron, must weigh about 300 quintals.

Some specimens of the iron accompanied this paper, and were laid before the Society; who afterwards presented them to the British Museum. C. B.

IV. Frigorific Experiments on the Mechanical Expansion of Air, explaining the Cause of the Great Degree of Cold on the Summits of High Mountains, the Sudden Condensation of Aerial Vapour, and of the Perpetual Mutability of Atmospheric Heat. By Erasmus Darwin, M. D., F. R. S. p. 43.

Having often revolved in my mind the great degree of cold produceable by the well-known experiments on evaporation; in which, by the expansion of a few drops of ether into vapour, a thermometer may be sunk much below the freezing point; and recollecting at the same time the great quantity of heat which is necessary to evaporate or convert into steam a few ounces of boiling water; I was led to suspect, that elastic fluids, when they were mechanically expanded, would attract or absorb heat from the bodies in their vicinity; and that when they were mechanically condensed, the fluid matter of heat would be pressed out of them, and diffused among the adjacent bodies. As this principle might possibly be extended to elastic solid bodies, as well as to fluid ones, and explain the cause of the heat occasioned by percussion or friction, and by some chemical combinations, as well as the perpetual mutability of it in the atmosphere, I have, at different times, endeavoured to subject it to experiment.

1. When Dr. Hutton of Edinburgh, and Mr. Edgeworth of Edgeworthtown in Ireland, were with me about 12 or 14 years ago, the following experiment, which had been proposed by one of the company, was carefully made. The blast from an air-gun was repeatedly thrown on the bulb of a thermometer, and it uniformly sunk it about 2 degrees. The thermometer was firmly fixed against a wall, and the air-gun, after being charged, was left for an hour in its vicinity, that it might previously lose the heat acquired in the act of charging; the air was then discharged in a continued stream on the bulb of the thermometer, and the event showed, that the air at the time of its expansion attracted or absorbed heat from the mercury of the thermometer.

In March 1785, by the assistance of Mr. Fox and Mr. Strutt, of Derby, a thermometer was fixed in a wooden tube, and so applied to the receiver of an air-gun, that on discharging the air by means of a screw pressing on the valve of the receiver, a continued stream of air, at the very time of its expansion, passed over the bulb of the thermometer. This experiment was 4 times repeated in the presence of many observers, and uniformly sunk the thermometer from 5 to 7 degrees. During the time of condensing the air into the receiver, there was a great difference in the heat, as perceived by the hand, at the two ends of the condensing syringe; that next the air-globe was almost painful to the touch; and the globe itself became hotter than could have been expected from its contact with the syringe. Add to this, that in exploding an air-gun, the stream of air always becomes visible, which is owing to the cold then produced precipitating the vapour it contained; and if this stream of air had previously been more condensed, or in greater quantity, so as not instantly to acquire heat from the common atmosphere in its vicinity, it would probably have fallen in snow, as in the fountain of Hiero, mentioned below.

2. About 12 or 14 years ago, by the assistance of Mr. Waltire, a celebrated itinerant teacher of philosophy, a thermometer was placed in the receiver of an air-pump, and some time being allowed, that it might accurately adapt itself to the heat of the receiver, the air was hastily exhausted; during which the mercury of the thermometer sunk 2 or 3 degrees, and after some minutes regained its previous height. In November 1787, by the assistance of my very ingenious friend Mr. Forester French, the above experiment was repeated; but with this

difference, that the thermometer was open at the top; so that the diminution of external pressure could not affect the dimensions of the bulb; and the result was the same, the mercury in the thermometer sunk 2 or 3 degrees, and gradually rose again. Does not this show, that the air in the receiver, being expanded during the exhaustion, attracted or absorbed heat from the mercury in the thermometer? Both during the exhaustion, and during the re-admission of the air into the receiver, a steam was regularly observed to be condensed on the sides of the glass, which in both cases was in a few minutes re-absorbed. This steam must have been precipitated by its being deprived of its heat by the expanded air: if it could have happened from any other cause, the vapour could not, in both situations, viz. of exhaustion, and of re-admission, have been taken up again.

3. In December 1784, with the assistance of Mr. Fox, the following experiment was carefully made. A hole, about the size of a crow-quill, was bored into a large air-vessel, placed at the commencement of the principal pipe in the water-works which supply the town of Derby. The water from 4 pumps, which are worked by a water-wheel, is first thrown into the lower part of this air-vessel, and thence rises to the top of St. Michael's Church into a reservoir, which may be about 35 or 40 feet above the level of the air-vessel. Two thermometers were previously suspended on the leaden air-vessel, that they might become of the same temperature with it; and, as soon as the hole was opened, had their bulbs reciprocally applied so as to receive the stream of air; and the mercury in both of them sunk 2 divisions, or 4 degrees. This sinking of the mercury in the thermometers could not be ascribed to any evaporation of moisture from their surfaces, because it was seen, both in exhausting and re-admitting the air into the exhausted receiver, that the vapour which it previously contained was deposited during its expansion.

4. There is a very curious phenomenon observed in the fountain of Hiero, constructed on a very large scale in the Chemnicensian mines in Hungary, which is very similar to the experiments above related. In this machine the air, in a large vessel, is compressed by a column of water 260 feet high: a stop-cock is then opened, and as the air issues out with great vehemence, and, in consequence of its previous condensation, becomes immediately much expanded, the moisture it contained is not only precipitated, as in the exhausted receiver above-mentioned, but falls down in a shower of snow, with icicles, adhering to the nosel of the cock. This remarkable circumstance is described at large, with a plate of the machine, in the Philos. Trans. for 1761, vol. 52.

5. From the 4 experiments already related; first, of the mercury sinking in the thermometer, by being exposed to the stream of air from an air-gun; 2dly, from its sinking in the receiver of an air-pump, during the time of exhausting

it; 3dly, from its sinking when exposed to a stream of air from the air-vessel of a water-engine; and, lastly, from the curious phenomenon of snow and ice being produced by the stream of expanding air from the fountain of Hiero in an Hungarian mine; there is good reason to conclude, that in all circumstances, when air is mechanically expanded, it becomes capable of attracting the fluid matter of heat from other bodies in contact with it.

Coldness of the summits of mountains.—Now, as the vast region of air which surrounds our globe is perpetually moving along its surface, climbing up the sides of mountains, and descending into the valleys; as it passes along it must be perpetually varying its degree of heat, according to the elevation of the country it traverses: for in rising to the summits of mountains it becomes expanded, having so much of the pressure of the super-incumbent air taken away, and when thus expanded it attracts or absorbs heat from the mountains contiguous to it; and when it descends into the valley, and is again compressed into less compass, it again gives out the heat it has acquired to the bodies it becomes in contact with. The same thing must happen in respect to the higher regions of the atmosphere, which are regions of perpetual frost, as was always suspected, and has of late been demonstrated by the aërial navigators. When large districts of air from the lower parts of the atmosphere are raised 2 or 3 miles high, they become so much expanded by the great diminution of the pressure over them, and thence become so cold, that hail or snow is produced from the precipitated vapour, if they contain any: and as there is, in these high provinces of the atmosphere, nothing else for the expanded air to acquire heat from, after the precipitation of its vapour, the same degree of cold continues till the air, on descending to the earth, acquires again its former state of condensation and of warmth. The Andes, almost under the line, rests its base on burning sands; about its middle height is a most pleasant and temperate climate, covering an extensive plain, on which is built the city of Quito; while its forehead is encircled with eternal snow, coeval perhaps with the elevation of the mountain: yet, according to the accounts of Ulloa, these 3 discordant climates seldom entrench much on each other's territories. The hot winds below, if they ascend, become cooled by their expansion, and hence cannot affect the snow on the summit; and the cold winds, that sweep the summit, become condensed as they descend, and of temperate warmth, before they reach the fertile plains of Quito.

Correspondence of the heat of the atmosphere with the height of the barometer.—From this principle some of the sudden changes of our atmosphere from hot to cold, and from dry to moist, may also be accounted for. During the last year I frequently observed, that when the barometer rose (the wind continuing in the same quarter, viz. N. E. or S. W.) the air became many degrees warmer. A similar fact is related from Musschenbroek, in Mr. Kirwan's ingenious work on the Temperature of different Latitudes; viz. that in winter, when the mercury in the barometer descends, the cold increases. More accurate observations on this subject, when the air is stationary, or when the wind continues in the same quarter, might lead to the discovery of the quantity of heat squeezed out of the air by a certain pressure.

The devaporation of aërial moisture.—As heat appears to be the principal cause of evaporation, as well as of solution, and of fluidity in general, the privation of heat may be esteemed the principal cause of devaporation: for though the air may, by its own power of attraction, or by means of the electricity it may contain, dissolve and suspend a portion of water, as water dissolves and suspends a portion of salt; yet, by the application of cold, these are respectively precipitated; and therefore heat may be assumed as the immediate cause of these solutions. Add to this, that water boils in vacuo with less heat; that is, it evaporates in vacuo faster or easier than in the open air, and therefore the attractive power of the atmosphere does not seem necessary to evaporation. Now, when the barometer sinks (from whatever cause not yet understood this may happen) the lower stratum of air becomes expanded by its elasticity, being released from a part of the super-incumbent pressure, and in consequence of its expansion robs the vapour which it contains of its heat; whence that vapour becomes condensed, and is precipitated in showers, as is visible in the receiver of an air-pump above-mentioned.

There are however 2 other curious circumstances belonging to the devaporation of water, which have not been perhaps much attended to. First, that the deduction of a small quantity of heat from a cloud or province of vapour, compared with the quantity of heat which was necessary to raise that vapour from water, will devaporate the whole. This circumstance is evident in the operation of common steam-engines, in which a small jet of water, whose heat is often above 48°, perpetually devaporates the steam raised by a comparatively very great quantity of heat under the boiler. This difficult problem is explicable from the principles before established: if a small part of a province of vapour be suddenly condensed, a vacuity takes place, and the contiguous walls of vapour expand themselves into this vacuity; and thus a large area of vapour, perhaps of many miles in circumference, becomes more or less expanded; by this expansion cold is produced, that is, its capacity of receiving heat is increased; and the whole is devaporated.

This very circumstance exactly takes place in the famous steam-engine of Messrs. Watt and Boulton; which, from the happy combination of chemical and mechanic power, may justly be esteemed the first machine of human invention. In this excellent machine, after the cylinder is filled with steam, a communication is opened between this reservoir of steam and a small cell, which is kept cold by surrounding water, and free from air by an air-syringe adapted to it. What then happens? The corner of the steam in the cylinder next to this vacuum, with which it now communicates, rushes into it, and the whole steam in the cylinder is thus suddenly expanded, and instantly devaporated: whence the very quick reciprocations of the piston; and that, though the cylinder itself is always kept as hot as boiling water, that is, as hot as the steam was previous to its devaporation.

Conclusion.—1. When a small portion of air, suppose a few acres, becomes suddenly contracted into a less compass, either by incidental cold, or by any other cause not yet understood (as the combination of dephlogistic and inflammable gases,) the air next in vicinity suddenly expands itself to occupy the vacuity; and by its expansion produces cold and devaporates, and then becomes compressible into less space than it occupied before it parted with its vapour. This then gives occasion to the next circum-ambient portion of air to go through the same process, that is, to expand, attract the heat from its vapours, devaporate, and then become compressible into less space; and thus, from a small and partial contraction or dimunition of air, it seems possible to devaporate a great province.

2. The vapour of a great province of air being thus condensed, would leave a great vacuity in that part of the atmosphere, which would be supplied by winds rushing in on all sides. Suppose this to happen to the north of our climate, a south-west wind would be produced here, which is otherwise very difficult to understand: and if it should ever be in the power of human ingenuity to govern the course of the winds, which probably depends on some very small causes; by always keeping the under currents of air from the S. W. and the upper currents from the N. E., I suppose the produce and comfort of this part of the world would be doubled at least to its inhabitants, and the discovery would thence be of greater utility than any that has yet occurred in the annals of mankind.

V. Some Observations on the Heat of Wells and Springs in the Island of Jamaica, and on the Temperature of the Earth below the Surface in Different Climates. By John Hunter, M. D., F. R. S. p. 53.

The great difference, says Dr. H. between the temperature of the open air, and that of deep caverns or mines, has long been taken notice of, both as matter of curiosity and surprize. After thermometers were brought to a tolerable degree of perfection, and meteorological registers were kept with accuracy, it became a problem, to determine what was the cause of this difference between the heat of the air and the heat of the earth; for it was soon found that the temperature of mines and caverns did not depend on any thing peculiar to them; but that a certain depth under ground, whether in a cave, a mine, or a well, was sufficient to produce a very sensible difference in the heat. In observations of this kind, there was perhaps nothing more striking, than that the heat in such caves was nearly the same in summer and winter; and this even in changeable climates, that admitted of great variation between the extremes of heat in summer, and cold in winter. There is an example of this in the cave of the Royal Observatory at Paris. The explanations, which have been attempted of this phænomenon, have turned chiefly on a supposition, that there was an internal source of heat in the earth itself, totally independent of the influence of the sun *. M. de Mairan has bestowed much labour on this subject, and by observation and calculation is led to conclude, that of the 1026° of heat, by Reaumur's scale, which he finds to be the heat of summer at Paris, 34°.02 only proceed from the sun, and the remaining 991°.98 from the earth, by emanations of heat from the centre †. The proportion therefore of heat derived from this latter source is to that of the sun, as 29.16 to 1. It must be evident that an hypothesis of this kind, which renders the influence of the sun of small account, is directly contrary to the general experience and conviction of mankind. Without entering however into any discussion of the data from whence M. de Mairan draws his conclusions, it will be more satisfactory to consider what would be the effect of the operation of those laws of heat with which we are acquainted.

And first, it is well known, that heat in all bodies has a tendency to diffuse itself equally through every part of them, till they become of the same temperature. Again, bodies of a large mass are both cooled and heated slowly. Besides the mass of matter, there are two other considerations of much importance in the slow or quick transmission of heat through bodies; these are their different conducting powers, and their being in a state of solidity or fluidity. The conducting powers of heat are well known to be very various in different bodies; nor are they hitherto reducible to any law, depending either on the density or chemical properties of matter. Metals of all kinds are good conductors of heat, while glass, a heavy, solid, homogeneous body, is an extremely bad conductor, even when a metallic calx enters largely into its composition, as in flint-glass. A state of fluidity greatly promotes the diffusion of heat; for a body in a fluid state, by the particles moving readily among each other from their different densities or other causes, mixes the warm and cold parts together, which occasions a quick communication of heat. To apply these observations to the present subject; the surface of the earth being exposed to the great heats of summer, and the colds of winter, or more properly the low degree of heat of winter, will receive a larger proportion of heat in the former season, and a

* Vid. Martine's Essays, p. 319.

† Memoir. de l'Acad. des Sciences, An. 1719 et 1765.

smaller in the latter; and being further of a large mass, and of a porous and spongy substance, and therefore not quickly sensible to small variations of heat, it will become of a mean temperature at a certain depth, between the heat of summer, and the cold of winter, provided it contain no internal source of heat within itself. This conclusion is strictly agreeable to the experiments and observations hitherto made, in heating and cooling bodies, or in mixing portions of matter of the same kind of different temperatures*. Water, though in a large mass, follows in some degree the heat and cold of our summer and winter, from the mobility of its parts occasioning a more speedy diffusion of heat. Air is quickly susceptible of heat, and from the expansions produced in it, and consequent motions in the whole mass, the temperature is soon rendered uniform.

The changes in the heat of the air are what we have measured, and we are to be understood to speak of them, when we talk of the temperature of summer and of winter. It may be asked then, is the heat of the sun first communicated to the air, and thence to the earth? No, the air is susceptible of a very small degree of heat from the rays of the sun passing through it; for it is well known that they produce no heat in a transparent medium, and consequently, that the air is only so far heated as it differs from a medium that is perfectly transparent. The heat produced by the rays of the sun bears a proportion to their number, their duration, and their angle of incidence; and it takes place at the points where they strike an opaque and non-reflecting surface. The surface of the earth may therefore be considered as the place from which the heat proceeds, which is communicated to the air above, and the earth below. That this is really the case, is evident from the superior degree of heat produced by the action of the rays of the sun on an opaque body, which will often be heated to 150° of Fahrenheit, while the temperature of the air is not above 90°†. It may seem therefore, that to measure the heat communicated to the earth, it should be done at the surface, where the action of the rays immediately takes place. But though the heat be produced at the surface, it is communicated freely to the air as well as the earth; and though the apparent intensity of heat be greater in the earth, from the rays of light acting for a longer time on the same parts of matter, yet there is little doubt that much the greater part is carried off by the air, which as it is heated flies off, and allows a fresh portion of cold air to come in contact with the heated surface. But still it is immaterial, whether the heat of the sun be excited more in the earth or in the air; for whichever has the larger proportion will in the end communicate a part to the other, and so restore the balance. The same observation applies to such causes of cold as may operate at the surface of the earth, as evaporation, &c. The air therefore, near the surface of the earth,

* De Luc Modifications de l'Atmosphere, vol. 1, p. 285.

† Martine's Essays, p. 309.

will show by a thermometer in the shade nearly, if not exactly, the same degree of heat that the sun communicates to our terrestrial globe; and if a mean of the heats thus shown be taken for the year round, and we penetrate into the earth to that depth, that it is no longer affected either by the daily, monthly, or annual variations of heat, the temperature at such depth should be equal to the annual mean above mentioned. To ascertain this with the utmost precision, it must be obvious that numerous observations should be made every day, corresponding to the frequent changes of temperature, which are known to happen in the course of the 24 hours in all climates; and on these a daily mean should be taken, and the annual mean deduced from them. This has not yet been done, but where we have observations from which a mean temperature can be deduced with any degree of certainty, it will be found not to differ greatly from the heat of deep caves, or wells in the same climate.

For obtaining the temperature of the earth, the best observations are probably to be collected from wells of a considerable depth, and in which there is not much water. Springs issuing from the earth, though indicating the temperature of the ground from which they proceed, are not so much to be depended on as wells; for the course of the spring may be derived from high grounds in the neighbourhood, and it will thence be colder; it may run so near the surface as to be liable to variations of heat and cold from summer and winter; or it may be exposed to local causes of heat in the bowels of the earth. Wells seem also better than deep caverns, for the apertures to such are often large, and may admit enough of the external air to occasion some change in their temperature. Wells are not however to be met with in all places, and in that case we must remain satisfied with the temperature of the springs.

The following observations were made in the island of Jamaica, where there are flat lands in many parts towards the coast, but all the interior part of the country is mountainous. The heat is greatest in the low lands, and decreases a you ascend the mountains. The town of Kingston is supplied with water from wells. The ground on which it stands rises with a gentle ascent as you recede from the sea. In the low part of the town the wells are but a few feet deep, and many of them brackish. The heat of the water in some of them I have found as high as 82°; but they were evidently too near the surface not to be affected by the heat of the seasons. As you ascend, the wells are deeper, and the temperature is nearly 80° in all of them. What variations there are, come within 1°, that is, half a degree less than 80°, or half a degree more. They are of different depths, and some not less than 100 feet; though, after they are of half that depth, the temperature is nearly uniform. At the Governor's Pen, which is also in the low part of the country, a well, which is above 60 feet deep, is $79\frac{1}{2}$°. There is a well at Half-way-tree, 243 feet deep, which is 79°. Half-way-tree is 2 miles from Kingston, with a very gentle ascent. Near Rock-Fort is a spring, immediately at the foot of the long mountain, which throws out a great body of water; the heat of it is 79°. All the places mentioned are but very little above the level of the sea, probably not more than the depth of the wells at the respective places; for near Kingston there are springs that appear just below the water-mark of the sea, and those that supply the wells are probably on the same level.

The temperature of the air at Kingston admits but of small variation. The thermometer, at the hottest time of the day, and during the hottest season of the year, ranges from 85° to 90°; in the coolest season, and observed about sun-rise, which is the coldest time in the 24 hours, it ranges from 70° to 77°. I have seen it once as low as 69°, and 2 different times as high as 91°. The annual mean temperature cannot therefore either much exceed, or fall much short of, 80°, as indicated by the wells.

The following springs were examined with much accuracy by the Hon. Mr. Sewell, Attorney-General of the island. Ayscough's spring, on the road from Spanish Town to Pusey's, in St. John's parish, 75°. Pusey's spring, still higher in the mountains, $72\frac{3}{4}$°. A spring near the barracks at Points Hill in St. John's parish, 70°. The thermometer in the shade at Pusey's, during part of the month of June, was found to range from $69\frac{1}{2}$° to $79\frac{1}{2}$°. It was observed both late at night, and early in the morning before sun-rise. The spring in Brailsford Valley, about 10 miles above Spanish Town, is 75°. The spring at Stoney Hill is 71°. These were examined by Mr. Home.

Mr. Wallen's house, at Cold Spring, stands the highest of any in the island. By a measurement, said to have been made by Mr. M'Farlane, it is reported to be 1400 yards above the level of the sea. On the road to it, and about a mile below Mr. Wallen's house, there is a spring that issues from the side of the hill, of the temperature of 65°. Cold Spring, which gives a name to the place, is about 50 feet below the house, and the heat of it is $61\frac{3}{4}$°. The thermometer in the shade at Mr. Wallen's house, for some days in the month of April, ranged from 57° to 67°. It may be remarked, that the higher the springs the colder they are; and, as far as a conjecture can be formed from so few observations, they would appear not to differ much from the mean temperature of their respective places.

It will not be out of place to add some observations made in England, relative to the same subject. The wells in and about London are either of no great depth, or are full of water, which are both considerable objections to their giving a mean temperature. The want of depth will make them subject to the variations of the seasons; and a large quantity of water, even in a deep well, will take the temperature of the air more or less: for any change of temperature communicated at the surface will, from the fluidity of the water, be readily diffused through the whole. It is probably owing to this cause, that the wells in the neighbourhood of Brighthelmstone vary from 50° to 52°, for those were the highest that had most water in them. The observations were made in summer. These wells are of various depths, from 15 to 150 feet. That which is always found the coldest is not more than 22 feet deep; its heat was never greater than 50°. It is near the beach, and is a tide well, that is, the water in it rises and falls, and yet does not correspond exactly with the tides, but follows them with an interval of about 3 hours. At the lowest there is not more than a foot of water in it; and it may be considered as a subterraneous spring running through the bottom of the well. There are in fact numerous springs that break out on the sand, a few feet above the low-water mark, which are doubtless the same that supply the wells. As we are not acquainted with any cause that produces cold in the bowels of the earth, we must necessarily, in every climate, consider the lowest degree of heat as approaching nearest to the mean temperature; and therefore we cannot conclude the mean temperature at Brighthelmstone to be more than 50°. The mean temperature of London is computed about 52°; but Brighthelmstone is nearly 50 miles farther south than London, and is immediately on the sea, and must therefore be at least as warm as London. It is evident that the observations from which the mean is taken, must generally contain more of the extremes of heat than of cold, as the former happen in the day-time, and the latter in the night, in consequence of which they will often escape notice. There is a table, next following this paper, constructed by Dr. Heberden, expressing the heat in London for every month in the year, from a mean of 10 years beginning with 1763, and ending with 1772. The mean temperature is given both at 8 A.M. and 2 P.M. There is further in the table, a column of the mean of the greatest monthly colds in the night, observed during the same 10 years by Lord Charles Cavendish, in Marlborough-street. There will not probably be any great error in considering the heat observed at 2 P.M. as the greatest daily heat; and taking a mean between the greatest heats of the day, and greatest colds of the night, they give 49°.196 for an annual mean, which is much lower than is commonly supposed. At the house of George Glenny, Esq. near Bromley, there is a well 75 feet deep, which in November was $49\frac{1}{2}$°. M. de Mairan has given a table of the greatest heats and greatest colds observed at Paris for 56 years, beginning from 1701; and a mean of them is 10° above freezing, or 1010°, of Reaumur's scale*. The temperature of the cave of the Observatory where those

* Mem. de l'Acad. des Sciences, An. 1765, p. 202.

observations were made, is $10\frac{1}{4}°$ above freezing, by the same scale of Reaumur. There appears not therefore any necessity for an internal heat; on the contrary, it is matter of demonstration, that were there any source of heat in the earth which was not equally in the air, the heat of the interior parts ought to be higher than a mean: and if the central heat bore as high a proportion to that of the sun as M. de Mairan alleges, the heat of the earth itself ought to be a great deal above the mean temperature of the air, which from observation there is no ground for believing. It is easy to see the source of M. de Mairan's error; he has founded his calculations on the scale of Reaumur, and considers the degrees of his thermometer as marking the real proportions, and absolute quantity of heat. It is a matter that cannot be denied, that we know nothing of the absolute quantities of heat; and that the degrees of our thermometers are only to be considered as a few of the middle links of a chain, the length of which we are totally ignorant of, and therefore in no condition to compare its proportional parts. It deserves however to be remarked, that observations of a late date have shown, that the notions of cold on which Reaumur's scale was constructed, and on which M. de Mairan's calculations are founded, are imaginary and without foundation.

The sea admits of change of temperature more quickly than the earth, particularly near the shore. The mean heat of the sea at Brighthelmstone, during the months of July, Aug. Sept. and Oct. was as annexed:

July	$63\frac{1}{7}°$
Aug.	$63\frac{1}{2}°$
Sept.	58°
Oct.	53°

The wells at New York are from 32 to 40 feet in depth, and Dr. Nooth found them to have an annual variation of 2°, from 54° to 56°. There are few countries, in which the annual range of the thermometer is greater than at New York, and the neighbouring parts of America. In the summer it is often as high as 96°, and in winter it has been observed several degrees below the zero of Fahrenheit's scale. On the whole, we may, from all the observations we are yet in possession of, conclude, that there is at present no source of heat in the earth, capable of affecting the temperature of a country, which is not derived from the sun; and that the earth, whatever changes of temperature it may be conjectured to have undergone in former periods, is now reduced to a mean of the heat produced by the sun in different seasons, and in different climates.

VI. A Table of the Mean Heat of every Month for Ten Years in London, from 1763 to 1772 inclusively. By Wm. Heberden, M. D., F. R. S., and A. S. p. 66.

		At 8 A. M.	At 2 P. M.	Mean.	Night.
12	January	35°	39°	37°	34.7°
10	February	38	43	40.5	36.6
9	March	39	45	42	37.1
7	April	44	52	48	41.3
5	May	51	59	55	46.4
3	June	57	65	61	52.4
2	July	59	68	63.5	55.6
1	August	60	68	64	55.1
4	September	55	63	59	51.7
6	October	48	55	51.5	45.5
8	November	43	48	45.5	40
11	December	39	42	40.5	37.3

The first column of figures denotes the order of the months according to their degrees of heat, beginning with August, in which the heat is greatest. The 2d and 3d are the heats marked at the hour expressed at the top of each column, and the 4th is the mean between two. The last column is the mean of the greatest cold at night, observed in Marlborough-street for 20 years, by Lord Charles Cavendish.

VII. On Centripetal Forces. By Edw. Waring, M. D., F. R. S. p. 67.

PROP. 1.—1. Let a curve $\text{P}p\text{N}$ (pl. 4, fig. 3), of which the perpendiculars to the two nearest points P and p of the curve are PO and pO, and consequently O the centre of a circle having the same curvature as the given curve in the point P; draw PY and ly tangents to the curve in the points P and p; from S draw Sy and ShY respectively perpendiculars to the tangents ly and PY; and let ShY cut the tangent ly in h; then will ultimately hY ($-\dot{\text{P}}$) be the decrement of the perpendicular SY = P; and the triangles lhY and POp be similar: for the angles POp and hlY are equal, and the angles lYh and OPp right ones; therefore PO : Pp :: lY ultimately $\doteq$ PY : Yh decrement of the perpendicular, whence $\text{P}p = \frac{\text{Y}h \times \text{PO}}{l\text{Y}} = \frac{\text{Y}h \times \text{PO}}{\text{PY}}$.

1. 2. Fig. 4 and 3. The force in the direction PS is as the ultimate ratio of $2 \times \text{QR}$ (the space through which a body is drawn from the direction of its motion in the tangent in a given time towards the centre of force); but ultimately $2\text{QR} = \frac{2\text{QP}^2}{\text{PV}}$, where QP is as the space described in a given time, and consequently as the velocity (V) of the body at the given point P, and PV the chord of curvature in the direction SP.

1.3. The increment (Pp) of the space divided by the velocity V, is ultimately as the increment of the time, and = the increment of the velocity ($\dot{V}$) divided by the force $\frac{2V^2}{PV} \times \frac{PY}{SP}$ in the direction of the tangent, that is, $\frac{Pp}{V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; for P$p$ substitute $\frac{Yh \times PO}{PY}$, and there results $\frac{-\dot{P} \times PO}{PY \times V} = \frac{\dot{V} \times PV \times SP}{2V^2 \times PY}$; and consequently $\frac{-2\dot{P} \times PO}{SP \times PV} = \frac{\dot{V}}{V}$; but $\frac{SP \times PV}{2PO} = SY = P$, hence $\frac{-\dot{P}}{P} = \frac{\dot{V}}{V}$, and $V = \frac{a}{P}$, where a is an invariable quantity.

Corol. Since V × P, that is, SY the perpendicular multiplied into the velocity (which is ultimately as Pp the space described in a given time) is ultimately as the areas described round the centre S in a given time; but this rectangle = a, a given quantity; therefore the area, described round the centre of force S in a given time, will be a given quantity, and thence in unequal times will be proportional to the times.

1.4. The sagitta QR is ultimately as the force, when the time is given; and when the time is not given, it will be as the force into the square of the time; from which expression, by substituting for QR and the time their values, may be deduced several others. Sir Isaac Newton has demonstrated this proposition with the greatest simplicity; and this is given to show that the same proposition may be deduced from different principles.

PROP. 2.—1. Fig. 3. Given the relation between SP′ the distance from a point S, and SY′ a perpendicular from the point S to P′Y, a line touching a curve in the point P′; to find the relation between Sp' and Sy' (a perpendicular from the point S to $p'y$, a line touching the curve in the point p'); in which two curves PP′L and $pp'l$, the forces and velocities at any equal distances SP and Sp are equal, and consequently the perpendiculars SY and Sy, at the above-mentioned equal distances SP and Sp, are to each other in a given ratio N : n. In the equation expressing the relation between SP′ and SY′, for SP′ and SY′ write respectively Sp and $\frac{Sy' \times N}{n}$, and there results the equation sought: for the distances SP and Sp' being equal, the perpendiculars SY′ and Sy' are as N : n.

Exam. 1. Let S be the focus of a conic section, then will $\frac{1}{4}C^2 \times \frac{D}{T \pm D} = SY^2 = P^2$, where T and C denote its transverse and conjugate axes, and D the distance SP; for P write $\frac{N}{n} \times p$, and there results the equation $\frac{1}{4}C^2 \times \frac{D}{T \pm D} = \frac{N^2}{n^2} \times p^2$, which is an equation to a conic section of the same name (viz. ellipse, parabola, or hyperbola) as the given curve, of which the transverse axis is T, and conjugate $= \frac{C \times n}{N}$, and perpendicular from the focus to the tangent $= p$. If T and C are infinite, and consequently the curve a parabola, and the equation $\frac{1}{4}L \times D = P^2$, then will the latus rectum of the resulting equation be $\frac{L \times n^2}{N^2}$.

Exam. 2. Let S be the centre of the logarithmic spiral, then will the equation be $a \times SP = a \times D = SY = P$, and consequently the resulting equation $a \times D = \frac{N}{n} \times p$, whence $\frac{an}{N} \times D = p$ an equation to a logarithmic spiral having the same centre.

Exam. 3. Let T and C be the semi-conjugate axes of a conic section, and S its centre; then will the equation expressing the relation between the distance D and perpendicular P be $D^2 \pm \frac{T^2C^2}{P^2} = T^2 \pm C^2$; for P write as before $\frac{Np}{n}$, and there results the equation $D^2 \pm \frac{n^2T^2C^2}{N^2p^2} = T^2 \pm C^2$, an equation to a conic section of the same name, of which the transverse and conjugate diameters are respectively two roots (x) of the equation $x^2 \pm \frac{n^2T^2C^2}{N^2x^2} = T^2 \pm C^2$, because in this case $p = D$. The sum or difference of the squares of the transverse and conjugate diameters, in all the resulting equations, will be the same.

Corol. In every equal distance, the chord of curvature passing through the centre of force is the same; for the forces in that direction, and the velocities at every equal altitude are the same.

PROP. 3.—1. Fig. 6 and 5. Given an equation $A = 0$, expressing the relation between the absciss $SM = x$ and ordinate $MP = y$; to find the equation expressing the relation between $SP = \sqrt{(x^2 + y^2)}$ and $SY = P$, the perpendicular from S on the tangent PY. From the equation $A = 0$ find $\dot{x} = B\dot{y}$, which substitute for $\dot{x}$ in the equation $(\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} \times P = \dot{x}y \pm x\dot{y}$ deduced from the similar triangles P*lo*, MTP, and STY, where $lo = \dot{x}$ and $Po = \dot{y}$; let the resulting equation be $C = 0$; reduce the three equations $A = 0$, $C = 0$, and $x^2 + y^2 = SP^2 = D^2$ into one, so that the unknown quantities x and y may be exterminated, and there results an equation expressing the relation between D and P.

Corol. Hence, from the equation expressing the relation between x and y, the absciss and ordinate of a curve, can be deduced an equation expressing the relation between the distance SP and perpendicular SY; and from the equation expressing the relation between the distance SP and SY can be deduced an equation expressing the relation between the distance Sp and perpendicular Sy from the point S to the tangent py of a curve, whose force and velocity at every equal distance is the same as in the given curve, but the direction different.

2. Given an equation $K = 0$, expressing the relation between $SP = D$ and $SY = P$; to find an equation expressing the relation between $SM = x$ and $PM = y$, the absciss and ordinate of the same curve. In the given equation $K = 0$ for D and P write respectively $\sqrt{(x^2 + y^2)}$ and $\frac{y\dot{x} \pm x\dot{y}}{\sqrt{(\dot{y}^2 + \dot{x}^2)}}$, and there results a fluxional equation $L = 0$ of the first order, of which the fluent expresses the general relation between x and y.

Corol. If in the given equation for P be written nP', there results the equation $K' = 0$, which expresses the relation between the perpendicular $sy = P'$ and distance $sp = D'$, of every curve which at equal distances has the same velocity and force tending to S; reduce the equations $K' = 0$, $D = \sqrt{(x^2 + y^2)}$ and $nP' = \frac{y\dot{x} \pm x\dot{y}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}}$ into one, so that D and P′ may be exterminated, and there will result the same fluxional equation of the first order, expressing the relation between x, y, and their fluxions, whatever may be the value of n. The general fluent of this fluxional equation contains the relation between the absciss and ordinates of all curves, which have the same force and velocity at the same distance as the force and velocity in the given curve.

PROP. 4.—1. Let a body move in a given curve PH (fig. 7), of which the velocity (v) at any point P is given: and let the forces f'', f''', &c. tending to all the given centres S″, S‴, &c. (except two S and S′) be given; to find the forces f and f' tending to the two points S and S′.—Draw a line PO perpendicular to the tangent yPy'; and from the given centres S, S′, S″, &c. draw lines Sl and Sy, S′l' and S′y', S″l'' and S″y'', &c. perpendicular to the lines PO and yPy', &c.; then will $\frac{v^2}{PO} = f \times \frac{Pl}{PS} \pm f' \times \frac{Pl'}{PS'} \pm f'' \times \frac{Pl''}{PS''} \pm$ &c. where PO is the radius of the circle having the same curvature as the curve in the point P, and $\frac{-v\dot{v}}{\dot{A}} = f \times \frac{Py}{PS} \pm f \times \frac{Py'}{PS} \pm$ &c. where A denotes the arc of the curve PH; from the data may be deduced all the quantities contained in the above-mentioned two equations, except f and f'; and consequently from the two given simple equations be deduced the forces sought f and f'.

2. Let the velocity of the body moving in the given curve be supposed always uniform; then $f \times \frac{Py}{PS} \pm f' \times \frac{Py'}{PS'} \pm f'' \times \frac{Py'}{PS''} \pm$ &c. $= 0$.

Exam. Let the curve HPl be an ellipse, and the two foci S and S′ the centres of forces; then will $f \times \frac{Py}{SP} = f' \times \frac{Py'}{SP'}$; but the angle SP$y$ = S′Py', and consequently $\frac{Py}{SP} = \frac{Py'}{S'P}$ and $f = f'$; but since $\frac{v^2}{PO} = f \times \frac{Sy}{SP} + f \times \frac{Sy'}{SP} = 2f \times \frac{Sy}{SP}$, and $v = a$, then will $f = \frac{a^2 \times SP}{2Sy \times PO}$ be the force tending to each focus.

In these and the subsequent cases the lines Py, Py', Py'', &c. are to be taken negatively or affirmatively, as they are situated on the same or different sides of P; and in the same manner the lines Pl, Pl', Pl'', &c. are to be taken negatively or affirmatively as they are situated on the same or different sides of the tangent yPy', &c.

3. Let the centres M, M′, M″, M‴, &c. of forces, be points not situated in the plane of the given curve HPI, &c. and the forces f''', f'''', &c. tending to each of the centres M‴, M⁗, &c. (except three M, M′, and M″) be given; to find the forces f, f', and f'' tending to those three points M, M′, and M″.—Draw MS, M′S′, M″S″, &c. perpendicular to the plane HPI, &c. from the above-mentioned points,

and assume the equation $f \times \frac{MS}{\sqrt{(MS^2 + SP^2)}} \pm f' \times \frac{M'S'}{\sqrt{(M'S'^2 + S'P^2)}} \pm f' \times \frac{M''S''}{\sqrt{(M''S''^2 + S''P^2)}} \pm f''' \times \frac{M'''S'''}{\sqrt{(M'''S'''^2 + S'''P^2)}} + \&c. = 0$, and the two preceding equations $\frac{v^2}{PO} = f \times \frac{Pl}{PM} \pm f' \times \frac{Pl'}{PM'} \pm f'' \times \frac{Pl''}{PM''} \pm \&c.$ and $\frac{-v\dot{v}}{\dot{A}} = f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm \&c.$; from the data may be found all the quantities f''', f'''', &c.; and consequently from the above-mentioned equations may be deduced the forces f, f', and f''.

4. Let the body move in different planes, that is, in a curve of double curvature at the same points; draw PR a tangent to the curve at the point P, and PQ an arc of the curve of double curvature; draw also two planes PRV and PRT, cutting one another in the line PR; from the point Q let fall QV and QT perpendicular to those planes respectively, and from the points V and T draw Vv and Tt respectively perpendicular to the line PR; let v be the velocity of a body moving in the given curve at the point P, and assume $\frac{PQ^2}{Vv} = 2C$ and $\frac{PQ^2}{Tt} = 2C'$ respectively; from the given centres of forces M, M$'$, M$''$, M$'''$, &c. draw MS, MS$'$, M$''$S$''$, M$'''$S$'''$, &c.; Ms, M$'s'$, M$''s''$, M$'''s'''$, &c. respectively perpendicular to the two planes RPV and RPT; and PL and Pl perpendicular to the line PR in the same two planes RPV and RPT; and also SP, S$'$P, S$''$P, S$'''$P, &c.; sP, s'P, s''P, &c.: from the points S, S$'$, S$''$, S$'''$, &c. s, s', s'', &c. draw the lines SH, S$'$H$'$, S$''$H$''$, S$'''$H$'''$, &c. sh, $s'h'$, $s''h''$, $s'''h'''$, &c. respectively perpendicular to the lines PL and Pl; and SK, S$'$K$'$, S$''$K$''$, S$'''$K$'''$, &c. sk, $s'k'$, $s''k''$, $s'''k'''$, &c. perpendicular to the line RP; and let the forces f''', f'''', &c. tending to all the points M$'''$, M$''''$, &c. (except three, M, M$'$, and M$''$) be given; then from the three given equations $\frac{v^2}{C} = \frac{PH}{MP} \times f \pm \frac{PH'}{M'P} \times f' \pm \frac{PH''}{M''P} \times f'' \pm \&c.$ and $\frac{v^2}{C'} = \frac{Ph}{MP} \times f \pm \frac{Ph'}{M'P} \times f' \pm \frac{Ph''}{M''P} \times f'' \pm \&c.$ and $\frac{-v\dot{v}}{\dot{A}} = \frac{PK}{MP} \times f \pm \frac{PK'}{M'P} \times f' \pm \frac{PK''}{M''P} \times f'' \pm \frac{PK'''}{M'''P} \times f''' \pm \&c. = \frac{Pk}{MP} \times f \pm \frac{Pk'}{M'P} \times f' \pm \frac{Pk''}{M''P} \times f'' \pm \&c.$ which contain only three unknown quantities, can be deduced the forces f, f', and f'', required, tending to the points M, M$'$, and M$''$.

PROP. 5.—Let a body, acted on by forces tending to any given points S, S$'$, S$''$, &c. move in a given curve; to find its velocity in any point of the curve.—Find the fluent of the fluxion $(f \times \frac{Py}{PS} \pm f' \times \frac{Py'}{PS'} \pm f'' \times \frac{Py''}{PS''} \pm f''' \times \frac{Py'''}{PS'''} \pm \&c.)\, \dot{A} = f\dot{D} \pm f'\dot{D}' \pm f''\dot{D}'' \pm \&c. = -v\dot{v}$ when the forces are all contained in the same plane; or the fluent of $(f \times \frac{Py}{PM} \pm f' \times \frac{Py'}{PM'} \pm f'' \times \frac{Py''}{PM''} \pm \&c.) \times \dot{A}$ (when contained in different planes) $= f \times \dot{PM} \pm f' \times \dot{PM}' \pm f'' \times \dot{PM}'' \pm \&c. = f \times \dot{D} \pm f' \times \dot{D}' \pm f'' \times \dot{D}'' \pm \&c.$; but since f, f', f'', &c. are given functions of the quantities D, D$'$, D$''$, &c. the fluents of $f \times \dot{D}, f' \times \dot{D}', f'' \times \dot{D}''$, &c. can be found; which, when properly corrected, will be as $\frac{1}{2}v^2 = \frac{1}{2}$ the square of the velocity in any point P. A denotes the arc of the curve, and D, D′, D″, &c. the respective distances of the body from the centres of forces.

Corol. The increment of the time of describing any arc of the above-mentioned curve will be as the increment of the arc $= \dot{A}$ divided by the velocity found above, and consequently the time itself will be as the fluent of it properly corrected.

PROP. 6.—1. Let a body move in any curve, and be acted on by forces tending to any given points, S, S′, S″, S‴, &c.; all of which, except the force f tending to the point S, let be given; to find f the force tending to S.—Let Sy, S′y', S″y'', &c. be perpendicular to the tangent Py of the curve at the point P; resolve the forces f, f', f'', f''', &c. tending to S, S′, S″, S‴, &c. respectively into two forces, of which one acts perpendicular to Py, the other, Sl, S′l', S″l'', &c. perpendicular to PO, which is perpendicular to Py; let PO be radius of the circle of the same curvature as the curve, and v the velocity of the body at the point P; then will $\frac{v^2}{PO} = f \times \frac{Sy}{SP} \pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm f''' \times \frac{S'''P}{S'''y'''} \pm$ &c. and $-\frac{v\dot{v}}{\dot{A}} = f \times \frac{Py}{SP} \pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P} \pm f''' \times \frac{Py'''}{S'''P} \pm$ &c.: for $\frac{Sy \times PO}{SP} = \frac{1}{2}$ chord of the circle of curvature, which passes through S, write C; and for $PO \times (\pm f' \times \frac{S'y'}{S'P} \pm f'' \times \frac{S''y''}{S''P} \pm$ &c.) substitute H, and for $\frac{Py}{SP}$ write B; and for $\pm f' \times \frac{Py'}{S'P} \pm f'' \times \frac{Py''}{S''P}$ + &c. substitute D, and the two preceding equations become $v^2 = f \times C + H$ and $-v\dot{v} = (Bf + D)\dot{A}$, where $\dot{A}$ denotes as before the increment of the arc of the curve; from the first equation $v\dot{v} = \frac{f \times \dot{C} + C\dot{f} + \dot{H}}{2} = -(Bf\dot{A} + D\dot{A})$ and consequently $C\dot{f} + (\dot{C} + 2B\dot{A})f + \dot{H} + 2D\dot{A} = 0$, from which fluxional equation may be deduced the force f tending to the centre (S) $= -C^{-1} \times e^{-\int\frac{2B\dot{A}}{C}} \times \int(\dot{H} + 2D\dot{A}) \times e^{\int\frac{2B\dot{A}}{C}}$; where e is the number whose hyp. log. = 1.

Corol. Fig. 3. Let f', f'', f''', &c. be each = 0, then will D = 0, H = 0, and consequently $\int(2\dot{D}A + \dot{H}) \times e^{\int\frac{2B\dot{A}}{C}} = \text{const} = a$, and $f = -a \times C^{-1} \times e^{-\int\frac{2BA}{C}}$, $= \frac{2B \times S\dot{y} \times PO}{C \times Py} = \frac{2S\dot{y}}{Sy} = -aC^{-1} \times e^{\int\frac{2S\dot{y}}{Sy}}$; whence $f = \frac{-a}{Sy^2 \times C}$ as is generally known, where a denotes an invariable quantity.

Corol. The force f being found, the square of the velocity may be deduced from the equation $v^2 = f \times C + H$, and the time from the fluent of the fluxion $\frac{\dot{A}}{v} = \frac{\dot{A}}{\sqrt{(f \times C + H)}}$.

2. Let the body move in a curve of double curvature, and let the forces f'', f''', &c. tending to all the points M″, M‴, &c. (except two, M, and M′,) be given; to find the forces tending to the points M and M′.

Assume the three equations before given in prop. 4, viz. $\frac{v^2}{\mathrm{C}} = \frac{\mathrm{PH}}{\mathrm{MP}} \times f \pm \frac{\mathrm{PH}'}{\mathrm{M'P}} \times f' \pm \frac{\mathrm{PH}''}{\mathrm{M''P}} \times f'' \pm$ &c. $\frac{v^2}{\mathrm{C}'} = \frac{\mathrm{P}h}{\mathrm{MP}} \times f \pm \frac{\mathrm{P}h'}{\mathrm{M'P}} \times f \pm \frac{\mathrm{P}h''}{\mathrm{M''P}} \times f'' \pm$ &c. and $- v\dot{v} = (\frac{\mathrm{PK}}{\mathrm{MP}} \times f \pm \frac{\mathrm{PK}'}{\mathrm{M'P}} \times f' \pm \frac{\mathrm{PK}''}{\mathrm{M''P}} \times f'' \pm \text{&c.} = \frac{\mathrm{P}k}{\mathrm{MP}} \times f \pm \frac{\mathrm{P}k'}{\mathrm{M'P}} \times f' \pm \text{&c.}) \times \dot{\mathrm{A}}$; from the two former may be deduced the equations $v\dot{v} = \alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma}$, and $v\dot{v} = \alpha'\dot{f} + \beta'\dot{f}' + f\dot{\alpha} + f'\dot{\beta}' + \dot{\gamma}'$, where $\alpha = \frac{\mathrm{C} \times \mathrm{PH}}{2\mathrm{MP}}$, $\beta = \pm \frac{\mathrm{C} \times \mathrm{PH}'}{2\mathrm{M'P}}$, $\gamma = \pm \frac{1}{2}\mathrm{C}\,(\frac{\mathrm{PH}'' \times f''}{\mathrm{M''P}} \pm \frac{\mathrm{PH}''' \times f'''}{\mathrm{M'''P}} \pm \text{&c.})$; $\alpha' = \frac{\mathrm{C}' \times \mathrm{P}h}{2\mathrm{MP}}$, $\beta' = \pm \frac{\mathrm{C}' \times \mathrm{P}h'}{2\mathrm{M'P}}$, $\gamma' = \pm \frac{1}{2}\mathrm{C}'\,(\frac{\mathrm{P}h'' \times f''}{\mathrm{M''P''}} \pm \frac{\mathrm{P}h''' \times f'''}{\mathrm{M'''P}} \pm \text{&c.})$; whence may be derived the two equations $\alpha\dot{f} + \beta\dot{f}' + f\dot{\alpha} + f'\dot{\beta} + \dot{\gamma} = \alpha'\dot{f} + \dot{f}' + f\dot{\alpha}' + f'\dot{\beta}' + \dot{\gamma}' = \pi f \pm \rho f' \pm \sigma$, where $\pi = -(\frac{\mathrm{PK}}{\mathrm{MP}} = \frac{\mathrm{P}k}{\mathrm{MP}}) \times \dot{\mathrm{A}}$, $\rho = \pm (\frac{\mathrm{PK}'}{\mathrm{M'P}} = \frac{\mathrm{P}k'}{\mathrm{M'P}}) \times \dot{\mathrm{A}}$, $\sigma = -(\pm \frac{\mathrm{PK}''}{\mathrm{M''P}} \times f'' \pm \frac{\mathrm{PK}'''}{\mathrm{M'''P}} \times f''' \pm \text{&c.} = \text{&c.}) \times \dot{\mathrm{A}}$. Reduce these 2 equations to 1, so that f', f'', &c. and their fluxions may be exterminated; and there results a fluxional equation of the formula $\mathrm{H}\ddot{f} + \mathrm{K}\dot{f} + \mathrm{L}f + \mathrm{M} = 0$, where H, K, L, and M, are functions of one of the before-mentioned variable quantities (for example, $\mathrm{MP} = w$) which may be supposed to flow uniformly, and its fluxion.

PROP. 7.—1. Fig. 8. Given the force tending to any point S, the velocity and direction of the body; to find the curve described.

Let the body acted on by a force f tending to S, at the distance D′ from S, be projected in the direction P′Y′, with a velocity H; and let the perpendicular from S to the tangent P′Y′ be A; from the general fluent of $f \times \dot{\mathrm{D}}$, where D denotes the distance from S, and f is a function of D, properly corrected, find its velocity V at distance D, and consequently the perpendicular SY from the centre S to the tangent PY at distance $\mathrm{D} = \mathrm{SP}$, which will be $\frac{\mathrm{A} \times \mathrm{H}}{\mathrm{V}} = \mathrm{SY}$; but A and H are given quantities, and V a known function of D; therefore SY and $\sqrt{(\mathrm{SP}^2\,(\mathrm{D}^2) - \mathrm{SY}^2)} = \mathrm{PY}$ will be known functions of D; and from the similar triangles SPY and PQT may be deduced $\mathrm{PY} : \mathrm{SY} :: \mathrm{PT} = \dot{\mathrm{D}} : \mathrm{QT}$, and consequently $\mathrm{SP} \times \mathrm{QT} = \mathrm{D} \times \frac{\dot{\mathrm{D}} \times \mathrm{SY}}{\mathrm{PY}}$ (which is a known function of D multiplied into $\dot{\mathrm{D}}$) will be as the increment of the area described round the centre of force, of which the fluent properly corrected is proportional to the area described round the centre of force, and consequently to the time. In like manner, $\frac{\mathrm{S}y \times \dot{\mathrm{D}}}{\mathrm{D} \times \mathrm{PY}} = \frac{\mathrm{QT}}{\mathrm{SP}}$ (proportional to the increment of the angle described by the body round S) is a function of D multiplied into $\dot{\mathrm{D}}$, of which the fluent properly corrected, or angle, will be as a function of D.

1.2. Fig. 9. Given the above-mentioned force, &c.; to find an equation expressing the relation between the absciss $\mathrm{SM} = x$ and ordinate $\mathrm{MP} = y$ of the curve described, and their fluxions.—From the similar triangles Ppo and LPM can be deduced $po = \dot{y} : \text{OP} = \dot{x} :: \text{PM} = y : \text{LM} = \frac{y\dot{x}}{\dot{y}}$; but $\text{LM} \pm \text{SM} = \frac{y\dot{x}}{\dot{y}} \pm x = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} = \text{LS}$; and consequently $\text{P}p = \sqrt{(\dot{x}^2 + \dot{y}^2)} : po = \dot{y} :: \text{LS} = \frac{y\dot{x} \pm x\dot{y}}{\dot{y}} : \text{SY} = \frac{y\dot{x} \pm x\dot{y}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}}$; but SY is a function to be deduced as above of $\text{SP} = \sqrt{(x^2 + y^2)}$, whence the fluxional equation $\frac{y\dot{x} \pm x\dot{y}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}} = \varphi : (x^2 + y^2)$.

2. Fig. 10. Let a body be acted on by any number of forces (f, f', f'', f''', &c.) in the same plane, tending to the given points S, S′, S″, S‴, &c.; to find an equation expressing the relation between SP = D and S′P = D′, and their fluxions, where P is a point situated in the curve which the body describes.—Suppose YP a tangent to the curve at the point P, and PZ perpendicular to it; and resolve all the forces tending to S, S′, S″, &c. respectively into two others; one in the direction PY, and the other in the direction PZ; substitute for SP, S′P, S″P, S‴P, &c. respectively D, D′, D″, D‴, &c.; and suppose SY, S′Y′, S″Y″, S‴Y‴, &c. perpendicular to the line PY: then will the triangles PQT and SPY, PQT′ and S′PY′ be similar, where PQ denotes a very small arc, and QT and QT′ are perpendicular to the lines SP and S′P; hence $\text{PQ} = \frac{\text{PT} \times \text{SP}}{\text{PY}} = \frac{\dot{\text{D}} \times \text{D}}{\text{PY}} = \frac{\text{PT}' \times \text{S}'\text{P}}{\text{PY}'} = \frac{\dot{\text{D}}' \times \text{D}'}{\text{PY}'}$; and consequently $\text{PY} : \text{PY}' :: \dot{\text{D}} \times \text{D} : \dot{\text{D}}' \times \text{D}'$; and if the quantities D, D′, $\dot{\text{D}}$ and $\dot{\text{D}}'$ are given, the ratio of PY : PY′ will be given; which being given, together with the line $\text{SS}' = a$, the lines PY and PY′, SY and S′Y′, can be found; for, drawing SL parallel to PY, and meeting S′Y′ in L, let $\text{PY}' = m \times \text{PY}$, then $\text{YY}' = (m \pm 1)\,\text{PY} = \text{SL}$, $\text{SY} = \sqrt{(\text{SP}^2 - \text{PY}^2)} = \sqrt{(\text{D}^2 - \text{PY}^2)}$, $\text{S}'\text{Y}' = \sqrt{(\text{S}'\text{P}^2 - \text{PY}'^2)} = (\text{D}'^2 - m^2 \times \text{PY}^2)$, $\text{LS}' = \text{S}'\text{Y}' \pm \text{SY} = \pm \sqrt{(\text{D}'^2 - m^2\text{PY}^2)} \pm \sqrt{(\text{D}^2 - \text{PY}^2)}$; and $\text{SS}'^2 = \text{SL}^2 + \text{LS}'^2$, an equation in which all quantities (except PY) are given, and consequently PY is determined by an equation, which will be a quadratic; but PY being found, from thence PY′, SY and S′Y′ may be deduced, which are consequently all functions of D, D′, $\dot{\text{D}}$, $\dot{\text{D}}'$, and invariable quantities; and their fluxions $\dot{\text{PY}}'$, $\dot{\text{SY}}$, and $\dot{\text{S}'\text{Y}'}$, functions of D, D′, $\dot{\text{D}}$, $\dot{\text{D}}'$, $\ddot{\text{D}}$, and $\ddot{\text{D}}'$: from the similar triangles before given $\dot{\text{SY}} : \text{PQ} = \frac{\text{D}\dot{\text{D}}}{\text{PY}} :: \text{PY} : \frac{\text{D}\dot{\text{D}}}{\dot{\text{SY}}} = \text{PO}$ the radius of curvature; hence PO is a function of D, D′, $\dot{\text{D}}$, $\dot{\text{D}}'$, and $\ddot{\text{D}}'$, if $\ddot{\text{D}} = 0$; and from D, D′, SS′, $\dot{\text{D}}$, $\dot{\text{D}}'$, and the point S″ given in position can be determined S″P, S″Y″ and PY″; for let $\text{S}''h = \text{C}$ be drawn perpendicular to $\text{SS}' = a$, and $\text{S}h = b$; then will $\text{S}l$ (if $\text{P}l$ be a perpendicular from the point P to the line SS′) $= \pm \frac{a^2 + \text{D}^2 - \text{D}'^2}{2a}$, and $\text{S}'l = \pm \frac{a^2 + \text{D}'^2 - \text{D}^2}{2a}$, and $\text{P}l = \sqrt{(\text{SP}^2 - \text{S}l^2)}$, and $\text{S}''\text{P} = \sqrt{((b \pm \text{S}l)^2 + (\text{C} \pm \text{P}l)^2)}$; draw S″Y″ perpendicular to the tangent PY, and cutting the lines SS′ and SK parallel to PY in o and n respectively; then will $oh = \frac{\text{C} \times \sqrt{(\text{SS}'^2 - (\text{PY} \pm \text{PY}')^2)}}{(\text{PY} \pm \text{PY}') = l}$, $\text{S}''o = \frac{\text{C} \times \text{SS}'}{l}$; (and from the similar triangles S″oh and Son) $on = (b \pm oh) \times \frac{oh}{\text{S}''o}$; whence $\text{S}''\text{Y}'' = \pm \text{S}''o \pm on \pm \text{SY}$ will be a known function of D, D′, $\dot{\text{D}}$ and $\dot{\text{D}}'$, and invariable quan-

tities: the same may be predicated of similar lines drawn to the centres s‴, s⁗, &c.; and consequently $(f \times \frac{\text{PY}}{\text{SP}} \pm f \times \frac{\text{PY}'}{\text{S}'\text{P}} \pm f'' \times \frac{\text{PY}''}{\text{S}''\text{P}} \pm f''' \times \frac{\text{PY}'''}{\text{S}'''\text{P}} \pm \text{\&c.}) \times \dot{\text{A}}$ (where $\dot{\text{A}}$, as before, denotes the fluxion of the arc of the curve) $= f \times \dot{\text{D}} \pm f' \times \dot{\text{D}}' \pm f'' \times \dot{\text{D}}'' \pm f''' \times \dot{\text{D}}''' \pm \text{\&c.} = -v\dot{v}$, if v denotes the velocity; but as f, f', f'', f''', &c. are functions of D, D′, D″, D‴, &c. respectively, the fluent of the above-mentioned quantity $f\dot{\text{D}} \pm f'\dot{\text{D}}' \pm f''\dot{\text{D}}'' \pm$ &c. can be found in terms of D, D′, D″, D‴, &c. from the fluents of the fluxions $f\dot{\text{D}}, f'\dot{\text{D}}'$, &c.; and consequently in terms of D and D′, which let be Z, then will $\text{Z} = \frac{-v^2}{2}$; but $v^2 = -2\text{Z} = \text{PO} \times (f \times \frac{\text{SY}}{\text{SP}} \pm f' \times \frac{\text{S}'\text{Y}'}{\text{S}'\text{P}} \pm f'' \times \frac{\text{S}''\text{Y}''}{\text{S}''\text{P}} \pm f''' \times \frac{\text{S}'''\text{Y}'''}{\text{S}'''\text{P}} \pm$ &c.) a fluxional equation of the second order expressing the relation between D and D′, and their fluxions.

2. To find an equation expressing the relation between $x = \text{SM}$ and $y = \text{MP}$, where SM (x) is the absciss beginning from S and continued in the line SS′, and MP (y) the perpendicular ordinate of the curve described by a body acted on by the above-mentioned forces; in the fluxional equation found before for D and D′ and their fluxions substitute $(x^2 + y^2)^{\frac{1}{2}}$ and $((\text{SS}' \pm x)^2 + y^2)^{\frac{1}{2}}$ and their fluxions, and there results the equation sought.

Corol. It easily appears, that the general fluent may contain two invariable quantities to be assumed at will, or according to the conditions of the problem; that is, at a given distance the velocity and the direction may be assumed at will, and consequently the general fluxional equation, expressing the above-mentioned relation, will be of the second order, if no fluents are contained in it.

Corol. From Py and Py', and the points S and S′ being given, can easily be deduced geometrically the direction of the tangent and the lines Sy, Sy', &c.; for divide the line SS′ in r, so that Py ± Py' : SS′ :: Py : Sr, and through r draw the line Pr, the perpendicular to Pr through P will be the tangent yPy'; to this line the perpendiculars from S and S′ will be the lines Sy and S′y' required.

Corol. From the fluent of the above-mentioned fluxional equation may be deduced the velocity V in terms of D and D′; and from the fluent of $\frac{\text{D} \times \dot{\text{D}}}{\text{P}y \times \text{V}}$, which is a function of D multiplied into $\dot{\text{D}}$, may be deduced the time.

3. If the plane in which the body (P) moves, and all the forces f', f'', f''', &c. tending to points M′, M″, M‴, &c. not situated in the same plane (except one f tending to a given point M) be given; then the force tending to that point can be found, and the curve described. Resolve all the forces tending to the points M, M′, M″, M‴, &c. into two others; one MS, M′S, M″S, M‴S, &c. perpendicular to the plane in which the body moves, and the other SP, S′P, S″P, S‴P, &c. in the plane; then will $f \times \frac{\text{MS}}{\text{MP}} \pm f' \times \frac{\text{M}'\text{S}'}{\text{M}'\text{P}} \pm f'' \times \frac{\text{M}''\text{S}''}{\text{M}''\text{P}} \pm \text{\&c.} = 0$, from which equation f the force tending to the point M may be found; then, from the preceding

proposition find the curve which a body, agitated by forces $f \times \frac{\text{SP}}{\text{MP}}$, $f' \times \frac{\text{S}'\text{P}}{\text{M}'\text{P}}$, $f'' \times \frac{\text{S}''\text{P}}{\text{M}''\text{P}}$, &c. tending to the points S, S′, S″, &c. describes, and it will be the curve required.

4. If the body moves in a curve of double curvature, and the forces f, f', f'', &c. tending to all the centres M, M′, M″, M‴, &c. be given; from the fluent of the fluxional quantity $(f \times \frac{\text{P}y}{\text{MP}} \pm f' \times \frac{\text{P}y'}{\text{M}'\text{P}} \pm f'' \times \frac{\text{P}y''}{\text{M}''\text{P}} \pm f''' \times \frac{\text{P}y'''}{\text{M}'''\text{P}}$ &c.$) \times \dot{\text{A}}$ (A denoting the same quantity as before) $= f \times \text{M}\dot{\text{P}} \pm f' \times \text{M}'\dot{\text{P}} \pm f'' \times \text{M}''\dot{\text{P}} \pm f''' \times \text{M}'''\dot{\text{P}} \pm$ &c. $= f \times \dot{\text{D}} \pm f' \times \dot{\text{D}}' \pm f'' \times \dot{\text{D}}'' \pm f''' \times \dot{\text{D}}''' \pm$ &c. $= \dot{z} = -v\dot{v}$ (f, f', f'', f''', &c. being given functions of D, D′, D″, &c. respectively) can be deduced the square of the velocity $= -2z$, which will be a function of D, D′, D″, D″, D‴, &c., and consequently a function of D, D′, D″, easily to be derived: substitute this function $-2z$ for v^2 in the two following equations $\frac{v^2}{\text{R}'} = \text{F}'$ and $\frac{v^2}{\text{R}''} = \text{F}''$, where R′ and R″ denote the radii of curvature in two different planes of which the tangent above-mentioned in prob. 4, art. 4, is their intersection, and F′ and F″ the sum of the forces in lines perpendicular to the tangent, and in the respective planes: from these forces, calculated in terms of the distances from 3 given points D, D′, and D″; or in terms of 2 abscissæ and 1 ordinate, and from the radii R′ and R″ may be deduced 2 fluxional equations of the 2d order, expressing the relation between 3 distances D, D′, and D″, &c. which may always be reduced to 1 fluxional equation of the 4th order, expressing the relation between 1 absciss and its correspondent ordinates, or the distances from 2 given points.

5. The general fluxional equation expressing the relation between the distances from 2 given points will be of the 4th order, if no fluents are contained in it; for it admits of 4 different quantities to be assumed at will, or according to the conditions of the problem.

6. If some points, to which the forces tend, are situated at an infinite distance; that is, some forces always act parallel to themselves; from the given forces acting either to given points, or in parallel directions, by the equation $f \times \dot{\text{D}} \pm f' \times \dot{\text{D}}' \pm f'' \times \dot{\text{D}}'' \pm$ &c. $= -\dot{z}$, can be deduced the square of the velocity at a point P in terms of the distances from 2 given points, or of an absciss and ordinate; if the centres, &c. and parallel forces are all situated in the same plane: or in terms of the distances from 3 points, or 2 abscissæ and an ordinate, if situate in different planes; from the centres, &c. and forces given, find the sum F of the forces in any direction (PL) (the direction of the tangent excepted) acting on the body at the point P, and the chord of curvature C of the curve at the same point and in the same direction; in the equation $v^2 = \frac{1}{2}\text{F} \times \text{C}$ for v^2 substitute the value before found, and there results an equation expressing the relation between the distances from 2 points, or an absciss and ordinate, &c. if the forces act in the same plane: but if the forces act in different planes,

find the sum F and F′ of the forces at the point P in directions which are not both situated in one plane with the tangent and each other; and also the chords C and C′ of curvature in those directions in terms of the distances from 3 points, or 2 abscissæ and 1 ordinate, &c. In the equations $v^2 = \frac{1}{2}F \times C$ and $v^2 = \frac{1}{2}F' \times C'$ for v^2 substitute its value found from the principles before given; and there result 2 fluxional equations of the 2d order, expressing the relation between the distances from 3 points, or 2 abscissæ and an ordinate, &c.

PROP. 8.—Fig. 11. Let a body move in a curve P*p*, &c. and be acted on at P′ by a force *f*, (which is as any function of the distance SP′) tending to S; let the velocities at P and *p* be represented by the lines YP and *yp* in the direction of the tangents to the points P and *p*; resolve these forces YP and *yp* into 2 others Y*k* and *k*P, and *yl* and *lp*, of which one *k*Y and *yl* is parallel to the line SL; the other *k*P and *lp* is parallel to MP; let a body fall in the right line LS, and the force acting on the body at M′ be to the force acting on the body moving in the curve at P′ :: SM′ : SP′, and P′M′, PM and *pm* be perpendicular to SL; then if the velocity of the body falling in the right line SL at the point M be *k*Y, the velocity of the body at the point *m* acted on by the above-mentioned forces will be *yl*. This is easily demonstrated from the resolution of forces.

2. Through S draw SN parallel to PM or *pm*, &c., and assume in the line (SN) SP = PM and S*p* = *pm*, and let the force at P′ in the line SN and distance = M′P′: the force of the body moving in the curve at the distance P′S :: P′M′ : SP′; then if the velocity at the distance SP = PM be P*k*, the velocity at the distance S*p* = *pm* will be *pl*.

Cor. The force in the direction of the line SL vanishes in the point where a perpendicular SN to the line SL passing through the point S cuts the curve, and consequently the velocity in the direction of SL in that point is the greatest or least, &c.; but if the tangent of the curve be perpendicular in any point to LS, then the velocity in the direction LS is nothing: the same may be applied to the velocity in any other direction.

Exam. Fig. 12. Let a body move in the circumference of a circle SPA, of which the centre of force is a point S in the circumference; it is known that the force in the direction and at the distance SP is as SP^{-5}; but the force in the direction SP is by the hypothesis to the direction (SA) :: SP : SM, if PM be perpendicular to SM, and consequently the force in the direction (SA) is as $SM \times SP^{-6}$; but if AS be a diameter, $AS \times SM = SP^2$; therefore $SM \times SP^{-6} = SM \times AS^{-3} \times SM^{-3} = \frac{SM^{-2}}{AS^3}$; and the diameter AS being given, the force in the line SA varies as SM^{-2}, that is, inversely as the square of the distance: if the force varies as $SM^{-2} = x^{-2}$, then $v\dot{v}$ will vary as $-\frac{\dot{x}}{x^2}$, where v denotes the velocity; and v^2 will vary as $\frac{1}{x} - \frac{1}{SA}$, which agrees with the square of the velocity deduced from the preceding principles; for $v = \text{PY}$, the velocity at P, is inversely as the perpendicular $\text{SY} = \text{SM}$ let fall from the centre of force on the tangent; but $\text{SA}^2 : 2\text{SP} \times \text{PA} ::$ velocity PY as $\frac{1}{\text{SY}} = \frac{1}{\text{SM}}$: Pl the velocity at M; whence $\text{P}l^2$ (the square of the velocity at M) $= \frac{4\text{SP}^2 \times \text{PA}^2}{\text{SA}^4} \times \text{PY}^2$, which varies as $\frac{4\text{SP}^2 \times \text{PA}^2}{\text{SA}^4} \times \frac{1}{\text{SM}^2} = \frac{4\text{PA}^2}{\text{SA}^3 \times \text{SM}} = \frac{4\text{SA}^2 - 4\text{SA} \times x}{\text{SA}^3 \times x}$, and consequently as $\frac{1}{x} - \frac{1}{\text{SA}}$, the same as above.

2. Fig. 11. If any number of forces act on a body at P in any given directions parallel, or tending to given points; resolve all the forces into 2 others; 1 in a given direction SM, and the other in a direction PM perpendicular to it, of which let F be the sum of the forces resulting in the direction MmS, and f the sum of the forces resulting in the direction PM; resolve the velocity V of the body at P, which is in the direction of the tangent PY, into 2 others V′ and V″, one in the direction parallel to the line SM, and the other perpendicular to it: in the same manner resolve the velocity v of the body at p, which is in the direction of the tangent py, into 2 others v' and v'', one in the direction parallel to the line SM, and the other perpendicular to it: then if the velocity of the body moving in the right line SM at M be V′, and it be constantly acted on by a force = F, the velocity of the body at m will be v': and if the body move from P in a direction perpendicular to SM with a velocity as V″, and be always acted on by a force f, the velocity at the distance PM — pm will be v''.

Cor. From the forces given and the velocities in the above-mentioned directions at the point P, can be deduced the velocities in the same directions at the point p, and consequently the tangent to the curve at the point p.

Prop. 9.—1. Let the resistance of a body, moving in a right line, be as any function V of the velocity $\dot{v}$; then will $\dot{t} = \frac{\dot{v}}{\text{V}}$, $\dot{x} = \frac{-v\dot{v}}{\text{V}}$; where $\dot{t}$, $\dot{v}$, and $\dot{x}$, denote the increments of time, velocity, and space; their fluents properly corrected will give the time and space in terms of the velocity.

2. Let a body move in a right line, and be acted on by an accelerating force in that line, which varies as any function X of the distance x from a given point; and resisted by a force which is as any function V of the velocity v into its density X′, which varies also as a function of x and v, then will $(\text{X} + a\text{VX}')\,\dot{x} = -v\dot{v}$, from its fluent x can be found in terms of v, or v in terms of x; and thence $\dot{t} = \frac{\dot{v}}{\text{X} + a\text{VX}'}$, of which the fluent properly corrected gives the time.

Exam. 1. Let $\text{V} = v^2$, and X′ a function of x; that is, let the resistance be as the square of the velocity and density; whence $(\text{X} + av^2\text{X}')\,\dot{x} = -v\dot{v}$, of which equation the fluential will be

$$e^{\int 2a\text{X}'\dot{x}} \times \tfrac{1}{2}v^2 = -\int e^{\int 2a\text{X}'\dot{x}} \times \text{X}\dot{x} + \text{A}, \text{ and } t = \int \frac{\dot{x}}{\sqrt{-(e^{-2\int a\text{X}'\dot{x}} \times (\int e^{\int 2a\text{X}'\dot{x}} \times \text{X}\dot{x} + \text{A}))}}$$

+ B, where A and B are invariable quantities to be assumed according to the conditions of the problem.

1. 2. Let $e^{x} = \mathrm{x}'$ and $\mathrm{x} = b$, which is supposed to correspond nearly to the state of our atmosphere; then will $v^2 = -2 \times e^{-\int 2a\mathrm{x}'\dot{x}} \times \int e^{\int 2a\mathrm{x}'\dot{x}} \times \mathrm{x}\dot{x} = -2 \times e^{-\int 2ae^{x}\dot{x}} \int e^{\int 2ae^{x}\dot{x}} \times b\dot{x} = -2e^{-2ae^{x} - h} - (\int e^{2ae^{x} + h} \times b\dot{x} + \mathrm{A})$, e being the number, whose hyp. log. is 1, and h and A quantities to be assumed according to the conditions of the problem.

1. 3. Let $\mathrm{x} = \mathrm{x}'$; then it becomes $\mathrm{x}\dot{x} = \frac{-v\dot{v}}{1 + a\mathrm{v}}$ and $\dot{t} = \frac{\dot{v}}{\mathrm{x}(1 + a\mathrm{v})}$.

2. Let x be an homogeneous function of one dimension of x, that is, $= ax$, and v a similar function of n dimensions of v, that is $= bv^{n}$, and x' a similar function of r dimensions of x and v, and $n + r = 1$; then by substituting zx and its fluxion for v and its fluxion, can be found the fluent of the fluxional equation $(\mathrm{x} + a\mathrm{v}\mathrm{x}')\,\dot{x} = -v\dot{v}$, and consequently the velocity and time by the quadrature of curves in terms of the space; and in like manner of many other cases.

3. Fig. 6. Let a body, moving in a given curve, be acted on at any point P by a force f tending to a given point S, and resisted by a medium proportional to V, a function of its velocity multiplied into its density x', a function of the distance $\mathrm{SP} = \mathrm{D}$; to find its velocity, time, and distance from the given point S in terms of each other. Let $\mathrm{F} = f \times \frac{\mathrm{PY}}{\mathrm{SP}}$ the force in the direction of the tangent PY, and consequently $(\mathrm{F} + \mathrm{V}\mathrm{x}')\,\dot{\mathrm{A}} = -v\dot{v}$, and $v^2 = \frac{1}{2}\,\mathrm{C} \times f$, where $\dot{\mathrm{A}}$ is the increment of the arc, and C the chord of curvature in the direction SP; but since the curve is given, the chord of curvature may be deduced from the distance, &c. and the increment $\dot{\mathrm{A}}$ of the arc from a function of the distance multiplied into the increment of the distance; then, if f or v be a given function of the distance, the other may be deduced from it, and consequently $-v\dot{v} = \varphi : (\mathrm{D}) \times \dot{\mathrm{D}}$ will be a given function of the distance D multiplied into $\dot{\mathrm{D}}$, whence we have $\varphi : (\mathrm{D}) \times \dot{\mathrm{D}} = \dot{\mathrm{D}}\,(f \times \frac{\mathrm{PY}}{\mathrm{SP}} + \mathrm{x}'\mathrm{V})$ divide by $\dot{\mathrm{D}}$, and there results an algebraical equation, from which $\mathrm{V} \times \mathrm{x}'$ may be found.

If neither v nor f be given, reduce the 2 equations $(f \times \frac{\mathrm{PY}}{\mathrm{SP}} + \mathrm{V}\mathrm{x}')\,\dot{\mathrm{A}} = -v\dot{v}$ and $v^2 = \frac{1}{2}\mathrm{C}f$, into 1, so as to exterminate either f or v and its fluxions; and there results an equation expressing the relation between the other v or f and D and their fluxions: from the velocity given in terms of D may be deduced the time from the equation $\dot{t} = \frac{\dot{\mathrm{A}}}{\mathrm{V}}$.

3. 2. If the body be acted on by forces tending to more points S, S′, S″, S‴, &c. in the same plane; resolve each of the forces into 2; one in the direction of the tangent, and the other perpendicular to it; let the sum of the forces in the direction of the tangent be F; and in the direction perpendicular to it be F′; and 2R the diameter of curvature at the point P, which will be given in terms of the distances from 2 points, or of an absciss and ordinate, and their fluxions, &c.:

assume the 2 equations before given $(\text{F} + \text{X}'\text{V})\,\dot{\text{A}} = -\,v\dot{v}$ and $v^2 = \text{F}'\text{R}$, and since $\dot{\text{A}}$ is always given in terms of D and $\dot{\text{D}}$, if F and F′ be given in terms of D, D′, &c. the value of $\text{V} \times \text{X}'$ may be acquired by a simple algebraical equation; but if F and F′ be not given, and consequently v not given, but V a given function of v, and X′ a given function of the above-mentioned distances; then substitute for v its value $\surd(\text{F}'\text{R})$ in the function V, and the fluxion of $\frac{1}{2}\text{F}'\text{R}$ for $v\dot{v}$, and there will result an equation involving D and F′ and their fluxions, and F; but if the forces tending to all the points but one are given in terms of the distance D, or absciss or ordinate of the curve, and their fluxions; then from F′ can be found F, and, vice versâ, from F can be found F′, and consequently there results a fluxional equation expressing the relation between F or F′ and the distance D or D′, &c. or absciss or ordinate, and their fluxions. From F and F′, and consequently v being found in terms of D, D′, &c. can be deduced $\dot{t} = \frac{v}{\text{F}}$.

The same method may be applied, if some forces tend to an infinite distance, that is, act parallel to themselves, and others tend to given points.

Exam. Let the accelerating force be directly as the arc $= x$, and the resistance uniform $= a$; then will $(x - a)\,\dot{x} = -\,v\dot{v}$, and consequently $x^2 - 2ax + \text{B} = -\,v^2$, let A be the arc, where the velocity $= 0$; then will the equation $\text{A}^2 - 2a\text{A} - x^2 + 2ax = v^2$, and the increment of the time $\dot{t} = \frac{\dot{x}}{\text{V}} = \frac{\dot{x}}{\surd(\text{A}^2 - 2a\text{A} - x^2 + 2ax)}$, whose integral is $\frac{1}{\text{A} - a} \times$ arc of a circle, of which the radius is $\text{A} - a$ and cos. $= x - a$, where A is the distance of the point from which the body begins to fall, and the lowest point of the curve; and the accelerating force $x - a$ is as the distance from a point (a) of a curve, of which the distance from the lowest is a.

Corol. The times of the body falling from any point of the curve to a will be equal.

Corol. The body on this hypothesis will either rest at the point a, or at the lowest point, or any point between $+\,a$ and $-\,a$; for it may rest at any point, where the resisting force is always equal to or greater than the accelerating force.

Corol. Let n be the number of vibrations; then the distance of the arc, to which it will ascend from the lowest point at n vibrations, will be $\text{A} - 2na$; if $\text{A} - 2na$ be not greater than $2a$, it will never pass the lowest point.

Philosophical inquiries require some corrections, which do not enter into mathematical calculus; for example, in some cases the calculus changes the quantities from negative to affirmative, &c. when from philosophical considerations they are not changed; and, vice versâ, they may be changed to affirmative, &c. on philosophical considerations, when they are not changed from the calculus: and also a body may stop, &c. from philosophical considerations, as in the preceding example, when it does not follow from the algebraical calculus, &c. It is further to be observed, that resistances are always to be taken affirmatively.

Exam. 2. Let the accelerating force be as the arc, that is, the distance from the lowest point, and the resistance as the velocity; then will the fluxional equation $(F - V)\dot{A} = -v\dot{v}$ be $(ax - v)\dot{x} = -v\dot{v}$, which is an homogeneous equation of the first order: write in it zx for v, and its fluxion for $\dot{v}$, and there results the equation $(ax - zx) \times \dot{x} = -zx^2\dot{z} - z^2x\dot{x}$, whence $(a - z)\dot{x} = -zx\dot{z} - z^2\dot{x}$, and $\frac{\dot{x}}{x} = \frac{-z\dot{z}}{a - z + z^2}$, and thence log. $x = -\frac{1}{2}$ log. $(a - z + z^2)$ (W) $- \frac{2}{4a - 1} \times$ cir. arc, whose radius is $\frac{1}{2}\sqrt{(4a - 1)}$ and tangent $(z - \frac{1}{2}) + B$; whence can be found $v = xz$, and from curvilinear areas $\dot{t} = \frac{\dot{x}}{v}$.

If $4a$ be less than 1; then it becomes log. $x = W - \frac{1}{4\sqrt{(\frac{1}{4} - a)}} \times \log. \frac{z - \sqrt{(\frac{1}{4} - a)} - \frac{1}{2}}{z + \sqrt{(\frac{1}{4} - a)} - \frac{1}{2}} + B$; where B is an invariable quantity to be assumed according to the conditions of the problem.

Corol. If the force be directly as the distance, or as the arc of the curve from the body to the lowest point, and the resistance as the velocity; then will the velocity in one arc be to the velocity in the corresponding point of another arc, as the arcs to be described; and consequently the times equal.

4. If the body is acted on by forces tending to points S, S′, S″, &c. situated in different planes; then let F be the sum of the forces in the direction of the tangent at the point P; F′ and F″ the sum of the forces acting on the body in 2 different directions at the same point, which are not situated in the same plane with the tangent and each other; from the 3 equations $(F + X'V)\dot{A} = -\dot{v}v$ and $\frac{v^2}{C} = \frac{1}{2}F'$ and $\frac{v^2}{C'} = \frac{1}{2}F''$, in which the same letters denote the same quantities as before, and C and C′ denotes the chords of curvature in the same directions as the forces F′ and F″, which from the curve being given can be found at any point; and if F′ or F″ is given in terms of the distance from a given point, or an absciss or ordinate, &c. the velocity v can be found in terms of the same, and X′V by a simple algebraical equation: if F′ is not given, and V is a given function of v, substitute in V for v its value $\sqrt{(\frac{1}{2}C \times F')}$, and there results an equation expressing the relation between F (which can be deduced from F′ or F″) and the distance of the body from some given point, or the abscissæ and ordinates of the curve required, and their fluxions. If some of the forces act in parallel directions; the forces, velocities, &c. may be found by the same method.

PROP. 10.—Fig. 13. Let a body be projected in a direction HL with a given velocity, and be acted on by a force in a direction parallel to $AP = x$, which varies as X a function of x; and also by another force in a direction parallel to $MP = y$, that is perpendicular to AP, which force varies as Y a function of y, and let it move in a medium, of which the resistance is proportional to the velocity; to find the curve described. Find the fluent of $(X + av)\dot{x} = -v\dot{v}$, which corrected according to the conditions of the problem (viz. so that v at the point H may be to the velocity of projection :: HC : Hb, where bc is drawn perpendicular to AP) suppose $v = x'$; find the fluent of $\frac{\dot{x}}{x'}$, which corrected so as to become $= 0$, when $x =$ AH, let be x''. In the same manner find the fluent of $(Y + a'v')\,\dot{y} = -\dot{v}'v$, which corrected, so that v' at the point H may be to the velocity of projection :: cb : Hb, suppose $v' = Y'$; find the fluent of $\frac{\dot{y}}{Y'}$, which corrected so as to become $= 0$, when PM $= 0$, let be $= Y''$; assume $x'' = Y''$, and thence from x find y: take AP $= x$ and PM $= y$, and M will be a point in the curve, which a body projected in the line HL describes; and if Mm in the direction parallel to HAP : mo perpendicular to it :: velocity v : velocity v' then will Mo be a tangent to the curve in the point M.

2. If a body is acted on by forces tending to any given points S, S′, S″, &c. which vary as given functions of their distances from the body, and resisted by a force which varies according to a given function V of the velocity (v) into its density X′, where X′ varies according to some function of the distances from the given points, &c.; to find the curve described.

1. From the distances of the body from 2 given points, or the absciss and ordinate of the curve described, and their fluxions, &c. find the forces acting in the direction of the tangent to the curve, and in some other direction, which suppose F and F′; and also the chord of curvature in the above-mentioned direction, which let be C; then from the equations $(F + V \times X')\,\dot{A} = -\dot{v}v$ and $v^2 = \frac{1}{2}C \times F$ reduced into one by writing for v its value in the function V, and for $\dot{v}v$ its value deduced from the equation $v^2 = \frac{1}{2}C \times F$, and for $\dot{A}$ (the fluxion of the arc) its value deduced from the distances, &c. will result an equation expressing the relation between the distances from 2 given points to the curve, or its absciss and ordinates, and their fluxions.

3. If the forces are not all situated in the same plane, then from the before given equation $(F + V \times X')\,\dot{A} = -\dot{v}v$, and the 2 others $v^2 = \frac{1}{2}C \times F'$ and $v^2 = \frac{1}{2}C'F''$, where F denotes the force in the direction of the tangent, and F′ and F″ are the forces in different directions, which both are not situated in the same plane with each other and the tangent, and in which directions the chords of curvature are respectively C and C′; since the quantities F, F′, and F″; C and C′ and $\dot{A}$ (as proved before) can all be expressed in terms of the distances from 3 given points, or from 2 abscissæ and 1 ordinate, and their respective fluxions; may be deduced 2 fluxional equations expressing the relation between the distances from 3 given points, or 2 abscissæ and an ordinate, &c. The same principles may be applied to cases, in which some of the forces act in parallel directions.

On Moveable Centres.

PROP. 11.—1. Given the respective places of (n) bodies S, S′, S″, S‴, &c. in the curves A, A′, A″, A‴, &c. at the same time, and in the same plane, and the forces of all the bodies acting on S, except two, S′ and S″; to find the forces of the 2 bodies S′ and S″ on the body S.—This proposition may be resolved by the method given in prop. 4, for to produce the same effect the same finite forces will be requisite, whether the centres of forces rest or move in given curves.

1. 2. If the bodies S, S′, S″, &c. move in different planes, then all the forces acting on the body, except 3, may be given, which may be acquired from the method given in the same proposition. Hence it appears, that $2n$ forces may be requisite to be found from the conditions of the problem to determine all the bodies to move in their respective curves, when they are all situated in the same plane, and that $3 \times n$ forces may be requisite in different planes, &c. if the force of one body (S′) on another (S″) does not at all depend on the force of the same body (S′) on any other (S‴); and if the same can be prædicated of the rest, then $n \cdot n - 3$ forces of the above-mentioned bodies in the same, or $n \cdot n - 4$ forces in different planes may be assumed at will.

3. If the velocities v, v', v'', &c. at every point of the arcs a, a', a'', &c. of the (n) above-mentioned curves A, A′, A″, &c. be given in terms of their arcs, abscissæ, or ordinates, &c. and the places in which the bodies are situated at the same time in the arcs b, b', b'', &c. of some other curves B, B′, B″, &c. find the corresponding velocities V, V′, V″, &c. at the same time of the bodies in the curves B, B′, B″, &c.; then make $\frac{\dot{a}}{v} = \frac{\dot{a}'}{v'} = \frac{\dot{a}''}{v''}$ &c. $= \frac{\dot{b}}{V}$, or which is equal to it $= \frac{\dot{b}'}{V'}$ or $= \frac{\dot{b}''}{V''} =$ &c. From the fluents of the fluxional equations resulting properly corrected will be found the arcs a, a', a'', &c., described by the bodies in the curves A, A′, A″, &c. in the same time as the correspondent arcs b, b', b'', &c.; and thence, by the method given in the preceding case, may be deduced the forces.

The same principles may be applied to bodies moving in resisting mediums.

PROP. 12.—Given the law of the forces of 2 bodies acting on each other, to find the 2 curves by them described.—Fig. 14. Assume x and y for the absciss (AP) and ordinate (PM) of one curve, and z and u for the absciss (AP′) and ordinate (P′M′) of the other; where the abscissæ AP and AP′ begin from the same point A, and are situated in the same line; then will the distance (D = M′M) between the bodies $= \sqrt{z \pm x^2} + \sqrt{u \pm y^2}$; let the forces of the body placed at M on that at M′, and of the body placed at M′ on that at M vary as $\varphi : (\mathrm{D}) = \mathrm{F}$, and $\varphi' : (\mathrm{D}) = \mathrm{F}'$; and let $\mathrm{M}p = \dot{x}$ and $pm = \dot{y}$; then will cosine of the angle mMM′ to radius (1) be $\frac{y \pm u}{\mathrm{D}} \pm \frac{\dot{y}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}} \pm \frac{x \pm z}{\mathrm{D}} \times \frac{\dot{x}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}} = c$; and consequently the force in the direction of the tangent Mm will be $c \times \mathrm{F}$, whence $- v\dot{v} = c \times \mathrm{F} \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$ (A) and $v^2 = \frac{1}{2}\mathrm{CF}$, where C is the chord of curvature in the direction of the force (F) $= \surd(1 - c^2) \times 2 \frac{(\dot{x}^2 + \dot{y}^2)^{1\frac{1}{2}}}{\dot{y}\ddot{x} - \dot{x}\ddot{y}}$; and v the velocity of the body in the curve, whose absciss is x and ordinate y.

In the same manner let $\frac{x \pm z}{D} \times \frac{\dot{z}}{\surd(\dot{z} + \dot{u}^2)} \pm \frac{Y \pm u}{D} \times \frac{\dot{u}}{\surd(\dot{z}^2 + \dot{u}^2)} = c'$, the cosine of the angle made between the distance MM′ and arc of the curve of which the absciss is z and ordinate u, and consequently $c' \times F'$ will be the force in the direction of its tangent, and therefore $- \dot{v}'v' = c' \times F' \times \surd(\dot{z}^2 + \dot{u}^2)$ (A) and $v'^2 = \frac{1}{2}C'F')$, where C' is the chord of curvature in the direction of the force (F′) $= \surd(1 - c'^2) \times 2 \frac{(\dot{z}^2 + \dot{u}^2)^{1\frac{1}{2}}}{\dot{u}\ddot{z} - \dot{z}\ddot{u}}$, and v' the velocity of the body in the curve whose absciss is z and ordinate u; then, because the times of describing correspondent arcs in the two curves are equal, their increments will be equal, and consequently $\dot{t} = \frac{\surd(\dot{x}^2 + \dot{y}^2)}{v} = \frac{\surd(\dot{z}^2 + \dot{u}^2)}{v'}$; and there are deduced 5 fluxional equations, containing 6 variable quantities v, v', x, y, z, and u, and their fluxions; reduce these equations, so that 4 of them (v, v', &c.) may be exterminated, and there will result an equation expressing the relation between x and y the absciss and ordinate of one curve, or z and u the absciss and ordinate of the other curve, and their fluxions; the fluential equation of which being found, and properly corrected, gives the equation to the curve. The 5 equations are easily reduced to 3 by exterminating the quantities v and v'. The fluxional equation resulting will most commonly be of the 5th order, as evidently appears from the nature of the problem.

2. The same principles may be applied to determine the curves, when the bodies move in mediums, of which the resistances are given: for example, suppose the resistances to vary as a function of the distance from a given point into a function of the velocity: to the forces in the directions of the tangents contained in the preceding case must be added or subtracted the given resistances for the forces in the directions of the tangents, and the remaining process will be the same as is before given. If two bodies describe similar orbits round a common centre, either quiescent or moving uniformly in a right line; the forces and velocities and resistances of the medium will be to each other in correspondent points as the respective distances from the centre.

PROP. 13.—Given the forces acting on any bodies, and tending to points either moveable or quiescent, or in the direction of the tangents, &c.; to find the curve described by one of the bodies.

1. Assume x and y for the absciss and ordinate of the curve required, and from thence may be deduced the distances from any quiescent centre of force, and consequently the force f in that direction; resolve it into 2 others, one in the direction of the tangent, and the other in a different one; for example, let it be in a

direction perpendicular to the tangent, and from their fluxions $\dot{x}$ and $\dot{y}$, and the force f may, by the method before given, be deduced the forces in the 2 directions above-mentioned; and in the same manner may be found, from x, y, $\dot{x}$, and $\dot{y}$, the forces in the directions of the tangent and perpendicular to it, which follow from all the forces tending to given points, and acting on the body moving in the curve to be investigated. 2. If some of the centres of force move in given curves B, B$'$, B$''$, &c. whose arcs let be denoted by B, B$'$, &c. and their respective places at the same time are given; then from their respective places given and forces, and x and y, and $\dot{x}$ and $\dot{y}$, can, as before, be deduced the forces in the direction of the tangent and its perpendicular to the curve required. 3. If other centres of forces move in given curves A, A$'$, A$''$, &c. and the velocities are given at every point of the curves; let A, A$'$, A$''$, &c. be the arcs of the curves A, A$'$ A$''$, &c. and suppose v, v', v'', &c. their correspondent velocities; then, if the increments of the time be given, will $\frac{\dot{\mathrm{A}}}{v} = \frac{\dot{\mathrm{A}}'}{v'} = \frac{\dot{\mathrm{A}}''}{v''} =$ &c. but as the velocities are given at every point of the curves, v in the curve (A) will be given in terms of its absciss, ordinate, arc, &c. and consequently $\frac{\dot{\mathrm{A}}}{v}$ in terms of the same quantities and their first fluxions; the same may be affirmed of the fluxions $\frac{\dot{\mathrm{A}}'}{v'}$, $\frac{\dot{\mathrm{A}}''}{v''}$, in the curves A$'$, A$''$, &c.; hence, from the equation $\frac{\dot{\mathrm{A}}}{v} = \frac{\dot{\mathrm{A}}'}{v'}$, can be deduced the relation between the absciss or ordinate, &c. of the curve A and its correspondent absciss or ordinate, &c. of the curve A$'$; and so of the remaining curves; hence this case is reduced to the preceding; but it is necessary also, that the times of the bodies in the two cases should be the same, in order that the places may correspond, and consequently ${}_{v} = {}_{\mathrm{V}}$, where V denotes the velocity of the body at any point of the curve B, from which equation can be deduced the correspondent abscissæ and ordinates, &c. of the curves B and A; and thence the two cases are reduced to the preceding, whence the correspondent forces in the directions of the tangent, and perpendicular to it, can be found as above. 4. If some (m) of the centres move in curves L, L$'$, L$''$, &c. to be deduced from the laws of the forces being given which act on them; assume z and u, z$'$ and u', z$''$, and u'', &c. for their respective abscissæ and correspondent ordinates; and from them and y and x, $\dot{y}$ and $\dot{x}$, find the forces acting on the body moving in the curve required in the direction of the tangent, and perpendicular to it, as before; then add all the forces deduced which act perpendicular to the tangent, and also all contained in the direction of the tangent together with the resisting force in the same direction, and let the sums resulting be respectively F and F$'$: by the same method find the sum of the forces which act on the bodies moving in L, L$'$, L$''$, &c. in the directions of the tangents, and perpendiculars to them, which suppose S and

s, s' and s', s'', s'', &c: then reduce the 2 $(m + 1)$ equations of the formulæ found above, viz. $v^2 = \mathrm{F} \times \frac{(\dot{x}^2 + \dot{y}^2)^{1\frac{1}{2}}}{\dot{y}\ddot{x} - \dot{x}\ddot{y}}$ and $- \dot{v}v = \mathrm{F}'\sqrt{(\dot{x}^2 + \dot{y}^2)}$; $v'^2 = s \times \frac{(\dot{z}^2 + \dot{u}^2)^{1\frac{1}{2}}}{\dot{u}\ddot{z} - \dot{z}\ddot{u}}$ and $- v'\dot{v}' = \mathrm{s} \times \sqrt{(\dot{z}^2 + \dot{u}^2)}$; $v''^2 = s \times \frac{(\dot{z}'^2 + \dot{u}'^2)^{1\frac{1}{2}}}{\dot{u}'\ddot{z}' - \dot{z}'\ddot{u}}$ and $- v'' \times \dot{v}'' = \mathrm{s}' \times \sqrt{(\dot{z}''^2 + \dot{u}''^2)}$, &c. where v', v'', v''', &c. respectively denote the correspondent velocities of the bodies moving in the curves whose abscissæ are x, z, z', z'', &c.; and also the $(m + 1)$ equations $\frac{\dot{\mathrm{B}}}{\mathrm{V}} = \frac{\sqrt{(\dot{x}^2 + \dot{y}^2)}}{v} = \frac{\sqrt{(\dot{z}^2 + \dot{u}^2)}}{v'} = \frac{\sqrt{(\dot{z}'^2 + \dot{u}'^2)}}{v''} = \frac{\sqrt{(\dot{z}''^2 + \dot{u}''^2)}}{v'''} =$ &c. containing the 3 $(m + 1') + 1$ variable quantities x and y, z and u, z' and u', z'' and u'', &c., v, v', v'', &c. and the variable quantity contained in B and V, into 1, so that all the variable quantities except x and y and their fluxions may be exterminated, and there results an equation to the curve required expressing the relation between x and y its absciss and ordinate, and their fluxions. 5. If the forces are not situated in the same plane, assume X, x and y, for the 2 abscissæ and ordinates of the curve required; and Z, z and u; z' and u'; Z$''$, z'' and u''; &c. for the 2 abscissæ and ordinates of the (m) curves L, L$'$, L$''$, &c. respectively; and from the preceding method may be acquired the 3 $(m + 1)$ equations $v^2 = \mathrm{F} \times \mathrm{C}$, $v^2 = \mathrm{F}' \times c'$, and $- \dot{v}v = \mathrm{F}'' \times \sqrt{(\dot{\mathrm{X}}^2 + \dot{x}^2 + \dot{y}^2)}$; $v'^2 = \mathrm{s} \times \mathrm{C}'$, $v'^2 = \sigma c'$ and $- \dot{v}'v' = s \times \sqrt{(\dot{\mathrm{Z}}^2 + \dot{z}^2 + \dot{u}'^2)}$; $v''^2 = \mathrm{s}'\mathrm{C}'' = \sigma'c''$ and $- \dot{v}''v'' = s' \times \sqrt{(\dot{\mathrm{Z}}'^2 + \dot{z}'^2 + \dot{u}^2)}$; $v'''^2 = \mathrm{s}''\mathrm{C}''' = \sigma''c'''$ and $- \dot{v}'''v''' = s'' \times \sqrt{(\dot{\mathrm{Z}}''^2 + \dot{z}^{2''} + \dot{u}'^2)}$; in which v denotes the velocity in the required curve, and v' v'', v''', &c. the correspondent velocities in the curves L, L$'$, L$''$ &c.; and F, F$'$, and F$''$; S, σ and s; S$'$, σ' and s'; S$''$, σ'' and s''; &c. denote the forces acting on the respective bodies in 2 different planes and in the tangents, which planes cut each other in the tangents of the curves; and C and c, &c., C$'$ and c', &c., C$''$ and c'', &c., the $\frac{1}{2}$ chords or radii of curvature in those 2 planes to the different curves in the directions of the forces; and also the $(m + 1)$ equations before-mentioned $\frac{\dot{\mathrm{B}}}{\mathrm{V}} = \frac{\sqrt{(\dot{\mathrm{X}}^2 + \dot{x}^2 + \dot{y}^2)}}{v} = \frac{\sqrt{(\dot{\mathrm{Z}}^2 + \dot{z}^2 + \dot{u}^2)}}{v'} = \frac{\sqrt{(\dot{\mathrm{Z}}'^2 + \dot{z}'^2 + \dot{u}'^2)}}{v''} =$ &c. where $\sqrt{(\dot{\mathrm{X}}^2 + \dot{x}^2 + \dot{y}^2)}$, $\sqrt{(\dot{\mathrm{Z}}'^2 + \dot{z}'^2 + \dot{u}'^2)}$, &c. are the fluxions of the arcs of the required curve, and of the curves, L, L$'$, L$''$, &c. reduce these $4m + 4$ equations containing $4m + 5$ variable quantities into 2, so that all the variable quantities except 3, X, x, and y, and their fluxions may be exterminated; and there result the 2 equations required.

It may be observed, that when the resistance, arising from the density of the medium and velocity (v) of the body, varies as $\mathrm{X}' \times v^2 + \mathrm{X}$, where X and X$'$ are as functions of the distances from the given points, the resolution of the fluxional equations will generally be more easy, than when the resistance varies as other functions of the velocities. If the force acts equally on the particles of the body and fluid, then the force by which a body descends in a medium is as the whole force X acting on the body at the given distance, multiplied into a fraction whose numerator is difference between the density of the body (D) and fluid (x′) at that distance and denominator D, that is, as $x \times \frac{D - x'}{D}$.

Many cases might have been given, in which the fluxional equations could have been resolved; but in general their fluents can only be found by means of converging serieses.

VIII. Experiments on Local Heat. By James Six, Esq., of Canterbury. p. 103.

The following experiments are a continuation of those Mr. S. communicated some time before, relating to the diversity of local heat in the atmosphere; and confirm, in a more particular manner, his former observations respecting a remarkable refrigeration, which, in clear weather, takes place near the earth; for though its surface in the day-time is then most liable to be heated by the sun, yet after that is set, and during the night, the air is always found coldest near the ground, particularly in valleys. The experiments in the former paper were made partly in autumn, and partly in winter; and, the local variations differing in some measure with the seasons, he was desirous of continuing a series of experiments throughout one entire year. To this end therefore he suspended proper thermometers in a shady northern aspect, in the open air, at different heights; one in the garden at 9 feet, and another in the Cathedral Tower 220 feet from the ground; continuing his journal, with the omission of a few days only, from July 1784 till July 1785. The result entirely corresponded with what was before observed respecting the nocturnal diminution of heat, and the particular state of the atmosphere requisite to produce it. From the 25th to the 28th of October, the heat below in the night exceeded, in a small degree, the heat above, at which time there was frequent rain, sometimes mingled with hail. From the 11th to the 14th, and also on the 31st, there was no variation at all; during which time likewise the weather was rainy; all the rest of the month proving clear, the air was found colder below than it was above, sometimes 9 or 10 degrees. On cloudy nights, in June, the lowest thermometer sometimes showed the heat to be a degree or 2 warmer than the upper one; but in the day time the heat below constantly exceeded the heat above more than in the month of October.

Being desirous of knowing whether the nocturnal refrigeration increased on a nearer approach to the surface of the earth, Mr. S. placed, in the midst of an open meadow, on the bank of the river, 2 thermometers; one on the ground, and the other 6 feet above it; with these, and the 2 others before mentioned, one on the tower, and the other in the garden, he made observations from the 10th to the 23d of October, 1786. Here he found, as before, the nocturnal variations entirely regulated by the clearness, or the cloudiness, of the sky; and though they did not always happen in the same proportion to the respective altitudes, yet, when the thermometers differed at all, that on the ground was always the coldest.

Finding so considerable a difference as $3\frac{1}{2}°$ within 6 feet of the earth's surface, Mr. S. increased the number of thermometers in the meadow to 4; one of them he sunk in the ground, another he placed just on the ground, a third he suspended at 3 feet, and a fourth at 6 feet from the ground. At the same time he placed 3 thermometers in an open garden on St. Thomas's Hill, where the land is level with the Cathedral Tower, and about a mile distant from it; here he likewise put one in the ground, another just on it, and suspended a third 6 feet above it. With these 7 thermometers and the 2 before-mentioned, in the city, he continued a diary for 20 days, taking also every morning the temperature of the water in the river; but the weather proving cloudy soon after, the thermometers hardly varied at all, 7 or 8 days only excepted. After this time he never rectified them but when the appearance of the weather gave reason to expect that they would vary considerably: by which it appears, that the cold in the night was generally greater in the valley than that on the hill; but that the variations between the thermometers on the ground, and those 6 feet above them, were often as great on the hill as in the valley.

From the foregoing experiments it appears, that a greater diminution of heat frequently takes place near the earth in the night-time, than at any elevation in the atmosphere within the limits of Mr. Six's inquiry; and that the greatest degrees of cold are at such times always found nearest to the surface of the earth; that this is a constant and regular operation of nature, under certain circumstances and dispositions of the atmosphere, and takes place at all seasons of the year; that this difference never happens in any considerable degree but when the air is still, and the sky perfectly unclouded; but the moistest vapour, such as dews and fogs, did not, as far as he could perceive, at all impede, but rather increase the refrigeration. In very severe frosts, when the air frequently deposits a great quantity of frozen vapour, he generally found it greatest; but the excess of heat, which in day-time, in the summer season, was found at the lower station, in the winter diminished almost to nothing.

The foregoing experiments related to the difference of heat which, at certain times, is found at different altitudes; the following to the different degrees of heat observed at different situations in respect to the sea-shore. Mr. S. exhibits a set of corresponding observations; among which are some taken at Chislehurst, by the Rev. Mr. Wollaston; others at the same time were taken in Mr. S.'s garden, and on the Cathedral Tower; and others on the sea-shore, about 7 miles N. N. W. from Canterbury, where the thermometer was suspended about 40 feet above high-water mark, 14 from the ground, and about 100 yards from the sea. Hence it appears that every night, one only excepted, during that time, the air was coldest at Chislehurst; and that the mean heat at the sea-shore was equal to that on the tower at Canterbury. In the month of June the cold was still greater in the night at Chislehurst than at any of the other places, excepting where there appeared 2 currents of wind, the upper current from the S. W. and the lower from the N. E.; at which time also there was the greatest difference between the thermometer in the garden and that on the tower.

The following experiments relate to the variation of local heat in the earth itself; the diversity of which appears from the different heat of the water issuing from it at different places. It has been conjectured, that the diversity of the temperature of springs may probably depend on their different elevations in the earth, with respect to the level of the sea. Two remarkably deep wells, both near the sea-shore, and not far distant from Canterbury, gave a favourable opportunity of making experimental inquiry into this matter; especially as the situation of the 2 springs differed considerably from each other in respect to the level of the sea. One of these is a well in Dover Castle, which is sunk 360 feet through the high cliff of chalk on which the castle stands, and the depth of the well is nearly equal to the height of the cliff from the sea. The other is King's-Well at Sheerness, which was sunk 330 feet through almost one entire stratum of firm clay, where the surface of the ground is only 4 feet above high water. Supposing therefore the spring in Dover well to lie level with the sea, the spring of the well at Sheerness lies 326 feet below it; a circumstance extremely favourable to the experiment. The temperature of the springs he took in the following manner. After fathoming each well with a line and plummet, he let one thermometer down to the bottom, and fixed another on the line, so as to reach to half the depth only, keeping a 3d to take the temperature of the air at the top.

Sept. 28, 1784. Temperature of the water in the new well in Dover Castle.

By the thermometer at the top	56°
By ditto at the middle	52
By ditto at the bottom	48¾

Found the well 360 feet deep with 21 feet water.

Oct. 6, 1784. Temperature of the water in King's Well at Sheerness.

By thermometer at the top	53°
By ditto at the middle	51
By ditto at the bottom	56

Found the well 280 feet deep* with 180 feet water.

About noon was the time of day when Mr. S. made the experiments at both places, and the top of the respective wells varying from each other depended wholly on the accidental temperature of the atmosphere at the time; but that the thermometer at half the depth of the well at Dover gave nearly the mean

* The sand brought up from the bottom of the well, by the force of the spring, has reduced it to its present depth.—Orig.

heat of the top and bottom, while that in a corresponding situation in the well at Sheerness gave it colder than either top or bottom, he attributes to the following circumstance. Over the well at Sheerness a machine is erected, which raises the water by means of an horizontal windmill, working an endless chain. This chain, consisting of jointed double bars, with a number of buckets fixed at certain distances from each other, continually descending into, and ascending out of the water, to an elevation of 8 or 9 feet above the top of the well, may be supposed to reduce the water as far as it reaches to the mean temperature of the air above; and thus he found it; for 51° had been the mean temperature of the air near the sea-shore for several days before. At the bottom of the well, near to which the chain never descends, he found the temperature 56°; above 7° warmer than that at Dover well.

The water at the bottom of these wells is, he presumes, too deep beneath the surface of the earth ever to be affected by the temperature of the atmosphere; for if the heat of the summer could have had any influence on either of them, that at Dover must have been most considerably affected by it, especially in the month of September; and the air was something warmer when the experiment was made at Dover than at Sheerness. From the nature of the different kinds of strata in which these wells are dug, had they been in all other circumstances the same, one might reasonably expect to find the warmer spring in the chalk, and the colder in the clay; but here the reverse is seen, without any apparent local cause, except the different elevations of the springs in respect to the level of the sea.

IX. Observations on the Manner in which Glass is Charged with the Electric Fluid, and Discharged. By Edw. Whitaker Gray, M. D., F. R. S.* p. 121.

Dr. Franklin, in various parts of the first volume of his experiments and observations, asserts, That the natural quantity of electric fluid in glass cannot be increased or decreased; and that it is impossible to add any to one surface of a plate or jar, unless an equal quantity be, at the same time, given out from the other surface.† This error has been adopted by succeeding electricians; among others, by the late Mr. Henly, who in one of his last papers, printed in the Philos. Trans. for the year 1777, has the following words: "According to Dr. Franklin's theory, the same quantity of the electric matter which is thrown upon one of the surfaces of glass, in the operation of charging it, is at the same time repelled or driven out from the other surface; and thus one of the surfaces becomes charged plus, the other minus; and that this is really the case is, I think,

* Dr. Gray died in Jan. 1807, being 59 years of age. At the time of his death he was senior secretary of the R. S. and keeper of the department of natural history and antiquities at the British Museum.

† "The quantity proportioned to glass it strongly and obstinately retains, and will have neither more nor less." Experiments and Observations, vol. i. p. 26. See also p. 75, 81, et alibi.

satisfactorily proved, &c." Beccaria also has adopted the same opinion, saying, "That a quantity of excessive fire cannot be introduced into one surface, but inasmuch as an equal dose of natural fire can quit the other surface."

These assertions Dr. G. apprehends directly contrary to what really happens. Instead of which, he believes, we may safely assert, that glass, and every other known substance, may have its natural quantity of electric fluid either increased or diminished to a certain limited degree; which degree bears no proportion to the quantity of matter contained in a body, but is, cæteris paribus, in proportion to the extent of its surface. This law, which is perhaps without exception, he thinks, may be considered as one of the fundamental laws of electricity, and one on which many of its principal phenomena depend. At present he only considers it so far as it is the cause of what is commonly called the charge of a coated jar. Suppose such a jar insulated, and connected by its knob to the prime-conductor of an electric machine; if then the machine be put in action, a certain quantity of electric fluid, agreeable to the above-mentioned law, is added to the natural quantity belonging to the inner surface of the jar. After which, if the finger, or any other conducting substance, be presented to the outer coating of the jar, a quantity of electric fluid, nearly equal to that thrown in, comes from it. But this departure of electric fluid from the outside of the jar, cannot be, as Dr. Franklin supposes it, the cause which permits the addition of fluid to the inside, but is merely the consequence of the action of that superfluous quantity which was thrown in. And the operator may, if he pleases, instead of taking electric fluid from the outside of the jar, take out again, by touching the knob, nearly the whole of what he had thrown in, which he could not do if an equal quantity had already gone from the outside of the jar.* When the quantity already spoken of has been taken from the outside of the jar (the equilibrium being nearly restored) another quantity like the first may again be added to the inner surface: after which a similar quantity may again be taken from the outside: thus, by the succession of a sufficient number of the quantities allowed by the before-mentioned law, the jar may at length be completely charged.

There are other ways of charging coated glass; but if it be allowed that the charge, in the foregoing instance, is produced in the manner I have supposed, it will not, I think, says Dr. G., be disputed, that all other charges are produced by a similar alternation of small quantities. This however will appear

* Much dispute has arisen among electricians respecting the degree of charge which may be given to an insulated jar; but no one, that I know of, has taken notice of a deception which will happen, if care be not taken that the same side, by which the jar is attempted to be charged, be first touched in trying whether it be charged or not; whereas it is clear, from what has been said, that if the contrary surface be first touched, a small charge will, from that very circumstance, be produced.—Orig.

more clear from the observations I shall now make on the manner in which the discharge is produced. When the astonishing velocity, with which the charge of a jar or battery moves through a considerable space, is considered, it may at first appear impossible, that the discharge should be made by the alternate giving and receiving such small quantities as those by which the charge was produced; yet a more ample consideration of the matter will, I think, show that it cannot possibly be brought about any other way. I presume it will be granted, that the charge of a jar, in discharging, either leaves it all at once, or goes out by the same small quantities by which it went in. To suppose any intermediate manner would neither lessen the difficulty, nor would it be consonant to any of the known laws of electricity.

If then the whole charge leave the jar all at once, there must be a point of time at which the jar will be without any electric fluid either on one side or the other: nay more, suppose a large jar or battery to be discharged by means of a few inches of thin wire, there will then be a point of time at which the whole quantity of electric fluid, which constituted the charge, must be contained in a piece of wire, weighing only a few grains. Now, if it be considered, that time, like matter, is infinitely divisible, may we not rather suppose, that the discharge of a jar is nothing more than an inconceivably rapid succession of such small quantities as may be sent off, without causing such a destruction of the equilibrium as the laws of electricity seem not to admit? That this supposition is not quite free from objections I readily admit; but before they are permitted to overthrow it, let it be well considered, whether they are, on the whole, as strong as those I have stated against the opposite opinion, which I think may be pronounced to militate not only against what has been here mentioned as a fundamental law of electricity, but also against every known fact.

X. Experiments on the Cooling of Water below its Freezing Point. By Charles Blagden, M. D., Sec. R. S., and F. A. S. p. 125.

When the experiments for determining the degree of cold at which quicksilver becomes solid, related in the Philos. Trans. for 1783, were under consideration, no difficulty occurred in explaining the phenomena that had been observed, except in the few instances where the mercury in the thermometer congealed, while it was surrounded with some of the same metal in a fluid state. The well-known property of water, that under different circumstances it will bear to be cooled several degrees below its freezing point without congealing, afforded from analogy the most probable solution of this difficulty; but as neither the cause of that property had been investigated, nor the circumstances by which it is modified had been ascertained, I was led to attempt some experiments on the subject; not only in hopes of elucidating the above-mentioned phenomenon of the quicksilver, but also because this very quality in water was itself a curious subject of research.

I began with endeavouring to determine, whether this property belongs to it as pure water, or depends on extraneous admixtures. For that purpose I poured some clean distilled water into a common tumbler glass, till it reached 2 or 3 inches above the bottom, and then set the glass in a frigorific mixture, made with snow and common salt. This was the method I used in most of the following experiments, sometimes employing ice instead of snow, substituting a glass jar or cylinder instead of a tumbler, and filling the vessel to a greater or less height above the bottom. I found that, in the frigorific mixture, the distilled water readily sunk many degrees below 32°, still continuing fluid; and by repeating the experiment with care, I several times cooled it to 24°, 23°½, and even almost to 23°. From these experiments therefore it seemed evident, that the property of being cooled below the freezing point did not depend on extraneous admixture, especially as I found, by comparative trials, that common pump-water would scarcely ever bear to be cooled so much. An ambiguity however still remained, on account of the air which is always mixed with water that has lain exposed to the atmosphere. In order to determine what might be ascribed to this circumstance, I put some of the same distilled water over the fire to boil, in a clean silver vessel, and kept it in violent ebullition for a considerable time. In a few minutes after it had been taken off the fire, and before it was nearly cold, I set it in the frigorific mixture after the usual manner; when, instead of freezing more readily, it bore to be cooled 2° lower than I had ever been able to reduce the unboiled water, not congealing till the thermometer in it had sunk to 21°. Subsequent experiments were attended with a similar result, and have sufficiently convinced me, that, other things equal, boiled water may be cooled a greater number of degrees below the freezing point, without congealing, than water which, not having undergone that operation, retains the air it naturally imbibes.

As a further proof that the presence of an aerial fluid in water rather lessens than increases its quality of being cooled below the freezing point, I found that distilled water, which had been for that purpose impregnated with fixed air, generally shot into ice at a less degree of cold than the same water in its ordinary state. I suspect however, that it is usually by the admixture of other aerial substances, such as dephlogisticated air, phlogisticated air, or perhaps both, and not of fixed air, that water is inclined to congeal soon after it has passed the freezing point; for, as will be seen hereafter, acids rather improve than diminish the quality in water of resisting congelation.

To determine the effect of other extraneous substances, I took some very hard pump water, such as is found in the northern parts of London, and set it in the frigorific mixture. In general it congealed sooner by 1 or 2° than unboiled distilled water; that is, at 25° or 24° of the thermometer; and as there was some variation in this respect, I was led to remark, that the greatest cooling usually took place when the water was most clear and transparent. With a view to this circumstance I took some New-River water, which happened at that time to be considerably turbid, and tried it in the frigorific mixture; when I found, very unexpectedly, that it was not in my power to cool any of it below the freezing point; a crust of ice always forming round the sides and at the bottom of the vessel, while the thermometer, suspended about the middle of the water, was 2 or 3° above 32°. To try how far this depended on the foulness of the water, I collected some of the muddy sediment which had been deposited from the New-River water, and added it to the pump water, which had before borne to be cooled to 24° or 25°, so as to render it turbid; when it congealed, in the same manner as the New-River water had done, before the thermometer in the middle of it came to the freezing point. It must not however be imagined, that water thus made turbid is incapable of being cooled below 32°, without freezing: I have since repeated the experiments, with more caution in conducting them, and reduced it 2 or 3° below the point of congelation. But still they have all confirmed the general fact, that substances which lessen the transparency of water, render it at the same time much more difficult to be cooled below the freezing point, and dispose it to shoot into ice more readily, after it has passed that point, than pure water would do. It seems to be of little consequence what the substance is that renders the water turbid; small particles of any kind floating through it, I believe, have this effect, which does not take place, or at least to the same degree, when the extraneous substance has subsided to the bottom. It is this circumstance, I suppose, which gave rise to the opinion, that boiled water freezes sooner than unboiled: for if the water contain calcareous earth, held in solution by means of fixed air, as is the case with most kinds of spring water, this will be precipitated by the boiling, and will sensibly trouble the transparency of the water; which, if exposed to the cold in that state, will be liable to freeze sooner than the same kind of water unboiled and transparent.

The effect of this want of transparency was very different from that of chemical mixture, as appeared by subsequent experiments. Though the property of being cooled below the freezing point appeared to belong essentially to water in its pure state, it was probable that it would be in some measure altered or modified by the various substances which are capable of being dissolved in, or chemically combining with, the water. But here a further circumstance came to be considered. It is well known, that such substances, uniting with water, have a power of lowering its point of congelation a greater or less number of degrees, according to the nature and quantity of the substance employed. The first object therefore was, to determine in what manner the property of bearing to be cooled would be affected with regard to that new point of congelation. For this purpose I made many experiments with several different substances, which it would be too long to relate in detail, but the principal were as follows.

Having dissolved in distilled water as much common salt as lowered its freezing point to 28°, I cooled it to 18°$\frac{1}{4}$ before it congealed. Another solution of the same salt, whose freezing point was 16°, bore to be cooled to 9°; and a stronger solution, whose freezing point was 13°$\frac{1}{2}$, cooled to 5° before it shot. A solution of nitre, whose freezing point was 27°, cooled to 16°, that is, 11° below its new freezing point; a solution of sal ammoniac, whose freezing point was 12°, cooled to 3°; and one of Rochelle salt, freezing point 27°$\frac{1}{2}$, suffered the thermometer to sink in it to 16° before it froze; a cooling equal to the greatest I ever obtained with the purest distilled water boiled. A solution of green vitriol, whose freezing point was near 30°, cooled below 19°: and of salts with an earthy basis, a solution of the common bitter purging salt, whose freezing point was at 25°$\frac{1}{2}$, bore to be cooled to 19°.

Acids rather augment this quality of being cooled below the freezing point. A combination of nitrous acid with distilled water, in such proportions that the new freezing point was between 18° and 19°, sunk down to 6° before it congealed; which being fully 12° of cooling, is greater than I have been able to produce with pure water. Another mixture of the same kind, so strong as to have its freezing point about 11°, cooled down to 1°. A mixture of vitriolic acid and distilled water, whose freezing point was 24°$\frac{1}{2}$, cooled to 14°; and one with the acid of salt, having its freezing point at 25°, sunk to 16° before it froze. It is here to be observed, that these acid mixtures were rather remarkable for the steadiness with which they bore to be cooled, and the little tendency they showed to shoot before they were sunk much below the freezing point, than for exceeding the number of degrees which pure water might be cooled. Of the alkalis, a solution of tartar, whose freezing point was 25°$\frac{1}{2}$, cooled to 18°; and another, with the freezing point at 15°, sunk to 8°. A solution of crystallized soda, freezing point 30°, cooled to 21°; and a solution of mild volatile alkali, freezing point 19°, to 11°. A mixture of rectified spirit of wine and water, whose freezing point was 12°, cooled to 5°; and another, with the freezing point at 8°$\frac{1}{2}$, to 2°.

All these facts, with many others of the same nature, sufficiently show, that foreign substances, chemically combined with or dissolved in water, do not take away its property of being cooled below its point of congelation; though, by depressing that point, they alter the degree of cold at which the property commences. The experiments show, that in some cases the mixed water bore to be cooled as much below its new freezing point, as pure water below 32°; and with regard to the others, I think the variation was no greater than usually takes place with different portions of common water. Scarcely any, perhaps none, of the above-mentioned points were absolutely the lowest to which the solutions or mixtures could have been reduced, if the experiment had been conducted still more slowly and cautiously. But however much they might all possibly have borne to be cooled, a great difference occurred among them in the ease with which the operation succeeded.

Want of transparency however is only one among several causes which impair the property water naturally possesses, of bearing to be cooled many degrees below its freezing point. M. Mairan, in his elaborate treatise on ice, having occasion to examine this subject, was led by his experiments to conclude, that the cooling of water below its freezing point depends on rest, and that agitation is the general cause by which it is brought to shoot into ice. In this opinion he has been almost implicitly followed by all the writers I have seen, excepting only Professor Wilcke, of Stockholm. To bring it to the test of experiment, I set in the frigorific mixture some distilled water, which by boiling had been rendered capable of sustaining a cold of 21° before it froze. When this water was cooled to 22° I agitated it by moving the tumbler, by shaking a quill in it, and by blowing on it so as to ruffle the surface; but it supported all these trials without congealing, and did not shoot till a minute or 2 afterwards, when by continuance in the frigorific mixture, it was cooled down to 21°. In other experiments however all the above-mentioned kinds of agitation made similar water instantly congeal, even when not cooled so low by several degrees. The congelation therefore, must in these cases have depended on some further circumstance than the mere want of rest. One that I suspected is a sort of tremulation, rather agitating small portions of the water separately, than moving the whole together. I have found, that striking the bottom of the tumbler against a board would produce instant congelation, when stirring the water, or shaking the tumbler in the hand, would have no effect. In like manner when, in stirring the cooled water, the quill or stick of glass, employed for that purpose, strikes against the side or bottom of the tumbler, the water, which had resisted the general stirring, is often by this percussion made to freeze. The same effect is produced, and with less uncertainty, if the quill or stick of glass be rubbed, and as it were ground, against the side of the tumbler. But of all such methods of bringing on the congelation, that which I have found to fail the seldomest, is to rub a bit of wax against the side of the tumbler under the water; a particular roughness in the motion is felt, with some sound, approaching to a musical tremulation, and a crust of ice is immediately perceived under the wax on the glass. This effect of the wax I take to be mechanical, depending on its particular state of consistence. Wood acts in the same manner, though with less certainty; so does a quill, and also glass; but the latter, being very hard, produces the effect with least certainty. It is a mechanical action on the water in contact with the rubbing substance and the glass: for if the outside of the tumbler, or any part of the inside above the water, be rubbed, even if it be wet so as to communicate a similar feeling of tremulation, yet still the congelation is not produced. All these modes of bringing on the congelation succeed best, as might be expected, in proportion as the water is more cooled below the freezing point. Unless the cooling amount to 4 or 5°, the friction with wax is often in vain.

From the above-mentioned facts it appears, that M. Mairan's position, though not destitute of foundation, was enounced by him too generally, and without sufficient precision. It is the natural property of water to bear to be cooled a certain number of degrees without freezing; rest favours this property negatively, by giving it no interruption; but most kinds of agitation interfere with its operation to a greater or less degree, and some perhaps would prevent it altogether; while others affect it so little, as not to superinduce the congelation, even when the cooling is brought within 1° of the greatest that the water will bear.

Whatever be the effect of agitation, there is another cause which much more powerfully hastens the congelation of water. It has been long known, that when water is cooled below its freezing point, the contact of the least particle of ice will instantly make it congeal, the glacial crystals shooting all through the liquor, from the spot where the ice touches it, till the whole comes up to the freezing point. Few experiments of the minute kind afford a more striking spectacle than this, especially when the water has been cooled nearly as much as possible below the freezing point; both from the beautiful manner in which the crystals shoot through it, and the rapidity with which the mercury in the thermometer immersed in it runs up through a space of 10 or 11°, stopping and fixing always at 32 in pure water. If from any circumstance however, as a less cooling, or the addition of a salt, the shooting of the ice proceed more slowly, the thermometer will often remain below the freezing point even after there is much ice in the liquor; and does not rise rapidly, or to its due height, till some of the ice is formed close to its bulb; which exemplifies the evolution of the latent heat from the very particles that congeal.

Many of the circumstances attending the greater or less cooling of water below its freezing point depend on this principle. In a calm day, when the temperature of the air was about 20°, I exposed 2 vessels with distilled water to the cold; one of them was slightly covered with paper, the other was left open: the former bore to be cooled many degrees below the freezing point, while a crust of ice always formed on the surface of the other before the thermometer immersed in the middle of it came to the freezing point. This phenomenon, which other observers have remarked without being able to account for it, appears clearly owing to frozen particles, which in frosty weather are almost always floating about in the air, often perceptibly to the senses. They come most commonly either from clouds passing over head, or from snow or hoar-frost lying on the earth; and when they touch the cooled surface of the water, instantly make it freeze. That the effect does not depend simply on the contact of cold air, is plain from the following experiment. I exposed to the cold a glass jar, with some distilled water, and placed in it 2 thermometers; one immersed in the water, the other suspended a little above its surface, in the empty part of the jar. The latter sunk faster than the former; but after a certain time, the thermometer above the surface was at 25°, and that in the water at 25°½, yet the water continued unfrozen. I perceive too by M. Wilcke's experiments, that in much more intense cold than we usually experience in this country, vessels of water standing within doors in a laboratory are often cooled so far below the freezing point as to become almost full of ice on being made to shoot, though the surface of the water be in no wise defended from the cold air of the laboratory. Oil spread over the surface of water has been found to prevent it from freezing, when other water similarly exposed has had a crust of ice formed on it. This I ascribe entirely to the prevention of frozen particles from coming in contact with the water: for in experiments with frigorific mixtures, in a room of moderate temperature, I do not find that oil on the surface has any sensible effect in enabling water to support more cold, unless indeed where the operation is otherwise too much precipitated. Also a crack in the tumbler containing the water prevents it from cooling below the point of congelation, a thin film of ice insinuating itself through the crack into contact with the water. And often, in experiments with frigorific mixtures, the congelation is brought on by raising the immersed thermometer a little out of the water, and lowering it down again; some of the adhering water having frozen on its stem.

Several other circumstances, though not so distinctly ascertained as the preceding, appear to facilitate the congelation of cooled water. For instance, in experiments with frigorific mixtures, if the cold be very intense, the water freezes almost immediately round the sides of the vessel, as if something depended on too sudden a change of temperature. Accordingly, the only way of insuring the greatest degree of cold in water without freezing, is to cool it in a very gradual manner, keeping the cold of the frigorific mixture regularly only 2 or 3° below that of the water. Sudden cooling therefore may be considered as one of the causes which hasten congelation. No doubt this will sometimes depend on such a cold as water cannot resist without freezing, being propagated through the glass to the nearest part of the water, quicker than it can be distributed to the rest of the water; but I think the above-mentioned effect takes place when no part of the fluid can be supposed to be many degrees below the freezing point.

It has been alleged, that metal in contact, either with the outside of the vessel containing the water, or with the water itself, disposes it to freeze sooner after it is cooled below 32°. Though on repeating this experiment I have found it possible to cool water in a metal vessel many degrees below its freezing point, and even to touch it, when so cooled, with metal equally cold, without producing congelation; yet the metal certainly tends to hasten the freezing, and I believe on the above-mentioned principle of too quick a change of temperature, occasioned by its quality as a good conductor of heat. For the same reason it is more difficult to cool water much below the freezing point in thin vessels, than in those whose bottom and sides are of considerable thickness; the latter transmitting the heat more slowly, and allowing it thus to be diffused more equably.

In cooling water below its freezing point by frigorific mixtures, it is of consequence to keep the mixture some way below the upper edge of the water within the tumbler, otherwise the congelation quickly begins at that place. This very likely depends on the principle last mentioned, that the thin edge of water rising up against the side of the glass, being more in contact with air than with the general mass of water, does not so easily distribute its cold, and therefore suffers a more rapid change of temperature by the action of the mixture. Hence one of the most essential precautions for cooling water to the utmost without congelation, is to perform the experiment in a warm room, that the air in contact with the edges and surface of the water may prevent their sudden cooling. And one of the most convenient vessels for the purpose is a round body terminating in a neck, the body to be surrounded with the frigorific mixture, while the water in the neck is kept above the freezing point. These are the principal facts with which my experiments have furnished me relative to the cooling of water below its point of congelation. I see no general circumstance that applies to them all.

Sudden cooling may promote congelation simply by occasioning the water at the bottom and sides of the vessel to acquire a greater degree of cold than the rest. But perhaps it may have also another effect, admitting of a particular explanation. Water in freezing undergoes a considerable expansion. This may be ascribed to such a form of its particles, and position of their poles, as shall make them, when touching and adhering by those poles alone, intercept very large interstices, which may be considered as the pores of the ice. Various positions of the poles and figures of the particles may be conceived, which should cause them to occupy more space, when touching in certain points only, than they filled when lying near without any contact. But in whatever way the expansion is produced, experiment hath shown that it begins some time before congelation; so that when water is cooled down to 32°, it is already sensibly expanded; and if the congelation does not take place here, this expansion augments, in proportion as the water is further cooled*. The expansion therefore being so evidently an approach to freezing, may be considered as an indication that the polarity already prevails so far as to draw the particles somewhat out of the situation they naturally assume in the higher temperatures. And it is conceivable, that if this operation go on very quick, and the consequent change of position in the particles be made with some degree of velocity, they may acquire a small momentum of motion, enabling them to overcome a resistance which would otherwise prevent their junction.

To assist the conception, I have here reasoned on the particles of water as solid, and of a determinate shape. But it seems most probable, that the particles of matter in general are nothing more than centres to certain attractive and repulsive powers; on which hypothesis it may be understood, that if 2 or more of these central points are brought much within the limits of their respective attractions and repulsions, these powers will no longer be equal at equal distances from their common centre. Now such a combination of central points may be considered as 1 particle of any particular matter; and the unequal distances from the common centre at which the attractions and repulsions are equal will define what may be called the shape of that particle. And if, at equal distances, the attraction or repulsion is much greater at one point than at another, that will constitute a polarity.

The greatest cold I have been able to make water acquire without freezing, is near 12° of Fahrenheit's scale below its common freezing point. Some distilled water was boiled about a quarter of an hour in a tin cup, and placed in the same vessel, while still warm, in the frigorific mixture. The mixture was made to act very slowly, so that the operation continued more than an hour. When the immersed thermometer had sunk to $20°\frac{1}{3}$, the water was still fluid: I then shook it considerably, but no ice formed. After waiting some time, and finding the thermometer would sink no lower, because by the length of the process the snow of the mixture was almost consumed, I added some fresh materials, which could not be done without shaking the tin cup. Still however the water did not freeze instantly, though it shot as soon after as it can be supposed to have felt the influence of the new frigorific mixture. When this water was cooled to 24°, I tried the temperature of the air near its surface, and found it 34° or 35°, the

* In experiments where the water has cooled much below its freezing point, I have seen the expansion so great as to bear a considerable proportion to the whole expansion produced by freezing, which last I believe is more than $\frac{1}{7}$ of the volume of the water. It seemed as if the expansion proceeded in an increasing ratio, being much greater on the last degrees of cooling than it was on the first. The difficulty of procuring a proper apparatus for these experiments has hitherto prevented me from ascertaining the quantities with precision.

experiment being performed in a room with a fire. Another time I cooled some distilled water, covered with oil, below 21°, by similar precautions.

This however is by no means the greatest cooling of which water is susceptible. In Fahrenheit's experiment, with an exhausted globe half full of boiled rain water, it seems to have been cooled to 15°*. M de Luc also informs us, † that having filled a thermometer with some water he had purged of air by the means described in his great work on the atmosphere, he exposed it to a cold which sunk a mercurial thermometer to 14° of Fahrenheit's scale. The water in the thermometer continued transparent, and on breaking the ball that was found to be liquid, but froze that instant. In some of my experiments too with mixtures of nitrous acid and water, the liquor bore to be cooled as much as 13° below its new freezing point; and it has been already observed, that the addition of an acid always expelled much air from the water. It is not improbable therefore, that if water could be thoroughly purged of air, it would readily bear to be cooled 18°, or more, below its freezing point, without congelation; though the deprivation of air, obtained by boiling it, is such only as will barely enable it to admit a cooling of 12°.

Other fluids may bear to be cooled much more below their proper point of consolidation. This is evidently the case in what Mr. Cavendish calls ‡ the spirituous congelation of acids. Mr. M'Nab's nitrous acid bore to be cooled from 30 to near 40° below its freezing point §; and Mr. Kier's vitriolic acid at the strength of easiest freezing continued fluid at 29°, though its heat became 46°½ when it began to congeal ||. How low quicksilver may be cooled has not yet been ascertained, but probably many degrees below − 40°. So many of the above-mentioned facts were observed in the year 1783, that I then ventured to remark, that " independently of these circumstances, neither stirring, agitation, a current of fresh air on the surface, nor the contact of any extraneous body not colder, would [necessarily] cause the water to shoot into ice, notwithstanding the repeated assertions of authors to the contrary ¶." Similar experiments, made in the course of the succeeding winters, have confirmed in general the former results, and furnished the materials of the preceding sheets. I am very sensible, that the subject still remains involved in great obscurity; nor should I have troubled the Society with an account of experiments which leave so much uncertainty, had I not thought that they tended to elucidate a few points, and to correct some erroneous opinions. I hope that persons inhabiting a climate more advantageous for the purpose, will be induced to undertake such experiments in another, and probably a more successful way, by exposure to natural cold.

* Philos. Trans. vol. 32, p. 81. † Idées sur la Météorologie, tom. 2, p. 105.
‡ Philos. Trans. v. 76, p. 261. § Ibid. p. 252. || Ibid. v. 77, p. 279. ¶ Ibid. v. 73, p. 358.

XI. Experiments and Observations relating to the Principle of Acidity, the Composition of Water, and Phlogiston. By Joseph Priestley, LL. D., F. R. S. p. 147.

That water consists of two kinds of air, dephlogisticated and inflammable, is now, I believe, generally admitted as one of the most important, and best ascertained, doctrines in chemistry. My own experiments having seemed to favour it, I made no difficulty of receiving it myself: but having, at the time of the publication of the last volume of my experiments, found that, in decomposing the two kinds of air above mentioned by the electric spark, I got much less water than I expected, and, instead of it, a dark-coloured vapour, not easily condensed, I could not help concluding that something yet remained to be investigated with respect to this subject, and determined, at a proper opportunity, to resume my inquiries into it. It is not necessary here to detail these experiments, which we think were also re-printed, with additions by the author in a separate tract.

But from these experiments are inferred the following conclusions. The near coincidence of the results of these different experiments is remarkable, and makes it almost certain, that no marine acid is retained in the terra ponderosa that has been dissolved in it, after exposure to a red heat; that the generation of the fixed air carries off part of the water in the menstruum; and that this part of the weight is about one-half of the whole. It is observed also, that the supposition of water entering into the constitution of all the kinds of air, and being as it were their proper basis, that without which no aëriform substance can subsist, which the preceding experiments render in a high degree probable, makes it unnecessary to suppose that water consists of dephlogisticated air and inflammable air, or that it has ever been either composed or decomposed in any of our processes. That water is decomposed when inflammable air is procured from iron by steam, is not probable; since the inflammable principle may very well be supposed to come from the iron, and the addition of weight acquired by the iron may be ascribed to the water which has displaced it. Also when the scale of iron, or finery cinder, is heated in inflammable air, it gives out what it had gained, viz. the water.

The most plausible objection to this hypothesis is, that iron gains the same addition of weight, and becomes the same thing, whether it be heated in contact with steam, or surrounded by dephlogisticated air. But from the preceding experiments it appears, that by far the greatest part of the weight of dephlogisticated air is water; and the small quantity of acid that is in it may well be supposed to be employed in forming the fixed air, which is always found in this process: for that there is one common principle of acidity, and that all the acids are convertible into one another, at least the nitrous acid into fixed air, is by no means an improbable supposition, though we are not yet in possession of any process by which it may be done. It is pretty evident that, in this respect, nature actually does what we are not able to do.

Further, that the doctrine of the decomposition of water being set aside, that of phlogiston (which, in consequence of the late experiments on water, has been almost universally abandoned) will much better stand its ground, as all the newly discovered facts are more easily explained by the help of it. If water be not decomposed, both metals and sulphur do certainly yield inflammable air, when steam is made to pass over them in a red heat. They cannot therefore be simple substances, as the antiphlogistic theory makes them to be. Also, the same thing that they have parted with, viz. inflammable air (or rather something that is left of inflammable air when the water is taken from it, and which may as well be called phlogiston as any thing else) may be transferred to other substances, and thus contribute to form any of the metals, sulphur, phosphorus, or any thing else that has been deemed to contain phlogiston. This phlogiston also, no doubt, having weight, it perfectly corresponds to the definition of a substance, having certain affinities, by means of which it is transferred from one body to another, as much as the different acids. The discovery that the greatest part of the weight of inflammable air, as well as of other kinds of air, is water, does not make the use of the term phlogiston less proper: for it may be still given to that principle, or thing, which, when added to water, makes it to be inflammable air; as the term oxygenous principle may be given to that thing which, when it is incorporated with water, makes dephlogisticated air. As there is something in dephlogisticated air that seems to be the principle of universal acidity, so I am still inclined to think, as I observed in my last volume of experiments, that phlogiston is the principle of alkalinity, if such a term may be used; especially as alkaline air may be converted into inflammable air.

In the course of experiments recited in this paper, I discovered more completely than before the source of my former mistake, in supposing that fixed air was a necessary part of the produce of red lead, and also of manganese. Both these substances, I find, give of themselves only dephlogisticated air, and that of the purest kind; and all the fixed air they yielded in my former experiments must have come from the gun-barrel I then made use of, which would yield inflammable air, which, with dephlogisticated air, forms fixed air. For though the dephlogisticated air from red lead was so pure that, mixed with 2 measures of nitrous air, the 3 measures were reduced to 500th parts of a measure, and the substance gave no fixed air at all when it was heated in an earthen tube or retort; yet by mixing iron filings with it, or with manganese, as I had formerly done with red precipitate, I got more or less fixed air at pleasure, and sometimes no dephlogisticated air at all.

XII. On the Irritability of Vegetables. By Ja. Edw. Smith, M.D., F.R.S.* p. 158.

Having often heard that the stamina of the Barberry, Berberis communis, were endued with a considerable degree of irritability, I made the experiment in Chelsea garden, May 25, 1786, on a bush then in full flower. It was about 1 o'clock P. M. the day bright and warm, with little wind. The stamina of such of the flowers as were open were bent backwards to each petal, and sheltered themselves under their concave tips. No shaking of the branch appeared to have any effect on them. With a very small bit of stick I gently touched the inside of one of the filaments, which instantly sprung from the petal with considerable force, striking its anthera against the stigma. I repeated the experiment a great number of times; in each flower touching one filament after another, till the tip of all the 6 were brought together in the centre above the stigma. I took home with me 3 branches laden with flowers, and placed them in a jar of water, and in the evening tried the experiment on some of these flowers, then standing in my room, with the same success.

In order to discover in what particular part of the filaments this irritability resided, I cut off one of the petals with a very fine pair of scissars, so carefully as not to touch the stamen which stood next it: then, with an extremely slender piece of quill, I touched the outside of the filament which had been next the petal, stroking it from top to bottom; but it remained perfectly immoveable. With the same instrument I then touched the back of the anthera, then its top, its edges, and at last its inside; still without any effect. But the quill being carried from the anthera down the inside of the filament, it no sooner touched that part than the stamen sprung forwards with great vigour to the stigma. This was often repeated with a blunt needle, a fine bristle, a feather, and several other things, which could not possibly injure the structure of the part, and always with the same effect. To some of the antheræ I applied a pair of scissars, so as to bend their respective filaments with sufficient force to make them touch the stigma; but this did not produce the proper contraction of the filament. The incurvation remained only so long as the instrument was applied; on its being removed, the stamen returned to the petal by its natural elasticity. But on the scissars being applied to the irritable part, the anthera immediately flew to the stigma, and remained there. A very sudden and smart shock given to any part of a stamen would sometimes have the same effect as touching the irritable part.

Hence it was evident, that the motion above described was owing to a high degree of irritability in the side of each filament next the germen, by which,

* Founder and President of the Linnæan Society, and highly distinguished for his botannical works.

when touched, it contracts, that side becomes shorter than the other, and consequently the filament is bent towards the germen. I could not discover any thing particular in the structure of that or any other part of the filament. This irritability is perceptible in stamina of all ages, and not merely in those which are just about discharging their pollen. In some flowers which were only so far expanded that they would barely admit a bristle, and whose antheræ were not near bursting, the filaments appeared almost as irritable as in flowers fully opened; and in several old flowers, some of whose petals with the stamina adhering to them were falling off, the remaining filaments, and even those which were already fallen to the ground, proved full as irritable as any I had examined.

From some flowers I carefully removed the germen, without touching the filaments, and then applied a bristle to one of them, which immediately contracted, and the stigma being out of its way, it was bent quite over to the opposite side of the flower. Observing the stamina in some flowers which had been irritated returning to their original situations in the hollows of the petals, I found the same thing happened to all of them sooner or later. I then touched some filaments which had perfectly resumed their former stations, and found them contract with as much facility as before. This was repeated 3 or 4 times on the same filament. I attempted to stimulate in the midst of their progress some which were returning, but not always with success; a few of them only were slightly affected by the touch.

The purpose which this curious contrivance of nature answers in the private economy of the plant, seems not hard to be discovered. When the stamina stand in their original position, the antheræ are effectually sheltered from rain by the concavity of the petals. Thus probably they remain till some insect coming to extract honey from the base of the flower, thrusts itself between their filaments, and almost unavoidably touches them in the most irritable part: thus the impregnation of the germen is performed; and as it is chiefly in fine sunny weather that insects are on the wing, the pollen is also in such weather most fit for the purpose of impregnation. It would be worth while to place a branch of the Barberry flower in such a situation, as that no insect, or other irritating cause, could have access to it; to watch whether in that case the antheræ would ever approach the stigma, and whether the seeds would be prolific.

The Barberry is not the only plant which exhibits this phænomenon. The stamina of Cactus Tuna, a kind of Indian fig, are likewise very irritable. These stamina are long and slender, standing in great numbers round the inside of the flower. If a quill or feather be drawn through them, they begin in the space of 2 or 3 seconds to lie down gently on one side, and in a short time they are all recumbent at the bottom of the flower. The motions in Dionæa muscipula, Mimosa sensitiva and pudica, are too well known to be mentioned here. A similar phænomenon has been observed, where indeed an obvious botanical analogy would lead one to expect it, in the Drosera. See Dr. Withering's Botanical Arrangement of British Plants. All these movements are, I think, certainly to be attributed to irritability. We must be careful not to confound them with other movements which, however wonderful at first sight, are to be explained merely on mechanical principles. The stamina of the Parietaria, for instance, are held in such a constrained curved position by the leaves of the calyx, that as soon as the latter become fully expanded, or are by any means removed, the stamina, being very elastic, fly up, and throw their pollen about with great force. I have lately observed a similar circumstance in the flowers of Medicago falcata. In this plant the organs of generation are held in a straight position by the carina of the flower, notwithstanding the strong tendency of the infant germen to assume its proper falcated form. At length, when the germen becomes stronger, and the carina more open, it obtains its liberty by a sudden spring, in consequence of which the pollen is plentifully scattered about the stigma. The germen may at pleasure be set at liberty by nipping the flower so as gently to open the carina, and the same effect will be produced.

As the foregoing experiments show vegetables to possess irritability in common with animals, so there are plants which seem to be endued with a kind of spontaneous motion. Linnæus having observed that the rue moves one of its stamina every day to the pistillum, I examined the Ruta chalepensis, which differs very little from the common rue, and found many of the stamina in the position which he describes, holding their antheræ over the stigma; while those which had not yet come to the stigma were laying back upon the petals, as well as those which, having already performed their office, had returned to their original situation. Trying with a quill to stimulate the stamina, I found them all quite devoid of irritability. They are stout, strong, conical bodies, and cannot, without breaking, be forced out of the position in which they happen to be. The same phænomenon has been observed in several other flowers; but it is no where more striking or more easily examined than in the rue.

I could wish to find an instance of this spontaneous motion combined with irritability in one and the same plant; but I confess I do not know one. From analogy I should think it not impossible that the Dionæa muscipula, and perhaps the Droseræ, may have the same motion in their stamina as the Ruta, Parnassia, and Saxifraga, while their leaves possess irritability. But if this be the case, the seats of these 2 properties, being so different and remote from each other, should seem to have as little connection as if in 2 different plants. There still remains then this difference between animals and vegetables, that though some of the latter possess irritability, and others spontaneous motion, even in a superior degree to many of the former, yet those properties have hitherto in animals only been found combined in one and the same part. Even Sertulariæ are not an exception to this observation. The greater part of their substance indeed resembles that of plants in being indefinitely extended, and in wanting irritability and spontaneous motion. But their animated flowers or polypes, in which the essence of their being resides, are endued with both these properties in a high degree.

I know it is the opinion of some philosophers, that a certain degree of irritability must pervade every part of vegetables, as the propulsion of their fluids cannot well be conceived to be accomplished by any other means. In a conversation on this subject with the celebrated M. Bonnet, of Geneva, he informed me that he is strongly of this opinion; and that he should not despair, by throwing acid or other stimulating injections into the vessels of some plants, of seeing with a microscope at once the propulsion of the sap, and the contractions by which it is performed. He urged me, with that amiable enthusiasm for which he is remarkable, to pursue the inquiry. Whether I do so or not, I think the idea too interesting to be kept to myself, and should be glad to see it realized by any one who has time and abilities for such investigations, who has accuracy and coolness in making his experiments, as well as fidelity and impartiality in recording them.

I cannot conclude this paper without taking notice of another very curious property which vegetables seem to possess in common with animals, though certainly in a very inferior degree: I mean that property, to use the words of Mr. Hunter, who has studied this principle to a vast extent in the animal economy, by which their constitution is capable only of a certain degree of action consistently with health; when that degree is exceeded, disease or death is the consequence. It is only by the help of this principle that I can explain why many plants resist a great degree of cold for several winters before flowering; but after that critical event they perish at the first approach of cold, and can by no art be preserved so as to survive the winter. But a more curious instance is that mentioned by Linnæus, without an explanation, in his Dissertation on the Sexes of Plants, of the long duration of the pistilla in the female hemp, while unexposed to the male pollen; whereas those to which the pollen had access immediately faded and withered away. In this case I cannot help thinking, that in those pistilla on which the pollen had acted, and which consequently had performed the function for which they were designed, the vital principle was much sooner exhausted than in those which had known no such stimulus. It is perhaps for the same reason that double flowers, in which, the organs of generation being obliterated, no impregnation can take place, last much longer in perfection than single ones of the same species, as is notoriously the case with poppies, anemonies, &c. In single poppies the corolla falls off in a few hours; but in double ones it lasts several days; and this may possibly, combined with other observations, lead to a discovery of the real use of the corolla of plants, and the share it has in the impregnation, about which there has yet been no probable conjecture.

XIII. An Account of Experiments made by Mr. John M'Nab, at Albany Fort, Hudson's Bay, relative to the Freezing of Nitrous and Vitriolic Acids. By Henry Cavendish, Esq. F. R. S., and A. S. p. 166.

From the experiments made by Mr. M'Nab, of which I gave an account in the 76th volume of the Philos. Trans. p. 241, it appeared that spirit of nitre was subject, not only to what I call the aqueous congelation, namely, that in which it is chiefly, and perhaps entirely, the watery part which freezes, but also to another kind, in which the acid itself freezes, and which I call the spirituous congelation. When its strength is such as not to dissolve so much as $\frac{243}{1000}$ of its weight of marble, or when its strength is less than .243, as I call it for shortness, it is liable to the aqueous congelation solely; and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold when the strength is .411, in which case the freezing point is at $-1\frac{1}{2}°$. When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of .411 than the unfrozen part. The freezing points answering to different degrees of strength, seemed to be as annexed.

Strength.	Freezing points.	
.54	$-31\frac{1}{2}°$	spirit. congel.
.411	$-1\frac{1}{2}$	spirit. congel.
.38	$-5\frac{1}{4}$	spirit. congel.
.243	$-44\frac{1}{4}$	aqu. congel.
.21	-17	aqu. congel.

As some of these properties however were deduced from reasoning not sufficiently easy to strike the generality of readers with much conviction, Mr. M'Nab was desired to try some more experiments to ascertain the truth of it; which he has executed with the same care and accuracy as the former. For this purpose, I sent him some bottles of spirit of nitre of different strengths, and he was desired to expose each of these liquors to the cold till they froze; then to try their temperature by a thermometer; afterwards to keep them in a warm room till the ice was almost melted, and then again expose them to the cold, and when a considerable part of the acid had frozen to try the temperature a 2d time; then to decant the unfrozen part into another bottle, and send both parts back to England, that their strength might be examined. The intent of this 2d exposure to the cold was as follows: spirit of nitre bears, like other liquors, to be cooled greatly below its freezing point without freezing: then the congelation begins suddenly; the liquor is filled with fine spicula of frozen matter, and the ice becomes so loose and porous, that if the process be continued long enough for a considerable portion of the acid to congeal, scarcely any of the fluid part can be decanted: whereas, if it be heated in this state till the frozen part is almost, but not entirely, melted, and be again exposed to the cold, as the liquor is then in contact with the congealed matter, it begins to freeze as soon as it arrives at the freezing point, and the ice becomes much more solid and compact.

The intent of decanting the fluid part, and sending both parts back, that their strength might be determined, was partly to examine the truth of the supposition laid down in my former paper, that the strength of the frozen part approaches nearer to .411 than that of the unfrozen; but it is also a necessary step towards determining the freezing point answering to a given strength of the acid; for as the frozen part is commonly of a different strength from the unfrozen, the strength of the fluid part, and the cold necessary to make it freeze, is continually altering during the progress of the congelation. In consequence of this, the temperature of the liquor is not that with which the frozen part congealed; but it is that necessary to make the remainder, or the fluid part, begin to freeze, or in other words it is the freezing point of the fluid part. This is the reason that a thermometer, placed in spirit of nitre, continually sinks during the progress of congelation; which is contrary to what is observed in pure water, and other fluids in which no separation of parts is produced by freezing.

Further, from the above-mentioned experiments of Mr. M'Nab it appeared, that oil of vitriol, as well as spirit of nitre, is subject to the spirituous congelation; but it seemed uncertain whether, like the latter, it had any point of easiest freezing, or whether it did not uniformly freeze with less cold as the strength increased. For this reason, some bottles of oil of vitriol, of different strengths, were sent, which he was desired to try in the same manner as the former. This point indeed has since been determined by Mr. Keir, who has shown that oil of vitriol has strength of easiest freezing; and that at that point a remarkably slight degree of cold is sufficient for its congelation. The result of Mr. M'Nab's experiments on the nitrous acid is given in the following table.

N°	Decanted part.		Undecanted part.		Strength of the whole mass.	Strength before sent.	Freezing point by first method.	Freezing point by second method.
	Quantity.	Strength.	Quantity.	Strength.				
6	— —	— —	— —	— —	— —	.561	−41.6	— —
7	1410	.445	2137	.435	.439	.437	+ 1.7	− 3.8
8	1658	.390	1940	.422	.407	.408	− 3.5	− 4
9	1368	.353	2438	.416	.393	.391	− 4.5	−11
10	2206	.343	1920	.373	.357	.357	−12.5	−13.8
11	3620	.310	602	.381	.320	.320	−22.5	−23
12	2155	.276	1494	.293	.283	.280	−39.1	−40.3
13	1618	.241	1961	.235	.238	.238	−34	−32

The first column contains the numbers by which Mr. M'Nab has distinguished the different bottles. The 2d and 3d columns contain the quantity and strength of the decanted part of the liquor; and the 4th and 5th show the quantity and strength of the undecanted part of the liquor. The 6th column gives the strength of both parts put together, or the strength of the whole mass; and the 7th is the strength of the same acid, as it was determined before it was sent to Hudson's Bay. The strengths of the decanted and undecanted parts were found by saturating the liquor returned home with marble; and that of the whole mass was inferred by computation from the quantity and strength of the decanted and undecanted parts; and as the strength thus inferred never differs from that determined before the liquors were sent to Hudson's Bay by more than $\frac{1}{100}$ part of the whole, it is not likely that the strengths of the decanted and undecanted parts here set down should differ from the truth by much more than that quantity. The 8th column contains the freezing points found in the first method, or the temperature of the liquors after the hasty congelation which took place on exposing them to the cold without any frozen matter in them; and the 9th contains their temperature after the more gradual congelation which took place when they were cooled with some frozen matter in them; and as the unfrozen part of the acid was decanted immediately after the temperature had been observed, it follows, that this column shows the true freezing points of the decanted liquors. In like manner the 8th column shows the freezing points of that part of the liquor which remained fluid in the first manner of trying the experiment; but as the strength of this part was not determined, the precise strengths to which these freezing points correspond are unknown. Thus much however is certain, that these points must be below those of the whole mass, and in all probability must be above those of the decanted liquor; as there is great reason to think that the quantity of frozen matter was always less, and consequently the strength of the fluid part differed less from that of the whole mass, in the first way of trying the experiment than in the second.

In all the foregoing acids the ice was heavier than the fluid part, and in consequence subsided to the bottom; a proof that it was the spirituous congelation which had taken place in them: but in N° 13 the frozen part swam at top, which shows that the congelation was of the aqueous kind.

As the temperatures in the 9th column of the foregoing table, are the freezing points answering to the strengths expressed in the 3d column; and as — $41\frac{1}{2}$ is the freezing point answering to the strength of .561; whence the freezing points determined by these experiments, and their respective strengths, are as annexed:

Strength.	Freezing point.
.561	— 41.6°
.445	— 3.8
.390	— 4
.353	— 11
.343	— 13.8
.310	— 23
.276	— 40.3

By interpolation from these data, according to Newton's method*, it appears

* Princip. Math. Lib. 3, prop. 40, lem. 5.

that the strength at which the acid freezes with the least cold is .418, and that the freezing point answering to that strength is — $2\frac{4}{10}°$.

In order to show more readily the freezing point answering to any given strength, I have computed, by the same method, the annexed table, in which the strengths increase in arithmetical progression. It was before shown that the freezing points, found by the first method, ought to be below those of the whole mass, and must in all probability be above those of the decanted liquor. In order to see how this agrees with observation, I computed in the above-mentioned manner the freezing points answering to the strength of the whole mass, and compared them with the observed freezing points. The result is given in the following table.

Strength.	Freezing point.	Difference.
.568	— 45.5	+ 15.4
.538	— 30.1	+ 12
.508	— 18.1	+ 8.7
.478	— 9.4	+ 5.3
.448	— 4.1	+ 1.7
.418	— 2.4	— 1.8
.388	— 4.2	— 5.5
.358	— 9.7	— 8
.328	— 17.7	— 10
.298	— 27.7	

N°	Strength of the whole mass.	Strength of the decanted liquor.	Computed freezing point of the whole mass.	Observed freezing point.	
				In first method.	In second method.
7	.439	.445	— 3.2	+ 1.7	— 3.8
8	.407	.390	— 2.6	— 3.5	— 4.
9	.393	.353	— 3.7	— 4.5	—11.
10	.357	.343	—10.	—12.5	—13.8
11	.320	.310	—19.9	— 22.5	—23.
12	.283	.276	—35.6	—39.1	—40.3

It may be observed, that the freezing point of N° 7, tried in the first way, is considerably above that corresponding to the strength of the whole mass; but as this experiment appears to be doubtful, and not unlikely to exceed the truth, we may safely reject it as erroneous. All the others, as might be expected, are lower than those corresponding to the strength of the whole mass, and above those observed in the 2d manner, and therefore serve to confirm the truth of the above determination of the freezing points of spirit of nitre; and also show, that in this acid the point of spirituous congelation is pretty regular, and does not depend much, if at all, on the rapidity with which the congelation is performed.

The point of aqueous congelation, however, seems liable to considerable irregularity; for N° 13, after having been exposed to the cold, froze on agitation, the congelation being of the aqueous kind, and the thermometer stood stationary therein at — 34°. The ice being then almost melted, it was again exposed to the cold, till a good deal was frozen; but yet its temperature was then no lower than — $32\frac{1}{4}°$, though the quantity of frozen matter must certainly have been much more than in the first trial. The fluid part being then decanted, and the frozen part melted, both were again exposed to the cold. They both were made to congeal by agitation, and the temperature of the undecanted was then found to be — 35°, and that of the decanted part — 37°: so that it should seem as if the freezing point found by the hasty congelation was always lower than that found the other way, which may perhaps proceed from this cause; namely, that when sufficient time is allowed, the watery part will separate from the rest, and freeze in a degree of cold much less than what is required to produce that effect, when it is performed in a more rapid manner.

These experiments confirm the truth of the conclusions I drew from Mr. M'Nab's former experiments; for, first, there is a certain degree of strength at which spirit of nitre freezes with a less degree of cold than when it is either stronger or weaker; and when spirit of nitre, of a different strength from that, is made to congeal, the frozen part approaches nearer to the foregoing degree of strength than the unfrozen. Also this strength, as well as its corresponding freezing point, and the freezing point answering to the strength of .54, come out very nearly the same as I concluded from those experiments; for by the present experiments they come out .418, — $2\frac{4}{10}$°, and — 31°, and by the former .411, — $1\frac{1}{2}$°, and — 31°. But the freezing point answering to the strength of .38 is totally different from what I there supposed. This must have been owing to the strength of that acid having been very different from what I thought it; which is not improbable, as its strength was inferred only from the quantity of snow which was added to it in finding the degree of cold produced by its mixture with snow.

On the Vitriolic Acid.—An irregularity of a remarkable kind occurred in trying 2 of these acids; namely, when the undecanted part was melted and again made to congeal, its freezing point was found to be much less cold than that of the decanted part, and the difference was much greater than could be attributed to the difference of strength. This seems to have happened only in the strongest 2 acids, namely, N° 1 and 2, and in great measure confirms the supposition which I formed from Mr. M'Nab's former experiments, that the congealed part of oil of vitriol differs from the rest, not merely in strength, but also in some other respect, which I am not acquainted with. It should seem however that this property does not extend to weak oil of vitriol. Some smaller irregularities occurred in trying the vitriolic acid, the cause of which I believe was, that when this acid has been cooled below the freezing point, and begins to freeze, the congelation proceeds but slowly; so that a considerable time elapses before it rises to the true freezing point. Something of the same kind seems to take place in the nitrous acid also, though in a less degree; for the decanted liquors usually continued to freeze and deposit a small quantity of ice, for a few minutes after they were poured off, though their cold, at least in some instances, was found rather to diminish during that time. It must be observed, that small spicula of ice always came over along with the decanted liquor; and to this in all probability the new-formed ice attached itself; for otherwise it is likely that no ice would have been produced.

The following table contains the strength of the acids as determined before they were sent to Hudson's Bay, and the quantity and strength of the decanted and undecanted parts when they arrived at London, and the strength of the whole mass as computed from thence. For the sake of uniformity, I have expressed their strengths, like those of the nitrous acid, by the quantity of marble necessary to saturate them, though I did not find their strength by actually trying how much marble they would dissolve; as that method is too uncertain, on account of the selenite formed in the operation, and which in good measure defends the marble from the action of the acid. The method I used was, to find the weight of the plumbum vitriolatum formed by the addition of sugar of lead, and thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of plumbum vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali as a quantity of nitrous acid sufficient to dissolve 33 of marble. It may be observed that the quantity of alkali, necessary to saturate a given quantity of acid, can hardly be determined with much accuracy, for which reason the foregoing less direct method was adopted; especially as the precipitation of plumbum vitriolatum shows the proportional strengths, which is the thing principally wanted, with as great accuracy as any method I know.

N°	Strength before sent.	Decanted part.		Undecanted part.		Strength of whole mass.
		Quantity.	Strength.	Quantity.	Strength.	
1	.977	1375	.967	3460	.963	.964
2	.918	3915	.919	1876	.905	.914
3	.846	88	.777	4915	.850	.849
4	.758	389	.710	3795	.753	.755
				547	.803	

The undecanted part of N° 4 was divided into 2 parts; viz. the less and the more congealable part; and it is the latter whose quantity and strength is given in the last line. It is well known that oil of vitriol attracts moisture with great avidity; and some of these acids were much exposed to the air during the experiments made with them, and may therefore be supposed to have attracted so much moisture from the air, as might sensibly diminish their strength; and this seems actually to have been the case with some of them. But as the bottles were well stopped, and as, except in one acid which was the most exposed to the air, the strength of the whole mass comes out not much less than that determined before the liquors were sent to Hudson's Bay, I imagine their strength could not sensibly al ter during their voyage home; and consequently their strength, at the time the last observations were made with them, could not differ much from that here set down.

It would be tedious to give the experiments for determining their freezing points in detail; but from these experiments it should seem, that the freezing point of oil of vitriol, answering to different strengths, is nearly as annexed: hence we may conclude, that oil of vitriol has not only strength of easiest freezing, as Mr. Keir has shown; but that, at a strength superior to this, it has another point of contrary flexure, beyond which if the strength be increased, the cold necessary to freeze it again begins to diminish. The strength answering to this latter point of contrary flexure must probably be rather more than .918, as the decanted or unfrozen part of N° 2 seemed rather stronger than the undecanted part; and for a like reason the strength of easiest freezing is rather more than .846. Mr. Keir found that oil of vitriol froze, with the least degree of cold, when its specific gravity at 60° of heat was 1.780, and that the freezing point answering to that degree of strength was + 46°: which agrees pretty nearly with these experiments, as the strength of oil of vitriol of that specific gravity is .848, that is, nearly the same as that of N° 3.

Strength.	Freezing point.
	°
.977	+ 1
.918	− 26
.846	+ 42
.758	− 45

A Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council. p. 191.

The result of the whole is as follows:

1787.	Thermometer without. Greatest height.	Least height.	Mean height.	Thermometer within. Greatest height.	Least height.	Mean height.	Barometer. Greatest height.	Least height.	Mean height.	Rain. Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January ..	51	27	39	54	45	49½	30.64	29.52	30.08	0.360
February	53	31	42	56	50	53	30.36	28.67	29.51	1.041
March ..	56	34	45	58	51½	54¾	30.51	29.03	29.77	1.283
April	58	37	47½	58	53	55½	30.48	29.07	29.77	0.923
May	71	39	55	64	53	58½	30.31	29.33	29.82	1.137
June	77	46	61½	67	59	63	30.29	29.55	22.92	0.662
July	78	51	64½	71	63	67	30 38	29.37	29.87	3.246
August ..	83½	48	65¾	73	62	67½	30.40	29.25	29.82	1.171
September	68	46	57	64	61	62½	30.45	29.11	29.78	0.990
October ..	63	39	51	62	54	58	30.15	29.21	29.68	1.818
November	56	29	42½	61	48	54½	30.45	29.12	29.78	1.340
December	53	30	41½	59	46	52½	30.40	29.21	29.80	3.000
Whole y ea			51			58			29.80	16.971

Explanation of the Instruments.—The instruments with which the foregoing observations were made are the same that were used in former observations of this kind, a full account of which was given by Henry Cavendish, Esq. in the 66th volume of the Philos. Trans.; but as they have been moved from the situations they had at that time, it may not be amiss to mention how they are placed now, in order the better to show what degree of accuracy may be expected from them. There being no one situation for a thermometer out of doors so good as could be wished, it became necessary to make use of 2 thermometers; each is placed out of a three pair of stairs window, one facing E.N.E. and the other W.S.W. and they stand about 2 or 3 inches from the wall, that they may be the more exposed to the air, and the less affected by the heat and cold of the house. As the sun shines on the eastern part of the building in the morning, the thermometer to the westward is made use of for the morning observation during that season of the year when the sun rises high enough to affect the other; for all other observations, that to the eastward is employed. Neither the building opposite, nor that on the south side of the thermometer to the east, are elevated above it in an angle of more than 13°; but the opposite building is not more than 25 feet distant. The thermometer to the westward will not be affected by any other building than one to the northward, which is elevated above it in an angle of 20°, and which is only 20 feet distant. The thermometer within doors is placed close to the barometer, the heights of which it is intended chiefly to correct; the windows of the room in which they are kept look to the westward, and in winter the room has constantly a fire in it. The vessel which receives the rain is fixed to a chimney at the top of the house, and rises 6 inches above the chimney; it is not screened from the rain by any building or chimneys, there being none higher than that to which it is fixed.

*XIV. On the Natural History of the Cuckoo. By Mr. Edward Jenner.** p. 219.

The first appearance of cuckoos in Gloucestershire, the part of England where these observations were made, is about the 17th of April. The song of the male, which is well known, soon proclaims its arrival. The song of the female, if the peculiar notes of which it is composed may be so called, is widely different, and has been so little attended to, that I believe few are acquainted with it. I know not how to convey a proper idea of it by a comparison with the notes of any other bird; but the cry of the dab-chick bears the nearest resemblance to it.

Unlike the generality of birds, cuckoos do not pair. When a female appears on the wing, she is often attended by 2 or 3 males, who seem to be earnestly contending for her favours. From the time of her appearance, till after the middle of summer, the nests of the birds selected to receive her egg are to be found in

* Now Dr. Jenner, the celebrated discoverer of vaccination.

great abundance; but like the other migrating birds, she does not begin to lay till some weeks after her arrival. I never could procure an egg till after the middle of May, though probably an early-coming cuckoo may produce one sooner.*

The cuckoo makes choice of the nests of a great variety of small birds. I have known its egg entrusted to the care of the hedge-sparrow, the water-wagtail, the titlark, the yellow-hammer, the green linnet, and the whinchat. Among these it generally selects the 3 former; but shows a much greater partiality to the hedge-sparrow than to any of the rest: therefore, for the purpose of avoiding confusion, this bird only, in the following account, will be considered as the foster-parent of the cuckoo, except in instances which are particularly specified.

The hedge-sparrow commonly takes up 4 or 5 days in laying her eggs. During this time, generally after she has laid 1 or 2, the cuckoo contrives to deposit her egg among the rest, leaving the future care of it entirely to the hedge-sparrow. This intrusion often occasions some discomposure; for the old hedge-sparrow at intervals, while she is sitting, not unfrequently throws out some of her own eggs, and sometimes injures them in such a way that they become addle; so that it more frequently happens, that only 2 or 3 hedge sparrow's eggs are hatched with the cuckoo's, than otherwise: but whether this be the case or not, she sits the same length of time as if no foreign egg had been introduced, the cuckoo's egg requiring no longer incubation than her own. However, I have never seen an instance where the hedge-sparrow has either thrown out or injured the egg of the cuckoo. When the hedge-sparrow has sat her usual time, and disengaged the young cuckoo and some of her own offspring from the shell,† her own young ones, and any of her eggs that remain unhatched, are soon turned out, the young cuckoo remaining possessor of the nest, and sole object of her future care. The young birds are not previously killed, nor are the eggs demolished; but all are left to perish together, either entangled about the bush which contains the nest, or lying on the ground under it.

The early fate of the young hedge-sparrows is a circumstance that has been noticed by others, but attributed to wrong causes. A variety of conjectures have been formed on it. Some have supposed the parent cuckoo the author of their destruction; while others, as erroneously, have pronounced them smothered by the disproportioned size of their fellow-nestling. Now the cuckoo's egg

* What is meant by an early-coming cuckoo, I shall more fully explain in a paper on the migration of birds; but it may be necessary to mention here, that migrating birds of the same species arrive and depart in succession. Cuckoos, for example, appear in greater numbers on the 2d than on the 1st week of their arrival, and they disappear in the same gradual manner.—Orig.

† The young cuckoo is commonly hatched first.—Orig.

being not much larger than the hedge-sparrow's, it necessarily follows, that at first there can be no great difference in the size of the birds just burst from the shell. Of the fallacy of the former assertion also I was some years ago convinced, by having found that many cuckoo's eggs were hatched in the nests of other birds after the old cuckoo had disappeared; and by seeing the same fate then attend the nestling sparrows as during the appearance of old cuckoos in this country. But, before proceeding to the facts relating to the death of the young sparrows, it will be proper to state some examples of the incubation of the egg, and the rearing of the young cuckoo; since even the well known fact, that this business is entrusted to the care of other birds, has been controverted by the Hon. Daines Barrington; and since, as it is a fact so much out of the ordinary course of nature, it may still probably be disbelieved by others.

Exam. 1. The titlark is frequently selected by the cuckoo to take charge of its young one; but as it is a bird less familiar than many others, its nest is not so often discovered. I have however had several cuckoo's eggs brought to me that were found in titlark's nests; and had one opportunity of seeing the young cuckoo in the nest of this bird: I saw the old birds feed it repeatedly, and, to satisfy myself that they were really titlarks, shot them both, and found them to be so.

Exam. 2. A cuckoo laid her egg in a water-wagtail's nest in the thatch of an old cottage. The wagtail sat her usual time, and then hatched all the eggs but one; which, with all the young ones, except the cuckoo was turned out of the nest. The young birds, consisting of 5, were found on a rafter that projected from under the thatch, and with them was the egg, not in the least injured. On examining the egg, I found the young wagtail it contained quite perfect, and just in such a state as birds are when ready to be disengaged from the shell. The cuckoo was reared by the wagtails till it was nearly capable of flying, when it was killed by an accident.

Exam. 3. A hedge-sparrow built her nest in a hawthorn bush in a timber-yard: after she had laid 2 eggs, a cuckoo dropped in a 3d. The sparrow continued laying, as if nothing had happened, till she had laid 5, her usual number, and then sat. On inspecting the nest, June 20, 1786, I found that the bird had hatched this morning, and that every thing but the young cuckoo was thrown out. Under the nest I found 1 of the young hedge-sparrows dead, and 1 egg by the side of the nest entangled with the coarse woody materials that formed its outside covering. On examining the egg, I found one end of the shell a little cracked, and could see that the sparrow it contained was yet alive. It was then restored to the nest, but in a few minutes was thrown out. The egg being again suspended by the outside of the nest, was saved a second time from breaking. To see what would happen if the cuckoo was removed, I took out the cuckoo, and placed the egg containing the hedge-sparrow in the nest in its stead. The old birds during this time flew about the spot, showing signs of great anxiety; but when I withdrew they quickly came to the nest again. On looking into it in a quarter of an hour afterwards, I found the young one completely hatched, warm and lively. The hedge-sparrows were suffered to remain undisturbed with their new charge for 3 hours, during which time they paid every attention to it, when the cuckoo was again put into the nest. The old sparrows had been so much disturbed by these intrusions, that for some time they showed an unwillingness to come to it: however, at length they came, and on examining the nest again in a few minutes, I found the young sparrow was tumbled out. It was a 2d time restored, but again experienced the same fate. From these experiments, and supposing, from the feeble appearance of the young cuckoo, just disengaged from the shell, that it was utterly incapable of displacing either the egg or the young sparrows, I was induced to believe that the old sparrows were the only agents in this seeming unnatural business; but I afterwards clearly perceived the cause of this strange phenomenon, by discovering the young cuckoo in the act of displacing its fellow-nestlings, as the following relations will fully evince.

June 18, 1787, I examined the nest of a hedge-sparrow, which then contained a cuckoo's and 3 hedge-sparrow's eggs. On inspecting it the day following I found the bird had hatched, but that the nest now contained only a young cuckoo and 1 hedge-sparrow. The nest was placed so near the extremity of a hedge, that I could distinctly see what was going forward in it; and to my astonishment, saw the young cuckoo, though so newly hatched, in the act of turning out the young hedge-sparrow. The mode of accomplishing this was very curious. The little animal, with the assistance of its rump and wings, contrived to get the bird on its back, and making a lodgement for the burden by elevating its elbows, clambered backward with it up the side of the nest till it reached the top, where resting for a moment, it threw off its load with a jirk, and quite disengaged it from the nest. It remained in this situation a short time, feeling about with the extremities of its wings, as if to be convinced whether the business was properly executed, and then dropped into the nest again. With these, the extremities of its wings, I have often seen it examine, as it were, an egg and nestling before it began its operations; and the nice sensibility which these parts appeared to possess seemed sufficiently to compensate the want of sight, which as yet it was destitute of. I afterwards put in an egg, and this, by a similar process, was conveyed to the edge of the nest, and thrown out. These experiments I have since repeated several times in different nests, and have always found the young cuckoo disposed to act in the same manner. In climbing up the nest, it sometimes drops its burden, and thus is foiled in its endeavours; but after a little respite, the work is resumed, and goes on almost incessantly till it is effected. It is wonderful to see the extraordinary exertions of the young cuckoo, when it is 2 or 3 days old, if a bird be put into the nest with it that is too weighty for it to lift out. In this state it seems ever restless and uneasy. But this disposition for turning out its companions begins to decline from the time it is 2 or 3 till it is about 12 days old, when, as far as I have hitherto seen, it ceases. Indeed, the disposition for throwing out the egg appears to cease a few days sooner; for I have frequently seen the young cuckoo, after it had been hatched 9 or 10 days, remove a nestling that had been placed in the nest with it, when it suffered an egg, put there at the same time, to remain unmolested. The singularity of its shape is well adapted to these purposes; for, different from other newly-hatched birds, its back from the scapulæ downwards is very broad, with a considerable depression in the middle. This depression seems formed by nature for the design of giving a more secure lodgement to the egg of the hedge-sparrow, or its young one, when the young cuckoo is employed in removing either of them from the nest. When it is about 12 days old, this cavity is quite filled up, and then the back assumes the shape of nestling birds in general.

Having found that the old hedge-sparrow commonly throws out some of her own eggs after her nest has received the cuckoo's, and not knowing how she might treat her young ones, if the young cuckoo was deprived of the power of dispossessing them of the nest, I made the following experiment. July 9. A young cuckoo, that had been hatched by a hedge-sparrow about 4 hours, was confined in the nest in such a manner that it could not possibly turn out the young hedge-sparrows which were hatched at the same time, though it was almost incessantly making attempts to effect it. The consequence was, the old birds fed the whole alike, and appeared in every respect to pay the same attention to their own young as to the young cuckoo, till the 13th, when the nest was unfortunately plundered.

The smallness of the cuckoo's egg, in proportion to the size of the bird, is a circumstance that hitherto I believe has escaped the notice of the ornithologist. So great is the disproportion, that it is in general smaller than that of the house-sparrow; whereas the difference in the size of the birds is nearly as 5 to 1. I have used the term in general, because eggs produced at different times by the same bird vary very much in size. I found a cuckoo's egg so light that it weighed only 43 grs., and one so heavy that it weighed 55 grs. The colour of the cuckoo's eggs is extremely variable. Some, both in ground and penciling, very much resemble the house-sparrow's; some are indistinctly covered with bran-coloured spots; and others are marked with lines of black, resembling in some measure the eggs of the yellow-hammer.

The circumstance of the young cuckoo's being destined by nature to throw out the young hedge-sparrows, seems to account for the parent-cuckoo's dropping her egg in the nests of birds so small as those I have particularized. If she were to do this in the nest of a bird which produced a large egg, and consequently a large nestling, the young cuckoo would probably find an insurmountable difficulty in solely possessing the nest, as its exertions would be unequal to the labour of turning out the young birds.* Besides, though many of the larger birds might have fed the nestling cuckoo very properly, had it been committed to their charge, yet they could not have suffered their own young to have been sacrificed, for the accommodation of the cuckoo, in such great number as the smaller ones, which are so much more abundant; for though it would be a vain attempt to calculate the numbers of nestlings destroyed by means of the cuckoo, yet the slightest observation would be sufficient to convince us that they must be very large. Here it may be remarked, that though nature permits the young cuckoo to make this great waste, yet the animals thus destroyed are not thrown away or rendered useless. At the season when this happens, great numbers of tender quadrupeds and reptiles are seeking provision; and if they find the callow nestlings which have fallen victims to the young cuckoo, they are furnished with food well adapted to their peculiar state.

It appears a little extraordinary, that 2 cuckoo's eggs should ever be deposited in the same nest, as the young one produced from one of them must inevitably perish; yet I have known 2 instances of this kind, one of which I shall relate. June 27, 1787, 2 cuckoos and a hedge-sparrow were hatched in the same nest this morning; one hedge-sparrow's egg remained unhatched. In a few hours after, a contest began between the cuckoos for the possession of the nest, which continued undetermined till the next afternoon; when one of them, which was somewhat superior in size, turned out the other, together with the young hedge-sparrow and the unhatched egg. This contest was very remarkable. The combatants alternately appeared to have the advantage, as each carried the other several times nearly to the top of the nest, and then sunk down again, oppressed by the weight of its burden; till at length, after various efforts, the strongest prevailed, and was afterwards brought up by the hedge-sparrows.

I come now, to consider the principal matter that has agitated the mind of the naturalist respecting the cuckoo; why, like other birds, it should not build a

* I have known an instance in which a hedge-sparrow sat on a cuckoo's egg and one of her own. Her own egg was hatched 5 days before the cuckoo's, when the young hedge-sparrow had gained such a superiority in size that the young cuckoo had not powers sufficient to lift it out of the nest till it was 2 days old, by which time it was grown very considerably. This egg was probably laid by the cuckoo several days after the hedge-sparrow had begun to sit; and even in this case it appears that its presence had created the disturbance before alluded to, as all the hedge-sparrow's eggs were gone except one.—Orig.

nest, incubate its eggs, and rear its own young? There is certainly no reason to be assigned, from the formation of this bird, why, in common with others, it should not perform all these several offices; for it is in every respect perfectly formed for collecting materials and building a nest. Neither its external shape nor internal structure prevent it from incubation; nor is it by any means incapacitated from bringing food to its young. It would be needless to enumerate the various opinions of authors on this subject, from Aristotle to the present time. Those of the ancients appear to be either visionary, or erroneous; and the attempts of the moderns towards its investigation have been confined within very narrow limits; for they have gone but little farther in their researches than to examine the constitution and structure of the bird, and having found it possessed of a capacious stomach with a thin external covering, concluded that the pressure on this part, in a sitting posture, prevented incubation. They have not considered that many of the birds which incubate have stomachs analogous to those of cuckoos: the stomach of the owl, for example, is proportionably capacious, and is almost as thinly covered with external integuments. Nor have they considered that the stomachs of nestlings are always much distended with food; and that this very part, during the whole time of their confinement to the nest, supports, in a great degree, the weight of the whole body; whereas, in a sitting bird, it is not nearly so much pressed on; for the breast in that case fills up chiefly the cavity of the nest, for which purpose, from its natural convexity, it is admirably well fitted.

These observations, I presume, may be sufficient to show that the cuckoo is not rendered incapable of sitting through a peculiarity either in the situation or formation of the stomach; yet, as a proof still more decisive, I shall state the following fact. In the summer of the year 1786, I saw, in the nest of a hedge-sparrow, a cuckoo, which, from its size and plumage, appeared to be nearly a fortnight old. On lifting it up in the nest, I observed 2 hedge-sparrow's eggs under it. At first I supposed them part of the number which had been sat on by the hedge-sparrow with the cuckoo's egg, and that they had become addle, as birds frequently suffer such eggs to remain in their nests with their young; but on breaking one of them I found it contained a living fœtus; so that of course these eggs must have been laid several days after the cuckoo was hatched, as the latter now completely filled up the nest, and was by this peculiar incident performing the part of a sitting-bird.*

Having under my inspection, in another hedge-sparrow's nest, a young cuckoo, about the same size as the former, I procured 2 wagtail's eggs which had been

* At this time I was unacquainted with the fact, that the young cuckoo turned out the eggs of the hedge-sparrow; but it is reasonable to conclude, that it had lost the disposition for doing this when these eggs were deposited in the nest.—Orig.

sat on a few days, and had them immediately conveyed to the spot, and placed under the cuckoo. On the 9th day after the eggs had been in this situation, the person appointed to superintend the nest, as it was some distance from the place of my residence, came to inform me, that the wagtails were hatched. On going to the place, and examining the nest, I found nothing in it but the cuckoo and the shells of the wagtail's eggs. The fact therefore of the birds being hatched, I do not give as coming immediately under my own eye; but the testimony of the person appointed to watch the nest was corroborated by that of another witness.

To what cause then may we attribute the singularities of the cuckoo? May they not be owing to the following circumstances? The short residence this bird is allowed to make in the country where it is destined to propagate its species, and the call that nature has on it, during that short residence, to produce a numerous progeny. The cuckoo's first appearance here is about the middle of April, commonly on the 17th. Its egg is not ready for incubation till some weeks after its arrival, seldom before the middle of May. A fortnight is taken up by the sitting bird in hatching the egg. The young bird generally continues 3 weeks in the nest before it flies, and the foster-parents feed it more than 5 weeks after this period; so that, if a cuckoo should be ready with an egg much sooner than the time pointed out, not a single nestling, even one of the earliest, would be fit to provide for itself before its parent would be instinctively directed to seek a new residence, and be thus compelled to abandon its young one; for old cuckoos take their final leave of this country the first week in July.

Had nature allowed the cuckoo to have staid here as long as some other migrating birds, which produce a single set of young ones, as the swift or nightingale for example, and had allowed her to have reared as large a number as any bird is capable of bringing up at one time, these might not have been sufficient to have answered her purpose; but by sending the cuckoo from one nest to another, she is reduced to the same state as the bird whose nest we daily rob of an egg, in which case the stimulus for incubation is suspended. Of this we have a familiar example in the common domestic fowl. That the cuckoo actually lays a great number of eggs, dissection seems to prove very decisively. On a comparison I had an opportunity of making between the ovarium, or racemus vitellorum, of a female cuckoo, killed just as she had begun to lay, and of a pullet killed in the same state, no essential difference appeared. The uterus of each contained an egg perfectly formed, and ready for exclusion; and the ovarium exhibited a large cluster of eggs gradually advanced from a very diminutive size, to the greatest the yolk acquires before it is received into the oviduct. The appearance of one killed on the 3d of July was very different: in this I could distinctly trace a great number of the membranes which had discharged yolks into the oviduct; and one of them appeared as if it had parted with a yolk the preceding day. The ovarium still exhibited a cluster of enlarged eggs; but the most forward of them was scarcely larger than a mustard seed.

I would not be understood to advance that every egg which swells in the ovarium, at the approach or commencement of the propagating season, is brought to perfection; but it appears clearly that a bird, in obedience to the dictates of her own will, or to some hidden cause in the animal economy, can either retard or bring forward her eggs. Besides the example of the common fowl above alluded to, many others occur. If you destroy the nest of a blackbird, a robin, or almost any small bird, in the spring, when she has laid her usual number of eggs, it is well known to every one, who has paid any attention to inquiries of this kind, in how short a space of time she will produce a fresh set. Now had the bird been suffered to have proceeded without interruption in her natural course, the eggs would have been hatched, and the young ones brought to a state capable of providing for themselves, before she would have been induced to make another nest, and excited to produce another set of eggs from the ovarium. If the bird had been destroyed at the time she was sitting on her first laying of eggs, dissection would have shown the ovarium containing a great number in an enlarged state, and advancing in the usual progressive order. Hence it plainly appears, that birds can keep back or bring forward, under certain limitations, their eggs at any time during the season appointed for them to lay; but the cuckoo, not being subject to the common interruptions, goes on laying from the time she begins, till the eve of her departure from this country: for though old cuckoos in general take their leave the first week in July, and I never could see one after the 5th day of that month,* yet I have known an instance of an egg's being hatched in the nest of a hedge-sparrow so late as the 15th. And a further proof of their continuing to lay till the time of their leaving us may, I think, be fairly deduced from the appearances on dissection of the female cuckoo above mentioned, killed on the 3d of July.

Among the many peculiarities of the young cuckoo, there is one that shows itself very early. Long before it leaves the nest, it frequently, when irritated, assumes the manner of a bird of prey, looks ferocious, throws itself back, and pecks at any thing presented to it with great vehemence, often at the same time making a chuckling noise like a young hawk. Sometimes, when disturbed in a smaller degree, it makes a kind of hissing noise, accompanied with a heaving motion of the whole body.† The growth of the young cuckoo is uncommonly

* Though I am unacquainted with an instance, yet I conceive it possible, that here and there a straggling cuckoo may be seen after this time.—Orig.

† Young animals, being deprived of other modes of defence, are probably endowed with the powers of exciting fear in their common enemies. If you but slightly touch the young hedge-hog,

rapid. The chirp is plaintive, like that of the hedge-sparrow; but the sound is not acquired from the foster-parent, as it is the same whether it be reared by the hedge-sparrow, or any other bird. It never acquires the adult note during its stay in this country.

The stomachs of young cuckoos contain a great variety of food. On dissecting one that was brought up by wagtails, and fed by them at the time it was shot, though it was nearly of the size and fulness of plumage of the parent-bird, I found in its stomach the following substances. Flies and beetles of various kinds: small snails, with their shells unbroken: grashoppers: caterpillars: part of a horse-bean: a vegetable substance resembling bits of tough grass, rolled into a ball: the seeds of a vegetable that resembled those of the goose-grass.

In the stomach of one fed by hedge-sparrows, the contents were almost entirely vegetable; such as wheat, small vetches, &c. But this was the only instance of the kind I had ever seen, as these birds in general feed the young cuckoo with scarcely any thing but animal food. However, it served to clear up a point which before had somewhat puzzled me; for having found the cuckoo's egg in the nest of a green linnet, which begins very early to feed its young with vegetable food, I was apprehensive, till I saw this fact, that this bird would have been an unfit foster-parent for the young cuckoo. The titlark, I observe, feeds it principally with grashoppers. But the most singular substance, so often met with in the stomachs of young cuckoos, is a ball of hair curiously wound up. I have found it of various sizes, from that of a pea to that of a small nutmeg. It seems to be composed chiefly of horse-hairs, and from the resemblance it bears to the inside covering of the nest, I conceive the bird swallows it while a nestling. In the stomachs of old cuckoos I have often seen masses of hair; but these had evidently once formed a part of the hairy caterpillar, which the cuckoo often takes for its food.

There seems to be no precise time fixed for the departure of young cuckoos. I believe they go off in succession, probably as soon as they are capable of taking care of themselves; for though they stay here till they become nearly equal in size and growth of plumage to the old cuckoo, yet in this very state the fostering care of the hedge-sparrow is not withdrawn from them. I have frequently seen the young cuckoo of such a size that the hedge-sparrow has perched on its back, or half-expanded wing, in order to gain sufficient elevation to put the food into its mouth. At this advanced stage, I believe that young cuckoos procure some food for themselves; like the young rook for instance,

for instance, before it becomes fully armed with its prickly coat, the little animal jumps up with a sudden spring, and imitates very closely the sound of the word hush! as we pronounce it in a loud whisper. This disposition is apparent in many other animals.—Orig.

which in part feeds itself, and is partly fed by the old ones till the approach of the pairing season. If they did not go off in succession, it is probable we should see them in large numbers by the middle of August; for as they are to be found in great plenty,* when in a nestling state, they must now appear very numerous, since all of them must have quitted the nest before this time. But this is not the case; for they are not more numerous at any season than the parent birds are in the months of May and June.

The same instinctive impulse which directs the cuckoo to deposit her eggs in the nests of other birds, directs her young one to throw out the eggs and young of the owner of the nest. The scheme of nature would be incomplete without it; for it would be extremely difficult, if not impossible, for the little birds, destined to find succour for the cuckoo, to find it also for their own young ones, after a certain period; nor would there be room for the whole to inhabit the nest.

XV. On the Temperament of those Musical Instruments, in which the Tones, Keys, or Frets, are fixed; as in the Harpsichord, Organ, Guitar, &c. By Mr. T. Cavallo, F. R. S. p. 238.

The scale of music used at present consists of 7 principal notes or sounds, which musicians denote by the letters of the alphabet A, B, C, D, E, F, G; which, together with some intermediate ones, commonly called flats and sharps, and the octave of the first, make 13 sounds. When those sounds are considered with respect to the first, they are called by the following names, viz. the prime or key-note, the 2d minor, 2d, 3d minor, 3d major, 4th, 4th major, 5th, 6th minor, 6th major, 7th minor, 7th major, and octave.

Musical sounds are produced by the vibrations of the sonorous bodies, and they are acuter or graver as the vibrations performed in a given time are more or less in number; so that if a string vibrating 100 times in a second produces a certain sound, and another string vibrating 120 times in a second produces another sound, the latter is said to be acuter, higher, or sharper than the former. The number of vibrations performed in a certain time chiefly depends on the thickness, length, and elasticity of the sonorous bodies; but as the simplest sonorous bodies, and the fittest for examination, are those strings which are equal in every other respect, excepting in their lengths, because the number of vibrations, which they perform in a given time, is inversely in the proportion of their lengths, we shall consider only those in the present investigation, the number of vibrations performed by other sorts of sonorous bodies being easily deduced from them. As those 13 sounds are all different from each other, the strings which produce them differ in length, and of course in the number of

* I have known 4 young cuckoos in the nests of hedge-sparrows in a small paddock at the same time.—Orig.

the vibrations which they make in a certain time. Here follow the proportions which the times of vibration, or the length of the strings which express those 13 sounds, bear to the first, prime, or key note.

First............	1	Fourth............	$\frac{3}{4}$	Seventh minor......	$\frac{5}{9}$
Second minor	$\frac{15}{16}$	Fourth major	$\frac{32}{45}$	Seventh major	$\frac{8}{15}$
Second	$\frac{8}{9}$	Fifth	$\frac{2}{3}$	Octave	$\frac{1}{2}$
Third minor......	$\frac{5}{6}$	Sixth minor........	$\frac{5}{8}$		
Third major......	$\frac{4}{5}$	Sixth major........	$\frac{3}{5}$		

If, instead of many strings having those lengths in order to express the 13 sounds, or notes of an octave, one string be divided according to those proportions, and this string be stopped consecutively in the different points or divisions; on being struck, it will express the corresponding sounds. Thus, if a string stretched between 2 fixed points, be struck, it will produce a sound called the prime, first, or key-note; if it be stopped in the middle, one half of the string will sound the octave, its length, compared to that of the whole string, being in the proportion of 1 to 2; if $\frac{2}{3}$ of the string be caused to vibrate, the sound produced will be the 5th, its length, compared to that of the whole string, being as 2 to 3, and so of the rest. The highest sound of the octave is expressed by the half of the string; and if this half be divided again in the same manner or proportion, a higher octave will be obtained, the highest note of which will be expressed by a quarter of the original string. This quarter may be divided again into a higher octave, and so on; therefore, a string so divided may express the sounds of all the keys of a harpsichord or organ.

In regard to those divisions it may be observed, that as the notes of the 2d octave bear the same proportion to the first note of that octave as the notes of the first octave respectively bear to the first note of that octave, or to the whole string; and as the length of the string expressing the first note of the 2d octave, is half the length of the first note of the first octave, it follows, that the length of the string of every note in the 2d octave is half the length of the corresponding note in the first octave. Hence, when the divisions of the first octave are ascertained, in order to find the divisions of the notes of the 2d octave, we need only take the half of the lengths expressing the notes in the first octave. By the very same reasoning it is evident, that to find the divisions for the 3d octave, we need only take the halves of the lengths which express the notes of the 2d octave, or the quarters of those of the first octave, and so of the rest.

The first string or line is divided in the above-mentioned manner, and in order to avoid confusion, the divisions of the principal notes only of the first and 2d octave are annexed to it. Numbers are set under the line to express the lengths from the beginning to the divisions to which they stand near. The letters just over the lines are the names of the notes or sounds expressed by the corresponding lengths of the string. The fractional numbers express the proportion which each particular division bears to the whole string; and the Roman numbers denote the numerical names of each note with respect to its distance from the first, which is always included. It is evident, that if any of those divisions be considered as the first or key-note, then the other notes, though they retain their alphabetical names, must have their numerical names altered accordingly: for example, if we take D for the key-note, then A will be the 5th of it, whereas A was the 6th when C was considered as the key-note; thus also B is the 3d of G, and the 7th of C; and so on.

Thus much having been premised, we may proceed to show the meaning of what is called the temperament in a system of musical sounds, and the necessity of it. For this purpose it is necessary to recollect, first, that the string, divided in the above-mentioned manner, exhibits the various notes or sounds of the keys of a harpsichord, the pipes of an organ, &c. 2dly, That those divisions remain unalterable, so that the harpsichord, when tuned, cannot be altered in the course of performing on it. And, 3dly, that when any of those notes or divisions is considered as the key-note, its 2d, 3d, 4th, 5th, &c. must bear their respective proportions, according to what has been said above. Now if, among the divisions of the first string CZ, we take D for the first or key-note, its length being 320 inches, the length of its 5th must be $213\frac{1}{3}$ inches, viz. $\frac{2}{3}$ of 320, that being the proportion which the 5th must bear to the key-note; but among the divisions of the string, there is none equal to $213\frac{1}{3}$ inches; therefore, there is not a note among them which may serve for a 5th to D; however, as the length of AZ, viz. 216, is the nearest to $213\frac{1}{3}$, this A must be taken for the 5th of D. It is evident, that this is an imperfect 5th of D; but if, in order to render it perfect, we make AZ equal to $213\frac{1}{3}$ inches instead of 216, then it will be a redundant 6th to C, when C is considered as the key-note; the best expedient therefore, is to divide the imperfection between the 2 lengths, viz. to make AZ neither so long as 216, nor so short as $213\frac{1}{3}$, which will render the disagreeable sensation, arising from the improper length, the least possible. This alteration of the just lengths of strings, necessary for adapting them to several key-notes, is called the temperament: and the best temperament in a set of musical sounds is evidently such a partition of the natural imperfections, as will render all the chords equally and the least disagreeable possible.

What has been exemplified in D and A may be said of all the other notes; so that if any one of them be a perfect 3d, 5th, &c. with respect to one key-note, it will be found to be imperfect with respect to others. Hence it is manifest, 1st, that in a set of musical keys, pipes, or frets, a temperament is absolutely necessary; and, 2dly, that the harpsichord, organ, guitar, or any other instrument in which the notes are fixed, so as not to be alterable by the performer's hands, must be imperfect even when tuned in the best manner possible; for by the temperament we can divide, but not annihilate the imperfection. Other instruments, in which the notes are not fixed, as the violin, violoncello, &c. are perfect, because the performer stops the strings on them in different places, even for sounding the notes of the same name. Thus a skilful performer, in order to sound A, will stop the string a little farther from the bridge when he plays in the key of C, viz. when C is considered as the key-note, than when he plays in the key of D.

Most people imagine, that the scale of musick is capable of many different temperaments; and, agreeable to this supposition, the writers on harmonics have proposed different temperaments; but the nature of the scale admits of only one temperament capable of rendering the imperfection and the harmony equal throughout; and it is impossible to form a different and more advantageous scale. It may also be remarked, 1st, that the proportion of 2 to 3 for the fifth, the proportion of 1 to 2 for the octave, and in short the proportions of all the notes, are not assumed at pleasure; but have been determined from constant experience, viz. from the agreeable or disagreeable effects produced when 2 different notes are sounded at the same time.

Thus, let 2 strings equal in every respect be struck at the same time, and they will express the same sound precisely, so that no ear can perceive any difference between them, and it is almost impossible to distinguish whether the sound arises from 2 strings, or from 1 only, excepting from the loudness. But if one of those strings be successively stopped in different parts of its length, while the other remains open as before, and if at every time they be both struck together, their combined sounds will be found to produce different effects, viz. sometimes more or less pleasing, and at other times more or less disagreeable. When the combinations of the 2 sounds are agreeable, they are called concords; and when disagreeable, they are called discords. Experience evinces, that the best concord is when the length of one string is to the length of the other as 1 to 2, every other circumstance being the same in both. This proportion forms the octave. The next best concord is the 5th, viz. when the lengths of the two strings are as 2 to 3, after which come the proportions of 3 to 4, 4 to 5, 3 to 5, 5 to 6, and 5 to 8, for the other concords. The other proportions besides these are disagreeable in a greater or less degree, unless they are greater than the proportion of 1 to 2; but in that case it will be found, that the proportions which produce agreeable combinations are the double, quadruple, octuple, &c. of those mentioned above, viz. are their octaves, double octaves, &c.: thus the proportion of 1 to 4 produces a very agreeable concord, because 1 to 4 is the double of 1 to 2, viz. it expresses a double octave. 2dly, Hence if we have the length of a string, or the proportion of a note in any part of the string, we may easily find its octaves by taking its double, or its half, or the double of the double, &c.: we may find its octave below by taking twice 90, viz. 180, or the octave of this octave, which is 360, viz. equal to twice 180, or to four times 90; and, on the other side, we may find the octave above of the given note by taking its half, which is 45, &c.

Mr. C. now shows why within the octave there are admitted only 13 different notes, viz. 8 principal ones, and 5 others, called sharps and flats. He assumes a line to represent a musical string, the length of which is supposed to be divided into a certain number of equal parts, suppose 13286025. On one side of this line are set the divisions of 7 successive octaves, viz. the half of it, a quarter of it, &c.; and on the other side are the divisions of a series of 5ths, viz. the 5th of the whole string, the 5th of this 5th and so on, which are found by taking $\frac{2}{3}$ of the whole string, then $\frac{2}{3}$ of those $\frac{2}{3}$, and so on. Here notice is taken only of the octaves and 5ths, because they are the principal and the best concords; so that a temperament being required, it is necessary first to take care that these concords be not rendered insufferable to the ear, the rest admitting of a greater latitude in the temperament or deviation from the perfect state. Besides, all the other notes are derived from the series of successive 5ths. In whatever key a piece of music is performed, its 5th is the most predominant of its concords; and as the notes of music must be so ordered as that, for the sake of modulation, any note may be considered as the key-note; therefore having found the 5th of the whole string by taking $\frac{2}{3}$ of its length, which gives a note called G, we must suppose, that this G may be considered as the key-note, consequently must find its 5th, which gives D, and so on, until we find one of those successive 5ths, which coincides with one of the successive octaves; for after that, to find more successive 5ths would be only repeating the same thing over again.

Indeed, if we carry the succession of octaves and of 5ths indefinitely far, we shall find, that no one of the 5ths ever coincides perfectly with one of the octaves, and therefore the division would have no end. However, as the length of the 7th octave comes so very near to the 12th fifth, we must be contented with taking this 7th octave for the 5th of F, the difference between them being about the 100th part of its length; whereas, if we carry on the succession of 5ths and of octaves, we shall find, that among 30 and more 5ths none comes nearer to one of the octaves than the above-mentioned one. Hence the number of 5ths in this series is 12; and as, when the division expressing a certain note has been assigned in any part of a string, we may easily find all its octaves above and below, it follows, that by finding all the octaves of those 12 divisions, we shall have 12 distinct notes within half the string, viz. within the first octave of the whole string; to which, if the sound of the whole string be added, we have 13 different sounds; which shows why an octave comprehends neither more nor less than 13 notes. Without dwelling any longer on the names or number of those notes, Mr. C. proceeds to find out the temperament.

It appears by the above divisions, that the length of the string for the last 5th is shorter than the length of the last octave, and also that one of them must necessarily be taken for both purposes; but here we must consult nature, examining by the ear which of the 2 is least disagreeable. This however is soon decided; for imperfect octaves are quite insufferable, whereas a certain degree of imperfection in the 5ths is tolerable; therefore we are necessitated to leave the octaves perfect, and to let the 7th octave serve for the 5th of F. In this case it is evident that each of the notes in the succession of 5ths is a perfect 5th to its preceding note, excepting the last, which would be by much too flat, and therefore it is necessary to divide the imperfection equally among them all. For this purpose it must be considered, that as the 12 successive 5ths, together with the whole string or first note, are each $\frac{2}{3}$ of its preceding note; they form a geometrical series, the ratio of which is $\frac{2}{3}$, its extremes are 13286025, the first length, and 102400, the 12th fifth, and the number of terms is 13. But because instead of 102400, which is the last 5th, we must take the number 103797, viz. the length nearly of the 7th octave, for the last term of the series; therefore the problem is reduced to the finding out of 11 mean proportionals between the two numbers 13286025 and 103797. Now by the nature of a geometrical progression, $\frac{13286025}{103797} = r^{12} = 128$, consequently $r = \sqrt[12]{128} = 1.4983$ the ratio of the series.

The ratio having been ascertained, the succession of tempered 5ths is thus easily determined; viz. divide the length of the whole string by this ratio, and the quotient gives the 1st tempered 5th; divide this 5th by the same ratio, and the quotient gives the 2d tempered 5th; divide this 2d 5th by the same ratio, and so on till the last 5th, which comes out equal to $103797\frac{5}{23}$, which is so nearly equal to the length of the 7th octave, that the difference is truly insignificant. The divisions, thus ascertained, form a series of notes, in which the octaves only are perfect; but all the 5ths, all the 3ds, and in short all the chords of the same denomination, are equally tempered throughout; so that whichever of them is taken for the key-note, its 5th, 6th, &c. will have always the same proportion to it, and consequently will always produce the same harmony when sounded with it. It is evident that, besides this, there can be no other temperament capable of producing equal harmony; for when the extremes of a geometrical series and number of mean proportionals are given, there can be only one set of those means. If, on the other hand, we endeavour to find a better temperament by introducing more than 13 notes within the limits of an octave, we shall find it impracticable, because that after the number 13, if the succession of 5ths be carried on further, they will recede more from a coincidence with any one of the octaves.

This explanation of the nature, origin, and necessity of the temperament has been thought necessary for the sake of perspicuity; but the same end may be obtained by the following easier method. As the 13 notes of an octave must be arranged so, that whichever of them be taken for the 1st or key-note, the 2d, 3d, 4th, &c. may bear the same constant proportion to it; they must therefore be in a geometrical proportion, so as to form a series of 13 numbers, the extremes of which are the whole string and its half, viz. any number and its half. The ratio of this series is found in the same manner as in the other series, viz. the greatest extreme is divided by the least, and the 12th root of the quotient is the ratio sought. But the extremes are any assumed number and its half: and as the quotient of a number divided by the half of the same number is always equal to 2; therefore, whatever be the length of the string, the ratio is always $\sqrt[12]{2} = 1.0594 +$; and if the length of the whole string be divided by this ratio, viz. $1.0594 +$, the quotient will be the length of the string expressing the 2d note, which, divided by the same ratio, gives the 3d note, and so on; or else, instead of dividing the length of the whole string by the ratio, we may multiply the half of it by the ratio, the product of which will give the 7th note, which multiplied by the same ratio gives the 6th, and so on in a retrograde order, which will give the tempered notes of the octaves as well as the former method. By this means the annexed divisions for the notes of an octave have been calculated, the length of the whole string having been supposed equal to 100000.

I.	100000
* b	94387
II.	89090
* b	84090
III.	79370
IV.	74915
* b	70710
V.	66743
* b	62997
VI.	59462
* b	56123
VII.	52973
VIII.	50000

If a monochord be divided in this manner, and a harpsichord tuned by it, this instrument will then be tuned so, that whichever note be taken for the first or key-note, its 5th, 6th, &c. will produce the same effect respectively.

At present, the harpsichords and organs are commonly tuned so, that some concords are very agreeable to the ear, while others are quite intolerable; or, in other words, when the performer plays in certain keys, the harmony is very pleasing, in others the harmony is just tolerable, and in some other keys the harmony is quite disagreeable. The best keys to be played in, are the keys of C, of F, of E flat, of B flat, of G and of D in the major mood; and the keys of C, of D, of A, and of B, in the minor mood. Next to those come the less agreeable keys of A, A flat, and E in the major mood; besides those, the rest are disagreeable in a greater or less degree, so that out of 12 keys, which, on account of the 2 moods, viz. the major and the minor, become 24, there are hardly 14 that can be used; and for this reason most of the modern compositions in musick are written in those keys.

So far the common method of tuning answers some purpose; for as long as the performer is to play in certain keys only, it is much better to have them tuned in the most advantageous manner, than to let those be tuned in a less perfect way for the sake of others, which he does not intend to use. Hence the great harpsichord players generally have their instruments tuned in a peculiar manner, viz. so as to give the most advantageous effect to those concords which they more frequently use in their compositions. And hence also the harpsichords and organs are always tuned different from each other, unless they be tuned by the same person with equal attention, and without any particular instructions. This practice cannot conveniently be laid aside, viz. when the instrument is to be tuned for solo playing; and for a certain style of music, it is very proper to tune it so as to give the greatest effect to those combinations of sounds which are mostly used in those compositions. But the case is far different when the instrument is to serve for accompanying other instruments in every sort of music, or the voices of good singers; for then the disagreement becomes very audible; and for this purpose the harpsichord or organ ought to be tuned according to the above demonstrated temperament of equal harmony, which is the only one that can possibly take place.

When the compositions of old masters are performed in concert, and with the organ or harpsichord tuned in the common manner, the effect is frequently very disagreeable. This is particularly the case with the songs of Handel, Galluppi, Leo, Pergolese, and others, who wrote in a great variety of keys, and very often in those for which the common way of tuning is not at all calculated.

XVI. Description of a New Electrical Instrument capable of Collecting together a Diffused or Little Condensed Quantity of Electricity. By Mr. T. Cavallo, F.R.S. p. 255.

One of the principal desiderata in practical electricity has been a method of ascertaining the presence and quality of suchdiffused or weak electricity as could not immediately affect an electrometer; of this nature is the electricity produced by effervescences and other processes, also the electricity of the atmosphere in serene and warm weather, &c. M. Volta's condenser, described in volume 72 of the Philos. Trans. was the first attempt of the kind, and indeed when this instrument is in good order it answers exceedingly well; but the difficulty of constructing and of preserving it, added to the frequent uncertainty of the result, have occasioned its being little, if at all, used by those who study the subject of electricity. Mr. Bennet's doubler, described in vol. 77 of the Philos. Trans. was also intended to manifest small, and otherwise unperceivable, quantities of electricity; but from the experiments and observations Mr. C. since laid before the R. S. he thinks it clearly shown, that this doubler cannot be of any use, on account of its being naturally always electrified. In the same paper he mentioned a method which he had used for collecting diffused quantities of electricity. Since that time he has improved the method; and, after several alterations, constructed an instrument for the purpose, which is deemed free from all those faults which render M. Volta's and Mr. Bennet's instruments of little, if at all of any, use.

The properties of this machine, which from its office may be called a collector of electricity, are first, that when connected with the atmosphere, the rain, or in short with any body which produces electricity slowly, or which contains that power in a very rarefied manner, it collects the electricity, and afterwards renders both the presence and quality of it manifest, by communicating it to an electrometer. 2dly, This collecting power, by increasing the size of the instrument, and especially by using a 2d or smaller instrument of the like sort to collect the electricity from the former, may be augmented to any degree. 3dly, It is constructed, managed, and preserved with ease and certainty; and it never gives, nor can it give, he thinks, an equivocal result.

Fig. 1 and 2, pl. 6, exhibit 2 perspective views of this collector. Fig. 1 shows the instrument in the state of collecting the electricity; and fig. 2 shows it in the state in which the collected electricity is to be rendered manifest. An electrometer is annexed to each. The letters of reference indicate the same parts in both figures. ABCD is a flat tin plate, 13 inches long and 8 inches broad; to the 2 shorter sides of which are soldered 2 tin tubes AD and BC, which are open at both ends. DE and CF are 2 glass rods covered with sealing wax by means of heat, and not by dissolving the sealing wax in spirits. They are cemented into the lower apertures of the tin tubes, and also in the wooden bottom of the frame or machine at E and F, so that the tin plate ABCD is supported by those glass rods in a vertical position, and is exceedingly well insulated. GHILKM and NOPV are 2 frames of wood which, being fastened to the bottom boards by means of brass hinges, may be placed so as to stand in an upright position and parallel to the tin plate, as shown in fig. 1; or they may be opened, and laid on the table which supports the instrument, as in fig. 2. The inner surfaces of those frames from their middle upwards is covered with gilt paper XY; but it would be better to cover them with tin plates, hammered very flat. When the lateral frame stands straight up, they do not touch the tin plate; but they stand at about $\frac{1}{5}$ part of an inch asunder. They are also a little shorter than the tin plate, that they might not touch the tin tubes AD, BC. In the middle of the upper part of each lateral frame is a small flat piece of wood S and T, with a brass hook; the use of which is to hold up the frames without the danger of their falling down when not required, and at the same time it prevents their coming nearer to the tin plate than the proper limit. It is evident that when the instrument stands as shown in fig. 1, the gilt surface of the paper XY, which covers the inside of the lateral frames, stands contiguous and parallel to the tin plate.

When the instrument is to be used, it must be placed on a table, a window, or other convenient support, a bottle electrometer is placed near it, and is connected, by means of a wire, with one of the tin tubes AD, BC; and by another conducting communication the tin plate must be connected with the electrified substance, the electricity of which is required to be collected on the plate ABCD: thus, for instance, if it be required to collect the electricity of the rain, or of the air, the instrument being placed near a window, a long wire must be put with one extremity into the aperture A or B of one of the tin tubes, and with the other extremity projecting out of the window. If it be required to collect the electricity produced by evaporation, a small tin pan, having a wire or foot of about 6 inches in length, must be put on one of the tin tubes, so that the wire going into the tube the pan may stand about 2 or 3 inches above the instrument. A lighted coal is then put into the pan, and a few drops of water poured on it will produce the desired effect.

Mr. C. adds, that having actually used this new instrument in several experiments, he had found it to answer perfectly well; one of its principal recommendations being the certainty of its operation.

XVII. On the Conversion of a Mixture of Dephlogisticated and Phlogisticated Air into Nitrous Acid, by the Electric Spark. By Henry Cavendish, Esq., F. R. S., and A. S. p. 261.

In volume 75 of the Phil. Trans., p. 372, I related an experiment, which showed, that by passing repeated electric sparks through a mixture of atmospheric and dephlogisticated air, confined in a bent glass tube by columns of soap-lees and quicksilver, the air was converted into nitrous acid, which united to the soap-lees and formed nitre. But as this experiment has since been tried by some persons of distinguished ability in such pursuits without success, I thought it right to take some measures to authenticate the truth of it. For this purpose, I requested Mr. Gilpin, clerk of the R. S., to repeat the experiment, and desired some of the gentlemen most conversant with these subjects to be present at putting the materials together, and at the examination of the produce. This laborious experiment Mr. Gilpin performed in the same manner, and with the same apparatus, which was used in my own experiments. The method used for introducing air into the bent tube, was that described in the last paragraph of p. 373 in that paper. The soap-lees, like those of my own experiments, were prepared from salt of tartar, and were of such strength as to yield $\frac{1}{10}$ of their weight of nitre when saturated with nitrous acid. The dephlogisticated air was prepared from turbith mineral, and seemed by the nitrous test to contain about $\frac{1}{18}$ part of phlogisticated air.

On Dec. 6, 1787, in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. J. Hunter, and Mr. Macie, the materials were put together. The quantity of soap-lees, introduced into the bent tube, was 180 measures, each of which contained 1 gr. of quicksilver; and, as the bore of the tube was rather more than $\frac{1}{3}$ of an inch in diameter, it formed a column of 5 or 6 tenths of an inch in length, which, by the introduction of the air, was divided into 2 parts, one resting on the quicksilver in one leg of the tube, and the other on that in the other leg. The dephlogisticated air was mixed with $\frac{1}{3}$ part of its bulk of atmospheric air of the room in a separate jar, and the reservoir was filled with the mixture; and from it Mr. Gilpin, as occasion required, forced air into the bent tube, to supply the place of that absorbed by means of the electric spark. Hence it appears, that the mixture employed contained a less proportion of common air than that used in either of my experiments. This made it necessary for Mr. Gilpin now and then to introduce some common air by means of the bent tube, whenever from the slowness of the absorption he thought there was too small a proportion of phlogisticated air in the tube. My reason for this manner of proceeding was, that as my first experiment seemed to show, that the dephlogisticated air ought to be in a rather greater proportion to the phlogisticated than the latter did, I was somewhat uncertain as to the proper quantities, and doubted whether I could proportion them in such manner as that it should not be necessary, during the course of the experiment, to add either dephlogisticated or common air. I therefore mixed the airs in such proportion, that I was sure there could be no occasion to add the former; since it was much easier, as well as more unexceptionable, to add common air than dephlogisticated.

On Dec. 24, as the air in the reservoir was almost all used, this apparatus was again filled in the presence of most of the above-mentioned gentlemen, with a mixture of the same dephlogisticated air and common air, in the same proportions as before; and the same thing was repeated on Jan. 19. On Jan. 23, the bent tube was, by accident, raised out of one of the glasses of mercury into which it was inverted, by which it was filled with air, and a good deal of the soap-lees were lost; there was enough however remaining for examination.

On Jan. 28, and 29, the produce of this experiment was examined in the presence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. It appeared that 9290 measures of the mixed air had been forced into the bent tube from the reservoir.*

* The method of ascertaining the quantity of air forced in was by weighing the reservoir, as mentioned in the above-mentioned paper, p. 374.—Orig.

Besides this, Mr. Gilpin had at different times introduced 872 measures of common air, which makes in all 10162 of air, consisting of 6968 of dephlogisticated air, and 3194 of common air. But as there were 900 measures of air remaining in the tube when the accident happened, the quantity absorbed was only 9262; but this is a much greater quantity than what from my own experiments seemed necessary for this quantity of soap-lees. The soap-lees were poured into a small glass cup, and the tube washed with a little distilled water, in order that as little as possible might be lost. As they were by this means considerably diluted, they were evaporated to dryness; but it was difficult to estimate the quantity of the saline residuum, as it was mixed with a few particles of mercury.

Some vitriolic acid, dropped on a little of this residuum, yielded a smell of nitrous acid, the same as when dropped on nitre phlogisticated by exposure to the fire in a covered crucible; but it was thought less strong. The remainder was dissolved in a small quantity of distilled water, and the following experiments were tried with the solution. It did not at all discolour paper tinged with the juice of blue flowers. It left a nauseous taste in the mouth like solutions of mercury, and most other metallic substances. Paper dipped into it, and dried, burnt with some appearance of deflagration, but not so strongly or uniformly as when dipped in a solution of nitre. The marks of deflagration however were stronger than when the paper was dipped into a solution of mercury in spirit of nitre, but not so strong as when equal parts of this solution and solution of nitre were used. A solution of fixed vegetable alkali, dropped into some of it diluted, produced a slight reddish-brown precipitate, which afterwards assumed a greenish colour. A bit of bright copper being dipped into it, acquired an evident whitish colour, though not so white as when dipped into the solution of mercury in spirit of nitre.

From these experiments it appears, that the mixture of the two airs was actually converted into nitrous acid, only the experiment was continued too long, so that the quantity of air absorbed was greater than in my experiments, and the acid produced was sufficient, not only to saturate the soap-lees, but also to dissolve some of the mercury. The truth of the latter part is proved by the metallic taste of the residuum, its not discolouring the blue paper, the precipitate formed by the addition of fixed alkali, and the white colour given to the copper; and the nitrous fumes produced by the addition of oil of vitriol, as well as the manner in which paper impregnated with the residuum burnt, show as plainly, that the acid produced was of the nitrous kind. It is remarkable however, that during this experiment there were no signs which showed when the soap-lees became saturated. The only time when the diminution proceeded much slower than usual was on Jan. 4. It then seemed to go on very slowly; but as the air absorbed at that time was only 4830 measures, which is much less than what seems requisite to saturate the alkali, and as the diminution immediately went on again on adding more common air, it seems not likely that the soap-lees were saturated at that time.

On Jan. 10, Mr. Gilpin observed a small quantity of whitish sediment on the surface of the mercury; which seems to show that the soap-lees were then saturated, and that the acid was beginning to corrode the mercury. The quantity of air absorbed was also 6840 measures, which is about as much as I expected would be required. However, as I was persuaded, from the event of my own experiments, that the diminution would either entirely cease, or go on very slowly, as soon as the soap-lees were saturated; and as I was unwilling to stop the experiments before that happened, I thought it best to continue the electrification. On the same morning Mr. Gilpin found, that about 120 measures of the air in the bent tube had been spontaneously absorbed during the night, the quantity therein being so much less than it was the preceding evening, though the electrical machine had not been worked, or any thing done to it during the intermediate time. The reason of this in all probability is, that as the acid was then corroding the mercury, the soap-lees became impregnated with nitrous air, which during the night united to the dephlogisticated air, and caused the diminution.

Though in reality the event of this experiment was such as to establish the truth of my position, that the mixture of dephlogisticated and phlogisticated air is converted by the electric spark into nitrous acid, as fully as if the experiment had been stopped in proper time; yet, as the event was in some measure different from that of my own experiments, and might afford room for cavil, I was desirous of having it repeated; and as Mr. Gilpin was so obliging as to undertake it again, the materials were, on Feb. 11, put together for a fresh experiment, in the presence of most of the above-mentioned gentlemen. The soap-lees employed were the same as before, but 183 measures were now introduced. The dephlogisticated air was different, the former parcel being all used. It was prepared, like the former, from turbith mineral, but was rather purer, as it seemed to contain only $\frac{1}{32}$ of phlogisticated air. The proportion in which it was mixed with common air was that of 22 to 10; so that a greater proportion of common air was now used, in consequence of which it was not necessary for Mr. Gilpin to introduce common air so often.

On Feb. 29, the reservoir was again filled with air of the same kind, in presence of some of the same gentlemen. As it was found by the last experiment that we must not depend on the saturation of the soap-lees being made known by any alteration in the rate of diminution, the process was stopped as soon as the air absorbed was such as from my own experiments I judged sufficient to neutralize the soap-lees. This was effected on the 15th of March. The air remaining in the tube, when Mr. Gilpin left off working, was 600 measures; but at the time the produce was examined, it was reduced to about 120, so much having been absorbed without the help of any electrification, which is a still more remarkable instance of spontaneous absorption than what occurred in the former experiment. A few days after the experiment began, a black film was formed in one of the legs, which I suppose must have been a mercurial ethiops; but whether owing to some small degree of foulness in the mercury or tube, or to any other cause, I cannot tell. This foulness seemed not to increase; but on March 10, when the air absorbed was about 5200, a whitish sediment began to appear on the surface of the mercury.

On March 19, the produce was examined in the presence of Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. The mixed air forced into the bent tube from the reservoir was 6650 measures, besides which Mr. Gilpin had at different times introduced 630 of common air, which makes in all 7280, containing 4570 of dephlogisticated, and 2710 of common air. The soap-lees were evaporated to dryness as before. The residuum weighed 2 gr., but there were 2 or 3 globules of mercury mixed with it, which might very likely weigh $\frac{1}{2}$ gr. This being dissolved in a small quantity of water, the following experiments were made with it. It did not at all discolour paper tinged with blue flowers. Slips of paper were dipped into it, and dried; and, by way of comparison, other slips of paper were dipped into a solution both of common nitre and phlogisticated nitre, and also dried. The former burnt in the same manner, and with as strong marks of deflagration, as the latter. It had a strong taste of nitre, but left also a slight metallic taste on the tongue. It did not give any white colour to a piece of clean copper put into it.

In order to see whether the whitish sediment, which was before said to be formed in the bent tube, contained any mercury, the remainder of this solution was diluted with some more distilled water, and suffered to stand till the white sediment had subsided. The clear liquor being then poured off, the remainder, containing the sediment, which seemed to amount only to a very small quantity, was put on a piece of bright copper, and dried on it; a piece of clean gold was then laid over it, and both were exposed to heat. Both metals acquired a whitish colour, especially the gold, but which was very indeterminate. In order to discover how nice a test of alcalinity the paper tinged with blue flowers was, a saturated solution of common nitre was mixed with $\frac{1}{120}$ of its bulk of the soap-lees; and this mixture was found to turn the paper evidently green; so that, as the solution of nitre contains about twice as much alkali as the soap-lees, it appears, that if the residuum had wanted only $\frac{1}{240}$ part of being saturated, it would have discoloured the paper.

From the foregoing trials it appears, that the mixture of dephlogisticated and common air in this experiment was actually converted into nitrous acid, and was sufficient not only to saturate the soap-lees, but also to dissolve some of the mercury. The quantity dissolved however was very small, and not sufficient to diminish sensibly the deflagrating quality of the nitre; so that the proof of the air being converted into nitrous acid was as evident as if no mercury had been dissolved. In this experiment, as well as the former, no indication of the soap-lees becoming saturated was afforded by any cessation in the diminution of the air; whereas in my experiments it was very manifest. I do not know what this difference should be owing to, except to Mr. Gilpin's giving much stronger electrical sparks than I did. In his experiments the metallic knob which received the spark, and conveyed it to the bent tube, was usually placed at about $2\frac{1}{2}$ inches from the conductor, so that the spark jumped through $2\frac{1}{2}$ inches of air, in passing from the conductor to the knob, besides from $1\frac{1}{2}$ to $2\frac{1}{2}$ inches of air in the tube; whereas in my experiments, I believe, the knob was never placed at the distance of more than $1\frac{1}{4}$ inch from the conductor, and the quantity of air in the tube was much less; but the conductor and electrical machine were the same.

Except this, the only difference that I know of in the manner of conducting the experiment is, first, that Mr. Gilpin usually continued working the machine for $\frac{1}{2}$ an hour at a time, whereas I seldom worked it more than 10 minutes; and 2dly, that in Mr. Gilpin's experiments the common air in the reservoir bore a less proportion to the dephlogisticated air than in mine; in consequence of which it was necessary for him frequently to introduce common air. On this account, the proportion of the 2 airs in the bent tube would be considerably different at different times; but on the whole, the common air absorbed bore a greater proportion to the dephlogisticated than in mine.

Though the whole quantity of air absorbed in these experiments is known with considerable precision, yet it is impossible to determine, with any accuracy, how much of each kind was absorbed, on account of our uncertainty about the nature of the air which remained at the end of the experiment. But if in the last experiment we suppose that the air absorbed spontaneously between the 15th and 19th of March was entirely dephlogisticated, and that what remained at the end of that time was of the purity of common air, it will appear, that 4090 of dephlogisticated and 2588 of common air, which is equivalent to 4480 of pure dephlogisticated air and 2198 of phlogisticated air, were absorbed at the time the electrification was stopped, and consequently the dephlogisticated air is $\frac{204}{100}$ of the phlogisticated air; whereas in my first experiment it seemed to be $\frac{220}{100}$, and in my last $\frac{253}{100}$. But the quantity of acid produced, and consequently I suppose the saturation of the soap-lees, depends only on the quantity of phlogisticated air absorbed; and the effect of the greater or less quantity of dephlogisticated air is only to make the nitre produced more or less phlogisticated. Now in this experiment the bulk of the phlogisticated air was $12\frac{2}{10}$ that of the soap-lees. In my first experiment it was $11\frac{9}{10}$, and in my last $10\frac{8}{10}$.

As many persons seem to have supposed that the diminution of the air in these experiments is much quicker than it really is, though I do not know any thing in my paper which should lead to suppose that it was not very slow, it may be proper to say something on this head. As the quickness of the diminution depends so much on the power of the electrical machine, I can only speak as to what happens with the machine used in these experiments. This was one of Mr. Nairne's patent machines, the cylinder of which is $12\frac{1}{2}$ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the spark was placed at 2 or 3 inches from another ball, fixed to the end of the conductor. Now, when the machine worked well, Mr. Gilpin supposes he got about 2 or 300 sparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 measures, but was usually greatest when there was most air in the tube, provided the quantity was not so great as to prevent the spark from passing readily.

The only persons I know of, who have endeavoured to repeat this experiment, are, M. Van Marum, assisted by M. Paets Van Trootswyk; M. Lavoisier, in conjunction with M. Hassenfratz; and M. Monge. I am not acquainted with the method which the 3 latter gentlemen employed, and am at a loss to conceive what could prevent such able philosophers from succeeding, except want of patience. But M. Van Marum, in his Premiére Continuation des Expériences, faites par le moyen de la Machine électrique Teylerienne, p. 182, has described the method employed by him and M. Van Trootswyk. They used a glass tube, the upper end of which was stopped by cork, through which an iron wire was passed, and secured by cement, and the lower end was immersed into mercury; so that the electric spark passed from the iron wire to the soap-lees. After so much of a mixture of 5 parts of dephlogisticated and 3 of common air as was equal to 21 times the bulk of the soap-lees * was absorbed, some paper was moistened with the alkali, which by its burning appeared to contain nitre, but showed that the alkali was not near saturated. The experiment was then continued with the same soap-lees till more of the air, equal to 56 times the bulk of the soap-lees, was absorbed, which is near double the quantity required to saturate them; but yet the diminution went on as fast as ever. It was then tried, by the burning of paper dipped into them, how nearly they were saturated; but they still seemed far from being so.

The circumstance of using the iron wire appears evidently objectionable, on account of the danger of the iron wire being calcined by the electric spark, and

* This is rather more than half of that requisite to saturate the soap-lees.—Orig.

absorbing the dephlogisticated air; and when I first read the account, I thought this the most probable cause of the difference in the result of our experiments; but I am now inclined to think that the case was otherwise. From the manner in which M. Van Marum expresses himself, it seems that the only circumstance, from which they concluded that the alkali was not saturated, was the imperfect marks of deflagration that the paper dipped into it exhibited in burning; which, as we have seen, might proceed as well from some of the mercury having been dissolved as from the alkali not being saturated. I am much inclined to think therefore, that, so far from the soap-lees not having been saturated, the quantity of acid produced was in reality much more than sufficient for this purpose, and had dissolved a good deal of the mercury; for the quantity of air absorbed favours this opinion, and the phenomena agree well with Mr. Gilpin's first experiment, in which this was certainly the case; whereas, if the diminution had proceeded chiefly from the dephlogisticated air being absorbed by the iron, the tube towards the end of the experiment would have been filled chiefly with phlogisticated air, which would have made the diminution proceed much slower than before; but we are told that it went on as fast as ever. It is most likely therefore, that the apparent disagreement between their experiment and mine, proceeded only from their having continued the process too long, and from their not having properly examined the produce.

M. Van Marum then proceeds to say: "Surpris de cette différence de résultat, j'envoyai une description exacte de nos expériences à M. Cavendish, le priant en même tems de m'instruire s'il pourroit trouver la cause de cette différence; et comme la seule différence essentielle, par laquelle notre expérience différoit de celle de M. Cavendish, consistoit en ce que nous avons employé de l'air pur produit du précipité rouge ou du minium, au lieu de l'air pur produit de la poudre noire formée par l'agitation du mercure avec le plomb, dont M. Cavendish ne donne pas la manière de le produire,* je le priai de me communiquer de quelle manière il étoit venu a cet air, parceque je desirois de répéter l'expérience avec ce même air: mais comme il ne m'a fourni aucune élucidation sur la cause vraisemblable de la différence du resultat de nos expériences, et qu'il ne lui a pas plu de me communiquer sa manière de produire l'air pur qu'il avoit employé pour ses expériences, m'écrivant, qu'il s'étoit proposé d'en parler dans un écrit public, la longueur ennuyante de ces expériences nous a fait prendre la resolution de différer leur continuation, pour obtenir une parfaite saturation de la lessive, jusqu'à

* The using the iron wire formed a material difference in our manner of conducting the experiment, and one which may perhaps have had great influence on the result; but I do not see how the using some other kind of dephlogisticated air, instead of that prepared from Dr. Priestley's black powder, can in the least degree form an essential difference, as in the same paragraph in which I mention my having used this kind of air in my first experiment, I say, that in my second experiment I used air prepared from turbith mineral.—Orig.

ce que M. Cavendish ait publié sa manière de produire l'air pur, dont il s'est servi; nous contentant pour le present d'avoir vu, que l'union du principe d'air pur et de la mofette produit le l'acide nitreux, suivant la découverte de M. Cavendish."

As I should be sorry to be thought to have refused any necessary information to a gentleman who was desirous to repeat one of my experiments, and who by his situation was able to do it with less trouble than any one else, I hope the society will indulge me in adding a copy of my answer, that they may judge whether this is in any degree a fair representation of it.

"*To M. Van Marum.*

"Sir,—I received the honour of your letter, in which you inform me of your ill success in trying my experiment on the conversion of air into nitrous acid by the electric spark. It is very difficult to guess why an experiment does not succeed, unless one is present and sees it tried; but if you intend to repeat the experiment, your best way will be to try it with the same kind of apparatus that I described in that paper. If you do so, and observe the precautions there mentioned, I flatter myself you will find it succeed. The apparatus you used seems objectionable, on account of the danger of the iron being corroded by absorbing the dephlogisticated air. As to the dephlogisticated air procured from the black powder formed by agitating mercury mixed with lead, as it was foreign to the subject of the paper, and as I proposed to speak of it in another place, I did not describe my method of procuring it. As far as I can perceive, the success depends entirely on carefully avoiding every thing by which the powder can absorb fixed air, or become mixed with particles of an animal or vegetable nature, or any other inflammable matter: for which reason care should be taken not to change the air in the bottle in which the mercury is shaken, by breathing into it as Dr. Priestley did, or even by blowing into it with a bellows, as thereby some of the dust from the bellows may be blown into it. The method which I used to change the air was, to suck it out by means of an air-pump, through a tube which entered into the bottle, and did not fill up the mouth so close but what air could enter in from without, to supply the place of that drawn out through the tube.—I am, &c."

With regard to the main experiment, it was not in my power to give him further information than I did; as I pointed out the only circumstance to which, at that time, I could attribute the difference in our results. And with regard to the manner of preparing the dephlogisticated air from the black powder, I have mentioned all the particulars in which my manner of proceeding differed from Dr. Priestley's, and have also explained on what I imagine the success entirely depends; so that I believe no one, at all conversant in this kind of experiments, will think that I did not communicate to him my method of procuring that air.

XVIII. Experiments on the Effect of various Substances in Lowering the Point *of Congelation in Water. By Chas. Blagden, M. D., Sec. R. S. and F. A. S.* p. 277.

The experiments necessary to determine what effect the admixture of various substances would produce on the property of water to be cooled below the freezing point, naturally led to a more particular consideration of the power of such admixtures in making water require a greater degree of cold before it congeals. Many curious questions occurred on this subject, which could only be answered by fresh experiments. These were made nearly in the same manner as the preceding; that is, the liquor, whose freezing point was meant to be tried, was put into a glass tumbler, to the height of 2 or 3 inches above the bottom, and the tumbler was then immersed in a frigorific mixture of common salt and ice or snow.

The first object of investigation was the ratio according to which equal additions of the same substance depress the freezing point. Dr. B. began with common salt, of the purest kind. This salt he dissolved in distilled water, in various proportions, and found the corresponding points of congelation to be as expressed in the annexed table; where the first column indicates the number of parts and decimals of water to one part of the salt, and the 2d column shows the freezing point found by the experiment. It appeared clearly, on comparing the proportions of water to salt, with the corresponding number of degrees which the freezing point was reduced below 32°, that the effect of the salt was nearly in a simple ratio; namely, that if the addition of a 10th part of salt to the water sunk the freezing point about 11°, or to 21°, it would be depressed double that quantity, or to 10° nearly, when a 5th part of salt was dissolved in the water. To show therefore how far this simple proportion is exact, a third column is added to the table, which is made by selecting the lowest freezing point that was obtained without ambiguity in the experiment, and calculating, by a simple inverse proportion, what all the other points should have been according to that ratio. Thus, when a 4th part of its weight of common salt was dissolved in water, the freezing point of the liquor was 4°; therefore, to determine what it should be when only $\frac{1}{32}$ part of salt was added to the water, the formula is 32 : 4 :: 28 (the number of degrees that the point 4° is below the freezing point of pure water) : $3\frac{1}{2}$; which subtracted from 32° gives $28°\frac{1}{2}$ for the freezing point of that solution. All the rest of the 3d column of the table is found in the same manner, and with very little trouble, because 4 × 28 = 112 is a constant number, which being divided by the numbers of the first column, the quotient is the number of degrees sought. In all the experiments, none but distilled water was employed.

Common salt.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
32 : 1	29°	$28\frac{1}{2}$°
32 : 1	28 +	$28\frac{1}{2}$
24 : 1	$27\frac{1}{2}$	$27\frac{1}{3}$
16 : 1	$25\frac{1}{4}$	25
10 : 1	$21\frac{1}{2}$	$20\frac{3}{4}$
7.8 : 1	$18\frac{1}{2}$	$17\frac{2}{3}$
6.2 : 1	$13\frac{1}{2}$	14
5 : 1	$9\frac{1}{2}$	$9\frac{1}{2}$
5.5 : 1	$7\frac{1}{5}$	7
4 : 1	4	4

The numbers in the 3d column of the table come so near to those in the 2d, that most likely the small differences between them ought to be ascribed to errors in the experiments; whence we should conclude, that the salt lowers the freezing point in the simple inverse ratio of the proportion which the water bears to it in the solution. This solution was in one instance cooled $8\frac{1}{2}$, and in several 5 or 6°, below its freezing point; but in general it shot rather more readily than some other solutions; which he ascribed, from the analogy of his former experiments, to its less transparency. This salt with snow, in the manner of frigorific mixtures, produced a cold of — 4°.

Nitre.

The next salt which was tried for its effect in lowering the freezing point of water, was nitre. It was part of a large compound crystal, or bundle of crystals, apparently very pure, such as is used in manufacturing the best gunpowder. This being mixed with distilled water, in different proportions, the solutions froze according to the annexed table. The 3d column is calculated from the 5th experiment, in which the freezing point of a solution of one part of salt in eight of water proved to be 26°.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
32 : 1	$30\frac{1}{2}$°	$30\frac{1}{2}$°
24 : 1	30	30
16 : 1	$28\frac{3}{4}$	29
10 : 1	27	$27\frac{1}{4}$
8 : 1	26	26
7.9 : 1	$26\frac{1}{2}$	salt deposited.
7 : 1	$26\frac{1}{2}$	salt deposited.
6.85 : 1	27	much salt deposited.

Nitre is well known to differ from common salt in being much more soluble in warm than in cold water. Hence it would be nothing remarkable, that the solutions being made in water above the freezing point, some of the salt should, when they exceeded a certain strength, be deposited before they began to freeze. But a further question occurred here, whether, when a solution was cooled below its freezing point, the salt would still continue to be deposited; or whether it would not have parted with all the salt it was obliged to let go by the time it came to the degree at which it was to freeze, and would retain the remainder notwithstanding any subsequent cooling. To determine this, Dr. B. noticed carefully the quantities of salt deposited at the bottom of the tumbler, in comparison with the cold of the solution as shown by the immersed thermometer; and he found, that in some cases (for instance, when the salt was to the water only as 1 : 10) the deposition did not begin till after the solution had passed its freezing point; and that when it began earlier, still there was no stop at the freezing point, but the quantity continued augmenting as the cold of the solution proceeded, and that rather in an increasing ratio. Thus when the saturated solution was cooled 8 or 10° below its freezing point, which often happened, the collection of nitre at the bottom was very great; and in this manner he could render a saturated solution of nitre no longer saturated when it came to freeze, the deficiency being sometimes so great as to raise the point of congelation a degree or more. Hence was ascertained the unexpected fact, that the lower such solutions are cooled, the higher is their freezing point.

The nitre deposited by the solution as it cooled, formed, if the vessel remained at rest, small but very white and compact prismatic or needle-like crystals, of considerable length, pointing different ways, and at last curiously interwoven with each other. But if these were broken down, or the solution was stirred with any force, the remaining nitre deposited itself in such minute crystals as to have much the appearance of a powder; probably from the destruction of the regular surfaces on which it would otherwise have continued to form. Frequently, in the stronger solutions, there appeared near the bottom and side of the tumbler many elegant stellated crystals, perhaps a quarter of an inch in diameter, all separate, but sometimes crouding very close on each other, so as to exhibit a spectacle of much beauty. The ice of solutions of nitre, especially when it began to thaw, was very different from common ice, having a soft woolly appearance, as if of a more tender and loose texture. Something of the same kind was observable in the ice of all the other solutions, sufficiently distinguishing it from any that can be formed of pure water. All the solutions of nitre were remarkably limpid, having no tendency to an opaque or turbid cast; and accordingly they were very easily cooled below the freezing point, and could not but with difficulty be made to shoot till they had passed it many degrees. In 2 instances they cooled more than 10°; namely, a solution of 1 part of nitre in 24 of water cooled slowly to $19\frac{1}{2}°$, and then shooting, the thermometer came up to 30°; and another solution, in which the nitre was to the water as 1 : 10, cooled rather below 16°, and having produced some stellated crystals, rose, when the perfect congelation took place, up to 27°.

As, when pure water is cooled below its freezing point, the least particle of ice or snow brought into contact with it causes an instant congelation, Dr. B. was curious to know whether the same effect would be produced when salts were dissolved in the water. Therefore, having one of these nitrous solutions, whose proportions were 8 : 1, he cooled it to 24°, about 2° below its freezing point, and then, no salt being deposited, he put into it a small bit of ice. The effect of this was not instantaneous, as in pure water, though ultimately the same; the bit of ice gradually enlarged, and when it was stirred about in the liquor, a number of star-like crystals formed, which being scattered through it soon brought it to a uniform temperature of 26°. This same solution, when cooled in a preceding experiment to 18°, had its freezing point at 27°, from the quantity of nitre that had been deposited. In all solutions therefore, of such salts as are much more soluble in hot than in cold water, if it be desired to find their freezing point when they are loaded with as much of the salt as the water can contain at that temperature, the most effectual method is to oblige them to shoot, as soon as they can be made to do so, by putting in a small bit of ice or snow; for thus the fallacy which might otherwise arise from the deposition of some of the salt will be avoided. A doubt having been suggested, whether the contact of a crystal of salt might not also bring on the congelation, that experiment was tried, but it produced no effect. Indeed, the formation of saline crystals in these experiments, the liquor still remaining fluid, was a sufficient proof to the contrary. On the whole it seems evident from the preceding table, that the effect of nitre, like that of common salt, is to depress the freezing point in the simple ratio of its proportion to the water; which will be found universally true when allowance is made for the deposition and other sources of fallacy before enumerated. This nitre produced, with snow, a cold of between 26° and 27°.

Sal Ammoniac.

As nitre sunk the freezing point of water so little, namely, but 6°, Dr. B. had recourse for the next set of experiments to that neutral salt which, after sea salt, produces the greatest cold with ice; which is, the common sal ammoniac. The different solutions of this salt in water, being submitted to the action of the frigorific mixtures, froze according to the following table.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
15.7 : 1	$24\frac{1}{2}$°	$24\frac{1}{3}$°
10 : 1	$20\frac{1}{2}$	20
9.8 : 1	20	$19\frac{3}{4}$
7.9 : 1	$16\frac{1}{2}$	$16\frac{3}{4}$
6 : 1	12	12
5 : 1	8	8
4 : 1	4	salt deposited.

The third column is calculated from the last experiment but one, in which the freezing point of a solution of 1 part of the sal ammoniac in 5 of water proved to be 8°. In this table also the numbers of the 3d column agree sufficiently with those of the 2d to show, that sal ammoniac, like the 2 preceding salts, depresses the freezing point in the simple ratio of the proportion in which it is mixed with the water.

It has been a question much contested, whether saline solutions deposit their salt on freezing. That some separation, or a tendency to separation, takes place, many facts concur to prove; and among the rest some phenomena observed in the above-mentioned experiments. For instance, the stellated crystals, when first formed, were barely suspended in the water, and sometimes they even gradually subsided to the bottom; which shows, that they consisted of salt chiefly, only inviscated with ice, or at at least of an over proportion of salt: for the principal mass of ice formed in a saturated solution floats in it like common ice in pure water. Sometimes in solutions of sal ammoniac, and such other salts as separate by the cooling of the water, a sort of flocculent substance is formed, which subsides in the water, and is thus distinguished from the proper ice of the solution, which it otherwise much resembles in appearance. It is composed chiefly of the deposited salt, in very minute crystals like powder, inviscated and kept together with a little ice. The sal ammoniac, mixed with snow, produced a cold of from 4° to $4\frac{1}{2}$° of Fahrenheit's scale.

Rochelle Salt.

Of all the solutions submitted to these experiments, there were none more transparent and elegant than those made with Rochelle salt. The water dissolved a large proportion of this substance, and had its freezing point sunk according to the following table.

Proportion of water to the salt.	Freezing point by the experiments.	Freezing point by calculation.
10 : 1	29½°	29¾°
5 : 1	27½	27½
4 : 1	26⅓	26½
2.6 : 1	24	23½
2.25 : 1	22½	22¼
2 : 1	21	21
1.6 : 1	24	salt deposited.

The 3d column is calculated from the last experiment but one, in which the freezing point of a solution of 1 part of the Rochelle salt in 2 parts of water proved to be 21°. All the solutions of Rochelle salt bore to be cooled remarkably well. In one instance the liquor sunk 11°½ below its freezing point; namely, the solution of 1 part of the salt in 5 of water, whose freezing point proved 27°½, and which cooled to 16° before the crystals of ice shot. In 2 other instances it sunk fully 9° below its freezing point. In trying the greatest cold to be obtained by mixing Rochelle salt with snow, the thermometer could not be got lower than 24°.

Glauber's salt was likewise subjected to the experiments; but its utmost effect in producing cold with snow appearing to be only 2°, this was too small a scale for settling any thing as to the ratio. A solution of it in water, in the proportion of 1 : 5, cooled readily to 31°; but the salt was deposited in great quantities, and often so fast as to stop the cooling of the bottom of the liquor entirely, though the vessel was immersed in a strong frigorific mixture. This phænomenon is exactly the converse of the cold produced by dissolving salts in water; for as there some heat is absorbed, and becomes latent, by the change of the salt from a solid to a fluid state, so here some heat is evolved as the salt assumes the solid crystalline form. The effect is so much more manifest with Glauber's salt only, because the formation of the crystals proceeds so rapidly; whence the quantity of heat generated equals or exceeds the cold communicated by the freezing mixture. Some odd appearances are produced by this sudden stop of the cooling, and the rapid deposition of salt; for instance, a particular ebullition in certain parts of the liquor; but any intelligible description of them would be too minute.

These were all the salts with an alkaline basis that were tried. They all agreed as to the chief object of these experiments, namely, to determine how much the freezing point of water would be sunk by dissolving them in it in various proportions; which by these experiments appears to be, as nearly as could be determined, according to the simple ratio of the proportion each salt bears to the water. Dr. B. now resolved to try a few salts with an earthy and metallic basis.

Sal Catharticus Amarus.

The common sal catharticus amarus of the shops was the specimen used of an earthy salt. It formed a turbid inelegant solution, as if dirty; and with various proportions of water produced the following points of congelation.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
16 : 1	31°	31°
10 : 1	30	30½
4 : 1	28¼	28
3 : 1	26⅘	26⅘
2.4 : 1	25½	25½

The 3d column is calculated from the last experiment, where the freezing point of a solution of 1 part of the sal catharticus amarus in 2.4 of water proved to be 25½°. No salt was deposited from the strongest of these solutions; and as that here used was a deliquescent salt, it must probably have been in a vast proportion to the water, before any such effect would have taken place. Dr. B. has sunk a thermometer with it and snow to 7°½; which according to the proportions in the table, would make more than 3 parts of the salt to 2 of water. Accordingly, a large quantity of the salt was required to the snow. No particular phænomenon was observed with this salt, except the singular configuration of its ice, which assumed the form of fungi, or of some kinds of lichen, with feathered striæ. The solutions were difficult to cool much below their freezing point.

Green Vitriol.

Of the salts with a metallic basis, green vitriol affords one of the most transparent solutions in water. It sinks the thermometer nearly to 27½° with snow, and reduced the freezing point of water according to the following table.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
10 : 1	30¾°	31°
6 : 1	30¼	30½
4 : 1	29¾	29½
3 : 1	28¾	28¾
2.4 : 1	28	28

The 3d column is calculated from the last experiment, in which the freezing point of a solution of 1 part of the green vitriol in 2.4 of water proved to be 28°. The ice formed by these solutions assumed a foliaceous configuration, with a texture of penniform striæ, in some respects like the appearance exhibited by a drop evaporating under a microscope, as delineated by Baker. Scarcely any salt gave the point of congelation so regularly in the proportion of the quantities mixed with the water, and none afforded solutions which cooled more easily and readily below the freezing point. In 2 instances the cooling was more than 11°.

White Vitriol.

Having found that white vitriol, mixed with snow, produced a cold of 20°, melting the snow remarkably fast, I was induced to try the freezing point of its solutions. But though it dissolved very readily in water, yet the liquor it formed was so turbid and thick, that little satisfaction could be derived from the experiments. The only numbers to be relied on are the following, which agree sufficiently with the general result.

Proportion of water to the salt.	Freezing point by the experiment.	Freezing point by calculation.
10 : 1	31°	31°
5 : 1	$29\frac{1}{2}$	30
3 : 1	$28\frac{2}{3}$	$28\frac{2}{3}$

The 3d column is calculated from the last experiment, in which the freezing point of a solution of 1 part of the white vitriol in 3 of water proved to be $28\frac{2}{3}$°. These solutions cooled very ill, none of them having sunk much below the freezing point, and the strongest, which had a copious sediment, forming a crust of ice at the bottom of the tumbler, before it was reduced at all below the term of congelation.

M. Achard, of Berlin, having alleged*, that borax, instead of raising the boiling point of water, like other saline substances, very sensibly depresses it, Dr. B. determined, however extraordinary the fact might appear, to try whether it had any peculiar effect on the freezing point. But having made the experiment with nearly a saturated solution of borax, the thermometer when it congealed was evidently below 32°: he believes about a degree.

As a neutral or middle salt, which when crystallized is always nearly of the same nature, and dissolves in a regular proportion in water, seemed likely to afford the most simple case of the effect of extraneous admixtures, it was with such that he began these experiments. But having found that with them the simple ratio prevailed, he proceeded to try substances of a more variable nature, and capable of being mixed with water in almost any proportion; such as acids, alkalis, and ardent spirits. A material difference in the law, which seemed to occur in these new experiments, renders it proper to defer the account of them till some reflections on the preceding facts, with a few additional experiments to which they gave rise, have been premised.

It seems now universally allowed, that in frigorific mixtures the melting of the snow or ice is the principal cause of the cold produced; all that heat which must become latent in order to give water its fluent form being taken from the sensible heat of the ingredients. But as, when crystallized salts are employed for the purpose, these also are reduced to a liquid form, there must, from this circumstance, be some additional cold produced, such for instance as would be occasioned by dissolving the same salt in water. Suppose then that the latent heat of water is 150°, and that sal ammoniac, in dissolving to saturation, produces so much cold as sinks the whole solution about 20°; it is evident, that if this salt and ice are mixed together in such proportions as just to melt each other, the total cold generated in the operation must amount to 170°. And so much actually is produced before the whole liquefaction is effected; and yet a mixture of these 2 substances will sink the thermometer no lower than to 4° of Fahrenheit's

* See Crell's Chem. Annalen, 1786, vol. 1, p. 501.

scale. The consideration of this apparent difficulty has led to the supposition, that a certain quantity of fire is contained in the crystals of the salt, which being disengaged in the solution keeps up the mixture to a certain temperature. But Dr. B. conceives that the phenomenon depends simply on the gradual liquefaction of the ingredients, a necessary consequence of the cold produced. A saturated solution of sal ammoniac freezes itself at 4°; therefore, when the mixture is reduced by the liquefaction of the ingredients, to that temperature, no more of them can melt, because any addition of cold would freeze what is already melted; and if the mixture, under such circumstances, were placed in an atmosphere of its own temperature, the ingredients would remain for ever in that same state, without any further liquefaction. But in an atmosphere warmer than 4°, they continue to melt, more or less slowly, as the heat which is gradually communicated furnishes what is necessary to become latent. This communicated sensible heat being immediately converted into latent, the mixture will always be kept down to the same temperature as long as there is a sufficient mass of unmelted materials; and it can sink no lower, because then the liquefaction would be stopped; consequently such mixtures must preserve, as they have been found to do, a pretty uniform temperature, so as to have been formerly used for graduating thermometers. And the whole cold produced, or, to speak properly, the whole of the heat made to disappear, he presumes to be ultimately equal to the full quantity of latent heat belonging to the dissolved ice and salt.

As it is well known that water, after it has been saturated with one salt, will take up a certain portion of another salt without depositing any of the former, Dr. B. was curious to try what effect the addition of this 2d salt would produce on the freezing point; and particularly whether it would depress the freezing point of the saturated solution the same number of degrees that an equal proportion of the same salt would depress the freezing point of water; and whether the same simple ratio would hold good, or any new law take place. To bring this to the test of experiment, he took a saturated solution of nitre, whose freezing point of course was between 26° and 27°; and adding to it the purified common salt in various proportions, he obtained the following results.

Compound solution of nitre and common salt.

Proportion of water to the nitre.	Proportion of water to the common salt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
	30.2 : 1	$23\frac{1}{2}$°	$22\frac{3}{4}$°	$\frac{3}{4}$°
A saturated solution.	15 : 1	$20\frac{3}{4}$	19	$1\frac{3}{4}$
	10 : 1	$17\frac{1}{2}$	$15\frac{1}{4}$	$2\frac{1}{4}$
	7.4 : 1	$13\frac{1}{2}$	$11\frac{1}{2}$	2
	5 : 1	$5\frac{1}{4}$	4	$1\frac{1}{4}$

It is evident, from the freezing points of this compound solution, that the com-

common salt depressed the freezing point of the solution of nitre something less than it would have depressed the freezing point of water, if added to it in the same proportion. To show this more evidently, he added a 4th and a 5th column to the table: the 4th column is formed by taking the freezing point of the saturated solution of nitre as $26\frac{1}{2}°$, and then finding how many degrees the quantity of common salt added would have depressed the freezing point of water; this number of degrees subtracted from the constant number $26\frac{1}{2}°$, gives the freezing point by calculation, namely, what it should have been if the salt had produced the same effect on the solution of nitre as it would on pure water; and the difference between this and the freezing point found by the experiment gives the numbers in the 5th column. From the table it is apparent, that the deficiency of effect from the salt goes on increasing to the 3d experiment, after which it decreases. Probably some particular law takes place, which it would require a great number of experiments to develope; but the decrease toward the last may in part be owing to the greater quantity of nitre which the water, when it began to be loaded with common salt, retained at the time of congelation, and which must have its effect in depressing the freezing point. The above-mentioned circumstance seems rather contradictory to an opinion which has been entertained, that when one salt, added to a saturated solution of another salt, enables it to take up more of the former salt, it is only because the water of crystallization of the 2d salt really adds to the quantity of the dissolving fluid.

Dr. B. next proceeded to try a similar experiment with sal ammoniac and the purified common salt, but with this difference, that neither salt should be added to the water in such quantity as to come near the point of saturation, suspecting that the diminution of effect observed in the foregoing experiments might depend, in part at least, on this circumstance. The sal ammoniac therefore was dissolved in water in the proportion of 1 : 10, and the corresponding point of congelation appeared by experiment to be $20\frac{1}{2}°$, agreeing very well with the table of sal ammoniac formerly given. To this solution was added the purified common salt, in proportion to the water as 1 : 15, and then as 1 : 10; the resulting points of congelation were as shown in the following table, constructed in all respects as the immediately preceding.

Compound solution of sal ammoniac and common salt.

Proportion of water to the sal ammoniac.	Proportion of water to the common salt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
10 : 1	15 : 1	$12\frac{3}{4}°$	13 °	$+\frac{1}{4}$
10 : 1	10 : 1	$9\frac{1}{4}$	$9\frac{1}{5}$	$-\frac{1}{20}$

Hence it appears, that in this compound solution both salts produced, as exactly as the experiments can be expected to show, their full effect in depressing the point of congelation. When the solutions at length froze, after cooling many degrees below the freezing point, the crystals shot in a very beautiful manner round the bulb and up the stem of the thermometer.

In a compound solution of Rochelle and common salt there was however a deficiency of effect. For the solution of Rochelle salt in the proportion of 1 part to 4 of water, having its freezing point at $26\frac{1}{3}°$; when common salt was dissolved in it, in the proportion of $\frac{1}{10}$th, the freezing point appeared by experiment to be $16\frac{1}{4}°$, whereas by calculation it should have been depressed nearly to 15°.

A composition of 3 salts was affected as follows:

Compound solution of Rochelle salt, common salt, and sal ammoniac.

Proportion of water to the Rochelle salt.	Proportion of water to the common salt.	Proportion of water to the sal ammoniac.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
9.8 : 1	10 : 1	17 : 1	13° —	$11\frac{1}{2}°$	$- 1\frac{1}{2}°$

The computation is made thus: Rochelle salt, in the proportion of 1 : 9.8, depresses the freezing point $2\frac{1}{4}°$; common salt, in the proportion of 1 : 10, sinks it $11\frac{1}{4}$; and sal ammoniac, in the proportion of 1 : 17, sinks it 7°; now $2\frac{1}{4} + 11\frac{1}{4} + 7 = 20\frac{1}{2}$; which subtracted from 32, leaves $11\frac{1}{2}°$ for the computed freezing point of this mixture. The moment Dr. B. had found by experiment, that the addition of a different salt to the saturated solution of any salt, would still further depress its freezing point, it was obvious to conclude, that greater cold could be produced with snow by a mixture of salts than by means of either taken separately. He made several experiments with this view, and found it uniformly the fact, that by adding a certain proportion of a salt which had less power of producing cold with snow, to one which had a greater power, the frigorific effect of the latter was sensibly increased. Passing over examples of less consequence, it may be sufficient to instance common salt and sal ammoniac. The ordinary common salt he used to mix with snow, sunk the thermometer to — 5°; the sal ammoniac to + 4°; but when some of the latter salt was mixed with the former, the composition produced with snow a cold of — 12°. On several occasions he made use of this composition to obtain a greater degree of cold than common salt alone would produce, and found it a very convenient method. On this principle it is that impure common salt always makes a stronger freezing mixture than the pure; it being, in fact, a composition of salts. Three salts have produced a greater cold than 2, but he had not carried the experiments far enough to ascertain the limits of this effect.

As the cold produced by common salt with snow is — 4° or more, and by sal ammoniac + 4°, it is difficult to conceive in what manner Fahrenheit fixed the zero of his thermometer. All those who have examined the few authentic passages to be found in authors on this subject, will be sensible how vaguely they are expressed, leaving it doubtful whether he used common salt alone, sal ammoniac alone, or both mixed together. There is no method of sinking a thermometer exactly to 0° with these salts and snow, but by means of a certain smaller proportion of common salt added to the sal ammoniac; and it would have been an extraordinary chance, that Fahrenheit should hit precisely this proportion often enough to make him rely on the point so found as the commencement of his scale; more especially as the proportion is probably no greater than a 7th or a 6th of common salt to the sal ammoniac. Is it possible that Fahrenheit, finding a considerable difference in his experiments, took the mean between them for his zero, without any respect to the different nature of the salts with which he operated? It appears that he was at this time so little acquainted with the subject, as to consider his zero as the utmost limit of cold.

Dr. B. comes now to certain substances which, by equal additions, seem to depress the freezing point of water in an increasing ratio. These, as was mentioned before, are acids, alkalis, and spirit of wine. The specific gravity of the vitriolic acid employed was 1.837 at 62° of heat; its effect on the freezing point is shown by the annexed table.

Vitriolic acid.

Proportion of water to acid.	Freezing point by the experiment	Freezing point by calculation.
10 : 1	24½°	22¼°
5 : 1	12½	12⅖
4 : 1	7½	7½

This table is constructed in the same manner as those formerly given of the solution of simple salts; the last experiment, where the proportion is 4 : 1, being taken as the standard for computation; and the extreme difference between the calculation and experiment is no less than 2¼°, on a reduction of the freezing point from 24½° to 7½°. The freezing point, set down in the table, is that to which the liquor rose on congealing, after having been cooled several degrees lower; which it is proper to remark, because the ice rose 2 or 3 degrees in thawing.

The nitrous acid employed was smoking, and had its specific gravity 1.454. It acted on the freezing point according to the following table, which is constructed in all respects like the preceding.

The greatest difference between the calculation and experiment appears here to be only ⅔ of a degree; but that is more than can well be attributed to inaccuracy. These mixtures cooled remarkably well; that in which the water was to the acid as 7.64 : 1 sunk down to 6° before it froze. The ice heated about a degree before it was melted.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
16.8 : 1	26⅓°	25⅔°
10 : 1	22 –	21½
7.64 : 1	18	18
5.06 : 1	10½	11
4.26 : 1	7	7

Spirit of salt being a very weak acid, its increase of ratio was not perceptible within the limits to which he was confined.

Muriatic acid.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	25°	25°
5.1 : 1	18⅘	18½
3.05 : 1	9⅓	9⅓

This table is constructed as the foregoing; and the specific gravity of the marine acid was 1.163.

Salt of tartar, such as is usually sold in the shops, was the vegetable alkali employed. It did not readily deliquesce, and consequently was not very caustic: the cold it produced with snow was — 12°.

Salt of tartar.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	27¼°	26½°
7.5 : 1	25½	24⅘
5 : 1	22¾	21⅕
3 : 1	15	14
2.5 : 1	11¾	10½
2 : 1	5	5

Here the greatest difference between the calculation and experiment is something more than 1½°, in sinking the freezing point from 22¾° to 5°; but in the higher freezing points of the table it is less, as well as in the lower. Perhaps this irregularity in the experiments is in part to be ascribed to an impurity in the salt of tartar; a turbid appearance, and at length a deposition took place in all these solutions, but principally in the stronger, occasioned probably by tartar of vitriol, with which that salt is so frequently mixed.

The mineral alkali tried, which was the crystallized soda of the shops, showed no increase of ratio; but the scale of its operation was too small for a proper judgment to be formed.

Mineral alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	30° —	29$\frac{9}{10}$°
5 : 1	27⅘	27⅘

This salt would not remain suspended in much greater proportion in the cooled water. He considered the solutions as decreasing in their freezing point by an equal ratio: possibly, if the salt of tartar had been crystallized, and perfectly saturated with fixed air, it would also have acted in the manner of a neutral salt, and produced no increase upon the ratio.

Volatile alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
10 : 1	25°	25¼°
5.18 : 1	19	19

Dr. B.'s volatile alkali, being the sal volatilis salis ammoniaci, was tried only in 2 proportions. Here also there is no appearance of an increase of ratio, but rather the contrary.

The specific gravity of the spirit of wine employed was .829 at 62°. It depressed the freezing point according to the following table: The total difference between the calculation and experiment, on a reduction of the freezing point from $24\frac{1}{2}$ to 4°, is $\frac{3}{4}$ of a degree; but the intermediate points are very irregular, and is an opposite sense, as if the ratio were decreasing. It seems more probable, that some inaccuracy in the experiments, perhaps owing in part to the evaporation of the spirits, should be the occasion of this, than that there should be such an irregularity in the law.

Spirit of wine.

Proportion of water to spirit.	Freezing point by experiment.	Freezing point by calculation.
	°	°
8.5 : 1	$24\frac{1}{2}$	$23\frac{3}{4}$
5 : 1	17	18
3.66 : 1	$12\frac{1}{4}$	$12\frac{3}{4}$
3 : 1	$8\frac{1}{2}$	$8\frac{2}{3}$
2.5 : 1	4	4

If any person should allege, that the difference between the observed and computed freezing points in the foregoing tables is not sufficient to establish the increase of ratio, Dr. B. can only reply, that it appears to him greater than can reasonably be ascribed to error in the experiment; especially as similar experiments with neutral salts, not conducted more attentively, agreed so well together in pointing out a different law. It must be allowed however, that the experiments do not show any increase of ratio, except in the vitriolic and nitrous acids, salt of tartar, and, with more ambiguity, in spirit of wine; from analogy only he suspects it to take place in the other acids, and in the mineral and volatile alkalis provided they are caustic. That a different law from what prevails in the neutral salts should take place with these substances, seems not surprizing, when it is considered how much stronger attraction they show for water, and how much less limited the proportion is in which they will unite to it: for the same reasons, he should not think it extraordinary, if deliquescent salts, combined with water, should be found to observe the same increasing ratio in depressing the freezing point.

Dr. B. concludes this paper with the account of an experiment to determine the effect of salt on the expansion of water by cold. Pure water begins to show this expansion about the temperature of 40°, that is, 8° above its freezing point. He put a solution of common salt, in the proportion of 4.8 parts of water to one of the salt, and consequently whose freezing point was $8\frac{2}{3}$°, into an apparatus he had used for other experiments of the same kind; and found that the solution continued to contract till it was cooled to 17°, but had sensibly expanded by the time it was cooled to 15. Suppose the expansion to have begun at $16\frac{2}{3}$°, it would be just 8° above its new freezing point. Hence we have reason to conclude, as far as one experiment goes, that the combination of a salt with water has no other effect on its quality of expanding by cold, than to depress the point at which that quality begins to be sensible, just as much as it depresses the point of congelation.

XIX. Additional Experiments and Observations relating to the Principle of Acidity, the Decomposition of Water, and Phlogiston. By Joseph Priestley, LL.D., F.R.S. p. 313.

When Dr. P. wrote the former paper on this subject, he says he had found that the decomposition of dephlogisticated and inflammable air, by means of the electric spark, produced an acid liquor, which Dr. Withering found to be the nitrous; though it should have been observed, that he expressed some doubt whether the liquor did not also contain some other acid besides the nitrous. Dr. P. has since that time been desirous to ascertain the quantity of acid producible from a given quantity of air; and with this view he gave Mr. Keir as much of the liquor as he had collected from the decomposition of about 500 ounce measures of dephlogisticated air, and the usual proportion of inflammable air mixed with it. The liquor, he informed Dr. P., was 442 grains, of the specific gravity of 1022, that of water being 1000, and that it contained as much acid as was equivalent to 12.54 grains of concentrated acid of vitriol; which quantity of vitriolic acid is capable of saturating as much vegetable fixed alkali as is contained in $22\frac{1}{2}$ grains of dry nitre, or about $23\frac{1}{4}$ grains of nitre crystallized in mean temperature. The sediment of the same liquor he also supposed to contain, at least, as much acid as the liquor itself. From the preceding data, given by Mr. Keir, and making allowance for the indefinite quantity of water contained in the concentrated acid of vitriol, Dr. P. thinks that not much more than a 20th part of dephlogisticated air is the acidifying principle, and that 19 parts are water.

Though Mr. Keir found the greatest part of the acid in the liquor with which Dr. P. furnished him to be the nitrous, there were evident signs of its containing a small portion of marine acid, by its making a precipitation with a solution of silver in nitrous acid. But this mixture of marine acid, he observes, is constantly found to accompany the production of nitre in the operations of nature. Whether the different substances from which the dephlogisticated air was extracted made any difference in this case, Dr. P. cannot tell; but that which he gave Dr. Withering was from minium, and that which Mr. Keir examined was from manganese. In the notes which Dr. P. took of the first production of this liquor he termed it blue, and Dr. Withering also calls it blue, and once a greenish blue; but that which he gave Mr. Keir, and all that he got afterwards, was a decided and deep green, which Mr. Keir thinks to be owing to the phlogistication of the nitrous acid.

That water enters into the constitution of every kind of air Dr. P. supposed, because it certainly does into that of inflammable, fixed, and dephlogisticated air, and because none of them can be produced except by processes in which water either certainly is, or may be well supposed to be present. That nitrous air also contains water, he found from the iron that is heated in it becoming a proper finery cinder. At the publication of his last volume of experiments, he had found, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogisticated air. Having since that time repeated this experiment, and afterwards heated the iron, which was by this means increased in weight, in inflammable air, the iron lost its additional weight, and water was copiously produced, as in the same process with finery cinder, or, as he sometimes calls it, scale of iron. As nitrous air may be deprived of its water, and become phlogisticated air by heating iron in it, he finds that it undergoes the same change by being repeatedly transmitted through hot porous earthen tubes, through which he had discovered that vapour will pass one way, while the air contiguous to the heated tube will pass the other.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. He heated 5 grains of charcoal of copper in 8 ounce-measures of nitrous air, till it was increased to 10 ounce-measures, and the charcoal had lost 1 grain. Examining the air, he found about $\frac{1}{5}$ of it to be fixed air, and the remainder phlogisticated. It seems therefore, that nitrous air consists of water, and something that may be called the basis of nitrous acid, or that substance which, when united to dephlogisticated air, will make nitrous acid; and this seems to be pure phlogiston, since it is found, as the preceding experiments show, in the purest inflammable air. May we not hence infer, that the nitrous is the simplest of all the acids, and perhaps the basis of all the rest?

Mr. Watt desires it might be mentioned as his conjecture, that the nitrous acid is contained in the inflammable air as the acid of vitriol is in sulphur, the phosphoric in phosphorus, &c.; and that the dephlogisticated air does nothing more than develope the acid. Mr. Keir, who was led to expect that an acid must be the result of the union of dephlogisticated and inflammable air, because some acid is always the consequence of its union with other inflammable substances, thinks that both may be necessary ingredients in it. Further experiments may throw more light on the subject.

To the analysis of the above acid liquor, by Dr. Withering, he adds the following inferences: these, Sir, are all the trials I have made with the liquors produced in your experiment. They pretty clearly prove the acid generated to be the same, whether the dephlogisticated air was procured from red precipitate of mercury, from minium, or from manganese, and that this acid is the nitrous acid. It is not quite so clear why the liquor and sediment in § 4 gave no stronger marks of the presence of nitrous acid; but it is evident that the acid had united itself to the iron, if not to the tin, of the vessel employed: and I find, that when nitrous acid is fully saturated with iron by being boiled with it, and fixed alkali is added, this mixture submitted to distillation, with the addition of concentrated vitriolic acid, yields no red vapours, and very little smell of nitrous acid.

And to Mr. Keir's letter on the analysis of the same acid, he adds, if, on examining the acids which you or others may hereafter obtain by the inflammation of airs, a mixture of marine acid be constantly found to accompany the production of the nitrous, the fact will be only analogous to all the other known productions of nitrous acid; in all which, either in the natural formation of nitre as in Spain and India, or in the nitre beds and walls made by art, a very large proportion of marine salts is constantly observed to accompany the nitre. Other particulars on this subject may be found by consulting the pamphlet in which this paper was separately printed by Dr. Priestley.

XX. On the Probabilities of Survivorships between Two Persons of any Given Ages, and the Method of Determining the Values of Reversions Depending on those Survivorships. By Mr. Wm. Morgan. p. 331.

The hypothesis of an equal decrement of life, adopted by M. de Moivre, for the purpose of facilitating the computations of life annuities, has not only been rendered unnecessary by the late publication of many excellent tables deduced from real observations, but has also been found so very incorrect in some cases, that probably little or no recourse will ever be had to it in future. But though the direct application of this hypothesis may be laid aside, there is danger of its not being entirely abandoned; and mathematicians may still be led to reason from this principle, by deriving their rules from the expectations rather than from the real probabilities of life. The ingenious Mr. Thomas Simpson has contented himself with this inaccurate method in his select exercises, and he has been followed in it by most other writers on the subject. Even in those cases which involve only 2 lives, the errors are often considerable, especially if the expectations are derived from the London, the Sweden, or any other tables in which the decrements of life are unequal. But when 3 lives are involved in the question, these errors are generally enormous; nor is it ever safe, when the ages of those lives differ very much, to have recourse to rules which are founded on this principle. The 3 following problems, though the most common in the doctrine of survivorships, have never hitherto been solved in a manner strictly true. The 2d of them is of particular importance, and I have taken much pains to examine how far Mr. Simpson's solution of it may be depended on. It has indeed been solved by M. de Moivre, and Mr. Dodson: but the first of these writers has erred most egregiously in the solution itself, and the other having derived his rule from a wrong hypothesis, has rendered it of no use. It is much to be wished that the solutions of all cases in reversions and survivorships were deduced, like the 3 following, from the real probabilities of life. Most of those which are now in use are at best but approximations, and can never be relied on with any tolerable degree of satisfaction.

PROB. 1.—Supposing the ages of 2 persons, A and B, to be given; to determine the probabilities of survivorship between them, from any table of observations.

Solution.—Let a represent the number of persons living in the table at the age of A the younger of the 2 lives. Let a', a'', a''', a'''', &c. represent the decrements of life at the end of the 1st, 2d, 3d, 4th, &c. years from the age of A. Let b represent the number of persons living at the age of B the older of the 2 lives, and c, d, e, f, &c. the number of persons living at the end of the 1st, 2d, 3d, 4th, &c. years from the age of B. Supposing now it were required to determine the probability of B's surviving A in the first year. It is manifest that this event may take place either by A's dying before the end of the year and B's surviving that period, or by the extinction of both the lives, restrained however to the contingency of B's having died last. The probability that A dies in the first year, and that B survives it, is expressed by the fraction $\frac{a'c}{ab}$. The probability that both the lives die in this year is expressed by the fraction $\frac{a'(b-c)}{ab}$; and as it is very nearly an equal chance that A dies first, this fraction should be reduced one-half, and then it will become $=\frac{a'(b-c)}{2ab}$. Hence the whole probability of B's surviving A in the first year will be $=\frac{a'c}{ab}+\frac{a'(b-c)}{2ab}$ $=\frac{a'(b+c)}{2ab}$. In like manner, the probability of B's surviving A in the 2d, 3d, 4th, &c. years may be found $=\frac{a''(c+d)}{2ab} \ldots \frac{a'''(d+e)}{2ab} \ldots \frac{a''''(e+f)}{2ab}$, &c. respectively; therefore the whole probability of B's surviving A will be $=\frac{1}{ab}$ $\times\left(\frac{b+c}{2}a'+\frac{c+d}{2}a''+\frac{d+e}{2}a'''+\frac{e+f}{2}a'''', \text{&c.}\right)$ Having found, by the preceding series, the probability of B, the elder, surviving A the younger; the other expression, which denotes the probability of A's surviving B, is well known to be the difference between the foregoing series and unity.

The sum of this series might easily be determined from tables of the expectations of single and joint lives. But no such table as the latter having ever been computed, it will by no means be found a laborious undertaking to compute a table of the probabilities of survivorship between 2 persons of all ages immediately from this series, without having recourse to the expectations of life. For if the probability of survivorship between any 2 persons be found, the probability between 2 persons 1 year younger is obtained with little difficulty, and by proceeding in this manner a whole table may be formed in less time than would be necessary for computing one of the expectations of 2 joint lives. To exemplify this, I shall just set down a few operations for determining the probability of survivorship, according to the Northampton Table of Observations, be-between 2 persons whose common difference of age is 10 years.

Age of B.	Age of A.	Probability of B's surviving A.	Probability of A's surviving B.
96	86	$\frac{1}{1 \times 145} \times (\frac{0+1}{2} \times 34) = 1173$	$1 - .1173 = .8887$
95	85	$\frac{1}{4 \times 186} \times (\frac{4+1}{2} \times 41 + 17) = .1606$..	$1 - .1606 = .8394$
94	84	$\frac{1}{9 \times 234} \times (\frac{9+4}{2} \times 48 + 119.5) = .2049$	$1 - .2049 = .7951$
93	83	$\frac{1}{16 \times 289} \times (\frac{16+9}{2} \times 55 + 431.5) = .2420$	$1 - .2420 = .7580$
92	82	$\frac{1}{24 \times 346} \times (\frac{24+16}{2} \times 57 + 1119) = .2720$	$1 - .2720 = .7280$
91	81	$\frac{1}{34 \times 406} \times (\frac{34+24}{2} \times 60 + 2259) = .2897$	$1 - .2897 = .7103$
90	80	$\frac{1}{46 \times 469} \times (\frac{46+34}{2} \times 63 + 3999) = .3022$	$1 - .3022 = .6978$

It may easily be seen, from these specimens, in what manner the probabilities of survivorship between 2 younger lives are deduced from the probabilities between 2 older lives, provided their common difference of age be the same; for the numbers 17 .. 119.5 ... 431.5, &c. in the 2d, 3d, 4th, &c. series, are the sums of the series next preceding. Thus 17 is $= 34 \times \frac{1}{2}$ ... 119.5 is $= 41 \times \frac{5}{2} + 17$ .... 431.5 is $= 48 \times \frac{13}{2} + 119.5$, &c. It may be necessary to observe further, that if the ages of the 2 persons be equal, the probability of survivorship between them being likewise equal, is expressed by the fraction $\frac{1}{2}$; and that this affords an instance of the accuracy of the foregoing investigation; for the series expressing the probability in this case is the same with this fraction, the chance of survivorship becoming then (since $a = b$; $a' = b - c$; $a'' = c - d$, &c.; and $(b + c) \times a' = (b + c) \times (b - c) = b^2 - c^2$, &c.) $= \frac{bb - cc}{2bb} + \frac{cc - dd}{2bb}$, &c. $= \frac{1}{2}$.

Mr. Simpson, in his Treatise on Annuities and Reversions, (Lemma 2, p. 100,) has given a curve whose area determines the probability of survivorship between 2 persons according to any table of observations. If one of the lives be not very young, so that the equidistant ordinates may not be too few, this area is sufficiently correct. But if the elder of the 2 lives is under 20 years of age, it becomes necessary to assume so many equidistant ordinates to render the solution accurate, when the decrements of life are unequal, that the operation is rendered much too laborious for use; nor do I know that it can be necessary to have recourse to this area in any case, especially as the true probabilities of survivorship are so easily computed from the preceding series.

The following table has been formed in the manner described above; and as no such table has ever been attempted before, I have been the more desirous to render it complete, by computing the probabilities of survivorship between 2 persons of all ages, whose common difference is not less than 10 years. Instead also of supposing certainty to be denoted by unity, I have assumed 100 for this purpose; so that the sums in the adjoining columns express the number of chances in 100 which are in favour of B or A's surviving the other.

Table, showing the Probabilities of Survivorship between 2 Persons of all Ages, whose Common Difference of Age is not less than 10 Years, computed from the Northampton Table of Observations in Dr. Price's Treatise on Reversionary Payments.

Ten Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
11.. 1	58.59	41.41	33.. 23	41.97	58.03	55.. 45	38.53	61.47	77.. 67	31.20	68.80
12.. 2	51.36	48.64	34.. 24	41.83	58.17	56.. 46	38.35	61.65	78.. 68	30.90	69.10
13.. 3	48.23	51.77	35.. 25	41.70	58.30	57.. 47	38.16	61.84	79.. 69	30.47	69.53
14.. 4	45.98	54.02	36.. 26	41.56	58.44	58.. 48	37.97	62.03	80.. 70	30.00	70.00
15.. 5	44.70	55 30	37.. 27	41.41	58.59	59.. 49	37.77	62.23	81.. 71	29.58	70.42
16.. 6	43.43	56.57	38.. 28	41.26	58.74	60.. 50	37.56	62.44	82.. 72	29.19	70.81
17.. 7	42.53	57.47	39.. 29	41.10	58.90	61.. 51	37.30	62.70	83.. 73	28.90	71.10
18.. 8	41.91	58.09	40.. 30	40.94	59.06	62.. 52	37.00	63.00	84.. 74	29.02	70.98
19.. 9	41.60	58.40	41.. 31	40.78	59.22	63.. 53	36.68	63.32	85.. 75	29.17	70.83
20.. 10	41.53	58.47	42.. 32	40.63	59.37	64.. 54	36.34	63.66	86.. 76	29.25	70.75
21.. 11	41.58	58.42	43.. 33	40.49	59.51	65.. 55	35.97	64.03	87.. 77	29.42	70.58
22.. 12	41.68	58.32	44.. 34	40.34	59.66	66.. 56	35.58	64.42	88.. 78	29.94	70.06
23.. 13	41.78	58.22	45.. 35	40.19	59.81	67.. 57	35.17	64.83	89.. 79	30.29	69.71
24.. 14	41.90	58.10	46.. 36	40.04	59.96	68.. 58	34.74	65.26	90.. 80	30.22	69 78
25.. 15	42.02	57.98	47.. 37	39.88	60.12	69.. 59	34.30	65.70	91.. 81	28.97	71.03
26.. 16	42.16	57.84	48.. 38	39.71	60.29	70.. 60	33.85	66.15	92.. 82	27.20	72.80
27.. 17	42.26	57.74	49.. 39	39.54	60.46	71.. 61	33.38	66.62	93.. 83	24.20	75.80
28.. 18	42.32	57.68	50.. 40	39.38	60.62	72.. 62	32.90	67.10	94.. 84	20.49	79.51
29.. 19	42.34	57.66	51.. 41	39.23	60.77	73.. 63	32.46	67.54	95.. 85	16.06	83.94
30.. 20	42.31	57.69	52.. 42	39.07	60.93	74 . 64	32.04	67.96	96.. 86	11.73	88.27
31.. 21	42.22	57.78	53.. 43	38.90	61.10	75.. 65	31.70	68.30			
32.. 22	42.09	57.91	54.. 44	38.81	61.19	76.. 66	31.45	68.55			

Twenty Years Difference.

21.. 1	52.46	47.54	32.. 12	34.42	65.58	43.. 23	33.59	66.41	54.. 34	30.60	69.40
22.. 2	44.33	55.67	33.. 13	34.44	65.56	44.. 24	33.35	66.65	55.. 35	30.32	69.68
23.. 3	40.88	59.12	34.. 14	34.47	65.53	45.. 25	33.09	66.91	56.. 36	30.03	69.97
24.. 4	39.00	61.00	35.. 15	34.51	65.49	46.. 26	32.83	67.17	57.. 37	29.73	70.27
25.. 5	37.69	62.31	36.. 16	34.56	65.44	47.. 27	32.56	67.44	58.. 38	29.43	70.57
26.. 6	36.40	63 60	37.. 17	34.58	65.42	48.. 28	32.28	67.72	59.. 39	29.13	70.87
27.. 7	35.48	64.52	38.. 18	34.54	65.46	49.. 29	31.99	68.01	60.. 40	28.82	71.18
28.. 8	34.84	65.16	39.. 19	34.44	65.56	50.. 30	31.70	68.30	61.. 41	28.49	71.51
29.. 9	34.52	65.48	40.. 20	34.29	65.71	51.. 31	31.43	68.57	62.. 42	28.14	71.86
30.. 10	34.41	65.59	41.. 21	34.08	65.92	52.. 32	31.16	68.84	63.. 43	27.74	72.26
31.. 11	34.40	65.60	42.. 22	33.84	66.16	53.. 33	30.88	69.12	64.. 44	27.33	72.67

Twenty Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
65..45	26.90	73.10	73..53	22.91	77.09	81..61	18.23	81.77	89..69	16.23	83.77
66..46	26.46	73.54	74..54	22.35	77.65	82..62	17.57	82.43	90..70	15.67	84.33
67..47	26.01	73.99	75..55	21.81	78.19	83..63	17.03	82.97	91..71	14.50	85.50
68..48	25.55	74.45	76..56	21.31	78.69	84..64	16.73	83 27	92 .72	13.05	86.95
69..49	25.09	74.91	77..57	20.80	79.20	85..65	16.55	83.45	93 .73	11.08	88.92
70..50	24.61	75.39	78..58	20.24	79.76	86..66	16.29	83.71	94..74	9.24	90.76
71..51	24.06	75.94	79..59	19.59	80.41	87..67	16.38	83.62	95..75	7.17	92.83
72..52	23.49	76.51	80..60	18.90	81.10	88..68	16.44	83.56	96..76	5.12	94.88

Thirty Years Difference.

31.. 1	48.23	51.77	48..18	26.90	73.10	65..35	20.93	79.07	82..52	12.46	87.54
32.. 2	39.36	60.64	49..19	26.72	73.28	66..36	20.47	79.53	83..53	11.94	88.06
33.. 3	35.57	64.43	50..20	26.50	73.50	67..37	20.01	79.99	84..54	11.60	88.40
34.. 4	32.86	67.14	51..21	26.21	73 79	68..38	19.54	80.46	85..55	11.30	88.70
35.. 5	31.35	68.65	52..22	25.89	74.11	69..39	19.06	80.94	86..56	11.04	88.96
36.. 6	29.85	70.15	53..23	25.56	74.44	70..40	18.59	81.41	87..57	10.80	89.20
37.. 7	28.83	71.17	54..24	25.22	74.78	71..41	18.10	81.90	88..58	10.63	89.37
38.. 8	27.98	72.02	55..25	24.87	75.13	72..42	17.58	82.42	89..59	10.25	89.75
39.. 9	27.53	72.47	56..26	24.52	75.48	73..43	17.05	82.95	90 60	9.65	90.35
40..10	27.33	72.67	57..27	24.16	75.84	74..44	16.53	83.47	91..61	8.68	91.32
41..11	27.24	72.70	58..28	23.79	76.21	75..45	16.04	83.96	92..62	7.54	92.46
42..12	27.18	72.82	59..29	23.41	76.59	76..46	15.60	84.40	93..63	6.18	93.82
43..13	27.14	72.86	60..30	23.02	76.98	77..47	15.51	84.85	94..64	4.93	95.07
44..14	27.11	72.89	61..31	22.63	77.37	78..48	14.68	85.32	95..65	3 68	96.32
45..15	27.08	72.92	62..32	22.23	77.77	79..49	14.16	85.84	96..66	2.58	97.42
46..16	27.06	72.94	63..33	21.81	78.19	80..50	13.61	86.39			
47..17	27.01	72 99	64..34	21.38	78.62	81..51	13.04	86.96			

Forty Years Difference.

41.. 1	43.57	56.4	55..15	20.11	79.89	69..29	15.32	84.68	83..43	8.76	91 24
42.. 2	33.98	66.02	56..16	20.05	79.95	70..30	14.83	85.17	84..44	8.46	91.54
43.. 3	29.85	70.15	57..17	19.96	80.04	71..31	14.34	85.66	85..45	8.18	91.82
44.. 4	26.88	73.12	58..18	19.81	80.19	72..32	13.84	86.16	86..46	7.95	92.05
45.. 5	25.20	74.80	59..19	19.59	80.41	73..33	13.35	86.65	87..47	7.74	92.26
46.. 6	23.53	76.47	60..20	19.31	80.69	74..34	12.87	87.13	88..48	7.59	92.41
47.. 7	22.30	77.70	61..21	18.96	81.04	75..35	12.42	87.58	89..49	7.32	92.68
48.. 8	21.40	78.60	62..22	18.55	81.45	76..36	11.99	88.01	90..50	6.90	93.10
49.. 9	20.86	79.14	63..23	18.12	81.88	77..37	11.56	88.44	91..51	6.17	93.83
50..10	20.59	79.41	64..24	17.68	82.32	78..38	11.12	88.88	92..52	5.33	94.67
51..11	20.45	79.55	65..25	17.22	82.78	79..39	10.64	89.36	93..53	4.32	95.68
52..12	20.35	79.65	66..26	16.76	83.24	80..40	10.21	89.79	94 .54	3.42	96.58
53..13	20.26	79.74	67..27	16.29	83.71	81..41	9.68	90.32	95..55	2.51	97.49
54..14	20.18	79.82	68..28	15.81	84.19	82..42	9.19	90.81	96..56	1.73	98.27

Fifty Years Difference.

51.. 1	39.16	60.84	63..13	13.90	86.10	75..25	10.32	89.68	87..37	5.91	94.09
52.. 2	28.87	71.13	64..14	13.76	86.24	76..26	9.91	90.09	88..38	5.74	94.26
53.. 3	24.46	75.54	65..15	13.62	86.38	77..27	9.50	90.50	89..39	5.48	94.52
54.. 4	21.28	78.72	66..16	13.50	86.50	78..28	9.07	90.93	90..40	5.11	94.89
55.. 5	19.48	80.52	67..17	13.35	86.65	79..29	8.61	91.39	91..41	4.55	95.45
56.. 6	17.68	82.32	68..18	13.14	86.86	80..30	8.15	91.85	92..42	3.92	96.08
57.. 7	16.35	83.65	69..19	12.86	87.14	81..31	7.70	92.30	93..43	3.15	96.85
58.. 8	15.35	84.65	70..20	12.53	87.47	82..32	7.27	92.73	94..44	2.47	97.53
59.. 9	14.74	85.26	71..21	12.11	87.89	83..33	6.88	93.12	95..45	1.80	98.20
60..10	14.42	85.58	72..22	11.65	88.35	84..34	6.60	93.40	96..46	1.23	98.77
61..11	14.22	85.78	73..23	11.20	88.80	85..35	6.35	93.65			
62..12	14.06	85.94	74..24	10.75	89.25	86..36	6.12	93.88			

Sixty Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
61.. 1	34.94	65.06	70.. 10	8.73	91.27	79.. 19	7.19	92.81	88.. 28	4.73	95.26
62.. 2	24.13	75.87	71.. 11	8.46	91.54	80.. 20	6.90	93.10	89.. 29	4.48	95.52
63.. 3	19.52	80.48	72.. 12	8.25	91.75	81.. 21	6.55	93.45	90.. 30	4.13	95.87
64.. 4	16.19	83.81	73.. 13	8.05	91.95	82.. 22	6.16	03.84	91.. 31	3.63	96.37
65.. 5	14.28	85.72	74.. 14	7.88	92.12	83.. 23	5.81	94.19	92.. 32	3.10	96.90
66.. 6	12.37	87.63	75.. 15	7.75	92.25	84.. 24	5.56	94.44	93.. 33	2.48	97.52
67.. 7	10.92	89.08	76.. 16	7.68	92.32	85.. 25	5.32	94.68	94.. 34	1.94	98.06
68.. 8	9.82	90.18	77.. 17	7.59	92.41	86.. 26	5.11	94.89	95.. 35	1.40	98.60
69.. 9	9.12	90.88	78.. 18	7.43	92.57	87.. 27	4.90	95.10	96.. 36	0.95	99.05

Seventy Years Difference.

71.. 1	30.49	69.51	78.. 8	5.53	94.47	85.. 15	3.68	96.32	92.. 22	2.63	97.37
72.. 2	19.45	80.55	79.. 9	4.84	95.16	86.. 16	3.70	96.30	93.. 23	2.10	97.90
73.. 3	14.87	85.13	80.. 10	4.44	95.56	87.. 17	3.73	96.27	94.. 24	1.64	98.36
74.. 4	11.59	88.41	81.. 11	4.18	95.82	88.. 18	3.75	96.23	95.. 25	1.18	98.82
75.. 5	9.80	90.20	82.. 12	3.97	96.03	89.. 19	3.67	96.33	96.. 26	0.80	99.20
76.. 6	7.98	92.02	83.. 13	3.80	96.20	90.. 20	3.45	96.55			
77.. 7	6.60	93.40	84.. 14	3.72	96.28	91.. 21	3.09	96.91			

Eighty Years Difference.

81.. 1	24.95	75.05	85.. 5	6.34	93.66	89.. 9	2.50	97.50	93.. 13	1.25	98.75
82.. 2	14.37	85.63	86.. 6	4.92	95.08	90.. 10	2.16	97.84	94.. 14	0.97	99.03
83.. 3	10.34	89.66	87.. 7	3.85	96.15	91.. 11	1.86	98.14	95.. 15	0.70	99.30
84.. 4	7.63	92.37	88.. 8	3.04	96.96	92.. 12	1.57	98.43	96.. 16	0.49	99.51

Ninety Years Difference.

Ages.	Probabilities.		Ages.	Probabilities.	
91.. 1	18.93	81.07	94.. 4	3.12	96.88
92.. 2	9.18	90.82	95.. 5	2.12	97.88
93.. 3	5.53	94.46	96.. 6	1.15	98.58

PROB. 2. Supposing the ages of A and B to be given; to determine, from any table of observations, the present value of the sum s payable on the contingency of one life's surviving the other.

Solution.—Let r denote 1*l.* increased by its interest for a year, and let all the other symbols be the same as in the preceding problem. Let the life of B also be supposed to be the older of the 2 lives; and then it will follow, by reasoning as in the solution of that problem, that the present value of s to be received on the death of A, should that happen in the life time of B, will be expressed by the series $s \times (\frac{b+c}{2abr} a' + \frac{c+d}{2abr^2} a'' + \frac{e+e}{2abr^3} a''' + \frac{c+f}{2abr^4} a''''$ &c.) This series may be resolved into the 2 following; $\frac{s}{2} \times (\frac{ca'}{abr} + \frac{da''}{abr^2} + \frac{ea'''}{abr^3} + \frac{fa''''}{abr^4}$ &c.) $+ \frac{s}{2} \times$ $(\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^2} + \frac{fa'''}{abr^3}$ &c.) The first of these 2 series may be again resolved into $\frac{s}{2} \times (\frac{c}{br} - \frac{ca - ca'}{abr} + \frac{d}{br^2} - \frac{da - da' - da''}{abr^2} + \frac{e}{br^3} - \frac{ea - ea' - ea'' - ea'''}{abr^3}$ &c.) —

$s \times \frac{c}{2br} \times (\frac{d}{cr} - \frac{da - da'}{acr} + \frac{e}{cr^2} - \frac{ea - ea' - ea''}{acr^2}$ &c.) Let B denote the value of an annuity on the life of B, C the value of an annuity on a life 1 year older than B, AB and AC the values of annuities on the joint lives of A and B and of A and C, and these series will be $= \frac{s \times (B - AB)}{2} - \frac{s \times c \times (C - AC)}{2br}$. Again, the 2d series above mentioned, or $\frac{s}{2} \times (\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3}$&c.), by pursuing the same steps, may be found $= \frac{\beta \times s}{2b} \times (K - AK) - \frac{s\,(B - AB)}{2r}$, where β denotes the number of persons living at the age of a person 1 year younger than B, K the value of an annuity on that life, and AK the value of an annuity on the joint lives of A and K. The whole value of the survivorship is therefore $= s \times (\frac{r-1) \cdot (B - AB)}{2r} + \frac{\beta \cdot (K - AK)}{2b} - \frac{c \cdot (A - AC)}{2br}$. Q. E. D.

Having now the value of the sum s payable on the contingency of B's surviving A, the value of the same sum, payable on the contingency of A's surviving B, is easily obtained by the well known method of subtracting the value found above from the whole value of the reversion after the extinction of the joint lives of A and B. It is evident that the exactness of the above rule must depend on the accuracy with which the values of the single and joint lives are computed. Being possessed of such tables for all ages, even with respect to the joint lives, I have computed the following values, that it may be seen how far Mr. Simpson's approximation*, the only rule now in use, may be depended on.

Age of B.	Age of A.	† Value of 100*l.* payable on the death of A if B survives him. True value.	Simpson's value.	Age of B.	Age of A.	Value of 100*l.* payable on the death of A if B survives him. True value	Simpson's value.	Age of B.	Age of A.	Value of 100*l.* payable on the death of A if B survives him. True value.	Simpson's value.
10	2	32.67	26.05	40	10	17.10	19.07	60	60	38.88	38.92
10	10	24.74	24.75	40	40	32.86	32.87	70	2	18.36	9.81
20	2	29.99	24.73	50	2	23.36	17.06	70	10	7.07	9.15
20	10	22.11	23.50	50	10	14.10	16.21	70	40	15.35	15.78
20	20	27.95	27.96	50	20	18.65	19.29	70	70	42.34	42.33
30	2	28.79	22.60	50	50	35.80	35.85	80	2	14.46	5.71
30	10	19.84	21.47	60	2	21.52	13.61	80	10	3.93	5.43
30	30	30.22	30.21	60	10	10.65	12.93	80	50	12.05	12.00
40	2	26.65	20.07	60	30	17.51	18.19	80	80	45.47	45.45

* It must be remembered, that the correction explained by Dr. Price, in vol. i. p. 39, &c. of his Treatise on Reversionary Payments, must be applied to Mr. Simpson's rule; that is, when the reversion is a sum and not an estate, the value found by the rule must be divided by 1*l.* increased by its interest for a year.

† These values have been computed at 3 per cent. and from the Northampton Table of Observations.

From this table it appears that Mr. Simpson's approximation in the middle stages of life is sufficiently accurate; but that it is exceedingly defective when the life of A is very young. It should also be remembered, that these values have been computed at a low rate of interest, and from the Northampton Table of Observations, in which the decrements of life come nearer to M. de Moivre's hypothesis than in any other table. But if the computations be made at a higher rate of interest even from this table, the approximation does not always agree so well, as will appear from the following specimens calculated at 5 per cent.

Age of B.	Age of A.	Value of 100*l.* payable on the death of A if B survives him, by the Northampton table at 5 per cent. True value.	Simpson's value.
20	2	25.09	18.46
20	10	15.49	17.54
40	2	23.57	15.97
60	2	19.83	11.75
60	40	18.73	19.61

Value of 100*l.* payable on the death of A if B survives him, according to the Sweden Table of Observations, and at 4 per cent.							
Age of B.	Age of A.	True value.	Simpson's value.	Age of B.	Age of A.	True value.	Simpson's value.
20	2	21.47	16.80	60	36	12.29	16.81
20	14	15.42	17.82	60	42	16.11	19.58
20	20	20.01	19.84	60	60	36.96	36.34
40	4	23.53	14.22	76	40	9.21	9.81
40	16	13.71	16.23	76	52	12.58	14.00
40	28	17.60	20.44	76	64	23.81	22.81
40	40	27.62	27.00	76	76	42.90	43.29
60	24	9.39	13.01				

In order further to compare Mr. Simpson's approximation with the true value, I have inserted in the foregoing table a few computations deduced from the Sweden Table of Observations, in which the decrements of life are unequal. From these instances the approximation appears to be more defective in proportion as the probabilities of life differ from the hypothesis.

PROB. 3.—The ages of A and B being given; to determine the value of the sum S, payable on the extinction of one life in particular, should that happen after the extinction of the other life.

Solution.—Supposing B to be the older of the 2 lives, and the sum S to become payable on his decease; it is evident that this payment at the end of the first year must depend on the contingency of both lives being extinct before this period and of B's dying last. Retaining the same symbols, and reasoning as in the solution of the first problem, this value will be expressed by the fraction $\frac{S \cdot a' \cdot (b - c)}{2abr}$. The payment of the sum S at the end of the 2d year will depend on either of 2 events happening. First, that A and B both die in the 2d year after having survived the first, restrained, as above, to the contingency of B's having died last; 2dly, that B dies in the 2d year and A in the 1st year. The value therefore of s for this year will be expressed by the 2 fractions $\frac{s \cdot a'' \cdot (c-d)}{2abr^2} + \frac{s \cdot (c-d) \cdot a'}{abr^2}$. Again, the payment of s in the 3d year will depend either on A and B's both dying in that year, and B having died last; or on B's dying in that year, and A's dying in the 1st or 2d years. The value therefore of s for this year will be $= \frac{s \cdot a''' \cdot (a-e)}{2abr^3} + \frac{s \cdot (d-e) \cdot (a'+a'')}{abr^3}$. By proceeding in this manner for the other years the whole value of the reversion will be found $= \frac{s}{2} \times \frac{a' \cdot (b-c)}{abr} + \frac{a'' \cdot (c-d)}{abr^2} + \frac{a''' \cdot (d-e)}{abr^3} + \frac{a'''' \cdot (e-f)}{abr^4} + \&c.) + s \times (\frac{a' \cdot (c-d)}{abr^2} + \frac{(a'+a'') \cdot (d-e)}{abr^3} + \frac{(a'+a''+a''') \cdot (e-f)}{abr^4} + \&c.$ The first of these series, by proceeding in the same manner as in the solution of the 2d problem, may be found $= \frac{\beta}{2b} \times (K - AK) - \frac{B - AB}{2r} - \frac{1}{2} \cdot (B - AB) + \frac{C}{2br} \times (C - AC)$; and the 2d series may be found $= - \frac{C}{br} \times (C - AC) + \frac{B - AB}{r}$. Hence the whole value of the reversion will be $= s \times (\frac{\beta r \cdot (K - AK) - C \cdot (C - AC)}{2br} - \frac{(r - 1 \cdot (B - AB)}{2r}$.

Q. E. D.

Having now the value of the sum s depending on the older of the 2 lives dying last, the value of the same sum depending on the younger of the 2 lives dying last is easily obtained, by subtracting the value first found from the whole value of the reversion after the extinction of both lives. The answers computed by this rule differ rather more from those computed by Mr. Simpson's approximation than they do in the preceding problem.

XXI. Of a Remarkable Transposition of the Viscera. By Matthew Baillie, M. D. p. 350.

Nothing tends more to illustrate the powers and the wisdom of nature than the investigation of the structure of animals. We there find a most wonderful delicacy of mechanism, and exquisitely adapted to a variety of purposes. This however is not to be better seen by following nature in her common track than by observing her wanderings. In these she often shows more particularly the extent of her powers, and throws light on her ordinary plans. Such circumstances give importance and value to the observation of singular phenomena. The variety in animal structure, an account of which is presented in this account, is a complete transposition in the human subject, of the thoracic and abdominal viscera, to the opposite side from what is natural. It is so extraordinary as scarcely to have been seen by any of the most celebrated anatomists, and indeed has been but very generally noticed at all. The circumstance has been mentioned, but it has not been particularly described so as to make it thoroughly known, or to establish its certainty. It was hanging in the minds of many as doubtful, whether such a variety did really exist. There is one circumstance that attends the account of the present case, which has not always happened in the record of singular phenomena, viz. that it has been examined by physicians and surgeons of the first reputation in this large town, and has been in some measure open to the gratification of public curiosity.

The person who is the subject of this paper was a male, nearly 40 years of age, somewhat above the middle stature, and of a clean active shape. He was brought for dissection in the common way to Windmill-street. On opening the cavity of the thorax and abdomen, the different situation of the viscera was so striking as immediately to excite the attention of the pupils who were engaged in dissecting it. I began immediately to examine every part of the change with considerable attention: for this purpose, after desiring a drawing to be made of the appearances as they were found on opening the body, I next day injected it.

The mediastinum, or anterior duplicature of the pleura, separating the 2 cavities of the chest from each other, was found to incline obliquely downwards to the right side fully as much as it does commonly to the left side of the chest. The pericardium too inclined obliquely to the right side. On pressing it gently away from the lungs the phrenic nerves came distinctly into view, in their common situation; but the right phrenic nerve ran more obliquely, and was longer than the left. The lung on the right side was divided by a single oblique fissure into 2 lobes, having at the same time a deficiency opposite to the apex of the heart; and the lung on the left side was divided into 3 lobes, exactly contrary to what is found in ordinary cases.

On opening the pericardium the apex of the heart was found to point to the right side nearly opposite to the 6th rib, and its cavities as well as large vessels were completely transposed. What are commonly called the right auricle and ventricle were situated on the left side, and the left auricle and ventricle on the right. The pulmonary artery ascended towards the right side of the chest. The aorta was also directing its arch to the right; and the vena cava superior, as well as inferior, were seen opening into their auricle on the left side of the spine. There was nothing remarkable in the size or general figure of the heart. On the outside of the pericardium the transposition of the larger vessels was very striking. The longer subclavian vein was passing from the left side obliquely to the right before the branches which are sent off from the arch of the aorta. The left carotid and subclavian arteries were found to arise from the arch of the aorta by one common trunk; the right carotid and subclavian separately.

In the duplicature of the pleura behind, or what may be called the posterior mediastinum, there was a change corresponding to what we have already described. The descending aorta was found passing on the right side of the spine. The œsophagus was before it, inclining more and more to the right towards its lower extremity, and it at length perforated the diaphragm somewhat on the right side of the spine.* The thoracic duct was seen in the middle between the descending aorta and vena azygos, in some places forming a plexus of small branches, in another dividing itself into 2 branches, which afterwards re-united in a common trunk, and at length climbing up to terminate in the angle between the jugular and subclavian veins on the right side of the body. The recurrent nerve of the parvagum on the right side passed round the beginning of the descending aorta, and on the left passed round the common trunk ot the carotid and subclavian arteries. The large intercostal nerves being exactly under the same circumstances on each side, it was impossible there could be any transposition in them. It appears then from the foregoing description, that every thing admitting of such a change was completely transposed in the thorax.

The liver was situated in the left hypochondriac region, the small lobe being towards the right, and the great lobe in the left side. The ligaments uniting it to the diaphragm corresponded to this change, the right transverse ligament being longer, and the left being shorter, than usual. The suspensory ligament could undergo little change, except being pushed to the left side along with the liver. On pressing upwards the liver, so as to exhibit its posterior and under surface, the gall bladder was seen on the left side preserving its proper relative situation to the great lobe of the liver, and the vessels of the portæ were found on dissection to be transposed corresponding to the change of circumstances. The hepatic artery was found climbing up obliquely from the right towards the left, before the lobulus spigelii, and entered at the portæ into the substance of the liver by two or three branches on the right of the other vessels. The ductus communis cholidochus was on the left of the other vessels, being formed from the ductus hepaticus and ductus cysticus in the common way, and it passed obliquely downwards on the left, to terminate in the duodenum. What was most remarkable, it terminated in the fore part of the duodenum. The vena portarum passed behind the hepatic artery and ductus communis cholidochus, ascending obliquely towards the left side.

The spleen was situated in the right hypochondriac region, adhering to the diaphragm in the common way. There were 3 spleens, nearly of the size of a pullet's egg, found adhering to the larger spleen by short adhesions, besides 2 other still smaller spleens which were involved in the epiploon at the great end of the stomach. The pancreas was found on the right side behind the stomach, running obliquely from the spleen to the curvature of the duodenum, and had its duct entering in common with the ductus communis cholidochus into the

* The vena azygos was on the left side of the spine opening in the common way into the vena cava superior, which we formerly mentioned to be also transposed in its situation.—Orig.

cavity of that intestine. The splenic vessels were passing along the upper edge of the pancreas to the right side, corresponding to the change of situation in the pancreas and spleen.

The stomach was situated on the right side, partly hid by the small lobe of the liver passing to the left, and terminating in the pylorus, rather on the left side of the spine. The duodenum took a most singular course; it first passed to the right side, behind the small end of the stomach; it then turned on itself, towards the left side; it afterwards took its proper sweep to the right side, passing behind the superior mesenteric artery and mesaraica major vein. The mesentery began to be formed on the right side, instead of the left, as in ordinary cases. The ilium terminated in the great intestine on the left side, and there was in it a diverticulum of considerable size, a lusus not unfrequently occurring. The cæcum was situated on the left psoas magnus and iliacus internus muscles. The transverse arch of the colon passed from the left to the right side of the body, and the sigmoid flexure crossed over the right psoas, to get into the cavity of the pelvis. The kidneys had their vessels transposed; the renal capsules had undergone no change, as no variety could be produced by a transposition.

The aorta passed between the crura of the diaphragm into the cavity of the abdomen, and adhered in its course to the spine on the right side of the vena cava inferior. Its branches were directed in their course corresponding to the peculiar situation of the viscera. The splenic and coronary arteries were passing to the right side, and the hepatic artery obliquely to the left. The superior and inferior mesenteric arteries were directed to the right side. There was no change in the spermatic arteries, any transposition in the testicles, if such a thing could take place, not being capable of affecting them. The lumbar arteries could also undergo little change, except that the left lumbar arteries must necessarily, from the peculiar situation of the aorta, be the longest. The vena cava inferior perforated the tendinous portion of the diaphragm, and adhered in its course to the spine on the left side of the aorta.

The right emulgent vein was much longer than usual, passing from the right kidney before the aorta to terminate in the vena cava superior; and the left emulgent much shorter, passing from the left kidney to the vena cava, which was situated on the left side of the spine. The right spermatic vein was found to open into the right emulgent, and the left into the vena cava inferior, about an inch under the left emulgent. The vena portarum was changed from its natural course, passing obliquely upwards to the left side, and its large branches, viz. the vena splenica, mesaraica major and minor, were all directed towards the right side of the spine. There was no change in the intercostal nerve within the cavity of the abdomen; nor does it seem to be capable of being affected by any transposition of parts. We see then, that there was a complete transposition of the abdominal viscera, each of them preserving its proper relative situation to the others. In the brain, organs of sense, of generation, the muscles, and blood vessels of the extremities, was found nothing remarkable.

The person seems to have used his right hand in preference to his left, as is usually the case, which was readily discovered by the greater bulk and hardness of that hand, as well as the greater fleshiness of the arm. It was not indeed to be expected he should be left handed. The person, while alive, was not conscious of any uncommon situation of his heart; and his brother has his heart pointing to the left side as in ordinary cases. Indeed, there was little reason to expect that we should meet with any thing particular in the account of his life. His health could not be affected by such a change of situation in his viscera; nor could there arise from it any peculiar symptoms of disease. Still less could there be any connection between such a change and his dispositions, or external actions. He might have known that his heart was directed towards the right side; but if we consider how little every person, especially those of the lower class, are attentive to circumstances not very palpable, it was scarcely to be expected he should know of it.

Notwithstanding the general similarity of parts in the same species of animals, there is no reason why nature should not sometimes deviate from her ordinary plans. Accordingly we find there is much variety in animal structure; but this does not commonly affect the animal functions. Under this restriction the variety is so great in the appearances of every part of an animal, that it is almost impossible to examine any 2 animals of the same species without remarking many differences. In the bony compages of an animal we find little variety in the extremities of bones where there is the apparatus of a joint, because a particular shape is best adapted to a particular kind or latitude of motion. In other parts of the bones, where a difference of features is not material, there is great variety, as in the foramina, depressions, ridges, and sutures of bones. The same general rule will apply to variety in muscles. The principal object is a certain insertion near a joint, so as to give a determined direction of motion. With respect to such insertions, there is, comparatively speaking, little variety; but there is a great difference in the bodies and connections of muscles, which have no share in the regulation of the motion.

There is no part of an animal where there is a greater latitude of variety than in the distribution of blood vessels. The reason of it is very obvious. The only object in the distribution of blood vessels is, to carry blood to every part of the body, and bring it back to the heart. The parts of an animal, in order to be supported, must be visited by successive changes of fresh blood; but it surely cannot be an object of importance whether the blood passes by one rout or another. Hence the variety of blood vessels is extremely great. Still however there is a method in the deviations of nature, so that they may be marked or noted, the same varieties occurring in different animals.

It cannot be at all important to the function of a viscus, whether it be in one mass, or in separate portions. The structure being the same, the same action will take place. Hence we often find the 2 kidneys joined together, forming one mass; and not unfrequently 2 or 3 spleens, besides the common one. Neither can it be important whether a viscus should always be of the same shape, because its functions do not depend on shape, but on structure: we find accordingly, in this particular, much variety.

There are many of the viscera which are connected together in their functions, or by the junction of large blood vessels, in such a way as to require nearly the same relative situation among themselves. This becomes also necessary in order to preserve the general shape of the animal. Accordingly we find, that when any important viscus is changed in its situation, it affects the situation of other viscera, requiring in them a similar change. We saw in the person who is the subject of this paper, that a change in the situation of the heart and liver was accompanied with a change of situation in the stomach, spleen, pancreas, and in short the whole abdominal viscera. This however is a great deviation in nature; for it is nothing less than changing almost the whole vital system in an animal, and therefore it rarely happens. In such a change it does not appear that the functions can be affected, as they depend on structure and situation, which are both preserved. Hence the person who is the subject of this paper arrived at the age of maturity, and might have continued to live to an extreme old age. The human machine might have been constructed in this way generally, and under such circumstances, what is now called the natural situation of parts would have been as singular as the present phænomenon.

There appears to be less variety in the nervous system of animals of the same species, than in most parts of the body. There is scarcely any difference in the appearance of the brain, and much less in the distribution of the nerves than of the blood vessels. There is also little variety in the organs of sense: perhaps the mechanism in both these is nicer, so that a considerable deviation would interfere with their peculiar functions. The most common great deviations which nature produces in the structure of an animal, are various kinds of monstrosity, by which the animal becomes often unfit for continuing its existence. Why nature should in its greater deviations fall into a very imperfect formation, much below the standard of her common work, does not appear very obvious. It seems that there might have been many varieties where the functions could have been preserved. Perhaps it is with a view to check the propagation of great varieties, so as to preserve a uniformity in the same species of animals.

It has been much agitated, whether monstrosities depend on the original formation, or are produced afterwards in the gradual evolution of an animal. This does not appear to be a question of much importance; nor perhaps can it be absolutely determined. But on the whole it is more reasonable to think that the same plan of formation is continued from the beginning, than that at any subsequent period there is a change in that plan. It may be observed, that it is exactly the same creative action which produces the natural structure, or any deviation from it; for in cases of deviation the action is either carried too far, ceases too soon, or is diverted into uncommon channels. This will explain the various kinds of monstrosity from redundancy, deficiency, or transposition of parts.

XXII. On the Georgian Planet and its Satellites. By Wm. Herschel, LL. D., F. R. S. p. 364.

In a paper, containing an account of the discovery of 2 satellites revolving round the Georgian planet, Dr. H. gave the periodical times of these satellites in a general way, and added that their orbits made a considerable angle with the ecliptic. The most convenient way of determining the revolution of a satellite round its primary planet, which is that of observing its eclipses, cannot now be used with the Georgian satellites; and as to taking their situations in many successive oppositions of the planet, which is also another very eligible method, that must of course remain to be done at proper opportunities. The only way then left, was to take the situations of these satellites, in any place where they could be ascertained with some degree of precision, and to reduce them afterwards by computation to such other situations as were required for the purpose. In Jan. Feb. and March, 1787, the positions were determined by causing the planet to pass along a wire, and estimating the angle a satellite made with this wire, by a high magnifying power. But then he could only use such of these situations where the satellite happened to be either directly in the parallel of declination, or in the meridian of the planet; or where, at least, it did not deviate above a few degrees from either of them; as it would not have been safe to trust to more distant estimations.

In computing the periods of the satellites Dr. H. contented himself with synodical appearances, as the position of their orbits, at the time when the situations were taken from which these periods are deduced, was not sufficiently known to attempt a very accurate sidereal calculation. By 6 combinations of positions at a distance of 7, 8, and 9 months of time, it appears that the first satellite performs a synodical revolution round its primary planet in $8^d\ 17^h\ 1^m$ and 19.3^s. The period of the 2d satellite deduced likewise from 4 such combinations, at the same distance of time, is $13^d\ 11^h\ 5^m$ and 1.5^s. The combinations of which the above quantities are a mean do not differ much among themselves; it may therefore be expected that these periods will come very near the truth; and indeed Dr. H. for many months after used to calculate the places of the satellites by them, and always found them in the situations where these computations gave reason to expect to see them. The epochæ, from which astronomers may calculate the positions of these satellites, are Oct. 19, 1787; for the first $19^h\ 11^m\ 28^s$; and for the 2d $17^h\ 22^m\ 40^s$. They were at those times $76^\circ\ 43'$ north following the planet; which is the place of the greatest elongation of the 2d satellite: where consequently its real angular situation is the same as the apparent one.

The next thing to be determined in the elements of these satellites, is their distance from the planet; and as we know that, when the periodical times are given, it is sufficient to have the distance of one satellite, in order to find that of any other, he confined his attention to the discovery of the distance of the 2d. As soon as he attempted measures, it appeared that the orbit of this satellite was seemingly elliptical; it became therefore necessary, in order to ascertain its greatest elongation, to repeat these measures in all convenient situations; the result of which was, that on the 18th of March, at $8^h\ 2^m\ 50^s$, he found the satellite at the distance of $46''.46$; this being the largest of all the measures he had an opportunity of taking. Hence by computation it appears, that the satellite's greatest visible elongation from its planet, at the mean distance of the Georgium Sidus from the earth, will be $44''.23$. Admitting therefore at present, that the satellite moves in a circular orbit about its planet, we cannot be much out in taking the calculated quantity of $44''.23$ for the true measure of its distance. And, having ascertained this point, we calculate, by the law of Kepler, and the assigned period of the first satellite, that its distance from the planet must be $33''.09$.

As we are now on the subject of such parts of the theory of planets as may be determined by calculation, it will not be amiss to see how the quantity of matter and density of our new planet will stand, when compared with the tables that have been given of the same in the other planets; and in order to this, let us admit the following data as a foundation for our computation, viz. The parallax of the sun $8''.63$. The parallax of the moon $57'\ 11''$.

Its sidereal revolution round the earth $27^d\ 7^h\ 43^m\ 11^s.6$.

The mean distance of the Georgian planet from the sun 19.0818.

The mean distance of its 2d satellite from the planet $44''.23$.

The periodical time of this satellite $13^d\ 11^h\ 5^m\ 1^s.5$.

Hence we find that a spectator, removed to the mean distance of the Georgian planet from the earth, would see the radius of the moon's orbit under an angle of $27''.1866$; and if 1, d, t, represent the quantity of matter in the earth, the distance of the moon, and its periodical time; also M, D, T, be made to stand for the same things in our new planet and its 2d satellite, we obtain, by known principles, $M = \frac{t^2 D^3}{T^2 d^3}$. And consequently the quantity of matter in the Georgian planet, is to that contained in the earth, as 17.740612 to 1.

In order to calculate the density, Dr. H. compares the mean of the 4 bright measures of the planet's diameter 3″.7975 to the mean of the 2 dark ones 4″.295; as they are given in his paper on the diameter and magnitude of the Georgium Sidus, in vol. 73 of the Philos. Trans. Whence we obtain another mean diameter 4″.04625; which is probably the most accurate of any yet ascertained. Let us now suppose this measure to belong to the situations of the earth and of the new planet as they were at 10 o'clock, Oct. 25, 1782; which is about the middle of the several times when those measures from which this is deduced were taken. Then by the tables we compute the distance of the two planets from the sun and the angle of commutation; whence, by trigonometry, we find the distance of our new planet from the earth for the supposed 25th of October; and thence deduce its mean diameter, which is 3″.90554. This, when brought to what it would appear if it were seen from the sun at the earth's mean distance, gives 1′ 14″.5246; which, compared with 17″.26, the earth's mean diameter, is as 4.31769 to 1. The Georgium Sidus therefore, in bulk, is 80.49256 times as large as the earth; and consequently its density less than that of the latter in the ratio of .220401 to 1. Also the force of gravity, on this planet's surface, is such as will cause a heavy body to fall through 15$\frac{2}{3}$ feet in one second of time.

It remains now only, in order to complete our general idea of the Georgian planet, to investigate the situation of the orbits of its satellites. It has before been remarked, that when Dr. H. came to examine the distance of the 2d, he perceived immediately that its orbit appeared considerably elliptical. This induced him to attempt as many measures as possible, that he might be enabled to come at the proportion of the axes of the apparent ellipsis; and thence argue its situation. But here he met with difficulties that were indeed almost insurmountable. The uncommon faintness of the satellites; the smallness of the angles to be measured with micrometers which required light enough to see the wires; the unwieldy size of the instrument, which, though very manageable, still demanded assistant hands for its movements, and consequently took away a great share of his own directing power, a thing so necessary in delicate observations; the high magnifiers he was obliged to use, by way of rendering the spaces and angles to be measured more conspicuous; in short, every circumstance seemed to conspire to make the case a desperate one. Add to this, that no measure could possibly succeed which had not the most beautiful sky in its favour; and we may easily judge how scarce the opportunities of taking such measures must be in the variable climate of this island. As far then as a small number of select measures will permit, which, out of about 21 that were taken, amounts only to 5, he enters on the subject of the position of the 2d satellite's orbit.

The following table contains in the first column the correct mean time when the measures were taken. The 2d gives the quantity of these measures. In the 3d column are the same measures reduced to the mean distance of the Georgian planet from the earth. The 4th contains the calculated positions of the satellite as it would have appeared to be situated if it had moved in a circular orbit at rect-angles to the visual ray; and the degrees are numbered from the first observation supposed to have been at zero, and are carried round the circle from right to left.

March	18^d	8^h	2^m	50^s	46″.46	44″.23	0°	0′
	19	7	47	59	44 .24	42 .15	26	28
	20	7	44	8	40 .23	38 .37	53	8
April	11	9	18	27	35 .32	34 .35	283	13
Nov.	9	15	56	15	44 .89	42 .88	199	59

In the use of this table Dr. H. partly contents himself with the construction of a figure, and only applies calculation to the most material circumstances. And from the whole calculations is inferred the following snmmary of results.

The first satellite revolves round the Georgian planet in $8^d\ 17^h\ 1^m\ 19^s$.—Its distance is 33″.—And on Oct. 19, 1787, at $19^h\ 11^m\ 28^s$, its position was 76° 43′ north following the planet.

The 2d satellite revolves round its primary planet in $13^d\ 11^h\ 5^m\ 1.5^s$.—Its greatest distance is 44″.23.—And on Oct. 19, 1787, its position at $17^h\ 22^m\ 40^s$, was 76° 43′ north following the planet. Last year its least distance was 34″.35; but the orbit is so inclined, that this measure will change very considerably in a few years, and by that alteration we shall know which of the double quantities set down for the inclination and node of its orbit are to be used.

The orbit of the 2d satellite is inclined to the ecliptic $\left\{\begin{matrix}91° & 1' & 32''.2\\ 89 & 48 & 27.5\end{matrix}\right\}$; its ascending node is in $\left\{\begin{matrix}18° \text{ of Virgo}\\ 6 \text{ of Sagittarius}\end{matrix}\right\}$. When the planet passes the meridian, being in the node of this satellite, the northern part of its orbit will be turned towards the $\left\{\begin{matrix}\text{east}\\ \text{west}\end{matrix}\right\}$. The situation of the orbit of the first satellite does not seem to differ materially from that of the 2d. We shall have eclipses of these satellites about the year $\left\{\begin{matrix}1799\\ 1818\end{matrix}\right\}$, when they will appear to ascend through the shadow of the planet almost in a perpendicular direction to the ecliptic.

The satellites of the Georgian planet are probably not less than those of Jupiter.

The diameter of the new planet is 34217 miles.

The same diameter seen from the earth, at its mean distance, is 3″.90554.

From the sun, at the mean distance of the earth, 1′ 14″.5246.

Compared to that of the earth as 4.31769 to 1.

This planet in bulk is 80.49256 times as large as the earth.
Its density as .220401 to 1.
Its quantity of matter 17.740612 to 1.
And heavy bodies fall on its surface 15 feet $3\frac{1}{3}$ inches in 1 second of time.

XXIII. Experiments on the Formation of Volatile Alkali, and on the Affinities of the Phlogisticated and light Inflammable Airs. By William Austin, M. D. p. 379.

In the former part of the year 1787 Dr. A. undertook to examine the elastic fluid produced on decomposing volatile alkali by the electric stroke, as first suggested by Dr. Priestley. Some alkaline air being thus decomposed, and all its inflammable part separated by combustion in glass vessels inverted in quicksilver, he observed a considerable remainder of phlogisticated air; and after many accurate experiments was fully convinced, that this phlogisticated air had made a part in the constitution of the alkali. This discovery induced him to make a variety of synthetical experiments on the phlogisticated and light inflammable airs, with the hopes of forming volatile alkali from its simple elements.

First, he endeavoured to combine the phlogisticated and light inflammable airs, by mixing them together in various proportions in their elastic state, and adding to them such substances as he thought likely to promote their uniting and forming an alkali. With this view, he threw up to the mixture of these airs, marine acid air, the marine and vitriolic acids, to which he also joined alkaline air. He tried the effect of cold on these mixtures, by applying to the tubes containing them clothes moistened with ether. He even passed the electric spark repeatedly through them, though with little probability of success. Lastly, he decomposed alkaline air, and tried to re-unite the identical parts which formed it by similar additions; but he could not perceive, that in any instance, volatile alkali was produced from its 2 constituent parts mixed together in their simple aëriform state.

Yet it is well known, that these 2 bodies unite very readily, when they are not in an elastic state. An unexpected appearance of volatile alkali had been observed by Dr. Priestley and Mr. Kirwan before we were acquainted with its constitution, and by M. Haussman since this discovery of M. Berthollet. An experiment was exhibited before several gentlemen at Sir Joseph Banks's house, some years ago, in which the quantity of volatile alkali produced is very remarkable. In this experiment a few ounces of powdered tin are moistened with some moderately strong nitrous acid, and after they have stood together a minute or two, about $\frac{1}{2}$ an ounce of fixed alkali is mixed with them. A very pungent smell of volatile alkali is immediately perceived. The experiment succeeds equally, if lime be used instead of fixed alkali. Any person, who moistens a drachm or 2 of filings of zinc with a solution of cupreous nitre; and after they begin to act on each other adds to them a little salt of tartar, will find volatile alkali to be produced. Nitrous acid, or cupreous nitre, mixed with iron filings, sulphur, and a little water, and kept in a close vessel for some hours, yields a smell of volatile alkali; and if a piece of paper, stained with a vegetable blue substance, be thrown into the vessel, it will soon be turned to a green colour. In each of these experiments the nitrous acid and the water are decomposed. Dephlogisticated air from each of them combines with the metal, and their other constituent parts, the phlogisticated air of the acid, and inflammable air of the water, being disengaged at the same instant, unite and form volatile alkali. Many other similar experiments might be mentioned; but these are abundantly sufficient to prove, that if phlogisticated and light inflammable air be presented to each other at the instant of their separation from solid or liquid substances, and before their particles have receded from each other, they readily combine and generate volatile alkali.

That these two substances do not combine in their elastic state, seems to be owing principally to the inflammable air. When these 2 airs combine, it seems necessary that they part with a certain quantity of that fire to which they owe their elasticity; and that, unless their attraction to each other exceed their attraction to fire, they will not unite. Even when they are combined in the form of volatile alkali, if heat be applied, they immediately recede from each other, and the alkali is decomposed. When they are not in an aëriform state their attraction to each other is greater, on account of the proximity of their parts; it is then superior to their attraction to fire, and therefore they combine; but when their particles have receded from each other, as in the aëriform state, their attraction to each other is so diminished by the distance of their parts, that their attraction to fire, which is uniform, prevails, and keeps them in a separate state. The specific gravity of inflammable air being 11 times less than that of phlogisticated air the distance of its particles must be greater than the distance of the particles of phlogisticated air, in the proportion of $\sqrt[3]{11}$ to 1, if the elementary particles of the 2 airs be of equal magnitude; and its effect, on this account, in diminishing attraction, must be greater than that of phlogisticated, in the proportion of those numbers, or more probably as the squares.

Into a cylindrical glass tube, filled with, and inverted in, quicksilver, Dr. A. introduced some phlogisticated air, and afterwards some iron filings moistened with distilled water. By this arrangement light inflammable air, which is given out from water in contact with iron filings, meeting with phlogisticated air at the instant of its extrication, combines with it, and forms volatile alkali. In order to detect the minute quantities of volatile alkali, which were thus generated, he fixed to the inside of the glass tube a small piece of paper, stained with the rind of the blue raddish. The vegetable blue was in 24 hours changed to a green colour. As an additional proof of the production of volatile alkali, he kept in the same tube some paper, which had been dipped in a solution of cupreous nitre, expecting to see its colour changed from green to blue, by the alkali which was to be produced. The green paper became gradually paler, and in a few days the blue colour appeared. This experiment affords a very satisfactory demonstration of the formation of volatile alkali. Water and iron filings mixed together yield inflammable air; but if this be given out in contact with phlogisticated air, volatile alkali is produced. In these circumstances a double attraction takes place: one part of the water is attracted by the iron; the other is attracted by the phlogisticated air; and the water seems by these compound affinities to be much more rapidly decomposed, than when iron and water are mixed by themselves.

Volatile alkali is formed in a very few hours, if nitrous air be used instead of the phlogisticated, all other circumstances remaining as in the former experiment. When Dr. A. used nitrous acid not well freed from its acid, by which the vegetable blue colour has been turned red, a sufficient quantity of alkali has been generated in 24 hours to change it to a green. If iron filings and water be exposed to nitrous air for a considerable time, the nitrous air is so altered that a candle burns in it with increased brightness, as was observed by Dr. Priestley. This change is accounted for by the formation of the alkali, which depriving the nitrous air of its phlogisticated part, leaves a greater proportion of dephlogisticated air.

This experiment also succeeds in atmospheric air, though a longer time is necessary to produce a sensible alteration in the colours employed as tests of the alkali; but the change is very evident in a day or two. Hence we may conclude, that whenever iron rusts in contact with water in the open air, or in the earth, volatile alkali is formed. Phlogisticated air is present in all parts of the terraqueous globe, and operations are constantly going on, by which inflammable air is separated from water, and perhaps from other bodies. Thus we may account for the frequent appearance of volatile alkali in the earth, particularly where inflammable matters abound, among coals and volcanic productions, as also in animal and vegetable substances.

When iron, water, and sulphur act on each other in atmospheric air, volatile alkali is produced. The eudiometer recommended by Scheele is, for this reason, incorrect. Some phlogisticated air disappears, and volatile alkali is formed. This method therefore seems to have misled that great chemist in his analysis of the atmosphere, and induced him to suppose, that the quantity of phlogisticated air in the atmosphere is only $2\frac{2}{3}$ times that of dephlogisticated air.

There is a combination of light inflammable air with sulphur forming hepatic air. It has been observed by the celebrated Mr. Kirwan, that if nitrous air be mixed with hepatic air, volatile alkali will be formed. Dr. A. often repe aed this experiment, and marked the formation of the volatile alkali by the change of the vegetable blue to a green colour. In hepatic air the parts of inflammable air are brought nearer to each other than they are in their simple aëriform state,* and therefore the phlogisticated air of the nitrous air combines with them, and generates volatile alkali.

From all these experiments it follows, that whether phlogisticated air be in a state of purity, or mixed with dephlogisticated air, as in the atmosphere, or combined with it as in nitrous air, it will in either case unite with the gravitating matter of light inflammable air, provided this substance be presented to it in a state of condensation; but if the circumstances be reversed, the same combination does not take place. No union is formed between inflammable air and the phlogisticated part of nitrous air, even though marine acid be added, which, by its attraction to dephlogisticated air, would contribute to decompose the nitrous air, and by its attraction to volatile alkali would tend to unite its constituent parts: or if to light inflammable air we add nitrous air and iron filings, no combination ensues; though it has been often observed that volatile alkali is readily generated, when nitrous air is presented to the inflammable at the instant of its extrication from water and iron.

The proportions of the phlogisticated and inflammable airs in volatile alkali, as discovered by calculation, approach very near to the result of M. Berthollet's experiments. If we take the specific gravities of these airs, given in Mr. Kirwan's late publication.

100 cubic inches contain 18.16 grains of alkaline air.
.... 30.535 .... of phlogisticated air.
.... 2.613 .... of inflammable air.

According to M. Berthollet alkaline air is expanded on decomposition from 1.7 to 3.3. Its specific gravity after decomposition must therefore be lessened in the same proportion; and 100 cubic inches will be found to contain only 9.355 grains of alkaline air thus expanded. In what proportion must the phlogisticated and inflammable airs be, in order to form a mixture of this specific gravity?

Let x represent the number of grains of phlogisticated air in 100 cubic inches of the mixture: then $9.355 - x$ will express the number of grains of inflammable air. As the weight of 1 cubic inch is to a cubic inch, so will the

* After these experiments were made, Dr. A. found that this is not the case. The electric spark decomposes hepatic air, and leaves a quantity of inflammable air equal in bulk to the hepatic air very nearly. However, as the inflammable air leaves the sulphur on the application of the electrical spark, it should seem that the proper matter of inflammable air is more disposed to combine with fire than with sulphur; which may be the reason why hepatic air is decomposed by nitrous air, while pure inflammable air is not affected by it.—Orig.

weight of either air in the mixture be to the cubic inches of that air in the mixture; and therefore .30535 the weight of a cubic inch of phlogisticated air, shall be to 1, as x is to $\frac{x}{.30535}$ which must be the number of cubic inches of phlogisticated air in 100 cubic inches of the mixture; and the weight of a cubic inch of inflammable air, that is, $.02613 : 1 :: 9.355 - x : \frac{9.355 - x}{.02613}$ the cubic inches of inflammable air in 100 cubic inches of the mixture. Thus we have an expression for the cubic inches of each air; these two quantities taken together are equal to 100 cubic inches by supposition; from which equation is found $x = 7.373$, the number of grains of phlogisticated air in 100 cubic inches, or in 9.355 grains of the mixture; and $9.355 - 7.373 = 1.982$, the grains of inflammable air. Now $7.373 : 1.982 :: 121 : 32$; and the quantity of phlogisticated air is to that of inflammable air, as 121 to 32.

According to M. Berthollet's experiments, the quantity of phlogisticated is to that of inflammable air, as 121 : 29. This is not very wide of calculation. If we consider the great difficulty of obtaining these specific gravities with exactness, we must be pleased to find so near a concurrence, and place more confidence in experiments on the specific gravities and combinations of aëriform bodies, than has generally been given them. M. Berthollet's experiments come within $\frac{1}{10}$ of calculation; and this difference will be diminished by $\frac{2}{3}$, if we take the specific gravities of the phlogisticated and inflammable airs in the proportion of 11 to 1, as he has done, instead of Mr. Kirwan's proportion, which Dr. A. followed in this calculation.

XXIV. Some Properties of the Sum of the Divisors of Numbers. By Edward Waring, M. D., F. R. S. p. 388.

1. Let the equation $x - 1 . x^2 - 1 . x^3 - 1 . x^4 - 1 . x^5 - 1 \ldots . x^n - 1 = x^h - px^{h-1} + qx^{h-2} - rx^{h-3} + sx^{h-4} - \&c. = x^h - x^{h-1} - x^{h-2} + x^{h-5} + x^{h-7} - x^{h-12} - x^{h-15} + x^{h-22} + x^{h-26} - x^{h-35} - x^{h-40} + x^{h-51} + x^{h-57} - \&c. \ldots x^{h-n} \pm \&c. = \mathrm{A} = 0$. The signs + and − proceed alternately by pairs unto the term x^{h-n}. The co-efficients of all the terms to the above-mentioned (x^{h-n}) will be + 1, − 1 or 0; they will be + 1 when multiplied into x^{h-v}, where $v = \frac{3z^2 + z}{2}$ or $= \frac{3z^2 - z}{2}$, and z an even number; but − 1, if z be an uneven number; in all other cases they will be = 0. The numbers 1, 2, 5, 7, 12, 15, 22, 26, 35, 40, &c. subtracted from h, may be collected from the addition of the numbers 1, 1, 3, 2, 5, 3, 7, 4, 9, 5, 11, 6, &c. which consist of two arithmetical serieses 1, 3, 5, 7, 9, 11, &c. 1, 2, 3, 4, 5, 6, 7, &c. intermixed.

2. The sum of any power (m) of each of the roots in the equation $\mathrm{A} = 0$ will

be s (m), where s (m) denotes the sum of all the divisors of the number m, if m be not greater than n.

Cor. Hence (by the rule for finding the sum of (m) powers of each of the roots from the sum of the inferior powers and co-efficients of the given equation) may be deduced s $(m) = p$s $(m-1) - q$s $(m-2) + r$s $(m-3) - s$s $(m-4) + t$s $(m-5) -$ &c. $=$ s $(m-1) +$ s $(m-2) -$ s $(m-5) -$ s $(m-7) +$ s $(m-12) +$ s $(m-15) -$ s $(m-22) - (m-26) +$ &c. which is the property of the sum of divisors invented by the late M. Euler.

Cor. By substituting for s $(m-1)$, s $(m-2)$, &c. their values s $(m-2) +$ s $(m-3) -$ s $(m-6) -$ s $(m-8) +$ &c., s $(m-3) +$ s $(m-4) -$ s $(m-7) -$ s $(m-9) +$ &c. &c. in the given equation s $(m) =$ s $(m-1) +$ s $(m-2) -$ s $(m-5) -$ s $(m-7) +$ &c. may be acquired an expression for the sum s (m) in terms of the sums of the divisors of numbers less than $m-1$, $m-2$, &c.: the same method may be used for a similar purpose in some of the following propositions.

Cor. By the rule for finding the sum of the contents of every (m) roots from the sums of the powers of each of the roots, may be deduced the equation $\pm$ $1.2.3.4\ldots m$, or $0 = 1 - m.\frac{m-1}{2}\text{s}(2) + m.m-1.\frac{m-2}{3}\text{s}(3)$

$$- m.m-1.m-2.\frac{m-3}{4}\text{s}\ (4) + \&\text{c.}$$
$$+ m.m-1.\frac{m-2}{2}.\frac{m-3}{2^2}\text{s}\ ((2))^2 - \&\text{c.}$$

in which the sum of the divisors of any number m is expressed by the sums of the divisors of the inferior numbers $m-1$, $m-2$, &c. and their powers. If v be an even number, then $\pm\ 1.2.3\ldots m$ will have the same sign as the co-efficient; if uneven, the contrary; but if the co-efficient $= 0$, then will the content $1.2.3\ldots m$ vanish. The law of this series is given in the Meditationes Algebraicæ.

3. Let H be the number of different ways by which the sum of any two numbers 1, 2, 3, 4, ... $m-2$, $m-1$, can become $= m$; H′ the number of ways by which the sum of any 3 of the above-mentioned numbers can make m; H″, H‴, H⁗, &c. the number of ways by which the sum of any 4, 5, 6, &c. of the above-mentioned numbers is $= m$ respectively; then will $1 - \text{H} + \text{H}' - \text{H}'' + \text{H}''' -$ &c. $= \pm 1$ or 0. Let $m = \frac{3z^2 \pm z}{2}$, and it will be $+1$ or -1, according as z is an odd or even number, in all other cases it will be $= 0$.

PART 2.—1. Let the equation be $x-1.x^2-1.x^3-1.x^5-1.x^7-1.x^{11}-1.x^{13}-1.x^{17}-1\ldots x^n-1.$ &c. $= x^{b'} - px^{b'-1} + qx^{b'-2} - rx^{b'-3} + sx^{b'-4} -$ &c. $= x^{b'} - x^{b'-1} - x^{b'-2} + x^{b'-4} + x^{b'-8} - x^{b'-10} - x^{b'-11} + x^{b'-12} + x^{b'-16} - x^{b'-17} - x^{b'-19} + x^{b'-20} - x^{b'-23} + 2x^{b'-24} - x^{b'-26} - x^{b'-27} +$

$x^{b'-28}$, &c. $= A' = 0$; the sum of any power (m) of each of the roots in the equation $A' = 0$ will be $s'(m)$, where $s'(m)$ denotes the sum of all the prime divisors of the number m, and m is not greater than n.

Cor. Hence, by the rule before-mentioned $s'(m) = s'(m-1) + s'(m-2) - s'(m-4) - s'(m-8) + s'(m-10) + s'(m-11) - s'(m-12) - s'(m-16) + s'(m-17) + s'(m-19) - s'(m-20) + s'(m-23) - 2s'(m-24) + s'(m-26) + s'(m-27) - s'(m-28) + s'(m-29)$, &c.

If in this, or the preceding, or subsequent analogous cases $s(m-r)$, or $s'(m-r)$, or $s^l(m-r)$, becomes $s(0)$, or $s'(0)$, or $s^l(0)$; for $s(0)$, or $s'(0)$, or $s^l(0)$, always substitute r.

Cor. Let L be the co-efficient of the term $x^{b'-m}$; then, by the above-mentioned series contained in the Meditationes Algebraicæ, will $1 . 2 . 3 . 4 \ldots . m \times L = 1 - m . \frac{m-1}{2} s'(2)$

$$+ m . m-1 . \frac{m-2}{3} \times s'(3) - m . m-1 . m-2 . \frac{m-3}{4} \times s'(4)$$

$$+ \text{\&c.} \qquad + m . m-1 . m-2 . \frac{m-3}{8} \times s'((2))^2$$

— &c. be an equation, which expresses a relation between the prime divisors of the numbers 1, 2, 3, 4 ... $m-1$, m, and their powers.

Cor. The co-efficient L = the difference between the two respective numbers of different ways that m can be formed by adding the prime numbers 1, 2, 3, 5, 7, 11, 13, 19, &c. the one with, and the other without, 2.

PART 3.—1. Let an equation $x^{\alpha} - 1 . x^{\beta} - 1 . x^{\gamma} - 1 . x^{\delta} - 1 \times$ &c. $= x^b - px^{b-1} + qx^{b-2} - rx^{b-3} +$ &c. $= 0$; then will the sum of the (m) powers of each of its roots be the sum of all the divisors of m, that can be found among the numbers α, β, γ, δ, &c.

2. The co-efficient of the term x^{b-m} will be the difference between the two respective numbers of different ways, that the number (m) can be formed from the addition of the numbers α, β, γ, δ, &c.; the one containing in it an odd number of the even numbers contained in α, β, γ, δ, &c.; the other not.

PART 4.—1. Let $x^l - 1 . x^{2l} - 1 . x^{3l} - 1 . x^{4l} - 1 \ldots . x^{nl} - 1$. &c. $= x^b - px^{b-l} + qx^{b-2l} - rx^{b-3l} +$ &c. $= x^b - x^{b-l} - x^{b-2l} + x^{b-5l} + x^{b-7l} - x^{b-12l} - x^{b-15l} +$ &c. $= B = 0$, of which equation all the co-efficients are the same as in case the first, and consequently ± 1 or 0 to the term (x^{b-nl}).

2. The sum of any power $l \times m$ of each of the roots of the equation $B = 0$ will be $s^l(m)$; where $s^l(m)$ denotes the sum of the divisors of m, which are divisible by l.

Cor. Hence $s^l(m) = s^l(m-l) + s^l(m-2l) - s^l(m-5l) - s^l(m-7l)$

$+ s^l(m-12l) + s^l(m-15l) - s^l(m-22l) - s^l(m-26l) +$ &c.; the law of the series has been given in case 1.

Cor. The sum of all the divisors of m not divisible by $l = s(m) - s^l(m) = s(m-1) - s^l(m-l) + (s(m-2) - s^l(m-2l)) - s(m-5) - s^l(m-5l)) - (s(m-7) - s^l(m-7l)) +$ &c.

A similar rule may be predicated of the sum of the divisors not divisible by the numbers a, b, c, d, &c.: for the sum of the divisors of the number (m) divisible by a, b, c, d, e, &c., where a, b, c, d, e, &c. are prime to each other $= (s^a(m) + s^b(m) + s^c(m) + s^d(m) + s^e(m) +$ &c.$)) - ((s^{a\times b}(m) + s^{a\times c}(m) + s^{b\times c}(m) + s^{a\times d}(m) + s^{b\times d}(m) + s^{c\times d}(m) + s^{a\times e}(m) +$ &c.$)) + (s^{a\times b\times c}(m) - s^{a\times b\times d}(m) + s^{a\times b\times d}(m) + s^{b\times c\times d}(m) + s^{a+b+c}(m) +$ &c.$)) - ((s^{a+b+c+d}(m) + s^{a+b+c+e}(m) +$ &c.$)) + (s^{a+b+c+d+e}(m) +$ &c.$)) -$ &c. $= l =$ the sum of all the divisors of m .... divisible by a, b, c, d, e, &c. respectively added together, $-$ the sum of all the divisors of m divisible by the products (ab, ac, bc, &c.) of any 2 of the quantities a, b, c, d, &c. $+$ the sum of all the divisors of m divisible by the contents (abc, abd, acd, bcd, &c.) of every 3 of the quantities a, b, c, d, &c. $-$ the sum of all the divisors of m divisible by the contents of every 4 of the above-mentioned quantities a, b, c, d, &c. $+$ and so on, and consequently $s(m) - c$ is the sum required.

The principles given in the former parts may be applied to this, and extended to equations of which the factors have the formula $x^a \pm k$; and from the sum of the inferior powers of each of the roots, and the co-efficients, may be collected the sum of the superior; the same may be performed by the co-efficients only, &c.

PART 5.—1. $S(\alpha \times \beta) = \alpha \times s(\beta) +$ sum of all the divisors of β not divisible by $\alpha = \beta \times s(\alpha) +$ sum of all the divisors of α not divisible by β.

2. $S^l(\alpha \times \beta) = \alpha \times s^l(\beta) +$ sum of all the divisors of β divisible by l but not by $\alpha =$ &c.

3. $S(\alpha \times \beta \times \gamma \times \delta \times$ &c.$) = \alpha \times s(\beta \times \gamma \times \delta \times \varepsilon$, &c.$) +$ sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not divisible by $\alpha = \alpha \times \beta \times s(\gamma \times \delta \times \varepsilon$, &c.$) +$ sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma \times \delta \times \varepsilon$, &c. not divisible by $\beta = \alpha \times \beta \times \gamma \times s(\delta \times \varepsilon$, &c.$) +$ sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma \times \delta \times \varepsilon$, &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta \times \varepsilon$, &c. not divisible by $\gamma = \alpha \times \beta \times \gamma \times \delta \times s(\varepsilon$, &c.$) +$ sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon$, &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma\delta\varepsilon$, &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta\varepsilon$, &c. not divisible by $\gamma + \alpha \times \beta \times \gamma \times$ sum of all the divisors of ε, &c. not divisible by $\delta =$ &c. The law of the series is manifest. The letters $\alpha, \beta, \gamma, \delta$, &c. which are not contained between the parentheses, denote prime numbers.

Cor. If some of the letters α, β, γ, δ, &c. be substituted for others, and others for them, the equations resulting will be just, and consequently many new equations may be deduced. If in the preceding equations for s be written s′, and for the sum of all the divisors of a certain quantity not divisible by a prime number (α, or β, or γ, &c.) be written the sum of all the divisors of that quantity not divisible by the same prime number, but divisible by *l*; the propositions resulting will be true. These equations may be applied to the equations given in the preceding parts, and from thence many others be deduced.

XXV. Experiments on the Production of Artificial Cold. By Mr. Richard Walker, Apothecary to the Radcliffe Infirmary at Oxford. p. 395.

Mr. W's most powerful frigorific mixture is the following: Of strong fuming nitrous acid, diluted with water (rain or distilled water is best) in the proportion of 2 parts of the former to 1 of the latter, each by weight, well mixed, and cooled to the temperature of the air, 3 parts; of vitriolated natron (Glauber's salt) 4 parts; of nitrated ammonia (nitrous ammoniac) $3\frac{1}{2}$ parts; each by weight, reduced separately to fine powder: the powdered vitriolated natron is to be added to the diluted acid, the mixture well stirred, and immediately afterward the powdered nitrated ammonia, again stirring the mixture: to produce the greatest effect, the salts should be procured as dry and transparent as possible, and used freshly powdered. These seem to be the best proportions when the temperature of the air and ingredients is $+ 50°$; as the temperature at setting out is higher or lower than this, the quantity of the diluted acid will evidently require to be proportionably diminished or increased. This mixture is but little inferior to one made by dissolving snow in nitrous acid, for it sunk the thermometer from $+ 32°$ to $- 20°$; perhaps it may be possible to reduce the salts to so fine a powder as to make it equal. In this last experiment the diluted acid was equal in quantity to the vitriolated natron, being 4 parts each, the nitrated ammonia $3\frac{1}{2}$ as before. A powder composed of muriated ammonia (crude sal ammoniac) 5 parts, nitrated kali (nitre) 4 parts, mixed, may be substituted in the stead of nitrated ammonia, with nearly equal effect, and in the same proportion.

Crystallized nitrated ammonia, reduced to very fine powder, sunk the thermometer, during its solution in rain water, 48°, from $+ 56°$, the temperature of the air and materials, to $+ 8°$; and when evaporated gently to dryness, and finely powdered, it sunk the thermometer 49°, to $+ 7°$, the temperature of the air and materials being as before at $+ 56°$: therefore, in this salt (which produces, as appears above, much greater cold during solution in water, than any other hitherto known) the water of crystallization is not in the least conducive to that effect. Mr. W. expected, that by diluting the strong nitrous acid to the proper strength with snow, instead of water, by which its temperature would be much reduced, and then adding the salts, a much greater degree of cold might be produced; but, by various diversified trials, but little advantage was gained. In the course of this winter, some diluted nitrous acid, in a wide mouthed phial, was immersed in a freezing mixture; when cooled to about — 32°, it froze intirely to the consistence of an ointment, when the thermometer suddenly rose to — 2°; on adding some snow that lay by, it became again liquid, and the mercury sunk into the bulb of a thermometer graduated to — 76°; he knew not its exact strength; but by the effect imagined it might correspond nearly with that which is capable of the easiest point of spirituous congelation. Cold, he found, may be produced by the union of such salts as on mixing are decomposed, and become liquid or partially so. The mineral alkali produces this effect with all the ammoniacal salts; but with nitrated ammonia to a considerable degree. The mineral alkali added in powder to nitrous acid, diluted as above, sunk the thermometer 22° only, from 53°, temperature of air and materials, to 31°. This salt contains nearly as much water of crystallization as vitriolated natron, and produces more cold during solution in water than that salt. The reason why it produces less when added to acid than the neutral salt does, is perhaps sufficiently evident. He has observed the thermometer to be stationary, or even to rise, during the violent effervesence produced on mixing those materials, and to sink as soon as that ceased.

Vitriolated natron dissolved indifferently in rectified spirit of wine, and produced neither heat nor cold; the disposition to produce cold, during its solution, being perhaps exactly counteracted by the tendency which the dissolved salt hath in uniting with the spirit to produce heat. Vitriolated magnesia, a salt very similar to vitriolated natron, during solution in the diluted nitrous acid, produced nearly as much cold as that salt: the small difference there is between them, as to this effect, may be owing to the former containing rather less water in its crystals.

Vitriolated natron, liquified by heat, was set to cool: when its temperature was reduced to 70°, it became solid, and the thermometer immediately rose to 88°, its freezing point. Does not the quantity of sensible heat evolved by this salt, in becoming solid, indicate its great capacity for heat, in returning to a liquid state, and consequently account in a great measure for its producing such intense cold during solution in the diluted mineral acids? Two salts, vitriolated argillaceous earth (alum) and tartarized natron (Rochelle salt,) each contain nearly as much water of crystallization as vitriolated natron; but produced neither of them any considerable effect during solution in the diluted nitrous acid; the latter made the thermometer rise: neither did their temperatures increase, like that salt, in changing from a liquid to a solid state.

From the obvious application of artificial frigorific mixtures to useful purposes, in hot climates especially, where the inhabitants scarcely know by the sense of feeling winter from summer, it may not be amiss to hint at the easiest and most economical method of using them. For most intentions perhaps, the following cheap one may be sufficient: of strong vitriolic acid, diluted with an equal weight of water, and cooled to the temperature of the air, any quantity; add to this an equal weight of vitriolated natron in powder: this is the proportion when the temperature set out with is + 50°, and will sink the thermometer to 5°; if higher, the quantity of salt must be proportionably increased. The obvious and best method of finding the necessary quantity of any salt to produce the greatest effect, by solution in any liquid, at any given temperature, is by adding it gradually until the thermometer ceases to sink, stirring the mixture all the while.

If a more intense cold be required, double aqua fortis, as it is called, may be used; vitriolated natron, in powder, added to this, produces very nearly as much cold as when added to the diluted nitrous acid: it requires a rather larger quantity of the salt, at the temperature of + 50°, about 3 parts of the salt to 2 parts of the acid: it will sink the thermometer from that temperature nearly to 0, and the consequence of more salt being required is, its retaining the cold rather longer. This mixture has one great recommendation, a saving of time and trouble. A little water in a phial, immersed in a small tea cup of this mixture, will be soon frozen in summer; and if the salt be added in crystals unpounded to double aqua fortis, even at a warm temperature, the cold produced will be sufficient to freeze water or creams; but if diluted with $\frac{1}{5}$ its weight of water, and cooled, it is about equal to the diluted nitrous acid above-mentioned, and requires the same proportion of the salt. A mixture of vitriolated natron and diluted nitrous acid sunk the thermometer from + 70°, temperature of air and ingredients, to + 10°. The cold in any of these mixtures may be kept up a long time by occasional additions of the ingredients in the proportions mentioned. A chemist would make the same materials serve his purpose repeatedly.

Equal parts of muriated ammonia and nitrated kali in powder make a cheap and convenient composition for producing cold by solution in water; it will, by the following management, freeze water or creams at Midsummer. June 12th, 1787, a very hot day, Mr. W. poured 4 oz. wine measure, of pump water, at the temperature of 50° (it is well known that water at springs retains nearly the same temperature winter and summer, viz. about 50°, to which temperature the water may be reduced during the warmest weather, by pumping off some first) on 3 oz. Avoirdupois weight, of the above powder (previously cooled by immersing the vessel containing it in other water at 50°,) and after stirring the mixture its temperature was 14°; some water contained in a small phial, immersed in this mixture, was consequently soon frozen. This solution was afterwards evaporated to dryness, in an earthen vessel, reduced to powder, and added to the same quantity of water, under the same circumstances as before, when it again sunk the thermometer to 14°. Since that time he has repeatedly used a composition of this kind for the purpose of producing cold, without observing any diminution in its effect after many evaporations. The cold may be economically kept up and regulated any length of time, by occasionally pouring off the clear saturated liquor, and adding fresh water, observing to supply it constantly with as much of the powder as it will dissolve.

The degree of cold at which water begins to freeze has been observed to vary much; but that it might be cooled 22° below its freezing point was perfectly unknown to him till lately. He filled the bulb of 2 thermometers, one with the purest rain water he could procure, the other with pump water; the water was then made to boil in each, till $\frac{1}{3}$ only remained: these were kept in a frigorific mixture, at the temperature of + 10°, for a much longer time than he thought necessary to cool the water to the same temperature; and by repeated trials he found it was necessary to lower the temperature of the mixture to near + 5°, to make the water in either of them freeze. These were likewise suspended out of doors, close to a thermometer, during the late frost, and the water never observed frozen. On March the 22d, at 6 in the morning, the water in each remained unfrozen, though the tubes were gently shaken, the thermometer standing at that time at 23°. There appeared to be little difference with respect to the degree of cold necessary to freeze the water, whether the tube of the thermometers were open or closed in vacuo (which was very nearly effected by suffering the water to boil up to the orifice of the tube, and then suddenly sealing it) or not, but unboiled water in the same situation froze in a higher temperature.

It is commonly supposed that gentle agitation of any kind will dispose water, cooled below its freezing point, to become ice; but Mr. W. repeatedly cooled rain water and pump water, boiled a long time, and unboiled, in open vessels to 30° or lower, and constantly succeeded, after trying other kinds of agitation in vain, by stirring, or rather scraping gently, the bottom and sides of the vessel containing the water to be frozen, when after some short time small filaments of ice appeared, and by continuing this motion about every part of the vessel beneath the surface of the water, about $\frac{2}{3}$ of the water commonly froze. A slender, pointed glass rod he used for this purpose.

Extract of a second Letter from Mr. Walker.—A more intense cold may be produced by a solution of salts in water in summer, than can be produced by a mixture of snow and salt in winter. To rain water 6 drs. by weight, I added 6 drs. of nitrated ammonia reduced to a very fine powder, which made the thermometer sink from + 50°, temperature of the materials, to 4°; then adding 6 drs. of mineral alkali very finely powdered, the thermometer sunk to — 7°, that is 57°. It is observable, that in the latter there are 2 causes concur in producing the effect, the liquefaction both of the snow and salt; but in the experiment just mentioned the liquefaction of the salts only. Vitriolated natron, after it had given out its water of crystallization by exposure to the atmosphere, produced no change of temperature by solution in the diluted nitrous acid, but during solution in water produced heat, as did also the mineral alkali.

XXVI. A Description of an Instrument which, by the turning of a Winch, produces the two States of Electricity without Friction or Communication with the Earth. By Mr. William Nicholson. p. 403.

Plate 6, fig. 3, represents the apparatus supported on a glass pillar 6½ inches long. It consists of the following parts. Two fixed plates of brass, A and C, are separately insulated and disposed in the same plane, so that a revolving plate B may pass very near them, without touching. Each of these plates is 2 inches in diameter; and they have adjusting pieces behind, which serve to place them accurately in the required position. D is a brass ball, also of 2 inches diameter, fixed on the extremity of an axis that carries the plate B. Besides the more essential purpose this ball is intended to answer, it is so loaded within on one side, that it serves as a counterpoise to the revolving plate, and enables the axis to remain at rest in any position. The other parts may be distinctly seen in fig. 4. The shaded parts represent metal and the white represent varnished glass. ON is a brass axis, passing through the piece M, which last sustains the plates A and C. At one extremity is the ball D before-mentioned; and the other is prolonged by the addition of a glass stick, which sustains the handle L and the piece GH separately insulated. E, F, are pins rising out of the fixed plates A and C, at unequal distances from the axis. The cross-piece GH, and the piece K, lie in one plane, and have their ends armed with small pieces of harpsichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is also a pin I, in the piece M, which intercepts a small wire proceeding from the revolving plate B.

The touching wires are so adjusted, by bending, that when the revolving plate B is immediately opposite the fixed plate A, the cross-piece GH connects the 2 fixed plates, at the same time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately opposite the fixed plate C, the ball becomes connected with this last plate, by the touching of the piece K against F; the 2 plates, A and B, having then no connection with any part of the apparatus. In every other position the 3 plates and the ball will be perfectly unconnected with each other.

Mr. Cavallo's discovery, so well explained in the last Bakerian lecture, that the minute differences of electrization in bodies, whether occasioned by art or nature, cannot be completely destroyed in any definite time, may be applied to explain the action of the present instrument. When the plates A and B are opposite each other, the 2 fixed plates A and C may be considered as one mass; and the revolving plate B, together with the ball D, will constitute another mass. All the experiments yet made concur to prove, that these 2 masses will not possess the same electric state; but that, with respect to each other, their electricities will be plus and minus. These states would be simple and without any compensation, if the masses were remote from each other; but as that is not the case, a part of the redundant electricity will take the form of a charge in the opposed plates A and B. From other experiments it appears that the effect of the compensation on plates opposed to each other, at the distance of $\frac{1}{40}$ part of an inch, is such that they require, to produce a given intensity, at least 100 times the quantity of electricity that would have produced it in either, singly and apart. The redundant electricities in the masses under consideration will therefore be unequally distributed: the plate A will have about 99 parts, and the plate C 1; and, for the same reason, the revolving plate B will have 99 parts of the opposite electricity, and the ball D 1. The rotation, by destroying the contacts, preserves this unequal distribution, and carries B from A to C, at the same time that the tail K connects the ball with the plate C. In this situation, the electricity in B acts on that in C, and produces the contrary state, by virtue of the communication between C and the ball; which last must therefore acquire an electricity of the same kind with that of the revolving plate. But the rotation again destroys the contact, and restores B to its first situation opposite A. Here, if we attend to the effect of the whole revolution, we shall find that the electric states of the respective masses have been greatly increased: for the 99 parts in A and in B remain, and the 1 part of electricity in C has been increased so as nearly to compensate 99 parts of the opposite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A 2d rotation will of course produce a proportional augmentation of these increased quantities: and a continuance of turning will soon bring the intensities to their maximum, which is limited by an explosion between the plates.

If one of the parts be connected with an electrometer, more especially that of Bennet, these effects will be very clearly seen. The spark is usually produced by a number of turns between 11 and 20; and the electrometer is sensibly acted on by still fewer. When one of the parts is occasionally connected with the earth, or when the adjustment of the plates is altered, there are some variations in the effects, not difficult to be reduced to the general principles, but sufficiently curious to excite the meditations of persons the most experienced in this branch of natural philosophy. If the ball be connected with the lower part of Bennet's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the position of the apparatus is such that the cross-piece GH touches the 2 pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity must be strong enough to destroy and predominate over any other electricity the plates may possess. It scarcely need be observed, that if this difficulty should hereafter be removed, the instrument will have great advantages as a multiplier of electricity in the facility of its use, the very speedy manner of its operation, and the unequivocal nature of its results.

XXVII. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland; with the Rain in Hampshire and Surrey, in 1787. Also some Account of the Annual Growth of Trees. By T. Barker, Esq. p. 408.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.				Hampshire.		Surry.
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	Selbourn	Fyfield.	S. Lamb.
		Inches.	Inches.	Inches	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30.13	29.11	29.70	45	34½	39	47	25	35	0.415	0.88	0.43	0.60
	Aftern.				46	34½	40	51	31	39½				
Feb.	Morn.	29.85	28.15	29.40	47	41	44	47½	27	39	0.860	3.67	3.40	1.68
	Aftern.				48½	41½	45	53½	37	46				
Mar.	Morn.	30.02	28.47	29.30	49	41	45½	49	31	40	1.782	4.28	3.80	1.62
	Aftern.				50½	42	46½	54	35½	48½				
Apr.	Morn.	30.00	28.54	29.47	50	44	46½	50½	35	42	1.721	0.74	0.69	0.93
	Aftern.				50½	45	47½	56½	42½	50				
May	Morn.	29.86	28.80	29.47	60½	42½	52½	55½	36	48½	1.573	2.06	1.27	1.60
	Aftern.				62	44	54	72½	46½	59				
June	Morn.	29.76	29.05	29.44	63	51½	58	60½	46	54½	1.800	1.50	1.43	0.68
	Aftern.				64½	53	59½	77	55	65½				
July	Morn.	29.93	28.92	29.36	69	56½	60½	66	50½	57	3.169	6.53	3.50	4.12
	Aftern.				70	57	62	79	63	68				
Aug.	Morn.	29.94	28.74	29.53	70	55½	61	63½	49½	56	1.969	0.83	0.74	0.60
	Aftern.				73	56½	63	80	51½	67½				
Sept.	Morn.	30.01	28.59	29.50	60	53½	57	57½	42	51	1.225	1.56	1.47	0.78
	Aftern.				60½	55	57½	65	55	61				
Oct.	Morn.	29.70	28.48	29.26	56	46½	51½	55	36	46	3.726	5.04	3.44	2.41
	Aftern.				57	47	53	61	44½	54½				
Nov.	Morn.	29.95	28.50	29.33	52	34	43	49	20	35½	1.462	4.09	2.57	1.51
	Aftern.				52½	35½	43	54½	30½	42				
Dec.	Morn.	29.93	28.75	29.22	48	34	41	48½	26	37	3.085	5.06	3.48	3.87
	Aftern.				49	35	41½	55½	30½	41				
Means and sums				29.42			50½			49⅓	22.787	36.24	25.82	20.40

On the annual growth of trees.

Oaks.

		Girth.		Girth.	Rate.		Girth.	Rate.			Girth.		Girth.	Rate.		Girth.	Rate.
		In.		In.	In.		In.	In.			In.		In.	In.		In.	In.
1	——	——	1772	19	——	1787	41	1.5	4	——	——	——	——	——	1787	156½	1.3
2	1758	13	1772	33	1.4	1787	55½	1.5	5	1758	41	1772	56	1.1	1787	77½	1.4
3	1758	18½	1772	40½	1.6	1787	58	1.2	6	1744	20	1765	45	1.2	1787	74½	1.3

Oaks.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
7	1758	18	1772	36	1.3	1787	54	1.2
8	1758	76	1772	93½	1.25	1787	109½	1.1
9	1751	124	1772	147	1.1	1787	164½	1.2
10	1744	23½	1765	49	1.2	1787	74	1.1
11	1744	69½	1772	99	1.1	1787	115	1.1
12	1744	14	1765	43	1.4	1787	60	0.8
13	1747	82	1765	99½	1.0	1787	120½	1.0
14	1744	21	1765	45	1.1	1787	64	0.9
15	1762	106½	1772	117	1.0	1787	130	0.9
16	1751	117	1770	132	0.8	1787	149½	1.0
17	1751	114	1770	131½	0.9	1787	145	0.8
18	1751	84½	1772	101	0.8	1787	109	0.5
19	1744	41	1765	58½	0.8	1787	69	0.5

Ash.

20	—	—	1772	71	—	1787	106	2.3
21	1745	23½	1765	67	2.2	1787	111	2.0
22	1744	22	1765	55½	1.6	1787	92	1.7
23	1744	32	1765	61	1.4	1787	94½	1.5
24	1744	66	1765	91½	1.4	1787	114	1.0
25	1751	20	1772	45	1.25	1782	58½	0.9
26	1765	55	1772	64	1.2	1787	75½	0.9
27	1747	77	1765	97	1.0	1787	116½	1.0
28	—	—	1772	67½	—	1787	82	1.0
29	1744	56	1765	77½	1.0	—	—	—
30	1755	51½	1772	67	0.9	1787	80	0.9
31	—	—	1765	74	—	1781	89	0.9
32	1751	45½	1772	67	1.0	1787	77	0.7
33	1744	17½	1765	34	0.8	1787	52	0.8
34	1744	17	1765	36½	0.9	1787	52½	0.7
35	1744	20	1772	40	0.7	—	—	—
36	1745	13½	1772	31½	0.7	1787	41	0.6

Elms.

37	1755	0	1772	42	2.5	1787	77	2.3
38	1744	28	1765	60	1.5	1787	96	1.6
39	1744	37	1758	50	0.9	1781	72	1.0
40	1744	46	1758	58	0.9			
41	1744	48	1758	59	0.8			

Except the first 2 ash trees, the growth of oak and ash are nearly the same. Mr. B. had some of both sorts planted at the same time, and in the same hedges, of which the oaks are the largest, but there is no certain rule as to that. The common growth of an oak or an ash is about an inch in girth in a year; some thriving ones will grow an inch and a half; the unthriving ones not so much, some probably less than any here, for he chose in general to measure those that seemed thriving.

Large trees grow more timber in a year than small ones; for if the annual growth be an inch, a coat of $\frac{1}{6}$ of an inch thick is laid on all round, and the timber added to the body every year is its length multiplied into the thickness of the coat, and into the girth; and therefore the thicker the tree is, the more timber is added. The body of N° 9 is 9 feet long, the girth under the bark above 13 feet, the thickness of the coat $\frac{1}{6}$ of an inch or $\frac{1}{72}$ of a foot: then $9 \times 13 \times \frac{1}{72}$ is $1\frac{5}{8}$ feet of timber added in a year to the body, besides the increase on all the branches, and it has a very great head; one limb squares 20 inches, and is itself equal to a moderate tree.

The hedge in which N° 4 grows was planted in 1665, probably the tree is not older than that year; it has therefore increased in girth about 1.3 inch every year since it was set. The oak, N° 5, he believed sowed itself; and he did not know there was such a one till about the year 1740, when the hedge being cut, the tree was found, and might be then 20 years old or more. The 2 ash trees N°

20 and 21, grow much faster than any of the rest, but are neither of them handsome growing trees. N° 20 has several seams where the bark is parting from the wood, and are likely to be dead sides. N° 21 was about as thick as a walking-stick in 1730. It does not grow round and smooth, has no dead side, but several deep furrows in it, so that these 2 trees seem to grow faster than they can grow well. In 1733, N° 23 was about as thick as a pitch-fork shaft. The elm N° 37 was planted with the quick in January, 1756, and cut down to the ground as that was. It is a kind of witch elm, which grow faster than the upright ones, and with large round heads. N° 38 is so far like a witch elm, that at 10 feet high it parts into a great head; but it grows much straighter and handsomer than that kind of tree generally does.

Planted trees at a distance from the hedge seem not to grow so large as sown trees in the hedge; whether from the check the roots receive in transplanting, or that the trees not in hedges are more rubbed by the cattle; perhaps both causes concur when the trees are transplanted large; but trees set in quicks, when very small, do not seem to be hurt by it. Mr. B. had some oaks set with the quick, and a row of acorns was some years after sown against it; but in between 40 and 50 years they have not overtaken the planted ones in size; the sown seem however inclined to be taller trees than the planted.

XXVIII. On the Era of the Mahometans, called the Hejerà. By William Marsden, Esq., F. R. S., and A. S.* p. 414.

In their computation of time, the Arabs, and other Mahometan nations, reckon by a year which is purely lunar. It has no reference to the solar revolutions, and is of course unconnected with the vicissitude of seasons. The purpose of its adoption appears to have been chiefly religious, for the regulation of fasts and ceremonies, rather than that of the civil concerns of the people. Perhaps a conscious ignorance in matters of science might have determined the institutors to prefer a period whose limits were marked and obvious to the senses, to one whose superior accuracy depended on astronomical calculation; and it may also be conjectured, that their habits of life rendered the adjustment of the tropical year less interesting to these turbulent and wandering fanatics, than to nations whose attention was directed to agriculture and other peaceful arts.

The era of the Mahometans, called by them the Hejerà, or Departure, is accounted from the year of the flight of Mahomet, their prophet, from Mecca, in Arabia Petræa, to Medina, at that time called Yatreb, which was the 13th of his pretended mission, the year of Christ 622, and of the Julian period 5335. This event, but little memorable in itself, and deriving no celebrity from

* As this mode of spelling the word differs from that commonly followed, it may be proper to observe, that the Arabic letters of which it is composed are h, j, r, â, or ah, and that the supplied vowels are to be pronounced short—Orig.

the circumstances immediately attending it, was, 18 years after, distinguished by the Caliph Omar, as the crisis of their new religion, and established as an epoch, to which the dates of all the transactions of the faithful should have reference in future. Before this, the people had been accustomed to compute from the commencement of a particular war, the day of a remarkable battle, or other occasional event of importance to their little communities. Accordingly, Mahomet is said to have been born in the first year of the era of the elephant, so called from an attack on the city and temple of Mecca, by a king of Abyssinian race, in which those animals were employed; and 20 years after this, the impious war, in which the animosity of two contending tribes occasioned them to violate the sacred or interdicted months, appeared of consequence sufficient to give rise to a new era. The uncertainty and confusion produced by this fluctuation demanded a reform, and more forcibly in proportion as the interests and concerns of the growing empire extended themselves. A dispute between two individuals, respecting the year in which the term of an obligation for money should be understood to expire, the parties being agreed as to the month, pointed out to the Caliph, to whose tribunal it was referred, the immediate necessity of enjoining the observance of a determinate era, in which the strongest prejudices of the people should be made to concur with the sovereign authority. The date of the Hejerà was thenceforth expressed in all the public acts and letters.

It must be understood, that though the account of the years, collectively considered, was vague, that of the months was certain, and their succession at all times scrupulously attended to. Omar did not think it expedient to attempt any innovation as to the time of beginning the year, against which the ideas of the people would have revolted; and therefore, though the escape of Mahomet from the indignation of his fellow citizens was effected, according to their records, on the first day of the 3d month, or rabee prior, on the 12th day of which he reached Medina, yet the Hejerà takes date from a period of 2 months antecedent to this flight, namely, from the first day of Moharram, being the day on which immemorial custom had established the celebration of the festival of the new year.

The Arabian and Syrian Christians, and the Mahometan astronomers in general, appear to have fixed this day to Thursday the 15th of the Syro-Macedonian month Tamooz, answering to our July; but some among the latter, and most of their historical writers, refer it to the next day, Friday the 16th, and this latter date has, in modern times, obtained almost universal acceptance. A religious preference which Friday claims above the rest of the week, seems to have given effect to the arguments in its favour. The difference of opinion on this subject has arisen, in the first place, from the uncertainty unavoidably attending a date, to be ascertained, at a distant period of time, from the phase of the moon, which is retarded or advanced by so complicated a variety of circumstances: and the ambiguity appears, in the 2d place, to have been promoted by the custom of the Arabs beginning their day at sun-set; conformably with which idea, the time when the moon became visible at Mecca, being the evening of Thursday the 15th, according to our mode of computation, was to them the commencement of Friday; which Friday, beginning a few hours later, we term the 16th of July. At that period the cycle of the sun was 15; the cycle of the moon, or golden number, 15; the Roman indiction 10; and the dominical letter c.

The year of the Mahometans consists of 12 lunar months, and no embolism being employed to adjust it to the solar period (as practised by the Chaldæans and Hebrews, who were in other particulars their guides, and anciently, it is said, by the Arabs themselves), the commencement of each successive lunar year anticipates the completion of the solar, and revolves through all its seasons, the months respectively preserving no correspondence. In order to form a just and accurate idea of the length of this year, and of its component months, it will be necessary to distinguish 2 modes of estimating their commencement and duration. These, though their difference is not progressive, never amounting to more than 2 whole days, and rarely to so much as 1, may yet, if misunderstood, occasion in some instances, uncertainty and error: and more especially as the writers on this subject have inadvertently fallen into contradictions, from neglecting to explain to their readers a distinction of which they must have been themselves sufficiently aware. These modes may be denominated the vulgar or practical, and the political or chronological reckoning.

The vulgar or practical reckoning is that which estimates the commencement of the year, or first day of the month Moharram, from the appearance of the new moon, on the evening of the 1st or 2d day after the conjunction, or from that time at which it might from its age be visible, if not obscured by the circumstances of the weather, which is scarcely ever so soon as 24 hours, and seldom later than 48 hours, after the actual change. This appearance is announced by persons placed on the pinnacles of the mosques or other elevated situations, to the people below, who welcome it with the sound of instruments, firing of guns, and other demonstrations of respect and zeal. The month thus commenced is computed to last till the new moon again becomes visible; and so of the remaining months, till she has completed her 12th lunation, and, emerging from the sun's rays, marks the practical commencement of another year.

In the political or chronological mode of reckoning, the return of a new year, or the duration of the months which compose it, is not regulated either by the appearance of the moon, or the calculated period of conjunction, but according to a certain division of a cycle of 30 years, adopted for this purpose.* Particular attention is due to the explanation of this mode, both as being more artificial and complex, and because it serves to regulate the dates in matters of historical record, and indeed of all writings where pretension is made to accuracy. On this the Turkish, Moorish, and every systematic Mahometan calendar are founded.

The lunar month, or mean synodic revolution, according to the computation of the Arabian astronomers, consists of 29 days, 12 hours, and 792 scruples or parts in 1080; and the year of 354 days, 8 hours, and 864 scruples. But, as the purposes of mankind require that the year should contain an integral number of days, it became expedient to collect and dispose of these fractional exceedings in a consistent and practical manner; and with this view, a cycle or period of 30 lunar years was chosen, as the lowest number that admitted of their being formed into days, without sensible deficiency or remainder. Their sum being 11 days, it was determined that 19 of those 30 years should be composed of 354 days, and 11 of 355 days each. The justness of this proportion will equally appear, if it be observed, that 8 hours and 864 scruples, or 48 minutes, constitute 11 parts in 30 of 24 hours, and consequently in 30 years produce an excess of 11 whole days.† It remained next to be considered in what order and method these additional or intercalary days should be inserted, so as to affect the compensation required with as much equability as possible, and maintain a correspondence, as near as circumstances would admit, with the periods marked by the phases of the moon. The following are the years to which, for reasons that shall be afterwards assigned, it was judged proper to annex an extraordinary day, and which, in contradistinction to those 19 that have only 354 days, are termed years of excess, viz. the 2d, 5th, 7th, 10th, 13th, 16th, 18th, 21st, 24th, 26th, and 29th, of the cycle of 30 years.

Their months, conformably with those of the Hebrew calendar, it was determined should consist alternately of 30 and 29 days; and therefore, in an ordi-

* A passage in Alfraganus (who wrote about the year of Christ 950) would lead us to infer, that besides the 2 ways of computing time here distinguished, the astronomers were accustomed to follow a 3d, whose periods were marked by the conjunction of the luminaries: but, as this learned Mahometan was a professed student of Ptolemy's works, which in this place he quotes, we may conclude that, when he speaks of astronomers, he does not mean to confine the expression to those of his own country or religion.—Orig.

† The mean synodic revolution being $29^d\ 12^h\ 44^m$ and nearly 3^s, this cycle falls short of 30 complete lunar years, by something more than 17^m, and consequently advances 1 day in about 2500 years. The Chaldæans, who made the time of the revolution to consist of 1 scruple, or 1080th part of an hour, more than the Arabs thought fit to allow, were wonderfully near to the truth. If, instead of 30 years, a cycle of 19 had been chosen, and 7 days intercalated, there would have been an excess of a 30th part of a day, which would have caused the reckoning to retrograde 1 day in 570 years.—Orig.

nary or simple year of 354 days, the 12th and last month, Dulhajee, would have only 29; but, in the years of excess, the intercalary day is added to this month, which is then made to consist of 30 days, and the year, consequently, of 355 days. Thus, for example, in the year of Christ 622, the Hejerà commenced on the 16th of July, with the Arabian month

	1st year.	2d year.
Moharram, which had days	30	30
Safar	29	29
Rabee prior	30	30
Rabee posterior	29	29
Joomad prior	30	30
Joomad posterior	29	29
Carried over	177	177
1st year ended 5 July 623.		

	1st year.	2d year.
Brought over	177	177
Rajab	30	30
Saban	29	29
Hamadan	30	30
Sawal	29	29
Dulkaidat	30	30
Dulhajee	29	30
	354	355
2d year ended 25 June 624.		

It may not be uninteresting to examine the rule by which the Arabians appear to have been guided, in placing the intercalary day at the end of those particular years which have been specified. It was observed that the annual excess is calculated to be 11 parts in 30 of a day. At the commencement of the first year of their first cycle, they appear to have assumed the fact, somewhat capriciously, that there was an excess of 11 parts, belonging to the preceding year, to be accounted for, or brought on. At the end of the first year there would consequently be 22 such parts; and at the end of the 2d year 33 parts. Here then the first intercalary day was applied; that 2d year was made to consist of 355 days, and there remained 3 parts, over and above, to be carried on to the next. At the expiration of the 3d year, the parts amounted to 14: of the 4th year, to 25; and of the 5th, to 36; when the intercalation was again applied, and a balance of 6 parts carried on. From this it will be understood in what manner the fractional exceedings of each year were combined and disposed of through the succeeding years of the cycle; and it will be necessary only further to remark that, when the aggregate of the fractions falls short no more than 2 or 3 parts of the number of 30, they still add the intercalary day, and deduct the deficiency from the excess of the following year, which, in the course of 1 cycle, takes place only 3 times. At the end of the 29th year, the accumulated fractions, amounting exactly to 30, are commensurate with the intercalation then applied; and the excess of the 30th, or last year, is accounted for in the first intercalation of the succeeding period. The operation would doubtless have appeared more methodical, if the first intercalary day were not to have been added till the end of the 3d year, and the 11th, or last, till the end of the 30th year or termination of the cycle. From this consideration some commentators have been led to dissent from the more general idea, as above given, and to suggest, that the embolism is in fact applied so soon after the commencement of the cycle, as the yearly accumulation of the fractional parts exceeds the sum of half a day, or 12 hours, and that it accordingly is made to take place at the end of the 2d year, because the fractions then amount to $17^h\ 36^m$, or 22 parts in 30; at the end of the 5th year, because they then amount to 25; and at the end of the 7th year, to 17 parts; keeping thus as near as possible to the mean division of time, by applying the compensation before it is fully wanted. The effect however is in both cases the same, and it is of but little moment to determine which theory is right. This cycle of 30 Mahometan years, contains 10,631 days, and is equal to 29 years and 39 days of our computation. The annual mean difference is 10 days and 21 hours nearly; which in common calculations, for short periods of time, may be reckoned at 11 days, by which number the lunar year anticipates the solar.

Annexed is a table exhibiting the correspondence of the years of the Hejerà, from the establishment of that epoch, with those of the Christian era, to the year of our Lord 2000. Until the beginning of the present century, it appears sufficient to distinguish every 10th year; the intervals between which may be calculated with ease and precision, by attending to what has been said respecting the cycle. From the year 1700 to the conclusion of the 20th century, for the convenience of historians yet unborn, the commencement of each year of the Hejerà is ascertained.

Table exhibiting the Correspondence of the Years of the Hejerà, with those of the Christian Era.

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1	622	16 July	F	231	845	7 Sept.	M	461	1068	31 Oct.	F
11	632	29 Mar.	Su	241	855	22 May	W	471	1078	14 July	Sa
21	641	10 Dec.	M	251	865	2 Feb.	F	481	1088	27 Mar.	M
31	651	24 Aug.	W	261	874	16 Oct.	Sa	491	1097	9 Dec.	W
41	661	7 May	F	271	884	29 June	M	501	1107	22 Aug.	Th
51	671	18 Jan.	Sa	281	894	13 March	W	511	1117	5 May	Sa
61	680	1 Oct.	M	291	903	24 Nov.	Th	521	1127	17 Jan.	M
71	690	15 June	W	301	913	7 Aug.	Sa	531	1136	29 Sept.	Tu
81	700	26 Feb.	Th	311	923	21 April	M	541	1146	13 June	Th
91	709	9 Nov.	Sa	321	933	1 Jan.	Tu	551	1156	25 Feb.	Sa
101	719	24 July	M	331	942	15 Sept.	Th	561	1165	7 Nov.	Su
111	729	5 April	Tu	341	952	29 May	Sa	571	1175	22 July	Tu
121	738	18 Dec.	Th	351	962	9 Feb.	Su	581	1185	4 April	Th
131	748	31 Aug.	Sa	361	971	24 Oct.	Tu	591	1194	16 Dec.	F
141	758	14 May	Su	371	981	7 July	Th	601	1204	29 Aug.	Su
151	768	26 Jan.	Tu	381	991	20 Mar.	F	611	1214	13 May	Tu
161	777	9 Oct.	Th	391	1000	1 Dec.	Su	621	1224	24 Jan.	W
171	787	22 June	F	401	1010	15 Aug.	Tu	631	1233	7 Oct.	F
181	797	5 March	Su	411	1020	27 April	W	641	1243	21 June	Su
191	806	17 Nov.	Tu	421	1030	9 Jan.	F	651	1253	3 Mar.	M
201	816	30 July	W	431	1039	23 Sept.	Su	661	1262	15 Nov.	W
211	826	13 April	F	441	1049	5 June	M	671	1272	29 July	F
221	835	26 Dec.	Su	451	1059	17 Feb.	W	681	1282	11 April	Sa

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
691	1291	24 Dec.	M	1125	1713	17 Jan.	Sa	1182	1768	18 May	W
701	1301	6 Sept.	W	1126	1714	7 Jan.	Th	1183	1769	7 May	Su
711	1311	20 May	Th	1127	1714	27 Dec.	M	1184	1770	27 April	F
721	1321	31 Jan.	Sa	1128	1715	16 Dec.	F	1185	1771	16 April	Tu
731	1330	15 Oct.	M	1129	1716	5 Dec.	W	1186	1772	5 April	Su
741	1340	27 June	Tu	1130	1717	24 Nov.	Su	1187	1773	25 March	Th
751	1350	11 Mar.	Th	1131	1718	13 Nov.	Th	1188	1774	14 March	M
761	1359	23 Nov.	Sa	1132	1719	3 Nov.	Tu	1189	1775	4 March	Sa
771	1369	5 Aug.	Su	1133	1720	22 Oct.	Sa	1190	1776	21 Feb.	W
781	1379	19 April	Tu	1134	1721	11 Oct.	W	1191	1777	9 Feb.	M
791	1388	31 Dec.	Th	1135	1722	1 Oct.	M	1192	1778	30 Jan.	F
801	1398	13 Sept.	F	1136	1723	20 Sept.	F	1193	1779	19 Jan.	Tu
811	1408	27 May	Su	1137	1724	9 Sept.	W	1194	1780	8 Jan.	Sa
821	1418	8 Feb.	Tu	1138	1725	29 Aug.	Su	1195	1780	28 Dec.	Th
831	1427	22 Oct.	W	1139	1726	18 Aug.	Th	1196	1781	17 Dec.	M
841	1437	5 July	F	1140	1727	8 Aug.	Tu	1197	1782	7 Dec.	Sa
851	1447	19 Mar.	Su	1141	1728	27 July	Sa	1198	1783	26 Nov.	W
861	1456	3 Nov.	M	1142	1729	16 July	W	1199	1784	14 Nov.	Su
871	1466	13 Aug.	W	1143	1730	6 July	M	1200	1785	4 Nov.	F
881	1476	26 April	F	1144	1731	25 June	F	1201	1786	24 Oct.	Tu
891	1486	7 Jan.	Sa	1145	1732	13 June	Tu	1202	1787	13 Oct.	Sa
901	1495	21 Sept.	M	1146	1733	3 June	Su	1203	1788	2 Oct.	Th
911	1505	4 June	W	1147	1734	23 May	Th	1204	1789	21 Sept.	M
921	1515	15 Feb.	Th	1148	1735	13 May	Tu	1205	1790	10 Sept.	F
931	1524	29 Oct.	Sa	1149	1736	1 May	Sa	1206	1791	31 Aug.	W
941	1534	13 July	M	1150	1737	20 April	W	1207	1792	19 Aug.	Su
951	1544	25 March	Tu	1151	1738	10 April	M	1208	1793	9 Aug.	F
961	1553	7 Dec.	Th	1152	1739	30 March	F	1209	1794	29 July	Tu
971	1563	21 Aug.	Sa	1153	1740	18 March	Tu	1210	1795	18 July	Sa
981	1573	3 May	Su	1154	1741	8 March	Su	1211	1796	7 July	Th
991	1583	15 Jan.	Tu	1155	1742	25 Feb.	Th	1212	1797	26 June	M
1001	1592	28 Sept.	Th	1156	1743	15 Feb.	Tu	1213	1798	15 June	F
1011	1602	11 June	F	1157	1744	4 Feb.	Sa	1214	1799	5 June	W
1021	1612	23 Feb.	Su	1158	1745	23 Jan.	W	1215	1800	24 May	Su
1031	1621	6 Nov.	Tu	1159	1746	13 Jan.	M	1216	1801	14 May	F
1041	1631	20 July	W	1160	1747	2 Jan.	F	1217	1802	3 May	Tu
1051	1641	2 April	F	1161	1747	22 Dec.	Tu	1218	1803	22 April	M
1061	1650	15 Dec.	Su	1162	1748	11 Dec.	Su	1219	1804	11 April	Th
1071	1660	27 Aug.	M	1163	1749	30 Nov.	Th	1220	1805	31 March	M
1081	1670	11 May	W	1164	1750	19 Nov.	M	1221	1806	20 March	F
1091	1680	23 Jan.	F	1165	1751	9 Nov.	Sa	1222	1807	10 March	W
1101	1689	5 Oct.	Sa	1166	1752	8 Nov.	W	1223	1808	27 Feb.	Su
1111	1699	19 June	M	1167	1753	29 Oct.	M	1224	1809	15 Feb.	Th
				1168	1754	18 Oct.	F	1225	1810	5 Feb.	Tu
1112	1700	7 June	F	1169	1755	7 Oct.	Tu	1226	1811	25 Jan.	Sa
1113	1701	28 May	W	1170	1756	26 Sept.	Su	1227	1812	15 Jan.	Th
1114	1702	17 May	Su	1171	1757	15 Sept.	Th	1228	1813	3 Jan.	M
1115	1703	6 May	Th	1172	1758	4 Sept.	M	1229	1813	23 Dec.	F
1116	1704	25 April	Tu	1173	1759	25 Aug.	Sa	1230	1814	13 Dec.	W
1117	1705	14 April	Sa	1174	1760	13 Aug.	W	1231	1815	2 Dec.	Su
1118	1706	4 April	Th	1175	1761	2 Aug.	Su	1232	1816	20 Nov.	Th
1119	1707	24 March	M	1176	1762	23 July	F	1233	1817	10 Nov.	Tu
1120	1708	12 March	F	1177	1763	12 July	Tu	1234	1818	30 Oct.	Sa
1121	1709	2 March	W	1178	1764	1 July	Su	1235	1819	19 Oct.	W
1122	1710	19 Feb.	Su	1179	1765	20 June	Th	1236	1820	8 Oct.	M
1123	1711	8 Feb.	Th	1180	1766	9 June	M	1237	1821	27 Sept.	F
1124	1712	29 Jan.	Tu	1181	1767	30 May	Sa	1238	1822	17 Sept.	W

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1239	1823	6 Sept.	Su	1296	1878	25 Dec.	Th	1353	1934	14 April	M
1240	1824	25 Aug.	Th	1297	1879	14 Dec.	M	1354	1935	3 April	F
1241	1825	15 Aug.	Tu	1298	1880	3 Dec.	Sa	1355	1936	22 March	Tu
1242	1826	4 Aug.	Sa	1299	1881	22 Nov.	W	1356	1937	12 March	Su
1243	1827	24 July	W	1300	1882	11 Nov.	Su	1357	1938	1 March	Th
1244	1828	13 July	M	1301	1883	1 Nov.	F	1358	1939	19 Feb.	Tu
1245	1829	2 July	F	1302	1884	20 Oct.	Tu	1359	1940	8 Feb.	Sa
1246	1830	22 June	W	1303	1885	9 Oct.	Sa	1360	1941	27 Jan.	W
1247	1831	11 June	Su	1304	1886	29 Sept.	Th	1361	1942	17 Jan.	M
1248	1832	30 May	Th	1305	1887	18 Sept.	M	1362	1943	6 Jan.	F
1249	1833	20 May	Tu	1306	1888	7 Sept.	Sa	1363	1943	26 Dec.	Tu
1250	1834	9 May	Sa	1307	1889	27 Aug.	W	1364	1944	15 Dec.	Su
1251	1835	28 April	W	1308	1890	16 Aug.	Su	1365	1945	4 Dec.	Th
1252	1836	17 April	M	1309	1891	6 Aug.	F	1366	1946	24 Nov.	Tu
1253	1837	6 April	F	1310	1892	25 July	Tu	1367	1947	13 Nov.	Sa
1254	1838	26 March	Tu	1311	1893	14 July	Sa	1368	1948	1 Nov.	W
1255	1839	16 March	Su	1312	1894	4 July	Th	1369	1949	22 Oct.	M
1256	1840	4 March	Th	1313	1895	23 June	M	1370	1950	11 Oct.	F
1257	1841	22 Feb.	Tu	1314	1896	11 June	F	1371	1951	30 Sept.	Tu
1258	1842	11 Feb.	Sa	1315	1897	1 June	W	1372	1952	19 Sept.	Su
1259	1843	31 Jan.	W	1316	1898	21 May	Su	1373	1953	8 Sept.	Th
1260	1844	21 Jan.	M	1317	1899	11 May	F	1374	1954	28 Aug.	M
1261	1845	9 Jan.	F	1318	1900	29 April	Tu	1375	1955	18 Aug.	Sa
1262	1845	29 Dec.	Tu	1319	1901	18 April	Sa	1376	1956	6 Aug.	W
1263	1846	19 Dec.	Su	1320	1902	8 April	Th	1377	1957	27 July	M
1264	1847	8 Dec.	Th	1321	1903	28 March	M	1378	1958	16 July	F
1265	1848	26 Nov.	M	1322	1904	16 March	F	1379	1959	5 July	Tu
1266	1849	16 Nov.	Sa	1323	1905	6 March	W	1380	1960	24 June	Su
1267	1850	5 Nov.	W	1324	1906	23 Feb.	Su	1381	1961	13 June	Th
1268	1851	26 Oct.	M	1325	1907	12 Feb.	Th	1382	1962	2 June	M
1269	1852	14 Oct.	F	1326	1908	2 Feb.	Tu	1383	1963	23 May	Sa
1270	1853	3 Oct.	Tu	1327	1909	21 Jan.	Sa	1384	1964	11 May	W
1271	1854	23 Sept.	Su	1328	1910	11 Jan.	Th	1385	1965	30 April	Su
1272	1855	12 Sept.	Th	1329	1910	31 Dec.	M	1386	1966	20 April	F
1273	1856	31 Aug.	M	1330	1911	20 Dec.	F	1387	1967	9 April	Tu
1274	1857	21 Aug.	Sa	1331	1912	9 Dec.	W	1388	1968	29 March	Su
1275	1858	10 Aug.	W	1332	1913	28 Nov.	Su	1389	1969	18 March	Th
1276	1859	31 July	M	1333	1914	17 Nov.	Th	1390	1970	7 March	M
1277	1860	19 July	F	1334	1915	7 Nov.	Tu	1391	1971	25 Feb.	Sa
1278	1861	8 July	Tu	1335	1916	26 Oct.	Sa	1392	1972	14 Feb.	W
1279	1862	28 June	Su	1336	1917	16 Oct.	Th	1393	1973	2 Feb.	Su
1280	1863	17 June	Th	1337	1918	5 Oct.	M	1394	1974	23 Jan.	F
1281	1864	5 June	M	1338	1919	24 Sept.	F	1395	1975	12 Jan.	Tu
1282	1865	26 May	Sa	1339	1920	13 Sept.	W	1396	1976	2 Jan.	Su
1283	1866	15 May	W	1340	1921	2 Sept.	Su	1397	1976	21 Dec.	Th
1284	1867	4 May	Su	1341	1922	22 Aug.	Th	1398	1977	10 Dec.	M
1285	1868	23 April	F	1342	1923	12 Aug.	Tu	1399	1978	30 Nov.	Sa
1286	1869	12 April	Tu	1343	1924	31 July	Sa	1400	1979	19 Nov.	W
1287	1870	2 April	Su	1344	1925	20 July	W	1401	1980	7 Nov.	Su
1288	1871	22 March	Th	1345	1926	10 July	M	1402	1981	28 Oct.	F
1289	1872	10 March	M	1346	1927	29 June	F	1403	1982	17 Oct.	Tu
1290	1873	28 Feb.	Sa	1347	1928	18 June	W	1404	1983	6 Oct.	Sa
1291	1874	17 Feb.	W	1348	1929	7 June	Su	1405	1984	25 Sept.	Th
1292	1875	6 Feb.	Su	1349	1930	27 May	Th	1406	1985	14 Sept.	M
1293	1876	27 Jan.	F	1350	1931	17 May	Tu	1407	1986	4 Sept.	Sa
1294	1877	15 Jan.	Tu	1351	1932	5 May	Sa	1408	1987	24 Aug.	W
1295	1878	4 Jan.	Sa	1352	1933	24 April	W	1409	1988	12 Aug.	Su

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1410	1989	2 Aug.	F	1414	1993	19 June	M	1418	1997	7 May	F
1411	1990	22 July	Tu	1415	1994	8 June	F	1419	1998	26 April	Tu
1412	1991	11 July	Sa	1416	1995	29 May	W	1420	1999	15 April	Sa
1413	1992	30 June	Th	1417	1996	17 May	Su	1421	2000	4 April	Th

END OF THE SEVENTY-EIGHTH VOLUME OF THE ORIGINAL.

I. Description of an Improvement in the Application of the Quadrant of Altitude to a Celestial Globe, for the Resolution of Problems dependant on Azimuth and Altitude. By Mr. John Smeaton, F. R. S. Anno 1789. *Vol. LXXIX.* p. 1.

The difficulty that has occurred in fixing a semi-circle, so as to have a centre in the zenith and nadir points of the globe, at the same time that the meridian is left at liberty to raise the pole to its desired elevation, Mr. S. supposes, has induced the globe-makers to be contented with the strip of thin flexible brass, called the quadrant of altitude; and it is well known how imperfectly it performs its office. The improvement he has attempted, is in the application of a quadrant of altitude, of a more solid construction; which being affixed to a brass socket of some length, and this ground and made to turn on an upright steel spindle, fixed in the zenith, steadily directs the quadrant, or rather arc, of altitude to its true azimuth, without being at liberty to deviate from a vertical circle to the right hand or left: by which means the azimuth and altitude are given with the same exactness as the measure of any other of the great circles.

With respect to the horary circle, as the common application seems very convenient on account of the ready adjustment of its index to answer the culmination of any of the heavenly bodies; and as a circle of 4 inches diameter is capable of an actual and very distinguishable division into 720 parts, answerable to 2 minutes of time each, which may serve a globe of the largest size; it seems that it should rather be improved than omitted; and if, instead of a pointer, an index stroke is used in the same plane with that of the divisions, the single minutes, and even half minutes, may be readily distinguished. This globe, though mounted merely as a model for experiment, and only 9 inches in diameter, appears capable of bringing out the solution to a quarter of a degree; which may be esteemed sufficient, not only as a check on numerical computation, but to come near enough to find stars in the day-time in the field of telescopes, which, having no equatorial motion, are only capable of direction in altitude and azimuth; but from globes of a larger size, we may expect to come proportionably nearer.

Mr. S. adds a minute description of the several new parts of this globe, which is illustrated with a large plate of the same; not necessary to be reprinted here.

II. Objections to the Experiments and Observations relating to the Principle of Acidity, the Composition of Water, and Phlogiston, considered; with further Experiments and Observations on the same Subject. By the Rev. J. Priestley, LL. D., F. R. S. p. 7.

Having never failed, says Dr. P., when the experiments were conducted with due attention, to procure some acid whenever I decomposed dephlogisticated and inflammable air in close vessels, I concluded that an acid was the necessary result of the union of those 2 kinds of air, and not water only; which is an hypothesis that has been maintained by Mr. Lavoisier and others, and which has been made the basis of an entirely new system of chemistry, to which a new system of terms and characters has been adapted. The facts that I alleged were not disputed; but to my conclusion it was objected, that the acid I procured might come from the phlogisticated air, which in one of my processes could not be excluded; and that it was reasonable to conclude that this was the case, because Mr. Cavendish had procured the same acid, viz. the nitrous, by decomposing dephlogisticated and phlogisticated air with the electric spark. In other cases it has been said, that the fixed air I procured came from the plumbago in the iron from which my inflammable air had been extracted.

With respect to the former of these objections I would observe, that my process is very different from that of Mr. Cavendish; his decomposition being a very slow one by electricity, and mine a very rapid one by simple ignition, a process by which phlogisticated air, as I found by actual trial, was not at all affected; the dephlogisticated and inflammable airs uniting, and leaving the phlogisticated air (as they probably would any other kind of air with which they might have been mixed) just as it was. I would also observe, that there is no contradiction whatever between Mr. Cavendish's experiment and mine, since phlogisticated air may contain phlogiston, and by means of electricity this principle may be evolved, and unite with the dephlogisticated air, or with the acid principle contained in it, as in the process of simple ignition the same principle is evolved from inflammable air, in order to form the same union; in consequence of which, the water, which was a necessary ingredient in the composition of both the kinds of air, is precipitated. That in other circumstances than those in which I made the experiments, the acid wholly escaped, and nothing but water was found, may be easily accounted for, from the small quantity of the acid principle in proportion to the water, and the extreme volatility of it, owing, I presume, to its high phlogistication when formed in this manner.

In order to ascertain the effect of the presence of phlogisticated air in this process, I now not only repeated the experiment of mixing a given quantity of phlogisticated air with the 2 other kinds of air, and found, as before, that it was not affected by the operation; but I made the experiment with atmospheric air, instead of dephlogisticated. Since the air of the atmosphere contains a greater proportion of phlogisticated air, it might be expected that, if the acid I got before came from the small quantity of phlogisticated air which I could not possibly exclude, I should certainly get more acid when, instead of endeavouring to exclude it, I purposely introduced a greater quantity. But the consequence was the production of much less acid than before, the liquor I procured being sometimes not to be distinguished from pure water, except by the greatest attention possible: for though the decomposition was made in the same copper vessel which I used in the former experiments, there was now no sensible tinge of green colour in it.

When I repeated this experiment in a glass vessel, I perceived, as I imagined, the reason of the small produce of acid in these new circumstances: for the vessel was filled with a vapour which was not soon condensed, and being diffused through the phlogisticated air, (which is not affected by the process) is drawn away along with it, when the exhausting of the tube is repeated; whereas, when there is little or no air in the vessel besides the 2 kinds that unite with each other, and are decomposed, the acid vapour, having nothing to attach itself to and support it (by being entangled with it) much sooner attacks the copper, making the deep green liquor which I have described. Sometimes however I have procured a liquor which was sensibly green by the decomposition of atmospheric and inflammable air, but by no means of so deep a colour, or so sensibly acid, as when the dephlogisticated air is used.

The extreme volatility of the acid thus formed (and which accounts for the escape of some part of it in all these processes) is apparent from this circumstance, that if the explosions be made in quick succession (the tube being exhausted immediately after each of them, and filled again as soon as possible) no liquor at all will be collected, the whole of the acid vapour, together with the water with which it was combined, being drawn off uncondensed in every process. I once made 20 successive explosions of this kind, in a copper tube, out of which I found that I drew 37 oz. measures of air by the action of the pump, and found not a single drop of liquid, though near an hour was employed in the whole process, and the vessel was never made more than a little warmer than my hand. This was a degree of heat by no mean sufficient to keep the whole of any quantity of water in a state of vapour; and is a circumstance which of itself sufficiently proves, that the vapour did not consist of water only.

Indeed, I think it impossible for any one to see this vapour in a tall glass vessel, and especially to observe how it falls from one end of it to the other, and the time that is required to its wholly disappearing, without being satisfied that it consists of something else than mere water, the vapour of which would be more equally diffused. If the appearance to the eye should fail to convince any person of this, the sense of smell would do it: for even in a glass vessel it is very offensive, though it might not be pronounced to be acid. I conjecture however that this, and every other species of smell, is produced by some modification of the acid or alkaline principle. Some may be disposed to ascribe this smell to the iron from which the inflammable air was produced; but the smell is the same, or nearly so, when the air is from tin, and would probably be the same if it were from any other substance. Besides using atmospheric air, which contains a greater proportion of phlogisticated air, I have sometimes used dephlogisticated air which was not very pure; and in this case I have always observed, that the liquor I procured had less colour, and was less sensibly acid. These observations might, I should think, satisfy any reasonable person, that the acid liquor which I procured by the explosion of dephlogisticated and inflammable air in close vessels did not come from the phlogisticated air which could not be excluded, whether it was that which remained in the vessel after exhausting it by the air-pump, or that with which the dephlogisticated air was more or less contaminated.

But besides these experiments, in which I procured the green acid liquor by the explosion of dephlogisticated and inflammable air in close vessels, I made another, to which I thought the same objection could not have been made, because no air-pump was used in it, and nothing but the purest dephlogisticated air was employed, being separated in the process from precipitate per se in contact with the purest inflammable air in a glass vessel which had been previously filled with mercury. Accordingly, the only objection made to this experiment was, that the preparation used might be impure, containing something which might yield phlogisticated air. This appeared to me highly improbable, as the precipitate had been made by M. Cadet, and for the purpose of philosophical experiments. Besides, if the heat of a burning lens should dislodge phlogisticated air from any unperceived impurity in this preparation, mere heat will not decompose this air. Let any person try the effect of a lens on such air, or any substance containing it, and produce an acid if he can.

M. Berthollet however, thinking that this might be the case, desired that I would send him a specimen of my precipitate per se. Accordingly, I sent him all that remained of it; and in return he sent me a quantity on the goodness of which I might depend. With this preparation I repeated my former experiment; and, by giving more attention to the process, found it to be far more decisively conclusive in favour of my opinion than I had imagined. In the former experiment I had attended only to the drop of water which was found in the vessel in which the process was made; and finding that it turned the juice of turnsole red, I concluded that it contained nitrous acid: but I now examined the air that remained in the vessel, and found that a considerable proportion of it was fixed air; so that I am now satisfied this was the acid with which it was impregnated, and not the nitrous. Still however some acid is the constant result of the union of the two kinds of air, and not water only. A quantity of the same precipitate per se yielded no fixed air by heat. Comparing this experiment with that in which iron is ignited in dephlogisticated air, this general conclusion may be drawn, viz. that when either inflammable or dephlogisticated air is extracted from any substance in contact with the other kind of air, so that one of them is made to unite with the other in what may be called its nascent state, the result will be fixed air; but that if both of them be completely formed before their union, the result will be nitrous acid.

It has been said, that the fixed air produced in both these experiments may come from the plumbago in the iron from which the inflammable air is obtained. But since we ascertain the quantity of plumbago contained in iron by what remains after its solution in acids, it is in the highest degree improbable, that whatever plumbago there may be in iron, any part of it should enter into the inflammable air procured from it. Besides, according to the antiphlogistic hypothesis, all inflammable air comes from water only. As it cannot be said, that any real fixed air is found in inflammable air from iron (since it is not discoverable by lime-water) it must be supposed, that the elements, or component parts of fixed air are in it; but one of these elements is pure air, and the mixture of nitrous air shows that it contains no such thing, though, according to M. Lavoisier, fixed air contains 72 parts in 100 of pure air.

However, being apprized of this objection to inflammable air from iron, I made use of inflammable air from tin, and I had the same result as with that from iron. I also calculated the weight of the fixed air which I got in the process, and comparing it with the plumbago which the iron necessary to make the inflammable could have contained, I found that in all the cases it far exceeded the weight of the plumbago; so that it was absolutely impossible, that the fixed air which I found should have had this origin. For the greater satisfaction, Dr. P. recites the particulars of a few experiments of this kind. But, for these, we must refer to a separate pamphlet which Dr. P. published on this subject.

III. Observations on the Class of Animals called, by Linnæus, Amphibia; particularly on the Means of Distinguishing those Serpents which are Venomous, from those which are not so. By Edward Whitaker Gray, M. D., F. R. S. p. 21.

Of the various classes of the animal kingdom, says Dr. G., no one has been so little attended to as the class, called by Linnæus, Amphibia. What he himself did in that class, though far superior to what any other person has done, was evidently done in a hurry; false references are at least as common in that as in any other part of his works, and many of his descriptions are given in a very careless manner; there are others however which are truly worthy of their author, and in which the specific characters are pointed out with that clearness and precision, which so eminently distinguish the descriptions of Linnæus from those of all his predecessors.

In the construction of the class, Linnæus has been particularly unfortunate; as he has erred, not only in making a unilocular heart one of the characters of it, but also in making the cartilaginous fishes a part of it. Every anatomist now agrees that the Amphibia Nantes are not furnished with lungs; and every naturalist is convinced of the propriety of removing them from the class of amphibia, to that of fishes. By the removal, the name of the class becomes much less objectionable; there being few genera, in the 2 orders of which it is now presumed to consist, which do not contain animals to which the term amphibious may, with some propriety, be given; whereas, in the order of Nantes, not one species occurs which has the smallest claim to that title. With respect to the other error, viz. that of supposing the hearts of the amphibia to be single, it would be easy to show that it was not an uncommon one, at the time Linnæus formed his system. And indeed it appears he was led into it, by following an author whom he probably supposed of too great fame not to be safely relied on. At least, in defence of his opinion, he quotes the following words of Boerhaave. "In omnibus animalibus in quibus sanguis non calet, ventriculus cordis est unicus." It is sufficient for the purpose to observe, that the hearts of most of the amphibia are now well known to be double, with an immediate communication between the 2 cavities; which structure seems peculiarly adapted to that change of element, which many of them can for a time support; and thereby furnishes another argument in favour of the name Linnæus has given to the class. To consider the structure of the heart however is not absolutely necessary in forming the characters of the class: the animals of which it consists being sufficiently distinguished from all others, by having cold red blood, and breathing by means of lungs. These 2 characters render the class perfectly distinct from the rest; the 2 superior ones, viz. mammalia and birds, having warm blood; and the 3 inferior ones, fishes, insects, and worms, not being furnished with lungs.

In his generic characters, Linnæus has been more successful than in those of the class. Whoever will be at the pains of comparing Linnæus's genera of amphibia with those of Gronovius, will find, that the generic characters of the former, though few in number, are precise and distinct; while those of the latter, though more numerous, are vague, indistinct, and sometimes inaccurate. But though Linnæus's genera of amphibia are generally well-formed, it must be allowed to be a great imperfection in them, that the venomous serpents are not separated from the others. From some expressions in the Preface to the Museum Regis, and in the introduction to the class amphibia, in the Systema Naturæ, it seems that he thought it not easy to distinguish them by any external characters; and his ideas respecting the venomous fangs themselves were so vague and confused, that it was hardly possible for him to attempt to found a generic distinction on them.

Whether venomous serpents can be with certainty distinguished from others, and if so, how they are to be known, is what is meant to be considered in this paper; in doing which Dr. G. examines, first, how far they may be distinguished by any external characters; 2dly, supposing the venomous fangs to be the only certain criterion, how those fangs are to be distinguished from common teeth. Though serpents, by their internal organization, naturally belong to the 3d class of the animal kingdom, they are in their external form more simple than most of the animals belonging to the 3 inferior classes; their external characters must consequently be very few.

In the 1st genus, crotalus, the head is broader than the neck, depressed or flat at top, and covered with small scales. These 3 characters are particularly observable in the 3 intermediate species horridus, dryinas, and durissus. In the miliarius the scales of the head are rather larger than in the others. The mutus certainly should not be placed among the crotali. As all the species of this genus are venomous, one is naturally led, by the examination of it, to consider the fore-mentioned characters as being, in some measure, proper to venomous serpents. In the genus coluber are many venomous species, and it is very certain that in general they have the fore-mentioned characters; examples of which may be seen in the atropos, cerastes, atrox, berus, and others. It is however equally certain that there are some in which they are not to be found. For example the naja, a species well known to be very venomous; the head of which is neither depressed nor broad, is covered with large scales, and is in every respect a complete exception to what has been said respecting the heads of venomous serpents. Since then there are venomous serpents in which the fore-mentioned characters, viz. a broad and depressed head, covered with small scales, are not to be found; let us examine whether those characters are to be found in any of those serpents which are not venomous. In the genus coluber there are very few, except venomous ones, which have the head much broader than the neck; and of those few every one has the head covered with large scales. But in the genus boa, though no species is venomous, except the contortrix, almost every one has the head broad, depressed, and covered with small scales. The canina, constrictor, hortulana, besides some others not described by Linnæus, furnish examples of this.

In the crotali Dr. G. has never found the tail, exclusive of the rattle, to exceed $\frac{1}{9}$ part of the whole length; sometimes it is much shorter. In some of the venomous colubri, the proportion is still less. In the atropos it is found only $\frac{1}{13}$. In the English viper, coluber berus, it is commonly about $\frac{1}{7}$ or $\frac{1}{8}$. In some venomous species however the proportion is something greater. In the naja it is sometimes $\frac{1}{6}$; which proportion is, he thinks, as great as he has ever observed: at any rate he never met with a venomous serpent the tail of which was equal to $\frac{1}{5}$ of the whole length. With respect to those colubri which are not venomous, there are many whose tails are within the limits assigned to the venomous ones. In the coluber Æsculapii, doliatus, getulus, and some others, the tail is not generally more than $\frac{1}{7}$ of the whole length. In the lemniscatus it sometimes does not exceed $\frac{1}{12}$ or $\frac{1}{13}$, but perhaps in no other Linnæan species it is so short. In the greater number however the proportion of tail is more considerable; in many it is full $\frac{1}{3}$. In the ahætulla, and in some species not described by Linnæus, it is sometimes more than $\frac{2}{5}$; but never quite so long as the trunk, or half of the whole length. None of the Linnæan species, of the Boæ, have their tails either remarkably long, or short; but in 2 species, not described by Linnæus, Dr. G. has found the tail very little exceeding the proportion here assigned to the coluber lemniscatus. In the thickness of the tail, or in the acuteness of its termination, he has observed no difference worth remarking. In every species of the first 3 genera, the tail is thinner than the trunk; and in most of them it is more or less acute. The few exceptions he has observed were none of them venomous; but they are too few to deserve any particular consideration.

A character of great use in distinguishing the species of serpents, and which was not overlooked by Linnæus, is that elevated line, or carina, with which the scales of many species are furnished. In order to show how far this is to be considered as serving to distinguish venomous serpents from others, Dr. G. has examined 112 species of serpents, not venomous, belonging to the first 3 genera; and found that 80 of them have smooth scales, and 32 only have carinated ones. Of venomous serpents he has examined 26; of which number, 20 have carinated scales, and only 6 have smooth ones. On the whole therefore, carinated scales must be considered as being, in some measure, a character of venomous serpents.

The other 3 genera, anguis, amphisbæna, and cæcilia, besides the characters assigned them by Linnæus, have some others which are common to all, and which render them very different, in their external appearance, from any of the first 3 genera. These are, a very thick and obtuse tail, and a head which is very indistinct, and furnished with very small eyes. This last character is sometimes, though very rarely, met with among the colubri, for instance, in the lemniscatus; in the last 3 genera however it takes place without exception. The thickness of the tail is also common to every species; and though in the anguis bipes, and in another species, not described by Linnæus, but figured in Browne's History of Jamaica (Tab. 44, fig. 1,) the tail has an acute termination, yet in both these species, especially in the last, it continues thick to the end, and becomes suddenly sharp, being what in botanical language would be called, obtusa cum acumine. With respect to the proportionate length of tail however, it is very remarkable that the genus anguis affords examples of much less proportion, and also of much greater, than is to be found in any of the first 3 genera. In the anguis scytale the tail is not above $\frac{1}{20}$ of the whole length; in the maculata it is not above $\frac{1}{40}$; yet in the anguis fragilis, and in the ventralis, the tail is always longer than the trunk, or is more than half the whole length. Indeed, in one specimen of the last-mentioned species, Dr. G. found the tail nearly $\frac{2}{3}$ of the whole length. It may however be questioned whether that species is really an anguis, or a lacerta.

The principal inferences to be deduced from those remarks, are the following: 1st. That a broad head, covered with small scales, though it be not a certain criterion of venomous serpents, is, with some few exceptions, a general character of them. 2dly, That a tail under $\frac{1}{5}$ of the whole length is also a general character of venomous serpents; but since many of those which are not venomous have tails as short, little dependance can be placed on that circumstance alone. On the other hand, a tail exceeding that proportion is a pretty certain mark that the species, to which it belongs, is not venomous. 3dly. That a thin and acute tail is by no means to be considered as peculiar to venomous serpents; though a thick and obtuse one is only to be found among those which are not venomous. 4thly. That carinated scales are, in some measure, characteristic of venomous serpents, since in them they are more common than smooth ones, in the proportion of nearly 4 to 1; whereas smooth scales are, in those serpents which are not venomous, more common, in the proportion of nearly 3 to 1.

On the whole therefore it appears, that though a pretty certain conjecture may, in many instances, be made from the external characters; yet, in order to determine with certainty whether a serpent be venomous or not, it becomes necessary to have recourse to some more certain diagnostic. This can only be sought for in the mouth. To those who form their ideas of the fangs of venomous serpents from those of the rattle-snake, or even from those of the English viper, it will appear strange that there should be any difficulty in distinguishing those weapons from common teeth; and indeed the distinction would really be very easy were all venomous serpents furnished with fangs as large as those of the fore-mentioned species. But the fact is, that in many species the fangs are full as small as common teeth, and consequently cannot, by their size, be known from them; this is the case with the coluber laticaudatus, lacteus, and several others.

Linnæus thought the fangs might be distinguished by their mobility; this, at least, may be fairly inferred, from his never mentioning them in the Museum Regis, without adding the epithet mobilia, except in one instance, the coluber aulicus. But with regard to mobility, considered in general as a character of venomous fangs, Dr. G. has not only never found it so, but he has also never been able to discover in them any thing which could properly be called mobility. He has indeed sometimes found some of them loose in their sockets; but then he has found others, in the same specimen, quite fixed. The same thing was observed both by Dr. Nicholls, and by the Abbé Fontana, in the common viper, even during life. The loose fangs may be such as have not yet been firmly fixed in their socket, or they may have been loosened by some accident: for the fangs may be at any time loosened, and even displaced, by a small degree of violence; which perhaps may be one reason why there is always a certain number of small fangs, near the base of the full grown ones, ready to enlarge and take their place, if they should be by any accident torn out.

Linnæus seems also to have thought that the fangs might be known by their situation. In the Introduction to the class Amphibia in the Systema Naturæ, he says they are, "Dentibus simillima sed extra maxillam superiorem collocata;" and in the description of the crotalus dryinas, in the Amœnitates Academicæ, he says, "Dentes ejus duo canini uti in reliquis venenatis serpentibus non in maxillis hærent, iis enim vulnerando, non autem ictus infligendo utitur." These 2 quotations show that Linnæus thought the situation of the fangs different from that of the common teeth; the last also shows that he thought their mode of action influenced by it. But the most singular opinion of Linnæus, respecting the venomous fangs, was, that they were sometimes fixed in the base of the jaw. Of this he has given 2 instances in the Museum Regis. One in the description of the coluber severus, of which he says, "Hastæ mobiles solitariæ versus basin maxillarum interius adhærent." The other in that of the coluber stolatus. His words there are, "Tela mobilia ad basin maxillarum affixa, ut vix vulnerare valeat hostes, solum cibos veneno inficere."

With respect to their size, it has already been observed that it is very various, consequently no certain judgment can, in all cases, be made from that circumstance. In some species they are so large, that their size alone sufficiently distinguishes them from common teeth; but in others they are so small, that it is very difficult to discover them. The size of the common teeth also varies very much, in different species. In the coluber mycterizans they are remarkably large, especially those which are situated near the apex of the upper jaw; which circumstance probably helped to lead Linnæus into the erroneous opinion he entertained, that this serpent was venomous. But in many species the teeth are so small, that it is impossible to discover, merely by looking into the mouth, that the animal has any. Yet in that case they may be very easily detected, by drawing a pin, or any other hard substance, with a moderate degree of pressure, along the edge of the jaw, from the apex to the angle of the mouth, when they will be felt to grate against the pin, like the teeth of a saw.

Though the size of the venomous fangs is very various, their situation is always the same; namely, in the anterior and exterior part of the upper jaw, which situation he considers as the only one in which venomous fangs are ever found. But as, in those serpents which are not venomous, common teeth are found in that part of the jaw, it is plain that we cannot, by situation alone, distinguish one from the other. They may however be easily distinguished, and with great certainty, by the following simple operation. When it is discovered that there is something like teeth in the fore-mentioned part of the upper jaw, let a pin be drawn, in the manner described, from that part of the jaw to the angle of the mouth; which operation may for greater certainty be tried on each side. If no more teeth are felt in that line, it may be certainly concluded, that those first discovered are what have been distinguished by the name of fangs, and consequently that the serpent is a venomous one. If, on the contrary, the teeth first discovered are found not to stand alone, but to be only a part of a complete row, it may as certainly be concluded that the serpent is not venomous.

In the upper jaw, both of venomous serpents and others, besides the teeth already spoken of, there are 2 interior rows; consequently the distinction endeavoured to be established might be expressed in other words, by saying, that all venomous serpents have only 2 rows of teeth in the upper jaw, and all others have 4. It may be better however to leave the interior rows out of the question, as, in many species, the teeth of which they are composed are so small, as to make it very difficult to discover them. Indeed, in 2 species of anguis, Dr. G. can hardly be sure that he has discovered them; but as, in every other species he has never failed to do so, with very little risk of error he may assert, that all serpents whatever are furnished with them; and that those only which are not venomous have the exterior rows.

What has been said sufficiently shows that Linnæus's ideas, respecting venomous serpents, were such as did not permit him to separate them from the others; if the method above proposed shall be found to render the distinction of them sufficiently clear and easy, it naturally follows that they should be made generically distinct. Some other reforms might also be made in Linnæus's class of amphibia, the consideration of which Dr. G. does not mean at present to enter further into. But, before concluding, he thinks it necessary to notice an inaccuracy of Linnæus of a different kind from those above pointed out. In the Preface to the Museum Regis, and in the Introduction to the class amphibia, in the Systema Naturæ, Linnæus says, that the proportion of venomous serpents to others, is 1 in 10; yet in the Systema Naturæ, in which the sum total of species is 131, he has marked 23 as venomous, which is somewhat more than 1 in 6. However, the last mentioned proportion seems to be not far from the truth; as out of 154 species of serpents Dr. G. examined, he finds 26 to be venomous. It has already been mentioned, that the coluber stolatus and the mycterizans, though marked by Linnæus as venomous serpents, certainly are not so; and Dr. G. suspects the same may be said of the leberis, and dipsas. He has also observed, that the boa contortrix, coluber cerastes, and laticaudatus, none of which are marked in the Systema Naturæ, are all of them venomous; to these last may be added the coluber fulvus.

IV. On the Dryness of the Year 1788. *By the Rev. B. Hutchinson.* p. 37.

As the defect of rain has been very considerable in 1788; and in consequence a great want of water on the close of the year universally felt; perhaps the quantity fallen here, at Kimbolton, compared with that of the 7 preceding years, may not be unacceptable to the Royal Society.

Rain in	Inches.
1781	21.6
1782	32.3
1783	23.6
1784	28.0
1785	21.0
1786	24.7
1787	23.8
1788	14.5

By estimation it therefore appears, that the average quantity of rain of the 7 preceding years is 25 inches, and the rain which fell last year is only 14.5, that is, not much above half that quantity, if we deduct 1.3 now lying in snow, which fell in December, and not in solution. On the supposition, which he believes is not far from truth, that the whole island has had the same defect; a greater failure of the produce of the earth might have been expected than what the country has experienced; for, except in hay, and a little failure in turneps, the crops have in general been as plentiful as in most of the former years, and in fruits of the orchard much more so. It has always been said of England, that drought never occasions want; this year verifies the assertion.

Having premised that there were no extremes of cold and heat throughout the year; the thermometer in a northern exposure never falling below the freezing point during the day-time, except on the 14th and 15th of January, the 6th, 7th, 8th, 10th, 11th, 12th, 13th, and 17th of March, and on none of those days at noon, so that there never were 24 hours together successive frost; therefore vegetation was never entirely at a stand. In summer it did not rise to 80°, except on some few days. Now, the rain that fell on February was towards the end of the month; which, together with that which fell in March, brought up the spring corn, gave an early first crop of hay to the large towns, and covered the meadows and pastures in the country; that they were not so entirely dried up through the defect of April, as to prevent the rain, which fell plentifully on the 29th of May, succeeded by more in June, giving a 2d crop to the former situations, and a first, though late one, to the latter: and as fructification chiefly depends on rain falling at the latter end of the season of flowering, this rain set the blossoms of wheat, and of the useful fruit-trees; as the great rains in August swelled the kernel, filled, as they term it, the bushel, and gave an opportunity for a 2d crop of turneps that proved more vigorous than the first.

V. On the Method of Determining, from the Real Probabilities of Life, the Value of a Contingent Reversion in which Three Lives are involved in the Survivorship. By Mr. Wm. Morgan. p. 40.

In a paper which Mr. M. lately communicated to the R. S. respecting the method of determining the values of reversions depending on survivorships between 2 persons from the real probabilities of life, he observed, that the investigation of those cases in which 3 lives were involved in the survivorship, though attended with much more difficulty, might however be effected in a similar manner. The further pursuit of this subject convinced him that, as it is never safe, so it can never be necessary to have recourse to the expectations of life in any case; and that the solution even of those problems which include 3 lives, is far from being so formidable as at first sight it appears to be. Mr. M. is sensible of the impropriety of entering minutely in this place into the vast variety of propositions which refer to the different orders of survivorship among 3 lives; but as the following problem seemed to be of considerable importance on account of its being applied to the solution of many other problems, the demonstration of it might not be thought an improper addition to his former paper. The problem is this: Supposing the ages of A, B, and C to be given; to determine, from any table of observations, the value of the sum S payable on the contingency of C's surviving B, provided the life of A shall be then extinct. Of this prob. Mr. M. gives a long and elaborate algebraic investigation. But, for all useful purposes, it may suffice to refer to Mr. Morgan's separate works, as well as to those of Dr. Price, whence ample satisfaction may be obtained.

VI. Result of Calculations of the Observations made at various Places of the Eclipse of the Sun, which happened June 3, 1788. By the Rev. Joseph Piazzi, C. R., Prof. of Astronomy at Palermo.* p. 55.

The methods of computing the longitudes of places, from the observations of solar eclipses, are well known. This M. Piazzi has here undertaken to do for several different places, from which he has collected the observations made on the above mentioned eclipse.

The observations of Loampit-Hill, Greenwich, and Oxford, as they serve for the basis of all his calculations, Mr. P. has calculated them 2 different ways, viz.

* The celebrated discoverer of one of the lately found planets.

by the method of parallactic angles, and by the method of the nonagesimal, and the results agreed together within a few 10ths of a second. By these 2 different methods he also calculated the observations of Vienna, Berlin, and Viviers, in order to show that the different latitudes of the moon, given by the various ob servations, were not owing to any error in his calculations. For these places, in which both the beginning and the end of the eclipse have been observed, he deduced the time of conjunction from the 2 phases conjointly, which have also given the duration of the eclipse, which cannot be obtained from a single observation.

The error of the tables which results from the observation at Greenwich is + 26″ in longitude, and + 11″.5 in latitude, at 20^h 58^m $47^s.3$ of apparent time, taking for the longitude of the sun 2^s 14° 16′ 54″.7, as deduced from the Nautical Almanac, and that of the moon at the same time to be greater than the sun by 26″, as deduced from the same Almanac. He supposed also the horary motion of the moon in the ecliptic, by taking it a half hour before and after the conjunction, to be 36′ 52″ + 0″.6 for the hour following the conjunction, and — 0″.6 for the hour preceding the conjunction; the moon's horary motion in latitude is 3′ 24″.3; the horizontal parallax of the moon minus that of the sun at Greenwich, to be 60′ 14″.4 for the commencement of the eclipse, and 60′ 16″.4 for the end; the sun's diameter 31′ 34″.6, less by 3″ than that given in the Almanac, according to the correction which Dr. Maskelyne found necessary to be made; the moon's diameter is stated as in the Almanac. In the opinion of M. de la Lande, some correction ought to be made to the parallax and to the diameter of the moon, as well as to the diameter of the sun; but on the one hand this would not make any alteration in the difference of the meridians which Mr. P. has found; and on the other he thought proper to make use of those elements the Nautical Almanac furnished, that being a work the most perfect of the kind that ever appeared, and to which all astronomers and navigators ought to pay the greatest attention.

In fine, he compared the moon's longitude in conjunction deduced from the eclipse with the new tables of the moon corrected by Mr. Mason, and found the longitude by those tables to be 2^s 14° 17′ 6″.4, and the latitude to be 15′ 1″.3. The error then of the new tables is + 11″.7 in longitude, and + 13″.1 in latitude; but M. de la Lande having lately sent to him from Paris the place of the sun, calculated with the new solar tables (a most useful improvement which M. de Lambre has, with much ingenuity, deduced from observations) he finds the error in longitude to be + 27″.4, the sun's place being 2^s 14° 16′ 39″.0 at 20^h 58^m $47^s.3$.

The following table contains the observations of the eclipse, and the results deduced from them. The first vertical column shows the name and place of the observers; the next 2 vertical columns contain all those observations which have been made, in apparent time; the other columns show the results, viz. the 4th column contains the true conjunction in apparent time; the 5th column contains the latitude of the moon, which, as it depends on the manner of observing the 2 phases, is subject to some variety; the 6th or last column contains the difference between the various meridians and that of Greenwich.

Table of the Observations made at Various Places on the Eclipse of the Sun, which happened June 3, 1788, and of results deduced from the same. Longitude of the Moon in Conjunction being 2ˢ 14° 16′ 54″.7.

Observers.	Beginning.	End.	Conjunction	Latitude in conjunction.	Difference of meridians.
Greenwich, Dr. Maskelyne	19ʰ 24ᵐ 46ˢ.5	21ʰ 1ᵐ 24ˢ.0	20ʰ 58ᵐ 47ˢ.3	14′ 48″.2	0ʰ 0ᵐ 0ˢ
Loampit-Hill, Mr. Aubert........	19 24 41 .9	21 1 20 .3	20 58 44 .1	14 48 .2	0 0 3 .2W.
Oxford, Dr. Hornsby............	19 20 36 .1	20 54 40 .0	20 53 46 .2	14 48 .7	0 5 1 .1W.
Dublin, Dr. Ussher..............	19 5 46 .5	20 27 42 .1	20 33 33 .9	14 48 .3	0 25 13 .4W.
Mittau, M. Beitler..............	21 20 15 .0	23 8 52 .0	22 33 41 .5	14 48 .7	1 34 54 .2E.
Berlin, M. Bode.................	20 23 9 .0	22 14 32 .0	21 52 20 .3	14 44 .2	0 53 33 E.
Vienna, M. Triesneker	20 25 49 .0	22 32 40 .0	22 4 18 .8	14 39 .0	1 5 31 .5E.
Viviers, M. Flaugerguas..........	19 26 38 .0	21 25 41 .0	21 17 29 .0	14 33 .0	0 18 41 .7E.
Perinaldo, M. Maraldi............	19 37 50 .0	*	21 29 40	*	0 30 53 .0E.
Rouen, M. Du Lange............	*	21 7 15 .0	21 3 9 .6	*	0 4 22 .3E.
Milan, Mess. De Cesaris and Reggio	19 48 23 .0	21 51 14 .0	21 35 24 .7	14 2 .0	0 36 37 .4E.
Bologna, M. Matteucci	19 55 10 .5	22 3 45 .5	21 44 15 .3	14 31 .0	0 45 28 E.
Padua, M. Chiminello............	19 59 20 .0	22 6 58 .0	21 46 21 .3	14 39 .0	0 47 34 E.
Warsaw, M. Bystrzyski..........	20 56 45	22 57 33	22 22 59 .3	14 44	1 24 12
Prague, M. Strnadt..............	20 21 29	22 21 15	21 56 30	14 45	0 57 42 .7
Marseilles, M. Bernard	19 26 42	21 29 23 .5	21 20 17 .5	14 40	0 21 30 .2
Cresmunster, M. Fixlmillner......	20 15 20	22 19 50 .7	21 54 59	14 23	0 56 11 .7
Bagdad, M. De Beauchamp	22 30 51	23 26 19	23 56 11	*	2 57 23 .7

VII. Of a Bituminous Lake or Plain in the Island of Trinidad. By Mr. Alex. Anderson. p. 65.

A most remarkable production of nature in the island of Trinidad, is a bituminous lake, or rather plain, known by the name of Tar Lake; by the French called La Bray, from the resemblance to, and answering the intention of, ship pitch. It lies in the leeward side of the island, about half way from the Bocas to the south end, where the Mangrove swamps are interrupted by the sand-banks and hills; and on a point of land which extends into the sea about 2 miles, exactly opposite to the high mountains of Paria, on the north side of the Gulf. This cape, or head-land, is about 50 feet above the level of the sea, and is the greatest elevation of land on this side of the island. From the sea it appears a mass of black vitrified rocks; but on a close examination it is found a composition of bituminous scoriæ, vitrified sand, and earth, cemented together; in some parts beds of cinders only are found. In approaching this Cape, there is a strong sulphureous smell, sometimes disagreeable. This smell is prevalent in many parts of the ground to the distance of 8 or 10 miles from it.

This point of land is about 2 miles broad, and on the east and west sides, from the distance of about half a mile from the sea, falls with a gentle declivity to it, and is joined to the main land on the south by the continuation of the Mangrove swamps; so that the bituminous plain is on the highest part of it, and only separated from the sea by a margin of wood which surrounds it, and prevents a distant prospect of it. Its situation is similar to a Savannah, and, like them, it is not seen till treading on its verge. Its colour, and even surface, present at first the aspect of a lake of water; but probably it got the appellation of lake when seen in the hot and dry weather, at which time its surface to the depth of an inch is liquid, and then from its cohesive quality it cannot be walked on. It is of a circular form, and about 3 miles in circumference. At the first approach it appeared a plane, as smooth as glass, excepting some small clumps of shrubs and dwarf-trees that had taken possession of some spots of it; but when Mr. A. had proceeded some yards on it, he found it divided into areolæ of different sizes and shapes: the chasms or divisions anastomosed through every part of it; the surface of the areolæ perfectly horizontal and smooth; the margins undulated, each undulation enlarged to the bottom till they join the opposite. On the surface the margin or first undulation is distant from the opposite from 4 to 6 feet, and the same depth before they coalesce; but where the angles of the areolæ oppose, the chasms or ramifications are wider and deeper. When he was at it, all these chasms were full of water, the whole forming one true horizontal plane, which rendered the investigation of it difficult and tedious, being necessitated to plunge into the water a great depth in passing from one areola to another. The truest idea that can be formed of its surface will be from the areolæ and their ramifications on the back of a turtle. Its more common consistence and appearance is that of pit-coal, the colour rather greyer. It breaks into small fragments, of a cellular appearance and glossy, with a number of minute and shining particles interspersed through its substance; it is very friable, and, when liquid, is of a jet black colour. Some parts of the surface are covered with a thin and brittle scoria, a little elevated. As to its depth he could form no idea of it; for in no part could he find a substratum of any other substance; in some parts he found calcined earth mixed with it.

Though he smelt sulphur very strong on passing over many parts of it, he could discover no appearance of it, or any rent or crack through which the steams might issue; probably it was from some parts of the adjacent woods: for though sulphur is the basis of this bituminous matter, yet the smells are very different, and easily distinguished, for its smell comes the nearest to that of pitch of any thing. He could make no impression on its surface without an axe: at the depth of a foot he found it a little softer, with an oily appearance, in small cells. A little of it held to a burning candle makes a hissing or cracking noise like nitre, emitting small sparks with a vivid flame, which extinguishes the moment the candle is removed. A piece put in the fire will boil up a long time without suffering much diminution: after a long time's severe heat, the surface will burn and form a thin scoria, under which the rest remains liquid. Heat seems not to render it fluid, or occupy a larger space than when cold; whence he imagines there is but little alteration on it during the dry months, as the solar rays cannot exert their force above an inch below the surface. He was told by one Frenchman, that in the dry season the whole was a uniform smooth mass; and by another, that the ravins contained water fit for use during the year; but can believe neither: for if, according to the first assertion, it was an homogeneous mass, something more than an external cause must affect it, to give it the present appearances: nor without some hidden cause can the 2d be granted. Though the bottoms of these ramified channels admitted not of absorption, yet from their open exposure, and the black surface of the circumjacent parts, evaporation must go on amazing quick, and a short time of dry weather must soon empty them; and from the situation and structure of the place there is no possibility of supply but from the clouds. To show that the progress of evaporation is amazingly quick here, at the time he visited it there were, on an average, $\frac{2}{3}$ of the time incessant torrents of rain; but from the afternoon being dry, with a gentle breeze, as is generally the case during the rainy season in this island, there evidently was an equilibrium between the rain and the evaporation; for in the course of 3 days he saw it twice, and perceived no alteration on the height of the water, nor any outlet for it but by evaporation.

Mr. A. takes this bituminous substance to be the bitumen asphaltum Linnei. A gentle heat renders it ductile; hence, mixed with a little grease or common pitch, it is much used for the bottoms of ships, and for which intention it is collected by many, and he conceives it a preservative against the Borer, so destructive to ships in this part of the world. Besides this place, where it is found in this solid state, it is found liquid in many parts of the woods; and at the distance of 20 miles from this about 2 inches thick, round holes of 3 or 4 inches diameter, and often at cracks or rents. This is constantly liquid, and smells stronger of tar than when indurated, and adheres strongly to any thing it touches; grease is the only thing that will divest the hands of it.

The soil in general, for some distance round La Bray, is cinders and burnt earths; and where not so, it is a strong argillaceous soil; the whole exceedingly fertile, which is always the case where there are any sulphureous particles in it. Every part of the country, to the distance of 30 miles round, has every appearance of being formed by convulsions of nature from subterraneous fires. In several parts of the woods are hot springs; some he tried, with a well graduated thermometer of Fahrenheit, were 20° and 22° hotter than the atmosphere at the time of trial. From its position to them, this part of the island has certainly experienced the effects of the volcanic eruptions, which have heaped up those prodigious masses of mountains that terminate the province of Paria on the north; and no doubt there has been, and still probably is, a communication between them. One of these mountains opposite to La Bray in Trinidad, about 30 miles distant, has every appearance of a volcanic mountain: however, the volcanic efforts have been very weak here, as no trace of them extend above 2 miles from the sea in this part of the island, and the greater part of it has had its origin from a very different cause to that of volcanos; but they have certainly laid the foundation of it, as is evident from the high ridge of mountains which surrounds its windward side to protect it from the depredations of the ocean, and is its only barrier against that over-powering element, and may properly be called the skeleton of the island.

From every examination Mr. A. has made, he finds the whole island formed of an argillaceous earth, either in its primitive state, or under its different metamorphoses. The bases of the mountains are composed of schistus argillaceus and talcum lithomargo; but the plains or low-lands remaining nearly in the same moist state as at its formation, the component particles have not experienced the vicissitudes of nature so much as the more elevated parts, consequently retain more of their primitive forms and properties. As argillaceous earth is formed from the sediment of the ocean, from the situation of Trinidad to the continent, its formation is easily accounted for, granting first the formation of the ridge of mountains that bound its windward side, and the high mountains on the continent that nearly join it: for the great influx of currents into the Gulf of Paria, from the coasts of Brazil and Andalusia, must bring a vast quantity of light earthy particles from the mouths of the numerous large rivers which traverse these parts of the continent; but the currents being repelled by these ridges of mountains, eddies and smooth water will be produced where they meet and oppose, and therefore the earthy particles would subside, and form banks of mud, and by fresh accumulations added would soon form dry land; and from these causes it is evident such a tract of country as Trinidad must be formed. But these causes still exist, and the effect from them is evident; for the island is daily increasing on the leeward side, as may be seen from the mud-beds that extend a great way into the Gulf, and there constantly increase. But from the great influx from the ocean at the south end of the island, and its egress to the Atlantic again, through the Bocas, a channel must ever exist between the continent and Trinidad.

VIII. Of a particular Change of Structure in the Human Ovarium. By Matthew Baillie, M. D. p. 71.

The ovaria in women are subject to a great variety of changes from their natural structure. Many of these are exactly similar to what take place in other parts of the body; but there is one which seems peculiar to them, the nature of which has probably not been hitherto very well ascertained. The change of structure here alluded to, is a conversion of the natural substance of an ovarium into a fatty mass, intermixed with hair and teeth. This sort of change is rare, though it occurs sufficiently often to have been seen by most persons who are very conversant in the examination of dead bodies. There are many cases of it related in the different books of dissections, but most commonly without any remarks on the mode of formation;* or they have been considered as very imperfect attempts at the growth of a fœtus in the ovarium, in consequence of connection between a male and a female. This conjecture rests no doubt on strong circumstances of probability, and yet there are many powerful reasons which seem to oppose its being well founded. Generation is a process always depending on the action of a certain cause, viz. the usual connection between a male and a female; and when effects similar to those in generation are perceived, it becomes very natural to conclude that this cause has been employed. The bias to such an opinion will become the stronger, from reflecting on the passions that are known to influence so powerfully mankind, by which the agency of this cause is frequently excited. When a change therefore was observed in an ovarium, by which it was converted into a fatty mass with hair and teeth, this should seem to correspond so much with a change taking place in consequence of generation, that the mind would scarcely entertain a doubt of its arising from the same cause, and would readily infer, that it had been preceded by a connection between the sexes. This doubt would still be the less, from the circumstance of a complete fœtus being sometimes formed in the ovarium, where the usual means of generation had been employed. The following case however exhibits many reasons why we should be led to believe, that the ovaria in women have some power within themselves of taking on a process which is imitative of generation, without any previous connection with a male; and it is with this view that it is here related

In a female child, about 12 or 13 years old, which was brought to Windmill-street for dissection, Dr. B. found the right ovarium converted into a substance, doughy to the touch, and about the size of a large hen's egg. On

* It has been the opinion of some, that hair, teeth, nails, feathers, &c. are animal vegetables or plants; and, agreeably to this opinion, Dr. Tyson considers the growth of hair and teeth in the ovarium as a lusus naturæ, where nature endeavours to produce something, and being disappointed in forming an animal, produces a vegetable. Vide Hooke's Lectures and Collection, N° 2, p. 11 and 15.—Orig.

cutting into the substance, he found an apparently fatty mass, intermixed with hair and an excrescence of bones. This startled him very much, as he had always been led to believe that such appearances were a sort of imperfect conception. The circumstances together being very singular, he was led to pay considerable attention to the change in the ovarium. The fatty mass was of a yellowish white colour, in some places more yellow than in others, was very unctuous to the feeling, and consisted of shortened or separated particles, not having the same coalescence which the fat has generally in the body. It became very soft when exposed to the heat of a fire, and sunk into a portion of paper, on which it was spread, so as to make it more transparent. When the paper to which it was applied was exposed to the flame of a candle, it burnt with considerable crackling. The hair with which the fatty substance was mixed grew out of the inner surface of the capsule containing it, in some places in solitary hairs, but chiefly in small fasciculi, at scattered irregular distances. Besides these, there were loose hairs involved in the fatty mass. The hairs were, some of them of considerable length, even to 3 inches, were fine, and of a light brown colour. They resembled much more the hairs of the head, than what are commonly found on the pubis, and corresponded very much in colour to the hair of the girl's head.

There arose also from the inner surface of the capsule some vestiges of human teeth. One appeared to be a canine tooth, another to be a small grinder, 2 others to be incisors, and there was also a very imperfect attempt at the formation of another tooth. These were not fully formed, the fangs being wanting; but in 2 of them the bodies were as complete as they are ever found in the common circumstances. They were each of them inclosed in a proper capsule, which arose from the inner surface of the ovarium, and consisted of a white thick opaque membrane. Attached to the capsules of 3 of the teeth, there was a white spongy substance. The membrane of the ovarium itself was of some considerable thickness, but unequal in the different parts, was very smooth in its inner surface, and more irregular externally. The uterus was smaller than it is commonly at birth, was perfectly healthy in its structure, and on opening into its cavity it exhibited the ordinary appearances of a child's uterus at that period. The left ovarium was very small, corresponding to the state of the uterus. It appears clearly from this, that the uterus had not yet received the increase of bulk, which is usual at the age of puberty. The hymen was entire, such as is commonly found in a child of the same age; and there was just beginning a lanugo on the labia, not more than what is often found on the upper lip of a boy of 15 years old. Such are the circumstances attending this singular case, and they present to the mind various grounds of consideration.*

* See also a remarkable instance of an ovarium containing teeth, hair, and bones, related by Mr. Cleghorn, in the first vol. of the Transactions of the Royal Irish Academy.

The formation of hair and teeth is a species of generation, for in fact it makes a part of it, and strikes the mind as being very different from any irregular substance which is formed by disease. This formation too takes place in a part of the body which is subservient to generation, and where a complete fœtus is sometimes formed. The whole of this looks very much as if the production of hair and teeth in the ovarium was a sort of imperfect impregnation. But when we take another view of it, there are reasons at least equally strong for believing that such productions may arise from an action in the ovarium itself, without any stimulus from the application of the male semen.

In the case before us, the uterus was as small as at birth, indeed more so, and the left ovarium, which was perfectly healthy, corresponded to the state of the uterus. It had not been at all stimulated, nor did appear capable of being stimulated by the application of the male semen. This seems to be a strong circumstance; for in a case where there was an ovum formed in one of the Fallopian tubes, the uterus was enlarged to more than twice its unimpregnated size; and, on opening into its cavity, the decidua was observed to be formed as completely as in the impregnated uterus. This preparation is still preserved in the collection of Windmill-street. Nothing can be a stronger proof, that when an impregnation takes place out of the cavity of the uterus, the uterus still takes a share in the action, and undergoes some of the changes of impregnation. In another preparation, which is preserved in the same collection, where there was a fœtus formed in the ovarium, the uterus was increased to more than twice its ordinary size, was very thick and spongy, and had its blood-vessels enlarged as in an impregnated uterus. This becomes another very strong proof of the action of the uterus in the formation of an extra-uterine fœtus. In the case before us however, the uterus had undergone no change, and does not seem to have arrived at that period when it could be capable of undergoing such a change.

Besides, we are not to consider the formation of teeth in the ovarium to be a quicker process than it is commonly in the head of a fœtus; but in the present case the teeth having advanced fully as far as they are at some months after birth, this process must have begun at least more than a twelvemonth before the death of the child. If then we consider it as an impregnation, since the appearances of the child do not warrant us to believe her to have been more than 12 or 13 years old, this brings the date of the impregnation to an earlier period than can well be believed. From all these circumstances we might be led to suppose, that the formation of the hair and teeth was not in consequence of any connection with a male, but arose from some action of the ovarium itself, in which the uterus did not participate. The existence of the hymen, especially in so young a girl, becomes a collateral confirmation of the same opinion, though much is not to be rested on it, when taken singly.

It will perhaps have some influence in removing the prejudices against this opinion, to make the following remarks. Hair is occasionally formed in parts of the human body, which are absolutely unconnected with generation. Encysted tumours are sometimes found containing hair. Mr. Hunter has a preparation of this sort in his collection, which he cut out from under the skin of the eyebrow. This tumour was perfectly complete, and unconnected with the skin, except by the common intervention of cellular membrane, so as to have no communication whatever with the hair of the eyebrow. In this instance there was certainly a species of generation taking place in the encysted tumour itself, forming hairs as completely and fully as in the common progress of the formation of a child. Such encysted tumours have been found in other parts of the human body, and still more frequently in quadrupeds. Mr. Hunter has in his collection many specimens of encysted tumours from cows and sheep containing hair and wool. These were perfectly complete, so as to have possessed a power of production within themselves, and were many of them found deeply seated at a considerable distance from the skin, which is the common parent of hair. In these tumours there is often the appearance of layers of cuticle, which is probably a preparatory step to the formation of hair. All this shows most clearly, that hair may be formed without any species of generation as it is commonly understood. But hair is in itself as distinct a consequence of generation as teeth, and as much a peculiar substance. If then the one be formed, there appears to be no reason why the other should not also be formed. The action producing the one is not better understood than that producing the other; nor does it appear to be really in itself less connected with that species of generation arising from the approach of a male, so that teeth may probably be formed by a peculiar action taking place in the ovarium itself, as well as the hair.

It will tend to add further weight to this opinion, to consider that many of the adult teeth are formed in a child after birth; and therefore their formation depends on an action taking place in the jaws at a particular period, and not on original growth. The same circumstance strikes more strongly in the occasional formation of teeth at an advanced time of life. Both of these processes take place after the animal has been formed, in consequence of a certain action being excited in a particular part of the body, and therefore there is less difficulty in believing that the same sort of process may go on in another part of the body not commonly employed in it. It seems reasonable also to suppose, that the ovaria should have a greater aptitude of taking on a process somewhat similar to generation than the other indifferent parts of the body, as they constitute a part of the organs which are so materially concerned in the real process itself*. These cir-

* As the formation of teeth and hair involved in a fatty mass may be said to be peculiar to the ovaria, and as there are strong reasons for believing that this formation may take place without an in-

cumstances, when taken collectively, would seem to render it very probable, that the formation of hair and teeth in the ovarium does not necessarily depend on a connection between a male and a female, as has been the common opinion, but arises from some action within the ovarium itself, which is imitative of generation.

IX. On the Vegetable and Mineral Productions of Boutan and Thibet. By Mr. Robert Saunders, Surgeon at Boglepoor in Bengal. p. 79.

Road to Buxaduar, May 11 and 12, 1783. The tract of country from Bahar to the foot of the hills contains but few plants that are not common to Bengal. Pine-apples, mango-tree, jack and saul timber, are frequently to be met with in the forests and jungles. Many orange-trees towards the foot of the hills, of a very good sort, and bearing much fruit. Saw a few lime-trees, and found 3 different species of the sensitive plant. One species is used medicinally by the natives of Bengal in fevers; it is a powerful astringent and bitter; another is the species from which Terra Japonica is made, a medicine the history of which we are but lately made acquainted with. The 3d species is well known as the sensitive plant, and common in Bengal.

The country, from Bahar to the foot of the mountains, to which we approach without any ascent, is rendered one of the most unhealthy parts of India, from a variety of causes. The whole, a perfect flat, is at all times wet and swampy, with a luxuriant growth of reeds, long grass, and underwood, in the midst of stagnated water, numerous frogs and insects. The exhalations from such a surface of vegetable matter and swamps, increased by an additional degree of heat from the reflection of the hills, affect the air to a considerable extent, and render it highly injurious to strangers and European constitutions. The thermometer at the foot of the hill, mid-day 86°, fell to 78° at 2 o'clock, the time we reached Buxaduar, and that hour of the day when it is generally highest.

Buxaduar, May 12 to 21. Many of the plants peculiar to Bengal require nursing at Buxaduar. There is one very good banian tree. In the jungles, met with the ginger, and a very good sort of yam; saw some pomegranate-trees in good preservation; shallots in great perfection; a species of the lychnis, arum, and asclepias, natives of more northern situations, and of little use; a bad sort of rasberry, and a species of the gloriosa. The plantains in use below do not thrive here. In the jungles they have a plantain-tree producing a very broad leaf, with which they cover their huts; but the fruit is not eaten. From the 15th to the 22d, the rains were almost incessant at Buxaduar. Our people became unhealthy,

tercourse between the sexes, it becomes difficult to account for this peculiarity in them, unless by supposing that they have a greater aptitude of running into such a process, than the other parts of the body.—Orig.

and were attacked with fevers, which, if neglected in the beginning, prove obstinate quartans. Buxaduar lies high, but is overtopped by the surrounding mountains, covered with forests of trees and underwood. In all climates, where the influence of the sun is great, this is a never-failing cause of bad air. The exhalation that takes place from so great a surface in the day-time falls after sunset in the form of dew, rendering the air raw, damp, and chilly, even in the most sultry climates. The thermometer at Buxaduar was never, at 2 o'clock in the afternoon, above 82°, nor below 73°.

Road to Murishong, May 22 and 23. In ascending the hill from Buxaduar there is to be seen much of an imperfect quartz, of various forms and colour, having in some places the appearance of marble; but from chemical experiments, it was found to possess very different properties. This sort of quartz, when of a pure white, and free from any metallic colouring matter, is used as an ingredient in porcelain. It is known to mineralists in that state by the name of quartz gritstone. The rock which forms the basis of these mountains dips in almost every direction, and is covered with a rich and fertile soil, but in no place level enough to be cultivated. Many European plants are met with on the road to Murishong; many different sorts of mosses, fern, wild thyme, peaches, willow, chickweed, and grasses common to the more southern parts of Europe, nettles, thistles, dock, strawberry, rasberry, and many destructive creepers, some common in Europe.

Murishong is the first pleasant and healthy spot to be met with on this side of Boutan. It lies high, and much of the ground about it is cleared and cultivated; the soil, rich and fertile, produces good crops. The only plant now under culture is a species of the polygonum of Linneus, producing a triangular seed, nearly the size of barley, and the common food of the inhabitants. It was now the beginning of their harvest; and the ground yields them, as in other parts of Boutan, a 2d crop of rice. Here are to be found in the jungles 2 species of the laurus of Linneus; one known by the name of the bastard cinnamon. The bark of the root of this plant, when dried, has very much the taste and flavour of cinnamon; it is used medicinally by the natives. The chenopodium, producing the semen santonicum, or worm-seed, a medicine formerly in great character, and used in those diseases from which it is named, is common here. Found in the neighbourhood of this place all the European plants we had met with on the road. The ascent from Buxaduar to Murishong is on the whole great, with a sensible change in the state of the air.

Road to Chooka, May 25. On the road to Chooka find all the Murishong plants, cinnamon-tree, willow, and 1 or 2 firs; strawberries every where, and very good, and a few bilberry plants. Much sparry flint, and a sort of granite with which the road is paved. There is a great deal of talc in the stones and soil, but in too small pieces to be useful. Frequent beds of clay and pure sand.

Find 2 mineral wells, slightly impregnated with iron, with much appearance of that metal in this part of the country; and they are not unacquainted with the method of extracting it from the stones, but still despise its use in building. Towards Chooka there are many well cultivated fields of wheat and barley.

Road to Punukha, May 26. From Chooka the country opens, and presents to view many well cultivated fields and distant villages; a rapid change in climate, the vegetable productions, and general appearance of the country. Towards Punukha, pines and firs are the only trees to be met with; but they do not yet seem in their proper climate, being dwarfish and ill-shaped: peaches, rasberries, and strawberries, thriving every where; scarcely a plant to be seen that is not of European growth. In addition to the many already mentioned, saw 2 species of the cratægus, one not yet described. Saw 2 ash-trees in a very thriving state, the star-thistle, and many other weeds, in general natives of the Alps and Switzerland.

Much of the rock is pure lime-stone; a valuable acquisition if they did not either despise its use, or were unacquainted with its properties. It was most advantageously situated for being worked, and the purest perhaps to be met with. There is likewise abundance of fire-wood in this part of the country. In building they would derive great benefit from the use of it. Their houses are lofty, the timbers substantial, and nothing wanting to make them durable, but their being acquainted with the use of lime. As a manure it might probably be used to great advantage. Many fields of barley in this part of the country; now the beginning of their harvest. The thermometer here fell, at 4 o'clock in the afternoon, to 60°: cold and chilly.

Road to Chepta, May 27. On the road to Chepta, the rock in general dips to the northward and eastward, in about an angle of 60 degrees. Much of lime-stone, and some veins of quartz, and loose pieces of sparry flint striking fire with steel. Several springs, and one slightly impregnated with iron. In addition to the plants of yesterday, found the coriandrum testiculatum, inula montana, and rhododendrum magnum. At Chepta met with a few turneps, one maple-tree, worm-wood, goose-grass (galium aparine), and many other European weeds; the first walnut-tree we had yet seen. Chepta lies high, and not above 6 miles from the mountain of Lomyla, now covered with snow. The wind from that quarter, S.E. made it cold and chilly, and sunk the thermometer at mid-day to 57°. Here are some fields of wheat and barley not yet ripe.

Road to Pagha, May 29. Soon after leaving Chepta find a mineral well, which, on a chemical examination, gave marks of a strong impregnation from iron. Traced it to its source, where the thermometer, on being immersed, fell from 68° to 56°. A little before we reach Pagha, met with some lime-stone, and a bed of chalk, which, near the surface, contained a great proportion of sand, but some feet under was much purer. The forests of firs of an inferior growth, several ash-trees, dog-rose, and bramble.

Road to Tassesudon, May 30 and 31, June 1. The road from here to Tassesudon presents us with little that we have not met with; fewer strawberries, and no rasberries; some very good orchards of peaches, apricots, apples, and pears. The fruit formed, and will be ripe in August and September. Met with two sorts of cranberry, one very good. Saw the fragaria sterilis, and a few poppies. At Wanakha found a few turnips, shallots, cucumbers, and gourds. Near to Tassesudon the road is lined with many different species of the rose, and a few jessamine plants. The soil is light, and the hills in many places barren, rocky, and with very little verdure. The rock in general laminated and rotten, with many small particles of talc in every part of the country incorporated with the stones and soil. Some lime-stone, and appearance of good chalk. Several good and pure springs of water. The hills are chiefly wood, with firs and aspen. Have not yet been able to find an oak-tree, and the ash very seldom. The elder, holly, bramble, and dog-rose, are common. Found the birch-tree, cypress, yew, and delphinium. Many different species of the vaccinium, among them the bilberry and the cranberry. Towards the top of the adjacent mountains met with two plants of the arbutus uva ursi, which is a native of the Alps, the most mountainous parts of Scotland, and Canada. Have seen a species of the rhubarb plant (rheum undulatum) brought from a distance, and only to be met with near the summits of hills covered with snow, and where the soil is rocky. The true rhubarb (rheum palmatum) is also the native of a cold climate; and though China supplies us with much of this drug, it is known to be the growth of its more northern provinces, Tartary, and part of the Russian dominions. The great difficulty is in drying the root. People versant in that business say, that 100lb. of the fresh root should not weigh above $6\frac{1}{2}$lb. if properly dried, and it certainly has been reduced to that. Have seen 80lb. of fresh root produced from 1 plant; but, after drying it with much care and attention, the weight of the dried root could not be made less than 12lb. It was suspended in an oven, with an equal and moderate degree of heat. Little more than the same quantity of this powder produced a similar effect with the best foreign rhubarb.

The other plants common here are the service-tree, blessed thistle, mock orange, spiræa filipendula, arum, echites, punica, ferula communis, erica, and viola. Of the rose-bush I have met with the 5 following species; rosa alpina, centifolia, canina, indica, spinosissima. The culture of pot-herbs is every where neglected; turnips, a few onions and shallots, were the best we could procure.

Mr. Bogle left potatoes, cabbage, and lettuce-plants, all which we found neglected and dispersed. They had very improperly, from an idea most probably of their being natives of Bengal, planted them in a situation and climate which approaches very near to that of Bengal at all seasons. Melons, gourds, brinjals, and cucumbers, are occasionally to be met with. The country is fitted for the production of every fruit and vegetable common without the tropics, and in some situations will bring to perfection many of the tropical fruits.

There are 2 plants which I have to regret the not having had as yet an opportunity of seeing; one is the tree from the bark of which their paper is made; and the other is employed by them in poisoning their arrows. This last is said to come from a very remote part of the country. They describe it as growing to the height of 3 or 4 feet, with a hollow stalk. The juice is inspissated, and laid as a paste on their arrows. Fortunately for them, it has not all the bad effects they dread from it. I had an opportunity of seeing several who were wounded with these arrows, and they all did well, though under the greatest apprehension. The cleaning and enlarging some of the wounds was the most that I found necessary to be done. The paste is pungent and acrid, will increase inflammation, and may make a bad or neglected wound mortal; but it certainly does not possess any specific quality as a poison.

The fir, so common in this country, is perhaps the only tree they could convert to a useful and profitable purpose. What I have seen would not, from their situation, be employed as timber. The largest I have yet met with were near Wandepore; they measured from 8 to 10 feet in circumference, were tall and straight. Such near the Burrampooter, or any navigable river, might certainly be transported to an advantageous market. I am convinced that any quantity of tar, pitch, turpentine, and resin, might be made in this country, much to the emolument of the natives. Firs, which from their size and situation are unfit for timber, would answer the purpose equally well. The process for procuring tar and turpentine is simple, and does not require the construction of expensive works. This great object has been so little attended to, that they are supplied from Bengal with what they want of these articles.

The country about Tassesudon contains great variety of soil, and much rock of many different forms, but still an unpromising field for a mineralist. I have not found in Boutan a fossil that had the least appearance of containing any other metal than iron, and a small portion of copper. From information, and the reports of travellers, I believe it is otherwise to the northward. The banks of the Ticushu, admitting of cultivation for several miles above and below Tassesudon, yield them 2 crops in the year. The first of wheat and barley is cut down in June; and the rice, planted immediately after, enjoys the benefit of the rains. This country is not without its hot wells, as well as many numerous springs. One hot well, near Wandepore, is so close to the banks of the river as to be overflowed in the rains, and it was impossible to get to it: the heat of this well is great; but I could not learn that the ground about it was much different from the general aspect of the country. Another, several days journey from hence, is on the brow of a hill perpetually covered with snow. This hot well is held in great estimation by the people of the country, and resorted to by valetudinarians of every description.

Tassesudon to Paraghon, Sept. 8 and 9. Much good rich soil, with more pasture, where the ground is not cultivated, than we had yet met with. Many fields of turnips in great perfection; a plant they seem better acquainted with the cultivation of than any other. Find on the road many large and well thriving birch, willows, pines, and firs, some walnut-trees, the arbutus uva ursi, abundance of strawberry, elderberry, bilberry, chrysanthemum or greater daisy, and many European grasses. See the datura ferox or thorn apple, a plant common in China and some parts of Thibet, where it is used medicinally. They find it a powerful narcotic, and give the seeds where they wish that effect to be produced. It has been used as a medicine in Europe, and is known to possess these qualities in a high degree. See holly, dog-rose, and aspen. The present crop near Paraghon, on the banks of the Pachu, is rice, but not so far advanced as at Tassesudon; the same may be said of their fruits. They say it is colder here at all seasons than at Tassesudon, which is certainly below the level of this place. Towards the summit of the mountain we crossed, found some rock of a curious appearance, forming in front 6 or 7 angular semi-pillars, of a great circumference, and some hundred feet high. This natural curiosity was detached in part from the mountain, and projected over a considerable fall of water, which added much to the beautiful and picturesque appearance of the whole. Numerous springs, some degrees colder than the surrounding atmosphere, gushing from the rock on the most elevated part of the mountain, furnish a very ample and seasonable supply of excellent water to the traveller. The rock, in many places laminated, might be formed into very tolerable slate. Near to Paraghon iron stones are found, and one spring highly impregnated with this mineral.

Road to Dukaigun, Sept. 11. Our road to Dukaigun, nearly due north, is a continued ascent for 8 miles, along the banks of the Pachu, falling over numerous rocks, precipices, and huge stones. Here we begin to experience a very considerable change in the temperature of the atmosphere; the surrounding hills were covered with snow in the morning, which had fallen the preceding night, but disappeared soon after sunrise. The thermometer fell to 54° in the afternoon, and did not rise above 62° at noon. The face of the mountains, in some places bare, with projecting rock of many different forms; quartz, flint, and a bad sort of freestone, common. Many very good springs, slightly impregnated with a selenitic earth. The soil is rich, and near the river in great cultivation. Many horses, the staple article of their trade, are bred in this part of the country. Found walnut-trees, peaches, apples, and pears.

Road to Sanha, Sept. 12. The road still ascending to Sanha, and near the river for 10 miles. The thermometer falling some degrees, we found it cold and chilly. The bed of the river is full of large stones, probably washed down from the mountains by the rapidity of its stream; they are chiefly quartz and granite. Here is excellent pasture for numerous herds of goats.

Road to Chichakumboo. From Sanha the ascent is much greater, and, after keeping for 10 miles along the banks of the Pachu, still a considerable stream, we reach its source, from 3 distinct rivulets, all in view, ramified and supplied by numerous springs, and soon after arrive at the most elevated part of our road. Here we quit the boundary of Boutan, and enter the territory of Thibet, where nature has drawn the line still more strongly, and affords perhaps the most extraordinary contrast that takes place on the face of the earth. From this eminence are to be seen the mountains of Boutan, covered with trees, shrubs, and verdure to their tops, and on the south side of this mountain to within a few feet of the ground on which we tread. On the north side the eye takes in an extensive range of hills and plains, but not a tree, shrub, or scarcely a tuft of grass to be seen. Thus, in the course of less than a mile, we bid adieu to a most fertile soil, covered with perpetual verdure, and enter a country where the soil and climate seem inimical to the production of every vegetable. The change in the temperature of the air is equally obvious and rapid. The thermometer in the forenoon 34°, with frost and snow in the night-time. Our present observations on the cause of this change confirmed us in a former opinion, and incontestably prove, that we are to look for that difference of climate from the situation of the ground as more or less above the general level of the earth. In attending to this cause of heat or cold, we must not allow ourselves to be deceived by a comparison with that which is immediately in view. We ought to take in a greater range of country, and where the road is near the banks of a river, we cannot well err in forming a judgment of the inclination of the ground. Punukha and Wandepore, both to the northward of Tassesudon, are quite in a Bengal climate. The thermometer at the first of these places, in the months of July and January, was within 2° of what it had been at Rungpore for the same periods. They seem in more exposed situations than Tassesudon; and, were we to draw a comparison of their heights from the surrounding ground, I should say they were above its level. The road however proves the reverse. From Punukha to Tassesudon we had a continued and steep ascent for 6½ hours, with a very inconsiderable descent on the Tassesudon side. From the south side of the mountain dividing Boutan from Thibet the springs and rivulets are tumbling down in cascades and torrents, and have been traced by us near to the foot of the hills, where they empty themselves to the eastward of Buxaduar. On the north side they glide smoothly along, and by passing to the northward as far as Tishoolumboo, prove a descent on that side, which the eye could not detect. This part of the country, being the most elevated, is at all times the coldest; and the snowy mountains, from their heights and bearings, notwithstanding the distance, are certainly those seen from Purnea. The soil on the Thibet side of the mountain is sandy, with much gravel and many loose stones. On the road found the aconitum pyreneum, and 2 species of the saxifraga. See a large flock of chowry tailed cattle; their extensive range of pasture seems to make amends for its poverty.

From Faro to Duina, Sept. 15, pass over an extensive plain, bounded by many small hills, oddly arranged; some of them detached and single, and all seem composed of sand collected in that form, having the plain for their general base. At Duina found a few plots of barley, which they are now cutting down, though green, as despairing of its ripening. The thermometer, at 6 o'clock in the morning below the freezing point, and the ground partially covered with snow.

Road to Chalu, Sept. 16. Continue on the plain; find 3 springs forcing their way through the ground with violence, and giving rise to a lake many miles in extent, stored with millions of water-fowl and excellent fish. Of the first saw the cygnus, solan geese, many kinds of ducks, pintados, cranes, and gulls of different sorts. The springs of this lake are in great reputation for the cure of most diseases. I examined the water, and found it contain a portion of alum with the selenitic earth. On the banks of the lake I found a crystallization, which proves to be an alkaline salt; it is used by the natives for washing, and answers the purpose as well as pot-ash. The pasture which is impregnated with this salt is greedily sought after by sheep and goats, and proves excellent food for them. The hills are chiefly composed of sand incrusted by the inclemency of the weather and violent winds, seeming at first view composed of freestone.

Road to Simadar, Sept. 17. Pass a lake still more considerable than the former, with which it communicates by a narrow stream, about 3 miles long. There never was a more barren or unpromising soil; little turf, grass, or vegetation of any sort, except near the lake. See a few huts, mostly in ruins and deserted. The only grain in this part of the country is barley, which they are cutting down every where green. Pass 2 springs, one of them slightly impregnated with alum. They form the principal source of a river, which empties itself in the Burrampooter near Tissoolumboo. The wind from the eastward of south is now the coldest and most piercing; passing over the snowy mountains and dry sandy desart before described, it comes divested of all vapour or moisture, and produces the same effect as the hot dry winds in more southerly situations. Mahogany boxes and furniture, that had withstood the Bengal climate for years, were warped with considerable fissures, and rendered useless. The natives say, a direct exposure to these winds occasions the loss of their fore-teeth; and our faithful guide ascribed that defect in himself to this cause. We escaped with loss of the skin from the greatest part of our faces.

Road to Seluh, Sept. 18. Near our road to-day found a hot-well, much frequented by people with venereal complaints, rheumatism, and all cutaneous diseases. They do not drink the water, but use it as a bath. The thermometer, when immersed in the water, rose from 40° to 88°. It has a strong sulphureous smell, and contains a portion of hepar sulphuris. Exposure to air deprives it, as most other mineral wells, of much of its property.

Road to Takui, Sept. 19. Pass some fields of barley and pease, and get into a milder climate. Find to-day a great variety of stone and rock, some containing copper, and others, a very pure rock crystal, regularly crystallized, with 6 unequal sides. The rock crystal is of different sizes and degrees of purity, but of one form. Find some flint and granite, several springs of water impregnated with iron, and nearly of the same temperature with the atmosphere. See a few ill thriving willows planted near the habitations, and which are the only trees to be met with.

Road to Tissoolumboo, Sept. 20, 21, and 22. The remaining part of our journey is over a more fertile soil, enjoying a milder climate. Some very good fields of wheat, barley, and pease; many pleasant villages and distant houses, less sand and more rock, part slaty, and much of it a very good sort of flint. The soil in the valley a light coloured clay and sand. They are every where employed in cutting down their crop. What a happy climate! The sky serene and clear, without a cloud; and so confident are they of the continuance of this weather, that their crop is thrown together in a convenient part of the field, without any cover, to remain till they can find time to thresh it out. Before we reached Tissoolumboo found some elms and ash trees.

The hills in Thibet have, from their general appearance, strong marks of containing those fossils that are inimical to vegation; such are most of the ores of metal and pyritical matter. The country, properly explored, promises better than any I have seen to gratify the curiosity of a philosopher, and reward the labours of a mineralist. Accident, more than a spirit of enterprise and inquiry, has already discovered the presence of many valuable ores and minerals in Thibet. The first in this list is deservedly gold. They find it in large quantities, and frequently very pure. In the form of gold-dust it is found in the beds of rivers, and at their several bendings, generally attached to small pieces of stone, with every appearance of its having been part of a larger mass. They find it sometimes in large masses, lumps, and irregular veins; the adhering stone is generally flint or quartz, and I have sometimes seen a half-formed, impure sort of precious stone in the mass. By a common process for the purification of gold, I extracted 12 per cent. of refuse from some gold-dust, and on examination found it to be sand and filings of iron, which last was not likely to have been with it in its native state, but probably employed for the purpose of adulteration. Two days journey from Tissoolumboo there is a lead mine. The ore is much the same as that found in Derbyshire, mineralized by sulphur, and the metal obtained by the very simple operation of fusion alone. Most lead contains a portion of silver, and some in the proportion to make it an object to work the lead ore for the sake of the silver. Cinnabar, containing a large proportion of quicksilver, is found in Thibet, and might be advantageously employed for the purpose of extracting this metal. The process is simple, by distillation; but to carry it on in the great would require more fuel than the country can well supply. I have seen ores and loose stones containing copper, and have not a doubt of its being to be found in great abundance in the country. Iron is more frequently to be met with in Boutan than Thibet; and, were it more common, the difficulty of procuring proper fuel for smelting the less valuable ores, must prove an insuperable objection to the working them. The dung of animals is the only substitute they have for fire-wood, and with that alone they will never be able to excite a degree of heat sufficiently intense for such purposes. Thus situated, the most valuable discovery for them would be that of a coal mine. In some parts of China bordering on Thibet, coal is found and used as fuel.

Tincal, the nature and production of which we have only hitherto been able to guess at, is now well known, and Thibet, from whence we are supplied, contains it in inexhaustible quantities. It is a fossil brought to market in the state it is dug out of the lake, and afterwards refined into borax by ourselves. Rock salt is likewise found in great abundance in Thibet. The lake, from whence tincal and rock salt are collected, is about 15 days journey from Tissoolumboo, and to the northward of it. It is encompassed on all sides by rocky hills, without any brooks or rivulets near at hand; but its waters are supplied by springs, which being saltish to the taste are not used by the natives. The tincal is deposited or formed in the bed of the lake; and those who go to collect it, dig it up in large masses, which they afterwards break into small pieces for the convenience of carriage, exposing it to the air to dry. Though tincal has been collected from this lake for a great length of time, the quantity is not perceptibly diminished; and as the cavities made by digging it soon wear out or fill up, it is an opinion with the people, that the formation of fresh tincal is going on. They have never yet met with it in dry ground or high situations, but it is found in the shallowest depths, and the borders of the lake, which deepening gradually from the edges towards the centre contains too much water to admit of their searching for the tincal conveniently; but from the deepest parts they bring up rock salt, which is not to be found in the shallows, or near the bank. The waters of the lake rise and fall very little, being supplied by a constant and unvarying source, neither augmented by the influx of any current, nor diminished by any stream running from it. The lake is at least 20 miles in circumference, and standing in a very bleak situation is frozen for a great part of the year. The people employed in collecting these salts are obliged to quit their labour so early as October, on account of the ice. Tincal is used in Thibet for soldering and to promote the fusion of gold and silver. Rock salt is universally used for all domestic purposes in Thibet, Boutan, and Naphaul.

The thermometer at Tissoolumboo during the month of October was, on an average, at 8 o'clock in the morning 38°, at noon 46°, and 6 o'clock in the evening 42°. The weather clear, cool, and pleasant, and the prevailing wind from the southward. During the month of November we had frosts morning and evening, a serene clear sky, not a cloud to be seen. The rays of the sun passing through a medium so little obscured had great influence. The thermometer was often below 30° in the morning, and seldom above 38° at noon in the shade; wind from the southward.

Of the diseases of this country, the first that attracts our notice, as we approach the foot of the hills, is a glandular swelling in the throat, which is known to prevail in similar situations in some parts of Europe, and generally ascribed to an impregnation of the water from snow. The disease being common at the foot of the Alps, and confined to a tract of country near these mountains, has first given rise to the idea of its being occasioned by snow water. If a general view of the disease, and situations where it is common, had been the subject of inquiry, or awakened the attention of any able practitioner, we should have been long since undeceived in this respect. On the coast of Greenland, the mountainous parts of Wales and Scotland, where melted snow must be continually passing into their rivers and streams, the disease is not known, though it is common in Derbyshire, and some other parts of England. Rungpore is about 100 miles from the foot of the hills, and much farther from the snow, yet the disease is as frequent there as in Boutan. In Thibet, where snow is never out of view, and the principal source of all their rivers and streams, the disease is not to be met with; but what puts the matter past a doubt, is the frequency of the disease on the coast of Sumatra, where snow is never to be found. On finding the vegetable productions of Boutan the same as those of the Alps in almost every instance, it occurred to me, that the disease might arise from an impregnation of the water by these plants, or the soil probably possessing similar qualities, the spontaneous productions of both countries, with very few exceptions, being so nearly alike. It however appears more probable, that the disease is endemial, proceeding from a peculiarity in the air of situations in the vicinity of mountains with such soil and vegetable productions. I am the more inclined to think so, that I have universally found this disease most prevalent among the lower class of people, and those who are most exposed to the unguarded influence of the weather, and various changes that take place in the air of such situations. The primary cause in the atmosphere producing this effect is, perhaps, not more inexplicable than what we meet with in the low-lands of Essex and fens of Lincolnshire. An accurate analysis of the water used in common by the natives, where this disease is more or less frequent, and where it is not known in similar exposures, might throw some light on this subject.

This very extraordinary disease has been little attended to, from obvious reasons; it is unaccompanied with pain, seldom fatal, and generally confined to the poorer sort of people. The tumor is unsightly, and grows to a troublesome size, being often as large as a person's head. It is certainly not exaggerating to say, that 1 in 6 of the Rungpore district, and country of Boutan, has the disease. As those who labour most, and are the least protected from the changes of weather, are most subject to the disease, we universally find it in Boutan more common with the women than men. It generally appears in Boutan at the age of 13 or 14, and in Bengal at the age of 11 or 12; so that in both countries the disease shows itself about the age of puberty, I do not believe this disease has ever been removed, though a mercurial course seemed to check its progress, but did not prevent its advance after intermitting the use of mercury. An attention to the primary cause will first lead to a proper method of treating the disease; a change of situation for a short while, at that particular period when it appears, might be the means of preventing it.

The people of this happy climate are not exempt from the venereal disease, which seems to rage with unremitting fury in all climates, and proves the greatest scourge to the human race. It has been long a matter of doubt, whether this disease has ever been cured by any other specific than mercury and its different preparations. In defence of the opinion of other specifics being in use, it has always been urged, that the disease is frequent in many parts of the world, where it could not be supposed that they were acquainted with quicksilver, and the proper method of preparing it as a medicine. I must own, that I expected to have been able to have added one other specific for this disease to our list in the Materia medica, being informed that the disease was common, and their method of treating it successful; nor could I allow myself to think that they were acquainted with the method of preparing quicksilver, so as to render it a safe and efficacious medicine. In this, however, I was mistaken.

The disease seems in this country to make a more rapid progress, and rage with more violence, than in any other. This is to be accounted for from the grossness of their food and little attention to cleanliness. There is one preparation of mercury in common use with them, and made after the following manner. A portion of alum, nitre, vermillion, and quicksilver, are placed in the bottom of an earthen pot, with a smaller one inverted put over the materials, and well luted to the bottom of the larger pot. Over the small one, and within the large one, the fuel is placed, and the fire continued for about 40 minutes. A certain quantity of fuel, carefully weighed out, is what regulates them with respect to the degree of heat, as they cannot see the materials during the operation. When the vessel is cool, the small inverted pot is taken off, and the materials collected for use. I attended the whole of the process, and examined the materials afterwards. The quicksilver had been acted on by the other ingredients, deprived of its metallic form, and rendered a safe and efficacious remedy.

A knowledge of chemistry has taught us a more certain method of rendering this valuable medicine active and efficacious; yet we find this preparation answering every good purpose, and by their guarded manner of exhibiting it perfectly safe. This powder is the basis of their pill, and often used in external application. The whole, when intimately mixed, formed a reddish powder, and was made into the form of pills by the addition of a plumb or date. Two or 3 pills taken twice a day generally bring on, about the 4th or 5th day, a spitting, which is encouraged by continuing the use of the pills for a day or 2 longer. As the salivation advances, they put a stick across the patient's mouth, in the form of a gag, and make it fast behind. This they say is done to promote the spitting, and prevent the loss of their teeth. They keep up the salivation for 10 or 12 days, during which time the patient is nourished with congee and other liquids. Part of this powder is often used externally by diffusing it in warm water, and washing sores and buboes. They disperse buboes frequently by poultices of turnip tops, in which they always put vermillion, and sometimes musk. Nitre, as a cooler, is very much used internally by them in this disease, and they strictly enjoin warmth and confinement during the slightest mercurial course. Buboes advanced to suppuration are opened by a lancet, with a large incision, which they do not allow to close before the hardness and tumor are gone. In short, I found very little room for improving their practice in this disease. I introduced the method of killing quicksilver with honey, gave them an opportunity of seeing it done, and had the satisfaction of finding it successfully used by themselves before we left the country.

This happy climate presents us with but little variety in their diseases. Coughs, colds, and rheumatism, are more frequent here than in Bengal. Fevers generally arise here from a temporary cause, are easily removed, and seldom prove fatal. The liver disease is occasionally to be met with, and complaints in the bowels are not unfrequent; but the grossness of their food, and uncleanliness of their persons, would in any other climate be the source of constant disease and sickness. They are ignorant, as we were not many years ago, of the proper method of treating diseases of the liver and other viscera; this is probably the cause of the most obstinate and fatal disease to be met with in the country, I mean the dropsy. As the Rajah had ever been desirous of my aid and advice, and had directed his doctors to attend to my private instructions and practice, I endeavoured to introduce a more judicious method of treating those diseases by mercurial preparations. I had an opportunity of proving the advantage of this plan to their conviction in several instances, and of seeing them initiated in the practice.

The Rajah favoured me with above 70 specimens of the medicines in use with them. They have many sorts of stones and petrifactions saponaceous to the touch, which are employed as an external application in swellings and pains of the joints. They often remove such complaints, and violent head-achs, by fumigating the part affected with aromatic plants and flowers. They do not seek for any other means of information respecting the state of a patient than that of feeling the pulse; and they confidently say, that the seat of pain and disease is easily to be discovered, not so much from the frequency of the pulse as its vibratory motion. They feel the pulse at the wrist with their three fore-fingers, first of the right, and then of the left hand; after pressing more or less on the artery, and occasionally removing 1 or 2 of the fingers, they determine what the disease is. They do not eat any thing the day on which they take physic, but endeavour to make up the loss afterwards by eating more freely than before, and using such medicines as they think will occasion costiveness.

The many simples in use with them are from the vegetable kingdom, collected chiefly in Boutan. They are in general inoffensive and very mild in their operation. Carminatives and aromatics are given in coughs, colds, and affections of the breast. The centaury, coriander, carraway, and cinnamon, are of this sort. This last is with them the bark of the root of that species of laurus formerly mentioned as a native of this country. The bark from the root is in this plant the only part which partakes of the cinnamon taste; and I doubt very much if it could be distinguished by the best judges from what we call the true cinnamon. The bark, leaves, berries, and stalks, of many shrubs and trees, are in use with them, all in decoction. Some have much of the astringent bitter taste of our most valuable medicines, and are generally employed here with the same view, to strengthen the powers of digestion, and mend the general habit. Their principal purgative medicines are brought by the Chinese to Lassa. They had not any medicine that operated as a vomit, till I gave the Rajah some ipecacuanha, who made the first experiment with it on himself. In bleeding they have a great opinion of drawing the blood from a particular part. For head-achs they bleed in the neck; for pains in the arm and shoulder, in the cephalic vein; and of the breast or side, in the median; and if in the belly, they bleed in the basilic vein.

They think pains of the lower extremity are best removed by bleeding in the ancle. They have a great prejudice against bleeding in cold weather; nor is any urgency or violent symptom thought at that time a sufficient reason for doing it.

They have their lucky and unlucky days for operating or taking any medicine; but I have known them get the better of this prejudice, and be prevailed on. Cupping is much practised by them; a horn, about the size of a cupping glass, is applied to the part, and by a small aperture at the other end they extract the air with their mouth. The part is afterwards scarified with a lancet. This is often done on the back; and in pain and swelling of the knee it is held as a sovereign remedy. I have often admired their dexterity in operating with bad instruments. Mr. Hamilton gave them some lancets, and they have since endeavoured, with some success, to make them of that form. They were very thankful for the few I could spare them. In fevers they use the kuthullega nut, well known in Bengal as an efficacious medicine. They endeavour to cure the dropsy by external applications, and giving a compounded medicine made up of above 30 different ingredients: they seldom or never succeed in effecting a cure of this disease. I explained to the Rajah the operation of tapping, and showed him the instrument with which it was done. He very earnestly expressed a desire that I should perform the operation, and wished much for a proper subject; such a one did not occur while I remained, and perhaps it was as well both for the Rajah's patients and my own credit; for after having seen it once done, he would not have hesitated about a repetition of the operation. Gravelish complaints and the stone in the bladder are probably diseases unknown here.

The small-pox, when it appears among them, is a disease that strikes them with too much terror and consternation to admit of their treating it properly. Their attention is not employed in saving the lives of the infected, but in preserving themselves from the disease. All communication with the infected is strictly forbidden, even at the risk of their being starved, and the house or village is afterwards erased. A promiscuous and free intercourse with their neighbours not being allowed, the disease is very seldom to be met with, and its progress always checked by the vigilance and terror of the natives. Few in the country have had the disease. Inoculation, if ever introduced, must be very general to prevent the devastation that would be made by the infection in the natural way; and where there could not be any choice in the subject fit to receive the disease, many must fall a sacrifice to it. The present Rajah of Thibet was inoculated, with some of his followers, when in China with the late Tishoo Lama. The hot bath is used in many disorders, particularly in complaints of the bowels and cutaneous eruptions. The hot wells of Thibet are resorted to by thousands. In Boutan they substitute water warmed by hot stones thrown into it. In Thibet the natives are more subject to sore eyes and blindness than in Boutan. The high winds, sandy soil, and glare from the reflection of the sun, both from the snow and sand, account for this. I have dwelt long on this subject, because I think the knowledge and observations of these people on the diseases of their country, with their medical practice, keep pace with a refinement and state of civilization, which struck me with wonder, and no doubt will give rise to much curious speculation, when known to be the manners of a people holding so little intercourse with what we term civilized nations.

Dec. 1. Left Tishoolumbo, and found the cold increase every day as we advanced to the southward, most of the running waters frozen, and the pools covered with ice strong enough to carry. Our thermometer having only the scale as low as 16°, we could not precisely determine the degree of cold, the quicksilver being under that every morning. The frost is certainly never so intense in Great-Britain. On our return to the lakes the 14th, we found them deserted by the water fowl, and were informed that they had been one solid piece of ice since the 10th of November. Here we resumed our amusement of skating, to the great astonishment of the natives and Bengal servants.

On the 17th we re-entered Boutan, and in 6 days more arrived at Punukha by Paraghon. No snow or frost to be met with in Boutan, except towards the tops of their highest mountains; the thermometer rising to 36° in the morning, and 48° at noon. Took leave of the Debe Rajah, and on the 12th arrived at Buxaduar. Calcutta, Feb. 17, 1784.

As Lac is the produce of, and a staple article of commerce in Assam, a country bordering on and much connected with Thibet, some account of it may not be an improper supplement to the above remarks. Lac is, strictly speaking, neither a gummy nor resinous substance, though it has some properties in common to both. Gums are soluble in water, and resins in spirits; lac admits of a very difficult union with either, without the mediation of some other agent. Lac is known in Europe by the different appellations of stick lac, seed lac, and shell lac. The first is the lac in pretty considerable lumps, with much of the woody parts of the branches on which it is formed adhering to it. Seed lac is only the stick lac broke into small pieces, garbled, and appearing in a granulated form. Shell lac is the purified lac, by a very simple process to be mentioned afterward.

Many vague and unauthenticated reports concerning lac have reached the public; and though among the multiplicity of accounts the true history of this substance has been nearly hit on, little credit is given in Europe to any description of it hitherto published. My observations, as far as they go, are the result of what I have seen, from the lac on the tree, the progress of the insect now in my custody, and the information of a gentleman residing at Goalpara on the borders of Assam, who is perfectly versant in the method of breeding the insect, inviting it to the tree, collecting the lac from the branches, and forming it into shell lac, in which state much of it is received from Assam, and exported to Europe for various great and useful purposes. The tree on which this fly most commonly generates is known in Bengal by the name of the Biher tree, and is a species of the rhamnus. The fly is nourished by the tree, and there deposits its eggs, which nature has provided it with the means of defending from external injury by a collection of this lac, evidently serving the twofold purpose of a nidus and covering to the ovum and insect in its first stage, and food for the maggot in its more advanced state. The lac is formed into complete cells, finished with as much regularity and art as a honey-comb, but differently arranged. The flies are invited to deposit their eggs on the branches of the tree, by besmearing them with some of the fresh lac steeped in water, which attracts the fly, and gives a better and larger crop. The lac is collected twice a year, in the months of February and August.

I have examined the egg of the fly with a very good microscope; it is of a very pure red, perfectly transparent, except in the centre, where are evident marks of the embryo forming, and opaque ramifications passing off from the body of it. The egg is perfectly oval, and about the size of an ant's egg. The maggot is about the 8th of an inch long, formed of 10 or 12 rings, with a small red head; when seen with a microscope, the parts of the head were easily distinguished, with 6 small specks on the breast, somewhat projecting, which seemed to be the incipient formation of the feet. This maggot is now in my custody, in the form of a nymph or crysalis, its annular coat forming a strong covering, from which it should issue forth a fly. I have never seen the fly, and cannot therefore describe it more fully, or determine its genus and species. I am promised a drawing of the insect in its different stages, and shall be able soon to add to a botanical description of the plant, a drawing of the branch, with the different parts of fructification and lac on it. The gentleman to whom I owe part of my information terms the lac the excrement of the insect. On a more minute investigation however, we may not find it more so than the wax or honey of the bee, or silk of the silk-worm. Nature has provided most insects with the means of secreting a substance which generally answers the twofold purpose of defending the embryo, and supplying nourishment to the insect from the time of its animation till able to wander abroad in quest of food. The fresh lac contains within its cells a liquid, sweetish to the taste, and of a fine red colour, miscible in water. The natives of Assam use it as a dye, and cotton dipped in this liquid makes afterwards a very good red ink.

The simple operation of purifying lac is practised as follows. It is broken into small pieces, and picked from the branches and sticks, when it is put into a sort of canvas bag of about 4 feet long, and not above 6 inches in circumference. Two of these bags are in constant use, and each of them held by 2 men. The bag is placed over a fire, and frequently turned till the lac is liquid enough to pass through its pores, when it is taken off the fire, and squeezed by 2 men in different directions, dragging it along the convex part of a plantain-tree prepared for the purpose; while this is doing, the other bag is heating, to be treated in the same way. The mucilaginous and smooth surface of the plantain-tree seems peculiarly well adapted for preventing the adhesion of the heated lac, and giving it the form which enhances its value so much. The degree of pressure on the plantain-tree regulates the thickness of the shell, and the quality of the bag determines its fineness and transparency. They have learned of late that the lac, which is thicker in the shell than it used to be, is most prized in Europe. Assam furnishes us with the greatest quantity of lac in use; and it may not be generally known, that the tree on which they produce the best and largest quantity of lac, is not uncommon in Bengal, and might be employed in propagating the fly, and cultivating the lac, to great advantage. The small quantity of lac collected in these provinces affords a precarious and uncertain crop, because not attended to. Some attention at particular seasons is necessary to invite the fly to the tree; and collecting the whole of the lac with too great an avidity, where the insect is not very generally to be met with, may annihilate the breed.

The best method of cultivating the tree, and preserving the insect, being properly understood in Bengal, would secure to the Coss possessions the benefit arising from the sale of a lucrative article, in great demand and of extensive use.

A Meteorological Journal kept at the Apartments of the Royal Society, for the Year 1788, by Order of the President and Council. p. 113.

A synopsis for the whole year is as follows:

1788.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January	48	26	39.7	56	49	52.7	30.70	28.89	29.97	0.439
February ..	50	29	41.3	56	50	52.7	30.21	28.65	29.68	1.461
March	59	28	40.8	59	46	50.9	30.08	29.32	29.68	0.336
April......	68	40	52.6	63	55	51.8	30.48	29.50	30.07	0.607
May	80	49	60.0	73	59	62.8	30.34	29.58	30.04	0.497
June	80	52	62.3	70	61	64.1	30.22	29.49	29.94	3.275
July	77	55	63.7	69	64	65.9	30.22	29.73	29.99	1.620
August	77	53	63.4	71	64	66.0	30.45	29.22	29.95	2.699
September..	74	45	58.6	67	59	63.7	30.25	29.37	29.86	3.345
October	67	33	51.4	62	54	59.4	30.55	29.64	30.32	0.103
November..	58	27	42.9	61	47	56.4	30.50	29.61	30.11	0.510
December ..	46	18	30.9	51	39	45.2	30.33	29.50	29.92	0.000
Whole year			50.6			57.6			29.96	14.892

XI. Experiments on the Phlogistication of Spirit of Nitre. By the Rev. J. Priestley, LL. D., F. R. S. p. 139.

In my former experiments, vol. 4, p. 2, I found that the colourless acid became smoking, or orange-coloured, and emitted orange-coloured vapours, on being exposed to heat in long glass tubes, hermetically sealed; and I then concluded, that this effect was produced by the action of heat, evolving, as it were, the phlogiston previously contained in the acid. Afterwards, having found that it was not heat, but light only, that was capable of giving colour to spirit of nitre, contained in phials with ground stoppers, in the course of several days; and that in this case the effect was produced by the action of light on the vapour, which gradually imparted its colour to the liquor on which it was incumbent (see vol. 5, p. 342,) I was led to suspect, that as the glass tubes, in which I had formerly exposed this acid to the action of heat, were only held near to a fire, in the day-light, or candle-light, it might have been this light, which in these circumstances had, at least in part, contributed to produce the effect.

In order to ascertain whether the light had had any influence in this case, I now put the colourless spirit of nitre into long glass tubes, like those which I had used before, and also sealed them hermetically, as I had done the others; but, instead of exposing them to heat in the open air, from which light could not be excluded, I now shut them up in gun barrels, closed with metal screws, so that it was impossible for any particle of light to have access to them; and I then placed one end of the barrels so near a fire as was sufficient to make the liquor contained in the tube to boil, which I could easily distinguish by the sound it yielded. The consequence was, that in a short time the acid became as highly coloured as ever it had been when exposed to heat without the gun barrel. It was evident therefore, that it had been mere heat, and not light, which had been the means of giving this colour to the acid, and which has been usually termed phlogisticating it.

When I made the former experiments, I had no suspicion that the air contained in the tube had any concern in the result of them; and, in those which I made in the phials in a moderate heat, I found that the acid received its colour when the best vacuum that I could make with an air pump was over it. My friend Mr. Kirwan, however, having always suspected, that the air was a principal agent in the business, I at this time gave particular attention to this circumstance; supposing that, if any part of the common air had been imbibed, it must have been the phlogisticated, and that it was the phlogiston from this kind of air which had phlogisticated the acid. The real result however was not so much in favour of this supposition as I had expected; for the principal effect of the process was the emission of dephlogisticated air, so that the acid seems to become what we call phlogisticated, by parting with this ingredient in its composition.

I put a small quantity of the colourless acid into a long glass tube, which, besides the acid, would have contained 1.23 ounce measures of common air, but that the vapour of the acid excluded about $\frac{1}{20}$ of the quantity. Having sealed the tube hermetically, I shut it up in a gun barrel, in the manner above mentioned, and exposed it to a boiling heat for several hours, and then opening it under water, there came out of it 2.03 ounces measures of air, very turbid and white; and when examined, it appeared to be of the standard of 1.02, with 2 equal measures of nitrous air; when with 1 measure of the same nitrous air the standard of the common air was 1.07.

In order to exclude all air from the contact of the acid, I made a quantity of it to boil in the tube, and when the vapour had expelled all the air, I sealed it hermetically, in the manner in which water hammers are made; and then exposing it to heat, found that it acquired as high a colour as when air had been confined along with it; so that it is evident, that air is not necessary to this effect. When the tube was opened under water, a quantity of dephlogisticated air rushed out, exceedingly white as before; but when I examined it, I found it to be of the standard of only 0.66. When this impurity is considered, it will appear, that when much air is yielded in this process, some phlogisticated air may have been imbibed, though, computing in the manner above mentioned, the phlogisticated air after the process should be in greater quantity than was contained in the tube before it, as was the case in the following experiment. In a glass tube which, besides the acid, contained 1.13 oz. m. of common air, I exposed colourless spirit of nitre to heat till it became of a deep orange colour; and when it was opened under water, there came out of it 2.83 oz. m. of air exceedingly turbid, of the standard of 0.66, with 2 equal quantities of nitrous air, when that of the common air, with one equal quantity of nitrous air, was 1.07. Computing in the manner above mentioned, there was in the tube before the process 0.7477 oz. m. of phlogisticated air, and after the process 0.8792 oz. m. But the dephlogisticated air, amounting to 1.7 oz. m. being of the standard of 0.66, will be found to contain 0.374 oz. m. of phlogisticated air, which being deducted from 0.8792, there will remain only 0.5052 oz. m. which is considerably less than 0.7477 oz. m.

Having repeatedly observed, that the acid became coloured in consequence of being exposed to heat in contact with any kind of air whatever, I exposed at the same time, and in the same circumstances, 3 equal quantities of the same colourless spirit of nitre, in 3 nearly equal tubes, one containing dephlogisticated, another phlogisticated, and a 3d inflammable air; that, if there should be any difference in the colouring of the acid in these cases, it might be the more easily perceived. But though I gave all the attention that I could, I did not perceive that there was any difference, except what arose from some of the tubes being placed a little nearer the fire than the rest; and, by changing their places, the colour was at length the very same in them all.

As the spirit of nitre can be rendered smoking, or phlogisticated, by the mere expulsion of dephlogisticated air, it is evident that it contains 2 principles in close affinity with each other, and that nothing is necessary to render either of them conspicuous besides the absence of the other.

It is also natural to suppose that, for the same reason that the dephlogisticating principle, as it may be called, is expelled, the phlogisticating principle should enter; so that the purification of the air in contact with the acid may be a necessary consequence of the expulsion of the pure air contained in it, the whole tending, as it were, to an equilibrium in this respect. It is therefore by no means difficult to conceive, that phlogiston should be extracted from the contiguous air at the same time that the dephlogisticated air not pure, that is containing a mixture of phlogisticated air, is driven out of it; for the acid always containing phlogistion, whatever air is contained in it, and expelled from it, may necessarily contain phlogiston or phlogisticated air; but the purer air may be emitted, and the less pure air be imbibed, till the whole come to be of the same quality. It may however perhaps follow from the emission of impure dephlogisticated air, and the imbibing of phlogisticated air at the same time, that the former does not consist of dephlogisticated and phlogisticated air loosely mixed, but of some intimate union of dephlogisticated air with phlogiston, though they may be separated by a mixture of nitrous air, and other processes, in the very same manner as dephlogisticated air may be separated from a loose mixture of phlogisticated air. It is evident from these experiments, that a red heat is not necessary to the conversion of nitrous acid into pure air, though this process, as appeared by my former experiments, produces this effect most quickly and effectually.

I cannot help considering the experiments above recited to be favourable to the doctrine of the phlogiston, and unfavourable to that of the decomposition of water, though not decisively so; for since the red vapour of spirit of nitre unquestionably contains the same principle that has been termed phlogiston, or the principal element in the constitution of inflammable air, and according to the antiphlogistians this is one constituent part of water, they must suppose, that the water in this acid is decomposed by a much more moderate heat than in most other cases. In general I believe they have thought a red heat to be necessary for this purpose. It is evident, that the conversion of water into steam by boiling, or by any heat that can be given to it under the strongest pressure, has no tendency whatever to decompose it. But if the mere boiling of water in nitrous acid could produce this effect, I do not see why the same should not be the case when water alone is boiled. I think it will also be more difficult to explain the purification of the incumbent atmospherical air on the antiphlogistic than on the phlogistic hypothesis, whatever be the constitution of phlogisticated air.

As, in the experiments above mentioned, heat without light gives colour to the nitrous acid, and the reflection or refraction of light is always attended with heat, it may perhaps be heat universally that is the means of imparting this colour, though the mode of its operation be at present unknown. And in these experiments, as well as the former, it is the vapour that first receives the colour, and imparts it to the liquid when it is sufficiently cold to receive it. The rushing out of a quantity of turbid white air from a transparent tube, quite cold, is a striking phænomenon in these experiments. It may be worth while to examine of what it is that this remarkable cloudiness of the air consists. There is the same appearance in the rapid production of any kind of air, which is perfectly transparent, as it passes along the glass tube through which it is transmitted, till it comes into contact with the water in which it is received.

For further observations, we may refer to Dr. P's separate publications on this subject.

XII. Observations on a Comet. By Wm. Herschel, LL. D., F. R. S. p. 151.

December 21, 1788, about 8 o'clock, I viewed the comet which my sister had a little while before pointed out to me with her small Newtonian sweeper. In my instrument, which was a 10 feet reflector, it had the appearance of a considerably bright nebula; of an irregular, round form; very gradually brighter in the middle; and about 5 or 6 minutes in diameter. The situation was low, and not very proper for instruments with high powers. Dec. 22, about half after 5 in the morning, I viewed it again, and perceived that it had moved apparently in a direction nearly towards δ Lyræ. I had been engaged all night with the 20 feet instrument, so that there had been no leisure to prepare my apparatus for taking the place of the comet; but in the evening of the same day, I took its situation 3 times, as follows: viz. Dec. 22, sidereal time,

At	$23^h\ 42^m\ 19^s$	$23^h\ 52^m\ 52^s$	$0^h\ 6^m\ 35^s$	comet passed the wire.
At	23 49 24	23 59 58	0 13 40	β Lyræ passed it.
Diff.	00 7 5	00 7 6	0 7 5	

I found in every observation the small star which accompanies β Lyræ, exactly in the parallel of the comet. These transits were taken with a 10 feet reflector; and the difference in right ascension, I should suppose, may be depended on to within a second of time. The determination also of the parallel can hardly err so much as 15″ of a degree. This, and several evenings afterwards, I viewed the comet again with such powers as its diluted light would permit, but could not perceive any sort of nucleus, which, had it been a single second in diameter, I think, could not well have escaped me. This circumstance seems to be of some consequence to those who turn their thoughts on the investigation of the nature of comets; especially as I have also formerly made the same remark on one of the comets discovered by M. Mechain in 1787, a former one of my sister's in 1786, and one of Mr. Pigott's in 1783; in neither of which any defined, solid nucleus could be perceived.

XIII. Indications of Spring, observed by Robert Marsham, Esq., F. R. S., of Stratton in Norfolk. Latitude 52° 45′.

This is merely a register, for each year from 1736 till 1788, of the earliest date, month and day in each year, when the following circumstances occurred or took place, each being arranged in a separate column; viz. snowdrop flower, thrush sings, hawthorn leaf, hawthorn flower, frogs and toads croak, sycamore leaf, birch leaf, elm leaf, mountain-ash leaf, oak leaf, beech leaf, horse-chesnut leaf, chesnut leaf, hornbeam leaf, ash leaf, ringdoves coo, rooks build, young rooks, swallows appear, cuckoo sings, nightingale sings, churn owl sings, yellow butterfly appears, turnip in flower, lime leaf, maple leaf, wood anemone flower.

XIV. Account of a Monster of the human Species, in two Letters; one from Baron Reichel to Sir Jos. Banks, Bart., and the other from Mr. Jas. Anderson to Baron Reichel. p. 157.

To Sir Joseph Banks, Bart. *Fort St. George, Feb.* 28, 1788.

SIR,—I have the pleasure to transmit to you the portrait of a Gentoo boy, an astonishing living subject, who being sent to me by a friend of mine residing in the environs of the native place of the boy, I made 2 drawings representing the alternate attitudes in which he can place half the body of his little brother, who adheres to his breast. Peruntaloo is a handsome well-made lad, possessing every due faculty of mind and body, rather more sagacious, and with a superior share of understanding, than young men in general of his age. In addition to the inclosed anatomical description of the boy by Mr. Anderson, you will observe in the drawings 2 circular dotted lines, about the lower part of the loins of the semi-monster. During the several sittings I had of Peruntaloo, I observed an internal motion about these parts rather more conspicuous than any other of the body; and on questioning the youth, he showed me, that by retaining his breath, he could force a current of air into them, so as to swell the parts like 2 blown-up bladders, with a rumbling noise at the time of action. Whether there is a connection with the lungs of Peruntaloo is a question I cannot venture to determine; Mr. Anderson however thinks it well worth my mentioning this observation. The erection of the little penis in the semi-monster, and the command Peruntaloo has of discharging the urine through it, are perfectly ascertained.

I am, &c. T. REICHEL.

To Baron Reichel. *Fort St. George, Feb.* 25, 1788.

SIR,—As you mean to send the elegant drawing of Peruntaloo to Sir Joseph

Banks, you may acquaint him from me, that the little brother is suspended by the os pubis; an elongation of the sword-like cartilage of Peruntaloo having anastomosed with that bone at the symphysis. The lower orifice of the stomach seems to lie in the sac or cylindrical cavity between the two brothers on the right side, and what may be reckoned the right hypochondre of the little one, as that part is tumid and full after eating. The alimentary canal must be common to both, as the anus of the little one is imperforate. There is a bladder of urine distinctly perceived, which occupies the left side of the sac, or left hypochondre of the monster. Besides which, there remain only the sacrum, ossa innominata, and lower extremities perfect.

Peruntaloo says he has as complete a sense of feeling with every part of the body of his little brother as of his own proper body, and this may account for the erections you saw, and making water distinctly; but this volition does not extend to the legs or feet, which are cold in comparison with the rest.

I am, &c. JAMES ANDERSON.

XV. A Supplementary Letter on the Identity of the Species of the Dog, Wolf, and Jackal; from John Hunter, Esq. F. R. S. p. 160.

In the year 1787 I had the honour of presenting to the R. S. a paper to prove the wolf, the jackal, and the dog to be of the same species. But as the complete proof of the wolf being a dog, which consisted in the half-bred puppy breeding again, had not been under my own inspection, though sufficiently well authenticated, I saved a female of one of the half-bred puppies, mentioned in that paper, in hopes of being myself a witness of the fact; but when the period of impregnation arrived, we unluckily missed that opportunity. However, another half-bred puppy has had young, which is equally satisfactory to me as if my own had bred. John Symmons, Esq. of Milbank, has had a female wolf in his possession for some time, which was lined by a dog, and brought forth several puppies. This was a very short time after the brood had been produced by Mr. Gough's wolf, the subject of my former paper, therefore the puppies were nearly of an age with mine. These puppies Mr. Symmons has reared; only one of them was a female, and she had much more of the mother or wolf in her than any of the rest of the same litter. I communicated my wish to Mr. Symmons, that either his puppy or mine should prove the fact to our own knowledge; which he immediately, with great readiness, acceded to. On the 16th, 17th, and 18th of December, 1788, this bitch was lined by a dog, and on the 18th of February she brought 8 puppies, all of which she now rears. If we reckon from the 16th of December, she went 64 days; but if we reckon from the 17th, the mean time, then it is 63 days, which is the usual time for a bitch to go with pup. These puppies are the 2d remove from the wolf and dog, similar to that given by my Lord Clanbrassil to the Earl of Pembroke, which bred again. (See Philos. Trans. vol. 77, p. 255.) It would have proved the same fact if she had been lined by either a wolf, a dog, or one of the males of her own litter.

I may just remark here, that the wolf seems to have only one time in the year for impregnation natural to her, and that is in the month of December; for every time Mr. Gough's wolf has been in heat was in this month, and it proves to be the same month in which Mr. Symmons's wolf was in heat; for his half-bred wolf is nearly of the same age with mine, and the time she was in heat was also the same with that of her own mother, and the present brood corresponds in time with the brood of Mr. Gough's wolf.

XVI. Abstract of a Register of the Barometer, Thermometer, and Rain, at Lyndon in Rutland, for 1788. *By T. Barker, Esq. Also of the Rain in Hampshire and Surrey,* p. 162.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.					Hampshire.	
		Highest.	Lowest.	Mean.	Hig.	Low	Mean	Hig.	Low	Mean	Lyndon.	Surry. S. Lamb.	Selbourn	Fyfield.
		Inches.	Inches.	Inches	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30.13	28.37	29.50	44	34	40	45	23½	36	0.970	0.68	1.60	1.10
	Aftern.				44	35	40½	49	30	41				
Feb.	Morn.	29.77	28.25	29.14	45	35	40½	44	27½	36	2.667	2.09	3.37	2. 6
	Aftern.				45	37	41	48	30	42				
Mar.	Morn.	29.65	28.84	29.23	51	34	40	50	22	35	1.072	0.64	1.31	1.36
	Aftern.				52½	35½	41	63	31	43				
Apr.	Morn.	30.02	28.94	29.59	56	42½	50	54	35	45½	0.588	0.47	0.61	0.50
	Aftern.				60	43½	51	68½	40	56				
May	Morn.	29.92	29.19	29.60	68½	51	58	64	43½	52½	1.517	0.81	0.76	0.28
	Aftern.				72	53	60	82	51	66				
June	Morn.	29.85	29.10	29.52	65½	56	60	64½	50	56	0.608	1.94	1.27	1.36
	Aftern.				69	57½	61	82½	58	67				
July	Morn.	29.78	29.21	29.52	67½	58	62	70½	51	59	1.795	1.84	3.58	1.81
	Aftern.				70	59½	63½	83	58	72				
Aug.	Morn.	30.01	28.88	29.49	68	57½	61½	64	54	56	2.780	4.30	3.22	3.40
	Aftern.				70½	59	63	77	62	68				
Sept.	Morn.	29.80	29.00	29.40	66	52½	58½	61½	42	52	2.430	3.81	5.71	3.78
	Aftern.				66½	53	59	75½	50	63				
Oct.	Morn.	30.15	29.15	29.68	59	46	52	57	32	46	1.412	0.08	0.00	0.03
	Aftern.				60	47	53	66	45	54½				
Nov.	Morn.	30.01	29.06	29.62	53	37	45	51½	25½	39	0.453	0.62	0.86	0.74
	Aftern.				53	37½	46	58	31	45				
Dec.	Morn.	29.85	29.12	29.47	39	27	34	40½	15	27	0.890	0.00	0.21	0.42
	Aftern.				40½	28	34½	44½	22½	31½				
Means and sums				29.48			50½			49⅔	17.182	17.28	22.50	16.84

XVII. On the Method of Correspondent Values, &c. By Edw. Waring, M.D., F.R.S. p. 166.

§ 1.—1. In the year 1762 I published a method of finding when 2 roots of a given equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + \&c. = 0$ are equal, by finding the common divisors of the 2 quantities $a^n - pa^{n-1} + qa^{n-2} - \&c.$, and $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$, and observed if they admitted only one simple divisor, $a - \text{A}$, then 2 roots only were equal; if a quadratic, $a^2 - \text{A}a + \text{B}$, then 2 roots of the equation became twice equal; if a cubic, $a^3 - \text{A}a^2 + \text{B}a - \text{C}$, then 2 roots became thrice equal; and so on: or, to express in more general terms what follows from the same principles, if the common divisor be $\overline{a - b}^r \times \overline{a - c}^s \times \overline{a - d}^t \times \&c.$, then $r + 1$ roots of the given equation will be b, $s + 1$ roots will be c, $t + 1$ will be d, &c.; and it immediately follows, from the principles delivered in the 2d edition of the same book, published in 1770, that to find when $r + 1$, $v + 1$, $t + 1$, &c. roots are respectively equal, requires $r + s + t$, &c. equations of condition, which are deducible from the well-known method of finding the common divisors of 2 quantities in this case of $a^n - pa^{n-1} + qa^{n-2} - \&c.$, $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$ of the terms of their remainders, &c.

In the book above-mentioned the equations of condition are given, which discover when 2 roots are equal in the equations $x^3 - px^2 + qx - r = 0$, $x^4 + qx^2 - rx + s = 0$, $x^5 + qx^3 - rx^2 + sx - t = 0$, in the 2 latter equations the 2d term is wanting, which may easily be exterminated; but it may as easily be restored by substituting for q, r, s, &c. in the equation of condition found the quantities resulting from the common transformation of equations to destroy the 2d term.

2. Another rule contained in the same book is the substitution of the roots of the equation $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c. = 0$ respectively for a in the quantity $a^n - pa^{n-1} + qa^{n-2} - \&c.$, and multiplication of of all the quantities resulting into each other; their content will give the equation of condition, when 2 roots are equal. Mr. Hudde first discovered, that if the successive terms of the given equation are multiplied into an arithmetical series, the resulting equation will contain one of any 2 equal roots, and m of the $m + 1$ equal roots in the given equation.

3. If 3, 4, 5, . . r roots of the equation are equal, find a common divisor of 3, 4, 5, . . r of the subsequent quantities $a^n - pa^{n-1} + qa^{n-2} - \&c.$, $na^{n-1} - (n-1)pa^{n-2} + (n-2)qa^{n-3} - \&c.$, $n \cdot (n-1)a^{n-2} - (n-1) \cdot (n-2)pa^{n-3} + (n-2) \cdot (n-3)qa^{n-4} - (n-3) \cdot (n-4)ra^{n-5} + \&c.$, $n \cdot (n-1) \cdot (n-2)a^{n-3} - (n-1) \cdot (n-2) \cdot (n-3)pa^{n-4} + (n-2) \cdot (n-3) \cdot (n-4) qa^{n-5} - \&c.$, . . . $n \cdot (n-1) \cdot (n-2) \,.\,.\, (n-r+2)a^{n-r+1} - (n-1) \cdot (n-2) \,.\,.\, (n-r+1)pa^{n-r} + \&c.$; which will probably be best done by dividing all the preceding quantities by the quantity of the least dimension of a, and the divisor and all the remainders by that quantity which has the least dimensions among them; and so on: there will result 2, 3, 4, . . $r - 1$ equations of condition; and in this case it is observed, in the before-mentioned book, that if the

common divisor be $a - \text{A}$, it will once only admit of 3, 4, 5, . . r equal roots; if it be a quadratic, then it will twice admit of those equal roots; and so on.

4. If the roots of the equation of the least dimensions be substituted for a in the remaining equations, and each of the resulting values of the same equation be multiplied into each other, there will result the $r - 1$ equations of condition: and the same may be deduced also from the several equations conjointly. The equations of conditions found by the first method, if the divisions were not properly instituted, may admit of more rational divisors than necessary, of which some are the equations of conditions required.

§ 2.—1. In the year 1776, I published in the Meditationes Analyticæ a new method of differences for the resolution of the following problem. Given the sums of a swiftly converging series $ax + bx^2 + cx^3 + dx^4 +$ &c., when the values of x are respectively π, ϱ, ς, &c.; to find the sum of the series when x is τ, that is, given $s\pi = a\pi + b\pi^2 + c\pi^3 + d\pi^4 +$ &c., $s\varrho = a\varrho + b\varrho^2 + c\varrho^3 +$ &c., $s\sigma = a\sigma + b\sigma^2 + c\sigma^3 +$ &c. &c.; to find $s\tau = a\tau + b\tau^2 + c\tau^3 +$ &c.

To resolve this problem I multiplied the quantities, $s\pi$, $s\varrho$, $s\sigma$, &c. respectively into unknown co-efficients α, β, γ, &c. and there resulted as in the margin; and then made the sum of each of the terms respectively equal to its correspondent term of the quantity $\tau a + \tau^2 b + \tau^3 c +$ &c., and consequently $\alpha\pi + \beta\varrho + \gamma\sigma +$ &c. $= \tau$, $\alpha\pi^2 + \beta\varrho^2 + \gamma\sigma^2 +$ &c. $= \tau^2$, $\alpha\pi^3 + \beta\varrho^3 + \gamma\sigma^3 +$ &c. $= \tau^3$, &c. I assumed as many equations of this kind as there were given values π, ϱ, σ, &c. of x; and consequently as many equations resulted as unknown quantities α, β, γ, &c.; whence, by the common resolution of simple equations, or more easily from differences, can be found the unknown quantities α, β, γ, &c., and thence the equation sought $\alpha \times s\pi + \beta \times s\varrho + \gamma \times s\sigma +$ &c. $= s\tau$ nearly.

$$\begin{array}{l} \alpha\pi a + \alpha\pi^2 b + \alpha\pi^3 c + \text{\&c.} \\ \beta\varrho a + \beta\varrho^2 b + \beta\varrho^3 c + \text{\&c.} \\ \gamma\sigma a + \gamma\sigma^2 b + \gamma\sigma^3 c + \text{\&c.} \\ \quad \text{\&c.} \quad \text{\&c.} \quad \text{\&c.} \end{array}$$

2. In the Meditationes are assumed for π, ϱ, σ, &c. the quantities p, $2p$, $3p$, $4p$, . . . $n - 2p$, $n - 1p$, and np for τ; which, if substituted for their values in the preceding equations, will give $\alpha + 2\beta + 3\gamma + 4\delta +$ &c. $= n$, $\alpha + 4\beta + 9\gamma + 16\delta +$ &c. $= n^2$, $\alpha + 8\beta + 27\gamma +$ &c. $= n^3$, $\alpha + 16\beta + 81\gamma +$ &c. $= n^4$; and if the sums of the series $ax + bx^2 + cx^3 +$ &c. which respectively correspond to the values p, $2p$, $3p$, . . . $n - 1p$ of x be s1, s2, s3, s4, . . . s$(n-1)$, and the sum of the series $ax + bx^2 + cx^3 +$ &c. which corresponds to n value of x be sn; then will $sn = ns(n-1) - n \cdot \frac{n-1}{2} s(n-2) + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} s(n-3) \ldots \pm ns1$ nearly, which equation is given in the above-mentioned book.

3. The logarithm from the number, the arc from the sine, &c. are found by serieses of the formula $ax + bx^2 + cx^3 +$ &c.; and consequently this equation is applicable to them.

4. In the same book is assumed a series $ax^r + bx^{r+s} + cx^{r+2s} + dx^{r+3s} +$ &c. of a more general formula than the preceding, and in it for x substituted α, β, γ, δ, &c., m; and sα, sβ, sγ, sδ, &c.; sm for the resulting sums, and thence deduced s$m =$

$$\frac{m^r \times m^s - \beta^s . m^s - \gamma^s . m^s - \delta^s . \&c.}{\alpha^r \times \alpha^s - \beta^s . \alpha^s - \gamma^s . \alpha^s - \delta^s . \&c.} \times s\alpha + \frac{m^r \times m^s - \alpha^s . m^s - \gamma^s . m^s - \delta^s . \&c.}{\beta^r \times \beta^s - {}^s . \beta^s - \gamma^s . \beta^s - \delta^s . \&c.} \times s\beta + \frac{m^r \times m^s - \alpha^s . m^s - \beta^s . m^s - \delta^s . \&c.}{\gamma^r \times \gamma^s - \alpha^s . \gamma^s - \beta^s . \gamma^s - \delta^s . \&c.} \times s\gamma + \frac{m^r \times m^s - \alpha^s . m^s - \beta^s . m^s - \gamma^s \times \&c.}{\delta^r \times \delta^s - \alpha^s . \delta^s - \beta^s . \delta^s - \gamma^s . \&c.}$$

$\times$ sδ + &c. nearly.

Cor. If for r and s be assumed respectively 1, the series becomes $ax + bx^2 + cx^3 +$ &c. of the same formula as the preceding: if $r = 0$ and $s = 1$, the series becomes $a + bx + cx^2 +$ &c. The latter case will be the same as the former, when one of the quantities α, substituted for x and its correspondent sum sα, both become $= 0$, and the equation deduced in both cases the same.

5. If π, ϱ, σ, &c. respectively denote $r, r + p, r + 2p, .. r + (n-2)p, r + (n-1)p$, and $\tau = r + np$; and s, s1, s2, s3, .. s$(n-2)$, s$(n-1)$, be the sums either resulting from the series $ax + bx^2 + cx^3 +$ &c. or the series A $+ ax + bx^2 + cx^3 +$ &c., which respectively correspond to the values r, $r + p$, $r + 2p$, &c. of x; and sn the sum of the same series which corresponds to the value $r + np$ of x; then will s$n = n$s$(n-1) - n . \frac{n-1}{2}$ s$(n-2) + n . \frac{n-1}{2} . \frac{n-2}{3}$ s$(n-3)$ $- .. \pm n . \frac{n-1}{2}$ s2 $\mp n$s1 $\pm$ s nearly; this equation differs from the preceding by the last term s not vanishing; in the preceding case s became $= 0$, for it was the sum of the series $ax + bx^2 + cx^3 +$ &c. which corresponded to $x = 0$.

6. From the Meditationes it appears that $r^m - n \times (r \pm p)^m + n . \frac{n-1}{2} (r \pm 2p)^m - n . \frac{n-1}{2} . \frac{n-2}{3} (r \pm 3p)^m +$ &c. to the end of the series $= 0$, if m is less than n, and m and n are whole numbers; but if $m = n$, then it will $= \pm 1 . 2 . 3 . 4 ... n - 1 . np^m$; whence it is manifest, that for the n first terms of the series A $+ ax + bx^2 +$ &c. the equations are true; and for the $n - 1$ first terms of the series $ax + bx^2 + cx^3 +$ &c. and in the successive term of both the serieses, they will err by a quantity nearly $= \pm 1 . 2 . 3 .. n \times p^n \times r^{-n} \times$ co-efficient of the term; and the errors of every subsequent term x^{4+n}, will be nearly as $\pm m . \frac{m-1}{2} . \frac{m-2}{3} . \frac{m-3}{4} ... \frac{m-h+1}{h} \times p^n \times r^{-} \times$ co-efficient of the term x^{b+n}, if for r, $r + p$, $r + 2p$, &c. be substituted $1, 1 + \frac{p}{r}, 1 + \frac{2p}{r}$, &c.

7. Let the preceding equation s$n = n$s$(n-1) - n . \frac{n-1}{2}$ s$(n-2) + n . \frac{n-1}{2} . \frac{n-2}{3}$. s$(n-3)$ $-$ &c. $= n \times \log. (r - p) - n . \frac{n-1}{2} \log. (r - 2p) + n . \frac{n-1}{2} . \frac{n-2}{3} \log. (r - 3p) +$ &c. but $\log. r - n \times \log. (r - p) + n . \frac{n-1}{2} \log. (r - 2p) - n . \frac{n-1}{2} . \frac{n-2}{3} \times \log. (r - 3p) +$ &c. $= \log.$

$\frac{r \times (r-2p)^{s} \times (r-4p^{s'}) \times (r-6p^{s''}) \times \&c.}{(r-p^{t}) - (r-3p^{t'}) \times (r-5p^{t''}) \times \&c.} = \log. \text{K}$, where s, s', s'', &c. denote the co-efficients of the alternate terms of the binomial theorem, viz. $s = n \cdot \frac{n-1}{2}$, $s' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}$, &c., and $t = n$, $t' = n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}$, &c. the co-efficients of the remaining alternate terms; the numerator $r \times (r-2p^{s}) \times (r-4p)^{s'} \times (r-6p)^{s''} \times$ &c. $=$ (if $\text{N} = 2^{n-1}$) $r^{\text{N}} - \text{P}pr^{\text{N}-1} + \text{Q}p^2r^{\text{N}-2} - \text{R}p^3r^{\text{N}-3} \ldots \text{L}p^{n-1} \times r^{\text{N}-n+1} \pm \text{M}p^n r^{\text{N}-n} \mp$ &c. and the denominator $(r-p)^{t} \times (r-3p)^{t'} \times (r-5p)^{t''} \times$ &c. $= r^{\text{N}} - \text{P}pr^{\text{N}-1} + \text{Q}p^2r^{\text{N}-2} - \text{R}p^3r^{n-3} + \ldots \text{L}p^{n-1}r^{\text{N}-n+1}(+\text{M} \pm 1 \cdot 2 \cdot 3 \ldots n-1)p^n r^{\text{N}-n} \mp$ &c. whence the numerator and denominator have the n first terms the same, and the next succeeding terms differ by $1 \cdot 2 \cdot 3 \ldots (n-1)p^n r^{\text{N}-n}$; the numerator divided by the denominator $= \pm \frac{1 \cdot 2 \cdot 3 \ldots n-1}{r^n} p^n$ nearly, if r be a great number in proportion to p, &c. it would be $+$ when n is an odd number, and $-$ when even.

8. The logarithm of the fraction K by the common series $= \text{K} - 1 - \frac{(\text{K}-1)^2}{2} + \frac{(\text{K}-1)^3}{3} -$ &c. has for its first term $= \pm \frac{1 \cdot 2 \cdot 3 \ldots n-1}{r^n} \times p^n$ nearly; for its 2d term the square of the first divided by 2, &c.

9. The error of this equation not only depends on the logarithm of K, which may be calculated to any degree of exactness, but in the calculus on the errors of the given logarithms.

10. If r be increased or diminished by any given number, the n first terms of the numerator and denominator will still result the same, and the next succeeding terms will differ by $1 \cdot 2 \cdot 3 \cdot 4 \ldots n-1 \times p^n \times r^{\text{N}-n}$.

11. Let $n \cdot \frac{n-1}{2}$ numbers be 2, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4}$ numbers be 4, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} \cdot \frac{n-4}{5} \cdot \frac{n-5}{6}$ numbers be 6, &c.; their sum, the sum of the products of every 2, the contents of every 3, 4, 5, &c. to $n-1$ of them, will be equal to the sum, the sum of the products of every 2, of the contents of every 3, 4, 5, &c. to $n-1$ of the following numbers, viz. n numbers which are 1, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3}$ numbers which are 3, $n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} \cdot \frac{n-4}{5}$ which are 5, &c.; and the sum of the contents of every n of the former will be less than the sum of the contents of every n latter numbers by $1 \cdot 2 \cdot 3 \cdot 4 \ldots n-1$.

12. The method given in art. 4, which I name a method of correspondent values, easily deduces and demonstrates the preceding equations, which cannot, without much difficulty, be done by the preceding method of differences; the method of correspondent values is much preferable to the method of differences, both for the facility of its deduction, and the generality of its resolution: for instance, from this method very easily can be deduced, &c. the subsequent and other similar equations.

Exam. 1. $sn = ns\,(n-1) - n\,.\,\frac{n-1}{2}\,s\,(n-2) + n\,.\,\frac{n-1}{2}\,.\,\frac{n-2}{3}\,s\,(n-3)$ $\ldots \pm ns1 \mp s$ nearly.

Exam. 2. $s\,(n+m) = \frac{m+n\,.\,m+n-1\,.\,m+n-2\ldots m+2}{1\,.\,3\,.\,.\,n-1} \times s\,(n-1) - \frac{n-1}{1} \times A \times \frac{m+1}{m+2} \times s\,(n-2) + \frac{n-2}{2} \times B \times \frac{m+2}{m+3}\,s\,(n-3) - \frac{n-3}{3} \times C \times \frac{m+3}{m+4} \times s\,(n-4) + \frac{n-4}{4} \times D \times \frac{m+4}{m+5} \times s\,(n-5)$ — &c. nearly, where the letters A, B, C, D, &c. denote the preceding co-efficients, and the converging series is the same as in the preceding example.

Exam. 3. Let the converging series be of the formula $ax + bx^3 \times cx^5 + dx^7 +$ &c.; then will $sn = (2n-2)\,s\,(n-1) - (2n-1) \times \frac{2n-4}{2}\,s\,(n-2) + (2n-1) \times \frac{2n-2}{2} \times \frac{2n-6}{3}\,s\,(n-3) - (2n-1)\,.\,\frac{2n-2}{2}\,.\,\frac{2n-3}{2} \times \frac{2n-4}{4}\,s(n-4)$ + &c. nearly, of which the general term is $(2n-1)\,.\,\frac{2n-2}{2}\,.\,\frac{2n-3}{3}\,.\,.\,\frac{2n-l+1}{l-1} \times \frac{2n-2l}{l} \times s\,(n-l)$.

These series may be made to begin from any term, which may be easily found by the method of correspondent values, and the subsequent terms from it by the given law; its preceding terms may be deduced from the same law reversed, that is, by putting the numerators of the fractions multiplied into it for the denominators, and the denominators for the numerators. From these different serieses may be formed, by adding 2 or more terms of the given series together for a term of the required series; which method has been applied to converging series in general in the Meditationes.

13. The method of correspondent values easily affords a resolution of the problems contained in Mr. Brigg's or Sir Isaac Newton's method of differences.

Exam. 1. Let the quantity be of the formula $a + bx + cx^2 + dx^3 +$ &c. . . $x^n = y$, and $n + 1$ correspondent values of x and y be given, viz. p, q, r, s, &c. of x; sp, sq, sr, ss, &c. of y; then will $y = \frac{x-q\,.\,x-r\,.\,x-s\,.\,\&c.}{p-q\,.\,p-r\,.\,p-s\,.\,\&c.} \times sp + \frac{x-p\,.\,x-r\,.\,x-s\,.\,\&c.}{q-p\,.\,q-r\,.\,q-s\,.\,\&c.} \times sq + \frac{x-p\,.\,x-q\,.\,x-s\,.\,\&c.}{r-p\,.\,r-q\,.\,r-s\,.\,\&c.} \times sr +$ &c. The truth of this problem very easily appears by writing p, q, r, s, &c. for x in the given series.

All the preceding examples may be applied to this case, by writing x for m in the given series; hence the resolutions of several cases of equi-distant ordinates by easy and not inelegant serieses, among which are included the 2 cases commonly given on this subject.

14. If a quantity be required, which proceeds according to the dimensions of x; reduce the above given value of y into a quantity proceeding according to the dimensions of x, and there results $y =$

$$\left(\frac{sp}{p-q\,.\,p-r\,.\,p-s\,.\,\&c. = A} + \frac{sq}{q-p\,.\,q-r\,.\,q-s\,.\,\&c. = B}\right) \times x^n - \left(\frac{sp \times (q+r+s+\&c.)}{A}\right.$$

$+\frac{sq\times(p+r+s+\&c.)}{B}+\frac{sr\times(p+q+s+\&c.)}{C}+\&c.)\,x^{n-1}+(\frac{sp\times(qr+qs+rs+\&c.)}{A}+\frac{sq\times(pr+ps+rs+\&c.)}{B}+\frac{sr\times(pq+ps+qs+\&c.)}{C}+\&c.)\times x^{n-2}-(\frac{sp\times(qrs+\&c.)}{A}+\frac{sq\times(prs+\&c.)}{B}+\frac{sr\times(pqs+\&c.)}{C}+\&c.)\,x^{n-3}+\&c.$

The law and continuation of this series is evident to any one versant in these matters from inspection. And these fractions may be reduced to a common denominator by substituting for sp and A the products s$p\times$ P and A $\times$ P, where P $= q-r\,.\,q-s\,.\,r-s\,.$ &c.; for sq and B the products s$q\times$ Q and B $\times$ Q, where Q $= p-r\,.\,p-s\,.\,r-s\,.$ &c.; for sr and C the products s$r\times$ R and C $\times$ R, where R $= p-q\,.\,p-s\,.\,q-s\,.$ &c.; for ss and D the products s$s\times$ S′ and C $\times$ S′, where S′ $= p-q\,.\,p-r\,.\,q-r\,.$ &c. &c.

The fractions, in particular cases, will often be reducible to lower terms.

15. Let $y = ax^b + bx^{b+l} + cx^{b+2l} +$ &c., and the correspondent values of x and y be given as before, then will $y =$

$$\frac{x^h\times x^l-q^l\times x^l-r^l\times x^l-s^l\times\&c.}{p^h\times p^l-q^l\times p^l-r^l\times p^l-s^l\times\&c.}\times sp+\frac{x^h\times x^l-p^l\times x^l-r^l\times x^l-s^l\times\&c.}{q^h\times q^l-p^l\times q^l-r^l\times q^l-s^l\times\&c.}\times sq+\&c.$$

This series may, in the same manner as the preceding, be reduced to terms proceeding according to the dimensions of x; and the serieses given in the examples may (mutatis mutandis) be predicated of it.

16. A more general method of correspondent values is given in the Meditationes, as also the subsequent $y = \frac{x-q.x-r.x-s.\&c.}{p-q.p-r.p-s.\&c.}\times sp + \frac{x-p.x-r.x-s.\&c.}{q-p.q-r.q-s.\&c.}\times sq$ + &c. as in exam. 1, $= sp + (x-p)(\frac{1}{p-q}\times sp + \frac{1}{q-p}\times sq) + (x-p)(x-q)(\frac{1}{p-q}\times\frac{1}{p-r}\times sp + \frac{1}{q-p}\times\frac{1}{q-r}\times sq + \frac{1}{r-p}\times\frac{1}{r-q}\times sr) + (x-p)(x-q)(x-r)(\frac{1}{p-q}\cdot\frac{1}{p-r}\cdot\frac{1}{p-s}\cdot\times sp + \frac{1}{q-p}\cdot\frac{1}{q-r}\cdot\frac{1}{q-s}\times sq + \frac{1}{r-p}\cdot\frac{1}{r-q}\cdot\frac{1}{r-s}\times sr + \frac{1}{s-p}\cdot\frac{1}{s-q}\cdot\frac{1}{s-r}\times ss) - \&c.$

The equality of these 2 different quantities will easily appear by finding the co-efficients of both, which are multiplied into the same given value of y as sp, sq, sr, &c. and the same power of x; for with very little difficulty they will in general be found equal.

It is evident from this resolution that, giving the ordinates and their respective distances from each other, the value of any other ordinate at a given distance from the preceding, found by this method, will result the same, whatever may be the point assumed from which the absciss is made to begin.

§ 3.—1. Let a series be $Ax + Bx^2 + Cx^3 + Dx^4 +$ &c. of such a formula, that if in it for x be substituted $a+b$, there results a series $A\times(a+b) + B\times(a+b)^2 + C\times(a+b)^3 + D\times(a+b)^4 + \&c. = (Aa + Ba^2 + Ca^3 + Da^4 + \&c.)\times(1 + qb + rb^2 + sb^3 + tb^4 + \&c.) + (1 + qa + ra^2 + sa^3 + ta^4 +$

&c.) $\times$ $(\mathrm{A}b + \mathrm{B}b^2 + \mathrm{C}b^3 + \mathrm{D}b^4 +$ &c.); then will the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. $= \mathrm{A}x + \frac{2\mathrm{B}}{1.2}x^2 + \frac{2.3\mathrm{C}}{1.2.3}x^3 + \frac{24\mathrm{ABC} - 8\mathrm{B}^3}{1.2.3.4\mathrm{A}^2}x^4 + \frac{36\mathrm{C}^2\mathrm{A}^2 + 24\mathrm{ACB}^2 - 16\mathrm{B}^4}{1.2.3.4.5\mathrm{A}^3}x^5 +$ &c.; and the series $1 + qx + rx^2 + sx^3 + tx^4 +$ &c$=$
$1 + \frac{\mathrm{B}}{\mathrm{A}}x + \frac{6\mathrm{CA} - 2\mathrm{B}^2}{1.2\mathrm{A}^2}x^2 + \frac{18\mathrm{CAB} - 8\mathrm{B}^3}{1.2.3\mathrm{A}^3}x^3 + \frac{36\mathrm{C}^2\mathrm{A}^2 - 8\mathrm{B}^4}{1.2.3.4\mathrm{A}^4}x^4 +$ &c.

The terms of these 2 serieses can easily be deduced by the subsequent method. Let $\mathrm{K}x^{n-2} + \mathrm{L}x^{n-1} + \mathrm{M}x^n$, be successive terms of the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 +$ &c., and $\mathrm{K}'x^{n-2} + \mathrm{L}'x^{n-1}$ successive terms of the series $1 + qx + rx^2 + sx^3 + tx^4 +$ &c.; then will $\mathrm{M} = \frac{2\mathrm{A}^2 \times \mathrm{B} \times \mathrm{K}' + 6\mathrm{CAK} - 2\mathrm{B}^2\mathrm{K}}{n.(n-1) \times \mathrm{A}^2}$, and $\mathrm{L}' = \frac{n \times \mathrm{A} \times \mathrm{M} - \mathrm{B} \times x\mathrm{L}}{\mathrm{A}^2}$.

Cor. 1. Let $\mathrm{B} = 0$, and the 2 serieses $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. and $1 + qx + rx^2 +$ &c. become respectively,
$\mathrm{A}x + \frac{2.3}{2.3}\mathrm{C}x^3 + \frac{2^2.3^2}{2.3.4.5} \times \frac{\mathrm{C}^2}{\mathrm{A}}x^5 + \frac{2^3.3^3}{2.3.4.5.6.7} \times \frac{\mathrm{C}^3}{\mathrm{A}^2} \times x^7 +$ &c., and $1 + \frac{2.3}{1.2} \times \frac{\mathrm{C}}{\mathrm{A}}x^2 + \frac{2^2.3^2}{1.2.3.4} \times \frac{\mathrm{C}^2}{\mathrm{A}^2}x^4 + \frac{2^3.3^3}{1.2.3.4.5.6} \times \frac{\mathrm{C}^3}{\mathrm{A}^3}x^6 +$ &c.

If in these serieses for A be substituted 1, and for C be substituted $-\frac{1}{2.3}$, there will result the serieses $x - \frac{x^3}{2.3} + \frac{x^5}{2.3.4.5} -$ &c., and $1 - \frac{x^2}{1.2} + \frac{x^4}{1.2.3.4} -$ &c. which give the sine and cosine in terms of the arc x.

Cor. 2. Let $\mathrm{C} = 0$, and the above-mentioned series $\mathrm{A}x + \mathrm{B}x^2 +$ &c. becomes $\mathrm{A}x + \frac{2}{1.2}\mathrm{B}x^2 * - \frac{2^3}{1.2.3.4} \times \frac{\mathrm{B}^3}{\mathrm{A}^2}x^4 - \frac{2^4}{1.2.3.4.5} \times \frac{\mathrm{B}^4}{\mathrm{A}^3}x^5 +$ &c. The law of this series is, first, that every 3d term vanishes; and 2dly, the signs of every 2 successive terms change alternately from + to − and − to +; and lastly, the co-efficient of the term x^n is $\frac{2^{n-1}}{1.2.3..n} \times \frac{\mathrm{B}^{n-1}}{\mathrm{A}^{n-2}}$; and the series $1 + qx + rx^2 +$ &c. becomes $1 + \frac{\mathrm{B}}{\mathrm{A}}x - \frac{2\mathrm{B}^2}{1.2\mathrm{A}^2}x^2 - \frac{2^3\mathrm{B}^3}{1.2.3\mathrm{A}^3}x^3 - \frac{2^3\mathrm{B}^4}{1.2.3.4\mathrm{A}^4}x^4 +$ &c. In this series the signs of 3 successive terms alternately change from + to − and − to +; and the co-efficient of the term x^n is $\frac{2^n \times \mathrm{B}^n}{1.2.3.n\mathrm{A}^n}$ or $\frac{2^{n-1} \times \mathrm{B}^n}{1.2.3..n\mathrm{A}^n}$ according as n is divisible by 3 or not.

2. Let a series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 + \mathrm{S}x^4 + \mathrm{T}x^5 +$ &c. be of such a formula, that if in it for x be substituted $a + b$, there results a series $1 + \mathrm{P} \times (a + b) + \mathrm{Q} \times (a + b)^2 + \mathrm{R} \times (a + b)^3 + \mathrm{S} \times (a + b)^4 +$ &c. $= (1 + \mathrm{P}a + \mathrm{Q}a^2 + \mathrm{R}a^3 + \mathrm{S}a^4 +$ &c.$) \times (1 + \mathrm{P}b + \mathrm{Q}b^2 + \mathrm{R}b^3 + \mathrm{S}b^4 +$ &c.$) + (\mathrm{A}a + \mathrm{B}a^2 + \mathrm{C}a^3 + \mathrm{D}a^4 +$ &c.$) \times (\mathrm{A}b + \mathrm{B}b^2 + \mathrm{C}b^3 + \mathrm{D}b^4 +$ &c.), then will the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 + \mathrm{D}x^4 +$ &c. $= \mathrm{A}x + \mathrm{B}x^2 + \left(\frac{2\mathrm{B}^2}{3\mathrm{A}} - \frac{\mathrm{PB}}{3} + \mathrm{A} \times \frac{\mathrm{A}^2 + \mathrm{P}^2}{6}\right)x^3 +$ &c., and the series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 +$ &c. $= 1 + \mathrm{P}x +$
$\frac{\mathrm{A}^2 + \mathrm{P}^2}{2}x^2 + \frac{2\mathrm{AB} + \mathrm{P} \times (\mathrm{A}^2 + \mathrm{P}^2)}{6}x^3 + \frac{4\mathrm{B}^2 + (\mathrm{A}^2 + \mathrm{P}^2)^2}{24}x^4 +$ &c.

Let $\mathrm{K}x^{n-2} + \mathrm{L}x^{n-} + \mathrm{M}x^n$ be successive terms of the series $\mathrm{A}x + \mathrm{B}x^2 + \mathrm{C}x^3 +$ &c.. and $\mathrm{K}'x^{n-2} + \mathrm{L}'x^{n-1} + \mathrm{M}'x^n$ successive terms of the series $1 + \mathrm{P}x + \mathrm{Q}x^2 + \mathrm{R}x^3 +$ &c.;

then will $A \times L + P \times L' = n \times M'$ and $B \times K + Q \times K' = n.\frac{n-1}{2} \times M'$ express the law of the serieses.

Cor. Let $B = 0$, then the series $Ax + Bx^2 + Cx^3 + Dx^1 = A \times (x + \frac{P^2 \times A^2}{2.3}x^3 + \frac{(P^2 + A^2)^2}{1.2.3.4.5}x^5 + \&c.)$, and the series $1 + Px + Qx^2 + Rx^3 + \&c. = 1 + Px + \frac{P^2 + A^2}{1.2}x^2 + P \times \frac{P^2 + A^2}{1.2.3}x^3 + \frac{(P^2 + A^2)^2}{1.2.3.4}x^4 + P \times \frac{(P^2 + A^2)^2}{1.2.3...5}x^5 + \frac{(P^2 + A^2)^3}{1.2.3..6}x^6 + \&c.$; the co-efficient of the term x^n will be $(P^2 + A^2)^{\frac{n}{2}}$ or $P \times (P^2 + A^2)^{\frac{n-1}{2}}$, according as n is even or odd.

If in the equations before given for x be substituted $a = b$ instead of $a + b$, then in the other quantities for b substitute $- b$.

3. If in case 2 the difference between the two quantities $(1 + Pa + Qa^2 + \&c.) \times (1 + Pb + Qb^2 + \&c.)$ and $(Aa + Ba^2 + Ca^3 + \&c.) \times (Ab + Bb^2 + Cb^3 + \&c.)$ is assumed $= 1 + P \times (a + b) + Q \times (a + b) + \&c.$, then in the serieses before given for A, B, C, &c. write respectively $\sqrt{-1}A$, $\sqrt{-1}B$, $\sqrt{-1}C$, &c., and there will result the corresponding serieses.

The same principles may be applied to many other cases.

4. Equations of these formulæ may be useful, when the sums of the serieses correspondent to a value (a) of x are given, and the sums of the series correspondent to a value $(a + b)$ of x is required, b having a small ratio to a: for instance, let the given series be $x - \frac{x^3}{2.3} + \frac{x^5}{2.3.4.5} - \frac{x^7}{2.3..7} + \&c.$; the equation found in the first case is $a + b - \frac{(a+b)^3}{2.3} + \frac{(a+b)^5}{2.3.4.5} - \&c. = (a - \frac{a^3}{2.3} + \frac{a^5}{2.3.4.5} - \&c.) \times (1 - \frac{b^2}{1.2} + \frac{b^4}{1.2.3.4} - \&c.) + (1 - \frac{a^2}{1.2} + \frac{a^4}{1.2.3.4} - \&c.) \times (b - \frac{b^3}{2.3} + \frac{b^5}{2.3.4.5} - \&c.)$; but $a - \frac{a^3}{2.3} + \frac{a^5}{2.3.4.5} - \&c.$, and $1 - \frac{a^2}{1.2} + \frac{a^4}{2.3.4} - \&c.$ are the sine s and cosine c of an arc a of a circle whose radius is 1; and consequently, if the sine s and consine c of an arc a be given, the sine of an arc $a + b = s \times (1 - \frac{b^2}{2} + \frac{b^4}{2.4} - \&c.) + c(b - \frac{b^3}{2.3} + \frac{b^5}{2.3.4.5} - \&c.)$, which series, if b be very small in proportion to a, converges much faster than the common series for finding the sine from the arc: it has been given from different principles in the Meditationes, and is also easily deducible from the series for finding the sine and cosine from the arc by the propositions usually given in plane trigonometry: the cosine of the same arc $a + b = c \times (1 - \frac{b^2}{1.2} + \frac{b^4}{2.3.4} - \&c.) - s \times (b - \frac{b^3}{1.2.3} + \frac{b^5}{1.2..5} - \&c.)$

5. Let a quantity P be a function of x, or the fluent of a function of $x \times \dot{x}$,

and the value X of it when $x = a$ be known, and the value of it when $x = a + b$ be required. Find a series of which the first term is X, and which proceeds according to the dimensions of b, if b be a very small quantity, and in general at least so small that the series from $x = a$ to $x = a + b$ neither becomes infinite nor 0. In the same manner, if an algebraical or fluxional equation or equations, expressing the relations between x, y, z, v, &c. be given, find the correspondent values of y, z, v, &c. to $x = a$, which let be Y, Z, V, &c.; then find serieses for y, z, v, &c. of which the first terms let be Y, Z, V, &c. respectively, and which proceed according to the dimensions of b, but subject to the same conditions as in the preceding case. From fluxional equations may be deduced series which express the value of y, &c. in terms of x, and always diverge, or always converge, whatever may be its value, as appears from the Meditationes.

XVIII. On the Resolution of Attractive Powers. By Edw. Waring, M. D., F. R. S., &c. p. 185.

1. A force acting at a given point may be resolved by an infinite number of ways into 2, 3, or more (n) forces acting at the same point, either in the same or different planes with the given force and each other; and, vice versâ, any number of such forces acting in the same or different planes may be reduced into one.

Exam. fig. 5, pl. 6. Let a body A be acted on by 3 forces AB, AC, and AD, not being in the same plane; reduce any 2 of them AB and AC to one AE, by completing the parallelogram ABEC; then reduce the 2 forces AE and AD to one AF by completing the parallelogram AEFD; then the 3 forces AB, AC, and AD, are reduced to the one AF.

2. If n forces act on the body A at the same time, and any $(n - 1)$ of them be reduced to 1, the force resulting will be situated in the same plane with the remaining, and force equivalent to the (n) forces.

3. If one force a be resolved into several others x, y, z, v, &c. situated in different planes, and the sines of the angles, which the forces y, z, v, &c. contain with the plane made by the direction of the forces x and a be respectively s, s', s'', &c. then will $sy \pm s'z \pm s''v \pm$ &c. $= 0$.

PROB. 1. fig. 6.—Given the law of attraction of each of the parts of a given line in terms of their distance from a given point P; to find the attraction of the whole line ab on the point P.—Find the attraction of the line ab on the point P in the 2 directions Pf and fb by the following method. Draw Px from the point P to any point x of the line ab; then the force acting on the point P by the particle xy will be the given function (determined from the given law of attraction) of the distance into the particle; draw also Ph perpendicular from the point P to the line ab; and let P$f = a$, $hf = b$, and $fx = y$; then will the distance $PX = \surd(a^2 \pm 2by + y^2)$, and the function of the distance into the particle $xy = \varphi \surd(a^2 \pm 2by + y^2) \times \dot{y} = F(y) \times \dot{y}$; let this be denoted by lx situated in the line Px, which resolve into 2 others $nx = \frac{y\dot{y} \times F:(\dot{y})}{Px = \surd(a^2 \pm 2by + y^2)}$ situated in the line ab, and ln (in a direction parallel to Pf) $= \frac{a\dot{y} \times F:(y)}{\surd(a^2 \pm 2by + y^2)}$; find the fluents of the fluxions $\frac{y\dot{y} \times F:(y)}{Px}$ and $\frac{a\dot{y} \times F:(y)}{Px}$ contained between the values af and fb of the line $fx = y$, which suppose Y and V respectively; through the point p draw Py parallel to $fb = Y$, and in the line Pf assume $Pu = V$; complete the parallelogram $Puzy$; $P\dot{z}$ will be the force of the line ab on the point P.

Cor. If $F:(y)$ varies as any power or root ($2n$) of the distance $Px = \surd a^2 \pm 2by + y^2)$, and $n - \frac{1}{2}$ be an integer affirmative number or 0, the fluents Y and V of both the fluxions can be found in finite algebraical terms of y; if $n - \frac{1}{2}$ be an integer negative number, both the fluents can be found in the above mentioned finite terms, together with the arc of a circle whose radius is $\surd\overline{a^2 - b^2}$ and tangent $y \mp b$, unless $n - \frac{1}{2} = -1$, in which case the fluent Y involves that circular arc, and also the logarithm of $y^2 \pm 2by + a^2$. If $n - \frac{1}{2}$ denotes a fraction whose denominator is 2, both the fluents can be expressed by the finite terms together with the log. of $y \pm b + \surd(y^2 \pm 2by + a^2)$. If the fluents be given, when n is a given quantity, and $n - \frac{1}{2}$ not a whole affirmative number, from them can be deduced the fluents of any fluxions resulting by increasing or diminishing n by a whole number, unless in the above-mentioned case of $n - \frac{1}{2} = -1$. If $b = 0$, and consequently the line Pf is perpendicular to the given line ab, the fluent Y will be expressed by the finite terms, unless $n - \frac{1}{2} = -1$, in which case it will be as $\frac{1}{2}$ log. $(y^2 + a^2)$ when properly corrected. These fluxions $\dot{Y}$ and $\dot{V}$ may be transformed into others, whose variable quantity is $Px = u$ the distance from P, by substituting in the fluxions for y and $\dot{y}$ their respective values $\surd(u^2 - a^2 + b^2) \mp b$ and $\frac{u\dot{u}}{\surd(u^2 - a^2 + b^2)}$, and consequently for $\surd(y^2 \pm 2by + a^2)$ its value u.

PROB. 2, fig. 7. Given the attraction of each of the parts of a given surface in terms of their distance from a given point P, and an equation expressing the relation between an absciss $Ap = x$, and its correspondent ordinates $pm = y$ of the surface; to find the attraction of the surface on the given point P.

First, by the preceding proposition find the attractions Y and V of any ordinate $m\,p\,m'$ in the directions of the ordinate pm and of the line Pp; and from the equation expressing the relation between the absciss and ordinates of the given curve, find the absciss in terms of the ordinates $(pm) = \pi:(y)$, and thence $\dot{x} = \varphi:(y) \times \dot{y}$ and $\surd(a'^2 \pm 2sa'x + x^2) = \varphi':(y)$, where $PA = a'$, and $s =$ cosine of the angle which the absciss Ap makes with the line PA; then find the fluents of the 3 fluxions $\dot{x} \times Y = \dot{y} \times Y \times \varphi:(y)$, $\dot{x} \times \frac{V \times x}{\surd a'^2 \pm 2sa'x + x^2} = \varphi:$

$(y) \times \dot{y} \times \frac{\pi : (y)}{\varphi : (y)} \times$ V and $\dot{x} \times \frac{a'\text{V}}{\surd(a'^2 \pm 2sa'x + x^2)} = \dot{y} \times \frac{a'\text{V}}{\varphi' : (y)}$, contained between the values of y, which correspond to the extreme values of x, which suppose Y′, V′, and Z; and draw through the point P the lines Py and PZ respectively parallel to the ordinates pm and to the absciss Ap, and equal to $r \times$ Y′ and V′; assume Pu in the line (PA) $= t \times$ Z, r and t denoting the sines of the angles, which the ordinates pm and line AP make with the absciss Ap: reduce these 3 forces Py, PZ, and Pu, to one Pf; thence Pf will be the force of the surface on the point P.

Cor. 1. If for y and $\dot{y}$ be substituted their values in terms of x and $\dot{x}$, deduced from the equation expressing the relation between the absciss Ap and ordinate pm of the given curve, thence will be deduced the above-mentioned fluents Y, V, Y′, V′, and Z, in terms of x: and in the same manner, if for x and $\dot{x}$ be substituted in the fluxions or fluents resulting their values $\surd(u^2 - a'^2 + 1^2 a'^2) \mp sa'$, and its fluxion, there will result the above-mentioned fluxions or fluents in terms of u the distance from the point P.

Cor. 2. Let the curve be a circle, of which A is the centre, PA a line perpendicular to the plane of the circle, and the ordinate pm perpendicular to the absciss Ap; the forces on each side of the absciss Ap will be equal, and the force in the direction of the absciss Ap will be equal to that in the contrary direction; the force in the direction (PA) $= 4 \times \int \frac{a\dot{u}}{\surd(u^2 - a^2)} \times \int \frac{u\dot{y}}{\surd u^2 + y^2} \times$ F$: \surd(u^2 + y^2)$ $=$ W, in which F $: \sqrt{u^2 + y^2}$ is the function of the distance, according to which the given force on the particles varies; the fluent $\int \frac{u\dot{y}}{\surd(u^2 + y^2)} \times$ F $: \surd(u^2 + y^2)$ is contained between the values 0 and $\surd(r^2 + a^2 - u^2)$ of the quantity y, and the fluent W is contained between the values a and $\surd(a^2 + r^2)$ of the quantity u, where $a =$ PA and r the radius of the circle; but the same force is $= 2 \times$ 3.14159 &c. $\times \int a\dot{u} \times$ F $: u$, where F $: u$ denotes the given function of the distance u, and the fluent is contained between the values a and $\sqrt{a^2 + r^2}$ of u.

PROB. 3. To find the attraction of a given solid on a given point P. Find the attraction of every parallel section on that point by the preceding problem, and multiply it into the correspondent fluxion of the first abscissa AP; and also find the fluent of the resulting fluxion, which, properly corrected, multiply into the sine of the angle which the first abscissa makes with the parallel sections, and the product will be proportional to the attraction of the solid on the given point P.

2. Fig. 8. Let the solid ABCH be generated by the rotation of a given curve round its axis AB, which passes through the point attracted P, and this solid be supposed to consist of small evanescent solids, whose bases are the surfaces EF, ef, &c. of spheres of which the centre is P, and altitudes Ff, &c. the increments of the base AB contained between the 2 contiguous surfaces EF and ef: from the

points E and e of the curve draw ED and ed perpendicular to the axis AB, and ES perpendicular to the arc Ee of the given curve at the point E, and meeting the axis AB in S: then will the evanescent solid EFfe $= p \times$ PE $\times$ FD $\times$ Ff $= p \times$ FD $\times$ PS $\times$ Dd (because Ff $= \frac{\text{PS} \times \text{D}d}{\text{PE}}$) $= p \times (\sqrt{(z^2 + y^2)} - z) \times (z\dot{z} \mp y\dot{y})$, where z and y denote respectively the absciss PD, and its correspondent ordinate DE of the given curve.

The increment of the attraction of the surface EF on the point P, in the direction PD, will be as the increment of the surface ($p \times$ PE $\times$ Dd) $\times \frac{\text{PD}}{\text{PE}} \times$ force of each particle $= p \times$ PD $\times$ Dd $\times$ given force of the particle; but the fluent of the fluxion PD $\times$ Dd contained between the points E and F is $= \frac{1}{2}\text{PE}^2 - \frac{1}{2}\text{PD}^2 = \frac{1}{2}\text{ED}^2$; whence the attraction of the evanescent solid EFfe is as $\frac{1}{2}p \times \text{ED}^2 \times$ Ff $\times$ F : $\sqrt{x^2 + y^2}$, force of each given particle at the distance (PE $= \sqrt{(x^2+y^2)}$) $= \frac{1}{2}p \times \text{ED}^2 \times \frac{\text{PS}}{\text{PE}} \times$ Dd $\times$ F : $\sqrt{z^2 + y^2} = \frac{1}{2}py^2 \times \frac{z\dot{z} \pm y\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $\sqrt{(z^2 + y^2)}$; the fluent of which, properly corrected, is as the attraction of the solid on the point P; p denoting the circumference of a circle whose radius is 1.

Cor. 1. The fluxion of this solid is $\frac{1}{2}py^2\dot{z} = \dot{\text{Y}}$, which deduced from the preceding principles $= p \times (\sqrt{(z^2 + y^2)} - z) \times (z\dot{z} \mp y\dot{y}) = \dot{\text{V}}$, and consequently their fluents between two values of z, which correspond to two values of $y = 0$, will be equal to each other.

Cor. 2. The increment of the attraction of this solid, as given in this proposition, $\frac{1}{2}p \times y^2 \times \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $\sqrt{(z^2 + y^2} = \dot{\text{U}}$; but in the preceding proposition the force of a circle on the point P $= p \times \int a\dot{u} \times$ F : u, where $u = \sqrt{(z^2 + y^2)}$, and $a = z$, and y or u the only variable quantity contained in the fluxion; consequently the fluxion of the attraction of the solid $p \times \dot{z} \int z \frac{y\dot{y}}{\sqrt{z^2 + y^2}} \times$ F : $(z^2 + y^2)^{\frac{1}{2}} = \dot{\text{W}}$; therefore, if for the fluent of $\frac{zy\dot{y}}{\sqrt{(z^2 + y^2)}} \times$ F : $(z^2 + y^2)^{\frac{1}{2}}$ be substituted its fluent contained between the values a and the value of y, which in the given equation corresponds to z; then the fluents of $\dot{\text{U}}$ and $\dot{\text{W}}$, contained between the 2 values of z which corresponds to 2 values of $y = 0$, will be equal to each other.

The difference of the fluents of $\dot{\text{Y}}$ and $\dot{\text{V}}$, &c. contained between any other 2 values of z, can easily be deduced from the difference of 2 segments of spheres.

1. It may not be improper to remark in this place, that from different methods of finding the sum of quantities, the fluents of fluxions, the integrals of increments, &c. quantities may often be deduced equal, which otherwise cannot with out some difficulty; of which instances are contained in the Meditationes, and I

shall here subjoin one more to those already given in this paper.—viz. *Exam.* Any curvilinear area ABC, &c. may be supposed to consist of evanescent areas EFef, of which the base EF is the arc of a circle, whose radius is PE $= \sqrt{(z^2 + y^2)}$ and sine ED $= y$, and altitude Ff, and consequently the fluxion of the area $=$ Ff $\times$ arc A of a circle whose radius is PE and sine ED $= \frac{\text{PS}}{\text{PE}} \times \dot{z} \times \text{A} = \frac{z\dot{z} \mp y\dot{y}}{\sqrt{(z^2 + y^2)}} \times \text{A} = \dot{\text{v}}$; the fluent of $\dot{\text{v}}$ contained between the 2 values of z which correspond to 2 values of $y = 0$, will be equal to the fluent of $y\dot{z}$ contained between the same 2 values of z.

2. From a similar method may be deduced equalities between other like fluents; for the curve may be supposed to consist of other similar curve surfaces equally as circles, and the solid of similar segments of other solids equally as spheres.

3. From the same principles may innumerable serieses equal to each other be deduced; for by different converging serieses find the sum of the same quantity or quantities, and there will result serieses equal to each other: for instance (fig. 9), if the time of falling down the arcs AC and BC, and their interpolations from the principles delivered in the Meditationes Analyticæ, of which the difference let be D; find the difference between the times of a body's falling through BC when it began to fall from A and from B by a series proceeding according to the dimensions of AB $= 0'$ a small quantity; and find, by a series of the same kind, the time of falling through AB; the sum of these 2 serieses will be equal to D. Similar propositions may be deduced from fluxional equations.

4. In some cases the ratios of the times of bodies falling through some particular distances to each other may be easily known; for instance, let the force vary as the $m - 1$ power of the distance x, and a be the distance from which the body began to fall; then the velocity varies as $\sqrt{(a^m - x^m)}$, and the increment of the time as $\frac{\dot{x}}{\sqrt{(a^m - x^m)}}$; but if the parts of different curves are proportional, then will a, x, and $\dot{x}$ vary in the same ratio as each other, and consequently the time through proportional parts of the distance will vary as $a^{1-\frac{1}{2}m}$; and if the bodies be resisted likewise by a force which varies as the $\frac{2m-2}{m}$ power of the velocities, then the times through proportional parts will vary as before, that is, as $a^{1-\frac{1}{2}m}$, where a denotes the proportional distances from the points where the forces and resistances are equal.

PROB. 4.—1. Fig. 10. Given an equation expressing the relation between the 2 abscissæ $z =$ AP and $x =$ Pp, and their correspondent ordinates $y = pm$ of a solid; to find its solid contents contained between 2 values of its first abscissæ z. Assume z as an invariable quantity, and from the equation resulting find the fluent z of $y\dot{x}$ contained between the extreme values of x or y; then find the fluent of $z\dot{z}$ contained between the given values of z; then the fluent multiplied into the product of the sines of the angles which the first abscissa makes with the plane of the ordinates and 2d absciss, and the 2d absciss makes with its correspondent ordinates, will be the solid content required.

2. Fig. 11. Let the 1st absciss z of a solid be perpendicular to the planes of the ordinates, and the 2d absciss $Pp = x$ perpendicular to the ordinates themselves $pm = y$. First, assume the 1st absciss as invariable, and find the increment of the arc $p'm = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$, then assume the 2d absciss Pp as constant, and let mu be the fluxion of the ordinate y or u, when the fluxion of the first absciss is $\dot{z} = ul$, where ul is perpendicular to the plane of the ordinates $p'pm$, and l a point of the surface of the solid; draw uh perpendicular to the arc $p'm$: then since ul is constituted at right angles to the plane $pp'm$, lh will cut the arc $p'm$ at right angles; but $uh = \frac{um \times pp'}{p'm} = \frac{\dot{u}\dot{x}}{\sqrt{(\dot{x}^2 + \dot{y}^2)}}$; $lh = (hu^2 + lu^2)^{\frac{1}{2}} = (\frac{\dot{u}\dot{x}}{\dot{x}^2 + \dot{y}^2} + \dot{z}^2)^{\frac{1}{2}}$; the fluxion of the surface will be $lh \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$. From the given equation expressing the relation between the 2 abscissæ z and x and ordinates y, find, by assuming z invariable, $p\dot{x} = \dot{y}$, and by assuming x invariable $q\dot{z} = \dot{y}' = \dot{u}$; which being substituted for their values in the quantity $lh \times \sqrt{(\dot{x}^2 + \dot{y}^2)}$, there will result $(q^2 + p^2 + 1)^{\frac{1}{2}} \times \dot{x} \times \dot{z} = \text{A}\dot{x}\dot{z} = \frac{(q^2 + p^2 + 1)^{\frac{1}{2}}}{p} \times \dot{y} \times \dot{z} = \text{B}\dot{y}\dot{z}$; in A and B for y and x respectively substitute their value deduced from the given equation, and let the resulting quantities be $\text{A}'\dot{x}\dot{z}$ and $\text{B}'\dot{y}\dot{z}$, where A′ is a function of x and z, and B′ a function of y and z; find the fluent of $\text{A}'\dot{x}\dot{z}$, from the supposition that x is only variable, contained between the extreme values of x to a given value of z, which let be $\text{L}\dot{z}$; then find the fluent of $\text{L}\dot{z}$ by supposing z only variable contained between given values of z; and it will be the surface of the solid contained between those values.

The same may be deduced by finding the fluent of $\text{B}'\dot{y}\dot{z}$ on the supposition that y is the only variable quantity contained between the extreme values of y, as before of x to a given value of z, which let be $\text{L}'\dot{z}$; then will the fluent of $\text{L}'\dot{z}$, contained between the given values of z, be the surface required. If the solid be a cone generated by the rotation of a rectangular triangle round a side containing the right angle as an axis; hu will be a given quantity, if $\dot{z}$ be given.—If the above-mentioned angles are given, but not right ones, the arc $p'm$ and perpendicular $l'h$ can easily be deduced, and consequently the increment of the surface.

3. To define a curve of double curvature, it is necessary to have 2 equations, expressing the relation between the abcissæ z and x and their ordinates y, given; and if the angles which they respectively make with each other be right ones, the fluxion of the arc, as given in the Proprietates Curvarum, is $(\dot{z}^2 + \dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$.

Find its value from the 2 given equations, in terms of x, y, or z, multiplied into its respective fluxions; then its fluent, properly corrected, will be the length of the arc required. If the angles are not right, they may easily be reduced to them.

4. The attractions of these surfaces, curves, &c. on a given point P, may be deduced from the preceding principles of finding the attractions of each of the parts in the directions of the first abscissa, which passes through the point P, the 2d abscissa, and the ordinates, and then finding the integrals of these increments. From the method which determines the attraction of a body, surface, &c. on a given point, can be determined the attraction of a body, &c. on any number of points, and consequently the attraction of one body, &c. on another, &c. It is sometimes advantageous to transform the first absciss, that it may pass through the point attracted; and the abscissæ and ordinates, that they may be at right angles to each other, &c.

PROB. 5.—1. Fig. 12. Given an equation expressing the relation between the 2 abscissæ AP and Pp of a solid, and their correspondent ordinates pm, or AP$'$, P$'p'$, and $p'm'$; to transform the first abscissa into any other Lh.

Let the abscissa Lh begin from a point L of the first abscissa AP, and meet an ordinate pm in the point h; draw hp, and let the sines of the angles Ppm, Php, and pPh; LPh, PhL, and PLh, be denoted respectively by r, s, and t, and r', s', and t'; through a point h of the line Ph draw $p'h'm'$ parallel to pm, and make L$h = z$, $hh' = x$, and $h'm' = y$: in the given equation for AP, Pp, and pm, substitute respectively their correspondent values $\frac{s'z}{r'} \pm$ AL (a), $\frac{st'z}{rr'} \pm \frac{sx}{r}$ (for P$h = \frac{t'z}{r'}$ and Ph' = P$h \pm hh' = \frac{t'z}{r'} \pm x$), and $y \pm \frac{tt'z}{rr'} \pm \frac{tx}{r'}$; then there results an equation to the same solid, expressing the relation between the 2 abscissæ z = Lh and x, and their correspondent ordinates y.

1. 2. If the absciss Lh does not begin from L, a point in the first given absciss AP, but from M a point given out of it, it may be reduced to the preceding case, by drawing from M a line MN $= c$ to the plane of the 1st and 2d abscissæ parallel to the ordinates pm; and from N to the 1st abscissa a line NO $= b$ parallel to the 2d abscissæ, and substituting in the equation expressing the relation between AP, Pp, and pm for AP, Pp, and pm respectively $z \pm$ AO (a), $x \pm b$ and $y \pm c$; and there results the equation required expressing the relation between the 2 abscissæ z and x, and their correspondent ordinates y, of which the 1st abscissa z passes through the point M.

2. To change the 2d abscissa Pp into any other Lh, the 1st abscissa and ordinates remaining the same. In the preceding figure let L be considered as a moveable point of the first absciss AL, and the sines of the respective angles denoted by the same letters as before, and let L$h = x$, AL $= z$, and $hm = y$; in

the given equation for AP, Pp, and pm, substitute $z \pm \frac{s'x}{r'}$, $\frac{st'x}{rr'}$, and $y \pm \frac{tt'x}{rr'}$; and there will result the equation required, expressing the relation between z and x the abscissæ, and their correspondent ordinates y.

3. Fig. 13. To change the ordinates, the abscissæ remaining the same, draw $p'm$ an ordinate transformed, $p'h$ parallel to the first abscissa AP, and meeting a 2d abscissa, of which pm is an ordinate in h: for the sines of the angles $p'hp$, hpp', and $hp'p$; $p'pm$, pmp', and $pp'm$, write r, s, and t, r', s', and t'; and for AP$'$, P$'p'$, and $p'm$, respectively z, x, and y; then substitute in the given equation for AP, Pp, and pm, their respective values z (AP$'$) $\pm \frac{ss'}{rr'} \times y$, x (P$'p \pm \frac{ts'}{rr'} y$, and $\frac{t'}{r'}y$; and there results an equation to the solid expressing the relation between the 2 abscissæ AP$'$ and P$'p'$ and the transformed ordinates $p'm$.

From these cases, which are easily reducible to one, may be transformed any given abscissæ and their correspondent ordinates into any other containing given angles, &c. with the before-mentioned abscissæ and ordinates. In the properties of curve lines, first published in 1762, is given a method of deducing the equation to any section of the solid, and in particular the case of deducing the equation to the projection of any curve on a given plane. From the principles given in this, and the paper on centripetal forces, which the R. S. did me the honour to print, can be deduced the fluxional equations, whose fluents express the relations between the abscissæ and their correspondent ordinates, of the curves described by bodies of which the particles act on each other with forces varying according to given functions of their distances.

XIX. Experiments on the Congelation of Quicksilver in England. With further Experiments on the Production of Artificial Cold. By Mr. Richard Walker. p. 199.

Exper. 1.—On December 18, 1788, a favourable opportunity offered of beginning some experiments on the congelation of mercury. For this purpose Mr. W. prepared a mixture of diluted vitriolic acid (reduced by water till its specific gravity was to that of water as 1.5596 to 1) and strong fuming nitrous acid, of each equal parts. The glass tube of a mercurial thermometer, with its bulb half filled with mercury, was provided, as a convenient method of ascertaining when the mercury was congealed; for if, after being subjected to the cold of a frigorific mixture, the thermometer glass should be taken out and inverted, and the mercury found to remain completely suspended in that half of the bulb now uppermost, no doubt can remain of the success of the experiment; an hydrometer, with its lower bulb half an inch in diameter, and $\frac{3}{4}$ full of mercury, was also provided, in case any accident should happen to the other.

It may be proper to premise here, that in all experiments of this kind Mr. W. removes each vessel, when the liquor it contains is sufficiently cooled, out of the mixture in which it is immersed for that purpose, immediately previous to adding the snow or salts with intention to generate a still further increase of cold; and also prefers adding the snow or powdered salts to the liquor, instead of pouring the liquor on these: it is necessary also to stir about the snow or salts, while cooling in a frigorific mixture, from time to time, otherwise it will freeze into a hard mass, and frustrate the experiment.

A half-pint glass tumbler, containing $2\frac{1}{2}$ oz. of the above-mentioned diluted mixture of acids, being immersed in mixtures of nitrous acid and snow, till the liquor it contained was cooled to -30°, was removed out of the mixture and placed on a table; snow, likewise previously cooled in a frigorific mixture to -15°, was added by degrees to the liquor in the tumbler, and the mixture kept stirring till a mercurial thermometer sunk to -60°, where it remained stationary; the hydrometer was then immersed in the mixture (the thermometer glass having been broken in the course of the experiment,) and stirred about in it for a short time, and on taking the hydrometer out, and gently shaking it, the mercury had already acquired the consistence of an amalgam, and after immersing it again for a few minutes, and then taking out and inverting it, he was gratified for the first time with the sight of mercury in a state of perfect congelation. Mr. W. applied his hand to the inverted glass bulb; this soon loosened the solid mercury, which, on shaking the hydrometer, was distinctly heard to knock with force against the glass; it was then immersed a 2d time, and when taken out was found adhering to the glass as before. He now inverted the glass again, and kept it in that situation till the whole of the mercury melted, and dropped down globule after globule into the stem of the hydrometer. The interval of time from taking the mercury out of the frigorific mixture in a solid state, the last time, to its perfect liquefaction, was not noticed; but, on recollection immediately afterwards, was supposed to be not less than 3 or 4 minutes. In a succeeding experiment this circumstance was attended to, and the frozen mercury, weighing 7 scruples, was not entirely melted under 7 minutes, the temperature of the air $+30^{\circ}$.

The experiment which follows Mr. W. considers the most extraordinary, because it proves beyond a doubt, that mercury may be frozen not only here in summer, but even in the hottest climate, at any season of the year, by a combination of frigorific mixtures, in the way described in the Philos. Trans. vol. 77, p. 285, in which attempt to freeze mercury, made April 20, 1787, the temperature of the air and materials being $+45^{\circ}$, he certainly reached, without the assistance of snow or ice, the point of mercurial congelation; but had then no satisfactory proof that any part of the mercury was absolutely congealed.

Exper. 2. On December 30, 3 oz. of a mixture composed of strong fuming nitrous acid 2 parts, and strong vitriolic acid and water each 1 part, were cooled in a half-pint tumbler immersed in a frigorific mixture, till the temperature of the diluted mixture of acids was reduced to — 30°. The tumbler was then removed out of the mixture, and vitriolated natron (Glauber's salt) in very fine powder, previously cooled to — 14° by a frigorific mixture, added by degrees to the liquor in the tumbler, stirring it together till the mercury in the thermometer sunk to — 54°. The hydrometer used in the former experiment, with its lower bulb $\frac{3}{4}$ full of mercury, was now immersed and stirred about in the mixture for a few minutes, when on taking it out, and inverting it, he had the satisfaction to find the same proof of the mercury being frozen as in the former instance. Nearly 4 oz. of the powdered salt was added; but some was added after the greatest effect was produced. The temperature of the room in which these experiments were made was + 30° each time, and the mercury taken from a jar containing several pounds.

Exper. 3. By an experiment made purposely on January 10, 1789, Mr. W. found that mercury may be congealed tolerably hard, by adding fresh fallen snow, at the temperature of + 32°, to strong fuming nitrous acid, previously cooled to between — 25° and — 30°, which may be very easily and quickly effected by immersing the vessel containing the acid in a mixture of snow and nitrous acid. He used the fuming nitrous acid on all occasions, because that does not require to be diluted, cold being immediately produced on the smallest addition of snow.

Exper. 4. On January 12, at Dr. Thomson's request, Mr. W. repeated the experiment of freezing mercury, at the Anatomy School in Christ Church, in the presence of the Honourable Mr. Wenman, the Rev. Dr. Hoare, Dr. Sibthorp, junior, Dr. Thompson, the Rev. Mr. Jackson, of Christ Church, and Mr. Wood of this place, a gentleman well known for his ingenuity in mechanics. For this purpose were provided a spirit thermometer graduated very low, and a mercurial thermometer graduated to — 76°, two thermometer-glasses, with bulbs very near, if not quite an inch in diameter each, one filled with mercury nearly to the orifice of the tube, which was left open, the other with its bulb half filled, and an hydrometer with its lower bulb, considerably less than either of the others, likewise half filled with mercury; the temperature of the room at this time + 28°.

A pan, containing 9 oz. of the mixture of acids prepared as in the first experiment, was placed in a larger pan, containing nitrous acid, and this, in a frigorific mixture of nitrous acid and snow, contained in another pan much larger. When the nitrous acid in the 2d pan was cooled by this mixture to — 18°, and the mixed acids in the smallest pan nearly as much, snow at somewhat between + 20° and + 25°, the temperature of the open air at that time, was added to the nitrous acid in the 2d pan, till the spirit thermometer sunk to near — 43°; then the thermometer, with its bulb half filled, was immersed a sufficient time, and when taken out, the mercury in it was found congealed, and adhering to the glass. The pan containing the mixed acids, and which had been removed while the snow was added to make the 2d mixture, was now replaced in it, in order to be cooled; and when the mixture of acids was reduced to the temperature of — 34°, snow previously cooled to — 18° was added, keeping the mixture stirred till the mercurial thermometer sunk to — 60°; its temperature by the spirit thermometer was then found to be — 51°.

The 3 glasses, containing the mercury to be frozen, were now immersed in this mixture, and having been moved about in it for a considerable time, during which the spirit thermometer rose scarcely 1°, were then severally taken out and examined. When the freezing mixture was supposed to have produced its effect, the bulb which was completely filled was taken out, and broken on a flat stone by a moderate stroke or 2 with an iron hammer. This bulb was 11 or 12 lines in diameter. The solid mercury was separated into several sharp and brilliant fragments, some of which bore handling for a short time before they returned to a fluid form. One mass, larger than the rest, consisting of nearly $\frac{1}{3}$ of the whole ball, afforded the beautiful appearance of flat plates, converging towards a centre. Each of these plates was about a line in breadth at the external surface of the ball, becoming narrower as it shot inwards. These facets lay in very different planes, as is common in the fracture of any crystallized ball, whether of a brittle metal or of the earths, as in balls of calcareous stalactite. The solid brittle mercury in the present instance bore a very exact resemblance, both in colour and plated structure, to sulphurated antimony, and especially to the radiated specimens from Auvergne, before they are at all tarnished.

Instead of a solid centre to this ball, it seemed as if there had been a central cavity, of about 2 lines in diameter, a considerable portion of which was evident in the fragment just described, at that part to which the radii converged. It is indeed possible, that this may have been merely the receptacle of some part of the mercury remaining fluid at the centre. The hollow within was shining, but its edges were neither soft nor mouldering; on the contrary, they were sharp and well defined: nor was the brilliancy of the radii attributable to any exudation of mercury as from an amalgam. In the 2 smaller bulbs, which were only half filled, the mercury preserved its usual lustre on the surface in contact with the glass, as well as on that surface which it had acquired in becoming solid. The latter was occupied by a conical depression, the gradations of which were marked by concentric lines. One of these hemispheres was struck with a hammer, as in the former instance, but was rather flattened and crushed than broken. The other, on being divided with a sharp chissel, showed a metallic splendour on its cut surface, but not equalling the polish of a globule of fluid mercury. Thirteen ounces of snow in the whole were found to have been added to the mixed acids; but some was added to lower its temperature after the glasses containing the mercury were taken out, and the spirit thermometer had risen a few degrees. This was a day remarkably favourable for such an experiment. The thermometer exposed to the open air stood, at ¾ past 8 this morning, at + 6°, which is a very extraordinary degree of cold here; but this experiment was not begun till noon.

Exp. 5. On Jan. 14, Mr. W. froze mercury at the Anatomy School again, in the presence of the Rev. the Dean of Christ Church, the Rev. Dr. Hornsby, and Dr. Thomson. Four ounces now of the mixture of acids, prepared as in the first experiment, were cooled in a tumbler to — 20°, which required somewhat more than an equal weight of snow, cooled nearly to the same temperature, to produce the greatest effect. This was somewhat less than in the last experiment, the spirit thermometer sinking no lower than — 46°, owing chiefly to the weather having become much warmer, the temperature of the open air being now + 36°. The mercurial thermometer immersed in this mixture sunk to — 55°, where it became stationary; then 2 thermometer glasses, one half filled with mercury, and the other filled to a considerable height up the tube, after being immersed some time, were examined. On breaking the shell of glass from the former of these, the mercury was found in a perfectly solid state; but its upper surface, which was highly polished, and of the colour of liquid mercury, instead of being only slightly depressed, as had been seen in every other instance which afforded an opportunity for inspection, now formed a perfectly inverted hollow cone. This great depression, as well as the concentric circles mentioned in a former instance, might be owing to a rotatory motion accidentally given to it while congealing. The solid mercury was beaten out; but having been suffered to lie some time on the table for inspection, very quickly melted into liquid globules. The flexibility of solid mercury was clearly to be observed in this beautiful specimen; for the external surface, particularly the upper thin rim of the concave part, was evidently bent by the first gentle stroke of the hammer. The globe of mercury in the other glass, which was very small, exhibited nearly the same phenomena, as in the instances before-mentioned.

It happened in these experiments, contrary to what has generally occurred to others, that the mercury never sunk lower than — 60°, seldom so low, in the thermometer, and but little below the point of mercurial congelation in the tubes of the thermometer glasses filled nearly up to the orifice, with a view to show the contraction of mercury in becoming solid by its great descent in the tube. On reflecting on this circumstance afterwards, it occurred to Mr. W. that the further descent of the mercury in these experiments was prevented not solely by the mercury freezing in the tube, the cause commonly assigned, but rather by the quick formation of a spherical shell of solid mercury within the bulb, by the sudden generation of cold.

Dr. Beddoes expressing a desire to exhibit solid mercury at his lecture before his class, Mr. W. undertook to freeze some at the Laboratory on March 12th, and now resolved to satisfy himself respecting the cause which prevented the lower descent of the mercury in his former experiments. In this, as well as the former, the mercury in a thermometer graduated to — 60°, and likewise in a thermometer-glass, filled nearly to the orifice, which lengthened its scale to near — 250°, sunk only a few degrees below the point of mercurial congelation, and then remained stationary. After waiting some time, he took the thermometer out of the mixture, and observed the bulb apparently full, and the short thread of mercury above unbroken. He now embraced the lower part of the tube with his hand a few seconds, resting it on the upper part of the bulb; and on taking it away, he found that the whole of the mercury had subsided into the bulb, which it did not now quite fill, a small space at the top of the bulb remaining empty. He then took out the thermometer glass, and applied his hand to the tube; but the mercury remained stationary till he sunk his hand so as to communicate heat to that part of the bulb which is immediately connected with the tube, when the thread of mercury dropped entirely into the bulb. It was now immersed again for a short time, then taken out, and the shell of glass beaten off, which exposed a globe of solid mercury, nearly an inch in diameter. This bore several very smart strokes with a hammer before it began to liquefy, but was not perfectly malleable. In the course of these experiments, several fragments of the solid mercury were thrown into mercury in its ordinary liquid state, and were found to sink with considerable celerity.

In continuing his researches respecting the means of producing artificial cold, Mr. W. found that phosphorated natron produces rather more cold by solution in the diluted nitrous acid than the vitriolated natron. At the temperature of + 50°, 4 parts of the diluted nitrous acid, prepared by mixing strong nitrous acid with half its weight of water, required 8 parts of that neutral salt in fine powder to be added, in order to cause the thermometer to sink to — 6°; and again, by the addition of 5 parts of nitrated ammonia in fine powder, the thermometer sunk so low as — 16°, in the whole 66°. A mixture of this kind made the thermometer sink from 80°, the temperature of the materials before mixing, to 0°.

Mr. W. was directed to the trial of this salt, by the like remarkable sensation of coldness without pungency, which, with its other similar properties to ice, first induced him, while pursuing the subject of cold, to try the effect of dissolving the vitriolated natron in the mineral acids. Equal quantities, by weight, of phosphorated natron and vitriolated natron, were evaporated separately over a gentle fire, till each was reduced to a perfectly dry powder. He then weighed them, and found the residuum of the phosphorated natron somewhat lighter than that of the vitriolated natron; whence it is probable the former contains the greater quantity of water of crystallization. He has found, that each of the neutral salts which produce any remarkable degree of cold by solution in the mineral acids, viz. phosphorated natron, vitriolated natron, and vitriolated magnesia, lose this property entirely, when deprived by any means of their water of crystallization.

A short time after he had first succeeded in freezing water in summer, by one mixture composed of 3 different salts in water (having been induced to try the effect of such a method, from the consideration that water, already saturated with one kind of salt, will dissolve a portion of another, and after that a 3d, or even more,) he met with the account of an experiment made by M. Homberg, related in one of the earlier volumes of the Philos. Trans. in which it is said he produced an extraordinary degree of cold, by pouring a pint and a half of distilled vinegar on 2 lb. of a powder composed of equal parts of crude sal ammoniac and corrosive sublimate, and shaking them well together. Mr. W. immediately (July 30, 1786) prepared a mixture of this kind in smaller quantity, but found it produced only 32° of cold, the temperature of the air and materials before mixing being 63°; which is no more than he had found may be effected by a solution in water of crude sal ammoniac alone, previously dried and powdered.

By a trial made with great accuracy, he found, that even the mixture composed of diluted vitriolic acid and vitriolated natron is adequate to any useful purpose that may be required in the hottest country; for, by adding 11 parts of the salt in fine powder to 8 parts of the vitriolic acid diluted with an equal weight of water, the thermometer sunk from 80°, the mean temperature of the hottest climate, and to which these materials were purposely heated before mixing, to rather below 20°. Vitriolated natron, added to the marine acid undiluted, produces very nearly as great a degree of cold as when mixed with the diluted nitrous acid. At the temperature of 50°, 2 parts of the acid require 3 parts of the salt in fine powder, which will sink the thermometer to 0°; and if 3 parts of a mixed powder, containing equal parts of muriated ammonia and nitrated kali, be added afterwards, the cold of the mixture will be increased a few degrees more.

The frigorific mixture above described, composed of phosphorated natron and nitrated ammonia dissolved in the diluted nitrous acid, being the most powerful, it will probably be found most convenient for freezing mercury, when snow is not to be procured. The materials for this purpose may be previously cooled in mixtures made of marine acid with vitriolated natron, muriated ammonia, and nitrated kali, in the proportions mentioned above, this being much cheaper than those made with diluted nitrous acid, and very nearly equal in effect. In his last paper Mr. W. mentioned a freezing mixture, made by dissolving a powder composed of equal parts of muriated ammonia and nitrated kali in water, and therein directed 6 parts of the mixed powder to be added to 8 parts of water; but he has found since, that the best proportions are, 5 parts of the former to 8 of the latter, by which he has sunk the thermometer from 50° to 11°.

Having now prosecuted his subject relative to mixtures for generating artificial cold without the use of ice, from a possible method proposed by Dr. Watson (Essays, vol. 3, p. 139,) for freezing water in summer in this climate, and carried it on to a certain method of freezing, not only water, but even mercury, in the hottest climate, Mr. W. takes his leave of it.

XX. Catalogue of a Second Thousand of New Nebulæ and Clusters of Stars; with a few Introductory Remarks on the Construction of the Heavens. By Wm. Herschel, LL. D, F. R. S. p. 212.

By the continuation of a review of the heavens with a 20 feet reflector, Dr. H. was now furnished with a 2d 1000 of new nebulæ. The form of this work is exactly that of the former part, the classes and numbers being continued, and the same letters used to express, in the shortest way, as many essential features of the objects as could possibly be crowded into so small a compass as that to which I thought it expedient to limit myself. The method I have taken of analyzing the heavens, as it were, is perhaps the only one by which we can arrive at a knowledge of their construction. In the prosecution of so extensive an undertaking, it may well be supposed that many things must have been suggested, by the great variety in the order, the size, and the compression of the stars, as they presented themselves to my view, which it will not be improper to communicate.

To begin our investigation according to some order, let us depart from the objects immediately around us to the most remote that our telescopes, of the greatest power to penetrate into space, can reach. We shall touch but slightly on things that have already been remarked. From the earth, considered as a planet, and the moon as its satellite, we pass through the region of the rest of the planets, and their satellites. The similarity between all these bodies is sufficiently striking to allow us to comprehend them under one general definition, of bodies not luminous in themselves, revolving round the sun. The great diminution of light, when reflected from such bodies, especially when they are also at a great distance from the light which illuminates them, precludes all possibility of following them a great way into space. But if we did not know that light diminishes as the squares of the distances increase, and that in every reflection a very considerable part is entirely lost, the motion of comets, by which the space through which they run is measured out to us, while on their return from the sun we see them gradually disappear as they advance towards their aphelia, would be sufficient to convince us that bodies shining only with borrowed light can never be seen at any very great distance. This consideration brings us back to the sun, as a refulgent fountain of light, while it establishes at the same time beyond a doubt that every star must likewise be a sun, shining by its own native brightness. Here then we come to the more capital parts of the great construction.

These suns, every one of which is probably of as much consequence to a system of planets, satellites, and comets, as our own sun, are now to be considered, in their turn, as the minute parts of a proportionally greater whole. I need not repeat that by my analysis it appears, that the heavens consist of regions where suns are gathered into separate systems, and that the catalogues I have given comprehend a list of such systems; but may we not hope that our knowledge will not stop short at the bare enumeration of phenomena capable of giving us so much instruction? Why should we be less inquisitive than the natural philosopher, who sometimes, even from an inconsiderable number of specimens of a plant, or an animal, is enabled to present us with the history of its rise, progress, and decay? Let us then compare together, and class some of these numerous sidereal groups, that we may trace the operations of natural causes as far as we can perceive their agency. The most simple form, in which we can view a sidereal system, is that of being globular. This also, very favourably to our design, is that which has presented itself most frequently, and of which I have given the greatest collection.

But first of all it will be necessary to explain what is our idea of a cluster of stars, and by what means we have obtained it. For an instance, I shall take the phenomenon which presents itself in many clusters: it is that of a number of lucid spots, of equal lustre, scattered over a circular space, in such a manner as to appear gradually more compressed towards the middle; and which compression, in the clusters to which I allude, is generally carried so far as, by imperceptible degrees, to end in a luminous centre, of a resolvable blaze of light. To solve this appearance, it may be conjectured that stars of any given, very unequal magnitudes, may easily be so arranged, in scattered, much extended, irregular rows, as to produce the above described picture; or, that stars, scattered about almost promiscuously within the frustrum of a given cone, may be assigned of such properly diversified magnitudes as also to form the same picture. But who, that is acquainted with the doctrine of chances, can seriously maintain such improbable conjectures? To consider this only in a very coarse way, let us suppose a cluster to consist of 5000 stars, and that each of them may be put into one of 5000 given places, and have one of 5000 assigned magnitudes. Then, without extending our calculation any further, we have five and twenty millions of chances, out of which only one will answer the above improbable conjecture, while all the rest are against it. When we now remark that this relates only to the given places within the frustrum of a supposed cone, whereas these stars might have been scattered all over the visible space of the heavens; that they might have been scattered, even within the supposed cone, in a million of places different from the assumed ones, the chance of this apparent cluster not being a real one, will be rendered so highly improbable that it ought to be entirely rejected.

Mr. Michell computes, (Phil. Trans. vol. 57, p. 246,) with respect to the 6 brightest stars of the Pleiades only, that the odds are near 500000 to 1, that no 6 stars, out of the number of those which are equal in splendour to the faintest of them, scattered at random in the whole heavens, would be within so small a distance from each other as the Pleiades are. Taking it then for granted that the stars which appear to be gathered together in a group, are in reality thus accumulated, I proceed to prove also that they are nearly of an equal magnitude.

The cluster itself, on account of the small angle it subtends to the eye, we must suppose to be very far removed from us. For, were the stars which compose it at the same distance from one another as Sirius is from the sun; and supposing the cluster to be seen under an angle of 10 minutes, and to contain 50 stars in one of its diameters, we should have the mean distance of such stars 12 seconds; and therefore the distance of the cluster from us about 17,000 times greater than the distance of Sirius. Now, since the apparent magnitude of these stars is equal, and their distance from us is also equal,—because we may safely neglect the diameter of the cluster, which, if the centre be 17,000 times the distance of Sirius from us, will give us 17,025 for the farthest, and 17,000 wanting 25 for the nearest star of the cluster;—it follows that we must either give up the idea of a cluster, and recur to the above refuted supposition, or admit the equality of the stars that compose these clusters. It is to be remarked that we do not mean entirely to exclude all variety of size; for the very great distance, and the consequent smallness of the component clustering stars, will not permit us to be extremely precise in the estimation of their magnitudes; though we have certainly seen enough of them to know that they are contained within pretty narrow limits; and do not perhaps exceed each other in magnitude more than in some such proportion as one full-grown plant of a certain species may exceed another full-grown plant of the same species.

If we have drawn proper conclusions relating to the size of stars, we may with still greater safety speak of their relative situations, and affirm that in the same distances from the centre an equal scattering takes place. If this were not the case, the appearance of a cluster could not be uniformly increasing in brightness towards the middle, but would appear nebulous in those parts which were more crowded with stars; but, as far as we can distinguish, in the clusters of which we speak, every concentric circle maintains an equal degree of compression, as long as the stars are visible; and when they become too crowded to be distinguished, an equal brightness takes place, at equal distances from the centre, which is the most luminous part.

The next step in my argument will be to show that these clusters are of a globular form. This again we rest on the sound doctrine of chances. Here, by way of strength to our argument, we may be allowed to take in all round nebulæ, though the reasons we have for believing that they consist of stars have not as yet been entered into. For, what I have to say concerning their spherical figure will equally hold good whether they be groups of stars or not. In my catalogues we have, I suppose, not less than 1000 of these round objects. Now whatever may be the shape of a group of stars, or of a nebula, which we would introduce instead of the spherical one, such as a cone, an ellipsis, a spheroid, a circle or a cylinder, it will be evident that out of 1000 situations, which the axes of such forms may have, there is but one that can answer the phenomenon for which we want to account; and that is, when those axes are exactly in a line drawn from the object to the place of the observer. Here again we have a million of chances of which all but one are against any other hypothesis than that which we maintain, and which, for this reason, ought to be admitted.

The last thing to be inferred from the above related appearances is, that these clusters of stars are more condensed towards the centre than at the surface. If there should be a group of stars in a spherical form, consisting of such as were equally scattered over all the assigned space, it would not appear to be very gradually more compressed and brighter in the middle; much less would it seem to have a bright nucleus in the centre. A spherical cluster of an equal compression within, for that such there are will be seen hereafter,—may be distinguished by the degrees of brightness which take place in going from the centre to the circumference. Thus, when a is the brightness in the centre, it will be $\sqrt{(a^2 - x^2)}$ at any other distance x from the centre. Or, putting $a = 1$, and $x =$ any decimal fraction; then, in a table of natural sines, where x is the sine, the brightness at x will be expressed by the cosine. Now as a gradual increase of brightness does not agree with the degrees calculated from a supposition of an equal scattering, and as the cluster has been proved to be spherical, it must needs be admitted that there is indeed a greater accumulation towards the centre. And thus, from the above-mentioned appearances, we come to know that there are globular clusters of stars nearly equal in size, which are scattered evenly at equal distances from the middle, but with an increasing accumulation towards the centre.

We may now venture to raise a superstructure on the arguments that have been drawn from the appearance of clusters of stars and nebulæ of the form we have been examining, which is that of which I have made mention in my Theoretical View—Formation of Nebulæ—Form I, Phil. Trans. vol. 75, p. 214. It is to be remarked that when I wrote the paragraph referred to, I delineated nature as well as I do now; but, as I there gave only a general sketch, without referring to particular cases, what I then delivered may have been considered as little better than hypothetical reasoning, whereas in the present instance this objection is entirely removed, since actual and particular facts are brought to vouch for the truth of every inference.

Having then established that the clusters of stars of the 1st form, and round nebulæ, are of a spherical figure, I think myself plainly authorized to conclude that they are thus formed by the action of central powers. To manifest the validity of this inference, the figure of the earth may be given as an instance; whose rotundity, setting aside small deviations, the causes of which are well known, is without hesitation allowed to be a phenomenon decisively establishing a centripetal force. Nor do we stand in need of the revolving satellites of Jupiter, Saturn, and the Georgium Sidus, to assure us that the same powers are likewise lodged in the masses of these planets. Their globular figure alone must be admitted as a sufficient argument to render this point incontrovertible. We also apply this inference with equal propriety to the body of the sun, as well as to that of Mercury, Venus, Mars, and the moon; as owing their spherical shape to the same cause. And how can we avoid inferring, that the construction of the clusters of stars, and nebulæ likewise, of which we have been speaking, is as evidently owing to central powers? Besides, the step that I here make in my inference is in fact a very easy one, and such as ought freely to be granted. Have I not already shown that these clusters cannot have come to their present formation by any random scattering of stars? The doctrine of chance, by exposing the very great odds against such hypotheses, may be said to demonstrate that the stars are thus assembled by some power or other. Then what do I attempt more than merely to lead the mind to the conditions under which this power is seen to act?

In a case of such consequence I may be permitted to be a little more diffuse, and draw additional arguments from the internal construction of spherical clusters and nebulæ. If we find that there is not only a general form, which, as has been proved, is a sufficient manifestation of a centripetal force, what shall we say when the accumulated condensation, which every where follows a direction towards a centre, is even visible to the very eye? Were we not already acquainted with attraction, this gradual condensation would point out a central power, by the remarkable disposition of the stars tending towards a centre. In consequence of this visible accumulation, whether it may be owing to attraction only, or whether other powers may assist in the formation, we ought not to hesitate in ascribing the effect to such as are central; no phenomena being more decisive in that particular, than those of which I am treating.

I am fully aware of the consequences I shall draw on myself in but mentioning other powers that might contribute to the formation of clusters. A mere hint of this kind, it will be expected, ought not to be given without sufficient foundation; but let it suffice at present to remark that my arguments cannot be affected by my terms: whether I am right to use the plural number,—central powers,—or whether I ought only to say,—the known central force of gravity,—my conclusions will be equally valid. I will however add, that the idea of other central powers being concerned in the construction of the sidereal heavens, is not one that has only lately occurred to me. Long ago I have entertained a certain theory of diversified central powers of attractions and repulsions; an exposition of which I have even delivered in the years 1780 and 1781, to the Philosophical Society then existing at Bath, in several mathematical papers on that subject. I shall however set aside an explanation of this theory, which would not only exceed the intended limits of this paper, but is moreover not required for what remains at present to be added, and therefore may be given some other time, when I can enter more fully into the subject of the interior construction of sidereal systems. To return, then, to the case immediately under our present consideration, it will be sufficient that I have abundantly proved that the formation of round clusters of stars and nebulæ is either owing to central powers, or at least to one such force as refers to a centre.

I shall now extend the weight of my argument, by taking in likewise every cluster of stars or nebula that shows a gradual condensation, or increasing brightness, towards a centre or certain point; whether the outward shape of such clusters or nebulæ be round, extended, or of any other given form. What has been said with regard to the doctrine of chance, will of course apply to every cluster, and more especially to the extended and irregular shaped ones, on account of their greater size: it is among these that we find the largest assemblages of stars, and most diffusive nebulosities; and therefore the odds against such assemblages happening without some particular power to gather them, increase exceedingly with the number of the stars that are taken together. But if the gradual accumulation either of stars or increasing brightness has before been admitted as a direction to the seat of power, the same effect will equally point out the same cause in the cases now under consideration. There are besides some additional circumstances in the appearance of extended clusters and nebulæ, that very much favour the idea of a power lodged in the brightest part. Though the form of them be not globular, it is plainly to be seen that there is a tendency towards sphericity, by the swell of the dimensions the nearer we draw towards the most luminous place, denoting as it were a course, or tide of stars, setting towards a centre. And—if allegorical expressions may be allowed—it should seem as if the stars thus flocking towards the seat of power were stemmed by the crowd of those already assembled, and that while some of them are successful in forcing their predecessors sideways out of their places, others are themselves obliged to take up with lateral situations, while all of them seem equally to strive for a place in the central swelling, and generating spherical figure. Since then almost all the nebulæ and clusters of stars I have seen, the number of which is not less than three and twenty hundred, are more condensed and brighter in the middle; and since, from every form, it is now equally apparent that the central accumulation or brightness must be the result of central powers, we may venture to affirm that this theory is no longer an unfounded hypothesis, but is fully established on grounds which cannot be overturned.

Let us endeavour to make some use of this important view of the constructing cause, which can thus model sidereal systems. Perhaps, by placing before us the very extensive and varied collection of clusters and nebulæ, furnished by my catalogues, we may be able to trace the progress of its operation, in the great laboratory of the universe. If these clusters and nebulæ were all of the same shape, and had the same gradual condensation, we should make but little progress in this inquiry; but, as we find so great a variety in their appearances, we shall be much sooner at a loss how to account for such various phenomena, than be in want of materials on which to exercise our inquisitive endeavours.

Some of these round clusters consist of stars of a certain magnitude, and given degree of compression, while the whole cluster itself takes up a space of perhaps 10 minutes; others appear to be made up of stars that are much smaller, and much more compressed, when at the same time the cluster itself subtends a much smaller angle, such as 5 minutes. This diminution of the apparent size, and compression of stars, as well as diameter of the cluster to 4, 3, 2 minutes, may very consistently be ascribed to the different distances of these clusters from the place in which we observe them; in all which cases we may admit a general equality of the sizes, and compression of the stars that compose them, to take place. It is also highly probable that a continuation of such decreasing magnitudes, and increasing compression, will justly account for the appearance of round, easily resolvable, nebulæ; where there is almost a certainty of their being clusters of stars. And no astronomer can hesitate to go still farther, and extend his surmises by imperceptible steps to other nebulæ, that still preserve the same characteristics, with the only variations of vanishing brightness, and reduction of size.

Other clusters there are that, when they come to be compared with some of the former, seem to contain stars of an equal magnitude, while their compression appears to be considerably different. Here the supposition of their being at different distances will either not explain the apparently greater compression, or, if admitted to do this, will convey to us a very instructive consequence: which is, that the stars which are thus supposed not to be more compressed than those in the former cluster, but only to appear so on account of their greater distance, must needs be proportionally larger, since they do not appear of less magnitude than the former. As therefore one or other of these hypotheses must be true, it is not at all improbable but that, in some instances, the stars may be more compressed; and in others, of a greater magnitude. This variety of size, in different spherical clusters, I am however inclined to believe may not go further than the difference in size, found among the individuals belonging to the same species of plants, or animals, in their different states of age, or vegetation, after they are come to a certain degree of growth. A further inquiry into the circumstance of the extent, both of condensation and variety of size, that may take place with the stars of different clusters, we shall postpone till other things have been previously discussed.

Let us then continue to turn our view to the power which is moulding the different assortments of stars into spherical clusters. Any force, that acts uninterruptedly, must produce effects proportional to the time of its action. Now, as it has been shown that the spherical figure of a cluster of stars is owing to central powers, it follows that those clusters which, ceteris paribus, are the most complete in this figure, must have been the longest exposed to the action of these causes. This will admit of various points of views. Suppose for instance that 5000 stars had been once in a certain scattered situation, and that other 5000 equal stars had been in the same situation; then that of the two clusters which had been longest exposed to the action of the modelling power, we suppose, would be most condensed, and more advanced to the maturity of its figure. An obvious consequence that may be drawn from this consideration is, that we are enabled to judge of the relative age, maturity, or climax of a sidereal system, from the disposition of its component parts; and, making the degrees of brightness in nebulæ stand for the different accumulation of stars in clusters, the same conclusions will extend equally to them all. But we are not to conclude, from what has been said, that every spherical cluster is of an equal standing in regard to absolute duration, since one that is composed of a thousand stars only, must certainly arrive to the perfection of its form sooner than another which takes in a range of a million. Youth and age are comparative expressions; and an oak of a certain age may be called very young, while a cotemporary shrub is already on the verge of its decay. The method of judging with some assurance of the condition of any sidereal system, may perhaps not improperly be drawn from the standard before laid down page 589; so that, for instance, a cluster or nebula which is very gradually more compressed and bright towards the middle, may be in the perfection of its growth, when another which approaches to the condition pointed out by a more equal compression, such as the nebulæ I have called planetary seem to present us with, may be considered as very aged, and drawing on towards a period of change, or dissolution. This has been before surmised, when, in a former paper, I considered the uncommon degree of compression that must prevail in a nebula to give it a planetary aspect; but the argument, which is now drawn from the powers that have collected the formerly scattered stars to the form we find they have assumed, must greatly corroborate that sentiment.

This method of viewing the heavens seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions, in different flourishing beds; and one advantage we may at least reap from it is, that we can, as it were, extend the range of our experience to an immense duration. For, to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing, whether we live successively to witness the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the plant passes in the course of its existence, be brought at once to our view?

Dr. H. then adds the catalogue of the 1000 new nebulæ and clusters of stars, the numbers, dates of observation, names, situations, and several other characteristic circumstances, are arranged in 8 columns of a table, which is divided into 8 classes or collections:—1. The 1st class is of such as are titled, from their appearance in the heavens, bright nebulæ; the 2d class are the faint nebulæ; the 3d class, the very faint nebulæ; the 4th class, planetary nebulæ; the 5th class, very large nebulæ; the 6th class, very compressed, and clusters of stars; the 7th class, pretty much compressed clusters of large or small stars; and the 8th or last class, coarsely scattered clusters of stars. To the catalogue Dr. H. adds the following postscript, to announce a newly discovered satellite of the planet Saturn.

P. S. The planet Saturn has a 6th satellite revolving round it in about 32^h 48^m. Its orbit lies exactly in the plane of the ring, and within that of the 1st satellite. An account of its discovery with the 40 feet reflector, and a more accurate determination of its revolution and distance from the planet, will be presented to the R. S. at their next meetings.

XXI. An Attempt to explain a Difficulty in the Theory of Vision, depending on the Different Refrangibility of Light. By the Rev. Nevil Maskelyne, D. D., F. R. S., &c. p. 256.

The ideas of sight are so striking and beautiful, that we are apt to consider them as perfectly distinct. The celebrated Euler, taking this for granted, has supposed, in the Memoirs of the Royal Academy of Sciences at Berlin for 1747, that the several humors of the human eye were contrived in such a manner as to prevent the latitude of focus arising from the different refrangibility of light, and considers this as a new reason for admiring the structure of the eye; for that a single transparent medium, of a proper figure, would have been sufficient to represent images of outward objects in an imperfect manner; but, to make the organ of sight absolutely complete, it was necessary it should be composed of several transparent mediums, properly figured, and fitted together agreeable to the rules of the sublimest geometry, in order to obviate the effect of the different refrangibility of light in disturbing the distinctness of the image; and hence he concludes, that it is possible to dispose 4 refracting surfaces in such a manner as to bring all sorts of rays to one focus, at whatever distance the object be placed. He then assumes a certain hypothesis of refraction of the differently refrangible rays, and builds on it an ingenious theory of an achromatic object-glass, composed of 2 meniscus glasses with water between them, with the help of an analytical calculation, simple and elegant, as his usually are.

He has not however demonstrated the necessary existence of his hypothesis, his arguments for which are more metaphysical than geometrical; and as it was founded on no experiment, so those made since have shown its fallacy, and that it does not obtain in nature. Also, which is rather extraordinary, it does not account, according to his own ideas, for the very phenomenon which first suggested it to him, namely, the great distinctness of the human vision, as was observed to Dr. M. many years ago, by the late Mr. John Dollond, F. R. S. to whom we are so much obliged for the invention of the achromatic telescope; for the refractions at the several humors of the eye being all made one way, the colours produced by the first refraction will be increased at the 2 subsequent ones, instead of being corrected, whether we make use of Newton's or Euler's law of refraction of the differently refrangible rays.

Thus Euler produced an hypothetical principle, neither fit for rendering a telescope achromatic, nor to account for the distinctness of the human vision; and the difficulty of reconciling that distinctness with the principle of the different refrangibility of light, discovered by Sir Isaac Newton, remains in its full force. In order to go to the bottom of this difficulty, as the best probable means of obviating it, Dr. M. calculated the refractions of the mean, most, and least refrangible rays, at the several humors of the eye, and thence inferred the diffusion of the rays, proceeding from a point in an object, at their falling on the retina, and the external angle that such coloured image of a point upon the retina corresponds to.

He took the dimensions of the eye from M. Petit, as related by Dr. Jurin; and the specific gravities of the aqueous and vitreous humors having been found to be nearly the same with that of water, and the refraction of the vitreous humor of an ox's eye having been found by Mr. Hauksbee to be the same as that of water, and the ratio of refraction out of air into the crystalline humor of an ox's eye having been found by the same accurate experimenter to be as 1 to .68327, he took the refraction of the mean refrangible rays out of air into the aqueous or vitreous humor, the same as into water, as 1 to .74853, or 1.33595 to 1; and out of air into the crystalline humor as 1 to .68327, or 1.46355 to 1. Hence he found, according to Sir Isaac Newton's 2 theorems, related at part 2, book 1 of Optics, p. 113, that the ratio of refraction of the most, mean, and least refrangible rays at the cornea, should be as 1 to .74512, .74853 and .75197; at the fore surface of the crystalline as 1 to .91173, .91282, and .91392; and at the hinder surface of the crystalline as 1 to 1.09681, 1.09550, and 1.09420. Now taking, with Dr. Jurin, 15 inches for the distance at which the generality of eyes in their mean state see with most distinctness, he found the rays from a point of an object so situated, will be collected into 3 several foci, viz. the most, mean, and least refrangible rays, at the respective distances behind the crystalline, .5930, .6034, and .6141 of an inch, the focus of the most refrangible rays being .0211 inch short of the focus of the least refrangible ones.

Also, assuming the diameter of the pencil of rays at the cornea, proceeding from the object at 15 inches distance, to be $\frac{1}{5}$ of an inch in a strong light, which is a large allowance for it, the semi-angle of the pencil of mean refrangible rays at their concourse on the retina will be 7° 12′, whose tangent to the radius unity, or .1264, multiplied into .0211 inch, the interval of the foci of the extreme refrangible rays, gives .002667 inch for the diffusion of the different coloured rays, or the diameter of the indistinct circle on the retina. Now, having found that the diameter of the image of an object on the retina, is to the object, as .6055 inch, to the distance of the object from the centre of curvature of the cornea; or the size of the image is the same as would be formed by a very thin convex lens, whose focal distance is .6055 inch; and consequently a line in an object which subtends an angle of 1′ at the centre of the cornea, will be represented on the retina by a line of $\frac{1}{5678}$ inch. Hence the diameter of the indistinct circle on the retina before found, .002667, will answer to an external angle of $.002667 \times 5678' = 15' 8''$, or every point in an object should appear to subtend an angle of about 15′, on account of the different refrangibility of the rays of light.

Dr. M. now shows that this angle of ocular aberration is compatible with the distinctness of our vision. This aberration is of the same kind as that which we experience in the common refracting telescope. Now, by computation from the tabular apertures and magnifying powers of such telescopes, it is certain that they admit of an angular indistinctness at the eye of no less than 57′; therefore the ocular aberration is near 4 times less than in a common refracting telescope, and consequently the real indistinctness, being as the square of the angular aberration, will be 14 or 15 times less in the eye than in a common refracting telescope, which may be easily allowed to be imperceptible.

Further, Sir Isaac Newton has observed, with respect to the like difficulty of accounting for the distinctness with which refracting telescopes represent objects, that the erring rays are not scattered uniformly over the circle of dissipation in the focus of the object-glass, but collected infinitely more densely in the centre than in any other part of the circle, and in the way from the centre to the circumference become continually rarer and rarer, so as at the circumference to become infinitely rare; and by reason of their rarity are not strong enough to be visible, unless in the centre and very near it. He further observes, that the most luminous of the prismatic colours are the yellow and orange, which affect the sense more strongly than all the rest together; and next to these in strength are the red and green; and that the blue, indigo, and violet, compared with these, are much darker and fainter, and compared with the other stronger colours, little to be regarded; and that therefore the images of the objects are to be placed not in the focus of the mean refrangible rays, which are in the confine of green and blue, but in the middle of the orange and yellow, there where the colour is most luminous, that which is in the brightest yellow, that yellow which inclines more to orange than to green.

From all these considerations, and by an elaborate calculation, he infers, that though the whole breadth of the image of a lucid point be $\frac{1}{55}$th of the diameter of the aperture of the object-glass, yet the sensible image of the same is scarce broader than a circle whose diameter is $\frac{1}{250}$th part of the diameter of the aperture of the object-glass of a good telescope; and hence he accounts for the apparent diameters of the fixed stars as observed with telescopes by astronomers, though in reality they are but points.

The like reasoning is applicable to the circle of dissipation on the retina of the human eye; and therefore we may lessen the angular aberration, before computed at 15′, in the ratio of 250 to 55, which will reduce it to 3′ 18″. This reduced angle of aberration may perhaps be double the apparent diameter of the brightest fixed stars to an eye disposed for seeing most distinctly by parallel rays;

or, if short-sighted, assisted by a proper concave lens; which may be thought a sufficient approximation in an explication grounded on a dissipation of rays, to which a precise limit cannot be assigned, on account of the continual increase of density from the circumference to the centre. Certainly some such angle of aberration is necessary to account for the stars appearing under any sensible angle to such an eye; and if we were, without reason, to suppose the images on the retina to be perfect, we should be put to a much greater difficulty to account for the fixed stars appearing otherwise than as points, than we have now been to account for the actual distinctness of our sight. The less apparent diameter of the smaller fixed stars agrees also with the theory; for the less luminous the circle of dissipation is, the nearer we must look towards its centre to find rays sufficiently dense to move the sense. From Sir Isaac Newton's geometrical account of the relative density of the rays in the circle of dissipation, given in his system of the world, it may be inferred, that the apparent diameters of the fixed stars, as depending on this cause, are nearly as their whole quantity of light.

In further elucidation of this subject Dr. M. adds his own experiment. When he looked at the brighter fixed stars, at considerable elevations, through a concave glass fitted, as he is short-sighted, to show them with most distinctness, they appeared to him without scintillation, and as a small round circle of fire of a sensible magnitude. When he looked at them without the concave glass, or with one not suited to his eye, they appeared to cast out rays of a determinate figure, not exactly the same in both eyes, somewhat like branches of trees, which doubtless arise from something in the construction of the eye, and to scintillate a little, if the air be not very clear. To see day objects with most distinctness, he requires a less concave lens by 1 degree, than for seeing the stars best by night; the cause of which seems to be, that the bottom of the eye being illuminated by the day objects, and so rendered a light ground, obscures the fainter colours, blue, indigo, and violet in the circle of dissipation, and therefore the best image of the object will be found in the focus of the bright yellow rays, and not in that of the mean refrangible ones, or the dark green, agreeable to Newton's remark, and consequently nearer the retina of a short-sighted person; but the parts of the retina surrounding the circle of dissipation of a star being in the dark, the fainter colours, blue, indigo, and violet, will have some share in forming the image, and consequently the focus will be shorter.

The apparent diameter of the stars here accounted for is different from that explained by Dr. Jurin, in his Essay on distinct and indistinct vision, arising from the natural constitution of the generality of eyes to see objects most distinct at moderate distances, and few being capable of altering their conformation enough to see distant objects, and among them the celestial ones, with equal distinctness. But the cause of error, which is here pointed out, will affect all eyes, even those which are adapted to distant objects. If this attempt to show the compatibility of the actual distinctness of our sight with the different refrangibility of light be admitted as just and convincing, we shall have fresh reason to admire the wisdom of the Creator, in so adapting the aperture of the pupil and the different refrangibility of light to each other, as to render the picture of objects on the retina relatively, though not absolutely, perfect, and fitted for every useful purpose; "where," to borrow the words of our religious and oratorical philosopher Derham, "all the glories of the heavens and earth are brought and exquisitely pictured."

Nor does it appear, that any material advantage would have been obtained, if the image of objects on the retina had been made absolutely perfect, unless the acuteness of the optic nerve should have been increased at the same time; as the minimum visible depends no less on that circumstance than the other. But that the sensibility of the optic nerve could not have been much increased beyond what it is, without great inconvenience to us, may be easily conceived, if we only consider the forcible impression made on our eyes by a bright sky, or even the day objects illuminated by a strong sun. Hence we may conclude, that such an alteration would have rendered our sight painful instead of pleasant, and noxious instead of useful. We might indeed have been enabled to see more in the starry heavens with the naked eye, but it must have been at the expense of our daily labours and occupations, the immediate and necessary employment of man.

To obviate an objection to the diffusion of the rays on the retina by the different refrangibility of light, it may be said, that the ocular aberration, being a separate cause from any effect of the telescope, should subsist equally when we observe a star through a telescope as when we look at it with the naked eye; and that therefore the fixed stars could not appear so small as they have been found to do through the best telescopes, and particularly by Dr. Herschel with his excellent ones. To this Dr. M. answers, that the ocular aberration, which is proportional to the diameter of the pupil when we use the naked eye, is proportional to the diameter of the pencil of rays at the eye when we look through a telescope, which being many times less than that of the pupil itself, the ocular aberration will be diminished in proportion, and become insensible.

XXII. Experiments and Observations on Electricity. By Mr. William Nicholson. p. 265.

Mr. N. divides this paper into 3 sections. 1st. On the excitation of electricity; 2d, On the luminous appearances of electricity, and the action of points; 3d, Of compensated electricity.

1. A glass cylinder was mounted, and a cushion applied with a silk flap, pro-

ceeding from the edge of the cushion over its surface, and thence half round the cylinder. The cylinder was then excited by applying an amalgamed leather in the usual manner. The electricity was received by a conductor, and passed off in sparks to Lane's electrometer. By the frequency of these sparks, or by the number of turns required to cause spontaneous explosion of a jar, the strength of the excitation was ascertained.—2. The cushion was withdrawn about 1 inch from the cylinder, and the excitation performed by the silk only. A stream of fire was seen between the cushion and the silk; and much fewer sparks passed between the balls of the electrometer.—3. A roll of dry silk was interposed, to prevent the stream from passing between the cushion and the silk. Very few sparks then appeared at the electrometer.—4. A metallic rod, not insulated, was then interposed, instead of the roll of silk, so as not to touch any part of the apparatus. A dense stream of electricity appeared between the rod and the silk, and the conductor gave many sparks.—5. The knob of a jar being substituted in the place of the metallic rod, it became charged negatively.—6. The silk alone, with a piece of tin foil applied behind it, afforded much electricity, though less than when the cushion was applied with a light pressure. The hand, being applied to the silk as a cushion, produced a degree of excitation seldom equalled by any other cushion.—7. The edge of the hand answered as well as the palm.—8. When the excitation by a cushion was weak, a line of light appeared at the anterior part of the cushion, and the silk was strongly disposed to receive electricity from any uninsulated conductor. These appearances did not obtain when the excitation was by any means made very strong.—9. A thick silk, or 2 or more folds of silk, excited worse than a single very thin flap. He used the silk called Persian.—10. When the silk was separated from the cylinder, sparks passed between them; the silk was found to be in a weak negative, and the cylinder in a positive state.

The foregoing experiments show, that the office of the silk is not merely to prevent the return of electricity from the cylinder to the cushion, but that it is the chief agent in the excitation; while the cushion serves only to supply the electricity, and perhaps increase the pressure at the entering part. There seems also to be little reason to doubt but that the disposition of the electricity to escape from the surface of the cylinder is not prevented by the interposition of the silk, but by a compensation after the manner of a charge; the silk being then as strongly negative as the cylinder is positive: and, lastly, that the line of light between the silk and cushion in weak excitations does not consist of returning electricity, but of electricity which passes to the cylinder, in consequence of its not having been sufficiently supplied, during its contact with the rubbing surface.

11. When the excitation was very strong in a cylinder newly mounted, flashes of light were seen to fly across its inside, from the receiving surface to the sur-

face in contact with the cushion, as indicated by the brush figure. These made the cylinder ring, as if struck with a bundle of small twigs. They seem to have arisen from part of the electricity of the cylinder taking the form of a charge.

12. With a view to determine what happens in the inside of the cylinder, recourse was had to a plate machine. One cushion was applied with its silken flap. The plate was 9 inches in diameter and $\frac{2}{10}$ of an inch thick. During the excitation, the surface opposite the cushion strongly attracted electricity, which it gave out when it arrived opposite the extremity of the flap. So that a continual stream of electricity passed through an insulated metallic bow terminating in balls, which were opposed, the one to the surface opposite the extremity of the silk, and the other opposite the cushion; the former ball showing positive, and the latter negative signs. The knobs of 2 jars being substituted in the place of these balls, the jar, applied to the surface opposed to the cushion, was charged negatively, and the other positively. This disposition of the back surface seemed, by a few trials, to be weaker the stronger the action of the cushion, as judged by the electricity on the cushion side.—Hence it follows, that the internal surface of a cylinder is so far from being disposed to give out electricity during the friction by which the external surface acquires it, that it even greedily attracts it.

13. A plate of glass was applied to the revolving plate, and thrust under the cushion in such a manner as to supply the place of the silk flap. It rendered the electricity stronger, and appears to be an improvement of the plate machine; to be admitted if there were not essential objections against the machine itself.

14. Two cushions were then applied on the opposite surfaces with their silk flaps, so as to clasp the plate between them. The electricity was received from both by applying the finger and thumb to the opposite surfaces of the plate. When the finger was advanced a little towards its correspondent cushion, so that its distance was less than between the thumb and its cushion, the finger received strong electricity, and the thumb none; and, contrariwise, if the thumb were advanced beyond the finger, it received all the electricity, and none passed to the finger. This electricity was not stronger than was produced by the good action of one cushion applied singly.

15. The cushion in experiment 12 gave most electricity when the back surface was supplied, provided that surface was suffered to retain its electricity till the rubbed surface had given out its electricity.

From the last 2 paragraphs it appears, that no advantage is gained by rubbing both surfaces; but that a well managed friction on one surface will accumulate as much electricity as the present methods of excitation seem capable of collecting; but that when the excitation is weak, on account of the electric matter not passing with sufficient facility to the rubbed surface, the friction enables the opposite surface to attract or receive it, and if it be supplied, both surfaces will pass off in the positive state; and either surface will give out more electricity than is really induced on it, because the electricity of the opposite surface forms a charge. For the substance of the remainder of this paper, reference may be had to Mr. Nicholson's ingenious Introduction to Natural Philosophy, in 2 vols. 8vo.

XXIII. Experiments on the Transmission of the Vapour of Acids through a Hot Earthen Tube, and further Observations relating to Phlogiston. By the Rev. Joseph Priestley, LL. D., F. R. S. p. 289.

In Dr. P's former experiments on the phlogistication of spirit of nitre by heat it appeared, that when pure air was expelled from what is called dephlogisticated spirit of nitre, the remainder was left phlogisticated. This he found abundantly confirmed by repeating the experiments in a different manner, and on a larger scale; and he applied the same process to other acids and liquors of a different kind. From these it will appear, that oil of vitriol and spirit of nitre, in their most dephlogisticated state, consist of a proper saturation of the acids with phlogiston, so that what we have called the phlogistication of them, ought rather to have been called their super-phlogistication.

He began with treating a quantity of oil of vitriol as he had done the spirit of nitre, viz. exposing it to heat in a glass tube, hermetically sealed, and nearly exhausted; and the result was similar to that of the experiment with the nitrous acid, with respect to the expulsion of air from it, though, the phlogistication not appearing by any change of colour, I did not in this method ascertain that circumstance. The particulars were as follow. After the acid had been made to boil some time, a dense white vapour appeared in quick motion at a distance above the acid; and though, on withdrawing the fire, that vapour disappeared, it instantly re-appeared on renewing the heat. When the tube was cool, he opened it under water, and a quantity of air rushed out, though the acid had been made to boil violently while it was closing, so that there could not have been much air in the tube. This air, which must therefore have been generated in the tube, was a little worse than common air, being of the standard of 1.12 when the latter was 1.04.

That this air should be worse than common air, Dr. P. could not well explain. But in his former experiments it appeared that vitriolic acid air injures common air; and that in proportion as pure air is expelled from this acid, the remainder becomes phlogisticated, or charged with vitriolic acid air, clearly appeared in the following experiment. Making a quantity of oil of vitriol boil in a glass retort, and making the vapour pass through a red-hot earthen tube, glazed inside and out, and filled with pieces of broken tubes, he collected the liquor that distilled over, and found it to be the same thing with water impregnated with vitriolic acid air. The smell of it was exceedingly pungent; and it was evident, that more of this air had escaped than could be retained by that quantity of water. The oil of vitriol used in this process was 1 oz. 9 dw. 18 gr. and the liquor collected was 6 dw. 12 gr. When he collected the air that was produced in this manner, it appeared to be very pure, about the standard of 0.3 with 2 equal measures of nitrous air. At another time, expending 1 oz. 11 dw. 18 gr. of oil of vitriol, of the specific gravity of 1856, that of water being 1000, he collected 19 dw. 6 gr. of the volatile acid, of the specific gravity of 1340, and 130 oz. measures of dephlogisticated air of the purest kind, viz. of the standard of 0.15. Going through the same process with spirit of nitre, the result was in all respects similar, but much more striking, the production of both dephlogisticated air and phlogisticated acid vapour being prodigiously quicker, and more abundant. Expending 5 oz. 8 dw. 6 gr. of spirit of nitre, he collected 600 oz. measures of very pure dephlogisticated air, being of the standard of 0.2. He also collected 1 oz. 7 dw. 14 gr. of a greenish acid of nitre, which emitted copious red fumes. All the apparatus beyond the hot tube was filled with the densest red vapour, and the water of the trough in which the air was received was so much impregnated with it, that the smell was very strong; and it spontaneously yielded nitrous air several days, just as water does when impregnated with nitrous vapour. Perceiving the emission of air from the water, after it had stood some time, he filled a jar containing 30 oz. measures with it, and without any heat it yielded 2 oz. measures of the strongest nitrous air.

To try whether the acid, thus supersaturated with phlogiston, was convertible into pure air by this process, Dr. P. heated the liquor collected after the distillation of the oil of vitriol, that is, water impregnated with vitriolic acid air, and made the vapour pass through the hot tube, but no air came from it; and when collected a 2d time, it was not at all different from what it had been before. The specific gravity was also the same. It is evident however, though this process does not show it, that the volatile vitriolic acid contains the proper element of dephlogisticated air; since by melting iron into vitriolic acid air, a quantity of fixed air, which is composed of inflammable and dephlogisticated air, is produced. Melting iron in 9 oz. measures of vitriolic acid air, it was reduced to 0.3 oz. measures, and of this 0.17 oz. measures was fixed air. He repeated the experiment with the same result, and putting the residuums together, found the air to be inflammable.

Though, in the process with spirit of salt, the result be different from that of those with oil of vitriol and spirit of nitre, yet there is an analogy among all these 3 acids in this respect, viz. that the marine and both the volatile acids of vitriol and nitre are made by impregnating water with the acid vapour; so that in its usual state it may be said to be phlogisticated as well as these. It was evident that the water in the worm-tub was much more heated by the distillation of the spirit of salt than by that of the oil of vitriol, and especially that of the spirit of nitre; so that much of the heat by which it had been raised in vapour must, in the latter case, have been latent in the air that was formed; whereas, in the other case, it was communicated to the water in the worm-tub.

The vapour of dephlogisticated marine acid, which M. Berthollet discovered, and with which water may be impregnated as with fixed air, being made to pass through the hot earthen tube, became dephlogisticated air, as in the following experiment. Having poured a quantity of spirit of salt on some manganese in a glass retort, he heated it as in the preceding experiments with a proper apparatus both for receiving the distilled liquor, and the air. He found $\frac{7}{10}$ of the air was fixed air, and the remainder very pure dephlogisticated. The liquor received in this distillation resembled strong spirit of salt in which manganese had been put. This process immediately succeeding that in which the glass tube, joining the earthen tube and worm-tub, was left full of black matter by the distillation of the alkaline liquor, mentioned hereafter, the blackness presently vanished, and the tube became transparent as before.

Distilled vinegar, submitted to this process, yielded air $\frac{4}{5}$ of which was fixed air, and the rest inflammable: expending 2 oz. 19 dw. 0 gr. of the acid, he got 1 oz. 19 dw. 0 gr. of a liquor which had a more pungent smell than it had before distillation. It had also some black matter in it, and some of the same remained at the bottom of the retort when the liquor was evaporated to dryness. The air received was 90 oz. measures.

Alkaline air is converted into inflammable air in this process, as well as by the electric spark, but by no means in so great a degree. Dr. P. put 2 oz. 10 dwt. of water pretty strongly impregnated with alkaline air into the retort, and heating it, sent the vapour through the hot tube; when he collected 2 oz. 3 dwt. of liquor, which had a disagreeable empyreumatic smell, as well as that of a volatile alkali, and it was quite opaque with a black matter, which subsided to the bottom of the vessel. Also the tube through which the air and vapour had been conveyed was left quite black, as mentioned above.

Dr. P. now recites a few experiments of a different kind from those above-mentioned, and more immediately relating to the doctrine of phlogiston. It is said, by those who do not admit the doctrine of phlogiston, that the metals are simple substances, which, having a strong affinity to dephlogisticated air, imbibe it when they become calces, without parting with any thing. But that something is really parted with in the calcination, as they will call it, of iron in dephlogisticated air, appears to be very evident, as well as in the process with steam. In $6\frac{1}{2}$ oz. measures of dephlogisticated air he melted turnings of malleable iron till there remained only $1\frac{1}{3}$ oz. measure, and of this $\frac{37}{30}$ oz. measure was fixed air. In 6 oz. measures of dephlogisticated air, of the standard of 0.2, he melted iron till it was reduced to $\frac{2}{3}$ of an ounce measure, of which $\frac{1}{2}$ was fixed air, and the remainder completely phlogisticated. Again, he melted iron in $7\frac{1}{2}$ oz. measures of dephlogisticated air, of the same purity with that in the last experiment, when it was reduced to $1\frac{1}{3}$ oz. measure, and of this $\frac{4}{5}$ was fixed air, and the remainder phlogisticated. In this case Dr. P. carefully weighed the finery cinder that was formed in the process, and found it to be 9 grains, so that the iron that had been melted, being about $\frac{2}{3}$ of this weight, had been about 6 grains.

When the dephlogisticated air is more impure, the quantity of fixed air will always be less in proportion. Thus, having melted iron in 7 oz. measures of dephlogisticated air of the standard of 0.65, it was reduced to 1.6 oz. m.; and of this only $\frac{1}{3}$ of an ounce measure was fixed air. This however is much more than can come from the plumbago in the iron; but as the production of this fixed air is by many ascribed to this plumbago, it may be worth while to show by computation that it is impossible that it should have this origin. Both the quantity of plumbago in iron, and the quantity of fixed air in plumbago, are much too small for the purpose. From half an ounce of the purest plumbago, Dr. P. first got, in a coated glass retort, 13 oz. measures of air, of which only 3 oz. measures were fixed air, the rest being inflammable; then putting it into an earthen tube, he kept it some hours in as great a heat as he could produce, and got 22 oz. m. more; and of this also only 3 were fixed, and the rest inflammable, and the last portion was wholly so.

But instead of supposing the fixed air that he got to be that which was expelled from the plumbago in the iron, he would suppose that even the whole of this plumbago afforded only 1 of the elements of the fixed air, viz. phlogiston, or that which the French chemists call carbone; and that this principle, by its union with the dephlogisticated air in the vessel, forms the fixed air, yet on this most unfavourable and improbable supposition the quantity will be found to be insufficient. If 100 gr. of iron contain, according to M. Bergman, 0.12 gr. of plumbago, 7 gr. would contain only 0.0084 gr. of plumbago; and if we suppose, with Mr. Kirwan, that 100 cubic inches of fixed air contain 8.14 gr. of phlogiston, the fixed air produced in one of the above-mentioned processes (viz. $\frac{4}{5}$ of an ounce measure) would contain .032 gr. of phlogiston, which is above 3 times more than the plumbago in the iron could furnish. It is evident therefore, that the quantity of fixed air that he found must have been formed by phlogiston from the iron uniting with the dephlogisticated air in the vessel.

Another argument against the antiphlogistic doctrine may be drawn from an experiment which Dr. P. made on Prussian blue; if the small quantity of fixed air, that may be expelled from it by heat, be compared with the much greater quantity which is produced when heated in dephlogisticated air. Prussian blue is generally said to be a calx of iron supersaturated with phlogiston, though of late it has been said by some that it has acquired something that is of the nature of an acid. From his experiments on it, with a burning lens in dephlogisticated air, he infers that the former hypothesis is true, except that the substance contains some fixed air, which is no doubt an acid; for much of the dephlogisticated air disappears, just as in the preceding similar process with iron.

He threw the focus of the burning lens on 2 dwt. 5 gr. of Prussian blue in a vessel of dephlogisticated air, of the standard of 0.53, till all the colour was discharged. Being then weighed, it was 1 dwt. 2 gr. In this process $7\frac{1}{4}$ oz. of fixed air had been produced, and what remained of the air was of the standard of 0.94. Heating the brown powder to which the Prussian blue was reduced in this experiment in inflammable air, it imbibed $8\frac{1}{2}$ oz. m. of it, and became of a black colour; but it was neither attracted by the magnet, nor was it soluble in oil of vitriol and water, as he had expected it would have been. Again, he heated Prussian blue in dephlogisticated air, of the standard of 0.2, without producing any sensible increase of its bulk, when he found 3 oz. measures of it to be fixed air, and the standard of the residuum, with 2 measures of nitrous air, was 1.35. The substance had lost 11 gr., the greatest part of which was evidently water. To determine what quantity of fixed air Prussian blue would yield by mere heat, he put half an ounce of it into an earthen tube, and got from it 56 oz. m. of air, of which 16 oz. m. were fixed air, in the proportion of $\frac{1}{3}$ in the first portion, and $\frac{1}{4}$ in the last. The remainder was inflammable. There remained 5 dwt. 20 gr. of a black powder, with a very little of it (probably the surface) brown.

Comparing these experiments, it will appear, that the fixed air procured by means of Prussian blue and dephlogisticated air, must have been formed by phlogiston from the Prussian blue and the dephlogisticated air in the vessel: for if 240 gr. of this substance yield 16 oz. measures of fixed air, 10 gr. of it, which is more than was used in the experiment, would have yielded only 0.6 oz. m. Nor is it possible to account for the disappearing of so much dephlogisticated air, but on the supposition of its being employed in forming this fixed air.

XXIV. On the Production of Nitrous Acid and Nitrous Air. By the Rev. Isaac Milner, B. D., F. R. S., &c. p. 300.

1. It has been known for some time, that a relation subsists between nitrous acid and volatile alkali. The latter has frequently been produced by help of the former; but Mr. M. does not recollect that, in any instance, the volatile alkali has been proved to contribute to the formation of nitrous acid or nitrous air. Some cases however have occurred where this evidently happens; and they appear so new and extraordinary, that he cannot but think they deserve the at-

tention of philosophical chemists. The history of the experiments alluded to is as follows.

2. As soon as he had heard of the production of inflammable air by the transmission of steam through red-hot iron tubes, he had the curiosity to try whether some other substances in the form of air or vapour might not, by a similar process, undergo material alterations. In particular, the nitrous acid seemed well to deserve a trial, both on account of the obscurity and difficulties attending the theory of its production, and also of its important and extensive usefulness in chemistry.

3. I began with boiling a little strong nitrous acid in a small retort, the neck of which was closely luted to one end of a gun-barrel. The other end of it was immersed sometimes in water, and sometimes in quicksilver, and 18 or 20 inches of the middle part was surrounded with burning charcoal in a proper furnace. In this manner the vapour and fumes of the boiling acid were transmitted through the red-hot tube, and the produce received at the end in the usual manner. When the acid was made to boil violently, there passed over a considerable quantity of undecomposed red nitrous vapour, together with a mixture of nitrous and phlogisticated airs. When the process was conducted more moderately, there was less nitrous vapour; and in the mixture of airs which was received in the glass vessels, there was a much greater proportion of phlogisticated air.

4. In order to increase the surface of the red-hot iron, and effect a more complete decomposition of the nitrous vapour, the gun-barrel was crammed full of iron filings. The experiments were repeated with great caution, and almost the whole of the produce was found to be phlogisticated air. It is however proper to mention, that notwithstanding every possible care, still there will generally be in some degree an admixture of nitrous air, and frequently of dephlogisticated nitrous air. But he is satisfied that if the iron tube were sufficiently long, so that a very large portion of it might be heated red-hot, all the air received in this manner from any quantity of nitrous acid slowly boiled, would be found of that species called phlogisticated air.

5. These experiments seem altogether analogous to those of Dr. Priestley, in which nitrous air, by exposure to iron, is converted first into dephlogisticated nitrous air, and afterwards into phlogisticated air. The only difference seems to be, that in these experiments the effect is brought about suddenly; whereas in the method of exposition to iron much time is required. And further, in this method of operating, it is very difficult to conduct the process so as to insure the production of that singular species of air called dephlogisticated nitrous air If the acid boil very quick, the product is nearly all nitrous vapour and nitrous air. If it boil very slow, and a sufficient quantity of the iron tube be well

heated, then the decomposition is almost complete, and little is received but phlogisticated air. In both cases, the progress of the conversion of nitrous acid to the state of phlogisticated air seems to be the same. First, nitrous air is formed, then dephlogisticated nitrous air, and lastly phlogisticated air. This seems to be the natural order of the conversion. From what has been said, the most common process will probably appear to be, that a particle of the acid in the form of vapour first generates nitrous air; that the parts of this are applied to fresh surfaces of hot iron, and suddenly changed into dephlogisticated nitrous air; which, lastly, is applied to still fresh surfaces of the tube or fragments of iron, and so converted into phlogisticated air. When these successive contacts with fresh surfaces of hot iron are not sufficiently numerous or exact, it is not unnatural to conclude, that some portion of air may escape not perfectly decomposed.

6. These considerations induced Mr. M. to alter the process a little. Instead of boiling the acid in the retort, he put some thin pieces of copper into a phial, poured nitrous acid on them, and forced the nitrous air, as it was generated, to pass through the red-hot tube. The event answered his expectation; the decomposition was effected in this way easier than in the former. But before making this experiment, he examined what would be the effect of mere heat on nitrous air, as he had already learned from the experiments of others, that nitrous acid, forced in the form of steam through red-hot tubes of clay or glass, underwent the most important alterations. What might be the effect of long continued exposure to a red heat he could not say; but he was soon convinced, that nitrous air might be forced through a red-hot glass tube, without suffering any material change.

7. Lastly, he determined to try the effect of the gun-barrel on dephlogisticated nitrous air. For this purpose, he diluted a saturated solution of copper in the nitrous acid, and put pieces of iron wire into it, and as the neck of the retort which contained the solution was luted to one end of the gun-barrel, the dephlogisticated nitrous air was exposed in its passage to the action of the red-hot tube, and also to the surfaces of the red-hot iron turnings which it contained. In this case, when the process is conducted with proper care, all the air which is received at the other end of the tube will be found phlogisticated.

8. When the air received at the end of the gun-barrel was in the last mentioned state, viz. perfectly phlogisticated, Mr. M. frequently observed a white fume issuing along with the air, and sometimes ascending through the water or mercury into the glass receivers. On examining this white fume, he soon perceived by the smell that it contained volatile alkali.

9. Most of the experiments hitherto related were made in the summer of 1786; in general they agree with those of Dr. Priestley; the changes and pro-

ductions are much the same, and the only new circumstance is, as was observed at art. 5. The same effects are brought about instantly by the action of red-hot iron, which require much time by the method of simple exposure to cold iron. For which reason, though it gave him much pleasure at the time to see such curious transmutations brought about in a few minutes, yet it scarcely appeared worth while to trouble the R. S. with a detail of the experiments; and he only does it now, because the conjectures he then formed have been sufficiently verified by future experiments. The conjectures were as follow:

10. Almost immediately on seeing the volatile alkali produced by means of nitrous acid and metals, Mr. M. conceived the possibility of inverting the order of the process, and of producing nitrous acid or nitrous air by the decomposition of volatile alkali. He knew of no experiments where this had been done, or any thing like it; yet as volatile alkali was beyond all dispute produced in the method just described, and as the iron turnings and inside of the gun-barrel were left after the operation in a state of calcination, it seemed not unnatural to suppose, that by forcing volatile alkali through the red-hot calces of some of the metals, nitrous acid or nitrous air might be produced; though in fact he neglected for near 2 years actually to make the trial. It was some time in the month of March, 1788, that the calx of manganese on account of its very great infusibility, and its yielding abundance of dephlogisticated air, occurred as a very proper substance for the purpose. He immediately crammed a gun-barrel full of powdered manganese; and to one end of the tube he applied a small retort, containing the caustic volatile alkali. As soon as the manganese was heated red-hot, a lighted candle was placed under the retort, and the vapour of the boiling volatile alkali forced through the gun-barrel. Symptoms of nitrous fumes and of nitrous air soon discovered themselves, and by a little perseverance he was enabled to collect considerable quantities of air, which on trial proved highly nitrous. He afterwards frequently repeated this experiment, and always in some degree succeeded. Much depends on the kind of manganese employed, much on the heat of the furnace, and much on the patience of the operator; as these are varied, there will be great variations of the products.

11. In general Mr. M. made use of clean gun-barrels with which no previous experiments had been made. The manganese was used in rough powder; for when it is too finely powdered, the tube is choked, and the air cannot pass. In some experiments he applied the vapour of the volatile alkali directly to the hot manganese. In others he suffered the manganese to remain a considerable time in a red heat before he made the volatile alkali, contained in the retort at the end of the tube, to boil; and by this means informed himself of the nature of the airs which the manganese yielded per se. In neither case could he ever perceive the least appearance of nitrous acid or nitrous air till the volatile alkali was used.

Manganese, per se, gives airs of different kinds, but chiefly fixed and dephlogisticated airs, as soon as ever it is subjected to a considerable heat; but nothing nitrous comes from it, neither on the first application of heat, or after it has been continued a long time; and he examined this point with great diligence. But soon after the volatile alkali begins to be applied, the jars in which the air is received will frequently turn slightly red, and this redness will increase on admitting atmospherical air.

Here however there exists a cause of deception, against which the operator ought to be on his guard, lest he should conclude that no nitrous air is formed, when in reality there is a considerable quantity. The volatile alkali, notwithstanding every precaution, will frequently pass over in great quantities undecomposed. If the receivers are filled with water, a great part of this will indeed be presently absorbed; but still some portions of it will mix with the nitrous air formed by the process. On admitting the atmospherical air, the nitrous air is decomposed, and the red nitrous fumes instantly combine with the volatile alkali. The receivers are presently filled with white clouds of nitrous ammoniac; and in this manner a wrong conclusion may easily be drawn, from the want of the orange colour of the nitrous fumes. A considerable quantity of nitrous air may have been formed, and yet no orange colour appear, owing to this circumstance; and therefore it is easy to understand how a small quantity of nitrous air may be most effectually disguised by the same cause.

12. These observations are made chiefly for the sake of those who may wish to repeat these experiments. The main point to be established, is the actual formation of nitrous air by this method. And this truth he considers as proved beyond all controversy; for by continuing the process patiently, and applying repeatedly fresh portions of strong volatile alkali to the same manganese, kept constantly hot in the gun-barrel, he often collected large jars of air, which was proved to be highly nitrous by mixture with atmospherical or with dephlogisticated air.

13. It is not easy to say whether, in this process, dephlogisticated nitrous air, or even nitrous acid itself, be not sometimes immediately formed by the action of the volatile alkali on the manganese. Traces of the former, in some instances, seem to discover themselves. As to the latter, it is very certain, that fumes of the nitrous acid often circulate in the jars that receive the air. But possibly these fumes may arise from the decomposition of nitrous air, by means of the superfluous dephlogisticated air of the manganese. 14. The steam of boiling water was applied to red-hot manganese in a similar way; not the least nitrous appearance; but the fixed and dephlogisticated airs were generated much more plentifully than when the manganese was urged by mere heat. When these airs had been collected in large quantities, the volatile alkali was applied as before to the residuum of the manganese, and nitrous air soon appeared.

15. As manganese is known to produce a very extraordinary change on spirit of salt in a moderate heat, it seemed not improbable, that a still greater change might take place by working in this method. Accordingly Mr. M. forced the vapour of boiling spirit of salt to pass through red-hot manganese. This experiment did not answer expectation; the product was a mixture of fixed and inflammable air. But it deserves to be noticed, that even in this case, after the effect of the spirit of salt had been tried for a long time, a production of nitrous air on the application of volatile alkali to the same manganese soon took place.

16. As there are many other substances besides the calx of manganese, which are known, per se, to afford dephlogisticated air, or a mixture of this with fixed air, it was natural to conclude from analogy, that such substances on the application of volatile alkali would not fail to afford nitrous air. It is best however in these matters to trust as little as possible to conjectures, and to bring every opinion to the test of experiment. Manganese is so singular a substance, that it is perhaps hardly safe, from what happens in making trials with it, to infer in any instance of another calx of a metal a similarity of effect. Red lead however, is known to agree in such a variety of chemical effects with manganese, that it was difficult to believe that the volatile alkali properly applied to it would not yield nitrous acid or nitrous air; yet he hitherto in vain attempted to bring this about. The red lead indeed melts during the process, flows into the cooler parts of the tube, and often chokes the passage of the air; but in some trials a great deal of air had been collected before that happened, and without any symptom of a nitrous mixture. It seems difficult to explain the reason of the failure; perhaps with a better adapted apparatus, and more perseverance, either the production in question may be obtained, or the cause of the failure discovered.

17. With calcined green vitriol Mr. M. had much better success. The salt was calcined to whiteness, and put into a gun-barrel; and, after several trials of forcing the volatile alkali through the hot tube, he procured by the operation some ounces of strong nitrous air. 18. As calcined green vitriol, per se, in a strong heat yields dephlogisticated air, Mr. M. had now no doubt but that any substance which had this property might, by similar treatment, be made to afford nitrous air. But in this supposition he was entirely mistaken. The volatile alkali was applied to some calcined alum at the moment when it was yielding in a strong heat plenty of dephlogisticated air. The product was an astonishing quantity of inflammable air, mixed with hepatic air and actual sulphur. The residuum of the alum had a strong hepatic smell, and contained particles of perfectly formed sulphur.

19. It now only remains briefly to propose what occurred as the probable theory and explanation of the facts related. The ingredients which enter into

the composition of nitrous acid seem to be the 2 principles or elements of the atmosphere, viz. phlogisticated and dephlogisticated air. That this is the case, there seems little reason to doubt. Both the composition and decomposition of nitrous acid renders the supposition probable. For, 1. Nitrous air and dephlogisticated air by mixture produce nitrous acid; and nitrous acid, by mere heat, is converted into a mixture of phlogisticated and dephlogisticated airs. 2. Nitrous air, by the methods already related, is changed into phlogisticated air, and these methods seem to consist in abstracting from the nitrous air a quantity of dephlogisticated air. 3. When nitrous acid and nitre are produced in a natural way, the process is not well understood; but the presence of the atmosphere is known to be necessary. 4. Mr. Cavendish's experiment is decisive on this point. The union of the 2 airs in question is effected by means of the electric spark, and nitrous acid is the product.

In the next place we are to consider, that volatile alkali contains phlogisticated air; for, 1. Volatile alkali, by mere heat, or by the electric spark, is changed into a mixture of phlogisticated and inflammable air; and, 2. The residuum of volatile alkaline air, after the calces of lead have been revived in it, is phlogisticated air. Therefore, when volatile alkali, in the form of fume or air, is applied to red-hot manganese, or calcined green vitriol (substances which are then yielding dephlogisticated air,) with these facts in view, it seems not difficult to conceive, that one of the ingredients of the alkali, viz. phlogisticated air, should combine with dephlogisticated air, and form nitrous acid or nitrous air. If nitrous acid be formed, it will indeed in that heat, as has been observed, be instantly decomposed; but if the effect of the union be nitrous air, that will sustain the heat without decomposition. How it happens that nitrous air should be formed, and not nitrous acid, or what the reason is, that nitrous air can sustain a red heat without decomposition, when nitrous acid cannot, Mr. M. is unable to say; and it is better to acknowledge our ignorance than advance groundless conjectures. So much may be pronounced as certain, viz. that nitrous air contains less dephlogisticated air than nitrous acid; because it requires the addition of dephlogisticated air to become nitrous acid.

And, lastly, the experiment with the calcined alum proves, that, in order to produce nitrous air, it is not sufficient merely to apply volatile alkaline air to a substance which is actually yielding dephlogisticated air. Perhaps the presence of another substance is required, which has a strong attraction for phlogiston. Perhaps, in the experiments with the calces of manganese and of iron, the inflammable principle of the volatile alkali combines with the calces of the metals, and the phlogisticated air, the other component part unites with the dephlogisticated air; and if so, it seems not improbable to suppose, that when alum is made use of, the inflammable principle of the volatile alkali having little or no attraction for clay, the basis of the alum, should combine with its acid and form sulphur. If this reasoning be true, then it follows, that the vitriolic acid has a stronger affinity to the inflammable principle than it has to phlogisticated air; and the process with the green vitriol and manganese is to be explained by the operation of a double affinity: the inflammable principle of the volatile alkali joins with the calx of iron, the basis of the vitriol, or with the manganese, and the phlogisticated air with the dephlogisticated air produced by the acid in the red heat. Those who chuse to reject the doctrine of phlogiston must make the necessary alterations in these expressions; but the reasoning will be much the same.

END OF THE SEVENTY-NINTH VOLUME OF THE ORIGINAL.

I. Discovery of a Sixth and Seventh Satellite of the Planet Saturn; with Remarks on the Construction of its Ring, its Atmosphere, its Rotation on an Axis, and its Spheroidical Figure. By Wm. Herschel, LL. D., F. R. S. Anno 1790, *vol. LXXX,* p. 1.

In a short postscript, added to Dr. H's last paper on Nebulæ, he announced the discovery of a 6th satellite of Saturn, and mentioned that he intended soon to communicate the particulars of its orbit and situation to the R. S. He now however presents an account of 2 new satellites instead of one, which he discovered by means of his large 40-foot telescope; and has called them the 6th and 7th, though their situation in the Saturnian system intitles them to the 1st and 2d place. This he did that in future we may not be liable to mistake, in referring to former observations or tables, where the 5 known satellites have been named according to the order they have hitherto been supposed to hold in the range of distance from the planet.

The planet Saturn is, perhaps, one of the most engaging objects that astronomy offers to our view. As such it drew Dr. H's attention so early as the year 1774; when, on the 17th of March, with a 5½-feet reflector, he saw its ring reduced to a very minute line, as represented in fig. 1, pl. 7. On the 3d of April, in the same year, he found the planet as it were stripped of its noble ornament, and dressed in the plain simplicity of Mars. See fig. 2. Dr. H. passes over the following year, in which, with a 7-feet reflector, he saw the ring gradually open, till it came to the appearance expressed in fig. 3. He observes, that the black disc, or belt, on the ring of Saturn, is not in the middle of its breadth; nor is the ring subdivided by many such lines, as has been represented in divers treatises of astronomy; but that there is one single, dark, considerably broad line, belt, or zone, on the ring, which he always permanently found in the place where the figure represents it. From his observations it appears, that the zone on the northern plane of the ring is not, like the belts of Jupiter or those of Saturn, subject to variations of colour and figure; but is most probably owing to some permanent construction of the surface of the ring itself. That however, for instance, this black belt cannot be the shadow of a chain of mountains, may be gathered from its being visible all round on the ring; for at the ends of the ansæ there could be no shades visible, on account of the direction of the sun's illumination, which would be in the line of the chain; and the same argument will hold good against supposed caverns or concavities. It is also pretty evident, that this dark zone is contained between 2 concentric circles, as all the phænomena answer to the projection of such a zone. Thus, in fig. 4, which was taken May 11, 1780, the zone is continued all round the ring, with a gradual decrease of breadth towards the middle answering to the appearance of a narrow circular plane, projected into an ellipsis.

With regard to the nature of the ring, we may certainly affirm, that it is no less solid and substantial than the planet itself. The same reasons which prove to us the solidity of the one will be full as valid when applied to the other. Thus we see in fig. 3 and 4, the shadow of the body of Saturn on the ring, which in fig. 3 is eclipsed towards the north, on the following side, and in fig. 4 about the middle, according to the opposite situation of the sun. In the same manner we see the shadow of the ring cast on the planet, where in fig. 1 and 2 we find it on the equatorial part; and May 28, 1780, it was seen towards the south. If we deduce the quantity of matter, contained in the body, from the power by which the satellites are kept in their orbits, and the time of their revolution, it must be remembered, that the ring is included in the result. It is also in a very particular manner evident, that the ring exerts a considerable force on these revolving bodies, since we find them strongly affected with many irregularities in their motions, which we cannot properly ascribe to any other cause than the quantity of matter contained in the ring; at least we ought to allow it a proper share in the effect, as we do not deny but that the considerable equatorial elevation of Saturn must also join in it.

The light of the ring of Saturn is generally brighter than that of the planet: for instance, April 19, 1777, the southern part of the ring, which passed before the body, was seen very plainly brighter than the disk of Saturn, on which it was projected; and on the 27th of the same month, with a power of 410, the 7-feet reflector had hardly light onough for Saturn, when the ring was sufficiently bright. Again, March 11, 1780, he tried the powers of 222, 332, and 449, successively, and found the light of Saturn less intense than that of the ring; the colour of the body with the high powers turning to a kind of yellow, while that of the ring still remained white. The same result happened on June 25, 1781, with the power 460.

Dr. H. comes now to one of the most remarkable properties in the construction of the ring, which is its extreme thinness. The situation of Saturn, for some months past, has been particularly favourable for an investigation of this circumstance; and his experiments have been so complete, that there can remain no doubt on this head. When nearly in the plane of the ring, he repeatedly saw the 1st, the 2d, and the 3d satellites, nay even the 6th and 7th, pass before and behind the ring in such a manner that they served as excellent micrometers to estimate its thickness by. It may be proper to mention a few instances, especially as they will serve to solve some phenomena that have been remarked by other astronomers, without having been accounted for in any manner that could be admitted, consistently with other known facts. July 18, 1789, at $19^h\ 41^m\ 9^s$, sidereal time, the 1st satellite seemed to hang on the following arm, declining a little towards the north, and gradually advanced on it towards the body of Saturn; but the ring was not so thick as the lucid point. July 23, at $19^h\ 41^m\ 8^s$, the 2d satellite was a very little preceding the ring; but the ring appeared to be less than half the thickness of the satellite. July 27, at $20^h\ 15^m\ 12^s$, the 2d satellite was about the middle, on the following arm of the ring, and towards the south; and the 6th satellite on the farther end, towards the north; but the arm was thinner than either of them. August 29, at $22^h\ 12^m\ 25^s$, the 3d satellite was on the ring, near the end of the preceding arm; and the arm seemed not to be the 4th, at least not the 3d, part of the diameter of the satellite, which, in the situation it was, seemed to be less than 1 single second in diameter. At the same time the 7th satellite, at a little distance following the 3d, was seen in the shape of a bead on a thread, projecting on both sides of the same arm: henee we are sure, that the arm also appeared thinner than the 7th satellite, which is considerably smaller than the 6th, which again is a little less than the 1st satellite. August 31, at $20^h\ 48^m\ 26^s$, the preceding arm was loaded about the middle by the 3d satellite. October 15, at $0^h\ 43^m\ 44^s$, he saw the 6th satellite, without obstruction, about the middle of the preceding arm, though the ring was but barely visible with the 40-feet reflector, even while the planet was in the meridian; however, we were then a little inclined to the plane of the ring, and the 3d satellite, when it came near its conjunction with the 1st, was so situated that it must have partly covered the first a few minutes after the time it was lost behind the house. In all these observations the ring did not in the least interfere with the view of the satellites. October 16, Dr. H. followed the 6th and 7th satellites up to the very disk of the planet; and the ring, which was extremely faint, opposed no manner of obstruction to seeing them gradually approach the disk, where the 7th vanished at $21^h\ 46^m\ 44^s$, and the 6th at $22^h\ 36^m\ 44^s$.

Many other instances might be brought, if necessary. There is however some considerable suspicion, that by a refraction through some very rare atmosphere on the 2 planes of the ring, the satellites might be lifted up and depressed, so as to become visible on both sides of the ring, even though the ring should be equal in thickness to the diameter of the smallest satellite, which may amount to 1000 miles. As for the argument of its incredible thinness, which some astronomers have brought from the short time of its being invisible, when the earth passes through its plane, we cannot set much value on them; for they must have supposed the edge of the ring, as they have also represented it in their figures, to be square; but there is the greatest reason to suppose it either spherical or spheroidical, in which case evidently the ring cannot disappear for any long time. Nay he ventures to say that the ring cannot possibly disappear on account of its thinness; since, either from the edge or the sides, even if it were square on the corners, it must always expose to our sight some part which is illuminated by the rays of the sun: and that this is plainly the case, we may conclude from its being visible in the telescopes during the time when others of less light had lost it, and when evidently we were turned towards the unenlightened side, so that we must either see the rounding part of the enlightened edge, or else the reflection of the light of Saturn on the side of the darkened ring, as we see the reflected light of the earth on the dark part of the new moon. Dr. H. will not however decide which of the 2 may be the case; especially as there are other very strong reasons to induce us to think that the edge of the ring is of such a nature as not to reflect much light.

Dr. H. cannot leave this subject without mentioning both his own former surmises, and those of several other astronomers, of a supposed roughness in the surface of the ring, or inequality in the planes and inclinations of its flat sides. They arose from seeing luminous parts on its extent, which were supposed to be projecting points, like the moon's mountains; or from seeing one arm brighter or longer than another; or even from seeing one arm when the other was invisible. He was, in the beginning of this season, inclined to the same opinion, till one of these supposed luminous points quitted the edge of the ring, and appeared to be a satellite. Now, as he had collected every inequality of this sort, it was easy enough for him afterwards to calculate all such surmises by the known periodical time of the several satellites; and he always found that such appearances were owing to some of these satellites which were either before or behind the ring. Oct. 20th, for instance, at $22^h\ 35^m\ 46^s$, he saw 4 of Saturn's satellites all in one row, and at almost an equal distance from each other, on the following side; and yet the 1st satellite, which was the farthest of them all, was only about half-way towards its greatest elongation from the body of Saturn, as may be seen in fig. 5. How easily, with an inferior telescope, this might have been taken for one of the arms of Saturn, he leaves those to guess who know what a degree of accuracy it must require to distinguish objects that are so minute, and at the same time so faint, on account of their nearness to the disc of the planet. On the whole therefore, he had not any one instance that could induce him to believe the ring was not of an uniform thickness; that is, equally thick at equal distances from the centre, and of an equal diameter throughout the whole of its construction. The idea of protuberant points on the ring of Saturn indeed is of itself sufficient to render the opinion of their existence inadmissible, when we consider the enormous size such points ought to be of, for us to see them at the distance we are from the planet.

From these supposed luminous points however, Dr. H. was, by imperceptible steps, brought to the discovery of 2 satellites of Saturn, which had escaped unnoticed, on account of their little distance from the planet, and faintness; which latter is partly to be ascribed to their smallness, and partly to being so near the light of the ring and disc of Saturn. Strong suspicions of the existence of a 6th satellite he had long entertained; and if he had been more at leisure 2 years before, when the discovery of the 2 Georgian satellites took him off the pursuit, he would certainly have been able to announce its existence as early as the 19th of August, 1787, when at $22^h\ 18^m\ 56^s$, he saw, and marked it down, as being probably a 6th satellite, which was then about $12°$ past its greatest preceding elongation.

In 1788 very little could be done towards a discovery, as his 20 feet speculum was so much tarnished by zenith sweeps, in which it had been more than usually exposed to falling dews, that he could hardly see the Georgian satellites. In hopes of great success with the 40 feet speculum, Dr. H. deferred the attack on Saturn till that should be finished; and having taken an early opportunity of directing it to Saturn, the very first moment he saw the planet, which was the 28th of last August, he was presented with a view of 6 of its satellites, in such a situation, and so bright, as rendered it impossible to mistake them, or not to see them. The retrograde motion of Saturn amounted to nearly $4\frac{1}{2}$ minutes per day, which made it very easy to ascertain whether the stars he took to be satellites really were so; and, in about 2 hours and a half, he had the pleasure of finding that the planet had visibly carried them all away from their places. He continued his observations constantly, whenever the weather would permit; and the great light of the 40 feet speculum was now of so much use, that he also, on the 17th of September, detected the 7th satellite, when it was at its greatest preceding elongation.

As soon as Dr. H. had observations enough to make tables of the motion of these new satellites, he calculated their place backwards, and soon found that many suspicions of these satellites, in the shape of protuberant points on the arms, were confirmed, and served to correct the tables, so as to render them more perfect. Fig. 6 represents the 7 satellites of Saturn, as they were situated October 18, at $21^h\ 22^m\ 45^s$. The small star s served to show the motion of the planet in a striking manner; as, in about $3\frac{3}{4}$ hours after the above-mentioned time, the whole Saturnian system was completely moved away, so as to leave the star s as much following the 2d and 1st satellites, which then were in conjunction, as it now was before the 2d.

By comparing together many observations of the 6th satellite, Dr. H. finds that it completes a sidereal revolution about Saturn in $1^d\ 8^h\ 53^m\ 9^s$. And if we suppose, with M. De La Lande, that the 4th is at the mean distance of 3′ from the centre of Saturn, and performs 1 revolution in $15^d\ 22^h\ 34^m\ 38^s$, we find the distance of the 6th, by Kepler's law, to be 35″.058. Its light is considerably strong, but not equal to that of the 1st satellite; for, on the 20th of October, at $19^h\ 56^m\ 46^s$, when these 2 satellites were placed as in fig. 7, the 1st, notwithstanding it was nearer the planet than the 6th, was still visibly brighter than the latter.

The most distant observations of the 7th satellite, being compared together, show that it makes one sidereal revolution in $22^h\ 40^m\ 46^s$: and, by the same data which served to ascertain the dimension of the orbit of the 6th, we have the distance of the 7th, from the centre of Saturn, no more than 27″.366. It is incomparably smaller than the 6th; and, even in my 40 feet reflector, appears no larger than a very small lucid point. The revolution of this satellite is not nearly so well ascertained as that of the former. The difficulty of having a number of observations is uncommonly great; for, on account of the smallness of its orbit, the satellite lies generally before and behind the planet and its ring, or at least so near them that, except in very fine weather, it cannot easily be seen well enough to take its place with accuracy. On the other hand, the greatest elongations allow so much latitude for mistaking its true situation, that it will require a considerable time to divide the errors that must arise from imperfect estimations. The orbits of these 2 satellites, as appears from many observations of them, are exactly in the plane of the ring, or at least deviate so little from it, that the difference cannot be perceived. It is true, there is a possibility that the line of their nodes may be in, or near, the present greatest elongation, in which case the orbits may have some small inclination; but as he has repeatedly seen them run along the very minute arms of the ring, even then the deviation cannot amount to more than perhaps 1 or 2 degrees; if, on the contrary, the nodes should be situated near the conjunction, this quantity would be so considerable that it could not have escaped his observation.

From the ring and satellites of Saturn we now turn our thoughts to the planet, its belts, and its figure. April 9, 1775, Dr. H. observed a northern belt on Saturn, which was a little inclined to the line of the ring. May 1, 1776, there was another belt, inclined about 15° to the same line, but it was more to the south, and on the following side came up to the place in which the ring crosses the body. July 13, the belt was again depressed towards the north, almost touching the line where the ring passed behind the body. April 8, 1777, there were 2 fine belts, both a little inclined to the ring. In like manner Dr. H. sets down many other similar observations till near the end of the year 1780; and then he adds, it will not be necessary to continue the account of these belts up to the present time; but that he had constantly observed them, and found them generally in equatorial situations, though now and then they were otherwise.

We may draw 2 conclusions from what has been reported. The first, which relates to the changes in the appearance of the belts, is, that Saturn has probably a very considerable atmosphere, in which these changes take place; just as the alterations in the belts of Jupiter have been shown, with great probability, to be in his atmosphere. This has also been confirmed by other observations: thus, in occultations of Saturn's satellites, they seem to hang to the disc a long while before they would vanish. And though we ought to make some allowance for the encroachment of light, by which a satellite is seen to reach up to the disc sooner than it actually does, yet, without a considerable refraction, it could hardly be kept so long in view after the apparent contact. The time of hanging on the disc, in the 7th satellite, has actually amounted to 20 minutes. Now, as its quick motion during that interval carries it through an arch of near 6°, we find that this would denote a refraction of about 2″, provided the encroaching of light had no share in the effect. By an observation of the 6th satellite, the refraction of Saturn's atmosphere amounts to nearly the same quantity; for this satellite remained about 14 or 15 minutes longer in view than it should have done; and as it moves about $2\frac{3}{4}$ degrees in that time, and its orbit is larger than that of the 7th, the difference is inconsiderable. What has been said will suffice to show, that very probably Saturn has an atmosphere of a considerable density.

The next inference we may draw from the appearance of the belts on Saturn is, that this planet turns on an axis, which is perpendicular to the ring. The arrangement of the belts, during the course of 14 years that Dr. H. had observed them, has always followed the direction of the ring, which is what he calls being equatorial. Thus, as the ring opened, the belts began to advance towards the south; and to show an incurvature answering to the projection of an equatorial line, or to a parallel of the same. When the ring closed up, they returned towards the north; and are now, while the ring passes over the centre, exactly ranging with the shadow of it on the body; generally one on each side, with a white belt close to it. The step from equatorial belts to a rotation on an axis is so easy, and, in the case of Jupiter, so well ascertained, that he hesitates not to take the same consequence for granted here. But, if there could remain a doubt, the observations of June 19, 20, and 21, 1780, where the same spot was seen in 3 different situations, would remove it completely.

There is another argument, of equal validity with the former, which Dr. H. now mentions. It is founded on the following observations, and will show that Saturn, like Jupiter, Mars, and the Earth, is flattened at the poles; and therefore ought to be supposed to turn on its axis. July 22, 1776, he thought Saturn was not exactly round. May 31, 1781, it appeared as if the body of Saturn was at least as much flattened as that of Jupiter. August 18, 1787, the body of Saturn is of unequal diameters, the equatorial one being the longest. Sept. 14, 1789, $23^h\ 36^m\ 32^s$, having reserved the examination of the 2 diameters of Saturn to the present as the most favourable time, he measured them with the 20 feet reflector, and a good parallel-wire micrometer.

Equatorial diameter.		Polar diameter.	
1st measure,	21″.94	1st measure,	20″.57
2d	23 .11	2d	20 .10
3d	21 .73	3d	21 .16
4th	22 .85	Mean	20 .61
Mean	22 .81		

By this it appears that Saturn is considerably flattened at the poles. And as the greatest measures were taken in the line of the ring and of the belts, we are assured that the axis of the planet is perpendicular to the plane of the ring; and that the equatorial diameter is to the polar, nearly as 11 to 10.

We may also infer the real diameter of Saturn from these measures, which are perhaps more to be depended on than any that have hitherto been given. But as in his journal Dr. H. had measures that were repeatedly taken 10 years past, not only of the diameter of Saturn, but of the ring, and its opening, by which its inclination may be known; as well as of the distance of the 4th and 5th, and other satellites, which will be of great use in ascertaining the quantity of matter contained in the planet, he reserves a full investigation of these things for another opportunity.

One beautiful observation of the transit of the shadow of the 4th satellite over the disc of Saturn, he adds, to conclude this paper. Last night, November 2, 1789, at $23^h\ 13^m$ sidereal time, being always in quest of any appearance that may afford the means of ascertaining the rotation of Saturn on an axis, he discovered a black spot on the following margin of the disc of that planet. At $23^h\ 21^m$, he perceived a protuberance on the south preceding edge of the disc, which he supposed to be the 4th satellite going to emerge. At 23^m, the black spot had advanced a little towards the preceding side. At 30^m, it was still advancing, and he saw that the spot was a little to the north of the equatorial belt, but so that a small part of it was on the belt. At 35^m, the black spot was a little more than $\frac{1}{8}$ of the diameter of Saturn advanced from the following edge towards the centre. At 39^m, the satellite was detached. At 49^m, the spot was advanced so as to be about $\frac{1}{3}$ of its way towards the centre; and the 4th satellite near half its own apparent diameter clear of the edge.

In this situation of the planet Dr. H. took an eye-draught of it (fig. 10) as it appeared with the black spot on the belt; the lately emerged 4th satellite; 2 parallel dark belts, the intermediate space between them and the equatorial one being a little brighter than the rest of the disc; the 6th, 3d, and 2d satellites on the preceding side; the ring projecting like 2 very slender lines on each side of the disc, and containing the 1st satellite on the following arm, with the 5th at a considerable distance following.

At $0^h\ 5^m$, the black spot was got a little more than half way towards the centre. It was much darker than the belt, and more on it than before. At $1^h\ 2^m$, by advancing gradually towards the south, it was now almost entirely drawn on the equatorial belt. At $1^h\ 13^m$, the black spot approached towards a central situation. At $1^h\ 21^m\ 51^s$, it was perfectly central, and at the same time on the middle of the equatorial belt. He followed the shadow of the satellite with great attention up to the centre, in order to secure a valuable epocha, which may serve to improve our tables of the mean motion of this satellite.

II. Astronomical Observations on the Planets Venus and Mars, made with a View to determine the Heliocentric Longitude and Annual Motion of the Nodes, and the Greatest Inclination of their Orbits. By Thos. Bugge, F. R. S., Regius Prof. of Astronomy at Copenhagen. p. 21.

1. *The heliocentric longitude and annual motion of Venus's nodes.*

The following astronomical observations were made at the Royal Observatory at Copenhagen with a 6-feet transit instrument, and with a mural quadrant of 6-feet radius. It is only necessary to set down the observed geocentric longitudes and latitudes, corrected for aberration and nutation, and compared with the tables of Dr. Halley and of M. de la Lande.

Mean time at Copenhagen.				Observed geocentric longitude of ♀.				Observed geocentric latitude of ♀.			Halley's error,		De la Lande's error,	
											in long.	in lat.	in long.	in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″	″	″
1781, Sept. 13	1	38	6	6	18	39	15	0	28	12 N	+ 11	+ 3		
22	1	43	24	6	29	38	48	0	3	9	+ 17	+ 1	+ 39	+ 3
Oct. 1	1	49	46	7	10	36	42	0	23	48 S	+ 16	− 7	+ 44	− 2
4	1	52	11	7	14	15	17	0	33	3	+ 12	− 4		
1784, Sept. 20	0	37	17	6	9	40	27	1	6	15 N	+ 41	+ 15	+ 17	+ 12
25	0	39	57	6	15	52	59	0	57	50	+ 15	+ 3		
Oct. 2	0	44	49	6	24	35	34	0	44	21	+ 36	+ 6		
14	0	54	7	7	9	30	40	0	16	41	+ 10	+ 1		
21	1	0	42	7	18	13	11	0	1	3 S	+ 17	+ 1	+ 18	+ 3
1786, Aug. 19	2	25	57	6	4	45	54	0	23	7 N	− 5	− 3		
20	2	26	14	6	5	56	11	0	19	40	− 17	− 9		
21	2	26	32	6	7	6	42	0	16	20	− 13	− 5	+ 17	− 12
28	2	28	30	6	15	7	50	0	8	46 S	− 0	− 2	+ 23	+ 2
29	2	28	47	6	16	27	39	0	12	32	− 22	− 4		

The angle at the planet, or the commutation = P, is not directly to be taken out of the table. The difference between the observed geocentric longitude of the planet and the geocentric longitude of the sun, calculated from M. Mayer's solar tables, is the angle at the earth, or the elongation = T. From this elongation, which is to be depended on to a very few seconds, and from the planet's and the earth's distances from the sun, according to the tables, the commutation is calculated, and the geocentric longitude is reduced to the heliocentric longitude. The angles P, T, and S, at the sun, thus found, are likewise used to calculate the heliocentric latitude. According to the different dimensions, given to the orbit of the planet in the different tables, the radius vector at a given time will also be somewhat different. These differences in the tables of Dr. Halley and M. de la Lande are but small: thus, 1784, September 20, at $0^h\ 37'\ 17''$ mean time, the observed and apparent geocentric longitude of Venus $6^s\ 9°\ 39'\ 54''$, the aberration and nutation $+ 33''$, the corrected and true geocentric longitude $6^s\ 9°\ 40'\ 27''$, the sun's geocentric longitude $5^s\ 28°\ 5'\ 51''$; hence the elongation $T = 0^s\ 11°\ 34'\ 36''$. If now the logarithm of the radius vector SP be taken out of Halley's tables $= 4.858251$, then sin. $P = \frac{ST \times \sin. T}{SP}$, and $P = 16°\ 11'\ 52''$; but if the logarithm SP be taken out of M. de la Lande's tables $= 4.858168$, the angle P will be found $= 16°\ 12'\ 4''$, the difference is $12''$. This uncertainty in the commutation, and consequently in the heliocentric longitude, would have been still greater if the calculations had been made only from the tables, or from the planet's geocentric longitude by the tables; thus this angle P is, according to Dr. Halley, $= 16°\ 10'\ 52''$; and from M de la Lande $= 16°\ 10'\ 13''$.

Mean time at Copenhagen.				Heliocentric longitude of ♀ in the ecliptic.				Heliocentric latitude of ♀.			Halley's error,		De la Lande's error,	
											in long.	in lat.	in long.	in lat.
	h.	m.	s.	s.	°	′	″	°	′	″	″	″	″	″
1781, Sept. 13	1	38	6	7	28	40	26	0	56	13 N	+ 21	+ 5		
22	1	43	24	8	12	59	47	0	6	4	− 70	+ 2	+ 63	− 6
Oct. 1	1	49	46	8	27	15	26	0	44	6 S	+ 41	− 13	+ 13	− 6
4	1	52	11	9	1	59	21	1	0	28	− 20	− 8		
1784, Sept. 20	0	37	17	6	25	51	19	2	23	46 N	+ 28	+ 34	+ 106	+ 27
25	0	39	57	7	3	52	40	2	13	7	+ 1	+ 7		
Oct. 2	0	44	49	7	15	6	32	1	40	46	+ 74	+ 13		
14	0	54	7	8	14	13	57	0	36	58	+ 33	+ 1		
21	1	0	42	8	15	21	40	0	2	17 S	+ 55	− 2	+ 43	+ 7
1786, Aug. 19	2	25	57	8	4	12	41	0	37	6 N	+ 25	− 4		
20	2	26	14	8	5	47	32	0	31	22	+ 52	− 15		
21	2	26	32	8	7	23	10	0	25	55	+ 68	− 6	+ 69	− 16
28	2	28	30	8	18	30	24	0	13	17 S	+ 108	− 3	+ 89	+ 3
29	2	28	47	8	20	5	16	0	18	52	+ 35	− 6		

Let the difference between two heliocentric longitudes, one before and the other after the passage through the node, be $= a$, the northern heliocentric latitude $= b$, and the southern $= \beta$; let the arc of the ecliptic from the node to the longitude, answering to the southern latitude, be $= x$; then tang. $x = \frac{\text{sin. } a \text{ . tang.}}{\text{tang. } b + \text{cos. } a \text{ tang. } \beta}$. By this formula the distance from every longitude with a southern latitude to the node may be found; and hence the heliocentric longitude of the node.

Observations compared.				Heliocentric longitude of ☋ ♀.			
				s	°	′	″
1781, Sept. 13	..	Oct. 1		8	14	42	42
13	..	4		8	14	42	23
22	..	1		8	14	42	29
22	..	4		8	14	42	12
		Mean		8	14	42	24
Reduced to 1786				8	14	44	53
1784, Oct. 21	..	Sept. 20		8	14	43	38
21	..	25		8	14	43	40
21	..	Oct. 2		8	14	43	42
21	..	4		8	14	43	30
		Mean		8	14	43	38
Reduced to 1786				8	14	44	34
1786, Aug. 19	..	Aug. 29		8	14	45	3
19	..	28		8	14	44	28
20	..	29		8	14	44	16
20	..	28		8	14	44	0
21	..	29		8	14	44	27
21	..	28		8	14	44	36
		Mean		8	14	44	28

Hence the heliocentric longitude of the descending node of the planet Venus was, 1786, August 25, at $8^h\ 39^m = 8^s\ 14^\circ\ 44'\ 38''$, which is to be depended on to 10 or 15″. According to Cassini's tables, the longitude of the node is $8^s\ 14^\circ\ 48'\ 31''$, the difference, or the error, $-\ 3'\ 53''$. According to Halley's $8^s\ 14^\circ\ 42'\ 39''$, the difference $+\ 1'\ 59''$. From De la Lande's $8^s\ 14^\circ\ 45'\ 15''$, the difference only $-\ 37''$.

In order to ascertain the annual motion of the node, this observation is to be compared with the other observations of other astronomers. The numbers in the column A are found by setting out from my own observations 1786. In the column B, M. de la Lande's observation of 1769 is taken as the first; the series C is begun with Mr. Horrox's determination of 1639; and in the series D, M. Cassini's observation 1698 is taken as the basis.

Astronomers' Names.	The time of observation.	Heliocentric longitude of ☊ ♀.				Annual motion of ☊ ♀. A.	B.	C.	D.
		s	°	′	″	″	″	″	″
Horrox......	1639, Dec. 4	2	13	27	50	31.3	31.6	—	
Cassini	1698, Sept. 4	2	14	1	45	29.2	29.2	36.5	
Cassini	1705, June 11	2	14	2	52	30.9	31.3	31.9	
Cassini	1731, April 7	2	14	17	2	30.0	30.2	32.7	24.9
De la Caille ..	1746, Dec. 21	2	14	23	10	32.1	34.3	31.0	26.8
De la Caille ..	1761, June 5	2	14	31	30	31.2	—	31.3	27.3
De la Lande ..	1769, June 3	2	14	36	20	28.1	—	31.6	29.2
Bugge	1786, Aug. 25	2	14	44	38	—	—	31.3	29.2
					Mean	30.4	31.3	32.3	27.5

If the mean be taken of these 4 means, the annual motion of Venus's node will be 30.37″, or nearly 31″, adopted in the tables of Halley and De la Lande.

2. *The greatest inclination of the orbit of Venus to the ecliptic.*

In the first place are set down the observed geocentric longitudes and latitudes, corrected for aberration and nutation.

Mean time at Copenhagen.				Geocentric longitude of ♀.				Geocentric latitude of ♀.			Halley's error, in long.	Halley's error, in lat.	De la Lande's error, in long.	De la Lande's error, in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″	″	″
1781, July 20	0	1	40	4	11	10	49	1	24	46 N	+ 30	+ 5		
24	1	5	45	4	16	5	52	1	27	21	+ 21	+ 5		
30	1	11	18	4	23	28	41	1	29	21	+ 29	+ 9	+ 69	+ 10
31	1	12	9	4	24	42	29	1	29	36	+ 32	+ 12		
Aug. 1	1	12	58	4	25	55	54	1	29	21	+ 18	+ 3		
4	1	15	21	4	29	37	13	1	29	4	+ 20	+ 7		
1782, July 13	21	9	19	2	10	54	17	2	12	22 S	+ 53	+ 2	+ 47	+ 3
Nov. 5	22	50	43	6	29	46	25	1	21	11 N	+ 40	+ 3	+ 65	+ 4
1783, Sept. 19	2	4	37	7	3	52	29	6	3	1 S	−115	+ 39	− 58	+ 24
20	2	2	2	7	4	15	51	6	10	26	−122	+ 49		
26	1	43	58	7	5	55	9	6	50	19	−190	+ 55		
Oct. 2	1	22	55	7	6	20	38	7	20	29	−130	+ 58	− 24	+ 44
1784, May 18	22	29	58	1	7	1	56	1	28	35 S	+ 57	+ 6	+ 44	+ 5
Sept. 8	0	30	11	5	24	45	11	1	20	2 N	+ 27	+ 3		
20	0	37	17	6	9	40	27	1	6	15	+ 41	+ 15	+ 77	+ 12
25	0	39	57	6	15	52	59	0	57	50	+ 15	+ 3		
1785, July 29	20	53	21	2	22	5	27	3	53	17 S	− 40	+ 13	− 13	+ 19
Nov. 27	21	29	57	7	9	30	40	1	37	43 N	− 5	+ 4	+ 26	− 2
1786, June 19	1	43	7	3	21	39	32	1	30	40 N	+ 12	+ 12		
24	1	49	20	3	27	44	41	1	35	52	+ 53	+ 19		
29	1	55	7	4	3	47	42	1	38	54	+ 3	+ 5		
July 1	1	57	17	4	6	13	9	1	39	45	+ 9	+ 7	+ 43	+ 8
14	2	9	7	4	21	54	24	1	37	40	− 6	+ 10		
1788, May 6	3	1	24	3	0	23	44	2	43	15 N			+ 20	+ 22
7	3	2	17	3	1	28	48	2	44	22			+ 27	+ 17
9	3	4	4	3	3	38	13	2	46	26			+ 39	+ 14

The angle at the planet is found in the manner before mentioned. The following table contains the heliocentric longitudes and latitudes to the moments of mean time in the foregoing table. The heliocentric place of the node is ascertained with a tolerable degree of accuracy; hence the arc of the ecliptic from the node to the circle of latitude, passing through the planet, is given $= d$; the inclination of the orbit to the ecliptic $= y$ is to be calculated by this formula, cot. $y = \frac{\text{sing. } d \times \text{sin. lat.}}{\text{tang. hel. lat.}}$.

	Heliocentric longitude of ♀ in the ecliptic.				Heliocentric latitude of ♀.				Halley's error, in long.	Halley's error, in lat.	De la Lande's error, in long.	De la Lande's error, in lat.	Inclination of ♀'s orbit.		
	s	°	'	"	°	'	"		"	"	"		°	'	"
1781, July 20	5	0	0	53	3	16	55	N	+ 18	+ 12			3	23	32
24	5	6	30	59	3	21	27		+ 7	+ 7			3	23	31
30	5	16	15	59	3	23	36		+ 12	+ 21	+115	+ 21	3	23	41
31	5	17	53	28	3	23	30		+ 16	+ 29			3	23	49
Aug. 1	5	19	30	31	3	22	43		+ 3	+ 7			3	23	27
4	5	24	22	35	3	20	41		+ 4	+ 15			3	23	35
1782, July 13	0	4	2	37	3	11	57	s	+116	+ 4			3	23	26
Nov. 5	6	9	46	4	3	4	18	N	+ 40	+ 5			3	23	27
1783, Sept. 19	11	6	41	21	3	21	45	s	− 97	+ 23	−183	+ 23	3	23	44
20	11	8	16	11	3	22	31		−105	+ 27			3	23	48
26	11	17	45	5	3	23	30		−175	+ 28			3	23	47
Oct. 2	11	27	16	44	3	18	56		−117	+ 31	−195	+ 30	3	23	49
1784, May 18	0	5	34	53	3	10	14	s	+ 71	+ 13	− 1	+ 11	3	23	34
Sept. 8	6	6	30	11	3	8	56	N	+ 18	+ 7			3	23	27
20	6	25	51	19	2	23	46		+ 28	+ 30	+116	+ 27	3	23	58
25	7	3	52	40	2	13	7		+ 1	+ 7			3	23	33
1785, July 29	11	15	44	9	3	23	30	s	− 68	+ 12	−154	+ 12	3	23	32
Nov. 27	6	0	50	45	3	15	22	N	+ 1	+ 2	+ 99	− 2	3	23	22
1786, June 19	4	25	53	12	3	12	43	N	+ 1	+ 26			3	23	41
24	5	4	2	2	3	20	31		+ 44	+ 43			3	24	0
29	5	12	8	55	3	23	18		− 5	+ 11			3	23	30
July 1	5	15	23	58	3	23	33		+ 1	+ 14	+109	+ 14	3	23	35
14	6	6	28	2	3	9	10		− 8	+ 18			3	23	38
1788, May 6	5	16	58	34	3	23	37	N			+ 20	+ 20	3	23	46
7	5	18	36	3	3	23	12				+ 27	+ 21	3	23	28
9	5	21	50	56	3	22	4				+ 39	+ 19	3	23	38
											Mean of all		3	23	37.7

The heliocentric latitudes observed 1781 July 30, 1783 Sept. 26, 1785 July 29, 1786 July 1, 1788 May 6, are very near the greatest latitude; the mean of the inclinations found on these days is 3° 23′ 40″.2, and very near the mean of all the observations 3° 23′ 37″.7. The inclination, or the greatest heliocentric latitude, may also be found by interpolation of the maximum among the observed heliocentric latitudes. This maximum is found 1781 = 3° 23′ 39″, 1783 = 3° 23′ 41″, 1786 = 3° 23′ 36″; the mean of these 3 maximums 3° 23′ 38″.6, which inclination may be depended on to 1 or 2″. The inclination of the orbit of

Venus has been supposed in the tables of Cassini, Halley, and De la Lande, = 3° 23′ 20″, and the error of the tables + 18″.6.

3. *The heliocentric longitude and motion of the nodes of Mars.*

In a Paper printed in the Memoirs of the Royal Academy of Sciences at Stockholm, is determined the heliocentric longitude of Mars's ascending node = 1ˢ 17° 54′ 24″.2, in the year 1783, December 7, 20^h 23^m 39^s, mean time at Copenhagen: the error of Cassini's tables — 10′ 35″, of Halley's tables — 23′ 27″, of De la Lande's tables — 4′ 37″. The annual motion of Mars's node may be found by comparing the following observations of the longitude of the node. In the column A the numbers are going upwards from the observation 1783; in the column B the numbers are going downwards from the observation 1595.

Astronomers' Names.	Time of observation.	Heliocentric longitude of ☊ ♂.	Annual motion. A.	Annual motion. B.
		s ° ′ ″	″	″
Tycho Brahe ..	1595, Oct. 28	1 16 24 33	28.7	
Cassini	1700, May 6	1 17 13 43	28.9	29.4
Cassini	1721, Nov. 13	1 17 29 49	23.8	31.3
De la Caille ...	1747, May 14	1 17 37 11	27.5	28.9
De la Caille ...	1753, Nov. 4	1 17 42 5	24 6	29.6
Maskelyne	1778, April 17	1 17 51 40	28.5	29.3
Bugge	1783, Dec. 7	1 17 54 24		28.7
		Mean	27.0	29.5

The mean of the 2 series A and B will give the most probable annual motion of Mars's node 28″.2. In Cassini's tables the annual motion is 34″, in Halley's 38″, and in De la Lande's 40″.

4. *The inclination of the orbit of the planet Mars.*

Mean time at Copenhagen.				Geocentric longitude of ♂				Geocentric latitude of ♂.			Error of the tables of M. de la Lande, in long.	in lat.
	h.	m.	s.	s	°	′	″	°	′	″	″	″
1788, Jan. 9	11	57	7	3	16	27	7	4	5	25 N	+17	+ 1
10	11	51	28	3	16	3	35	4	6	10	+19	+ 7
11	11	45	50	3	15	40	15	4	6	44	+23	+10
26	10	24	50	3	10	43	39	4	3	59	+45	+19
Feb. 14	8	58	52	3	8	13	29	3	39	35	— 8	+ 8
Mar. 9	7	37	35	3	11	13	55	3	1	13	— 7	— 4
12	7	29	7	3	11	59	16	2	56	52	+ 2	+ 7
13	7	26	19	3	12	15	21	2	55	24	+ 2	+ 9
14	7	23	35	3	12	31	44	2	53	56	+ 3	+ 8
16	7	18	10	3	13	5	38	2	50	56	—18	+ 7
April 6	6	27	49	3	20	29	42	2	22	46.7	—17	+ 8

The geocentric longitudes of Mars are corrected for aberration and nutation, and compared with De la Lande's newest tables, which after the last improvements commonly give the true place of Mars within the 4th part of a minute. The error in longitude + 17″ signifies that the longitude in the tables is 17″ too small; and that those 17″ are to be added to the calculated longitude, in order to make it agree with the observed longitude.

Mean time at Copenhagen.				Heliocentric longitude of ♂.				Heliocentric latitude of ♂.			Error of the tables of M. de la Lande,		Inclination of the orbit of ♂.		
											in long.	in lat.			
	h.	m.	s.	s	°	′	″	°	′	″	″	″	°	′	″
1788, Mar. 9	7	37	35	4	15	12	28	1	50	49.1 N	− 7	− 3	1	50	56
12	7	29	7	4	16	31	42	1	50	53.5	+ 2	− 4.5	1	50	56
13	7	26	19	4	16	58	8	1	50	56.0	+ 2	− 3	1	50	56.4
14	7	23	35	4	17	24	28	1	50	57.7	+ 3	− 1.3	1	50	57.7
16	7	18	19	4	18	16	50	1	50	56.7	− 4	− 2.3	1	50	56.7

The inclination of Mars is taken in Cassini's tables 1° 50′ 54″, and in the tables of De la Lande and Halley 1° 51′ 0″.

Mr. B. concludes this paper with the opposition of Mars according to the foregoing observations. The opposition of Mars to the sun happened 1788, Jan. 7, at $8^h\ 19^m\ 32^s$ true time; the apparent geocentric longitude of Mars at that moment = $3^s\ 17°\ 17'\ 8''$, and the geocentric latitude = 4° 4′ 3″ N. Saturn was in opposition to the sun, Aug. 29, $20^h\ 51^m\ 11^s$ true time; the apparent longitude ♄ = $11^s\ 7°\ 31'\ 34''$, and latitude 1° 59′ 33″ S. The new planet was in opposition to the sun Jan. 18, $0^h\ 28^m\ 33^s$ true time, the longitude = $3^s\ 28°\ 10'\ 7''$, and latitude 0° 34′ 35″ N.

III. An Account of some luminous Arches. By Mr. Wm. Hey, of Leeds, F.R.S. p. 32.

While Mr. H. was at Buxton, in March 1774, about half past 8 he saw a luminous arch, which appeared very beautiful in the atmosphere. Its colour was white, inclining to yellow; its breadth in the crown apparently equal to that of the rainbow. As it approached the horizon, each leg of the arch became gradually broader. It was stationary while he viewed it, and free from any sensible coruscations. Its direction seemed to be from about the N.E. to the S.W. at least its eastern leg was inclined to the north, and its western to the south. Its crown, or most elevated part, was not far from the zenith. The evening was clear, and the stars appeared bright. It continued about half an hour after it was first observed by the company.

In October 1775, he saw a similar arch at Leeds, of the same colour, breadth, and position. It began to disappear in 5 or 6 minutes after he had discovered it, without changing its situation. The manner in which it vanished was quite irregular; large patches in different parts, and of different dimensions, ceasing to be luminous, till the whole had disappeared. The evening was rather cloudy.

In the evening of March 21, 1783, between 8 and 9 o'clock, Mr. H. observed something like a bright cloud in the eastern part of the hemisphere, and also a similar appearance in the opposite part of the heavens. These luminous parts, which appeared in the eastern and western parts of the horizon, were connected by an arch of a fainter light.

It reached the horizon in the W.S.W. point. In its course it passed about 12° to the south of the zenith. Its breadth was about 9 or 10°. It remained visible about 10 or 12 minutes after he first discovered it, and then vanished gradually and irregularly. He observed no coruscations, nor any motion in this arch. A few minutes after another, and still more beautiful, arch made its appearance. It arose a point or 2 nearer the N.E. than the former had done. Its southern edge passed up a little to the north of the tail of the Great Bear, which was then in a vertical position. Its northern edge appeared at first a little to the south of the polar star; but, during the continuance of the phenomenon, it gradually receded about 10° to the south. The arch descended about the W.N.W.; but neither the eastern nor western extremities reached the horizon; each of them ending in a point gradually formed a little above the horizon. This arch might be about 10 or 12° at its vertex. It continued visible for half an hour; and though he could not discover any coruscations, or quick motion, in any part, yet the different portions of it were perpetually varying in the density of their light, and the whole arch, or at least its vertex, made a slow and equable motion towards the south. Where the light was the most dense, the smaller stars were rendered invisible by the arch, but stars of the 2d magnitude were not totally eclipsed by it. This arch disappeared, as the former, by patches; the light gradually becoming less intense. The colour of both these arches was white. Before the latter arch had entirely disappeared, a small one, not quite so broad as the rainbow, arose from its eastern leg, and ascending in a curvilineal direction to the polar star, terminated there. Its light was more faint than that of the other 2 arches; and it continued visible about a quarter of an hour. The evening was very fine when he saw these beautiful phenomena; the stars were bright, and there was not a cloud to be seen except in the horizon. There was a steady light in the north, without the least coruscation, extending from the N.E. to N.W. The wind blew from the N.E.

March 26, about the same time in the evening, Mr. H. was entertained with a similar appearance. He first observed 2 or 3 columns of aurora borealis shooting upwards in the north; and in a short time after a complete arch, like those already described, though somewhat different in its position. It arose between the E. and N. and N.E. points, passed obliquely to the south below Arcturus, and descended in the west through Orion, having almost the same direction through that constellation which the equator has. Its light was the most faint about the vertex of the arch. Its most dense parts were continually varying in the intensity of their light. The larger stars were visible through its densest parts. It varied its position, and it continued visible about half an hour; but there was nothing which could be called a shooting or quick coruscation. There was a steady northern light all the evening, or at least till the arch had disappeared.

The grandest specimen of this phenomenon which Mr. H. had seen appeared on the 12th of April, between 9 and 10 in the evening. He perceived a broad arch of a bright pale yellow, arising between Arcturus and Lyra, about the right leg of Hercules, and passing considerably to the south of the zenith, its northern border being a little south of Pollux, and descending to the horizon near Orion, which was then setting. This arch seemed to be about 15° in breadth, and was of such a varied density, that it appeared to consist of small columns of light, which had a sensible motion. After above 10 minutes he saw innumerable bright coruscations, shooting out at right angles from its northern edge, which was concave, and elongating themselves more and more till they had nearly reached the northern horizon. As they descended, their extremities were tipped with an elegant crimson, such as is produced by the electric spark in an exhausted tube. After some time this aurora borealis ceased from shooting, and formed a range of beautiful yellow clouds, extending horizontally about a quarter of a circle. The greatest part of the aurora borealis which darted from this arch towards the north, as well as the cloud-like and more stationary aurora, were so dense, that they hid the stars from view. The moon was 11 days old, and shone bright during this scene, but did not eclipse the brightness of these coruscations. The wind was at north, or a little inclined to the east.

The last phenomenon of this kind which Mr. H. saw was on the 26th of April. About a quarter before 10 in the evening, he observed in the W. a luminous appearance, of the colour of the most common aurora borealis. From this mass or broad column of light issued 3 luminous arches, each of which made a different angle with the horizon. That nearest to the south seemed to arise at right angles with the horizon; while that nearest to the north made the smallest angle, and passed towards the N. E. through the constellation Auriga, having Capella close to its upper edge. He had not viewed them many minutes when they were rendered invisible by a general blaze of aurora borealis, which possessed the space just before occupied by these arches. He was soon satisfied that where the aurora borealis was dense, it entirely hid from view the stars of the 2d magnitude. He observed this particularly with respect to the star β in the left shoulder of Auriga. But the coruscations were never so dense as to render Capella invisible. The wind was between the N. and N. E. this evening.

After comparing the phenomena above described with each other, and with those observed by Mr. Cavallo, in London; by Mr. Swinton, at Oxford; by Dr. Huxham, at Plymouth; and by Mr. Sparshal, at Wells, in Norfolk; Mr. H. cannot entertain a doubt, that these arches had all the same origin; and that they ought to be considered as a species of that kind of meteor called aurora borealis.

IV. Extract of a Letter from the Rev. F. J. H. Wollaston, M. A., F. R. S. (dated Sydney College, Cambridge, February 24, 1784,) to the Rev. Francis Wollaston, LL. B., F. R. S., containing the Observation of a Luminous Arch. p. 43.

I send you an account of a remarkable stream of light which appeared last night, from about $9^h\ 5^m$ to $9^h\ 25^m$, extending entirely across the hemisphere from W. to E. It rose from the horizon, about 10° S. of W., near δ and γ Ceti; thence ascended in a straight line, inclining a little S. to δ and ϵ Tauri, where it made an angle with its former course, and proceeded nearly in a vertical circle over β Aurigæ, ϑ Ursæ Majoris, by Cor Caroli to Arcturus, setting in the horizon about 20° N. of E. The light was steady, not undulating like Aurora; and as it converged towards the horizon at each end, had much the appearance which I conceive the tail of a comet must make whose nucleus is just in the horizon. That was particularly the case at the W. end, where it was brightest, becoming gradually fainter towards the zenith; the E. part was nearly of the same brightness. The greatest breadth of the stream in the zenith was about equal to the distance between the pointers in Ursa Major. It disappeared gradually. When first I saw it, it did not incline so much towards the S. at its W. end as afterwards; but rose directly up from δ Ceti to the zenith. I remarked this, because I never saw a stream extend so steadily across the heavens. There was very little of Aurora in any other part of the sky; indeed what would not have been observed at all, had it not been for this stream.

V. Of a Luminous Arch. By the Rev. Mr. B. Hutchinson. p. 45.

Last night, (Monday, Feb. 23, 1784,) at 9 o'clock, a very uncommon aurora borealis appeared at Kimbolton. When Mr. H. saw it, it had formed a perfect, uniform semi-circle, of the apparent breadth of half a yard, reaching like the rainbow (which it entirely resembled, only that its colour was simple,) from the W. S. W. horizon to that of the E. N. E. Some of the brightest stars of the Bull only just could be seen through it. The whole hemisphere was without a cloud; the wind had gone down at west; a slight frost, after a warm thaw, was taking place; and there was no other aurora borealis in the heavens till this began to fade away, which then however arose a little, due N., but without any streamers; the ring had no vibratory motion. Its zenith distance on the meridian south 11°. Kimbolton is 63 miles N. N. W. of London, latitude 52° 20′.

VI. On a Luminous Arch. By J. Franklin, Esq., of Blockley. p. 46.

Mr. F. states that on Feb. 23, 1786, he observed a very odd appearance in the heavens about a quarter before 9 that evening. He was much surprized to see a white light, broader than a rainbow, pass across the heavens from east to west. It was a bright white light, about 5° wide in the zenith, and gradually coming to a point both ways. The eastern point terminated between Arcturus and the bright star in the knee of Bootis. The western point came nearly to the star marked a in the Whale's mouth. The southern side of the light was about 5° above Castor, passing eastward above Berenice's hair, and westward near Aldebaran, and through the Hyades. He observed it till 9 o'clock. Aldebaran was south of it when he first saw it; but it passed, and got north, before 9 o'clock. At 5 minutes past 9, no more of it was to be seen. It gradually went off in a few minutes. The sky was very clear from clouds, and the stars shone bright.

VII. Of some Luminous Arches. By Edward Pigott, Esq. p. 47.

Being at Kensington on Feb. 23, Mr. P. saw, at 9 o'clock at night, a very singular, luminous arch in the sky, about 4° in breadth, resembling much a bright white cloud, drawn out in great length, or something like the uncoloured northern lights, without flashes, but seemingly of a more substantial texture; the stars appeared very bright through it; it probably had already existed some time. At about $9^h\ 7^m$ he noted its track thus: it was visible very near the horizon in the N. E., passing between Arcturus and η Bootis, almost covered the cluster of Coma Berenices, and β Geminorum, then passed to the south of Aldebaran, over the stars o, g, or π Orionis, where its light was fainter, and disappeared a few degrees lower. Though its first appearance was that of a beautiful regular arch, after a few minutes, its form had varied a little, and became rather twisted, so that β Π was sometimes to the north or south of its centre, without being uncovered. At $9\frac{1}{3}^h$ its light was much fainter, broader, and more crooked. At $9^h\ 40^m$ its length was decreased, extending only as far as the Gemini's feet. It also had moved to the south of the cluster in Berenice, and of β Π, passing through Cancer. Its breadth at this time was considerably increased, perhaps to more than double what it was at first, and its brightness much faded. The southern side became flaky, having about half a dozen parts hanging down, not unlike the tails of comets, the north side remaining even; it seemed approaching towards its dissolution. The north horizon exhibited a faint aurora borealis. Among the phenomena of this kind recorded in the

Philos. Trans. there are 2 resembling so exactly the above, that they deserve the consideration of the learned; one was seen in 1734-5, the other in 1749.

Some years before also, Mr. P. observed a few others, very similar to that just described; he therefore adds a short account of them, viz. at Brussels, March 14, 1774, at about 7 o'clock in the evening, the sky being very clear, there appeared an arch resembling a bright white fog, about 8 or 9° broad, tolerably well defined; the brightness of the stars it covered was diminished. The phenomenon lasted about $\frac{3}{4}$ of an hour. The air was cold, but not frosty. Towards midnight an aurora borealis was seen in the north, which appeared something like the phenomenon just mentioned. Again, March 15, 1774, at about $7\frac{1}{2}$ o'clock in the evening, a column of light appeared in the north, something like that of yesterday; weather very fine. Also, at Louvain, 1775, April 19, $9^{h}\ 30^{m}$, at night, after a storm, he saw a bright white line of light 1 or 2° in breadth, extending from N. N. E. through N. to N. W. almost parallel to the horizon, and elevated about 9°. It was brighter in the centre, and stars of the 3d and 4th magnitude which it covered were much diminished in brightness; it sometimes rapidly vanished and re-appeared, and altogether lasted near half an hour. Lastly, at Wickhill in Gloucestershire, 1777, Feb. 26, at about 7^{h} at night, he saw a faint white tract of light, not unlike a foggy column, about 6 or 8° in breadth. It extended from the horizon W. by S. to E. by S. passing over the stars in Orion's feet, and a very little to the north of Sirius. It seemed to have no motion, or to alter in brightness. The air was rather foggy, with a few clouds and a little wind. At about 10 o'clock a slight aurora borealis appeared in the north with streaks, extending sometimes to the zenith.

These kinds of lights seem to differ from the common aurora borealis in several particulars: their light is more condensed; they assume the form of an arch or column, and appear either to the north or south of the zenith, though he thinks oftenest to the south.

VIII. Experiments on the Analysis of the Heavy Inflammable Air. By Wm. Austin, M. D., Fellow of the College of Physicians. p. 51.

In a paper read before the R. S. in the year 1788, Dr. A. suggested an idea, that the heavy inflammable air is a compound of the light inflammable and phlogisticated airs. At that time he had observed, that the heavy inflammable air, or at least fixed air, is formed on the decomposition of nitrous ammoniac by heating it in close vessels; and that this air is affected by the electrical shock, like other elastic fluids into whose composition the light inflammable air enters. The conclusion he then drew from those facts seems to be supported by several subsequent experiments, which he here lays before the R. S. Several elastic fluids containing the light inflammable air, as the hepatic and alkaline airs, being decomposed by the electric spark, Dr. A. was induced to try it on the heavy inflammable air, as soon as he suspected that it contained the lighter air as a constituent part. This experiment immediately detected the light inflammable air; for such an expansion took place as could not arise from any other known substance. Thus the heavy inflammable air was sometimes expanded to twice its original volume; and yet not a 6th part of the whole was found to have undergone a decomposition: for instance, when $2\frac{3}{4}$ measures were expanded to 6, it appeared by experiment, that nearly $2\frac{1}{2}$ measures remained in their original state. After the inflammable air has been expanded to about double its original bulk, he does not find that it increases further by continuing the shocks.

From this partial decomposition of the heavy inflammable air we obtain a mixture of the 2 inflammable airs with phlogisticated air; that is, of the heavy inflammable air not decomposed, of the light inflammable air disengaged by the spark, and of phlogisticated air. How much of this phlogisticated air pre-existed in the heavy inflammable air, and how much was disengaged during the operation, it is not easy to determine. Neither are we acquainted with any substance which will separate the 2 kinds of inflammable air by combining with the one and leaving the other: but we know that dephlogisticated air will combine, in certain proportions, with each of them, either mixed or separate; that with one of them it forms fixed air, with the other water. Therefore, by inflaming dephlogisticated air with a mixture of these 2 airs, and observing the quantity of dephlogisticated air consumed, and the quantity of fixed air produced, we discover the excess of dephlogisticated air consumed, above what is sufficient for the production of the fixed air; and may conclude, that this excess of dephlogisticated air has combined with light inflammable air. This conclusion is further confirmed by attending carefully to the contraction which takes place on inflaming these airs, which is much greater in proportion to the quantity of fixed air produced, when a mixture of the 2 inflammable airs is inflamed, than when the heavy inflammable air is burnt alone. It is well known, that in all experiments of this kind, what remains after the combustion of the airs mixed together in due proportion, and after the separation of the fixed air, is chiefly phlogisticated air. From a considerable number of experiments conducted with great care and attention to all these circumstances, the Dr. endeavoured to approximate to the quantities of the phlogisticated and light inflammable airs disengaged, when a given quantity of the heavy inflammable air was decomposed. But all that could be attained to, was only an approximation to truth. The quantity of air decomposed by this method was so small, and the separation of the different parts into which it was resolved was attended with such difficulties, that an accurate analysis of the heavy inflammable air can never be obtained in this manner.

Dr. A. therefore attempted to decompose the heavy inflammable air by means of sulphur, which readily unites with the light inflammable air in a condensed state, and with it forms hepatic air. Having introduced some sulphur into a retort, filled with heavy inflammable air, and applied a sufficient heat to melt and sublime it, a considerable quantity of hepatic air was formed. After this air was absorbed by water, he could not perceive that the remaining air differed from the heavy inflammable air before the operation. Sulphur mixed with powdered charcoal, on being heated, yields hepatic air in great abundance, almost the whole of which is absorbed by water. The small unabsorbed residue, which does not exceed 100th part of the bulk of the whole air, appears to be phlogisticated air.

In whatever manner the heavy inflammable air was decomposed, whether by passing the electrical spark through it, by melting sulphur in it, or by heating sulphur and charcoal together, an appearance constantly occurred, which seemed to indicate, that volatile alkali is formed, whenever the heavy inflammable air is decomposed. The circumstance is this: a small piece of paper, stained with any blue vegetable substance, is turned green by standing in the air during any of these processes; and this green is changed to red on the addition of an acid. The inflammable air had been very long exposed to water, and had no such effect on blue vegetable substances before the operation. The Dr. has concluded these analytic attempts with several observations on the formation of fixed air from some substances, which consist only of the light inflammable, phlogisticated, and dephlogisticated airs, and from others, in which these 3 airs are combined with such matters as cannot be suspected of having any place in the composition of fixed air. He then gives a detail of the experiments on which these observations are founded. After which he adds: notwithstanding the utmost attention, we are liable to a small error in each of these experiments; and there is consequently a small variation in the results; but yet they concur sufficiently to justify the following conclusions. 1. That the heavy inflammable air contains the light inflammable air in great abundance. He apprehends this light inflammable air was, before the application of the electrical spark, a constituent part of the heavy inflammable air; because, if it were contained in the heavier air not as a constituent part, what should hinder its being burnt when the heavy inflammable air is burnt? Can it be supposed, that the heavy inflammable air should contain the light inflammable air in circumstances of combustion, and that the light inflammable air should escape the fire? And if the lighter air be burnt, the same quantity of dephlogisticated air would be necessary to saturate it before as after its being electrified. But it is evident from the preceding experiments, that much more dephlogisticated air is necessary to saturate the air, after it has been expanded by the electrical shock, than before.

2. That no fixed air is formed during the separation of the lighter air from the heavy inflammable air. Here it should be observed, that if the constitution of the heavy inflammable air depended on the union of the light inflammable and fixed airs, as some have supposed, we should certainly discover the fixed air, when the other part was separated from it. Or, should it be conjectured, that the light inflammable air is separated from water suspended in the heavy inflammable air, in that case, would not fixed air be formed from the other constituent part of the water uniting with the heavy inflammable air, in consequence of the repeated electrical shocks?

3. That the electrical shock separated a substance from the heavy inflammable air, which has some leading characters of an alkali. When inflammable air is decomposed by sulphur, or when hepatic air is made from charcoal and sulphur, we have the same appearance of an alkali. That this is the volatile alkali is evident from its evaporation, when hepatic air is made from sulphur and charcoal.

4. That the heavy inflammable air, through which the spark has been repeatedly passed, when burnt with any proportion of dephlogisticated air, does not produce so much fixed air, as the same quantity of inflammable air not electrified. Hence it is evident, that a part of the air is actually decomposed by the spark. Hence also we may infer, that the decomposed air is not resolved into light inflammable air and charcoal, of which some chemists have supposed it to consist, because the charcoal would combine with dephlogisticated air after its separation from light inflammable air, and we should not have such a defect of fixed air.

5. That the residues, after inflaming the decomposed air, are generally greater than those from the air in its natural state, or than can be accounted for from the mixture of the heavy inflammable and dephlogisticated airs. This affords a strong presumption, that phlogisticated air is extricated from the decomposed heavy inflammable air in a separate state, besides what enters into the volatile alkali, which is formed at the same time. If light inflammable air only were disengaged during the decomposition, the residues would certainly not be greater after inflammation with a sufficient quantity of dephlogisticated air; on the contrary, if the inflammable air were increased in proportion in the mixture, the combustion would be more complete, and the residues less.

Having observed, that sulphur readily combines with light inflammable air, if presented to each other at the instant that the inflammable air is detached from other bodies, before its particles have receded from each other, and that hepatic air is generally formed in this manner, he introduced some sulphur and heavy inflammable air into a glass retort, first filled with, and inverted in quicksilver, and applied a sufficient heat to melt it. The heat was continued till the sulphur was sublimed. The melted sulphur soon acquired a dark reddish colour; as it sublimed, it became quite black, and every part of the retort was covered with a black crust. On the depending part of the retort, where the melted sulphur lodged, and where the heat was strongest, there remained a black mark, which could not be removed by a much greater heat than that by which the sulphur was sublimed. The bulk of the air was not materially altered by this operation. A little blue paper being thrown up to the air after the operation, became green. Water absorbed about $\frac{1}{3}$ of it, and acquired a strongly hepatic smell. The inflammable air was carefully washed, so as to separate from it all the hepatic air. He then mixed this inflammable air with dephlogisticated air, and inflamed them, expecting to find a greater quantity of phlogisticated air in the residue, than when the inflammable air was burnt, which had not been subjected to this process. But the difference of the residue does not exceed $\frac{1}{13}$ the quantity of air decomposed in this manner, if we may judge from experiment.

The analogy between the heavy inflammable air and charcoal is illustrated by the formation of hepatic air from charcoal and sulphur. These substances, heated in a small glass retort, yield hepatic air in great abundance. The blue vegetable colour is turned green by exposure to this air. After hepatic air had been generated for a long time from the same materials, without admitting any common air into the retort, 99 parts in 100 of the air which came over last were absorbed by water. The insoluble part appeared to be phlogisticated air. Thus sulphur and charcoal, heated in a glass retort, yield hepatic air, phlogisticated air, and volatile alkali, or a substance very analogous to it.

As far as the Dr. has been able to discover by experiments, the heavy inflammable air and charcoal consist of the same elements in different proportion. The application of heat to pure charcoal confirms this opinion; for the production of heavy inflammable air from charcoal, by mere heat, is constantly accompanied with a production also of phlogisticated air. He apprehends, that in these cases the charcoal is decomposed and resolved into these 2 parts. Whenever charcoal or any substance containing it, is decomposed by heat only, the phlogisticated and heavy inflammable airs are produced; and when the heat is intense, Dr. Higgins has observed, that the air produced from these substances becomes rarer; probably in consequence of a portion of the heavy inflammable air itself being resolved by heat into its constituent parts. Dr. A. would not lay much stress on the appearance of phlogisticated air from the compound forms of vegetable, animal, and bituminous substances, all of which yield phlogisticated air and volatile alkali in great abundance; yet when the more simple modifications of the heavy inflammable air, as charcoal, vinegar, and, if Dr. Priestley is not mistaken, fixed air, give out phlogisticated air, when decomposed in close vessels, he cannot but infer, that phlogisticated air is an essential part of that peculiar substance which exists in all these states, whether that substance be called charcoal, or the gravitating matter of heavy inflammable air.

Hence it appears, that the phlogisticated and heavy inflammable airs combined, constitute charcoal; and that the mere application of heat always resolves charcoal into these 2 substances. But the heavy inflammable air is itself a compound of the lighter inflammable and phlogisticated airs. If phlogisticated air be combined with the heavy inflammable, or, which is the same thing, if light inflammable air be taken from it, charcoal is reproduced; therefore, when sulphur is melted in the heavy inflammable air, and hepatic air formed in it, the remaining parts of the heavy inflammable air return to the state of charcoal. And lastly, when sulphur is melted in contact with charcoal, the decomposition is complete; and the charcoal is resolved into its ultimate particles, the phlogisticated and light inflammable airs, with a small admixture of volatile alkali.

Thus far he had proceeded in the decomposition of the heavy inflammable air. The formation of this air, on many occasions, confirms what has been said concerning its analysis. In the resolution of compound bodies into their constituent parts, it may always be suspected, that the whole is not accounted for, that some part may have eluded observation, till the very parts we assign are put together, and the same compound is produced from them. The frequent production of fixed air, from substances generally not supposed to contain the heavy inflammable air, has lately given rise to a new system in chemistry. The author of this system has the merit of pointing out the appearance of fixed air in almost all phlogistic processes, in the combustion of various substances, in the reduction of metals, and in the decomposition of acids; phenomena which cannot otherwise be accounted for, than by showing that the specific matter of charcoal is a compound body; that its component parts are present in all these processes; and in some of them nothing else, if we except dephlogisticated air.

Dr. A. has already taken notice of the formation of fixed air from nitrous ammoniac, which is now well known to contain nothing but the phlogisticated, light inflammable, and dephlogisticated airs. This salt, heated in close vessels, yields dephlogisticated nitrous air in great abundance, mixed with a small proportion of fixed air. He has often repeated this experiment with nitrous ammoniac, which indicated no trace of fixed air either with lime water, or with acids, before its decomposition; but, when the salt was decomposed by heat, he always found lime-water rendered turbid by the generated air; and, on adding an acid to the turbid lime-water, has observed air-bubbles to be produced in it. When the 3 elementary airs are in a condensed state, and are set free from any combinations, they unite and form fixed air without the assistance of heat. Thus fixed air is generally produced when metals are dissolved in the nitrous acid. In these solutions, the component parts of nitrous acid and the light inflammable air, being extricated at the same time, unite before they have acquired the aëriform state, and constitute fixed air.

Objects are often too common or too near for our observation. Phlogisticated air presents itself in the decomposition of so many bodies, that its appearance excites no inquiry; and it is not regarded as essential to the chemical constitution of the bodies which yield it, excepting in the instances of nitrous acid and volatile alkali, 2 substances of very small extent in the scale of natural bodies. The calces of metals are well known to contain phlogisticated air; yet the effect of this air on calcination in general, and how far the very different calces of the same metal are influenced in colour or other properties by the different proportions of phlogisticated air, has never been considered. Fixed air is often formed from the calces of metals, mixed with water, or with some other substance containing light inflammable air. Red precipitate mixed with iron filings yielded very pure fixed air. Brass dust mixed with red precipitate, likewise gave out fixed air, though in less quantity. Turbith mineral and iron filings, treated in the same manner, afforded much less fixed air than the red precipitate and iron filings. It is probable, that the turbith mineral contains less phlogisticated air, than the red precipitate. The fixed air in all these experiments was mixed with phlogisticated and dephlogisticated air. Mr. Kirwan found, that the simple calx of mercury with iron filings and water produced fixed air. The same author also observed, that iron calcined with nitrous acid gave out, on being heated, fixed air; and he found the production of this air renewed on the addition of water. Dr. Priestley obtained fixed air from iron converted into rust by exposure to nitrous air. In all these experiments the 3 elementary airs are present, and, being expelled by heat from the metals with which they were combined, unite with each other, and form fixed air. It is not material to the present argument, whether the light inflammable air be supposed to be furnished from water, or from the regulus of a metal: it is enough for our purpose, that none of the substances employed in these experiments, contain heavy inflammable air or charcoal, in sufficient quantity to account for the fixed air produced, as Dr. Priestley has justly observed.

The growth of plants affords a strong proof of the formation of charcoal from the substances which have been assigned. If we may believe experiments, water and air alone are necessary to this natural process; yet vegetation is the great source of charcoal or heavy inflammable air. This inquiry is still in its infancy; but from the best experiments that have been made it should seem, that plants grow best in phlogisticated air; that they take in phlogisticated air, and give out dephlogisticated air. These phenomena cannot be accounted for but by supposing, that water is decomposed by growing plants; that part of its dephlogisticated air is discharged into the atmosphere; and that the other constituent part of water, with phlogisticated air, is taken into the growing substance. Thus the phlogisticated and light inflammable airs are brought together by the process of vegetation.

IX. On the Strata and Volcanic Appearances in the North of Ireland and Western Islands of Scotland. By Abraham Mills, Esq. p. 73.

At Moneymore, in Ireland, Mr. M. first perceived tumblers of lava; hence by Maghera, Garvagh, Coleraine, Portrush, and to Bush-Mills, lava is continually seen, either in solid masses, forming the basis of the vegetable soil, or else in tumblers dispersed over the surface. He employed 2 days in studying the various appearances at the Giant's Causeway, and regretted being obliged to quit it so hastily. So much has been already said on this spot, that he only remarks that the red ochry joints between the beds of rude lava, and the different heights at which the basalt pillars are seen, give probability to the conjecture, that the whole mass has been the produce of several successive eruptions. He embarked at Port Ballintrea, and after 12 hours sailing arrived at Ilay, where, inspecting the lead mines, it was impossible to avoid noticing the singular appearance of those masses which run in a kind of veins in various directions, and are called Whyn Dykes, which had in some places a basaltic appearance.

On returning from Ilay he landed at Portrush; and, in his way to Ballycastle, viewed the Giant's Causeway from the top of the cliffs, and was much struck with seeing below, in the 4th or eastern bay, a kind of Whyn Dyke, which ran into the sea towards the N. N. E. Examining the cliffs at Ballycastle, he found the horses, or faults, of which there are several between the coals, were veins of lava, resembling the Whyn Dykes of Ilay, standing vertically, intersecting the various strata of coal and freestone, and running into the sea. The largest of the veins or Whyn Dykes is near 12 feet in breadth, and ranges N. by E. and S. by W.

Returning to Dublin, through Clogh, Ballymena, Antrim, Glanevy, Moira, Banbridge, Loughbrickland, and to within a short distance of Newry, he constantly saw tumblers of lava, and in some places the fixed mass of lava, in which were fissures ranging N. E. and S. W. When Mr. M. reached home, his mind being strongly impressed with the similitude between the Ilay Whyn Dykes and those of Ballycastle, which take their rise in a country confessedly abounding with volcanic matter; that he might be enabled to form a better judgement of their substance when he should again visit Ilay, he repeatedly and attentively examined the Derbyshire and toad-stone in the neighbourhood of Buxton, and found it very like the specimens of the Whyn Dykes, which he had brought from Ilay.

Early in the last summer Mr. M. again visited Ireland, and having spent some time at the mines in the county of Wicklow, he proceeded to Belfast; and a little to the northward of that town he discovered in a bank a body of marl, running N. E. and S. W. between red and white sand-stone, the whole included and surmounted by a kind of toad-stone and rude lava, with joints having no particular direction. At Belfast he embarked for Ilay; but the wind obliged them to tide along the Irish shore, which, after passing Carrickfergus, chiefly consists of stupendous basalt cliffs. Farther north the cliffs are divided into horizontal beds of considerable thickness, by the intervention of a red substance, similar in appearance to that at the Giant's Causeway; near the water's edge, and under the lava, the white lime-stone is frequently seen; and these appearances continue all the way to Red Bay. Four miles from Clogh, under a bed of white lime-stone, 40 feet thick, saw the upper part of a bed of gneiss. Sailing from hence, plainly saw that the high broken point, which forms the N. E. point of Cushendun Bay, is composed of lava, with some rude appearance of pillars near the top; while close to the water's edge, and at some little distance in the sea, were tumblers of an immense size.

Entering the sound of Iona, saw that the rude coast of Mull, and the less elevated shore of Iona, were composed of red granite. At the landing place in Iona is laminated horn-stone; and a quarter of a mile north from the ruins of the cathedral is a vein of coarse red granite, 2 feet wide, standing nearly vertical, and ranging with the horn-stone E. N. E. and W. S. W.; on the surface are tumblers of red granite, and some few of lava. About a mile N. W. from the cathedral, and near the shore, is a vein, 2 feet wide, containing feld-spath and white mica, ranging E. and W. between granite sides. Many of the rocks are tinged with iron, and there is some bog iron ore in the mosses. In the S. W. part of the island, is a body of white marble, veined with pale green. At the Cove, where it is said St. Columb landed, the cliffs are of red granite, and the shore is covered with great variety of pebbles of serpentine, basaltes, granite, quartz, and other substances. Rowed from Icolmkill through the Bull Sound, which runs between Nun's Island and the island of Mull; on both sides the cliffs are of red granite, ragged and broken, without any regular beds or fissures, and having no particular range or inclination. Hence steered for Ardlun Head, which forms the S. W. point of Loch Leven, where they contemplated the wonderful arrangement of the basalt columns.

Near this place is a deep glen, running N. N. E. to the sea. It is about 30 yards in length, and 20 in breadth. The strata are disposed in the following extraordinary manner. The uppermost is 10 yards of lava, with horizontal divisions and vertical joints, taking the form of rude pillars. Under this is an horizontal bed of a perfectly vitrified substance, which appears to have been a shale, and is from 1 to 2 inches in thickness. Beneath this, is about 3 yards of a siliceous gravelly concrete; below which are horizontal beds of indurated marl, of various thicknesses, from 6 to 12 inches. The whole of these beds, taken together, are about 4 yards. Lastly, are 10 yards of rude lava, containing specks of quartz and mica unaltered, pieces apparently of granite, and some nodules of calcined chert. The whole is incumbent on regular basalt pillars, of various dimensions, from 18 to 6 inches diameter, varying in the number of their sides, some having 5, some 6, and others 7 sides. They are also as variously disposed; those on the western extremity of the glen being straight, and lying horizontally; while of those on the east side some are bare, and standing perpendicularly; and others, which are surmounted by the rude lava, are inclined and curved, as if they had taken that form in cooling from the pressure of the incumbent weight. See Tab. fig. 14, pl. 6. Many of the pillars are very full of bladder-holes; the articulations of the joints are close, though not so close as those of the Giant's Causeway; but, like those, their tops, where exposed, are either concave or convex.

At the extremity of the glen is an insulated rock, supported by basalt pillars (fig. 15,) which are somewhat curved and inclined. Incumbent on these are other pillars, lying nearly horizontal, and having a rude face of lava to the westward. At high-water this rock is inaccessible without a boat; but at low-water it may be easily got at, by stepping from one tumbler to another; and on the north side it is not difficult to climb to the top. The bottom of the glen is covered with large tumblers of lava the whole way down to the rock, and presents the rudest scene imaginable. Opposite Ardlun Head, on the north side of Loch Leven, is Ben Vawruch, a high promontory, with strata in horizontal beds; and the hill being of a circular figure gives it the appearance of having several terraces, with a kind of castle or cairn on the top. The columnar pillars at Ardlun are more or less regular for an extent of near a mile and a half; and all the projecting points of Loch Leven, as far as the eye could reach, appeared to be composed of lava.

Landed without difficulty on the eastern side of Staffa. The greatest extent of the island is about 1 mile from N. E. to S. W. and in one part not more than a quarter of a mile from S. E. to N. W. It is tolerably level, the shore every where steep, and the cliffs formed by basalt pillars or rude lava. On the south side, rising from a nearly horizontal bed of reddish stone, are beautiful basalt pillars of considerable height, and standing vertically; at a little distance are others inclined, and others which are curved, very similar to the ribs of a ship. There are 3 caverns amidst the basaltic pillars; one of them is now usually called Fingal's Cave; but the school-master at Icolmkill said that the Erse name for it is Fein, which signifies the melodious or echoing cave. On the northern part of the island, and at the cove where they landed, the cliffs are of coarse lava, without any pillars. In some parts of the island the tops of the pillars are standing bare; in other parts the surface is formed by a rude argillaceous lava, full of bladder-holes, some empty, others replete with quartz crystals. Calcareous spar, pebbles of indurated clay and shoerl, detached pieces of zeolite, are frequently seen, and the vegetable soil is a decomposed lava. In some places are met with gravel containing pebbles of basaltes, of red granite, and of quartz, their angles worn off, and they were become round and smooth.

In a small bay, about 1 mile to the S. E. of Ardlun Head, Loch Lyne, under a bed of jointed lava, which has some resemblance of pillars, and just at high-water mark, is a bed of coal, exactly 12 inches thick, intermixed with shale or bituminous shistus, dipping S. E. towards the loch 1 yard in 3; there is not any intervening substance between the coal and superincumbent lava, which contains many bladder-holes. Beneath the coal is also lava without any intervening matter. About 20 yards to the N. W. the coal again appears in the cliff, but is not more than from 8 to 10 inches thick. Here are tumblers of various sizes, scattered on the shore. Among them are some resembling the Derbyshire toadstone; and a short distance inland, to the S. W., are rude masses of lava, standing up at day, not unlike the great whyn dykes of Ilay. In the Loch, and at some distance from the opposite shore, there stood, within the memory of man, an insulated pillar of coal, from which the country people were accustomed to procure a supply for smiths' use; but the quantities they carried away, and the continual washing of the sea, have now entirely removed it. The island of Lismore, in the sound of Mull, is entirely limestone, excepting where it is crossed by the whyn dykes. In the island of Ulva are pillars somewhat resembling those of Staffa, but of a paler colour.—Canna also is basaltic, and resembles Staffa.—The Dutchman's Cap has rude pillars.—Cairnborough the same. Dunvegan in the isle of Skye has basaltic pillars, similar to Staffa.—On the S. W. side of the isle of Egg is a curious cavern.

Again embarked for Ilay; but, it being calm, and the tide against us, were obliged to anchor; and landed on an island which forms the S. E. point of the sound of Iona, which is a bare rock of red granite, broken and jointed in every direction. The upper surface of the granite, even in the very highest part, is all convex, which seems to prove, that by some convulsion it has been thrown up from the bed of the ocean, which, by long washing over it, had previously worn down its substance at the edges of all its numerous joints. On the east side of the point, and on the west side of a little bay, where the granite cliffs are at least 15 yards perpendicular, discovered a whyn dyke, or vein of lava, about 2 feet wide, included in a vertical fissure ranging S. E. by E. and N. W. by W. About 6 yards to the westward of the lava vein, or whyn dyke, is an immense fissure in the granite, ranging N. by W. and S. by E. It is from 9 to 10 feet wide, and, by estimation, about 120 feet deep, At the northern extremity, near the top, 2 stones are suspended in a most extraordinary manner between the sides: the under one is fixed, and on that the other appears to lie loose. (See fig. 16). There is a large cavern in the western side of the fissure, and a corresponding fissure is seen on the opposite shore.

Having shown that whyn dykes, or in other words veins of lava, are found in the vicinity of columnar basaltes, which latter are now, by almost universal consent, acknowledged to be of volcanic origin; Mr. M. now proceeds to describe the whyn dykes of Ilay. Ilay, from the northern to the southern extremes, is about 30 miles in length, and in one part extends nearly as much in breadth from the eastern to the western shores. After a rather minute account of several parts of the island, Mr. M. continues: The whyn dykes are too singular in their formation to escape the eye of the naturalist who traverses this island: they are masses, or rather veins, generally of a dark brown, apparently basaltic, matter, not unfrequently containing bladder-holes; from 3, 4, and 6 feet, to 8 or more yards in breadth, running in various directions. In some places they are straight for a considerable length; in others, their course, though progressive, is inflected; and in some parts they rise between 3 and 4 feet above the surface, forming natural boundaries or dykes, standing vertically, and appearing to fill up the chasms formed at some remote period in the strata. This is instanced in several of these dykes, in different parts of the island. One in particular stands vertically, is many yards in height, projects from the cliffs to the north-westward, and in that direction runs many fathoms into the sea. It bears the buffeting of the waves of the Atlantic Ocean from the south-west, and seems to defy their rage, though its breadth, compared with its height and length, is very inconsiderable, being not more than 5 or 6 yards wide. It is of a dark granular substance, very similar to the whyn dyke near Freeport, excepting that the central part is softer and of a paler colour. The outer sides, which are each about 2 feet thick, are of a very dark colour, hard, contain some bladder-holes and specks of zeolite, are generally detached from the centre by very small joints, and the whole is divided by transverse joints into irregular polygons of various dimensions. If this stupendous object is viewed from the north, it has much the appearance of a lofty wall of human fabrication. A small distance more to the southward is the great cave, in the Erse dialect called Ea mawr. The entrance is near 23 yards wide, and from 6 to 8 yards high. After going in a little way the roof rises, and the cavern extends in breadth; but at about 150 yards from the entrance, all its dimensions are contracted, and it becomes so small as barely to admit further progress by crawling on hands and knees. There are some calcareous stalactites pendent from the roof; and in this cave, as well as several others, wherever the water pervades through the joints of the chert, it tinges the sides of a ferruginous hue. Some veins of lead ore are also mentioned, but reported to be not worth the working.

If it be admitted, Mr. M. says, that he is right in his opinion of the volcanic origin of these different substances, a large tract will then be added to that already proved by others to have been subject to the effects produced by subterraneous fire; which, as far as has hitherto been discovered with us, commences in the s. w. part of Derbyshire, and is again seen in Seathwaite, about 5 miles from Hawkshead, in the N. W. part of Lancashire, and appears, N. W. from thence, in the neighbourhood of Belfast in Ireland, and ranging through the northern part of that kingdom; it is perceived in several of the western islands of Scotland, extending as far north as the island of Lewis, which is the northernmost of the Hebrides, and crossing east from Ilay, which is the southernmost, by Tarbut, Dumbarton, Stirling, and Edinburgh to Dunbar. Some persons may consider, with astonishment, the extent of those veins and masses of lava which appear in the northern part of the British isles, where no crater is visible; while others, who have read Von Troil, and recollect that he says, "That lava is seldom found near the opening of a volcano, but rather tuff, or loose ashes and grit," may perhaps unite with Mr. M. in opinion with Mr. Whitehurst, "that the crater whence that melted matter flowed, together with an immense tract of land towards the north, have been absolutely sunk and swallowed into the earth, at some remote period of time, and became the bottom of the Atlantic Ocean. A period indeed much beyond the reach of any historical monument, or even of tradition itself."

The more readily to compare the specific gravities of the Ilay lavas, and other substances, mentioned in this paper, with those from other parts, a table of their several weights is here added.

Ardlun coal	1.284
Jet, according to Dr. Watson	1.236
	1.180
Cannel coal from Haig in Lancashire	1.275
Whyn dyke, fr. near M'Arthur's head, N° 1,	2.863
Whyn dyke from Freeport, inside N° 2	2.881
The same from outside, N° 3	2.850
Whyn dyke from Gartness, N° 4	2.631
5	2.698
6	2.484
7	2.542
8	2.322
Whyn dyke from Gartness, N° 9	2.833
10	2.652
Basaltes from the Giant's Causeway	2.743
——— from Fairhead	2.950
——— from Ardlun	2.724
——— from Staffa	2.736
Vitrescent substance from Ardlun	2.800
Toadstone, from great rocks, Derbyshire, yellow grey	2.133
dark compact	2.634
ditto cellular	2.528
yellow grey from Bonsal	2.219

Fig. 14, pl. 6, is a view of the glen near Ardlun Head in Mull.

Fig. 15, a view of the insulated rock at the termination of the glen.

Fig. 16, a view of the great fissure, the cave, and the suspended stones, in the island of Mull. The fissure ranges N. and S. is about 10 feet wide and 40 yards deep; the sides and the suspended stones are granite.

X. On the Height of the Luminous Arch that was seen Feb. 23, 1784. By Henry Cavendish, Esq., F. R. S., and A. S. p. 101.

This arch was observed, at the same time, at Cambridge by Mr. Wollaston; at Kimbolton in Huntingdonshire, by the Rev. Mr. Hutchinson; and at Blockley near Campden in Gloucestershire, by Mr. Franklin; and is described in letters from those gentlemen read to the R. S. in December 1786. See p. 627, &c. of this volume. It has been remarked, that as the arches of the kind described in these papers have usually but a very slow motion, their height above the surface of the earth may readily be determined, provided they are observed about the same time, at places sufficiently distant; and they seem to be the only meteors of the aurora kind whose height we have any means of ascertaining. The places at which this phenomenon was seen are not so well suited for this purpose as might at first be expected from their distance, because they lie too much in the direction of the arch; they however seem sufficient to determine its height within certain limits, and perhaps are as well adapted for it as any observations we are likely to have of such phenomena.

The latitude of Cambridge is 52° 12′ 36″; that of Kimbolton is said by Mr. Hutchinson to be 52° 20′, and according to the survey of Huntingdonshire, published by Jefferies, is 52° 19′ 50″; so that we may suppose it to be 7 geographical miles north of Cambridge, and by the maps it seems to be about 18 such miles west of it; and Blockley is by the map 12 geographical miles south and 72 west of Cambridge. At Cambridge the observations of its track seem to have been made at about 9^h 15^m P. M. or 8^h sidereal time. At Kimbolton, allowing for the difference of meridians, they could hardly have been made more than 5^m sooner; and at Blockley they were most likely made nearly at the same times as at Cambridge.

At Blockley the arch passed about 7° south of the zenith; but it is unnecessary to determine this point with precision. At Kimbolton it was found by a quadrant to pase 11° to the south of it; and at Cambridge it was observed to pass through δ and ϵ Tauri, β Aurigæ, θ Ursæ majoris, Cor Caroli, and Arcturus. Now, if an arch was drawn through these stars, it must have appeared sensibly waved to the eye; whereas Mr. Wollaston did not take notice of any crookedness in this part of its course. It is most likely therefore, that the middle of the arch must have passed to the south of β Aurigæ, and to the north of θ Ursæ; and if a circle is drawn through δ Tauri, Arcturus, and a point 1° north of the zenith, it will differ but little from a great circle, and will agree as well with the positions of these stars as any regular line which can be drawn, and will pass $2\frac{1}{4}$° below β Aurigæ, and as much above θ Ursæ; which is not a greater difference from observation than may well have taken place, considering how much care and acquaintance with the fixed stars are required to determine a path by them so nearly.

The direction of the arch here described, in that part near the zenith, is W. 18° S.; and if a line be drawn through Cambridge in this direction, Kimbolton is 12.8 geographical miles north of it; and therefore, as the arch appeared 12° more south at Kimbolton than at Cambridge, the height of the arch above the surface of the earth must be 61½ geographical or 71 statute miles. If we suppose that the middle of the arch really passed through β Aurigæ, the height comes out 52 statute miles. On the whole the height could hardly be less than 52 miles, and is not likely to have much exceeded 71.

The common aurora borealis has been supposed, with great reason, to consist of parallel streams of light shooting upwards, which, by the laws of perspective, appear to converge towards a point; and when any of these streams are over our heads, they appear actually to come to a point, and form a corona. Hence, from analogy, it seems not unlikely, that these luminous arches may consist of parallel streams of light, disposed so as to form a long thin band, pretty broad in its upright direction, and stretched out horizontally to a great length one way, but thin in the opposite direction. If this is the case, they will appear narrow and well defined to an observer placed in the plane of the band; but to one placed at a little distance from it, they will appear broader, fainter, and less well defined; and when the observer is removed to a great distance from the plane, they will vanish, or appear only as an obscure ill-defined light in the sky.

There are two circumstances which rather confirm this conjecture: first, that though we have an account of another arch besides this* having been seen at great distances in the direction of the arch, we have none of any having been seen in places much distant from each other in the contrary direction; and 2dly, that most of them have passed near the zenith, whereas otherwise they ought frequently to appear in other situations; for if they appeared near the zenith to an observer in one latitude, they should appear in a very different situation in a latitude much different from that. Mr. C. however, does not offer this as a theory of which he is convinced; but only as an hypothesis which has some probability in it, in hopes that by encouraging people to attend to these arches, it may in time appear whether it is true or not. If it should hereafter be found, that these arches are never seen at places much distant from each other in a direction perpendicular to the arch, it would amount almost to a proof of the truth of the hypothesis; but if they ever are seen at the same time at such places, it would show that the hypothesis is not true. Supposing the hypothesis to be well founded, the height above determined will answer to the middle part of the band,

* That of Feb. 15, 1750. Phil. Trans. vol. 46, p. 472 and 647.—Orig.

provided the breadth of it was small in respect of its distance from the earth; but otherwise will be considerably below the middle. If the breadth of the band was equal to the distance of its lower edge from the earth, the height of the lower edge would be $\frac{3}{4}$ of that above found; and if the breadth was many times greater, would be half of it.

In the common aurora borealis, an arch is frequently seen low down in the northern part of the sky, forming part of a small circle. What this is owing to, he cannot pretend to say; but it is likely that it proceeds from streams of light which appear more condensed when seen in that direction than in any other, and consequently that the streams which form the arch to an observer in one place, are different from those which form it to one at a distant place, and consequently that no conclusion, as to its height, can be drawn from observations of it in different places. Attempts however have been made to determine the height of the aurora from such observations, and even from those of the Corona; though the latter method must surely be perfectly fallacious, and most likely the former is so too.

XI. Observations on Respiration. By the Rev. J. Priestley, LL.D., F.R.S. p. 106.

When Dr. P. wrote the observations on the subject of respiration, published in the Phil. Trans. vol. 66, p. 226, he supposed, that in this animal process there was simply an emission of phlogiston from the lungs. But the result of his late experiments on the mutual transmission of dephlogisticated air and of inflammable and nitrous air, through a moist bladder interposed between them, and likewise the opinions and observations of others, soon convinced him, that, besides the emission of phlogiston from the blood, dephlogisticated air, or the acidifying principle of it, is at the same time received into the blood. Still however there remained a doubt how much of the dephlogisticated air which we inhale enters the blood, because part of it is employed in forming the fixed air, which is the produce of respiration, by its uniting with the phlogiston discharged from the blood; for such he takes it for granted is the origin of that fixed air, since it is formed by the combination of the same principles in other, but exactly similar circumstances.

Dr. Goodwyn's very ingenious observations prove, that dephlogisticated air is consumed, as he properly terms it, in respiration; but, for any thing that he has noted, it may be wholly employed in forming the fixed air above-mentioned. He has proved indeed, that the application of dephlogisticated air to the outside of a vein will change the colour of the blood contained in it. But this might have been effected by the simple discharge of phlogiston from the blood, when it had an opportunity of uniting with the dephlogisticated air thus presented to it. He does not however seem to suppose, that there is any phlogiston discharged from the blood in the act of respiration, but only that dephlogisticated air enters into it. But that Dr. P.'s former supposition, as well as Dr. G.'s, is true, will appear, he presumes, from the experiments which he will presently recite.

As, in order to determine what proportion of the dephlogisticated air destroyed by respiration is employed in forming the fixed air which is the produce of it, it was necessary to ascertain as exactly as possible the proportion of dephlogisticated air and of phlogiston in the composition of fixed air, Dr. P. repeated with particular care experiments similar to those he had formerly made for that purpose. He heated charcoal of copper in 41 oz. measures of dephlogisticated air of the standard of 0.33, till it was reduced by washing in water to 8 oz. m. of the standard of 1.33. Again, he heated charcoal of copper in 40.5 oz. m. of dephlogisticated air of the standard of 0.34, till it was reduced to 6 oz. m. of the standard of 1.76. And in each of these cases there was a loss of 6 gr. of the charcoal of copper; so that there cannot be more than 6 gr. of phlogiston in 33 oz. m. of fixed air, and consequently that only a very little more than $\frac{1}{4}$ of the weight of fixed air is phlogiston. He also heated perfectly well burnt charcoal of wood in 60 oz. m. of common air, and found $\frac{1}{5}$ of the remainder to be fixed air, and the residuum of the standard of 1.7. Lastly, he heated $8\frac{1}{4}$ gr. of perfect charcoal in 70 oz. m. of dephlogisticated air, of the standard of 0.46, when it still continued 70 oz. m.; but after washing in water it was reduced to 40 oz. m. of the standard of 0.6, and the charcoal then weighed $1\frac{1}{4}$ gr.; so that from this experiment with common charcoal, as well as from the preceding with charcoal of copper, it appears, that about $\frac{1}{4}$ of the weight of fixed air is phlogiston, and consequently that the other $\frac{3}{4}$ are dephlogisticated air.

Having done this, he proceeded to ascertain how much fixed air was actually formed by breathing a given quantity both of atmospherical and of dephlogisticated air, in order to determine whether any part of it remained to enter the blood, after forming this fixed air. For this purpose he breathed in 100 oz. m. of atmospherical air, of the standard of 1.02, till it was reduced to 71 oz. m. and by washing in water to 65 oz. m. of the standard of 1.45. When the computations are properly made, as directed in a former paper, it will appear, that before the process this air contained 67.4 oz. m. of phlogisticated air, and 32.6 oz. m. of dephlogisticated air; that after the process there remained 53.105 oz. m. of phlogisticated air, and 11.895 oz. m. of dephlogisticated air; and that there were only 6 oz. m. of fixed air produced; for the quantity absorbed during the process could only have been very inconsiderable. It will therefore be evident that, in this experiment, 20.7 oz. m. of dephlogisticated air, which would weigh 12.42 gr. disappeared; whereas all the fixed air that was found would only have weighed 4.4 gr., and $\frac{1}{4}$ of this being phlogiston, the dephlogisticated air that entered into it would have weighed only 3.3 gr.; consequently 9.12 gr. of it must have entered the blood; which is 3 times as much as that which did not enter, but was employed in forming the fixed air in the lungs.

He breathed in 100 oz. m. of dephlogisticated air, of the standard of 1.0, till it was reduced to 58 oz. m., and by washing in water to 52 oz. m. of the standard of 1.75, with 2 equal quantities of nitrous air. The computations being made as before, it will appear, that before this process, this air contained 66 oz. m. of phlogisticated, and 34 oz. m. of dephlogisticated air; and that after the process there were 30.368 oz. m. of phlogisticated air, and 21.632 oz. m. of dephlogisticated air. In this case therefore, the dephlogisticated air that disappeared was 13.3 oz. m. weighing 7.8 gr. and the fixed air was 6 oz. m. weighing 4.4 gr.; so that here also about 3 times as much entered the blood as did not. These experiments he repeated many times, and though not with the same, yet always with similar, results, the greatest part of the dephlogisticated air, but never the whole, passing the membrane of the lungs, and entering the blood.

When the results above-mentioned are compared, it will appear, though the observation escaped Dr. Goodwyn, that part of the phlogisticated air entered the blood, as well as the dephlogisticated air; or, which is the same thing, that the dephlogisticated air which was consumed was not of the purest kind. This experiment Dr. P. repeated so often, and always with the same result, that he is confident he cannot be mistaken in this conclusion. This fact, of which he had no previous expectation, he first thought might be accounted for by supposing that the 2 constituent parts of atmospherical air, viz. the phlogisticated and dephlogisticated air, are not so loosely mixed as has been imagined; but rather that they have some principle of union, so that, though they may be completely separated by some chemical processes, they are not entirely so in this; but that the dephlogisticated air, passing the membrane of the lungs, carries along with it some part of the phlogisticated with which it was previously combined. But, at the suggestion of Dr. Blagden, he now thinks it more probable, that the deficiency of phlogisticated air was owing to the greater proportion of it in the lungs after the process, than before.

XII. An Account of the Trigonometrical Operation, by which the Distance between the Meridians of the Royal Observatories of Greenwich and Paris has been determined; By Major-general Wm. Roy, F. R. S., and A. S. p. 111.

The trigonometrical operation which is the subject of the present paper, had its commencement in the measurement of a base on Hounslow-heath in 1784, an account of which was given to the R. S. in the following year. On the completion of that first part of the business, it was little expected, that nearly 3 full years would have elapsed before an instrument could be obtained from Mr. Ramsden for taking the angles! At length however the instrument was produced, and placed on the 31st of July 1787, at the station near Hampton Poor-house, on the very spot where, about 35 months before, the measurement of the base had been completed. By commencing an operation of this nature, at so advanced a season of the year, it was sufficiently obvious, that only very faint hopes could be entertained of bringing it to a conclusion before the bad weather would set in. But it being of much importance to get the triangles, which extend across the Channel, at all events executed, it was therefore proposed to M. Cassini, who had been appointed by the Academy of Sciences to superintend their part of the business, that he should fix the time that might suit him best for meeting on the coast. This proposition being readily acceded to by him, the 20th of Sept. was appointed for repairing to the coasts of Dover and Calais respectively. In the mean time the operation was continued here with all imaginable care and assiduity, through the first 10 stations of the series of triangles from Hampton Poor-house to that at Wrotham-hill inclusively.

The instrument, and the various parts of the apparatus, were then removed to Dover, at which place Messrs. De Cassini, Mechain, and Le Gendre, members of the Academy of Sciences, arrived on the 23d of Sept., where, in the course of 2 days that these gentlemen staid, every thing was most amicably settled with regard to the times of reciprocal observations. A great number of white lights, fitted for long distances, and several reverberatory lamps, had been previously provided. Having been supplied with such a proportion of the lights as seemed necessary for their side of the channel, and one of the lamps, the French gentlemen departed for Calais on the 25th. For the greater part of the time, the weather was extremely bad; yet, on the particular nights when the most important observations on our side were made, namely, those at Dover and Fairlight Down, the nights happened very fortunately to be favourable, so as to enable us to intersect, with great accuracy, the two distant points on the French coast of Blancnez and Montlambert, or Boulemberg, and thereby to establish for ever, the triangular connection between the two countries. In finishing the co-operation with the French commissioners, at Lydd on the 17th of October, our instrument had now passed through 16 stations out of 23. There of course remained yet 7 stations where it was to be placed, and observations to be made. Eagerly wishing to bring the business to a conclusion, they struggled on through 5 of the 7. But the weather at length became so tempestuous, that it was utterly impossible to continue it, with any hopes of being able to make satisfactory observations. On the 2d of November therefore the instrument was sent to town, leaving the stations on Goudhurst and Frant Churches unoccupied till the ensuing season, and the winter months were employed in calculating the observations that had been made.

By various delays in repairing the instruments, the surveyors had again the mortification to be thrown into the latter season of the year, as it could not be placed on Goudhurst steeple before the 9th of August 1788. Having finished the few remaining observations, the paper proceeds to state the manner in which the calculations are made, and the account drawn up in several sections, viz.

Sect. 1. Description of the apparatus made use of in the measurement of the base of verification in Romney Marsh, with the hundred-feet steel chain, in the autumn of 1787, with the result of that operation. 2. General description of the great instrument with which the angles in the recent trigonometrical operation were observed; showing also its various adjustments for practice. 3. Description of various articles of machinery made use of in the course of the trigonometrical operation. 4. Calculation of the series of triangles extending from Windsor to Dunkirk, by which the geodetical distance between the meridians of the Royal Observatories of Greenwich and Paris is determined. 5. On the difference between horizontal angles on a sphere and spheroid. 6. Manner of determining the latitudes of the stations. Application of the pole star observations to computations on different spheres, and also on M. Bouguer's spheroid, for the determination of the difference of longitude. Ultimate result of the trigonometrical operation, by which the difference of the meridians of the Royal Observatories of Greenwich and Paris is determined. 7. An account of the observations made during the course of the trigonometrical operation for the determination of terrestrial refraction. 8. Secondary triangles, subdivided into 2 sets, for the improvement of the maps of the country, and the plan of the city of London and its environs. And the conclusion, containing propositions for extending trigonometrical operations over Great Britain.

On several accounts it is not necessary to enter into the particulars of this extremely long and very detailed account of the measurements and calculations of this important and extensive survey; particularly as this, and the former years operations of this kind, have been collected into volumes, and published separately, by Mr. Faden at Charing Cross; and also as it is stated by the R. S. in an appendix to this paper, that very numerous and great errors have been committed in the calculations, &c. so as to render the recomputation and reprinting of many sheets unavoidably necessary. This circumstance is thus stated by Dr. Blagden, one of the secretaries of the R. S. in the appendix, at p. 591.

"Our late much respected colleague, Major-general Roy, having finished, in September 1788, the trigonometrical measurement described in the first part of this volume, returned to London in a very indifferent state of health. From this time he employed all the leisure that his illness, and his various official avocations, allowed, in preparing the account of his operations, to be laid before the R. S. But toward the autumn of 1789 his infirmities increased so much, that the medical gentlemen he consulted advised him to spend the following winter at Lisbon, for which place he accordingly embarked in the beginning of November. Previous to this however he finished the first copy of his paper; but it was much hurried toward the latter part, and not rendered so perfect as the General would undoubtedly have made it with more time and better health. He returned to England in April 1790, and the paper was sent to the press before the end of the same month. Unfortunately the General did not live to see the printing quite completed; he corrected indeed all the sheets except the last 3; but without comparing his manuscript copy with the original papers and observations. Several errors which had been discovered in the course of the printing, together with the obscurity of the account in certain parts, induced some of the General's friends, members of the R. S., to request, after his decease, that the whole might be revised by a competent person, who should compare it with the original documents, correct such mistakes as might be discovered, and illustrate whatever required further explanation. No one could be found so proper for this task as Mr. Dalby, the gentleman of whom the General makes such honourable mention in his paper, and who, having assisted in all the operations, was as well acquainted with every part of them as the General himself. The result of Mr. Dalby's examination is the following remarks; which being much too long for insertion in the list of errata, where only the errors of the press are noticed, is here added separately, by way of appendix. C. BLAGDEN."

This task was accordingly very ably executed by Mr. Dalby, and his corrections printed, amounting to 22 quarto pages of the volume, under the title of Remarks on Major-General Roy's account of the Trigonometrical Operation, &c.

A Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council. p. 271.

A summary of the whole observations in the year 1789.

1789.	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Inches.
	°	°	°	°	°	°	Inches.	Inches.	Inches.	
January	53.0	17.5	35.7	56	36	46	30.75	28.58	29.72	1.345
February ..	51.0	34	42.5	57	49	52.5	30.34	28.65	29.70	1.605
March	46.5	26	36.6	52	44	47.8	30.13	28.94	29.72	1.549
April	62	29	47.2	61	48	54.2	30.18	29.10	29.77	0.957
May	66	45	56.9	65.5	55	60.5	30.27	29.57	29.88	1.103
June	72	50	58.5	66	58.5	61.5	30.23	29.40	29.84	3.244
July	71	52	61.9	65	59	63.5	30.09	29.54	29.85	2.467
August	74.5	54	63.7	69	62	66.2	30.33	29.70	30.06	1.864
September..	74	45	57.3	68	58.5	62.6	30.38	29.30	29.88	2.155
October	59	36	49.1	63	53	57.5	30.29	29.00	29.52	3.253
November ..	55	28.5	41.0	56	48	52.6	30.46	28.72	29.70	1.244
December ..	53	33	43.5	58	48	53.6	30.56	28.88	29.86	1.190
Whole year			49.5			56.5			29.79	21.976

XIII. An Account of the Tabasheer. By Patrick Russell, M.D., F. R. S.* p. 273.

This drug was first introduced to the knowledge of the western world through the works of the Arabian physicians, all of whom mention it as an important article in their Materia Medica; and, from what Dr. R. could observe in Syria, it still continues to be in much more general use in Turkey than in India. To the Arabs and Turks it is known under the name of Tabasheer only; under that name also it is mentioned by the Arabian writers. In this country, [Vizagapatam,] besides that of Tabasheer, which they had from the Persians, it is known under several other names. In the Gentoo language it is called Vedroo-Paloo, bamboo-milk; in the Malabar, Mungel Upoo, salt of bamboo; and in the Warriar, Vedroo Carpooram, bamboo camphor. Don Garzia dall' Horto has long ago exposed a dangerous error, common to the old translators of the Arabian writers, respecting this drug. In the Latin versions of Rhazis and Avicenna, Tabasheer is constantly rendered spodium; and this interpretation has been adopted by most of the subsequent translators of other Arabian medical writers.

The late Mr. Channing, when engaged in the translation of Rhazis on the small-pox, applied to Dr. R. then in Syria, for such information as he might be able to collect on the subject of Tabasheer at Aleppo. Dr. R. accordingly transmitted to him various specimens of the drug, together with several extracts relative to it, from books found in the Aleppo libraries. Some of those specimens differed considerably from these now laid before the R. S.; and from what he has had occasion to observe during his residence in India, he is convinced that much of the drug commonly vended in Turkey is fictitious or adulterated. He thinks the Arabian medical writers generally agree in the Tabasheer being a production of the Indian reed; more especially of such as have suffered from fire, kindled by the friction of the reeds one against the other; an accident supposed to happen frequently in the dry season, among the hills, where the bamboo forms vast and impenetrable thickets. Several of the mountaineers assured him that the bamboo is not the only tree subject to accidental ignition by friction, and named one or two other trees liable to the same accident; but added, they never looked for Tabasheer in the half-burnt fragments of the bamboo, though they doubted not it might sometimes be found there as well as in others. The genuine Tabasheer is doubtless a production of the Arundo Bambos of Linnæus, the Ily of the Hortus Malabaricus, and the Arundo Indica arborea maxima, cortice spinoso, of Herman. It is no less certain that fire is not a necessary

* Brother to Dr. Alexander Russell, (for a biographical notice of whom see Vol. X. p. 667 of these Abridgements,) and formerly physician to the British Factory at Aleppo. Dr. Patrick Russell was author of a Treatise on the Plague, (particularly valuable for the observations on quarantine, and for the regulations recommended to be adopted during the prevalence of a pestilential contagion,) and of a Natural History of Indian Serpents, illustrated by coloured engravings. He died in London in 1805.

agent in its production, whether the conflagrations in the mountains just now mentioned be reckoned fabulous or not. The bamboo in which the Tabasheer is found is vulgarly called the female bamboo, and is distinguished by the largeness of its cavity from the male, employed for spears or lances. They are said to be separate trees.

Of the 7 pieces of bamboo which accompany this paper, 4 are from the mountains in the vicinity of Vellore, and 3 from a place 20 miles from hence. The former were perfectly green on their arrival at Madras; and the others were selected from a large parcel, which were green also when they came to hand. These were all selected on a conjecture of their containing Tabasheer, from a certain rattling perceived on shaking the bamboo, as if small stones were contained in the cavity. This is considered by the natives as an indication of Tabasheer being contained in one or more joints of the bamboo, and they are seldom disappointed; but it does not always follow that there is no Tabasheer where a rattling is not perceptible; for, on splitting a number of reeds, it was sometimes remarked, that where the quantity of the drug was inconsiderable, it was found adhering so closely to the sides of the cavity, as to prevent any rattling from being perceived on shaking. In general however the rule of the natives for choosing the bamboos proved a good one.

In April, one of the bamboos, consisting of 6 joints, received from Vellore, being cautiously split, each joint was examined separately. In 2 of them no vestige of the drug was discovered; each of the others contained some, but in various quantity; the whole collected amounted to about 27 grs. The quality also was various. The particles reckoned of the first quality were of a bluish white colour, resembling small fragments of shells; they were harder than the others, but might easily be crumbled between the fingers into a gritty powder, and when applied to the tongue and palate had a slight saline testaceous taste: they did not exceed in weight 4 grs. The rest were of a cineritious colour, rough on the surface, and more friable; and intermixed with these were some larger, light, spongy particles, somewhat resembling pumice-stones. It is probable that the Arabs, from these appearances of the drug, were led into the opinion already mentioned of its production. The 2 middle joints were of a pure white colour within, and lined with a thin film; it was in these chiefly the Tabasheer was found. The others, particularly the 2 upper joints, were discoloured within, and in some parts of the cavity was found a blackish substance, in grains or in powder, adhering to the sides, the film being there obliterated. In 2 or 3 of the joints, a small round hole was found at top and bottom, which seemed to have been perforated by some insect.

In July, 43 green bamboos, each consisting of 5 or 6 joints, were brought from the hills 50 miles distant from hence. Six, appearing to contain more Tabasheer than the others, were set apart; the remaining 37 were split and examined in the manner before-mentioned. The result was as follows. In 9 out of the 37 there were no vestiges of Tabasheer. In 28 some were found in 1, 2, or 3 joints of each; but never in more than 3 joints of the same bamboo. The quantity varied, but in all was inconsiderable; and the empty joints were sometimes contiguous, sometimes interrupted, indifferently.

The drug consists of very dissimilar particles at first when taken from the bamboo. The whiter, smooth, harder particles, when not loose together with the others in the cavity, were mostly found adhering to the septum that divides the joints, and to the sides contiguous; but never to the sides about the middle of the joints; and it may be remarked that, instead of being chiefly found at the lower extremity of the joint, as might be expected from the juice settling there, they were found adherent indifferently to either extremity, and sometimes to both. In this situation they formed a smooth lining, somewhat resembling polished stucco, which usually was cracked in several places, and might easily be detached with a blunt knife. In some joints the Tabasheer was found thus collected at one or both extremities only, and in such no rattling was perceived on shaking the bamboo; but generally, while some adhered to the extremities of the joint, other detached pieces were intermixed with the coarser loose particles in the cavity. The quantity found in each bamboo was very inconsiderable; the produce of the whole 28 reeds, from 5 to 7 feet long, not much exceeding 2 drams. It is remarked by Garzius, that the Tabasheer is not found in all bamboos, nor in all the branches indiscriminately; but only in those growing about Bisnagur, Batecala, and one part of the Malabar Coast. From the inconsiderable quantity procured from 28 bamboos, it seems very probable that, though not absolutely confined to certain regions, it may be produced in greater abundance in some soils than in others; but that in all regions where the bamboo grows favourably, some proportion of the drug will be found, however it may vary in quality or quantity.

Rumphius on this subject refers to Garzius, candidly acknowledging that he had not himself had opportunities of making particular inquiry. Dr. R. expected answers from Ceylon to some queries sent thither some time ago; and in respect to Bisnagur, had been lately informed in a letter from Hydrabad, from a medical gentleman attending the present embassy to the Nizam, "That though Tabasheer be in great request at Hydrabad, and bears a high price, it is never brought thither from Bisnagur; that some of what is found in the Bazars is brought from the Atcour pass in Canoul, and some from Emnabad at the distance of about 80 miles to the N. W.; but that the greatest part comes from Masulipatam. That there are 2 sorts sold in the Bazars; one at the rate of a rupee a dram; the other, of inferior quality, at half the price; but that this is said to be chiefly composed of burnt teeth and bones. That he was informed by a Persee, who had been in Bengal, that the Tabasheer was produced in great quantities at Sylhet, where it is sold by the pound from 1 rupee to $1\frac{1}{2}$, and formed a considerable article of trade from Bengal to Persia and Arabia."

N° 3 is a specimen of the prime sort from Hydrabad. It differs materially from the others, not only in its superior whiteness, and the being less mixed with impure particles; but in the being much harder than the purest particles of Dr. R.'s specimens, much heavier, and hardly in any degree friable to the finger. Submitting the specimens to examination, he refrains from experiments on them, which may more successfully be made in England, and proceeds to offer a few observations on the juice of the recent bamboo supposed to form the Tabasheer. Rumphius remarks in Amboina, "Juniores arundines plerumque in inferioribus suis nodis semi-repletæ utcunque sunt limpida aqua potabili, quæ hisce in terris sensim evanescit, in aliis vero regionibus exsiccatur in substantiam albam et calceam, quæ Tabaxir vocatur." Garzius gives an account somewhat different from this. "Fra tutti gli intermezzi de' nodi, si genera un certo liquore dolce e grosso, e ridotto in guisa di farina d' amido, e della istessa bianchezza, et alle volte se ne genera assai, alle volte poco, ma non tutte le canne, nè meno tutti i rami generano tale humore...... Questo liquore dopo d' essere appreso, mostra d' essere di color nero, over cinericcio, e non perciò é tenuto per tristo, imperoche questo avvienne, ò perche sia troppo humido, ò perche sia stato lungo tempo nel legno rinchiuso, si come s' hanno pensato alcuni: conciosia che in molti rami, che non sono stati toccati dal fuoco, intravenga, questo."*

The existence of this fluid in the bamboo is known by shaking the joint. In a considerable number of bamboos split in order to procure it, Dr. R. never found water in more than 2 joints, and generally not more than 2 or 3 drs. in each; the largest quantity procured at one time was $1\frac{1}{2}$ oz. Very few joints in proportion contained any. The fluid was always transparent, but varied in consistence; when thicker it had a whiter colour than common; when more dilute it differed little to the eye from common water, or sometimes had a pale greenish cast. Applied to the tongue and palate, it had a slight saline, sub-astringent taste, more or less perceptible in proportion to the consistence of the fluid. After evaporation in the sun, the residuum had a pretty strong saline taste, with less astringency. Some of the fluid, of a darkish colour, thickened in the reed to the consistence of honey; and some, in another joint of the same reed, was perfectly white and almost dry: both had the sharp salt taste, which the Tabasheer itself loses in a great degree by keeping.

From 2 green bamboos, each of 5 joints, which had been cut only a few days before, he procured above 2 oz. of fluid; it had a slight saline taste, and in colour had a greenish cast. One oz. was put into a phial, N° 1, and about

* Capitolo XII.—Orig.

10 drs. into another phial, N° 2; both were stopped with glass stoppers. After 2 days they had both deposited a small sediment; but the sediment in N° 1 was 3 times more than that in the other. At the end of the week, the water in both was found sweet, and the sediment increased, but most in N° 1. At the end of a fortnight, the water in N° 1 had a fetid smell, with a whitish cottony sediment, and a thin film of the same kind suspended at top. The whole, well shaken together, was poured into a glass vessel, and left to evaporate slowly. The residuum consisted of small particles of a whitish brown colour, resembling the inferior sort of Tabasheer. The water in N° 2 had hardly any fetid smell at this time; and at the end of the month remained in the same state: the sediment had increased very little.

The recent green bamboos which, on shaking, appeared to contain water in the cavity, lost this appearance after standing a few days, some sooner, some later. When split, after they no longer gave any sound by shaking, sometimes no fluid was found in the cavity, as if the whole had escaped. The interior thin pellicle however was discoloured, as if by recent moisture; but generally some of the fluid remained in a mucilaginous state, more or less thick, at the lower part of the joint. It may be remarked that small worms were sometimes found in the same joints with the water, which survived several hours, swimming about in the water after its extraction.

In the latter end of Oct. a green bamboo of 5 joints was brought to him, which appeared to contain both water and Tabasheer. After 3 days, the sound of water on shaking the reed, could hardly be perceived; on the 5th day it was entirely imperceptible. On splitting the bamboo, about $\frac{1}{2}$ dr. of the fluid, now thickened into a mucilage, was found at the bottom of the upper joint. The 2d joint contained some perfect Tabasheer loose in the cavity. The 3d joint was empty, excepting a few particles of Tabasheer, which adhered to the sides near the bottom. The 4th joint, at the bottom, contained above 1 dr. of a brownish pulpy substance, adherent. The last joint, in like manner, contained $\frac{1}{2}$ dr. of a substance thicker and harder in consistence, and nearly of the colour of white wax. This specimen exhibited at one view the progress of the Tabasheer through its several stages. The sound distinctly perceived in the first joint on the 23d of Oct. was produced by the water in a fluid state; on the 31st, having become thicker, the sound, on shaking, was very obscure; on the 2d of Nov. no sound was perceptible; and when the reed was split, the water was found reduced to a mucilage. The 4th and 5th joints contained the drug in a more advanced state. In the first, it was thicker than a mucilage of a brownish colour; in the 2d, more of the fluid part having evaporated, the colour was whiter, and it wanted but little of the consistence of the perfect Tabasheer found in the 2d joint.

P. S. Four of the 7 reeds presented to the Society on the night this paper was read, being carefully split, the contents, on comparing them with the specimen sent from India, then on the table, were found to agree in all respects, as well as with the description of the more recent drug given in the above paper. The specimen, N° 3, sent from Hydrabad, and reckoned the prime sort, differed somewhat in hardness, as mentioned above, from the purest particles in the Tabasheer collected by himself; but in the opinion of several of the members present, who compared them, were the same substance with the particles mixed, in a small proportion, in some of the other specimens, as likewise with a few particles taken from the reeds opened in their presence; which puts it beyond doubt, that the substance is produced in the cavity of the bamboo.

XIV. Of the Nardus Indica, or Spikenard. By Gilbert Blane, M.D., F.R.S. p. 284.

Dr. B. received an account, some time ago from his brother in India, of the Spikenard, or Nardus Indica, a name familiar in the works of the ancient physicians, naturalists, and poets: but the identity of which has not hitherto been satisfactorily ascertained. His brother writes, in a letter dated Lucknow, Dec. 1786, that, "travelling with the Nabob Visier, on one of his hunting excursions towards the northern mountains, I was surprized one day, after crossing the river Rapty, about 20 miles from the foot of the hills, to perceive the air perfumed with an aromatic smell; and, on asking the cause, I was told it proceeded from the roots of the grass that were bruised or trodden out of the ground by the feet of the elephants and horses of the Nabob's retinue. The country was wild and uncultivated, and this was the common grass which covered its surface, growing in large tufts close to each other, very rank, and in general from 3 to 4 feet in length. As it was the winter season, there was none of it in flower. Indeed the greatest part of it had been burnt down on the road we went, in order that it might be no impediment to the Nabob's encampments. I collected a quantity of the roots to be dried for use, and carefully dug up some of it, which I sent to be planted in my garden at Lucknow. It there throve exceedingly, and in the rainy season it shot up spikes about 6 feet high. This is accompanied with a drawing of the plant in flower, and of the dried roots, in which the natural appearance is tolerably preserved. It is called by the natives Terankus, which means literally, in the Hindoo language, fever-restrainer, from the virtues they attribute to it in that disease. They infuse about a dram of it in half a pint of hot water, with a small quantity of black pepper. This infusion serves for one dose, and is repeated 3 times a day. It is esteemed a powerful medicine in all kinds of fevers, whether continued or intermittent. I have not made any trial of it myself; but shall certainly take the first opportunity of doing so. The whole plant has a strong aromatic odour; but both the smell and the virtues reside principally in the husky roots, which in chewing have a bitter, warm, pungent taste, accompanied with some degree of that kind of glow in the mouth which cardamoms occasion."

Besides the drawing, a dried specimen has been sent, which was in such good preservation as to enable Sir Joseph Banks, P. R. S., to ascertain it by the botanical characters to be a species of Andropogon, different from any plant that has usually been imported under the name of Nardus, and different from any of that genus hitherto described in botanical systems. There is great reason however to think, that it is the true Nardus Indica of the ancients; for first, the circumstance, in the account above recited, of its being discovered in an unfrequented country from the odour it exhaled by being trodden on by the elephants and horses, corresponds, in a striking manner, with an occurrence related by Arrian, in his History of the Expedition of Alexander the Great into India. It is there mentioned, lib. 6, cap. 22, that during his march through the desarts of Gadrosia, the air was perfumed by the Spikenard, which was trampled under foot by the army; and that the Phœnicians, who accompanied the expedition, collected large quantities of it, as well as of myrrh, in order to carry them to their own country, as articles of merchandize. This last circumstance seems further to ascertain it to have been the true Nardus; for the Phœnicians, who even in war appear to have retained their genius for commerce, could no doubt distinguish the proper quality of this commodity. Dr. B. was informed by Major Rennell, F. R. S., whose accurate researches in Indian geography are so well known to the public, that Gadrosia or Gedrosia answers to the modern Mackran or Kedge-Mackran, a maritime province of Persia, situated between Kerman (the ancient Carmania) and the river Indus, being of course the frontier of Persia towards India; and that it appears from Arrian's account, and from a Turkish map of Persia, that this desart lies in the middle of the tract of country between the river Indus and the Persian Gulph, and within a few days' march of the Arabian or Erythræan sea*. It would appear, that the Nard was found towards the eastern part of it; for Alexander was then directing his route to the westward, and the length of march through the desart afterwards was very great, as they were obliged to kill their beasts of burden in consequence of their subsequent distress. 2dly, Though the accounts of the ancients concerning this plant are obscure and defective, it is evident that it was a plant of the order of gramina; for the term arista, so often applied to it, was appropriated by them to the fructification of grains and grasses, and seems to be a word of Greek original to denote the most excellent portion of these plants, which are the most

* By the Erythræan Sea the ancients meant the northern part of the Æthiopic Ocean, washing the southern coasts of Arabia and Persia, and not, as the name would imply, what is, in modern times, called the Red Sea. The ancient name of the Red Sea was Sinus Arabicus.—Orig.

useful in the vegetable creation for the sustenance of animal life, and Nature has also kindly made them the most abundant in all parts of the habitable earth. The term spica is applied to plants of the natural order verticillatæ, in which there are many species of fragrant plants, and the lavender, which being an indigenous one, affording a grateful perfume, was called Nardus Italica by the Romans; but we never find the term arista applied to these. The poets, as well as the naturalists, constantly apply this latter term to the true Nardus. Statius calls the Spikenard odoratæ aristæ. Ovid, in mentioning it as one of the materials of the Phœnix's nest, calls it Nardi levis arista; and a poem, ascribed to Lactantius, on the same subject, says, his addit teneras Nardi pubentis aristas, where the epithet pubentis seems even to point out that it belonged to the genus andropogon, a name given to it by Linnæus from this circumstance. Galen says, that though there are various sorts of Nardus, the term Ναρδοσταχυς, or Spikenard, should not be applied to any but the Nardus Indica. It would appear that the Nardus Celtica was a plant of a quite different habit, and is supposed to be a species of Valeriana. The description of the Nardus Indica by Pliny does not indeed correspond with the appearance of our specimen; for he says it is frutex radice pingui et crassâ; whereas ours has small fibrous roots. But as Italy is very remote from the native country of this plant, it is reasonable to suppose that others, more easily procurable, used to be subtsituted for it; and the same author says, that there were 9 different plants by which it could be imitated and adulterated. There would be strong temptations to do this from the great demand for it, and the expense and difficulty of distant inland carriage; and as it was much used as a perfume, being brought into Greece and Italy in the form of an unguent manufactured in Laodicea, Tarsus, and other towns of Syria and Asia Minor, it is probable that any grateful aromatic resembling it was allowed to pass for it. It is probable that the Nardus of Pliny, and great part of what is now imported from the Levant, and found under that name in the shops, is a plant growing in the countries on the Euphrates, or in Syria, where the great emporiums of the eastern and western commerce were situated. There is a Nardus Assyria mentioned by Horace; and Dioscorides mentions the Nardus Syriaca, as a species different from the Indica, which certainly was brought from some of the remote parts of India; for both Dioscorides and Galen, by way of fixing more precisely the country whence it comes, call it also Nardus Gangites. 3dly, Garcias ab Horto, a Portuguese, who resided many years at Goa in the 16th century, has given a figure of the roots, or rather the lower parts of the stalks, which corresponds with our specimen; and he says expressly, that there is but this one species of Nardus known in India, either for the consumption of the natives, or for exportation to Persia and Arabia. It is remarkable that he is perhaps the only author who speaks of it in its recent state from his own observation.

It is not to be met with among the many hundreds of plants delineated in the Hortus Malabaricus. The Schœnanthus of Rumphius does not correspond with it, being only one palm in height; but he mentions having seen a dried specimen of it, of which the leaves were almost 5 feet high; and that Mackran was one of the countries whence it was brought. This must be the same as that mentioned by Arrian, but differs from that of Garcias in the length of the stalks; but this might be either because the measure was taken at different seasons of the year, for the specimen before us was much shorter in winter than when it shot into spikes, or because that of Garcias being, according to his own account, cultivated, it might not be so luxuriant as that which grew spontaneous in its native soil. 4thly, The sensible qualities of this are superior to what commonly passes for it in the shops, being possessed both of more fragrancy and pungency, which seems to account for the preference given to it by the ancients.

There is a question concerning which Mathiolus, the commentator of Dioscorides, bestows a good deal of argument, viz. whether the roots or stalks were the parts esteemed for use, the testimony of the ancients themselves on this point being ambiguous. The roots of this specimen are very small, and possess sensible qualities inferior to the rest of the plant; yet it is mentioned in the account above recited, that the virtues reside principally in the husky roots. It is evident, that by the husky roots must here be meant the lower parts of the stalks and leaves where they unite to the roots; and it is probably a slight inaccuracy of this kind that has given occasion to the ambiguity that occurs in the ancient accounts.

With regard to the virtues of this plant, it was highly valued anciently as an article of luxury as well as a medicine. The favourite perfume which was used at the ancient baths and feasts was the unguentum nardinum; and it appears, from a passage in Horace, that it was so valuable, that as much of it as could be contained in a small box of precious stone was considered as a sort of equivalent for a large vessel of wine, and a handsome quota for a guest to contribute at an entertainment, according to the custom of antiquity:

—— Nardo vina merebere
Nardi parvus onyx eliciet cadum.

It may here be remarked, that as its sensible qualities do not depend on a principle so volatile as essential oil, like most other aromatic vegetables, this would be a great recommendation to the ancients, as its virtues would thus be more durable, and they were not acquainted with the method of collecting essential oils, being ignorant of the art of distillation. The fragrance and aromatic warmth of the Nardus depends on a fixed principle like that of cardamoms, ginger, and some other species. Dr. B. tried to extract the virtues of the Nardus by boiling water, by maceration in wine and in proof spirits, but it yielded them but sparingly and with difficulty to all these menstrua.

It had a high character among the ancients as a remedy both external and internal. It is one in the list of ingredients in all the antidotes, from those of Hippocrates, as given on the authority of Myrepsus and Nicolaus Alexandrinus, to the officinals which have kept their ground till modern times under the names of Mithridate and Venice Treacle. It is recommended by Galen and Alexander Trallian in the dropsy and gravel. Celsus and Galen recommended it both externally and internally in pains of the stomach and bowels. The first occasion on which the latter was called to attend Marcus Aurelius was when that Emperor was severely afflicted with an acute complaint in the bowels, answering by the description to what we now call cholera morbus; and the first remedy he applied was warm Oleum nardinum on wool to the stomach. He was so successful in the treatment of this illness, that he ever afterwards enjoyed the highest favour and confidence of the Emperor. It would appear, that the natives of India consider it as an efficacious remedy in fevers, and its sensible qualities promise virtues similar to those of other simples now in use among us in such cases. Besides a strong aromatic flavour, it possesses a pungency to the taste little inferior to the serpentaria, and much more considerable than the contrayerva. It is mentioned in a work attributed to Galen, that a medicine, composed of this and some other aromatics, was found useful in long protracted fevers, which are the cases in which medicines of this class are employed in modern practice. Pl. 8, fig. 1, is a representation of the plant.

XV. On some Extraordinary Effects of Lightning. By Wm. Withering, M.D., F.R.S. p. 293.

This thunder cloud formed in the south, in the afternoon of Sept. 3, 1789, and took its course nearly due north. In its passage it set fire to a field of standing corn; but the rain presently extinguished the fire. Soon afterwards the lightning struck an oak tree, in the Earl of Aylesford's park at Packington. The height of this tree is 39 feet, including its trunk, which is 13 feet. It did not strike the highest bough, but that which projected farthest southward. A man, who had taken shelter against the north side of the tree, was struck dead instantaneously, his clothes set on fire, and the moss (lichen) on the trunk of the tree, where the back of his head had rested, was likewise burnt. Two men, spectators of the accident, ran immediately towards him on seeing him fall; and as it rained hard, and a small lake had collected almost close to the spot, the fire was very soon extinguished; but the effects of the fire on one-half of his body, and on his clothes, were such as to show that the whole burning was instantaneous, not progressive.

Part of the electric matter passed down a walking stick, which the man held in his hand, sloping from him; and where the stick rested on the ground, it made a perforation about $2\frac{1}{2}$ inches in diameter, and 5 inches deep. All observation would probably have ended here, had not Lord Aylesford determined to erect a monument on the spot, not merely to commemorate the event, but with an inscription, to caution the unwary against the danger of sheltering under a tree during a thunder storm. In digging the foundation for this monument, the earth was disturbed at the perforation before mentioned, and the soil appeared to be blackened to the depth of about 10 inches. At this depth, a root of the tree presented itself, which was quite black; but this blackness was only superficial, and did not extend far along it. About 2 inches deeper, some melted quartose matter began to appear, and continued in a sloping direction to the depth of 18 inches. Mr. Watt suggested the idea, that the hollows had been occasioned by the expansion of moisture while the fusion existed.

XVI. Account of a Child with a Double Head. By Everard Home, Esq., F. R. S. p. 296.

It is much to be regretted, that the histories of monstrous appearances in the structure of the human body which are to be found in the works of the older writers, and even of many of the moderns, are so little to be depended on. Few authors have contented themselves with giving a simple detail of facts that were extraordinary; but, from an over anxiety to make them still more wonderful, or from having given an implicit belief to the accounts received from the credulous and ignorant, they have commonly added circumstances too extravagant to deserve the attention of a reasonable mind, which prevent the reader from giving credit to any part of the narration. This has been so general, that whenever the history of any thing uncommon appears, the mind is impressed with a doubt of its authenticity, and requires some stronger evidence of the facts than the single testimony of an individual in other respects unimpeached in his veracity.

As the histories of remarkable deviations from the common course of nature in the formation of the human body, already registered in the Philos. Trans. are very numerous, Mr. H. is desirous of adding to them an account of one so truly uncommon, that no similar instance is to be found on record. It is a species of lusus naturæ so unaccountable, that, though the facts are sufficiently established by the testimonies of the most respectable witnesses, he should still be diffiden- in bringing them before the R. S., were he not enabled at the same time to produce the double skull itself, in which the appearances illustrate so clearly the different parts of the history that it must be rendered perfectly satisfactory to the minds of the most incredulous.

The child was born in May, 1783, of poor parents; the mother was 30 years old, and named Nooki; the father was called Hannai, a farmer at Mandalgent near Bardawan, in Bengal, and aged 35. At the time of the child's birth, the woman who acted as midwife, terrified at the strange appearance of the double head, endeavoured to destroy the infant by throwing it on the fire, where it lay a sufficient time, before it was removed, to have one of the eyes and ears considerably burnt. The body of the child was naturally formed, but the head appeared double, there being, besides the proper head of the child, another of the same size, and to appearance almost equally perfect, attached to its upper part. This upper head was inverted, so that they seemed to be 2 separate heads united together by a firm adhesion between their crowns, but without any indentation at their union, there being a smooth continued surface from the one to the other. The face of the upper head was not over that of the lower, but had an oblique position, the centre of it being immediately above the right eye. When the child was 6 months old, both of the heads were covered with black hair, in nearly the same quantity. At this period the skulls seemed to have been completely ossified, except a small space between the ossa frontis of the upper one, like a fontinel.

No pulsation could be felt in the situation of the temporal arteries; but the superficial veins were very evident. The neck was about 2 inches long, and the upper part of it terminated in a rounded soft tumor, like a small peach. One of the eyes had been considerably hurt by the fire, but the other appeared perfect, having its full quantity of motion; but the eyelids were not thrown into action by any thing suddenly approaching the eye; nor was the iris at those times in the least affected; but, when suddenly exposed to a strong light, it contracted though not so much as it usually does. The eyes did not correspond in their motions with those of the lower head; but appeared often to be open when the child was asleep, and shut when it was awake. The external ears were very imperfect, being only loose folds of skin; and one of them mutilated by having been burnt. There did not appear to be any passage leading into the bone which contains the organ of hearing. The lower jaw was rather smaller than it naturally should be, but was capable of motion. The tongue was small, flat, and adhered firmly to the lower jaw, except for about half an inch at the tip, which was loose. The gums in both jaws had the natural appearance; but no teeth were to be seen either in this head or the other. The internal surfaces of the nose and mouth were lubricated by the natural secretions, a considerable quantity of mucus and saliva being occasionally discharged from them. The muscles of the face were evidently possessed of powers of action, and the whole head had a good deal of sensibility, since violence to the skin produced the distortion expressive of crying, and thrusting the finger into the mouth made it show strong marks of pain. When the mother's nipple was applied to the mouth, the lips attempted to suck. The natural head had nothing uncommon in its appearance; the eyes were attentive to objects, and its mouth sucked the breast vigorously. Its body was emaciated.

The parents of the child were poor, and carried it about the streets of Calcutta as a curiosity to be seen for money; and to prevent its being exposed to the populace, they kept it constantly covered up, which was considered as the cause of its being emaciated and unhealthy. The attention of the curious was naturally attracted by so uncommon a species of deformity; and Mr. Stark, who resided in Bengal during this period, paid particular attention to the appearances of the different parts of the double head, and endeavoured to ascertain the mode in which the 2 skulls were united, as well as to discover the sympathies which existed between the 2 brains. On his return to England, finding that Mr. H. was in possession of the skull, and proposed drawing up an account of the child, he favoured him with the following particulars, and likewise allowed him to have a sketch taken from a very exact painting, made under his own inspection from the child while alive, by Mr. Smith, a portrait painter then in India. From this drawing, which is annexed, and 2 others, representing the heads in the natural state; and the skulls, when all the other parts were removed, a much more accurate idea will be given of the child's appearance than can be conveyed by any description.

At the time Mr. Stark saw the child, it must have been nearly 2 years old,* as it was some months before its death, which it appears happened in the year 1785. At this period the appearances differed in many respects from those taken notice of when only 6 months old. The burnt ear had so much recovered itself as only to have lost about $\frac{1}{4}$ part of the loose pendulous flap. The openings leading from the external ear appeared as distinct as in those of the other head. The skin surrounding the injured eye, which was on the same side with the mutilated ear, was in a slight degree affected, and the external canthus much contracted, but the eye itself was perfect. The eyelids of the superior head were never completely shut, remaining a little open, even when the child was asleep, and the eyeballs moved at random. When the child was roused, the eyes of both heads moved at the same time; but those of the superior head did not appear to be directed to the same object, but wandered in different directions. The tears flowed from the eyes of the superior head almost constantly, but never from the eyes of the other, except when crying. The termination of the upper neck was very irregular, a good deal resembling the cicatrix of an old sore. The superior head seemed to sympathise with the child in most of its natural actions. When the child cried, the features of this head were affected in a similar manner, and the tears flowed plentifully. When it sucked the mother, satisfaction was expressed by the mouth of the superior head, and the saliva flowed more copiously than at any other time; for it always flowed a little from it. When the child

* The dentes molares, or double teeth, which usually appear at 20 months or 2 years of age, were through the gum; and there was no reason to expect them very early in this child.—Orig.

smiled, the features of the superior head sympathised in that action. When the skin of the superior head was pinched, the child seemed to feel little or no pain, at least not in the same proportion as was felt from a similar violence being committed on its own head or body.

When the child was about 2 years old, and in perfect health, the mother went out to fetch some water; and on her return found it dead, from the bite of a cobra de capelo. The parents at this time lived on the grounds of Mr. Dent, the honourable East India Company's agent for salt at Tumloch, and the body was buried near the banks of the Boopnorain river. It was afterwards dug up by Mr. Dent and his European servant, the religious prejudices of the parents not allowing them to dispense with its being interred. The double skull was brought to Europe by Capt. Buchanan, late commander of the Ranger packet, in the service of the honourable the East India Company, and deposited by Mr. Home in Mr. John Hunter's curious collection.

The 2 skulls which compose this monstrous head appear to be nearly of the same size, and equally complete in their ossification, except a small space at the upper edge of the ossa frontis of the superior skull, similar to a fontinel. The mode in which the 2 are united is curious, as no portion of bone is either added or diminished for that purpose; but the frontal and parietal bones of each skull, instead of being bent inwards, so as to form the top of the head, are continued on; and, from the oblique position of the 2 heads, the bones of the one pass a little way into the natural sutures of the other, forming a zig-zag line, or circular suture uniting them together. The 2 skulls appear to be almost equally perfect at their union; but the superior skull, as it recedes from the other, is becoming more imperfect and deficient in many of its parts. The meatus auditorius in the temporal bone is altogether wanting. The basis of the skull is imperfect in several respects, particularly in such parts as are to connect the skull with a body. The foramen magnum occipitale is a small irregular hole, very insufficient to give passage to a medulla spinalis; round its margin are no condyles with articulating surfaces, as there were no vertebræ of the neck to be attached to it. The foramen lacerum in basi cranii is only to be seen on one side, and even there too small for the jugular vein to have passed through. The ossa palati are deficient at their posterior part; the lower jaw is too small for the upper, and the condyle and coronoid process of one side are wholly wanting. In most of the other respects, the 2 skulls are alike; the number of teeth in both is the same, viz. 16.

From an examination of the internal structure of the double skull, the 2 brains have certainly been inclosed in 1 bony case, there being no septum of bone between them. How far they were entirely distinct, and surrounded by their proper membranes, cannot now be ascertained; but from the sympathies which were noticed by Mr. Stark between the 2 heads, more particularly those of the superior with the lower, or more perfect, Mr. H. believes that there was a more intimate connection between them than simply by means of nerves, and therefore that the substance of the brains was continued into each other. Had the child lived to a more advanced age, and given men of observation opportunities of attending to the effects of this double brain, its influence on the intellectual principle must have afforded a curious and useful source of inquiry; but unfortunately the child only lived long enough to complete the ossification of the skull so as to retain its shape, by which means we have been enabled to ascertain and register the fact, without having enjoyed the satisfaction that would have resulted from an examination of the brain itself, and a more mature investigation of the effects it would have produced.

In pl. 8, fig. 2, the child is represented as it appeared at the age of 20 months, and is copied from a picture in the possession of Mr. Stark. The painting was taken from the child 6 months before its death by Mr. Smith, an ingenious artist, at that time residing in Bengal. It conveys a general idea of the appearance of this extraordinary child, and the relative proportions between the double head and the body. In fig. 3, the double head is represented of a larger size. One of the eyes of the upper face appears smaller or more contracted than the other; this is in consequence of the injury it received when the child was thrown upon the fire. The superficial veins on the forehead of the upper head are very distinctly seen. Fig. 4 is an exact representation of the double skull, which is in Mr. Hunter's collection, upon the same scale. It shows the curious manner in which the 2 skulls are united together, and the number of teeth formed before the child's death; which circumstance ascertains, with tolerable accuracy, its age.

XVII. On the Analysis of a Mineral Substance from New South Wales. By Josiah Wedgwood, Esq., F.R.S., and A.S.* p. 306.

This mineral is a mixture of fine white sand, a soft white earth, some colourless micaceous particles, and a few black ones resembling black mica or black-lead; partly loose or detached from each other, and partly cohering together in little friable lumps. None of these substances seem to be at all acted on by the nitrous acid, concentrated or diluted; nor by oil of vitriol diluted with about equal its measure of water; in the cold, or in a boiling heat; the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled on the mineral to dryness, as in the process of making alum from clay, produced no apparent change in it; but a lixivium made from this dry mass with water, on being saturated with alkali, became somewhat turbid, and deposited, exceeding slowly, a white earth in a gelatinous state, too

* From Sydney Cove, New South Wales. Along with the mineral here analyzed, Mr. W. was presented by Sir Joseph Banks with some clay from Sydney Cove, which Mr. W. found to be an excellent material for pottery, adding that it might certainly be made the basis of a valuable manufacture for our infant colony there.

small in quantity for any particular examination; but which, from its aspect, from the manner in which it was obtained, and from the taste of the lixivium before the addition of the alkali, was judged to be the aluminous earth. The marine acid, during digestion, seemed to have as little action as the other 2; but on pouring in some water, with a view only to dilute and wash out the remaining part of the acid, a remarkable difference presented itself; the liquor became instantly white as milk, with a fine white curdly substance intermixed; the strong acid having extracted something which the simple dilution with water precipitated.

The white matter being washed off, more spirit of salt was added to the remainder, and the digestion repeated, with a long tube inserted into the mouth of the glass, so as nearly to prevent evaporation. The acid, when cold and settled fine, was poured off clear; and on diluting it with water, the same milky appearance was produced as at first. The digestion was repeated several times successively, with fresh quantities of the acid, till no milkiness appeared on dilution. The quantity of mineral employed was 24 grs.; and the residuum, after the operations, washed and dried, weighed somewhat more than 19 grains; so that about $\frac{1}{5}$ of it had been dissolved. In some parcels of the mineral, taken up promiscuously, the proportion of soluble matter was much less, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act on: the white sand, much of the white soft earth, and all the black particles, remain unaltered.

To try whether this tedious process of solution could be expedited by trituration or calcination, some of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconsiderable proportion to the rest, yet the whiteness of the other was soon covered and suppressed by it, the whole becoming a uniformly black, shining, soft, unctuous mass, like black-lead rubbed in the same manner; with a few gritty particles perceptible on pressing hard with the pestle. A penny-weight of this mixture, spread thin on the bottom of a porcelain vessel, was calcined about an hour, with a fire between 30 and 40 degrees;* it became of a uniform, dull, white, or grey colour, excepting a very few, and very small, sparkling, black particles, suspected to be those which had eluded the action of the pestle; it lost in weight 6 grs. or a 4th part. The mineral, thus ground and calcined, was found to be just as difficult of solution as in its crude state; with this additional disadvantage, that the undissolved fine particles are indisposed to settle from the liquor.

* By degrees of fire, or of heat above ignition, I mean those of my thermometer; and some idea may be formed of their value, by recollecting, that they commence at visible redness; and that the extreme heat of a good air-furnace, of the common construction, is 160°, or a little more.—Orig.

In all the experiments of dissolution, as often as the heat was at or near the boiling point of the acid, frequent and pretty singular bursts or explosions happened, though the matter lay very thin in a broad-bottomed glass. They were sometimes so considerable as to throw off a porcelain cup with which the glass was covered, and once to shatter the glass in pieces. In a heat a little below this, the extraction seemed to be equally complete, though more slow; but a heat a little below that in which wax melts, or below 140° of Fahrenheit's thermometer, appeared insufficient.

To determine the degree of dilution necessary for the precipitation of the dissolved substance, and whether the precipitation by water be total, a measure of the solution was poured into a large glass, and the same measure of water added repeatedly. The 3d addition of water occasioned a slight milkiness, which increased more and more to the 6th. The liquor being then filtered off, another measure of water produced a little fresh milkiness, and an 8th rather increased it; a 9th and a 10th had no effect. The liquor being now again passed through a filter, solution of salt of tartar did not in the least alter its transparency; so that, after the solution has been diluted with 8 or 9 times its measure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the solution is necessarily prepared, it cannot but contain a great redundance of acid; for the small quantity of acid, sufficient for holding the soluble part suspended, would be soaked up or entangled by the undissolved part, so as scarcely to admit of any being poured off; and it cannot be diluted, or washed out, but by the strong acid itself. The solution with which the above experiment was made was reckoned to have only about 6 grs. of the soluble matter to 3 oz. of spirit of salt, having been prepared by digesting that quantity of the spirit by half an ounce at a time on 30 grs. of the crude mineral. A saturated solution was obtained by digesting, in a small portion of the solutions thus prepared, the precipitate thrown down by water from the larger portions, till the acid would take up no more. A solution thus saturated cannot bear the smallest quantity of water, a single drop, on the first contact, producing a milky circle round it.

This substance, washed and dried, is indissoluble in water, as indeed might be expected from the manner of its preparation. Nor is it acted on by the nitrous or vitriolic acids, concentrated or diluted, cold or hot; nor by alkaline solutions, mild or caustic, of the volatile or fixed kind. It is dissolved by strong marine acid, but not without the assistance of nearly the same degree of heat that is necessary for its extraction from the mineral. From this solution it is precipitated by water; and, after repeated dissolutions and precipitations, it appears to have suffered no decomposition or change.

Spirit of nitre, added to the saturated solution, makes no precipitation; and if the quantity of nitrous acid exceeds, or at least does not fall much short of, that of marine acid in the solution, the mixture suffers no precipitation from water. Nor does any precipitation happen, though the nitrous spirit be previously mixed with even a large quantity of water; provided the quantity of solution added to it does not exceed that of the nitrous spirit in the mixture. The appropriate menstruum for this substance, that is for keeping it in a state of dilute solution, appears therefore to be aqua regia; and the due proportions of the two acids, of any given strength, might be determined, if necessary, with greater accuracy and facility for this than perhaps for any other body; because, if there be even a very minute surplus of marine acid in the solution, that surplus will instantly betray itself on dropping a little into water, all that was dissolved by it, and no more, being precipitated by the water. It may be observed however, that where an addition of nitrous acid is used, a saturated solution cannot be obtained, unless by subsequent evaporation, the same quantity of marine acid being necessary with as without that addition: the change, or modification, which the nitrous acid produces in the marine, serves, in the present instance, not for effecting the solution, as in the case of gold and some other metals, but merely for enabling it to bear water without depositing its contents.

Oil of vitriol, dropped into the saturated marine solution, occasions no change till its quantity comes to be about equal to that of the solution; a considerable effervescence and heat are then produced, the liquor becomes milky, and the marine acid is extricated in its usual white fumes. The mixture, heated nearly to boiling, becomes transparent, and afterwards continues so in the cold. This vitriolic solution is precipitated by water, and the precipitate is re-dissolved by marine acid. The saturated marine solution is indisposed to crystallize. By continued evaporation in gentle heat, it becomes thick and butyraceous, and in this state it soon liquefies again on exposure to the air. The butyraceous mass, in colour whitish or pale yellow, is not corrosive, like the similar preparations made from some metallic bodies; nor is it more pungent in taste, but rather less so than the combination of the same acid with calcareous earth. In a heat increased nearly to ignition, the acid is disengaged, and rises in white fumes, which, received in a cold phial. condense into colourless drops, without any appearance of sublimate. From the remaining white mass, spirit of nitre extracts so little as to exhibit only a slight milkiness on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is dissoluble by the nitrous.

The substance in question is not precipitated by Prussian lixivium. A drop or two of the lixivium do indeed occasion a little white or bluish-white precipitation in the saturated marine solution; but in the more dilute no turbidness appears, till the quantity of lixivium is such as to produce that effect by its mere water;

and when the precipitate has at length been formed, it re-dissolves in marine acid as easily as that made by water; whereas the precipitates resulting from the union of the Prussian matter are not acted on by acids, till that matter has been extracted from them by an alkali. For further satisfaction in this important point, the experiment was repeated with a solution in aqua regia. Here the Prussian lixivium, in whatever quantity it was added, occasioned no precipitation at all, only the usual bluishness arising from the iron always found in the common acids; and pure alkali, added afterwards, precipitated the original white substance unchanged.

The following experiments of precipitation by alkalis were made with the marine solution, before the effect of an addition of nitrous acid had been discovered; and they were made with so much care and attention, that it was not thought necessary to repeat them afterwards. To obviate, as much as possible, the equivocal results that might arise from water contained in the precipitants, the different alkalis were applied in the dryest state they could be reduced to; viz. pure salt of tartar, kept for some time in a heat just below redness; crystals of marine alkali, melted and dried in the same manner; volatile alkali in crystals, a little surplus acid being, in this instance, previously added to the solution, to counteract the water of crystallization in the alkali; salt of tartar causticated by quick-lime, and hastily evaporated to dryness; the marine alkali causticated in like manner; and the vapour of caustic volatile alkali arising, with a very gentle heat, from a retort into a phial containing the solution. All these alkalis occasioned copious precipitations. All the precipitates, after washing and drying, were found to re-dissolve in marine acid; and from all these solutions the original substance was precipitated, unaltered, on diluting them with water.

In strong fire, from 142 to 156°, this substance discovers a much greater fusibility than any of the known simple earths. In a small vessel, made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, pressed smooth in the manner of a cupel, it did the same. Magnesia, or chalk, would indeed vitrify in the clay vessel; but on flint, no one of the known earths shows any tendency to vitrification in that heat.* In a cavity, scooped in a lump of chalk, this substance, in the heat above mentioned, ran

* It may be proper just to mention, that I find this to be a very commodious and sure method of trying, in small, whether any given earthy body be fusible with other earths. If the body is disposed to vitrify with any proportion of clay or flint, for instance, it will equally vitrify when a little of it is applied, or even dusted only, on the bottom of a small cup made of clay, or on a smooth close bed of finely powdered flint. The body, in this mode of application, seems to unite with only just so much of the matter of the substratum as is requisite for their most perfect fusion together, and has nothing else in contact with it, so that no deception can arise; whereas, if mixed with the same matter, there might be no appearance of fusion, unless certain favourable proportions of the two should chance to be hit upon; and even then, if the quantity be small, it would not be certain but that the fusion might have originated from the matter of the crucible.—Orig.

into a small round bead, smooth, whitish, and opaque, not in the least adhering to the calcareous mass. On a bed of powdered quick-lime it formed a brownish scoria, which in great part had sunk into the lime, and seemed to have united with it. On Mr. Henry's magnesia, uncalcined, it melted and sunk in completely, leaving only a slight brownish stain on the surface where it had lain. On beds of the baroselenite and barytic quick-lime, it likewise melted and sunk in, leaving a discoloured spot behind; but whether it really united with the substrata, or only penetrated into their interstices, could not be determined with certainty, on account of the smallness of the quantity of the mineral he had to work on. On a bed of powdered charcoal, in a crucible closely luted, this substance likewise melted; and therefore it may be presumed not to have owed its fusion, in the above experiments, to the same cause to which some of the common simple earths, in certain circumstances, owe theirs, namely, their union with the matter of the vessel or support, that is, with an earth or earths of a different kind from themselves; but to possess a fusibility strictly its own, which takes place in a fire of 150°, or perhaps less.

As charcoal in fine powder assumes a kind of fluidity in the fire, similar to that which powdered gypsum exhibits in a small heat, its surface had changed from concave to horizontal, and the bead had sunk to the bottom; it was rough and black on the outside, and whitish within. On repeating the experiment in a cavity scooped in a piece of charcoal, the result was a blackish bead like the former, only smooth on the outside, with something of metallic brightness, not unlike that of black-lead. Both beads were very light, and had a considerable cavity within. All the internal part was whitish, without the least metallic aspect; and the external glossy blackness appeared to be only the stain which charcoal powder communicates, in strong fire, to some earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid a part of the beads was dissolved, precipitable as at first by water; but an accident prevented the process from being continued sufficiently to determine whether the whole could be dissolved or not.

By this fusibility in the fire; solubility in one only of the common mineral acids, and parting with the acid in a heat below ignition; precipitability by water, and non-precipitability by Prussian lixivium; this substance is strongly discriminated from the known earths and metallic calces. And as it suffers no decomposition from any of the alkalis, in any of the usual modes of application, it cannot be considered as a combination of any of those earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal disengaged from the acid. Whether this substance belongs to the earthy or metallic class, he cannot absolutely determine; but is inclined to refer it to the earthy; because, though brought into perfect fusion, in contact with inflammable matter, and in close vessels, it does not assume the appearance which metallic bodies do in that circumstance.

The black particles, which bore but a very small proportion to the other matter, were in form of shining black scales, very thin, and very light. One grain weight of them, carefully picked out, exposed to a fire which was gradually raised to about 90°, and continued in all about 40 hours, in a vessel loosely covered, was almost wholly dissipated, and what little remained was perfectly white. Marine acid had no effect on it. Fifteen grains of the entire mineral lost, in the same fire, 3 grains. After separating from another portion of the mineral, by washing and otherwise, a considerable quantity of the white matter, 15 grs. of the remainder, continuing of course more than its due proportion of the black, lost 5 grains; so that it seems principally to be the substance on which the blackness depends that is destroyed or dissipated by fire. The same quantity, 15 grains, of common black-lead lost in the same fire above 14 grs. the residuum weighing less than 1 grain. Though no conclusion can be drawn from these experiments respecting the comparative loss of black-lead and the pure black matter of this mineral, on account of the heterogeneous parts intermixed with the latter, the colour of the residua seems to afford a sufficient discrimination between them; that of black-lead being dark reddish brown, but the others purely and uniformly white.

As this substance could not now be supposed to be either iron mica, or the common kind of black-lead, suspicion fell on molybdæna. Mr. W. had not, at that time, had an opportunity of procuring a specimen of molybdæna to compare it with; but from the singular and strongly-marked properties of the molybdænic acid, discovered by Scheele, it was judged, that a very small quantity of it, when disengaged from the sulphur with which it is naturally combined, would easily be distinguishable. Hjelm's process for disengaging the sulphur, by repeatedly burning linseed oil on the molybdæna in a crucible, and afterwards abstracting successive quantities of the same oil from it in a retort, was tried on a portion of the Sydney-Cove mineral, from which much of the white matter had been separated as above-mentioned. The black coal, remaining in the retort, became yellow by calcination, as that of molybdæna should do; but in this yellow powder, no vestige of molybdænic acid could be discovered.

Another quantity of the mineral was submitted to Scheele's own process, viz. repeated abstractions of diluted nitrous acid; but instead of becoming whiter every time, and at length white as chalk, which molybdæna should do, the blackness of this matter continued unaltered to the last. There is one circumstance in Mr. Scheele's experiments, which, though omitted by those who have given abstracts of them, may deserve, on the present occasion, to be more particularly noticed. He reduced the molybdæna into fine powder, and poured on it concentrated nitrous acid: "the mixture," he says, "was hardly lukewarm in the retort, when it passed all together into the recipient with great heat;" and it was for this reason that he afterwards used diluted acid. Presuming that this violent action of the concentrated nitrous acid might afford a decisive criterion of molybdæna, Mr. W. had the black residuum, after 5 or 6 abstractions of the diluted acid, ground fine on a levigating glass, and returned into the retort, with 6 times its weight of smoking spirit of nitre. The heat was increased cautiously far beyond lukewarm, but no commotion could be perceived, except the explosions already mentioned, which always took place when the mixture was near boiling. The distillation was continued to dryness, and repeated 5 times with smoking acid; but the mineral remained just as black as it was at first.

Now, as Scheele's molybdæna is slowly decomposed by the diluted nitrous acid, and rapidly acted on by the concentrated acid, while the black part of this mineral obstinately resists both, we cannot hesitate to conclude, that this black substance is not Scheele's molybdæna. There are some other circumstances which confirm this conclusion, though taken singly they would not perhaps be of much weight, considering the great proportion of other matter here mixed with the black. The principal of these circumstances are, that it yields no flowers before a blow-pipe, and that its particles seem to have no flexibility or elasticity, the only difficulty of reducing it into fine powder arising from a property of another kind, unctuosity. The difference, above noticed, between this black matter and common black-lead, consists only in the former leaving on calcination a white substance, seemingly siliceous, and the latter a brown ferrugineous one. In their aspect, unctuosity, resistance to acids, and the volatility (in open fire) of that part in which the blackness consists, they perfectly agree; and they appear to agree also in the nature or constitution of this volatile part; for the Sydney-Cove mineral, as well as black-lead, deflagrates and effervesces very strongly with nitre, produces an hepatic impregnation on fusion with vitriolated alkali, but none with pure alkali, and is manifestly rich in inflammable matter, without sulphur.

It seems therefore, that this substance is a pure species of plumbago, or black lead, not taken notice of by any writer. Fourcroy, in the last edition of his Chemistry, considers iron as an essential component part of black-lead, to which accordingly he gives a new name, expressive of that metal, carbure de fer. Lavoisier, in his Elements of Chemistry, lately published, mentions a carbure of zinc also, and says that both these carbures are called plumbago, or black-lead. The quantity of mineral Mr. W. had been furnished with was too far exhausted, before he met with this observation, to admit of any further experiments, for determining the presence of zinc in it; but those already stated, with the recollection of some circumstances attending them, persuade him, that that metallic body has no share in its composition. Neither before the blow-pipe, nor in calcination, was there any appearance of the peculiar flame, or flowers, by which zinc is so strongly characterized: if any such appearance had taken place, it could not have escaped notice, as some of the calcinations were particularly attended to during the process, though with a different view, the discovery of sulphur or arsenic. The white matter which remains after the calcination is certainly not calx of zinc, for it was not acted on by spirit of salt, cold or hot, while the calces of zinc are dissolved rapidly by that acid, even in the cold.

XVIII. Report on the Best Method of Proportioning the Excise on Spirituous Liquors. By Charles Blagden, M. D., Sec. R. S., and F. A. S. p. 321.

In consequence of an application from government to the president, Sir Joseph Banks, for the best means of ascertaining the just proportion of duty to be paid by any kind of spirituous liquor that should come before the officers of excise, Dr. B. was requested by that gentleman to assist in planning the proper experiments for this purpose, and to draw up the report on them when they should be finished.

Though various indications of the strength of spirituous liquors have been devised, applicable in a gross manner to general use, it is well known that no method admits of real accuracy but that of the specific gravity. The weights of an equal bulk of water and pure spirit differ from one another by at least a 6th part of the weight of the former; whence it is obvious, that when those two fluids are mixed together, the compound must have some intermediate specific gravity, approaching nearer to that of water or pure spirit, as the former or the latter is the more predominant ingredient. Were it not for a certain effect attending the mixture of water and spirits, which has been called their mutual penetration, the specific gravity of these compositions, in a given degree of heat, would be simply in the arithmetical proportion of the quantity of each of the fluids entering into them. But whenever different substances, which have a strong tendency to unite together, are mixed, the resulting compound is found to occupy less space than the substances forming it held in their separate state; therefore the specific gravity of such compounds is always greater than would be given by a simple calculation from the volume of their ingredients. Though it be a general fact, that such a decrease of bulk takes place on the mixture of substances which have a chemical attraction for each other, yet the quantity of this diminution is different in them all, and, under our present ignorance of the intimate composition of bodies, can be determined by experiment only. To ascertain therefore the quantity and law of the condensation, resulting from this mutual penetration of water and spirit, was the first object to which the following experiments were directed.

All bodies in general expand by heat; but the quantity of this expansion, as well as the law of its progression, are probably not the same in any 2 substances. In water and spirit they are remarkably different. The whole expansion of pure spirit from 30° to 100° of Fahrenheit's thermometer, is not less than $\frac{1}{25}$th of its whole bulk at 30°; whereas that of water, in the same interval, is only $\frac{1}{145}$th of its bulk. The laws of their expansion are still more different than the quantities. If the expansion of quicksilver be, as usual, taken for the standard, the expansion of spirit is indeed progressively increasing with respect to that standard, but not much so within the above-mentioned interval; while water kept from freezing to 30°, which may easily be done, will absolutely contract as it is heated for 10° or more, that is, to 40° or 42° of the thermometer, and will then begin to expand as its heat is augmented, at first slowly, and afterwards gradually more rapidly, so as to observe on the whole a very increasing progression. Now mixtures of these 2 substances will, as may be supposed, approach to the less or the greater of those progressions, according as they are compounded of more spirit or more water, while their total expansion will be greater, according as more spirit enters into their composition; but the exact quantity of the expansion, as well as law of the progression, in all of them, can be determined only by trials. These were therefore the 2 other principal objects to be ascertained by experiment.

The first step towards a right performance of the experiments, was to procure the two substances, with which they were to be made, as pure as possible. Distilled water is in all cases so nearly alike, that no difficulty occurred with regard to it; but the specific gravity of pure spirit, or alcohol, has been given so very differently by the authors who have treated of it, that a particular set of experiments appeared necessary for determining to what degree of strength rectified spirits could conveniently be brought. The person engaged to make these experiments was Dr. Dollfuss, an ingenious Swiss gentleman then in London, who had distinguished himself by several publications on chemical subjects. Dr. Dollfuss, having been furnished by government with spirit for the purpose, rectified it by repeated and slow distillations, till its specific gravity became stationary in this manner of operating: he then added dry caustic alkali to it, let it stand for a few days, poured off the liquor, and distilled it with a small addition of burnt alum, placing the receiver in ice. By this method he obtained a spirit whose specific gravity was .8188 at 60° of heat. Perceiving however that he could not conveniently get the quantity of spirit he wanted lighter than .82527 at 60°, he fixed on that strength as a standard, to which he found the above-mentioned lighter spirit could be reduced by adding to it a $\frac{27}{1000}$th part of water; and with this spirit and distilled water he made a series of experiments for determining the specific gravity of different mixtures of these fluids in different degrees of heat.

The process followed by Dr. Dollfus is not here given as the best possible for obtaining pure spirit; nor was the result of it in fact the lightest alcohol that has been procured. Some spirit has been tried since that time, whose specific gravity was .813 at 60°. This was furnished by Dr. George Fordyce, F. R. S., who succeeded in bringing it to that strength chiefly by adding the alkali very hot. Care must be taken that none of the caustic alkali comes over in the distillation. Some alcohol was also sent, for trial, by Mr. Lewis, an eminent distiller in Holborn, whose specific gravity, at the same temperature, was .814.

It was with spirit rectified from malt-spirits that Dr. Dollfuss's series of experiments was made; but he tried several comparative experiments with such as had been rectified from rum and brandy, and found no other difference than might fairly be ascribed to unavoidable errors. On examining the results of Dr. Dollfuss's experiments it was perceived, that though the numbers agreed together tolerably well on the whole, yet in some places there was that degree of irregularity in the first differences, as made it advisable to repeat several of the experiments; and Dr. Dollfus leaving England about that time, the business of this repetition was intrusted to Mr. Gilpin, clerk of the R. S. This gentleman had already taken a part in the business, by assisting Dr. Dollfuss in the former experiments, particularly in the very nice part of weighing the mixtures; and his great skill, accuracy, and patience, in conducting experiments, as well as in computations, had on other occasions been proved to many members of the society. One experiment leading on to another, Mr. Gilpin was at length induced to go through the whole series anew; and as the deductions in this report will be taken chiefly from this last set of experiments, it is proper here to describe minutely the method observed by Mr. Gilpin in his operation. This naturally resolves itself into 2 parts, the way of making the mixtures, and the way of ascertaining their specific gravity.

1. The mixtures were made by weight, as the only accurate method of fixing the proportions. In fluids of such very unequal expansions by heat as water and alcohol, if measures had been employed, increasing or decreasing in regular proportions to each other, the proportions of the masses would have been sensibly irregular; now the latter was the object in view, namely, to determine the real quantity of spirit in any given mixture, abstracting the consideration of its temperature. Besides, if the proportions had been taken by measure, a different mixture should have been made at every different degree of heat. But the principal consideration was, that with a very nice balance, such as was employed on this occasion, quantities can be determined to much greater exactness by weight, than by any practicable way of measurement. The proportions were therefore always taken by weight. A phial being provided of such a size as that it should be nearly full with the mixture, was made perfectly clean and dry, and being counterpoised, as much of the pure spirit as appeared necessary was poured into it. The weight of this spirit was then ascertained, and the weight of distilled water, required to make a mixture of the intended proportions, was calculated. This quantity of water was then added, with all the necessary care, the last

portions being put in by means of a well known instrument, which is composed of a small dish terminating in a tube drawn to a fine point: the top of the dish being covered with the thumb, the liquor in it is prevented from running out through the tube by the pressure of the atmosphere, but instantly begins to issue by drops, or a very small stream, on raising the thumb. Water being thus introduced into the phial, till it exactly counterpoised the weight, which, having been previously computed, was put into the opposite scale, the phial was shaken, and then well stopped with its glass stopple, over which leather was tied very tight, to prevent evaporation. No mixture was used till it had remained in the phial at least a month, for the full penetration to have taken place; and it was always well shaken before it was poured out to have its specific gravity tried.

2. There are 2 common methods of taking the specific gravity of fluids; one by finding the weight which a solid body loses by being immersed in them; the other by filling a convenient vessel with them, and ascertaining the increase of weight it acquires. In both cases a standard must have been previously taken, which is usually distilled water; namely, in the first method by finding the weight lost by the solid body in the water, and in the 2d method, the weight of the vessel filled with water. The latter was preferred for the following reasons. When a ball of glass, which is the properest kind of solid body, is weighed in any spirituous or watery fluid, the adhesion of the fluid occasions some inaccuracy, and renders the balance comparatively sluggish. To what degree this effect proceeds is uncertain; but from some experiments made by Mr. Gilpin, with that view, it appears to be very sensible. Also, in this method a large surface must be exposed to the air during the operation of weighing, which, especially in the higher temperatures, would give occasion to such an evaporation as to alter essentially the strength of the mixture. It seemed also, as if the temperature of the fluid under trial could be determined more exactly in the method of filling a vessel, than in the other: for the fluid cannot well be stirred while the ball to be weighed remains immersed in it; and as some time must necessarily be spent in the weighing, the change of heat which takes place during that period will be unequal through the mass, and may occasion a sensible error. It is true, on the other hand, that in the method of filling a vessel, the temperature could not be ascertained with the utmost precision, because the neck of the vessel employed, containing about 10 grains, was filled up to the mark with spirit not exactly of the same temperature, as will be explained presently; but this error, it is supposed, would by no means equal the other, and the utmost quantity of it may be estimated very nearly. Finally, it was much easier to bring the fluid to any given temperature when it was in a vessel to be weighed, than when it was to have a solid body weighed in it; because in the former case the quantity was smaller, and the vessel containing it more manageable, being readily heated with the hand or warm water, and cooled with cold water: the very circumstance, that

so much of the fluid was not required, proved a material convenience. The particular disadvantage in the method of weighing in a vessel, is the difficulty of filling it with extreme accuracy; but when the vessel is judiciously and neatly marked, the error of filling will, with due care, be exceedingly minute. By several repetitions of the same experiments, Mr. Gilpin seemed to bring it within the $\frac{1}{15000}$th part of the whole weight. The above-mentioned considerations induced the gentlemen employed in the experiments to give the preference to weighing the fluid itself; and that was accordingly the method practised both by Dr. Dollfuss and Mr. Gilpin in their operations.

The vessel chosen, as most convenient for the purpose, was a hollow glass ball, terminating in a neck of a small bore. That which Dr. Dollfuss used, held 5800 grains of distilled water; but, as the balance was so extremely accurate, it was thought expedient, on Mr. Gilpin's repetition of the experiments, to use one of only 2965 grains capacity, as admitting the heat of any fluid contained in it to be more nicely determined. The ball of this vessel, which may be called the weighing-bottle, measured about 2.8 inches in diameter, and was spherical, except a slight flattening on the part opposite to the neck, which served as a bottom for it to stand on. Its neck was formed of a portion of a barometer tube, .25 of an inch bore, and about $1\frac{1}{4}$ inch long; it was perfectly cylindrical, and on its outside, very near the middle of its length, a fine circle or ring was cut round it with a diamond, as the mark to which it was to be filled with the liquor. This mark was made by fixing the bottle in a lathe, and turning it round with great care, in contact with the diamond. The glass of this bottle was not very thick; it weighed 916 grains, and with its silver cap 936.

When the specific gravity of any liquor was to be taken by means of this bottle, the liquor was first brought nearly to the required temperature, and then the bottle was filled with it up to the beginning of the neck only, that there might be room for shaking it. A very fine and sensible thermometer was then passed through the neck of the bottle into the contained liquor, which showed whether it was above or below the intended temperature. In the former case the bottle was brought into colder air, or even plunged for a moment in cold water; the thermometer in the mean time being frequently put into the contained liquor, till it was found to sink to the right point. In like manner, when the liquor was too cold, the bottle was brought into warmer air, immersed in warm water, or more commonly held between the hands, till on repeated trials with the thermometer the just temperature was found. It will be understood, that during the course of this heating or cooling, the bottle was very frequently shaken between each immersion of the thermometer; and the top of the neck was kept covered, either with the finger, or a silver cap made on purpose, as constantly as possible. Hot water was used to raise the temperature only in heats of 80° and upwards, inferior heats being obtained by applying the hands to the bottle; when the hot water was employed, the ball of the bottle was plunged into it, and again quickly lifted out, with the necessary shaking interposed, as often as was necessary for communicating the required heat to the liquor; but care was taken to wipe the bottle dry after each immersion, before it was shaken, lest any adhering moisture might by accident get into it. The liquor having by these means been brought to the desired temperature, the next operation was to fill up the bottle exactly to the mark on the neck, which was done with some of the same liquor, by means of a glass funnel with a very small bore. Mr. Gilpin endeavoured to get that portion of the liquor, which was employed for this purpose, pretty nearly to the temperature of the liquor contained in the bottle; but as the whole quantity to be added never exceeded 10 grains, a difference of 10° in the heat of that small quantity, which is more than it ever amounted to, would have occasioned an error of only $\frac{1}{30}$ of a degree in the temperature of the mass. Enough of the liquor was put in, to fill the neck rather above the mark, and the superfluous quantity was then absorbed to great nicety, by bringing into contact with it the fine point of a small roll of blotting paper. As the surface of the liquor in the neck would be always concave, the bottom or centre of this concavity was the part made to coincide with the mark round the glass: and in viewing it care was taken, that the near and opposite sides of the mark should appear exactly in the same line, by which means all parallax was avoided. A silver cap, which fitted tight, was then put upon the neck, to prevent evaporation; and the whole apparatus was in that state laid in the scale of the balance, to be weighed with all the exactness possible.

The spirit employed by Mr. Gilpin was furnished to him by Dr. Dollfuss, under whose inspection it had been rectified from rum supplied by Government. Its specific gravity, at 60° of heat, was .82514. It was first weighed pure, in the above-mentioned bottle, at every 5° of heat, from 30 to 100 inclusively. Then mixtures were formed of it and distilled water, in every proportion from $\frac{1}{20}$ of the water to equal parts of water and spirit; the quantity of water added being successively augmented, in the proportion of 5 grs. to 100 of the spirit; and these mixtures were also weighed in the bottle, like the pure spirit, at every 5° of heat. The numbers hence resulting are delivered in the following table; where the first column shows the degrees of heat; the 2d gives the weight of the pure spirit contained in the bottle at those different degrees; the 3d gives the weight of a mixture in the proportions of 100 parts by weight of that spirit to 5 of water, and so on successively till the water and the spirit are in equal parts. The bottle itself, with its cap, having been previously counterpoised, these numbers are the weights of the liquor contained in it, in grains and 100ths of a grain. They are the mean of 3 several experiments at least, as Mr. Gilpin always filled and weighed the bottle over again that number of times, if not oftener. The heat was taken at the even degree, as shown by the thermometer, without any allowance in the first instance, because the coincidence of the mercury with a division can be perceived more accurately than any fraction can be estimated; and the errors of the thermometers, if any, it was supposed would be less on the grand divisions of 5°, than in any others. Mr. Gilpin used the same mixture throughout all the different temperatures, heating it up from 30° to 100°; hence some small error in its strength may have been occasioned, in the higher degrees, by more spirit evaporating than water; but this, it is believed, must have been trifling, and greater inconvenience would probably have resulted from interposing a fresh mixture.

TABLE I.

Weights at the different Degrees of Temperature.

Heat.	The pure spirit.	100 grs. of spirit to 5 grains of water.	100 grs. of spirit to 10 grains of water.	100 grs. of spirit to 15 grains of water.	100 grs. of spirit to 20 grains of water.	100 grs. of spirit to 25 grains of water.	100 grs. of spirit to 30 grains of water.	100 grs. of spirit to 35 grains of water.	100 grs. of spirit to 40 grains of water.	100 grs. of spirit to 45 grains of water.	100 grs. of spirit to 50 grains of water.
	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30°	2487.32	2519.98	2548.59	2573.86	2596.65	2617.24	2636.16	2653.54	2669.64	2684.63	2698.41
35	2480.79	2513.48	2541.96	2567.34	2590.15	2610.80	2629.77	2647.30	2663.48	2678.43	2692.32
40	2474.18	2506.98	2535.52	2560.83	2583.70	2604.50	2623.42	2641.02	2657.35	2672.37	2686.37
45	2467.52	2500.33	2528.90	2554.24	2577.16	2597.99	2617.04	2634.68	2650.96	2666.13	2680.25
50	2460.77	2493.48	2522.10	2547.61	2570.64	2591.50	2610.59	2628.26	2644.68	2659.95	2674.04
55	2453.84	2486.51	2515.30	2540.88	2563.94	2584.79	2604.07	2621.77	2638.25	2653.55	2667.72
60	2446.86	2479.75	2508.60	2534.19	2557.23	2578.22	2597.50	2615.26	2631.82	2647.20	2661.45
65	2440.04	2472.97	2501.87	2527.51	2550.56	2571.48	2590.86	2608.72	2625.41	2640.80	2655.09
70	2433.37	2466.28	2495.00	2530.65	2543.84	2564.89	2584.23	2602.14	2618.89	2634.30	2648.65
75	2426.47	2459.18	2488.03	2513.63	2536.91	2558.14	2577.47	2595.43	2612.20	2627.78	2642.17
80	2419.18	2451.95	2480.83	2506.61	2529.85	2551.10	2570.52	2588.61	2605.32	2621.03	2635.47
85	2412.02	2444.80	2473.68	2499.59	2523.08	2544.41	2563.80	2581.91	2598.76	2614.48	2628.87
90	2404.92	2437.72	2466.64	2492.62	2516.20	2537.57	2556.95	2575.20	2592.17	2607.86	2622.30
95	2397.75	2430.56	2459.51	2485.51	2509.15	2530.51	2549.95	2568.18	2585.12	2601.12	2615.70
100	2390.64	2423.53	2452.63	2478.59	2502.15	2523.59	2543.08	2561.28	2578.37	2594.45	2609.11

Heat.	100 grs. of spirit to 55 grains of water.	100 grs. of spirit to 60 grains of water.	100 grains of spirit to 65 grains of water.	100 grains of spirit to 70 grains of water.	100 grains of spirit to 75 grains of water.	100 grains of spirit to 80 grains of water.	100 grains of spirit to 85 grains of water.	100 grains of spirit to 90 grains of water.	100 grains of spirit to 95 grains of water.	100 grains of spirit to 100 grains of water.
	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.	Grains.
30°	2711.19	2723.00	2733.84	2744.19	2753.67	2762.61	2771.26	2779.21	2786.47	2793.26
35	2705.08	2716.96	2727.78	2738.24	2747.90	2756.97	2765.47	2773.53	2780.75	2787.59
40	2699.09	2711.02	2721.90	2732.45	2742.18	2751.35	2759.85	2767.78	2775.15	2782.06
45	2692.97	2704.82	2715.98	2726.38	2736.21	2745.47	2754.13	2762.03	2769.55	2776.40
50	2686.81	2698.63	2709.92	2720.37	2730.27	2739.52	2748.22	2756.25	2763.67	2770.62
55	2680.57	2692.44	2703.67	2714.27	2724.20	2733.47	2742.25	2750.32	2757.82	2764.72
60	2674.31	2686.16	2697.44	2708.18	2718.26	2727.56	2736.26	2744.32	2751.87	2758.82
65	2667.97	2680.00	2691.22	2701.99	2712.06	2721.47	2730.27	2738.35	2745 93	2752.82
70	2661.67	2673.68	2685.02	2695.66	2705.87	2715.40	2724.22	2732.42	2740.00	2746.88
75	2655.19	2667.32	2678.60	2689.34	2699.57	2709.08	2717.95	2726.25	2733.92	2740.83
80	2648.47	2660.69	2672.02	2682.77	2693.03	2702.57	2711.50	2719.78	2727.49	2734.49
85	2641.85	2654.19	2665.54	2676.40	2686.77	2696.33	2705.37	2713.69	2721.47	2728.60
90	2635.38	2647.61	2659.07	2670.09	2680.60	2690.22	2699.10	2707.44	2715.22	2722.32
95	2628.83	2641.10	2652.56	2663.64	2674.20	2683.79	2692.81	2701.18	2708.91	2716.04
100	2622.22	2634.38	2645.95	2657.14	2667.61	2677.25	2686.36	2694.76	2702.50	2709.75

In order to deduce the specific gravities from the numbers in the preceding table, it was necessary to weigh distilled water in the same vessel. This Mr. Gilpin did, in the same manner as before, at the different degrees of heat; and the result of his experiments is delivered in the annexed table, where the first column shows the heat, and the 2d gives the weight of the water, at that temperature, contained in the bottle.

TABLE II.

Weights and Specific Gravities of Distilled Water.

Heat	Weight of the water.	Specific gravity of the water.
°	Grains.	
30		
35	2967.03	1.00087
40	2967.34	1.00091
45	2967.29	1.00084
50	2966.97	1.00066
55	2966.39	1.00040
60	2965.39	1.00000
65	2964.17	.99952
70	2962.72	.99896
75	2961.03	.99832
80	2959.13	.99762
85	2957.03	.99685
90	2954.80	.99602
95	2952.20	.99507
100	2949.36	.99404

There would be 2 methods of computing the specific gravity, at the different temperatures, from these numbers; one, by taking the weight of the water, at the particular temperature in question, for the standard; and the other by fixing on one certain temperature of the water, for instance 60°, to be the standard, with its bulk at which that of the spirit at all different degrees shall be compared. The latter method was preferred, though not the most usual, because it shows, more readily and simply, the progression observed in the changes of specific gravity, according to the heat and strength of the mixture. This method however rendered it necessary to make an allowance for the contraction and expansion of the bottle used for weighing the liquors, according to the deviation of their temperature from 60°, either below or above. To obtain this correction, the expansion of hollow glass was taken from General Roy's experiments in the 75th volume of the Philos. Trans. as .0000517 of an inch on a foot for every degree of heat; whence its effect, in enlarging the capacity of a sphere, was computed, and the resulting correction added to the weight of the liquors in heats below 60°, and subtracted from it in heats above. On the same account a 3d column is given, in the preceding table, to show the specific gravity of water at the different temperatures, its weight at 60° being taken as the standard.

Another correction also became necessary, on account of the part of the stem of the thermometer which was not immersed in the liquor. This instrument made by Ramsden, had its ball .22 of an inch in diameter, and its stem 13 inches in length. From the ball to the commencement of the scale 3.6 inches of the stem were bare, and then the scale began, which reached from 15 to 110°. The part of it particularly used in these experiments, namely, from 30° to 100°, measured 6.82 inches. The scale was made of ivory, and carried divisions to every 5th of a degree, the quarters of which could be readily estimated; so that the instrument could be read off to 20ths of degrees. When the thermometer was immersed in the weighing-bottle, the liquor reached up nearly to what would have been 0° on its stem; hence, as the heat of the room in which the experiments were made remained about 60°, the correction for the different heat of the quicksilver in the stem from that in the ball of the thermometer was calculated according to Mr. Cavendish's table, given in the 67th volume of the Philos. Trans. Thus the real heat of the fluid in the weighing-bottle being found, an allowance was made to reduce it to the exact degree indicated on the scale of the thermometer. The precise specific gravity of the pure spirit employed was .82514; but to avoid an inconvenient fraction it is taken, in constructing the table of specific gravities, as .825 only, a proportionable deduction being made from all the other numbers. Thus the following table gives the true specific gravity, at the different degrees of heat, of a pure rectified spirit, whose specific gravity at 60° is .825, together with the specific gravities of different mixtures of it with water, at those different temperatures, as far as equal parts by weight.

TABLE III.

Real Specific Gravities at the different Temperatures.

Heat.	The pure spirit.	100 grs. of spirit to 5 grs. of water.	100 grs. of spirit to 10 grs. of water	100 grs. of spirit to 15 grs. of water.	100 grs. of spirit to 20 grs. of water.	100 grs. of spirit to 25 grs. of water.	100 grs of spirit to 30 grs. of water.	100 grs. of spirit to 35 grs. of water.	100 grs. of spirit to 40 grs. of water.	100 grs. of spirit to 45 grs. of water.	100 grs. of spirit to 50 grs. of water.	100 grs. of spirit to 55 grs. of water.
30°	.83899	.85001	.85967	.86819	.87589	.88284	.88922	.89509	.90053	.90558	.91023	.91454
35	.83673	.84776	.85737	.86592	.87363	.88061	.88700	.89292	.89839	.90343	.90811	.91242
40	.83445	.84551	.85515	.86367	.87140	.87843	.88481	.89074	.89626	.90133	.90605	.91034
45	.83215	.84321	.85286	.86140	.86913	.87617	.88260	.88855	.89405	.89916	.90393	.90822
50	.82981	.84084	.85051	.85910	.86688	.87392	.88036	.88632	.89187	.89702	.90177	.90608
55	.82741	.83843	.84815	.85677	.86455	.87159	.87809	.88406	.88963	.89479	.89957	.90390
60	.82500	.83609	.84583	.85445	.86223	.86931	.87582	.88181	.88740	.89259	.89741	.90173
65	.82262	.83374	.84350	.85213	.85991	.86698	.87352	.87954	.88518	.89037	.89518	.89952
70	.82032	.83142	.84111	.84975	.85758	.86469	.87121	.87725	.88291	.88810	.89294	.89733
75	.81792	.82896	.83869	.84731	.85517	.86234	.86886	.87491	.88058	.88585	.89068	.89507
80	.81543	.82649	.83623	.84491	.85276	.85993	.86648	.87258	.87822	88352	.88839	.89277
85	.81291	.82396	.83371	.84243	.85036	.85757	.86411	.87021	.87590	.88120	.88605	.89043
90	.81044	.82150	.83126	.84001	.84797	.85518	.86172	.86787	.87360	.87889	.88376	.88817
95	.80794	.81900	.82877	.83753	.84550	.85272	.85928	.86542	.87114	.87654	.88146	.88588
100	.80548	.81657	.82639	.83513	.84308	.85031	.85688	.86302	.86879	.87421	.87915	.88357

Heat.	100 grs. of spirit to 60 grs. of water.	100 grs. of spirit to 65 grs. of water.	100 grs. of spirit to 70 grs. of water.	100 grs. of spirit to 75 grs. of water.	100 grs. of spirit to 80 grs. of water.	100 grs. of spirit to 85 grs. of water.	100 grs. of spirit to 90 grs. of water.	100 grs. of spirit to 95 grs. of water.	100 grs. of spirit to 100 grs. of water.
30°	.91853	.92219	.92568	.92888	.93191	.93483	.93751	.93996	.94225
35	.91644	.92009	.92362	.92687	.92995	.93281	.93553	.93796	.94027
40	.91438	.91805	.92161	.92489	.92799	.93086	.93353	.93602	.93835
45	.91222	.91599	.91950	.92281	.92595	.92887	.93153	.93407	.93638
50	.91007	.91388	.91740	.92075	.92388	.92681	.92952	.93202	.93436
55	.90791	.91170	.91528	.91863	.92176	.92472	.92744	.92997	.93230
60	.90570	.90954	.91316	.91656	.91971	.92264	.92536	.92791	.93025
65	.90359	.90738	.91100	.91440	.91769	.92055	.92328	.92584	.92816
70	.90136	.90522	.90880	.91225	.91547	.91845	.92121	.92377	.92608
75	.89916	.90298	.90660	.91005	.91326	.91625	.91909	.92164	.92397
80	.89690	.90072	.90435	.90780	.91103	.91404	.91683	.91943	.92179
85	.89460	.89843	.90209	.90558	.90882	.91186	.91465	.91729	.91969
90	.89230	.89617	.89988	.90342	.90668	.90967	.91248	.91511	.91751
95	.89003	.89390	.89763	.90119	.90443	.90747	.91029	.91290	.91531
100	.88769	.89158	.89536	.89889	.90215	.90522	.90805	.91066	.91310

From this table, when the specific gravity of any spirituous liquor is ascertained, it will be easy to find the quantity of rectified spirit, of the above-mentioned standard, contained in any given quantity of it, either by weight or measure. As common arithmetic is competent to furnish the rules for this purpose, it would be superfluous to give them here. All the objects of inquiry relative to this business should, Dr. B. thinks, be reduced to tables; the first of which might exhibit the specific gravities of different mixtures, from 1 to 100 parts of water, increasing by 1, at every degree of heat from 40 to 80, being the utmost limits of temperature that can be wanted in common practice. This table need only be calculated to 3 places of figures, which will always give the quantity of spirit true within a 50th part of the whole, and in the most usual degrees of heat within a 100th; and to this number of figures the areometer, or hydrometer, showing the specific gravities, could be suited. A further reason for continuing only to 3 places of figures is, that, accurate as Mr. Gilpin's experiments have been, some irregularities are found in the last 2 of the 5 decimals to which his tables are calculated. The greatest of these irregularities do not exceed the quantity corresponding to a difference of $\frac{1}{50}$ of a degree of heat, and in general they are much less. A table might be constructed to show what the numbers would probably have been, to the 5 places of decimals, if there had been no kind of error in the experiments.—Another table should be of the volumes, exhibiting what proportion the spirit and water bore to each other by measure or bulk, in the different mixtures; whence might be calculated a very useful table of diminutions, to show when a given weight, or volume, of a certain spirit and water are mixed together, how much their bulk would be diminished; or, what is called by the distillers the concentration. From such a table the distiller could learn what quantity of water he must mix with spirit of a given strength, in order to reduce it to proof spirit, or any other strength; and also what quantity of proof spirit, or spirit of any other strength, he may obtain, by adding water to spirit of a given strength; both circumstances very necessary to be known in the trade, and which some of the sliding rulers now in use profess to point out.

It may appear odd, that no mention has been made till now of proof spirit, the standard to which most of the regulations of the excise have hitherto been referred. The reasons for not adopting this standard are: first, that the strength of spirit to be called proof is a mere arbitrary point, and by no means so exactly determined as could be wished; and 2dly, that it seemed most convenient to take for the standard the highest strength of spirit usually found in commerce, and beyond which it cannot be rectified without a process of some expense, so that all the other degrees of strength might be reckoned one way, without the intervention of a middle point, inducing the necessity of denominating some above and others under. If however Government should find it expedient to preserve the reference to proof spirit, from the tables given in this report others may be constructed, in which all the old terms of over and under proof should be retained, and have a precise meaning, as soon as the strength to be called proof shall be finally settled. By the Act of 2 Geo. III. it is ordered, that the gallon of brandy or spirits of the strength of 1 to 6 under proof, shall be taken and reckoned at 7 lb. 13 oz. which is understood by the trade to mean at 55° of heat. Hence, taking the weight of a gallon of water at the same heat to be 8 lb. 5.66 &c. oz.,* the specific gravity of this diluted spirit will be found .9335 at 60; † whence, by a computation founded on the tables in this report, the specific gravity of proof spirit will come out .916. But the rulers of correction belonging to Dicas's and Quin's hydrometers give the specific gravity of proof spirits about .922 at 55°, equivalent to .920 at 60°. The former, .916, corresponds to a mixture of 100 parts of spirit with 62 by measure, or 75 by weight, of water; and the latter, .920, to a mixture of 100 parts of spirit and 66 by measure, or 80 by weight, of water. The difference is considerable; but the first is undoubtedly most conformable to the existing acts of parliament. If therefore it be thought right to preserve the term proof-spirit in our excise laws, it may be understoood to mean spirit, whose specific gravity is .916, and which is composed of 100 parts of rectified spirit at .825, and 62 parts of water by measure, or 75 by weight; the whole at 60 degrees of heat.

Dr. B. has chosen this point of the thermometer, 60°, in preference to 55°, because it is much the most suitable for experiments, being the temperature at which a room feels pleasant, and in which any operation, however slow and tedious, can be executed without the uneasy sensation of cold: for this reason it has been adopted by many English philosophers. In the table formerly recommended, from 40 to 80 degrees of the thermometer, it will be the middle temperature.

The specific gravity of .825 having been fixed on as the standard of rectified spirit in our tables, Mr. Gilpin was desired to ascertain by experiment what proportion of water would be necessary, to reduce the lightest alcohol in his possession to that standard. This was some of the alcohol which Mr. Lewis had furnished; and its specific gravity being .814196 at 60°, 3000 grains of it mixed with 135 grains of distilled water formed a compound, whose specific gravity was .825153; that is, in round numbers, 100 grains of alcohol at .814 with 4.5 grains of water, form our standard of spirit at .825.

* Probably 8 lb. 5.72 oz. is nearer.—Orig.

† This specific gravity indicates a mixture of 107 grains of water with 100 of spirit, and consequently is below Mr. Gilpin's present tables, which go only to equal parts.—Orig.

On Hydrometers.—The readiest way of ascertaining specific gravities, and doubtless the most convenient for public business, is by hydrometers; and those of the simplest construction must be best on the whole, especially if more accurate means are kept at hand, to be resorted to in case of disputes. An hydrometer of glass would be the most certain; but whether it be of that substance, or of metal, it should consist of a ball, or rather bulb, so poised as that a certain part should be always downmost in the liquor, and having a stem rising from it on the opposite part, which would consequently keep upright in using the instrument. On the size of this stem, the sensibility of the hydrometer chiefly depends. In the old areometers the stem was made so large, that the volume of water displaced between its least and greatest immersions was equal to the whole difference of specific gravity between water and alcohol, or perhaps more; whence its scale of divisions must be very small, and could not give the specific gravity with much accuracy. To remedy this defect, weights were introduced, by means of which the stem could be made smaller, each weight affording a new commencement of its scale; so that the size of the divisions on a given length of stem was doubled, tripled, quadrupled, &c. according as 1, 2, 3, or more weights were employed, the diameter of the stem being lessened in the subduplicate proportion of the increased length of the divisions. Of late this principle seems to have been carried to excess; the number of weights adapted to some hydrometers being so great as to prove very inconvenient in practice. A mean between the 2 methods would certainly be best, which might be suited to our tables in the following manner.

It is proposed to determine the specific gravity to 3 places of decimals, water being taken as unity: the whole compass of numbers therefore, from rectified spirit of water, at 60° of heat, would be the difference between .825 and 1.000, that is, 175; call it 220 to include the lightest spirit and heaviest water, at all the common temperatures. Of these divisions the stem might give every 20, and then 10 weights would be sufficient for the whole 220. By making the stem carry 20 divisions, an inconvenience much complained of, that of shifting the weights, would in great measure be avoided; because a person conversant in such business would seldom err to that extent in judging of the strength of his spirit previous to trial; and yet the stem would not need to be so large, or the divisions so small as to preclude the desired accuracy. In conformity to this arrangement it would be proper, that the weights adapted to the hydrometer should be marked with the numbers of the specific gravity, zero on the top of its stem, without a weight, being supposed to mean 800, and 20 at the bottom of the stem to signify 820, which number the first weight would carry; the successive weights would be marked 840, 860, &c.; and the division on the stem cut by the fluid under trial would be a number to be always added to the number marked on the weight, the sum of the two showing the true specific gravity. The weights should unquestionably be made to apply on the top of the stem, so as never to come into contact with the liquor; and in using the hydrometer, its stem should always be pressed down lower than the point at which it will ultimately rest, that by being wetted it may occasion no resistance to the fluid. The instrument itself should be of as regular a shape, and with as few inequalities and protuberances, as possible, that all unnecessary obstruction to its motions may be avoided.

As it is not probable but disputes will sometimes arise, it would be advisable, that some of the principal excise offices should be provided with a good pair of scales, and a weighing-bottle properly marked, the quantity of whose contents of distilled water at 60° had been previously determined. By filling this bottle up to the mark with the spirit in question, and dividing its increase of weight by the given weight of water required to fill it, the specific gravity of the spirit might be better ascertained, even under the management of a common operator, than by the most dexterous use of the hydrometer.

The simplest and most equitable method of levying the duty on spirituous liquors would be, to consider rectified spirit as the true and only excisable matter. On this principle, all such liquors would pay exactly according to the quantity of rectified spirit they contain; so that when a cask, for instance, of any spirits was presented to the revenue officer, his business would be to determine from the quantity, specific gravity, and temperature, of the liquor, how many gallons, or pounds, of rectified spirit enter into its composition; each of which gallons, or pounds, should be charged a certain sum. The complicated regulations attending the adaption of the duties to different degrees of strength would thus be avoided; and it is believed that many frauds might be prevented, which artful persons have now an opportunity of practising, by altering the strength of their spirit in a variety of ways. From the tables already recommended, it would be easy to deduce this quantity of rectified spirit, either by weight or measure, in any given quantity of a spirituous liquor; or other tables might be constructed which should show it at once by inspection.

If however it be thought by government most expedient not to make any essential change in the present manner of collecting this article of the revenue, Dr. B. would at least recommend, that the specific gravity should be substituted for the relation to proof spirit. Thus, instead of ordering so much duty per gallon to be paid by spirits 1 to 6 under proof, it may be enacted, that the same sum shall be paid by spirit of .9335 specific gravity, or, not to be too precise, by spirit from .930 to .935, and so on for any other degrees of strength; a certain temperature, suppose 60°, being always understood to be meant when specific gravity is mentioned in an Act of Parliament. The duties to be laid according to either of these methods may readily be adjusted or equalized to those paid at present, as far as the latter can be determined from the act of 2 George III. referred to above, or by any of the instruments now in use.

XIX. Observations on the Sugar Ants. By John Castles, Esq.* p. 346.

The sugar ants, so called from their ruinous effects on the sugar-cane, first made their appearance in Grenada about the year 1770, on a sugar plantation at Petit Havre, a bay 5 or 6 miles from the town of St. George, the capital, conveniently situated for smuggling from Martinique. It was therefore concluded, they were brought from thence in some vessel employed in that trade; which is very probable, as colonies of them in like manner were afterwards propagated in different parts of the island by droghers, or vessels employed in carrying stores, &c. from one part of the island to another. Thence they continued to extend themselves on all sides, for several years; destroying in succession every sugar plantation between St. George's and St. John's, a space of about 12 miles. At the same time colonies of them began to be observed in different parts of the island, particularly at Duquesne on the north, and Calavini on the south side of it.

All attempts of the planters to put a stop to the ravages of these insects having been found ineffectual, it well became the legislature to offer great public rewards for discovering a practicable method of destroying them, so as to permit the cultivation of the sugar-cane as formerly. Accordingly, an act was passed, by which such discoverer was entitled to 20,000 pounds, to be paid from the public treasury of the island. Many were the candidates on this occasion, but very far were any of them from having any just claim: yet considerable sums of money were granted, in consideration of trouble and expenses in making experiments. In Grenada there had always been several species of ants, differing in size, colour, &c. which however were perfectly innocent with respect to the sugar-cane. The ants in question, on the contrary, were not only highly injurious to it, but to several sorts of trees, such as the lime, lemon, orange, &c.

These ants are of the middle size, of a slender make, of a dark red colour, and remarkable for the quickness of their motions; but their greatest peculiarities were, their taste when applied to the tongue, the immensity of their number, and their choice of places for their nests. All the other species of ants in Grenada have a bitter musky taste. These, on the contrary, are acid in the highest degree, and, when a number of them were rubbed together between the palms of the hands, they emitted a strong vitriolic sulphureous smell; so much so, that when this experiment was made, a gentleman conceived that it might be owing to this quality that these insects were so unfriendly to vegetation. This criterion

* This species of ant is perhaps the *Formica saccharivora* of the Gmelinian edition of the Systema Naturæ.

to distinguish them was infallible, and known to every one. Their numbers were incredible. The roads are seen coloured by them for miles together: and so crouded were they in many places, that the print of the horses feet would appear for a moment or two, till filled up by the surrounding multitude. All the other species of ants, though numerous, were circumscribed and confined to a small spot, in proportion to the space occupied by the cane ants, as a mole hill to a mountain. The common black ants of that country had their nests about the foundation of houses or old walls; others in hollow trees; and a large species in the pastures, descending by a small aperture under ground. The sugar ants universally constructed their nests among the roots of particular plants and trees, such as the sugar-cane, lime, lemon, and orange trees, &c.

The destruction of these ants was attempted chiefly by poison and the application of fire. For the first purpose, arsenic and corrosive sublimate, mixed with animal substances, such as salt fish, herrings, crabs, and other shell-fish, &c. were used, which was greedily devoured by them. Myriads of them were thus destroyed; and the more so, as it was observed by a magnifying glass, and indeed by the naked eye, that corrosive sublimate had the effect of rendering them so outrageous that they destroyed each other; and that effect was produced even by coming into contact with it. But it is clear, and it was found, that these poisons could not be laid in sufficient quantities over so large a tract of land, as to give the hundred-thousandth part of them a taste, and consequently they proved inadequate to the task.

The use of fire afforded a greater probability of success; for it was observed, that if wood, burnt to the state of charcoal, without flame, and immediately taken from the fire, was laid in their way, they crouded to it in such amazing numbers as soon to extinguish it, though with the destruction of thousands of them in effecting it. This part of their history appears scarcely credible; but, on making the experiment himself, Mr. C. found it literally true. He laid fire, as above described, where there appeared but a very few ants, and in the course of a few minutes thousands were seen crouding to it and on it, till it was perfectly covered by their dead bodies. Holes were therefore dug at proper distances in a cane piece, and fire made in each of them. Prodigious quantities perished in this way; for those fires, when extinguished, appeared in the shape of molehills, from the numbers of their dead bodies heaped on them. Yet they soon appeared again as numerous as ever. This may be accounted for, not only from their amazing fecundity, but that probably none of the breeding ants, or young brood, suffered from the experiment. For the same reason, the momentary general application of fire by burning the cane trash, or straw of the cane, as it lay on the ground, proved as little effectual; for though perhaps multitudes of ants might have been destroyed, yet in general they would escape by retiring to their nests under cover, and out of its reach, and the breeding ants, with their young progeny, must have remained unhurt.

Mr. Smeathman, who wrote a paper on the termites, or white ants, of Africa, and was at Grenada at this time, imagined that these ants were not the cause of the injury done to the canes. He supposed it was owing to the blast, a disease the canes are subject to, said to arise from a species of small flies, generated on their stems and leaves; and that the ants were attracted in such multitudes merely to feed on them. There is no doubt, that where this blast existed, it constituted part of the food of the ants: but this theory was overthrown, by observing, that by far the greatest part of the injured canes had no appearance of that sort, but became sickly and withered, apparently for want of nourishment. Besides, had that been the case, the canes must have been benefited instead of being hurt by these insects. For the cure of the blast, he proposed the application of train oil, which had not the least effect in preventing the mischief, and, if it had, could never have been generally enough used to answer the purpose.

This calamity, which resisted so long the efforts of the planters, was at length removed by another, which, however ruinous to the other islands in the West Indies, and in other respects, was to Grenada a very great blessing, namely, the hurricane in 1780; without which it is probable the cultivation of the sugar-cane in the most valuable parts of that island must have in a great measure been thrown aside, at least for some years. How this hurricane produced this effect has been considered rather as a matter of wonder and surprise than attempted to be explained. By attending to the following observations, the difficulty will probably be removed.

These ants make their nests, or cells for the reception of their eggs, only under or among the roots of such trees or plants as are not only capable of protecting them from heavy rains, but are at the same time so firm in the ground as to afford a secure basis to support them against any injury occasioned by the agitation of the usual winds. This double qualification the sugar-cane possesses in a very great degree; for a stool of canes, which is the assemblage of its numerous roots where the stems begin to shoot out, is almost impenetrable to rain, and is also, from the amazing numbers and extension of the roots, firmly fixed to the ground. Thus, when every other part of the field is drenched with rain, the ground under those stools will be found quite dry, hence, in ordinary weather, their nests are in a state of perfect security. The lime, lemon, orange, and some other trees, afford these insects the same advantages, from the great number and quality of their roots, which are firmly fixed to the earth, and are very large; besides which, their tops are so very thick and umbrageous as to prevent even a very heavy rain from reaching the ground underneath.

On the contrary, these ants' nests are never found at the roots of trees or plants incapable of affording the above protection: such, for instance, is the coffee tree. It is indeed sufficiently firm in the ground, but it has only one large tap root, which goes straight downwards, and its lateral roots are so small as to afford no shelter against rain. So again, the roots of the cotton shrub run too near the surface of the earth to prevent the access of rain, and are neither sufficiently permanent, nor firm enough to resist the agitation by the usual winds. The same observation will be found true with respect to cocoa, plantains, maize, tobacco, indigo, and many other species of trees and plants. Trees or plants of the first description always suffer more or less in lands infested with these ants; whereas those of the latter never do. Hence we may fairly conclude, that the mischief done by these insects is occasioned only by their lodging and making their nests about the roots of particular trees or plants. Thus the roots of the sugar-canes are somehow or other so much injured by them, as to be incapable of performing their office of supplying due nourishment to the plants, which therefore become sickly and stinted, and consequently do not afford juices fit for making sugar in either tolerable quantity or quality.

That these ants do not feed on any part of the canes or trees affected, seems very clear, for no loss of substance in either the one or the other has ever been observed; nor have they ever been seen carrying off vegetable substances of any sort. The truth of this will further appear by the following fact. A very fine lime-tree, in the pasture of Mount William estate, at a considerable distance from any canes, but near the dwelling house, had sickened and died soon after the ants made their appearance on that estate. After it had remained in that state, without a single leaf, or the least verdure, for several months, on examination, a very few ants appeared about it; but when with the manager's permission it was grubbed out, a most astonishing quantity of ants and ants' nests, full of eggs, were found about its roots, all of which were quite dead, and many of them rotten. That this tree constituted no part of their food is quite certain; but, while it continued to afford them proper security for their nests, they still continued their abode.

On the contrary, there is the greatest presumption that these ants are carnivorous, and feed entirely on animal substances; for if a dead insect, or animal food of any sort, was laid in their way, it was immediately carried off. It was found almost impossible to preserve cold victuals from them. The largest carcases, as soon as they began to become putrid, so as that they could separate the parts, soon disappeared. Negroes with sores had difficulty to keep the ants from the edges of them. They destroyed all other vermin, rats in particular, of which they cleared every plantation they came on, which they probably effected by attacking their young. It was found that poultry, or other small stock, could be raised with the greatest difficulty; and the eyes, nose, and other emunctories of the bodies of dying or dead animals, were instantly covered with them. In the year 1780, many of the sugar estates which had been first infested with these ants had been either abandoned, or put into other kinds of produce, principally cotton; which, as above observed, do not afford conveniency for their nests. In consequence, the ants had there so much decreased in number, that the cultivation of sugar had again begun to be re-assumed. But it was very different in those plantations which had but lately been attacked, and were still in sugar. At Duquesne particularly at that time they were pernicious in the highest degree, spreading themselves on all sides with great rapidity, when a sudden stop was put to their progress by the hurricane which happened near the middle of October that year. How this was effected may be explained by attending to the above observations.

From what has been said it appears, that a dry situation, so as to exclude the ordinary rains from their nests or cells, appropriated for the reception of their eggs or young brood, is absolutely necessary; but that these situations, however well calculated for the usual weather, could not afford this protection from rain during the hurricane, may be easily conceived. When by the violence of the tempest heavy pieces of artillery were removed from their places, and houses and sugar-works levelled with the ground, there can be no doubt that trees and every thing growing above ground must have greatly suffered. This was the case. Great numbers of trees and plants, which resist commonly the ordinary winds, were torn out by the root. The canes were universally either lodged or twisted about as if by a whirlwind, or torn out of the ground altogether. In the latter case, the breeding ants, with their progeny, must have been exposed to inevitable destruction from the deluge of rain which fell at the same time. The number of canes however, thus torn out of the ground, could not have been adequate to the sudden diminution of the sugar ants; but it is easy to conceive that the roots of canes which remained on the ground, and the earth about them, were so agitated and shaken, and at the same time the ants' nests were so broken open, or injured by the violence of the wind, as to admit the torrents of rain accompanying it. Probably therefore the principal destruction of these ants must have been thus effected.

Two circumstances tended to facilitate this happy effect. Many of the roots of the canes infected, as above observed, were either dead or rotten, so as not to be capable of making the same resistance to the wind as those in perfect health. And this hurricane happened so very late as the month of October, when the canes are always so high above ground as to give the wind sufficient hold of them, which at an earlier period would not have been the case. That many of the cane ants were swept off by the torrents of rain into the rivers and ravines, and thus perished, cannot be doubted; but if we consider the obstacles to this being very general, it could have had but small effect in considerably reducing their numbers; for on flat land it could not have happened. In hanging or hilly land, the cane trash would afford great shelter, and the ants would naturally retire to their nests for security, when they found their danger.

Some have supposed, that the sugar ants, after a certain time, degenerate, and become inoffensive; and in proof of this, they say, Martinique and Barbadoes were freed from their bad effects without a hurricane or any other apparent cause. The idea of any such extraordinary and unheard-of deviation of nature, is too contemptible to deserve an answer; but the reason is obvious. The planters there either abandoned their cane lands, or planted them in coffee, cocoa, cotton, indigo, &c. none of which, according to the above observations, afford the ants proper conveniency for the propagation of their species; and therefore their numbers must have so much decreased as to re-admit the culture of the sugar-cane as before. At the same time it is very probable, that this diminution might have in part been owing to something of the hurricane kind; for it is well known that strong squalls of wind, attended with heavy rains, are frequent in the West Indies, though they do not last so long, nor are so violent, as to deserve the name of a hurricane.

All that has been said on this subject would certainly be of little or no consequence, did it not lead to the true method of cultivating the sugar-cane on lands infested with those destructive insects; in which point of view however it becomes important. If then the above doctrine be just, it follows that the whole of our attention must be turned to the destruction of the nests of these ants, and consequently the breeding ants with their eggs or young brood. In order to effect this, all trees* and fences, under the roots of which these ants commonly take their residence, should first be grubbed out: particularly lime fences, which are very common in Grenada, and which generally suffered from the ants before the canes appeared in the least injured. After which the canes should be stumped out with care, and the stools burnt as soon as possible, together with the field trash, or the dried leaves and tops of the canes, to prevent the ants from making their escape to new quarters. The best way of doing this would probably be, to gather the field trash together in considerable heaps, and to throw the stools as soon as dug out of the ground into them, and immediately apply fire. By this means multitudes must be destroyed; for the field trash, when dry, burns with great rapidity. The land should then be ploughed or hoe-ploughed twice, or once at least, in the wettest season of the year, to admit the rains, before it is hoed for planting the cane: by these means these insects might be so much reduced in number as at least to secure a good plant cane.

* Particular fruit trees may probably be preserved, without detriment, by carefully removing the earth from about their roots, destroying the ants' nests, and afterwards replacing either the same or new earth.—Orig.

But it is the custom in most of the West India islands to permit the canes to rattoon; that is, after the canes have once been cut down, for the purpose of making sugar, they are suffered to grow up again, without replanting; and this generally for 3 or 4 years, but sometimes for 10, 15, or 20. In this mode of culture the stools become larger every year, so as to grow out of the ground to a considerable height, and by that means afford more and more shelter to the ants' nests; therefore, for 2 or 3 successive crops, the canes should be replanted yearly, so as not only to afford as little cover as possible for the ants' nests, but continually to disturb such ants as may have escaped, in the business of propagating their species.

That considerable expense and labour will attend putting this method into execution, cannot be doubted. An expensive cure however is better than none; but from the general principles of agriculture, Mr. C. is of opinion, that the planter will be amply repaid for his trouble, by the goodness of his crops, in consequence of the superior tilth the land will receive in the proposed method. Of this we have a proof in the island of St. Kitt's, where they constantly replant their canes yearly; and it is very well known, that an acre of cane land there gives a greater return than the same quantity in any other island. In St. Kitt's, 5 hogsheads per acre is common yielding in good land. In Grenada, from 2 to 3 hogsheads from plant canes, and half that quantity from rattoons. Thus, though the St. Kitt's planter cuts only one half his cane land yearly, in a given number of years he makes a greater revenue than the Grenada planter on the present mode of rattooning, when $\frac{4}{5}$ of the cane land is yearly cut.

Some may be of opinion, that it would be more advantageous to change the produce than to pursue the proposed method; on which Mr. C. observes, that it appears $\frac{1}{2}$ of the usual crop of sugar, thus produced, will be more advantageous to the planter, when at the same time progress is making in destroying the sugar ants, than a full crop of any other produce. In some very few situations cotton perhaps may be excepted. As to coffee, it is to be considered that it gives no return till the 3d year after planting, and not a full crop till the 5th. Cocoa begins to bear in 5 years; but yields little till the 7th; and indigo not only exceedingly impoverishes the land, but is unhealthy to the negroes. Add to this, that far the greatest part of sugar lands are unfit for the culture of any of these.

XX. Experiments and Observations on the Dissolution of Metals in Acids, and their Precipitations; with an Account of a New Compound Acid Menstruum, useful in some Technical Operations of Parting Metals. By James Keir, Esq., F. R. S. p. 359.

In the following paper, says Mr. K., I intend to relate 2 sets of experiments; one, showing the effects of compounding the vitriolic and nitrous acids in dissolving metals; and the other, describing some curious appearances which occur in the precipitation of silver from its solution in nitrous acid by iron, and by some other substances. In a subsequent paper I hope to continue the subject of metallic dissolution * and precipitation, first, by adding some experiments on the quantities and kinds of gas produced by dissolving different metals in different acids, under various circumstances; 2dly, by submitting certain general propositions, which seem deducible from the facts related; and lastly, by concluding with some reflections relative to the theory of metallic dissolution and precipitation.

PART 1. *On the effects of compounding the vitriolic and nitrous acids, under various circumstances, on the dissolution of metals.*

§ 1. *On the mixture of oil of vitriol and nitre.*—1. The properties of the several acids, in their separate states, have been investigated with considerable industry and success; and those of one compound, aqua regis, are well known, on account of its frequent use in dissolving gold: yet not only various other combinations of different acids remain to be examined; but also the changes of properties to which these mixed acids are subject, from the difference of circumstances, especially those of concentration, temperature, and of that quality which is called, properly or improperly, phlogistication, are subjects still open for inquiry.

2. As I shall have frequent occasion to speak of the phlogistication and dephlogistication of acids, I wish to premise, that by these terms I mean only certain states or qualities of those bodies, but without any theoretical reference. Thus vitriolic acid may be said to be phlogisticated by addition of sulphur or other inflammable matter, by which it is converted into sulphureous acid, without determining whether this change be caused by the addition of the supposed principle phlogiston, as one set of philosophers believe, or by the action of the added inflammable substance in drawing from the acid a portion of its aërial principle, by which the sulphur, its other element, is made to predominate, as others have lately maintained. It were much to be wished that we had words totally unconnected with theory; that chemists, who differ from each other in some speculative points, may yet speak the same language, and may relate their facts and observations, without having our attention continually drawn aside from these, to the different modes of explanation which have been imagined. But at present

* The English word solution has two significations in chemistry; one, expressive of the act of dissolving, as when we say, that "solution is a chemical operation;" and the other, denoting the substance dissolved in its solvent, as "a solution of silver in nitrous acid." The French language is equally equivocal, as the word "dissolution" is used in both the above-mentioned senses. In treating on this subject, in which both meanings were very frequently required, sometimes in the same sentence, I could not but be sensible of confusion in the stile, and I have therefore confined the word solution to express the substance dissolved together with its solvent, and the word dissolution to denote the act of dissolving.—Orig.

we have only the choice of terms between words derived from the ancient theory, and those which have been lately proposed by the opposers of that theory. In this dilemma I have preferred the use of the former, not that I wish to show any predilection to either theory, but because that system, having long been generally adopted, is understood by all parties; and principally because, by using the words of the old theory, I am at liberty to define them, and to give significations expressive merely of facts, and of the actual state of bodies; whereas the language and theory of the antiphlogistic chemists being interwoven and adapted to each other, the former cannot be divested of its theoretical reference, and therefore seems inapplicable to the mere exposition of facts, but ought to be reserved solely for the explanation of the doctrines from which this language is derived. Thus, by the definition before mentioned of phlogistication, this word expresses not the presence or existence of an hypothetical principle of inflammability; but a certain well-known quality of acids and of other bodies, communicated to them by the addition of many actual inflammable substances. Thus nitrous acid acquires a phlogisticated quality by addition of a little spirit of wine, or by distillation with any inflammable substance.

3. No two substances are more frequently in the hands of chemists and artists than vitriolic acid and nitre, yet I have found, that a mere mixture of these, when much concentrated, possesses properties which neither the vitriolic acid nor the nitrous, of the same degree of concentration, have singly, and which could not easily be deduced, à priori, by reasoning from our present knowledge of the theory of chemistry.

4. Having found by some previous trials that a mixture composed of nitre dissolved in oil of vitriol was capable of dissolving silver easily and copiously, while it did not affect copper, iron, lead, regulus of cobalt, gold, and platina, I conceived, that it might be useful in some cases of the parting of silver from copper and the other metals above mentioned; and having also observed, that the dissolving powers of the mixture of vitriolic and nitrous acids varied greatly in different degrees of concentration and phlogistication, I thought that an investigation of these effects might be a subject fit for philosophical chemistry, and might tend to illustrate the theory of the dissolution of metals in acids. With these views I made the following experiments.

5. I put into a long-necked retort, the contents of which, including the neck, were 1400 grain measures, 100 grain measures of oil of vitriol of the usual density at which it is prepared in England, that is, whose specific gravity is to that of water as 1.844 to 1, and 100 grs. of pure and clean nitre, which was then dissolved in the acid by the heat of a water-bath. To this mixture 100 grs. of standard silver were added; the retort was set in a water bath, in which the water was made to boil, and a pneumatic apparatus was applied to catch any air

or gas which might be extricated.—The silver began to dissolve, and the solution became of a purple or violet colour. No air was thrown into the inverted jar, excepting a little of the common air of the retort, by means of the expansion which it suffered from the heat of the water-bath, and from some nitrous fumes which appeared in the retort, and which having afterwards condensed, occasioned the water to rise along the neck of the retort, and mix with the solution. The remaining silver was then separated and weighed, and it was found that 39 grains had been dissolved; but probably more would have been dissolved if the operation had not been interrupted by the water rushing into the retort.

6. In the same apparatus 200 grs. of standard silver were added to a mixture of 100 grs. of nitre, previously dissolved in 200 grain-measures of oil of vitriol; and in this solvent 92 grs. of the silver were dissolved, without any production of air or gas. The solution, which was of a violet colour, having been poured out of the retort while warm (for with so large a proportion of nitre, such mixtures, especially after having dissolved silver, are apt to congeal with small degrees of cold,) in order to separate the undissolved silver from it, and having been returned into the retort without this silver, I poured 200 grs. of water into the retort, on which a strong effervescence took place between the solution and the water, and 3100 grain-measures of nitrous gas were thrown into the inverted jar. On pouring 200 grs. more of water into the retort, 600 grain-measures of the same gas were expelled. Further additions of water yielded no more gas; neither did the silver, when afterwards added to this diluted solution, give any sensible effervescence, or suffer a greater loss of weight than 2 grains.

7. In the same apparatus 100 grs. of standard silver were exposed to a mixture of 30 grs. of nitre dissolved in 200 grain-measures of oil of vitriol; and in this operation, 80 grs. of silver were dissolved, while at the same time 4500 grain-measures of nitrous gas were thrown into the inverted jar. When the undissolved silver was removed, 200 grs. of water were added to the solution, which was of a violet colour, and on the mixture of the 2 fluids an effervescence happened; but only a few bubbles of nitrous gas were then expelled.

8. In the same apparatus 100 grs. of standard silver were exposed to a mixture of 200 grain-measures of oil of vitriol, 200 grs. of nitre, and 200 grs. of water; and in this operation 20 grs. of the silver were dissolved without any sensible emission of air or gas.

9. In these experiments, the copper contained in the standard silver gave a reddish colour to the saline mass which was formed in the solution, and seemed to be a calx of copper interspersed through the salt of silver. I perceived no other difference between the effects of pure and standard silver dissolved in this acid.

10. I then exposed tin to the same mixture of oil of vitriol and nitre, in the

same apparatus, and in the same circumstances, taking care always to add more metal than could be dissolved, that, by weighing the remainder, the quantity capable of being dissolved might be found, as I had done with the experiments on silver: and the results were as follow.

11. No tin was dissolved nor calcined by the mixtures in the proportion of 200 grain-measures of oil of vitriol to 200 grs. of nitre; nor by any other mixture in the proportion of 200 grain-measures of oil of vitriol to 150 grs. of nitre, and consequently no gas was produced in either instance.

12. With a mixture in the proportion of 200 grain-measures of oil of vitriol and 100 grs. of nitre, the tin began soon to be acted on, and to be diffused through the liquor; but no extrication of gas appeared till the digestion had been continued 2 hours in boiling water; and then it took place, and gave a frothy appearance to the mixture, which was of an opaque white colour, from the powder of tin diffused among it. In this experiment the quantity of tin thus calcined was 73 grs., and the quantity of nitrous gas extricated during this action on the tin was 8500 grain-measures. Then, on pouring 200 grs. of water into the retort, a fresh effervescence took place between the water and the white opaque mass, and 4600 grain-measures of nitrous gas were thrown into the inverted receiver.

13. With a mixture in the proportion of 100 grain-measures of oil of vitriol to 30 grs. of nitre, 30 grs. of tin were dissolved or calcined, and the nitrous gas, which began to be extricated much sooner than in the last-mentioned experiment with a larger proportion of nitre, amounted to 6300 grain-measures. Water, added to this solution of tin, did not produce any effervescence.

14. With a mixture in the proportion of 200 grain measures of oil of vitriol, 200 grs. of nitre, and 200 grs. of water, 133 grs. of tin were acted on with an effervescence, which took place violently, and produced 6500 grain-measures of nitrous gas.

15. The several mixtures above mentioned, in different proportions of nitre and oil of vitriol, did, by the help of the heat of the water-bath, calcine mercury into a white or greyish powder. Nickel was also partly calcined and partly dissolved by these mixtures. I did not perceive that any other metal was affected by them, excepting that the surfaces of some of them were tarnished.

16. These mixtures of oil of vitriol and nitre were apt to congeal by cold, those especially which had a large proportion of nitre. Thus, a mixture of 1000 grain-measures of oil of vitriol and 480 grs. of nitre, after having kept fluid several days, in a phial not so accurately stopped as to prevent altogether the escape of some white fumes, congealed at the temperature of 55° of Fahrenheit's thermometer; whereas some of the same liquid, having been mixed with equal parts of oil of vitriol, did not congeal with a less cold than 45°. The

congelation is promoted by exposure to air, by which white fumes rise, and moisture may be absorbed, or by any other mode of slight dilution with water.

17. Dilution of this compound acid, with more or less water, alters considerably its properties, with regard to its action on metals. Thus it has been observed, that in its concentrated state it does not act on iron; but by adding water, it acquires a power of acting on that metal, and with different effect according to the proportion of the water added. Thus, by adding to 2 measures of the compound acid 1 measure of water, the liquor is rendered capable of calcining iron, and forming with it a white powder, but without effervescence. With an equal measure of water effervescence was produced. With a larger proportion of water the iron gave also a brown colour to the liquor, such as phlogisticated nitrous acid acquires from iron, or communicates to a solution of martial vitriol in water.

18. Dilution with water renders this compound acid capable of dissolving copper and zinc, and probably those other metals which are subject to the action of the dilute vitriolic or nitrous acids.

§ 2. *An account of a new process for separating silver from copper.*—19. The properties of this liquor, in dissolving silver easily, without acting on copper, have rendered it capable of a very useful application in the arts. Among the manufactures at Birmingham, that of making vessels of silver plated on copper is a very considerable one. In cutting out the rolled plated metal into pieces of the required forms and sizes, there are many shreds, or scraps as they are called, unfit for any purpose but the recovery of the metals by separating them from each other. The easiest and most economical method of parting these 2 metals, so as not to lose either of them, is an object of some consequence to the manufacturers. For this purpose 2 modes were practised, 1, by melting the whole of the mixed metals with lead, and separating them by eliquation and testing; and the 2d, by dissolving both metals in oil of vitriol, with the help of heat, and by separating the vitriol of copper, by dissolving it in water, from the vitriol of silver, which is afterwards to be reduced and purified. In the first of these methods, there is a considerable waste of lead and copper; and in the 2d, the quantity of vitriolic acid employed is very great, as much more is dissipated in the form of volatile vitriolic, or sulphureous acid, than remains in the composition of the 2 vitriols.

Some years ago I communicated to an artist the method of effecting the separation of silver and copper by means of the above-mentioned compound of vitriolic acid and nitre; and, as I am informed, that it is now commonly practised by the manufacturers in Birmingham, I have no doubt but it is much more economical, and it is certainly much more easily executed, than any of the other methods: for nothing more is required than to put the pieces of plated metal into an earthen glazed pan; to pour on them some of the acid liquor, which may be in the proportion of 8 or 10 lbs. of oil of vitriol to 1 lb. of nitre; to stir them about, that the surfaces may be frequently exposed to fresh liquor, and to assist the action by a gentle heat from 100° to 200° of Fahrenheit's scale. When the liquor is nearly saturated, the silver is to be precipitated from it by common salt, which forms a luna cornea, easily reducible by melting it in a crucible with a sufficient quantity of pot-ash; and lastly, by refining the melted silver, if necessary, with a little nitre thrown on it. In this manner the silver will be obtained sufficiently pure, and the copper will remain unchanged. Otherwise, the silver may be precipitated in its metallic state, by adding to the solution of silver a few of the pieces of copper, and a sufficient quantity of water to enable the liquor to act on the copper. The property which this acid mixture possesses of dissolving silver with great facility, and in considerable quantity, will probably render it a useful menstruum in the separation of silver from other metals; and as the alchemists have distinguished the peculiar solvent of gold under the title of aqua regis, a name sufficiently distinctive, though founded on a fanciful allusion; so, if they had been acquainted with the properties of this compound, they would probably have bestowed on it the appellation of aqua reginæ.

§ 3. *The change of properties communicated to the mixture of vitriolic and nitrous acids by phlogistication.*—20. The above-described compound acid may be phlogisticated in different methods, of which I shall mention 3. 1st, By digesting the compound acid with sulphur by means of the heat of a water-bath, the liquor dissolves the sulphur with effervescence, loses its property of yielding white fumes; and if the quantity of sulphur be sufficient, and if the heat applied be long enough continued, it exhibits red nitrous vapours, and assumes a violet colour.

2dly, If, instead of dissolving nitre in concentrated vitriolic acid, this acid be impregnated with nitrous gas, or with nitrous vapour, by making this gas, or vapour pass into the acid, this compound will be phlogisticated, as it contains not the entire nitrous acid, but only its phlogisticated part, or element, the nitrous gas, without the proportion of pure air is necessary to constitute an acid. This impregnation of oil of vitriol with nitrous gas, or nitrous vapour, was first described, and some of the properties of the impregnated liquor noticed, by Dr. Priestley. See Exp. and Obs. on Air, vol. 3, p. 129 and 217. 3dly, By substituting nitrous ammoniac instead of nitre in the mixture with oil of vitriol.

21. The compound prepared by any of these methods, but especially by the 1st and 2d, differs considerably in its properties with regard to its action on metals from the acid described in the first section. It has been observed, that the latter compound has little action on any metals but silver, tin, mercury, and nickel. On the other hand, the phlogisticated compound not only acts on these,

but also on several others. It forms with iron a beautiful rose-coloured solution, without application of any artificial heat; and in time a rose-coloured saline precipitate is deposited, which is soluble in water with considerable effervescence. It dissolves copper, and acquires from this metal, and also from regulus of cobalt, zinc, and lead, pretty deep violet tinges. Bismuth and regulus of antimony were also attacked by this phlogisticated acid. To ascertain more exactly the effects of this phlogisticated acid on some metals, I made the following experiments, with a liquor prepared by making nitrous gass pass through oil of vitriol during a considerable time.

22. To 200 grain-measures of the oil of vitriol impregnated with nitrous gas, put into a retort with a long neck, the capacity of which, including the neck, was 1150 grain-measures, I added 144 grs. of standard silver, and immersed the mouth of the retort in water, under an inverted jar filled with water, to catch the gas which might be extricated. The acid began to dissolve the silver with effervescence without application of heat; the solution became of a violet colour, and the quantity of nitrous gas received in the inverted jar was 14700 grain-measures. On weighing the silver remaining, the quantity which had been dissolved was found to be 70 grs. When water was added to the solution, an effervescence appeared, but only a very small quantity of gas was extricated. By means of the water, a white saline powder of silver, soluble in a larger quantity of water, was precipitated from the solution. The solution of silver, when saturated and undiluted, congeals readily in cool temperatures, and, when diluted to a certain degree with water, gives foliated crystals.

23. In the same apparatus, and in the same manner, 100 grain-measures of this impregnated oil of vitriol were applied to iron. An effervescence appeared without application of heat, the surface of the iron acquired a beautiful rose colour or redness mixed with purple: and this colour gradually pervaded the whole liquor, but disappeared on keeping the retort some time in hot water. Notwithstanding a considerable apparent effervescence, the quantity of air expelled in the inverted jar was only 400 grain-measures, of which $\frac{1}{4}$ was nitrous, and the rest phlogisticated. The solution was then poured out of the retort, and the iron was found to have lost only 2 grs. in weight. The solution was returned into the retort, without the iron, and 200 grs. of water were added to it; on which a white powder was immediately precipitated, which re-dissolved with great effervescence. When 2000 grain-measures of nitrous gas had been expelled in the inverted jar, without application of heat, the retort was placed in the water-bath, the heat of which rendered the effervescence so strong, that the liquor boiled over the neck of the retort, so that the quantity of gas extricated could not be ascertained.

24. In the same manner 11 grs. of copper were dissolved in 100 grain-

measures of impregnated oil of vitriol. The solution was of a deep violet colour, and at last was turbid. The quantity of nitrous gas expelled into the inverted jar during the operation was 4700 grain-measures. When the copper was removed, and 200 grs. of water were added to the solution, an effervescence took place, 1700 grain-measures of nitrous gas were expelled, and the solution then acquired a blue colour.

25. In the same apparatus and manner, 100 grain-measures of the impregnated oil of vitriol were applied to tin, which was thence diminished in weight 16 grs., while the liquor acquired a violet colour, became turbid by the suspension of the calx of tin, and a quantity of nitrous gas was thrown into the inverted receiver equal to 4100 grain-measures, without application of heat, and another quantity equal to 4900 grain-measures, after the retort was put into the water-bath.

26. Mercury added to the impregnated oil of vitriol formed a thick white turbid liquor, which was rendered clear by addition of unimpregnated oil of vitriol. In a little time this mixture continuing to act on the remaining mercury acquired a purple colour. The mercury acted on sunk to the bottom of the glass in the form of a white powder, and the purple liquor, when mixed with a solution of common salt in water, gave no appearance of its containing any mercury in a dissolved state.

27. The nitrous gas with which the oil of vitriol is impregnated shows no disposition to quit the acid by exposure to air; but, on adding water to the impregnated acid, the gas is expelled suddenly with great effervescence, and with red fumes, in consequence of its mixture with the atmospherical air. On adding 240 grs. of water to 60 grain-measures of impregnated oil of vitriol, 2300 grs. of nitrous gas were thrown into the receiver; but as the action of the 2 liquors is instantaneous, the quantity of gas expelled from the retort before its neck could be immersed in water, and placed under the receiver, must have been considerable. The whole of the gas however was not extricated by means of the water, for the remaining liquor dissolved 5 grs. of copper, while 800 measures of nitrous gas were thrown into the retort.

28. The following facts principally are established by the preceding experiments. 1. That a mixture of the vitriolic and nitrous acids in a concentrated state has a peculiar faculty of dissolving silver copiously. 2. That it acts on, and principally calcines, tin, mercury, and nickel; the latter of which however it dissolves in small quantity: and that it has little or no action on other metals. 3. That the quantity of gas produced while the metal is dissolving is greater, relatively to the quantity of metal dissolved, when the proportion of nitre to the vitriolic acid is small, than when it is large; and that when the metals are dissolved by mixtures containing much nitre, and with a small production of gas,

the solution itself, or the metallic salt formed in it, yields abundance of gas when mixed with water. 4. That dilution with water renders the concentrated mixture less capable of dissolving silver, but more capable of acting on other metals. 5. That this mixture of highly concentrated vitriolic and nitrous acids acquires a purple or violet colour when phlogisticated, either by addition of inflammable substances, as sulphur, or by its action on metals, or by very strong impregnation of oil of vitriol with nitrous gas *. 6. That this phlogistication was found to communicate to the mixture the power of dissolving, though in small quantities copper, iron, zinc, and regulus of cobalt. 7. That water expels from a highly phlogisticated mixture of concentrated vitriolic and nitrous acids, or of oil of vitriol impregnated with nitrous gas, a great part of its contained gas; and that therefore this gas is not capable of being retained in such quantity by dilute as by concentrated acids. Water unites with the mixture of oil of vitriol and nitre, without any considerable effervescence.

29. To these observations I shall subjoin one other fact, namely, that when, to the mixture of oil of vitriol with nitre, a saturated solution of common salt in water is added, a powerful aqua regis is produced, capable of dissolving gold and platina; and this aqua regis, though composed of liquors perfectly colourless and free from all metallic matter, acquires at once a bright and deep yellow colour. The addition of dry common salt to the concentrated mixtures of vitriolic and nitrous acids produces an effervescence, but not the yellow colour; for the production of which therefore a certain proportion of water seems to be necessary.

PART 2. *On the precipitation of silver from nitrous acid by iron.*

§ 1. Bergman relates, that on adding iron to a solution of silver in the nitrous acid, no precipitation ensued; though the affinity of iron to acids in general is known to be much stronger than that of silver; and though, even with regard to the nitrous acid, other experiments evince the superior affinity of iron: for as iron precipitates copper from this acid, and as copper precipitates silver, we must infer the greater affinity of iron than of silver. In the course of his experiments however, some instances of precipitation occurred, which he attributed to the peculiar quality of the irons which he then employed †. I was desirous of dis-

* Dr. Priestley has noticed this colour communicated to oil of vitriol by impregnation with nitrous gas or vapour, and also the effervescence produced by adding water to this impregnated liquor. See Exp. and Obs. vol. 3, p. 129 and 217.—Orig.

† Bergman tried many different kinds of iron, and he thought he found 2 that were capable of precipitating silver. But as he did not discover the circumstances according to which this precipitation sometimes does, and at other times does not happen, he may have been mistaken with regard to the peculiar quality of these 2 kinds of iron. At least the several kinds which I have tried always precipitated silver in certain circumstances, and always failed to precipitate in certain other circumstances. I do not know any other author who has mentioned this subject, excepting Mr. Kirwan; who, in the conclusion of his valuable papers on the Attractive Powers of Mineral Acids, says, " I have always found silver to be easily precipitated from its solution in the nitrous acid by iron. The sum of the

covering the circumstances, and of investigating the cause, if I should be able, of this irregularity and exception to the generally received laws of affinity.

2. I digested a piece of fine silver in pure and pale nitrous acid, and while the dissolution was going on, and before the saturation was completed, I poured a portion of the solution on pieces of clean and newly-scraped iron wire into a wine glass, and observed a sudden and copious precipitation of silver. The precipitate was at first black, then it assumed the appearance of silver, and was 5 or 6 times larger in diameter than the piece of iron wire which it enveloped. The action of the acid on the iron continued some little time, and then it ceased; the silver re-dissolved, the liquor became clear, and the iron remained bright and undisturbed in the solution at the bottom of the wine glass, where it continued during several weeks, without suffering any change, or affecting any precipitation of the silver.

3. When the solution of silver was completely saturated, it was no longer affected by iron, according to Bergman's observation.

4. Having found that the solution acted on the iron, and was thus precipitated, before it had been saturated, and not afterwards, I was desirous of knowing, whether the saturation was the circumstance which prevented the action and precipitation. For this purpose I added to a portion of the saturated solution some of the same nitrous acid, of which a part had been employed to dissolve the silver; and into this mixture, abounding with a superfluous acid, I threw a piece of iron, but no precipitation occurred. It was thence evident, that the saturation of the acid was not the only circumstance which prevented the precipitation.

5. To another portion of the saturated solution of silver I added some red smoking nitrous acid; and I found, on trial, that iron precipitated the silver from this mixture, and that the same appearances were exhibited as had been observed with the solution before its saturation.

quiescent affinities being 625, and that of the divellent 746. Yet Mr. Bergman observed, that a very saturated solution of silver was very difficultly precipitated, and only by some sorts of iron, even though the solution was diluted, and an access of acid added to it. The reason of this curious phenomenon appears to me deducible from a circumstance first observed by Scheele, in dissolving mercury, namely, that the nitrous acid when saturated with it will take up more of it in its metallic form. The same thing happens in dissolving silver in the nitrous acid in a strong heat; for, as I before remarked, the last portions of silver thrown in afford no air, and consequently are not dephlogisticated. Now this compound of calx of silver, and silver in its metallic form, may well be unprecipitable by iron, the silver in its metallic form preventing the calx from coming into contact with the iron, and extracting phlogiston from it." In this paper I shall not enter into the explanation of these appearances; but I thought it necessary to premise what so eminent a chemist as Mr. Kirwan has suggested on the subject, that the reader may see at once the present state of the question. I shall only remark, that the above explanation, not being founded on any peculiarity in the nature of iron, seems to suppose that the silver is also incapable of being precipitated from such solutions as iron cannot act on by any other metal. But this is not the case: copper and zinc readily precipitate silver from these solutions.—Orig.

6. The same effects were produced when vitriolic acid was added to the saturated solution of silver, and iron afterwards applied.

7. To some of the same nitrous acid, of which a part had been employed to dissolve the silver, I added a piece of iron; and while the iron was dissolving I poured into the liquor some of the saturated solution of silver; on which a precipitation of silver took place instantly; though, when the same acid had been previously mixed with the solution of silver, and the iron was then added to the mixture, no precipitation had ensued.

8. The quantity of vitriolic acid, or of the red fuming nitrous acid, necessary to communicate to the saturated solution of silver the property of being acted on by iron, varies according to the concentration, and to the degree of phlogistication of the acids added; so that a less quantity than is sufficient does not produce any apparent effect. Yet, when the solution of silver is by addition of these acids brought nearly to a precipitable state, the addition of spirit of wine will, in a little time, render it capable of acting on iron.

9. It appears then, that a solution of silver is not precipitated by iron in cold, unless it have a superabundance of phlogisticated acid.*

10. Heat affects the action of a solution of silver on iron: for if iron be digested with heat, in a perfectly saturated solution of silver, such as a solution of crystals of nitre of silver in water, the silver will be deposited in its bright metallic state on different parts of the iron, and the iron which has been acted on by the solution appears in form of a yellow ochre.

11. Bergman relates, that he has sometimes observed beautiful crystallizations or vegetations of metallic silver formed on pieces of iron immersed long in a solution of silver. I have found that no time is able to effect this deposition, unless the solution be in a state nearly sufficiently phlogisticated to admit of a precipitation by iron, but not completely phlogisticated enough to effect that purpose immediately.

12. Dilution with a great deal of water seemed to dispose the solutions of silver to be precipitated by iron more easily. A solution of silver, which did not act on iron, on being very much diluted, and having a piece of iron immersed

* It was said, at § 4, that the addition of dephlogisticated nitrous acid to a saturated solution of silver did not render this solution precipitable by iron. Yet, as this acid dissolves iron, such a quantity may be added, as to overcome the counteracting quality of the solution of silver, so that the acid shall be able to act on the iron; and while this metal is dissolving, it phlogisticates the mixture, which then becomes capable of being precipitated, and is in fact reduced to the same circumstances as are described at § 7. The limits of the quantities which produce changes cannot be ascertained, because they depend on the degrees of concentration and phlogistication of the substances employed; and therefore, whenever a change is said to be produced by a certain substance, it means that it may be produced by some proportion, but does not imply by every proportion, of that substance. Without attending to these considerations, persons trying to repeat the experiments mentioned in this paper will be liable to be deceived.—Orig.

in it, during several hours, gave a precipitate of silver in the form of a black powder.

§ 2. *On the alterations which iron or its surface undergoes by the action of a solution of silver in nitrous acid, or of a pure concentrated nitrous acid.*—13. It has been said, that when iron is exposed to the action of a phlogisticated solution of silver, it instantly precipitates the silver, is itself acted on or dissolved by the acid solution during a certain time, longer or shorter, according to the degree of phlogistication, quantity of superabundant acid, and other circumstances, and that at length the solution of the iron ceases; the silver precipitate is re-dissolved, if there is superfluous acid; the liquor becomes clear again, but only rendered a little browner by its having dissolved some iron; while the piece of iron remains bright and undisturbed at the bottom of the liquor, where it is no longer able to affect the solution of silver.

14. I poured a part of the phlogisticated solution of silver which had passed through these changes, and which had ceased to act on the piece of iron, into another glass, and dropped another piece of fresh iron wire into the liquor; on which I observed a precipitation of silver, a solution of part of the iron, a re-dissolution of the precipitated silver, and a cessation of all these phenomena, with the iron remaining bright and quiet at the bottom of the liquor, as before. It appeared then, that the liquor had not lost its power of acting on fresh iron, though it ceased to act on that piece which had been exposed to it.

15. To one of the pieces of iron which had been employed in the precipitation of a solution of silver, and from which the solution, no longer capable of acting on it, had been poured off, I added some phlogisticated solution of silver which had never been exposed to the action of iron, but no precipitation happened. It appeared then, that the iron itself, by having been once employed to precipitate a solution of silver, was rendered incapable of any further action on any solution of silver. And it is to be observed, that this alteration was produced without the least diminution of its metallic splendour, or change of colour. The alteration however was only superficial, as may be supposed; for by scraping off its altered coat, it was again rendered capable of acting on a solution of silver. To avoid circumlocution, I shall call iron thus affected, altered iron; and iron which is clean, and has not been altered, fresh iron.

16. To a phlogisticated solution of silver, in which a piece of bright altered iron lay, without action, I added a piece of fresh iron, which was instantly enveloped with a mass of precipitated silver, and acted on as usual; but, what is very remarkable, in about a quarter of a minute, or less, the altered iron suddenly was covered with another coat of precipitated silver, and was now acted on by the acid solution like the fresh piece. In a little time the silver precipitate was re-dissolved, as usual, and the two pieces of iron were reduced to an altered state.

When a fresh piece of iron was then held in the liquor, so as not to touch the two pieces of altered iron, they were also soon acted on by the acid solution, and suddenly covered with silver precipitate as before; and these phenomena may be repeated with the same solution of silver, till the superfluous acid of the solution becomes saturated by the iron, and then the re-dissolution of the precipitated silver must cease.

17. I poured some dephlogisticated nitrous acid on a piece of altered iron, without any action ensuing, though this acid readily acted on fresh iron; and when, to the dephlogisticated nitrous acid, with a piece of altered iron lying immersed in it, I added a piece of fresh iron, this immediately began to dissolve, and soon afterwards the altered iron was acted on also by the acid.

18. On a piece of altered iron I poured a solution of copper in nitrous acid; but the copper was not precipitated by the iron; neither did this iron precipitate copper from a solution of blue vitriol.

19. Altered iron was acted on by a dilute phlogisticated nitrous acid; but not by a red concentrated acid, which is known to be highly phlogisticated.

20. I put some pieces of clean fresh iron wire into a concentrated and red fuming nitrous acid. No apparent action ensued; but the iron was found to be altered in the same manner as it is by a solution of silver; that is, it was rendered incapable of being attacked either by a phlogisticated solution of silver, or by dephlogisticated nitrous acid.

21. Iron was also altered by being immersed some little time in a saturated solution of silver, which did not show any visible action on it.

22. The alteration thus produced on the iron is very superficial. The least rubbing exposes some of the fresh iron beneath the surface, and thus subjects it to the action of the acid. It is therefore with difficulty that these pieces of altered iron can be dried, without losing their peculiar property. For this reason, I generally transferred them out of the solution of silver, or concentrated nitrous acid, into any other liquor, the effects of which I wanted to examine. Or they may be transferred first into a glass of water, and thence into the liquor to be examined. But it is to be observed, that if they are allowed to remain long in the water, they lose their peculiar property or alteration. They may be preserved in their altered state by being kept in spirit of sal ammoniac.

23. To a saturated solution of copper in nitrous acid, which was capable of being readily precipitated by fresh iron, I added some saturated solution of silver. From this mixture a piece of fresh iron neither precipitated silver nor copper: nor did the addition of some dephlogisticated nitrous acid effect this precipitation.

24. A solution of copper, formed by precipitating silver from nitrous acid by means of copper, was very reluctantly and slowly precipitated by a piece of fresh iron; and the iron thus acted on by the acid was changed to an ochre.

25. A saturated solution of silver having been partly precipitated by copper, acquired the property of acting on fresh iron, and of being precipitated by it.

26. Fresh iron immersed some time in solutions of nitre of lead, or of nitre of mercury in water, did not occasion any precipitation of the dissolved metals; but acquired an altered quality. These metals then in this respect resemble silver.

27. It is well known, that a solution of martial vitriol, added to a solution of gold in aqua regis, precipitates the gold in its metallic state. I do not recollect, that the precipitation of a solution of silver by the same martial vitriol has been observed. However, on pouring a solution of martial vitriol into a solution of silver in the nitrous acid, a precipitate will be thrown down, which acquires in a few minutes more and more of a metallic appearance, and is indeed perfect silver. When the 2 solutions are pretty concentrated, a bright argentine film swims on the surface of the mixture, or silvers the sides of the glass in which the experiment is made. When a phlogisticated solution of silver is used, the mixture is blackened, as happens generally to a solution of martial vitriol, when a phlogisticated nitrous acid is added to it.

I added about equal parts of water to a mixture of a phlogisticated solution of silver and a solution of martial vitriol, in which all the silver had been precipitated, and digested the diluted mixture with heat, by which means most of the precipitated silver was re-dissolved. Bergman has observed a similar re-dissolution of gold precipitated by martial vitriol on boiling the mixture; but he attributes the re-dissolution to the concentration of the aqua regis by the evaporation. As this explanation did not accord with my notions, I diluted the mixture with water, and found that the same re-dissolution occurred both with the solution of silver and with that of gold. But with neither of the metals did I find that the re-dissolution ever took place, unless there had been a superabundant acid in the solutions of gold and silver employed.

28. Mercury is also precipitated in its metallic state from its solution in nitrous acid by a solution of martial vitriol. When the liquor is poured off from the precipitate, this may be changed into running mercury by being dried near the fire.

29. I found also, that silver may be precipitated in its metallic state, from its solution in vitriolic acid, by addition of a solution of martial vitriol. A vitriol of mercury may also be decompounded by a solution of martial vitriol, and the mercurial precipitate, which is a black powder, forms globules, when dried and warmed.

30. Luna cornea is not decompounded by martial vitriol; consequently there is no operation of a double affinity. Yet this luna cornea may be decompounded by the elements of martial vitriol, while they are in the act of dissolution; that is, the silver may be precipitated in its metallic state, by digesting luna cornea with a dilute vitriolic acid, to which some pieces of iron are added. And it is to be observed, that this reduction of the silver and precipitation take place, while

the acid is yet unsaturated. Marine acid and iron applied to luna cornea effect the same reduction of the silver to a metallic state, even when there is more acid than is sufficient for both metals.

The explanation of these phenomena will be attempted in the subsequent papers which I propose to present on this subject to the Society.

XXI. Determination of the Longitudes and Latitudes of some Remarkable Places near the Severn. By Edward Pigott, Esq. p. 385.

Difference of longitudes in time between Greenwich and Frampton-house, deduced from observed meridian transits of the moon's limbs. There were 13 of these observations, from all of which the medium is $14^m\ 32^s$ for the difference of long. between those two places. This method of determining terrestrial longitudes Mr. P. had detailed in the Philos. Trans. vol. 76, and still thinks it cannot be too strongly recommended. The latitude of the same place, taken with an 18-inch quadrant made by Bird, by a medium of 7 observations, was 51° 24′ 58″. The same as given by his father in the Philos. Trans. vol. 71, is 51° 25′ 1″; the mean of both he takes at 51° 25′ 0″, for the latitude of the observatory at Frampton-house.

Having thus settled the position of the Observatory, Mr. P. next proceeds to give the particulars of the trigonometrical operations. He measured the same base 3 times by different methods, the results were 2046, 2042, 2042 feet. As the view from its extremities was very confined, another base of 1861 yards was deduced from it, situated on the high lands that edge the Severn. From the extremities of this 2d base, all the angles were taken with a tolerably good theodolite, on which 2′ might be easily read off. The results here given are the distances from the various places to the western extremity of their base, their perpendicular distances to its meridian, and its distance from these perpendiculars.

Distances in yards,					
Direct.	To the meridian.		To the perpendicular.		
3307	1254	E	3059	N	Frampton-house.
45654	42239	E	17324	S	Brin Hill, the centre.
36928	21853	E	29768	S	Quantock Hill, the east part.
40446	15586	E	37322	S	Land Mark, a tower.
35543	11542	E	33617	S	Watchet Hill, the centre.
21911	1465	E	21862	S	Minehead.
21336	6664	W	20268	S	Porlock, or Huston Point.
30238	23152	W	19450	S	Leemouth.
46264	40398	W	22547	S	Hangman Hill.
2921	2842	W	673	N	St. Donat's Castle.
1564	491	E	1483	N	Llantwit Church.
10140	448	W	10130	N	Llangwynewar Hill, east part.
25126	2299	E	25020	N	A remarkable hill.
3135	2063	E	2361	N	Llanmace Church.
8864	5906	E	6609	N	St. Hilary's Church.

The direct distances are the most accurate, the others being affected according to the exactness of the meridian of the west extremity of the base; the direction of which was found by the variation needle, its declination having been determined at Frampton-house, and therefore sufficiently correct; for an error in that angle, even of half a degree, would make a difference of a very few seconds in any of the places observed.

The following are the longitudes and latitudes of the same places, deduced by Gen. Roy's most accurate and useful tables, showing the value of each degree, &c.

Longitudes west of Greenwich, in time. m. s.	Longitudes west of Greenwich, in deg. &c. ° ′ ″	Latitudes North. ° ′ ″	
12 24 −	3 5 58	51 14 56½	Brin Hill, the centre.
13 28 −	3 21 57	51 8 48½	Quantock Hill, east part.
13 47½	3 26 52	51 5 5	Land-mark, a tower.
14 0 +	3 30 1	51 6 55	Watchet Hill, the centre.
14 17½	3 34 23	51 26 44½	St. Hilary's Church.
14 29 −	3 37 12	51 35 49	A remarkable hill.
14 29½	3 37 24	51 24 39	Llanmace Church.
14 31½	3 37 52	51 12 42½	Minehead.
14 32 +	3 38 2	51 25 0 −	Frampton-house.
14 34½	3 38 38	51 24 13	Llantwit Church.
14 36 +	3 39 1	51 23 29	Station, west extremity of the base.
14 37½	3 39 22	51 28 28½	Llangwynewar Hill, east part.
14 45	3 41 15	51 23 49	St. Donat's Castle.
14 57 −	3 44 14	51 13 29½	Porlock or Huston Point.
15 48½	3 57 7	51 13 54	Leemouth.
16 42½	4 10 35	51 12 22	Hangman Hill.

XXII. Experiments and Observations on the Matter of Cancer, and on the Aërial Fluids extricated from Animal Substances by Distillation and Putrefaction; together with some Remarks on Sulphureous Hepatic Air. By Adair Crawford, M. D., F. R. S.* p. 391.

There are several varieties in the colour and consistence of the matter discharged by cancerous ulcers. It is in some cases of a pale ash colour; in others, it has a reddish cast; and in many instances it has more or less of a brown tinge, sometimes approaching nearly to black. Its consistence is for the most part thin; but in the cancerous, as well as in the other malignant ulcers, we frequently meet with a white sordes, which closely adheres to the surface of the sore, and which appears to be scarcely miscible with water. In the same patient the appearance of the discharge is frequently varied by internal remedies, or by external applications; but if we except the temporary variations produced by accidental circumstances, the cancerous ulcer is, in its advanced stage, very generally ac-

* Author of a very ingenious philosophical treatise on Animal Heat.

companied with a peculiar odour more highly fetid and offensive than that which is emitted by other malignant ulcers.

Apprehending that some light might be thrown on the nature of cancerous diseases, by inquiring into the properties of this substance, Dr. C. procured a portion of it from a patient who had for several years been afflicted with a cancer in the breast. Having diffused it through pure water, he divided it into 3 parts, which were put into small glass vessels. To one of these he added a solution of vegetable fixed alkali; to the 2d, a little concentrated vitriolic acid; and to the 3d, syrup of violets. By the vegetable fixed alkali no sensible change was produced: on the addition of the vitriolic acid, the liquor in the 2d glass acquired a deep brown colour, a brisk effervescence took place, and at the same time the peculiar odour of the cancerous matter was greatly increased, and diffused itself to a considerable distance through the surrounding air. The syrup of violets communicated to the liquor in the 3d glass a faint green colour. The cancerous matter used in these experiments had a brownish cast. It had been imbibed by cotton, and kept for some days before the trials were made.

Mr. Geber has shown, that animal substances on their first putrefaction do not effervesce with acids; that after the process has continued for some time, a manifest effervescence takes place; and that this effect again disappears before the putrefaction has ceased. Suspecting that the effervescence in the preceding experiment might have arisen from a change which the matter underwent, in consequence of its having been kept some days before the trial was made, Dr. C. repeated the experiment with a portion of reddish matter recently obtained from a cancerous penis. On the addition of the acid, the liquor, as before, acquired a brown colour, its fetor was much increased, and a manifest effervescence took place, though it was not so considerable as in the former instance. A portion of the same matter diffused through distilled water communicated a blue tinge to tincture of litmus, and a greenish cast to syrup of violets. It is proper to observe, that when syrup of violets was mixed with portions of cancerous matter from a variety of different subjects, the change produced was in some cases scarcely perceptible; but in every instance the presence of an alkali was detected by dipping into the matter a slip of paper that had been previously tinged blue by tincture of litmus, and afterwards slightly reddened by acetous acid. The red colour was invariably in the course of a few minutes abolished, and the blue restored.

The cancerous matter, as has been already remarked, acquired, on the addition of the vitriolic acid, a brown hue. It is well known, that this acid, when it is highly concentrated, communicates a brown or black colour to all animal and vegetable substances. Being desirous of learning whether the change which took place on the addition of the acid to the cancerous matter in this experiment, was different from that which would be produced by the same acid in other animal substances, and particularly in recent healthy pus; Dr. C. took equal quantities of the latter, and of ash-coloured cancerous matter, and having diffused each of them through thrice its weight of distilled water, he added to them equal quantities of concentrated vitriolic acid; the weight of the acid being nearly the same with that of the matter used in the experiment. The mixture containing the pus acquired from the acid a faint brown colour; but that which contained the cancerous matter, was suddenly changed to a deep brown, approaching to black. When these mixtures were diluted with about twice their weight of distilled water, the brown tinge of the former entirely disappeared; but the latter still retained its brown colour, though it was somewhat fainter than it had been on the first addition of the acid.

The aërial fluid which was disengaged in the foregoing trials from the matter of cancer, by the vitriolic acid, appeared from its odour to have a nearer resemblance to hepatic than to any other species of air. As it seemed, from its sensible qualities, to be a very active, and probably a deleterious principle, he endeavoured more particularly to inquire into its nature, and to compare it with common hepatic air. But before relating the trials which were made with that view, it may not be improper briefly to mention the characters by which common hepatic air is distinguished. It has a smell resembling that of rotten eggs; it is inflammable, and during its combustion in the open air, sulphur is deposited; it communicates a black colour to silver and copper, and a brownish tinge to lead and iron; it is soluble in water, and when a solution of nitrated silver is dropped into water impregnated with it, the mixture becomes turbid, and a dark-coloured precipitate falls to the bottom; by the addition of the nitrated silver, the odour of the hepatic air is rendered much fainter; and it is entirely destroyed by concentrated nitrous, or by dephlogisticated marine acid.

To determine whether the aërial fluid contained in the cancerous matter possessed these properties, a portion of this substance was diffused through distilled water. The mixture being filtered, a small quantity of nitrated silver was dropped into it. In a little time, an ash-coloured cloud was produced, which soon afterwards acquired a brownish purple hue, and at the end of 2 hours the colour of the mixture was changed to a deep brown. The fetid smell was now rendered much fainter than that of a similar mixture of cancerous matter, and of distilled water, to which nitrated silver had not been added. When a little concentrated nitrous acid was dropped into the mixture which had been thus altered by the addition of nitrated silver, a slight effervescence took place, the brown hue was instantly changed to an orange colour, and the fetid smell was abolished. The fetor was likewise entirely destroyed, when dephlogisticated marine acid was added either to cancerous matter in its separate state, or to a portion of that substance which had been previously mixed with nitrated silver.

By the foregoing properties the cancerous virus is distinguished from common pus: for when dilute vitriolic acid is added to common pus, no effervescence is produced; and when a solution of nitrated silver is dropped into this substance previously diffused through distilled water, the mixture does not acquire a brown colour; nor does any sensible precipitation take place for several hours. It appeared however, that when the last experiment was repeated with matter obtained from a venereal bubo, the mixture on the addition of the nitrated silver became slightly turbid, and, at the end of 2 hours, it acquired a brownish cast. The same effects were perceived when the trial was made with matter obtained from a carious bone. But in these instances the precipitation was much less considerable than that which was produced by the cancerous matter.

Dr. C. next endeavoured to procure, in its separate state, a portion of the air which is extricated from the matter of cancer by the vitriolic acid. With this intention a quantity of reddish cancerous matter was mixed in a small proof, with about thrice its weight of distilled water. To this mixture a little vitriolic acid was added; on which an effervescence took place, and the air that was disengaged was received in a phial over mercury. When one half of the mercury was expelled from the phial, the latter was inverted over distilled water, and the portion of the mercury that remained in it being suffered to descend, and the water to rise into its place, the phial was closely corked. The air and water were then briskly agitated together, and the phial being a 2d time inverted over distilled water, the cork was removed; when it appeared by the height to which the water rose, that a part of the air had been absorbed. The water contained in the phial was now found to be strongly impregnated with the odour of the cancerous matter, and a little nitrated silver being dropped into it, a purplish cloud, inclining to red, was produced. It is proper to observe, that the change of colour on the addition of the nitrated silver, in this experiment, was at first scarcely perceptible; but in the course of a few minutes it became very distinct. As it might perhaps be doubtful, whether this alteration would not be produced in the nitrated silver by exposure to the air alone, the colour of the mixture was compared with that of a similar mixture of nitrated silver and of pure distilled water, which had remained exposed to the open air for an equal length of time. Though a slight change of colour was produced in the latter instance, yet it was much less considerable than that which took place in the former. In the above recited experiment, the air came over mixed with the common air that was contained in the proof. The quantity of aërial fluid that can be thus extricated by the addition of the acid without the assistance of heat, is not very considerable. If heat be applied, a larger portion of fetid air, having the odour of cancerous matter, may be disengaged; but in that case it will be found to be mixed with vitriolic acid air.

With a view to obtain the former of these fluids in as pure a state as possible, the experiment was repeated in the following manner. A portion of the cancerous virus, diffused through distilled water, was introduced into a small proof; a little vitriolic acid was added; the vessel was filled with distilled water, and a crooked tube, also filled with that fluid, was fixed to its neck. The extremity of the tube being then introduced into the mouth of an inverted bottle containing water, and the flame of a candle being applied to the bottom of the proof, a quantity of air was expelled, which was received in the bottle. This air, when it was first disengaged, rose in the form of white bubbles; it had a very fetid smell, similar to that of the cancerous matter; and the water which was impregnated with it occasioned a dark-brown precipitate in a solution of nitrated silver. The crooked tube being separated from the proof, a very offensive white vapour, resembling in its odour the air extricated during the experiment, arose from the mixture, and continued to ascend for nearly half an hour. When to a portion of this smoking liquor, previously filtered, a little concentrated nitrous acid was added, the fetid smell was entirely destroyed, a slight effervescence took place, and a flaky substance that floated through the mixture was disengaged.

The foregoing experiments prove, in general, that the fetid odour of the matter of cancer is increased by the vitriolic, but entirely destroyed by the concentrated nitrous and dephlogisticated marine acids; that the aërial fluid, which is disengaged by the vitriolic acid, is soluble in water, and that the solution deposits a reddish brown precipitate on the addition of nitrated silver. Whence it follows, that the cancerous matter contains a principle which has many of the properties of hepatic air, and which may perhaps not improperly be termed animal hepatic air. It has also been shown, that the matter of cancer is impregnated with an alkali which is in such a state as to change the colour of vegetable tinctures. Dr. C. had very little doubt that this was the volatile alkali: for it is well known, that putrid animal substances frequently abound with that salt; but have never, he believes, been found to contain a fixed alkali in a disengaged state. With a view however, more decisively to determine this point, he tried the following experiment. A quantity of cancerous matter, diffused through distilled water, was introduced into a glass retort to which a receiver was adapted. The mixture was slowly distilled by means of a sand heat; and a small quantity of the liquor which came over into the receiver being poured into an infusion of Brazil wood, instantly imparted to it a deep red colour. Hence it clearly appears, that the alkali contained in the cancerous matter was the volatile, because it was separated by distillation with a heat which did not exceed that of boiling water.

It seemed extremely probable, that the above-mentioned alkali was united to the aërial fluid with which the matter of cancer is impregnated. Of the truth of this fact he was persuaded by observing, that the smell of the cancerous matter was greatly increased by the addition of the vitriolic acid: for he could scarcely avoid concluding, that this phenomenon arose from an union between the acid and alkali, in consequence of which the odoriferous principle was extricated by a superior attraction. This conclusion will be confirmed by experiments to be recited in the sequel, which prove, that the volatile alkali is capable of entering into a chemical combination with the aërial fluid contained in the matter of cancer.

Of the air extricated from cancerous matter, and from other animal substances, by distillation.—A portion of matter from a cancerous breast was diffused through distilled water, and introduced into a small coated glass retort, which was gradually exposed to heat in a sand bath till the bottom of the retort became red-hot. The neck of the latter was introduced below an inverted jar filled with water, and a quantity of air was received in the jar, which was found to consist of the common air contained in the retort. Two measures of it, mixed with one of nitrous air, occupied the space of a little less than 2 measures. This portion of air was strongly impregnated with the peculiar smell of the cancerous matter. The heat continuing to increase, the water began to boil, and a large quantity of aqueous vapour arose; which, as soon as it came into contact with the common air, produced a white smoke. The smell now perceived was similar to that of fresh animal substances when boiled. The aqueous vapour in this part of the process was not mixed with any permanently elastic fluid.

When the greater part of the water was evaporated, the jar containing the first portion of air was removed, and the neck of the retort was introduced beneath an inverted vessel filled with mercury. Soon after this, a considerable quantity of air, having a fetid smell similar to that of burned bones, was extricated. This aërial fluid was mixed with a yellow empyreumatic oil. A portion of it being agitated with water was found to be partly imbibed by that fluid; and nitrated silver, dropped into the water thus impregnated, produced a reddish precipitate.

One measure of the air, obtained in the foregoing experiment, being mixed over mercury with an equal bulk of alkaline air, the volume of the mixture was found gradually to decrease; and, at the end of 3 hours, the air in the tube occupied the space of only 1 measure and $\frac{8}{10}$. An oily deposit was now made on the inner surface of the tube. At the expiration of 8 days, the interior surface of the tube was covered with slender films, which had a yellowish cast, and which were irregularly spread on it. The upper surface of the mercury within the tube was corroded; in some places it had a reddish burnished appearance; in others, it was changed into an ash-coloured powder, interspersed with brown spots. The tube was now removed from the mercury, and the air that remained in it had a strong fetid smell, resembling that of burnt bones.

It has been already observed, that before the water was entirely evaporated, the vapour had lost the odour of the cancerous matter, and had acquired that of animal substances recently boiled. Hence it appears, that the matter on which the peculiar smell of cancerous ulcers depends, is a very volatile substance, for it escaped at the beginning of the process. It also appears that this volatile substance, which is probably the active principle in the matter of cancer, is not changed, by simple exposure to heat, into a permanently elastic fluid; for the air that escaped at the beginning of the process, though it smelled strongly of the cancerous matter, was found by Dr. Priestley's test to be as pure as common air; and it was evident, that the aqueous vapour which came over in the middle of the process was not mixed with any permanently elastic fluid; because, when this vapour was received in an inverted bottle filled with mercury, it was condensed into water, without any admixture of air. Indeed, if the odoriferous principle in the matter of cancer consist of volatile alkali combined with animal hepatic air, it could not be expected that it should acquire a permanently elastic form by simple exposure to heat; because when alkaline and animal hepatic air unite together, they form a non-elastic substance that condenses on the inner surface of the vessel in which they are mixed.

To discover whether other animal substances yield an aërial fluid, similar to that which was extricated in the foregoing experiment from the matter of cancer by means of heat, a portion of the flesh of the neck of a chicken was introduced into a small coated glass retort, which was gradually exposed to heat in a sand bath till it became red-hot. A thin phlegm, of a yellowish colour, first came over: this was soon succeeded by a yellow empyreumatic oil, and at the same time a permanently elastic fluid, having an odour resembling that of burnt feathers, began to be disengaged. A slip of paper, tinged with litmus and reddened by acetous acid, being held over this fluid, became blue. The neck of the retort was now introduced below an inverted jar filled with mercury, and a considerable quantity of air, together with a fetid empyreumatic oil, were received in the jar. This air was highly inflammable: it had a very fetid odour. When a bottle, containing a portion of it, was agitated with distilled water, nearly one-half of it was absorbed. The residue was inflammable, and burned first with a slight explosion, and afterwards with a blue lambent flame. A little nitrated silver being dropped into the water with which the air had been agitated, the mixture instantly acquired a reddish brown colour; after some time it became turbid, and a brown precipitate fell to the bottom. When 2 measures of the air, extricated in this experiment, were mixed with 1 of alkaline air, they occupied the space of a little more than 1 measure and a half. A 2d measure of alkaline air being added, and the airs being suffered to remain together for 3 days, at the end of that time the residue occupied the space of $2\frac{1}{8}$ measures. Soon after they were mixed, an oily fluid, of a pale colour, was deposited on the internal surface of the jar. At the end of the 3d day this substance had acquired a light olive colour. It was collected in globules, irregularly distributed over the interior surface of the jar. These globules were nearly of a solid consistence. When the jar was removed from the mercury, the air contained in it at first smelled strongly of volatile alkali. After a little time the smell of the alkali disappeared, and the odour of empyreumatic oil was distinctly perceived. A small quantity of distilled water, which was now agitated in the jar, acquired a brown colour, but did not entirely dissolve the viscid substance that adhered to its surface. The water, thus coloured, was divided into 2 portions. To one of these was added a little strong vitriolic acid, by which the smell was exalted, and a slight effervescence was produced. Concentrated nitrous acid being added to the other portion, the smell and colour were destroyed, and a brisk effervescence took place.

When a portion of the solid substance that adhered to the interior surface of the jar was separated, it felt viscid and adhesive between the fingers, and smelled strongly of empyreumatic oil. A little spirit of wine being introduced into the jar, this viscid substance was dissolved; the spirit acquired a yellow colour and empyreumatic smell, and on adding to it distilled water the mixture became whitish and slightly turbid.

Dr. C. next examined the air extricated from putrid veal by distillation. A portion of the latter substance being introduced into a coated glass retort was exposed to a red heat, and the air disengaged was received in a jar over mercury. This aërial fluid was found to possess nearly the same properties with that which was obtained in the preceding experiments. It was very inflammable; about $\frac{1}{2}$ of it was soluble in distilled water. The water, thus impregnated, became turbid on the addition of nitrated silver, and a brown precipitate fell to the bottom. To another portion of distilled water saturated with this fluid, dephlogisticated marine acid being added, the fetid smell was destroyed, a brisk effervescence took place, and a whitish gelatinous substance was separated. This substance being evaporated to dryness, became black on the addition of concentrated vitriolic acid. When a quantity of the air obtained in the experiment was agitated with distilled water till no more was absorbed, the residue took fire on the application of an ignited body, and burned with a lambent flame. The air extricated from the putrid veal had less of the empyreumatic smell than that which was disengaged from fresh animal substances. Its odour indeed was nearly similar to that of animal substances in a state of putrefaction.

We learn from these experiments that the aërial fluids, which are extricated from fresh as well as from putrid animal substances by distillation, have nearly the same properties with that which is disengaged, by a similar process from the matter of cancer. Each of them appears to consist of 2 distinct fluids; one of which is soluble, and the other insoluble, in water. The portion that is insoluble burns with a lambent flame, and has all the characters of heavy inflammable air; whereas the soluble part resembles the fluid which is extricated from cancerous matter by the vitriolic acid: it has a fetid odour, it decomposes nitrated silver, combines with caustic volatile alkali, and possesses many of the properties of common hepatic air.

There are several particulars however, in which the animal and common hepatic air materially differ from each other. Though they are both fetid, yet their odours are not exactly similar. When common hepatic air is decomposed by the concentrated nitrous or dephlogisticated marine acid, sulphur is separated; but when animal hepatic air is decomposed by these acids, a white flaky matter is disengaged which is evidently an animal substance, because it becomes black by the addition of concentrated vitriolic acid. Sulphur is also separated during the combustion of common hepatic with atmospherical air; but when the air from animal substances is burned with atmospherical air, no precipitation of sulphur takes place. Indeed, that animal hepatic air does not contain sulphur will be apparent from the following experiment. Equal parts of pure air and of air extricated from fresh beef by distillation, were fired by the electric shock in a strong glass tube over mercury. A little distilled water was then introduced through the mercury into the tube, and was agitated with the air which it contained. A portion of this water being filtered, and a small quantity of muriated barytes being dropped into it, the mixture remained perfectly transparent. Hence it appears, that the air extricated from fresh beef by distillation does not contain sulphur; for, if it had contained that substance, the sulphur, by its combustion with the pure air, would have been changed into the vitriolic acid, and the muriated barytes would have been decomposed.

The following experiments were made with a view more accurately to analyze the airs which are disengaged from animal substances by heat, and to determine the products resulting from the union of these fluids with pure air. About an ounce of the lean of fresh mutton was introduced into a small coated glass retort, and exposed to a red heat. The air extricated towards the end of the distillation was received over mercury; and soon after its production, being agitated with water, very nearly $\frac{1}{2}$ of it was absorbed. A similar experiment being made with the air disengaged towards the middle of the distillation, the part of it which was soluble in water was found to be to the part not soluble in that fluid, as 2 to 3. Having suffered a separate portion of the air disengaged towards the end of the distillation to remain over mercury for 7 hours, it was found gradually to diminish in bulk, and a fluid, which had the colour and the odour of a thin empyreumatic oil, was collected at the bottom of the jar. The air being now agitated with water, only $\frac{1}{8}$ of it was absorbed. Hence it appears, that a portion of the air, extricated from animal substances by heat, resembles a species of hepatic air which was first discovered by Mr. Kirwan, and which exists in an intermediate state between the aërial and the vapourous; this fluid not being permanently elastic like air, nor immediately condensed by cold like vapour, but gradually assuming the non-elastic form, in consequence probably of the tendency of its several parts to unite with each other. The air produced in the foregoing experiment rendered lime-water turbid; it therefore contained a quantity of fixed air; and towards the end of the distillation a little volatile alkaline air came over, agreeably to the observation of M. Berthollet: for when a portion of the air received during this part of the process was mixed with an equal quantity of marine acid air, a white vapour was produced, and a diminution of about $\frac{1}{25}$ of the whole took place.

Dr. C. endeavoured, by the following experiment, to ascertain the proportion of fixed air contained in the aerial fluid which is disengaged from the lean of animal substances by heat. A quantity of air, extricated from the lean of fresh mutton, was received over mercury in a large phial with a narrow neck. When the phial was a little more than half filled, the remaining portion of the mercury was displaced by introducing water that had been previously boiled. The phial being then closely corked, the air and water were briskly agitated together; and the liquor, thus impregnated with the soluble part of the animal air, was put into a proof, to the bottom of which heat was applied. By this means a portion of the air was again disengaged, which was received in a tube inverted over mercury. The process was continued till the liquor in the proof no longer rendered lime-water turbid. As the air received in the tube contained the fixed air that had been extricated from the liquor, together with a quantity of common air expelled from the proof, it was a 2d time agitated with water; and the exact measure of the fixed air was known by the portion which the water imbibed. The fixed air, thus ascertained, being compared with the entire quantity of air that had been originally absorbed, it appeared, that the former was to the latter in bulk as 1 to 4. Therefore $\frac{1}{4}$ of the volume of the soluble part of animal air consists of fixed air, and the remaining $\frac{3}{4}$ of hepatic, mixed with a very small proportion of alkaline air.

It appeared from the experiment, that animal hepatic air, when it was absorbed by water, was not capable of being again disengaged by a heat which raised the water to the boiling temperature; for, after the fixed air was expelled, the liquor in the proof was made to boil nearly half an hour, but no permanently elastic fluid could be disengaged. The portion of the liquor which now remained had a faint yellow colour; it smelled strongly of animal hepatic air, and deposited a brown precipitate on the addition of nitrated silver. It appears therefore, that the soluble part of the air which is disengaged from the lean of animal substances by heat, consists of 3 distinct fluids; of alkaline air, fixed air, and animal hepatic air. It seemed extremely probable, that these 3 aërial fluids, slowly combining together, formed the oily empyreumatic substance which was collected at the bottom of the jar, while the air was undergoing the diminution described above. This conclusion was confirmed by trials that were made with the empyreumatic oil that came over during the latter part of the distillation: for when it was examined by chemical tests, soon after it was obtained, it was found to contain fixed air, volatile alkali, and animal hepatic air.

Dr. C. next endeavoured to determine the products which result from the combustion of pure air, with animal air, or with the compound aërial fluid extricated from the lean of animal substances by heat. With this intention he exposed the lean of fresh mutton, in a small coated glass retort, to a red heat. The air which was received over mercury towards the end of the distillation, was divided into 2 separate portions; one of which was agitated with water till the soluble part was absorbed; the other was not agitated with that fluid. One measure of the former was introduced, over mercury, into a strong glass tube adapted for the purpose of firing aërial fluids by the electric shock. This was mixed with one measure and a half of pure air. The portion of the tube occupied by the mixture was $1\frac{2}{10}$ inch. A small shock being made to pass through it, a violent explosion took place, and the space occupied by the residue was $\frac{9}{10}$ of an inch. The height of the mercury in the tube, before the combustion, was 4.8 inches. After the airs were fired, its height was 5.1 inches. Allowance being made for the difference of expansion produced by this cause, it appeared that the volumes of the airs, before the combustion, and after it, were as 100 to 75 nearly. The residue being agitated with water, $\frac{6}{10}$ were absorbed; and the portion which was thus absorbed was found, by the precipitation it produced in lime-water, to be fixed air. Of the insoluble remainder, 5 parts being mixed with 5 of nitrous air, a diminution of 3 parts took place; whence it follows, that $\frac{1}{5}$ of the insoluble residue was pure air. The pure air used in this experiment had been previously agitated with water, to free it entirely from fixed air, and the inflammable air had undergone a similar agitation. It is therefore manifest that, by the combustion of the pure and inflammable air in the foregoing trial, fixed air was produced; the phlogisticated air, found in the residue, being that which was contained in the pure air before the inflammation took place.

Dr. C. next examined the products resulting from the combustion of pure air with that portion of the animal air which had not been previously agitated with water. One measure of this fluid, at the expiration of 3 quarters of an hour after it had been obtained, was mixed over mercury with one measure and a half of pure air, and fired by the electric shock. The portion of the tube occupied by the mixture, before the deflagration, was 1 inch and $\frac{15}{100}$; after the deflagration, it occupied the space of 1 inch and $\frac{1}{10}$. Being agitated with lime-water, very nearly $\frac{1}{3}$ was absorbed. A portion of the insoluble residue was exposed to a lighted taper, and burned with a faint blue flame.*

The dephlogisticated air used in this experiment had been previously agitated with water, to free it entirely from fixed air. It was the purest dephlogisticated air he had ever seen: for when 1 measure of it was mixed with 1 measure and $\frac{9}{10}$ of nitrous air, the residue occupied the space of only $\frac{1}{40}$ of a measure. From the foregoing trial it was evident, that $1\frac{1}{2}$ parts of pure air were insufficient to saturate one of the animal air that had not been previously agitated with water. The experiment was therefore repeated as follows. Two parts of pure air being mixed with 1 of animal air, occupied $\frac{8}{10}$ of an inch. The mixture being fired by the electrical shock, the residue stood at a little less than $\frac{5}{10}$. When this residue was agitated with lime-water, it was almost wholly absorbed. By a subsequent trial it was found, that nearly half the animal air used in this experiment was soluble in water. Hence it appears, that the quantity of pure air required to saturate the insoluble part of the animal air, is somewhat less than that required to saturate the compound fluid which had not been previously agitated with water. But the latter fluid has been shown to consist almost entirely of heavy inflammable, animal, hepatic, and fixed air; and as the last of these is already saturated with pure air, it is manifest that the above-mentioned difference must depend on the animal hepatic air. Whence it follows, that the latter contains a large portion of the inflammable principle. From the quantity of fixed air produced in the last of the preceding experiments, there is also the utmost reason to believe, that the basis of heavy inflammable forms one of the constituent parts of animal hepatic air. When equal parts of pure and animal air were burned together, a considerable increase of bulk almost invariably took place; and when the proportion of the animal was to that of pure air as 21 to 15, the bulk of the mixture was increased one half. The air that remained after the combustion in the last-mentioned experiments was inflammable: for a portion of it being introduced into a small phial, and exposed to a lighted candle, it first exploded, and then burned with a blue lambent flame.

Being desirous of learning the cause of the increase of bulk in the foregoing experiments, the following trials were made. Three measures of animal were mixed with 2 of pure air, and several strong electrical shocks were made to pass through the mixture; but it would not take fire. Half a measure of pure air

* When this experiment was first made, the residue did not appear to be inflammable. It had been tried by applying an inflamed slip of paper to the mouth of a phial which was filled with it; but, upon repeating the experiment, when the phial containing the residuary air was carried into a dark room, and an ignited wax taper was applied to its mouth, an evident inflammation took place. —Orig.

was then added, and the mixture being fired, its bulk was increased from .9 of an inch to 1.3 inch.

Three measures of this residuary air were then mixed with 3 of pure air, and fired by the electric shock. The bulk of the mixture was reduced from 1 inch to .56. This being agitated with lime-water, $\frac{2}{3}$ were absorbed, and the remainder consisted almost wholly of pure air. From these facts it seems probable, that animal hepatic air consists of a combination of heavy and light inflammable air; and that when it is fired with a quantity of pure air not sufficient to saturate it, a portion of the animal air is resolved into its elementary principles, in consequence of which its bulk is increased.

Dr. C. was next desirous of learning whether an increase of size would be produced by making the electric shock pass through a mixture of pure and alkaline air. Having first accidentally taken 2 or 3 small shocks through a little alkaline air, and not observing a sensible augmentation of bulk, he then mixed it with an equal volume of pure air; and, as he supposed that no decomposition had taken place, he was not apprehensive of an explosion. Contrary however to expectation, the airs, when the electric shock was made to pass through them, entered rapidly into a union with each other. The jar which he held loosely in his hand, as it was inverted over the mercury, was carried obliquely upwards with great violence. Having broken the stand of the prime-conductor in its passage, it forced its way through the cylinder of the electrical machine, which it shivered to a thousand pieces. Dr. C. afterwards repeated this experiment with a very strong apparatus, the jar being pressed down by a plate of iron, for the purpose of retaining it in its place. It appeared, that when the alkaline and pure air were immediately mixed together, and a small shock was made to pass through them, they would not take fire; but when 3 or 4 shocks were previously taken through the alkaline air, and the latter was afterwards mixed with an equal bulk of pure air, they exploded with great violence. The residue, having cooled to the temperature of the surrounding air, was reduced to half the original bulk of the mixture. Of this residue $\frac{1}{6}$ was undecomposed alkaline air. The remainder was phlogisticated air.

Of the products which result from the combustion of sulphureous hepatic with pure air.—The hepatic air employed in the following experiments was procured, agreeably to the method which Mr. Kirwan has recommended, by adding marine acid to an artificial combination of sulphur and iron. Three measures of the air thus obtained were mixed in a strong glass tube over mercury, with 4 of pure air, and fired by the electric shock.

The pure air was previously agitated with lime-water to free it from fixed air, and a portion of the hepatic air, having been likewise agitated with lime-water, was found not to occasion any precipitation in that fluid. The airs were reduced by the explosion to $\frac{1}{4}$ of their original bulk. The residue was then transferred over mercury into a slender graduated tube, and distilled water being admitted, $\frac{8}{10}$ were absorbed. To a portion of this water, when filtered, vitriolated silver was added, which instantly occasioned a copious precipitate. To a 2d portion was added muriated barytes, which occasioned a slight white precipitate not re-dissolvable in a large quantity of water; lime-water being added to a 3d portion, did not produce any sensible precipitation. From the last fact it does not follow, that no fixed air existed in the residue, because the marine acid, which it evidently contained, would dissolve the calcareous earth of the lime-water. As a great diminution however resulted from the combustion; and as it appeared from chemical tests, that the residue was mostly composed of marine and vitriolic acid airs, it is manifest that if any fixed air was produced, its quantity must have been very inconsiderable.

It has been already observed, that a slight precipitation took place on the addition of the muriated barytes. The precipitate was much more considerable when, on repeating the experiment, the residue after the explosion was not transferred into a graduated tube before the admission of the distilled water; but the latter was immediately introduced into the vessel in which the airs were fired. The reason of this difference is evident. The slight precipitate by the muriated barytes, in the first instance, depended on the existence of a small quantity of vitriolic acid in an aërial form, or in the state of volatile vitriolic acid; which was transferred together with the phlogisticated and marine acid air into the 2d tube; but the greater part of the vitriolic acid produced by the combustion adhered, in a fixed state, to the surface of the tube in which the airs were fired; and therefore, when the distilled water was immediately introduced into this tube, a copious precipitate was deposited on the addition of muriated barytes. Hence it appears, that when pure air and sulphureous hepatic air, obtained from artificial pyrites by the marine acid, are fired together in the above proportions, the products are fixed vitriolic acid, together with a small quantity of the volatile vitriolic and marine acids, in an aërial form. The residue, which the distilled water did not absorb, was the phlogisticated air that existed in the pure air before the combustion.

From subsequent trials it appeared, that when hepatic and pure air were fired in equal bulks, the residue had a strong odour of volatile vitriolic acid, and also contained a small proportion of undecomposed hepatic air. These facts seem to prove, that the conversion of sulphur into volatile or fixed vitriolic acid depends on the quantity of pure air with which it is supplied. The marine acid air, found in this experiment, did not appear to form one of the constituent principles of the hepatic air, but to be merely diffused through it; for it was almost wholly separated, by means of distilled water, from a different portion of the same air, which was placed in a tube inverted over mercury; the water having a stronger attraction to the marine acid than to the hepatic air.

By the following experiment Dr. C. endeavoured to determine whether vitriolic acid be produced by the combustion of hepatic with atmospherical air. One measure of hepatic air, obtained from artificial pyrites, was mixed over mercury with about 6 measures of atmospherical air, and fired by the electric shock. A copious precipitation of sulphur took place, the remaining air was then agitated with distilled water, the latter was filtered, and muriated barytes was added, which produced a white precipitate not dissoluble in a large quantity of water. From this, and the foregoing experiment it appears, that when sulphureous hepatic is burned with atmospheric air, a part of the sulphur is changed into vitriolic acid, and the rest is precipitated; but when it is burned with a sufficient quantity of pure air, the sulphur is wholly converted into vitriolic acid. Agreeably to this conclusion, the odour of the volatile vitriolic acid constantly accompanies the combustion of hepatic with common air in open vessels; and when concentrated nitrous acid is added to water impregnated with hepatic air, the filtered liquor becomes turbid on the addition of muriated barytes.

The quantity of pure air required to saturate sulphureous hepatic air, does not appear to correspond with the supposition that the last of these fluids consists of sulphur dissolved in light inflammable air: for sulphur, in order to its complete saturation, requires only 1.43 times its weight of pure air; but light inflammable air requires for its saturation at least 6 times its weight of that fluid. The specific gravity of hepatic air, as determined by Mr. Kirwan, is nearly equal to that of pure air. If therefore $\frac{1}{6}$ of the weight of hepatic consisted of light inflammable air, that fluid would require for its saturation 2.26 times its bulk of pure air: for the portion of it which consisted of light inflammable air would require a quantity of pure air equal in bulk to the hepatic; and the remaining portion, consisting of sulphur, would require a quantity equal to 1.26 of the hepatic. The entire quantity of pure air would therefore be to that of the hepatic as 2.26 to 1. If the hepatic contained $\frac{1}{12}$ of its weight of light inflammable air, it would require for its saturation 1.64 of its bulk of pure air. But from the foregoing experiments it appears, that the quantity of pure air, necessary to saturate 1 measure of hepatic air, is only 1.33 measures. Hence it is probable that this fluid does not consist of sulphur dissolved in light inflammable air.

If we make allowance for the marine acid which was diffused through the hepatic air, it will be found, that the quantity of pure air required to saturate it, is nearly the same with that which would be required to change an equal weight of sulphur into vitriolic acid. Whence it may be inferred, agreeably to the opinion of Mr. Kirwan, that hepatic air is sulphur which has acquired an aërial form by the application of heat. This conclusion is, he thinks, confirmed by the following experiment. A little pure sulphur was introduced into an inverted tube, which had been previously filled with mercury, and the flame of a candle was applied to the extremity of the tube. In a short time a permanently elastic fluid was produced, which was found to have all the characters of hepatic air. It is probable however, that some degree of moisture is necessary to the success of this experiment, because the quantity of hepatic air which was thus obtained was not very considerable.

It has been already shown, that an oily matter was produced by the union between fixed air, volatile alkali, and animal hepatic air. The following experiment proves, that a substance, which has very much the appearance of oil, is formed by the combination of sulphureous hepatic air with fixed air and volatile alkali. A quantity of impure hepatic air was obtained by adding vitriolic acid to common liver of sulphur. When this fluid was agitated with lime-water, it produced a copious precipitation. It therefore contained a considerable proportion of fixed air. One measure of it was now introduced into a slender graduated tube, inverted over mercury, and was mixed with an equal bulk of alkaline air. As soon as the airs came into contact with each other, a white cloud was produced, the mercury began gradually to rise in the tube, and at the end of 6 hours the air that remained occupied the space of only 1 measure and $\frac{1}{3}$. The surface of the mercury within the tube first became black, and a part of it afterwards acquired a red colour resembling cinnabar. In the course of the experiment, a yellowish oleaginous substance was deposited on the interior surface of the tube. This substance, in some parts of the surface, formed itself into globules; in others it was extended into ramifications, having the resemblance of trees in miniature, and it gradually assumed a deeper colour, till at length it acquired a greenish cast. The substance, thus obtained, had a very fetid odour: it appeared to have a near resemblance to an animal oil which had become green by putrefaction. It was however soluble in water, and the odour of the solution was increased by the vitriolic, and destroyed by the concentrated nitrous and dephlogisticated marine acids.

Mr. Cruikshank, who assisted in most of the foregoing experiments, and on whose accuracy he could place the greatest reliance, examined, in Dr. C.'s absence, the red and black powders formed by the action of the hepatic air on the surface of the mercury, and found them to be æthiops mineral, and cinnabar.

Of the air extricated from animal substances by putrefaction.—In the beginning of July, 1789, about 2 ounces of veal, slightly putrid, was introduced into a large phial, filled with distilled water, and inverted over a quantity of the same fluid. At the end of 3 days a few bubbles of air had appeared at the bottom of the phial; the water had acquired a light brown colour, and emitted a fetid smell. At the expiration of 7 days we could perceive that the quantity of air at the bottom of the phial was manifestly increased, though its progress was very slow. The water, by the dissolution of a part of the veal, had now acquired the consistence of a thin mucus, its brown colour was somewhat deepened, and it emitted a highly fetid smell. A little nitrated silver being dropped into a portion of this water, previously filtered, a dark brown precipitate was immediately produced. Lime-water, mixed with another portion of it, occasioned an ash-coloured precipitate; and when concentrated nitrous acid was added to a 3d portion, the fetid smell was destroyed, a slight effervescence took place, and a yellow flaky matter was disengaged. At the end of 7 weeks, a quantity of air, amounting to $2\frac{1}{6}$ dram measures was collected in the phial. This air had a fetid odour. Being agitated with water, $\frac{6}{10}$ of it was absorbed. The residue extinguished flame.

Dr. C. next examined the air extricated from veal suffered to putrefy over mercury.

On July 28, 1789, 2 drams and 24 grains of the lean of fresh veal was introduced into a narrow jar, filled with mercury, and inverted over that fluid. At the end of 8 days the air, which was slowly extricated, had communicated a brown colour to the surface of the mercury. On Sept. 13, the quantity of air disengaged was a little more than 2 ounce measures. This fluid had a very fetid smell. Two separate portions of distilled water being saturated with it, the first, on the addition of nitrated silver, deposited a brown precipitate; and the last, when it was mixed with lime-water, produced a brownish ash-coloured cloud. A 3d portion of the air being strongly agitated with distilled water, was reduced to $\frac{1}{16}$ of its original bulk. The residue extinguished flame. The veal which had remained so long in contact with the mercury had not lost its firm texture. Its smell was putrid, but not very offensive.

The quantity of elastic fluid collected in this experiment was much greater than in the preceding one; because in the preceding experiment, though the putrefaction advanced more rapidly, yet the fixed and hepatic air were absorbed by the water nearly as fast as they were disengaged from the putrid substance. Hence it appears, that the aërial fluids, which are extricated from the mucular fibres of animals by putrefaction, consist of fixed and animal hepatic, mixed with a very small proportion of phlogisticated air.*

Of the effects produced by exposing fresh animal substances to atmospherical, hepatic, and pure air.—Two tubes, of nearly the same size, were inverted over mercury. Into one of these was introduced common air, and into the other an equal bulk of hepatic air, obtained from liver of sulphur by the vitriolic acid. Equal quantities of fresh veal, consisting of a mixture of muscular fibres and of

* It may be proper to remark, that I have obtained, by distillation from the green leaves of a cabbage, an aërial fluid, which, in most of its properties, resembles animal hepatic air.—Orig.

fat, and weighing each 1 dram, were then exposed to these airs. At the end of 3 days the piece that was in contact with the common air had not altered its colour or consistence, but smelled a little putrid. The colour of the fatty parts of the piece that was exposed to the hepatic air was changed to a dark green, the muscular fibres were cracked and shrivelled on the surface as if they had been seared with a hot iron, and the whole had acquired a soft consistence.

Similar trials were made with 2 pieces of fresh veal, one of which was exposed over mercury to common air, and the other to air extricated from putrid veal by distillation. The former in 3 days had not changed its appearance; the latter had become green round the edges, and was interspersed with green spots. The surface of the mercury in the jar which contained the last had acquired a brown colour; whereas that of the mercury in the jar which contained the common air was clear and bright. The pieces of veal were suffered to remain in this situation for 6 weeks. After a few days had expired, that which was exposed to the animal air did not appear to suffer any further change. Its colour, which in the course of a week had become brown, continued unaltered, and no dissolution took place. The air at the last was very fetid; it occasioned a copious precipitate in lime-water; it was highly inflammable, and burned with a blue lambent flame. On the contrary, the piece which was exposed to the common air, did not, as has been already observed, so soon lose its fibrous texture, nor so speedily acquire a dark colour, as that which was in contact with the animal air. But the progress of its putrefaction did not appear to stop at the end of a few days, as in the latter instance. It advanced slowly, and at the end of 6 weeks a considerable part of the muscular fibres had run down to a brown liquid. The air in which it was placed now occasioned a copious precipitation in lime-water, and the brown liquid was found to be impregnated with animal hepatic and fixed air; the existence of the latter being known by means of lime-water, and that of the former by its occasioning a dark precipitate in a solution of nitrated silver, as well as by its fetid odour, which was increased by the vitriolic, and destroyed by the concentrated nitrous and dephlogisticated marine acids.

The following experiment was made with a view to determine whether pure air accelerates the progress of putrefaction in animal substances. In the month of Dec. 1789, equal portions of pure and of common air were introduced into 2 equal jars over mercury, in each of which was placed about 2 drams of fresh beef. At the end of a week, the beef which was exposed to the pure air had become highly putrid; but very little change was produced in that which was exposed to the common air.

The facts which have been ascertained by the preceding experiments, appear to lead to the following conclusions respecting the process of putrefaction in the lean of animal substances. The muscular fibres of animals contain fixed and phlogisticated air, the inflammable principle in the state of heavy and of light inflammable air, and a substance which, by means of heat or of putrefaction, is capable of being converted into animal hepatic air. When the muscular fibre, after the death of the animal, is exposed to the pure air of the atmosphere; the latter, by a superior attraction, combining with the heavy inflammable air, produces fixed air, and at the same time furnishes the quantity of heat necessary to the formation of animal hepatic air. The cohesion of the fibre being thus destroyed, the fixed, as well as the light inflammable and phlogisticated air, which enter into its composition, are disengaged, and the 2 latter fluids, uniting with each other, produce the volatile alkali. The alterations which take place in putrefaction are in most respects similar to those which arise from destructive distillation. By exposure to heat the fixed air of the animal fibre is extricated, hepatic air and volatile alkali are produced, and the inflammable principle, not coming into contact with the pure air of the atmosphere, is raised in the form of heavy inflammable air.

He has found, that the fetid odour of animal hepatic air is destroyed by mixing it with pure air, and suffering it to remain in contact with that fluid for several weeks. When it was placed in this situation, it acquired an odour which was not exactly similar to any that he had ever before perceived, but which bore some resemblance to that of inflammable air obtained by dissolving iron in spirit of vitriol. The peculiar smell of animal hepatic air is likewise destroyed by agitating it with vinegar, or with the concentrated vitriolic acid. But the fluids which most speedily produce this effect, are the concentrated nitrous and dephlogisticated marine acids; and these fluids are known to abound with pure air. It is therefore extremely probable, that this alteration depends on a union between the pure air of the latter substances and the animal hepatic air, or some of its constituent parts.

It appears from the experiments which have been recited above, that in cancerous and other malignant ulcers, the animal fibres undergo nearly the same changes which are produced in them by putrefaction, or by destructive distillation. The purulent matter prepared for the purpose of healing the ulcer is, in such cases, mixed with animal hepatic air and volatile alkali. The compound formed by the union of these substances, which may perhaps not improperly be termed hepatised ammonia, decomposes metallic salts, and acts on metals: for we have seen, that when it was placed in a jar over mercury for several days, the surface of the mercury acquired a black colour; and that it instantly occasioned a dark precipitate in a solution of nitrated silver. These facts seem to afford an explanation of the changes produced in metallic salts, when they are applied to malignant ulcers. The volatile alkali combines with the acid of the metallic salt, and the animal hepatic air revives the metal, either by imparting to it the inflammable principle, or by uniting with the pure air which the calx is supposed to contain. The metal, thus revived, is probably in some cases again corroded by the hepatised ammonia, which communicates to it a black colour. Thus we may account for the dark incrustation frequently formed on the tongue and internal fauces, when venereal ulcers of the throat are washed with a solution of corrosive sublimate. And hence also the dark tinge which is frequently communicated by ill-conditioned ulcers to poultices made with a solution of sugar of lead. The action of the hepatised ammonia likewise explains the reason why the probes are frequently corroded when they are introduced into sinuous ulcers, or applied to the surfaces of carious bones. To the same cause it is probably owing, that polished metallic vessels are quickly tarnished, when they are exposed to the effluvia of putrid animal substances.

From the foregoing experiments it also appears, that animal hepatic air imparts to the fat of animals recently killed a green colour; that it renders the muscular fibres soft and flaccid, and increases the tendency to putrefaction. It is therefore a septic principle; and hence it is extremely probable, that the compound of this fluid with volatile alkali, which is found in the matter discharged by the open cancer, produces deleterious effects; for though the mischief in cancerous ulcers seems principally to depend on a morbid action of the vessels, whence the unhealthy state of the matter discharged by such ulcers is supposed to derive its origin, yet from the corrosion of the coats of the larger blood-vessels, and the obstructions in the contiguous glands, there can be little doubt that this matter aggravates the disease. The experiments recited above appear to prove, that the hepatised ammonia is the ingredient which communicates to the cancerous matter its putrid smell, its greater thinness, and in short, all the peculiar properties by which it differs from healthy pus.

From these considerations it was inferred, that a medicine which would decompose the hepatised ammonia, and destroy the fetor of the animal hepatic air, without at the same time increasing the morbid action of the vessels, would be productive of salutary effects. The nitrous acid does not destroy the fetor of hepatic air, unless it be highly concentrated; and in this state it is well known that it speedily corrodes animal substances. But the fetor of hepatic air quickly disappears when it is mixed with the dephlogisticated marine acid, even though the latter be so much diluted with water as to render it a very mild application. Dr. C. has found that this acid, diluted with thrice its weight of water, gives but little pain when it is applied to ulcers that are not very irritable; and in several cases of cancer it appeared to correct the fetor, and to produce a thicker and more healthy pus. It is proper however to remark, that other cases occurred in which it did not seem to be attended with the same salutary effects. Indeed some cancerous ulcers are so extremely irritable, that applications which are at all of a stimulating nature cannot be ventured on with safety. And hence if the observations made on the efficacy of this acid as an external application, should be confirmed by future experience, it must be left to the judgment of the surgeon to determine both the degree of its dilution, and the cases in which it may be employed with advantage.

The dephlogisticated marine acid, as is generally known, has the power of destroying the colour, the smell, and perhaps the taste, of the greater part of animal and vegetable substances. We have seen that it corrects the fetor of putrid flesh. And he has found that, when it is poured in sufficient quantity on hemlock and opium, these narcotics speedily lose their sensible qualities. As it appears therefore to possess the power of correcting the vegetable, and probably many of the animal poisons, it seemed not unlikely that it might be useful as an internal medicine. Conceiving that its exhibition would be perfectly safe, Dr. C. once took 20 drops of it diluted with water. He soon afterwards however felt an obtuse pain, with a sense of constriction, in the stomach and bowels. This uneasiness, notwithstanding the use of emetics and laxatives, lasted for several days, but was at length removed by drinking water impregnated with sulphureous hepatic air. He afterwards found that the manganese, which had been used in the distillation of the acid, contained a small portion of lead.

Dr. Ingenhousz informed Dr. C. that a Dutchman of his acquaintance, some time ago, drank a considerable quantity of the dephlogisticated marine acid: the effects which it produced were so extremely violent, that he narrowly escaped with his life. If therefore this acid should hereafter be employed as an internal medicine, it would be necessary to prepare it by means of manganese that has been previously separated, by a chemical process, from the lead and the other metals with which that substance is usually contaminated.

XXIII. On the Satellites of the Planet Saturn, and the Rotation of its Ring on an Axis. By Wm. Herschel, LL.D., F.R.S. p. 427.

In Dr. H.'s last paper on the planet Saturn, the principal object of which was to give an immediate account of the most interesting phenomena that had occurred till the beginning of November, many things were left unnoticed for want of time to treat of them with sufficient accuracy; but having now before him the whole series of observations from July 18 till Dec. 25, 1789, he enters into a proper examination, assisted by such necessary calculations as then could not conveniently be made.

One of the principal motives which have induced Dr. H. to hasten this inquiry, is the frequent appearance of protuberant and lucid points on the arms of the ring of Saturn. He has mentioned before that such phenomena had been resolved by the situation of satellites that put on these appearances; but as his observations were continued near 2 months afterwards, and as he had from them corrected the epochæ of the old satellites, and improved the tables of the new ones, he found that, besides many of these bright points which were completely accounted for by the calculated places of the satellites, there were also many more mentioned in his journal that would not accord with the situation of any of them.

The question then presented itself very naturally, what to make of these protuberant points? To admit 2 or 3 more satellites by way of solving such phenomena appeared too hazardous an hypothesis; especially as these lucid points, though some of them had a motion, did not seem willing to conform to the criterion he had before used of coming off the ring, and showing themselves as satellites. And yet a suspicion of at least one more satellite would often return; it was even considerably strengthened when he discovered, by means of re-calculating with great precision the whole series of observations, that in the beginning of the season there had been some few mistakes in the names of the satellites, when the observations of them were entered in the journal. In setting them right, which threw a great light on the revolution of the 6th, and more especially on that of the 7th, he found also, that some of the observations which were entered by the name of the 7th satellite could not belong to that, nor to any other known one. It remained therefore to be examined whether there might not be sufficient ground to suspect the existence of an 8th satellite.

In this situation of things, he thought it most advisable to draw out the whole series of observations in a paper, beginning at the 5th satellite, and thus gradually through the 4th, 3d, 2d, 1st, 6th, and 7th, to approach towards the centre of Saturn; that it might appear at last what observations were left unaccounted for. By this means also it may be seen clearly with how scrupulous an attention the identity of every satellite has been ascertained; and with a view to give the strongest satisfaction in this respect, at least one observation of each has been calculated for each night; and the place thus computed is set down in the notes, that it may be compared with the observed one. To facilitate this comparison, he delineated a scheme, in which the orbits of the satellites are drawn in their due proportion. A few words will explain the construction and use of this figure, which, notwithstanding its simplicity, is yet amply sufficient to ascertain the accuracy of every observation.

In each of the orbits, described round the centre of Saturn, at their proportional distances, by way of marking them, is placed the satellite to which it belongs, as it appeared to be situated the 18th of October, 1789; also a graduated circle, the use of which is to find, by means of the tables, the apparent place of a satellite for any given time; or, the apparent situation of the same satellite being given, its real Saturnicentric place may be deduced from it. In the centre of the scheme of course is the planet Saturn, and its ring, expressed by a line which represents the direction of its ansæ; or the ring itself, as it appeared in the telescopes during the months of July, August, September, October, and November, 1789. The 5 lines which are carried on parallel to each other serve to convey the measure of the planet, and its ring, to the orbits of the satellites, as will be seen in several instances that occur hereafter. The graduated circle is divided into degrees, and begins to count from that part of every satellite's orbit beyond the planet, which is intercepted by a plane passing from the eye of the observer, at rectangles to the ring, through the centre of Saturn. Hence it follows, that the point of zero, or 360°, is the same with the geocentric place of the planet in those 4 parts of the orbit of the satellite where the eye is in the plane of the ring, and where it appears the most open; and that, in other places, it may be had by solving one spherical triangle. This is to be understood as relating only to the inner satellites; the 5th, or outermost, requiring a different reduction, on account of its deviation from the plane of the ring. But Dr. H. is inclined to believe, that the surest way of observing the 5th, is to trust only to measures, taken with micrometers which give the distance and angle of position, except in such cases when the eye is nearly in the plane of this satellite's orbit, where the different reductions may be neglected, without bringing on any considerable inaccuracies.

The calculations of the places of all the satellites have been made according to tables which are given at the end of this paper. Their form being very simple, Dr. H. thought it not amiss to communicate them, for the use of those who may wish to enter into a more particular examination of the followiug observations; or to follow the satellites in their orbits at any future time. Dr. H. has deduced the epochæ of all the 7 satellites from his own observations, and they will be found to differ considerably from those given by De la Lande, in the Connoissance des Temps for 1791. But he has not attempted to extend them further than a few years backwards or forwards, as he is not in possession of any observations that could authorize him to undertake such a work. On the contrary he is well convinced, that no tables will give the situation of the satellites accurately, till we have at least established the dimensions of their elliptical orbits, and the motion as well as the situation of their aphelia. The epochæ for 1789 therefore must be considered not as mean ones, but such as respect the orbits of these satellites in their situation during the time of the following observations; and the 2 preceding and 2 following years must be already a little affected with those errors which are the necessary consequence of our not knowing the required elements. Dr. H. flatters himself however, that the observations, which are delivered in this paper, will serve as a beginning to a proper foundation for investigating them. The many conjunctions between the satellites, for instance, will undoubtedly throw some light on the situation and excentricity of their orbits; as it will be found, that the calculated places of these conjunctions require elliptical motions to bring the satellites to such appearances, which, in circular orbits, could not so accurately have taken place. Nor can we ascribe the disagreements to the fault of the observations, since a very few minutes will suffice to determine the time of a conjunction, which never lasts long. For this reason also, he carefully avoided deducing the epochæ from conjunctions, even with the 6th satellite, which moves so rapidly, that at first sight we might think those situations favourable.

The mean motion of the 5 old satellites being sufficiently accurate for the present purpose, Dr. H. has taken from the above-mentioned tables of De la Lande; but those of the 6th and 7th, of course, are the result of his own observations. The geocentric place of Saturn, whose complement is to be added, in order to reduce the Saturnicentric situation of the satellites to the apparent one, he has taken from the nautical almanac to the nearest minute; and, as he had always confined himself to a literal transcription of the observations from the original journal, all the memorandums which are necessary either to explain them, or to correct mistakes in the names of the satellites, are thrown into notes, that there may be no interruption in the succession of the observations. Dr. H. then gives a long series of the observations of all the satellites, as copied from his original journal, with all the minute particulars, of course not necessary to be re-printed here.

Dr. H. then resumes his discourse. From the observations on the 7 satellites of Saturn above-mentioned, and closely compared with their calculated places, it appears evidently that the revolutions of these satellites are so well ascertained, that we may, without hesitation, determine that no phenomenon on the ring of Saturn, in the shape of lucid spot, protuberant point, or latent satellite, can be occasioned by any of them, when, on computation, we find that the place of the satellite differs from that where such appearances were observed. In consequence of this deduction, he found that the observations could not be explained by any of the known satellites; it remained therefore to be examined, to what cause to ascribe the appearance of such lucid spots.

The first idea that occurred was that of another satellite, still closer to the ring than the 7th; and if a revolution, slower than about 15 hours and a quarter, could have been found, which would have taken in the most material places in which bright spots were seen, he should have continued of opinion that an 8th satellite, exterior to the ring, did exist, notwithstanding more observations had been wanting to put the matter out of all doubt. But this being impracticable, he examined, in the next place, what would be the result if these supposed satellites, or protuberant points, were attached to the plane or edge of the ring.

As observations, carefully made, should always take the lead of theories, Dr. H. will not be concerned if such lucid spots as he is now going to admit, should seem to contradict what has been said in his last paper, concerning the idea of inequalities, or protuberant points. We may however remark, that a lucid, and apparently protuberant point, may exist without any great inequality in the ring. A vivid light, for instance, will seem to project greatly beyond the limits of the body on which it is placed. If therefore the luminous places on the ring should be such as proceed from very bright reflecting regions, or, which is more probable, owe their existence to the more fluctuating causes of inherent fires acting with great violence, we need not imagine the ring of Saturn to be very uneven or distorted, in order to present us with such appearances as will be related. In this sense of the word, then, we may still oppose the idea of protuberant points, such as would denote immense mountains of elevated surface.

On comparing together several observations, a few trials show that the brightest nd best observed spot agrees to a revolution of $10^h\ 32^m\ 15^s{\cdot}4$; and, calculating its distance from the centre of Saturn, on a supposition of its being a satellite, we find it $17''.227$, which brings it on the ring. It is therefore certain, that unless we should imagine the ring to be sufficiently fluid to permit a satellite to revolve in it, or suppose a notch, groove, or division in the ring, to suffer the satellite to pass along, we ought to admit a revolution of the ring itself. The density of the ring indeed may be supposed to be very inconsiderable by those who imagine its light to be rather the effect of some shining fluid, like an aurora borealis, than a reflection from some permanent substance; but its disapparition in general, and in the telescopes its faintness when turned edgeways, are in no manner favourable to this idea. When we add also, that this ring casts a deep shadow on the planet, is very sharply defined both in its outer and inner edge, and in brightness exceeds the planet itself, it seems to be almost proved, that its consistence cannot be less than that of the body of Saturn; and that consequently, no degree of fluidity can be admitted sufficient to permit a revolving body to keep in motion for any considerable time.

A groove might afford a passage, especially as on a former occasion we have already considered the idea of a divided ring. A circumstance also which seems rather to favour this idea is, that in some observations a bright spot has been seen to project equally on both sides, as the satellites have been observed to do when they passed behind the ring. But, on the other hand, we ought to consider that the spot has often been observed very near the end of the arms of Saturn's ring, and that the calculated distance is consequently a little too small for such appearances, and ought to be 19 or 20″ at least. We should also attend to the size of the spot, which seems to be variable; for it is hardly to be imagined that a satellite, brighter than the 6th, and which could be seen with the moon nearly at the full, should so often escape our notice in its frequent revolutions, unless it varied much in its apparent brightness.

To this we must add another argument drawn from the number of lucid spots, which will not agree with the motion of one satellite only; whereas, by admitting a revolution of the ring itself, in $10^h\ 32^m\ 15^s.4$; and supposing all the spots to adhere to the ring, and to share in the same periodical return, provided they last long enough to be seen many times, we shall be able to give an easy solution to all the remaining observations.

For instance, let α, β, γ, δ, ε, represent five spots on the ring of Saturn, situated as in fig. 5, pl. 8; where the ring is supposed to be divided into 360 degrees, and the spot α placed at 271°.5; β at 70°.2; γ at 183°.0; δ at 142°.5; and ε at 358°.6. Then will the ring, with the spots thus placed, serve as an epocha for the year 1789; by which, with the assistance of a table constructed on the before mentioned period of the rotation of the ring, we may calculate their situation for any required time; and to render this calculation perfectly convenient, a table is given, ready prepared for the purpose, at the end of the other tables. Dr. H. then gives another set of observations, which had all been previously calculated by the tables of such of the 7 satellites as were not already in view, and had been found to belong to neither of them; but in the notes that are given with them they have been again calculated by the table of the rotation of the ring for every time they were observed, on a supposition of their being spots adhering to it. He then adds:

The great accordance between the observed places of these spots and the calculated ones, seems to establish the rotation of the ring of Saturn on an axis, so as hardly to leave any doubt on the subject. The time of it, we have already seen, is $10^h\ 32^m\ 15^s.4$. It may be objected, that many of the observations are such as would also agree with other assignable periods, especially when the number of spots is so considerable as 5; but the most material observations, which are those on the spot α, setting aside all the rest, seem alone to amount to a proof not only of a rotation of the ring, but of the time in which it is performed.

It may be expected, that having now sufficiently examined the whole series of observations of the last new satellites, we can give their periodical times and distances more accurately than before. The times indeed are full as well ascertained as we can expect to have them: for on calculating 6 satellites by his tables back to Aug. $19^d\ 12^h\ 19^m\ 56^s$, 1787, we find their places 341°.1 the 5th; 10°.6 the 4th; 211°.1 the 3d; 158°.9 the 2d; 80°.2 the 1st; and 288°.8 the 6th. And Dr. H's journal contains the fullest assurance that they were thus situated at the time for which this calculation is made. We may therefore fix the period of the 6th at $1^d\ 8^h\ 53^m\ 8^s.9$. The 7th satellite can only be traced back as far as the 8th of Sept. 1789; so that its revolution will require at least another season to come to some degree of accuracy; till when he states it at $22^h\ 37^m\ 22^s.9$.

The distance of these satellites, deduced from calculation, depends entirely on the time and distance of the 4th, which is the satellite that has been used. In order to obtain more accuracy in these elements, he applied himself to measuring the distance of the 4th satellite in those moments which were most favourable for the purpose. It is well known that this subject, on account of the quantity of matter in Saturn, to be deduced from the periodical times and distances of the satellites, is of considerable importance to astronomers; he therefore defers a full investigation of it till he can have an opportunity of calculating a great number of measures, not only of the 4th and 5th, but also of the other satellites which he had already by him, and still intends next season to take. Mean while, having brought the measures of the 30th of November, which seem to be very good ones, to the mean distance of Saturn from the sun, he finds they give the distance of the 4th satellite from Saturn $3'\ 8''.918$. In reducing these measures to the mean distance, he used the new tables of De Lambre for Saturn, and Mayer's for the sun. Admitting therefore the above quantity as the distance, and $15^d\ 22^h\ 41^m\ 13^s.4$ as the period of the 4th satellite, we compute that the distance of the 6th from the centre of Saturn is $36''.7889$; and that of the 7th, $28''.6689$.

Tables for the seven satellites of Saturn.

Epochs of the mean longitude of the satellites.

	5 sat.	4 sat.	3 sat.	2 sat.	1 sat.	6 sat.	7 sat.
Years.	°	°	°	°	°	°	°
1787	335.91	149.16	87.21	272.18	176.46	269.31	307.07
1788	196.84	132.41	93.86	173.95	131.91	307.48	65.02
1789	53.23	93.09	20.82	304.19	256.66	82.92	161.00
1790	269.63	53.77	307.78	74.43	21.41	218.36	256.98
1791	126.02	14.45	234.74	204.68	146.16	353.81	352.97

Saturnicentric motion of the satellites in months.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
Months.	°	°	°	°	°	°	°
January,	000.00	000.00	000.00	000.00	000.00	000.00	000.00
February,	140.68	339.89	310 40	117.58	151.64	224.54	320.81
March,..	267.75	252.05	21.73	200.56	91.18	20.91	215.73
April,...	48.43	231.95	332.13	318.14	242.81	245.45	176.54
May, ...	184 57	189.26	202 84	304.19	203.75	207.27	115.39
June, ...	325.25	169.16	153.24	61.77	355.39	71.81	76.20
July,....	101.39	126.47	23.94	47.82	316.33	33.63	15.05
August,	242.07	106.37	334.34	165.40	107.96	258.17	335.86
Septem.	22.75	86 26	284.74	282.98	259.60	122.72	296.67
October,	158.89	43.58	155.45	269.03	220.54	84.54	235.52
Novem.	299.57	23.47	105.85	26.61	12.17	309.08	196.33
Decem.	75.71	340 78	336.56	12.66	333.11	27.090	135.17

In the months January and February of a bissextile year substract 1 from the number of days given.

Motion of the satellites in days.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
Days	°	°	°	°	°	°	°
1	4.54	22.58	79.69	131.53	190.70	262.73	21.96
2	9.08	45.15	159.38	263.07	21.40	165.45	43.92
3	13.61	67.73	239.07	34.60	212.09	68.18	65.88
4	18.15	90.31	318.76	166.14	42.79	330.91	87.85
5	22.69	112.89	38.45	297.67	233.49	233.64	109.81
6	27.23	135.46	118.14	69.21	64.19	136.36	131.77
7	31.77	158.04	197.83	200.74	254.89	39.09	153.73
8	36.30	180.62	277.52	332.28	85.58	301.82	175.69
9	40.84	203.19	357.21	103.81	276.28	204.55	197.65
10	45.38	225.77	76.90	235.35	106.98	107.27	219.62
11	49.92	248.35	156.59	6.88	297.68	10.00	241.58
12	54.46	270.93	236.28	138.42	128.38	272.73	263.54
13	58.99	293.50	315.97	269.95	319.07	175.45	285.50
14	63.53	316.08	35.66	41.49	149.77	78.18	307.46
15	68.07	338.66	115.35	173.02	340.47	340.91	329.42
16	72.61	1.24	195.04	304.56	171.17	243.64	351.39
17	77.15	23.81	274.74	76.09	1.87	146.36	13.35
18	81.69	46.39	354.43	207.63	192.56	49.09	35.31
19	86.22	68.97	74.12	339.16	23.26	311.82	57.27
20	90.76	91.54	153.81	110.70	213.96	214.54	79.23
21	95.30	114.12	233.50	242.23	44.66	117.27	101.19
22	99.84	136.70	313.19	13.77	235.35	20.00	123.16
23	104.38	159.28	32.88	145.30	66.05	282.73	145.12
24	108.91	181.85	112.57	276.84	256.75	185.45	167.08
25	113.45	204.43	192.26	48.37	87.45	88.18	189.04
26	117.99	227.01	271.95	179.91	278.15	350.91	211.00
27	122.53	249.58	351.64	311.44	108.84	253.64	232.06
28	127.07	272.16	71.33	82.98	299.54	156.36	254.92
29	131.60	294.74	151.02	214.51	130.24	59.09	276.89
30	136.14	317.32	230.71	346.05	320.94	321.82	298.85
31	140.68	339.89	310.40	117.58	151.64	224.54	320.81

Motion of the satellites in hours.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
Hours	°	°	°	°	°	°	°
1	0.19	0.94	3.32	5.48	7.95	10.95	15.92
2	0.38	1.88	6.64	10.96	15.89	21.89	31.83
3	0.57	2.82	9.96	16.44	23.84	32.84	47.75
4	0.76	3.76	13.28	21.92	31.78	43.79	63.66
5	0.95	4.70	16.60	27.40	39.73	54.73	79.58
6	1.13	5.64	19.92	32.88	47.67	65.68	95.49
7	1.32	6.58	23.24	38.36	55.62	76.63	111.41
8	1.51	7.53	26.56	43.84	63.57	87.58	127.32
9	1.70	8.47	29.88	49.33	71.51	98.52	143.24
10	1.89	9.41	33.20	54.81	79.46	109.47	159.15
11	2.08	10.35	36.52	60.29	87.40	120.42	175.07
12	2.27	11.29	39.84	65.77	95.35	131.36	190.98
13	2.46	12.23	43.17	71.25	103.29	142.31	206.90
14	2.65	13.17	46.49	76.73	111.24	153.26	222.81
15	2.84	14.11	49.81	82.21	119.19	164.20	238.73
16	3.03	15.05	53.13	87.69	127.13	175.15	254.64
17	3.21	15.99	56.45	93.17	135.08	186.10	270.56
18	3.40	16.93	59.77	98.65	143.02	197.05	286.47
19	3.59	17.87	63.09	104.13	150.97	207.99	302.39
20	3.78	18.81	66.41	109.61	158.91	218.94	318.30
21	3.97	19.75	69.73	115.09	166.86	229.89	334.22
22	4.16	20.70	73.05	120.57	174.81	240.83	350.13
23	4.35	21.64	76.37	126.05	182.75	251.78	6.08
24	4.54	22.58	79.69	131.53	190.70	262.73	21.96

Motion of the satellites in minutes.

Min.	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1	0°.00	0°.02	0°.06	0°.09	0°.13	0°.18	0°.27
2	0 .01	0 .03	0 .11	0 .18	0 .26	0 .36	0 .53
3	0 .01	0 .05	0 .17	0 .27	0 .40	0 .55	0 .80
4	0 .01	0 .06	0 .22	0 .37	0 .53	0 .73	1 .06
5	0 .02	0 .08	0 .28	0 .46	0 .66	0 .91	1 .33
6	0 .02	0 .09	0 .33	0 .55	0 .79	1 .09	1 .59
7	0 .02	0 .11	0 .39	0 .64	0 .93	1 .28	1 .86
8	0 .03	0 .13	0 .44	0 .73	1 .06	1 .46	2 .12
9	0 .03	0 .14	0 .50	0 .82	1 .19	1 .64	2 .39
10	0 .03	0 .16	0 .55	0 .91	1 .32	1 .82	2 .65
11	0 .04	0 .17	0 .61	1 .00	1 .46	2 .01	2 .92
12	0 .04	0 .19	0 .66	1 .10	1 .59	2 .19	3 .18
13	0 .04	0 .20	0 .72	1 .19	1 .72	2 .37	3 .45
14	0 .05	0 .22	0 .77	1 .28	1 .85	2 .55	3 .71
15	0 .05	0 .24	0 .83	1 .37	1 .99	2 .74	3 .98
16	0 .05	0 .25	0 .89	1 .46	2 .12	2 .92	4 .24
17	0 .06	0 .27	0 .94	1 .55	2 .25	3 .10	4 .51
18	0 .06	0 .28	1 .00	1 .64	2 .38	3 .28	4 .78
19	0 .06	0 .30	1 .05	1 .73	2 .52	3 .47	5 .04
20	0 .07	0 .31	1 .11	1 .83	2 .65	3 .65	5 .31
21	0 .07	0 .33	1 .16	1 .92	2 .78	3 .83	5 .57
22	0 .07	0 .34	1 .22	2 .01	2 .91	4 .01	5 .84
23	0 .08	0 .36	1 .27	2 .10	3 .05	4 .20	6 .10
24	0 .08	0 .38	1 .33	2 .19	3 .18	4 .38	6 .37
25	0 .08	0 .39	1 .38	2 .28	3 .31	4 .56	6 .63
26	0 .09	0 .41	1 .44	2 .37	3 .44	4 .74	6 .90
27	0 .09	0 .42	1 .49	2 .47	3 .57	4 .93	7 .16
28	0 .09	0 .44	1 .55	2 .56	3 .71	5 .11	7 .43
29	0 .10	0 .45	1 .60	2 .65	3 .84	5 .29	7 .69
30	0 .10	0 .47	1 .66	2 .74	3 .97	5 .47	7 .96
31	0 .10	0 .49	1 .72	2 .83	4 .10	5 .66	8 .22
32	0 .11	0 .50	1 .77	2 .92	4 .24	5 .84	8 .49
33	0 .11	0 .52	1 .83	3 .01	4 .37	6 .02	8 .75
34	0 .11	0 .53	1 .88	3 .10	4 .50	6 .20	9 .02
35	0 .12	0 .55	1 .94	3 .20	4 .63	6 .39	9 .29
36	0 .12	0 .56	1 .99	3 .29	4 .77	6 .57	9 .55
37	0 .12	0 .58	2 .05	3 .38	4 .90	6 .75	9 .82
38	0 .13	0 .60	2 .10	3 .47	5 .03	6 .93	10 .08
39	0 .13	0 .61	2 .16	3 .56	5 .16	7 .12	10 .35
40	0 .13	0 .63	2 .21	3 .65	5 .30	7 .30	10 .61
41	0 .14	0 .64	2 .27	3 .74	5 .43	7 .48	10 .88
42	0 .14	0 .66	2 .32	3 .83	5 .56	7 .66	11 .14
43	0 .14	0 .67	2 .38	3 .93	5 .69	7 .85	11 .41
44	0 .15	0 .69	2 .43	4 .02	5 .83	8 .03	11 .67
45	0 .15	0 .71	2 .49	4 .11	5 .96	8 .21	11 .94
46	0 .15	0 .72	2 .55	4 .20	6 .09	8 .39	12 .20
47	0 .16	0 .74	2 .60	4 .29	6 .22	8 .58	12 .47
48	0 .16	0 .75	2 .66	4 .38	6 .36	8 .76	12 .73
49	0 .16	0 .77	2 .71	4 .47	6 .49	8 .94	13 .00
50	0 .17	0 .78	2 .77	4 .57	6 .62	9 .12	13 .27
51	0 .17	0 .80	2 .82	4 .66	6 .75	9 .30	13 .53
52	0 .17	0 .82	2 .88	4 .75	6 .88	9 .49	13 .80
53	0 .17	0 .83	2 .93	4 .84	7 .02	9 .67	14 .06
54	0 .18	0 .85	2 .99	4 .93	7 .15	9 .85	14 .33
55	0 .18	0 .86	3 .04	5 .02	7 .28	10 .03	14 .59
56	0 .18	0 .88	3 .10	5 .11	7 .41	10 .22	14 .86
57	0 .19	0 .89	3 .15	5 .20	7 .55	10 .40	15 .12
58	0 .19	0 .91	3 .21	5 .30	7 .68	10 .58	15 .39
59	0 .19	0 .93	3 .27	5 .39	7 .81	10 .76	15 .65
60	0 .20	0 .94	3 .32	5 .48	7 .94	10 .95	15 .92

Table of the rotation of the ring of Saturn.

Epochs for 1789.	
Spot α	271.5
β	183.0
γ	70.2
δ	142.5
ε	358.6

Motion of the spots in Months.

Months.	Deg.
January...	000.00
February..	217.52
March....	135.28
April.....	352.80
May	110.40
June	327.92
July......	85.52
August....	303.04
September	160.56
October...	278.16
November	135.68
December	253.28

Motion of the spots in days, hours, and minutes.

Days	Degrees.	Hou.	Degrees.	Min.	Degrees.	Min.	Degrees.
1	99.92	1	34.16	1	0.57	31	17.65
2	199.84	2	68.33	2	1.14	32	18.22
3	299.76	3	102.49	3	1.71	33	18.79
4	39.68	4	136.65	4	2.28	34	19.36
5	139.60	5	170.81	5	2.85	35	19.93
6	239.52	6	204.98	6	3.42	36	20.50
7	339.44	7	239.14	7	3.99	37	21.07
8	79.36	8	273.30	8	4.56	38	21.64
9	179.28	9	307.46	9	5.12	39	22.21
10	279.20	10	341.63	10	5.69	40	22.78
11	19.12	11	15.79	11	6.26	41	23.35
12	119.04	12	49.95	12	6.83	42	23.91
13	218.96	13	84.11	13	7.40	43	24.48
14	318.88	14	118.28	14	7.97	44	25.05
15	58.80	15	152.44	15	8.54	45	25.62
16	158.72	16	186.60	16	9.11	46	26.19
17	258.64	17	220.76	17	9.68	47	26.76
18	358.56	18	254.93	18	10.25	48	27.33
19	98.48	19	289.09	19	10.82	49	27.90
20	198.40	20	323.25	20	11.39	50	28.47
21	298.32	21	357.41	21	11.96	51	29.04
22	38.24	22	31.59	22	12.53	52	29.61
23	138.16	23	65.75	23	13.10	53	30.18
24	238.08	24	99.91	24	13.67	54	30.75
25	338.00			25	14.24	55	31.32
26	77.92			26	14.80	56	31.89
27	177.84			27	15.37	57	32.46
28	277.76			28	15.94	58	33.03
29	17.68			29	16.51	59	33.59
30	117.60			30	17.08	60	34.16
31	217.52						

Example of the use of the tables.—Let it be required to calculate the apparent place of the 7 satellites for 1789, Oct. 18, $7^h\ 51^m\ 54^s$, to the nearest minute of time and to 10ths of a degree.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1789	53.23	93.09	20.82	304.19	256.66	82.92	161.00
Oct.	158.89	43.58	155.45	269.03	220.54	84.54	235.52
18	81.69	46.39	354.43	207.63	192.56	49.09	35.31
7	1.32	6.58	23.24	38.36	55.62	76.63	111.41
52	0.17	0.82	2.88	4.75	6.88	9.49	13.80
* ♄	12.58	12.58	12.58	12.58	12.58	12.58	12.58
	307.9	203.0	209.4	116.5	24.8	315.3	209.6

* The quantity marked ♄ 12°.58, which is applied to every one of the satellites, is the complement of 11ˢ 17° 25′, or geocentric place of Saturn, taken from the Nautical Almanac, for midnight of the required day, and to the nearest minute, which is sufficiently exact. This complement, or 12° 35′ in conformity with the tables, is reduced to decimals of a degree 12°.58.—Orig.

The situation of the spot α calculated for July 28, $13^h\ 53^m\ 39^s$; β for Sept. 16, $7^h\ 45^m\ 48^s$; ε for Nov. 2, $7^h\ 15^m\ 58^s$.

1789, α	271.5	β	183.0	ε	358.6
July	85.52	Sept.	160.56	Nov.	135.68
28	277.76	16	158.72	2	199.84
13	84.11	7	239.14	7	239.14
54	30.75	46	26.19	16	9.11
♄	7.20	♄	10.45	♄	13.18
	36.8		58.1		235.6

*XXIV. On Spherical Motion. By the Rev. Charles Wildbore.** p. 496.

This paper, which has cost me much pains in patient investigation, says Mr. W. is occasioned by that of Mr. Landen, in the Philos. Trans. vol. 75. I am no stranger to this gentleman's great judgment and abilities in these abstruse speculations, but have a very high opinion of both; yet I could not but think it strange, that two such mathematicians as M. D'Alembert and M. L. Euler should both follow each other on the same subject, both agree, and still not be right. I therefore resolved to try to dive to the bottom of their solutions, which those who are acquainted with the subject know to be no light task; and, if possible, to give the solution, independent of the perplexing consideration of a momentary axis changing its place both in the body and in absolute space every instant; and which I consider as not absolutely essential to the determination of the body's motion. But finding that I could not thus so readily show the agreement or disagreement of my conclusions with those of the gentlemen who have preceded me in this inquiry; I have also added the investigation of the properties of this axis. And I suppose it will be found that I have added many properties unknown before, or at least unnoticed by any of them.

M. Landen's very important discovery, that every body, be its form ever so irregular, will revolve in the same manner as if its mass were equally divided and placed in the 8 angles, or disposed in the 8 octants of a regular parallelopipedon, whose moments of inertia round its 3 permanent axes are the same as those of the body, serves admirably to shorten the investigation, and render the solution

* Mr. Wildbore died Oct. 30, 1802, being 65 years of age, at Broughton Sulney, Nottinghamshire, in which village he had been pastor more than 30 years. He commendably raised himself from a low origin, by his industry and natural talents, to an eminent rank in mathematics and classical learning. Before his care of the parish of Broughton, he kept an academy for young gentlemen several years, at Bingham, in the same county. Though an eminent philosopher and mathematician, Mr. W. never favoured the world with any separate publication of his own; but commonly amused himself in various periodical publications; as in Martin's Miscellaneous Correspondence, the Magazines, the Ladies' and Gentleman's Diaries, the Monthly Review, &c. After he became compiler of the Gentleman's Diary, in 1780, his writings in that annual little work were given under the signature Eumenes; and from that time, till his death, under that of Amicus in the Ladies' Diary.

perspicuous. I have therefore here taken its truth for granted, because it is also exactly agreeable to the solutions of the other gentlemen, and saves the trouble of repeating what they have done before. I have also shown wherein, and why, his solution differs from theirs, and proved, as I think, undeniably, in what respects it is defective.

That the inertia, or, as M. Euler calls it, the momentum of inertia, is equal to the fluent or sum of every particle of the body drawn into the square of its distance from the axis of motion; and the determination of the 3 permanent axes, or the demonstration that there are, at least 3 such axes in every body, round any one of which, if it revolved, the velocity would be for ever uniform, I have also taken for granted, because these things have been proved before, and all the gentlemen are agreed in them. Difficulties that occurred I have not concealed, but shown how to obviate, and endeavoured to place the truth in as clear a light as possible; which to discover is my wish, or to welcome it by whomsoever found.

Mr. W. divides his work into several propositions, which he demonstrates in an intricate algebraical method.

Prop. 1. While a globe, whose centre is at rest, revolves with a given velocity about an axis passing through that centre, to find with what velocity any great circle on the surface, but oblique to that axis, moves along itself. After the determination of this velocity, Mr. W. infers these 2 corollaries.

Corol. 1. Hence it follows, that in whatever manner a globe revolves, its velocity, measured on the same great circle on its surface, must be the same at the same time at every point of the periphery of that circle.—*Corol.* 2. Consequently, however the plane of a great circle varies its motion, the velocity at any instant is at every point of the periphery equal along its own plane.

Prop. 2. Supposing the centre of a sphere to be at rest, while the surface moves round it in any manner whatever; then, if the same invariable point o, considered as the pole of an axis of the sphere, be itself in motion, the angular velocity of the spherical surface about that axis will be unequable, or that of one point in it different from that of another.—*Corollary*. Hence, about whatever axis the angular motion of a sphere is equable, the pole of that axis, and consequently the axis itself, must be at rest at the instant. Different motions may have different correspondent poles, and consequently, when the motion is variable, the place of the pole of equable motion on the surface may vary; but whatever point on the surface corresponds with that pole must at the instant be at rest.

Prop. 3. Let ABC (fig. 6, pl. 8) be an octant of a spherical surface in motion, while the centre is at rest; and let the velocity of the great circle BC in its own plane $= a$, and in a sense from B towards C; that of CA in the sense from C towards A = b, and of AB from A towards B = c. If these 3 velocities a, b, and c, be constant, the spherical surface will always revolve uniformly about the same axis of the sphere at rest in absolute space.

PROP. 4. If a spherical surface, whose centre is at rest, revolve in any manner whatever, so that the velocities along the 3 quadrants bounding any octant of it be expressed by any 3 variable quantities x, y, and z; to find the necessarily corresponding accelerating forces with which the place of the natural or momentary axis, and the angular velocity of the surface round it, are varied.—After the investigation, Mr. W. adds: the preceding general properties of motion obtain in all bodies revolving round a centre at rest, be their motion ever so irregular; the 3 great circles bounding an octant of the spherical surface revolving with the body are also taken ad libitum, being any such circles whatever on the surface; and hence the following very important consequence is drawn, viz. If any body be in motion, or put in motion, by instantaneous impulse or otherwise, about its centre of gravity at rest in absolute space; if, by any means, the accelerating forces acting along the 3 great circles bounding any octant of a spherical surface that has the same centre of gravity and revolves with the body, can be found, those acting at every other point of such surface will necessarily follow as natural consequences of these 3, and thus all the motions of such body will be absolutely determined.

PROP. 5. The same being given, as in the last proposition, it is proposed to illustrate the manner in which the surface moves with respect to a point at rest in absolute space.

PROP. 6. If a parallelopipedon, or other solid, revolving uniformly with an angular velocity = z about one of its permanent axes of rotation, receive an instantaneous impulse in a direction parallel to that axis, the centre of gravity of the body being supposed to be kept at rest by an equal and contrary impulse given to it, and no other force acting on the body; it is proposed to determine the alteration in its motion, in consequence of such instantaneous impulse.

PROP. 7. If a body of any form revolve in any manner whatever with its centre of gravity at rest in absolute space, and so as not to be disturbed by the action of any external force; to determine in what manner it will continue its motion for ever.

We have omitted the analytical solutions of these propositions, both because they are not every where exempt from errors, and because various other solutions of the general problem are to be found as well in these Transactions as elsewhere.

XXV. On the Chronology of the Hindoos. By Wm. Marsden, Esq., F. R. S., and A. S. p. 560.

Unfortunately for the gratification of rational curiosity, history seems to have been, of all branches of study, that which the Hindoos cultivated with the least care, and we regret to find the periods marked by the revolutions of the heavenly bodies, of which other nations have availed themselves to ascertain and record the important events in human affairs, by them unprofitably applied to the dreams of their mythology. The unremitted labour of ages has been devoted to perfecting the calculation of the lunar motions, in which their correctness is surpassed only by the European improvements of very modern times; but, by a strange perversion, the accuracy thence acquired in their prediction of eclipses, appears to have no other object than that of administering to an idle superstition, which it ought to destroy. Though the fabulous exceedingly prevails in all the ancient documents hitherto introduced to our knowledge, and in none more conspicuously than those which contain the genealogies and reigns of their early kings, yet we are not hastily to conclude, that this people are destitute of records of true history. The portion of their literary stores which we have had opportunity to examine is comparatively small. Perseverance may discover annals, more or less ancient, whose present obscurity is perhaps occasioned by that very circumstance which constitutes their real value—the want of the miraculous. Some authentic monuments have already been elucidated by the learned skill of a gentleman, now a member of the R. S. Facts will accumulate by degrees, and acquire authority by mutually bearing on each other; and the Hindoos, like many other nations of the world, may hereafter be indebted to strangers, more enlightened by philosophy than themselves, for a rational history of their own country.

In different parts of India, and even in one and the same part, we observe various chronological eras referred to, as well in their astronomical treatises, as in their political and private writings. These being productive of confusion, if not clearly understood and discriminated, it is intended here to exhibit such a comparative statement of their respective commencements and coincidences as may tend to remove this impediment to the progress of historical knowledge. The present purpose does not lead to attempt a discussion of what may be termed the factitious periods of Hindoo computation, or to explain the nature and duration of the 4 ages, or Yoogs, which this speculative people, in the wanton exercise of numerical power, have portioned out from the boundless region of time. The 3 former of these divisions, even though the progressive numbers assigned to them should be admitted as the result of astronomical combination, can be presumed to have but little reference to practical chronology, which seems to trace its origin no higher than the commencement of the 4th, or present age, denominated the Kalee Yoog. This constitutes the principal era here to be attended to, and comprehends within it these that follow; the era of Bikramajit, the era of Salabân, the Bengal era, not strictly Hindoo, and the cycle of 60 years.

Before proceeding to a comparison of their several dates, it will be proper to define the nature of the year and its constituent parts, according to which these eras are computed by the Brahmans, who are the depositaries of science as well as of religion. Their astronomical year is the measure of that portion of time which is employed in a revolution of the sun, from the moment of his departure from a certain star in their zodiac, as seen from the earth, till his return to the same. It is therefore solar and sidereal, and contains, by their calculation, 365^d 6^h 12^m 30^s; and as they suppose the annual movement of the stars in longitude, or the precession of the equinoxes, to be $54''$, or 21^m 36^s of time (admitting with them, the sun to move at the rate of a degree each day), their tropical year will be 365^d 5^h 50^m 54^s: but as the sun really moves over $54''$ in 21^m 55^s, its length is strictly 365^d 5^h 50^m 35^s, or 1^m 52^s greater than the tropical year as determined by Mayer at 365^d 5^h 48^m 43^s. The true precession being $50''.3$, which space the sun describes in 20^m 25^s, the true sydereal year is 365^d 6^h 9^m 8^s, and consequently the Hindoo year exceeds it by 3^m 22^s, or 1 day in 430 years. If the opinion of astronomers is well founded, that a sensible diminution in the length of the year, as well as in the angle of obliquity of the ecliptic, has gradually taken place in the lapse of many ages, it will follow, that this error may not have existed, or been so great, at the period of adjusting the Hindoo tables: and when we consider that there appears no ground to believe their apparatus for observing was ever much superior to what it is discovered among the Brahmans of this day, we are led to wonder at the precision attained to in this determination, and which, in the calculation of the moon's apogee, is still more remarkable. The defect of art can have been compensated only by the remote antiquity in which the series of their observations originated, affording an opportunity of correcting the inaccuracy of particular measurements by a mean of large numbers and distant intervals.

They divide the zodiac into 28 lunar, and into 12 solar constellations or signs, and their astronomical year commences with the sun's arriving at the first point of their constellation of Aries. This division of the zodiac, so far as the accuracy of their observations allows, is connected with the actual phenomena of the heavens, and advances with the apparent motion of the stars, from east to west, leaving gradually behind it the equinoctial points, and is not, like our zodiac, an abstract division of space, attached to those points, and independent of the starry system. Calculating on their principles, the difference of the 2 zodiacs, or the accumulated amount of the annual precession, since the coincidence supposed to be in the year of the Christian era 499, is in the present year $19° 21' 54''$.

The length of their months is determined by the time which the sun employs in passing each sign, and they are accordingly longer in the apogee, and shorter in the perigee; that which corresponds with the higher apsis being $31^d\ 14^h\ 39^m$, and that with the lower $29^d\ 8^h\ 21^m$ only. It does not appear that the Hindoos are accustomed to enumerate, for civil purposes, the days of the solar month, but to date from the age of the moon that happens to fall within that month, or frequently from the simple phase of the moon.

Their festivals and fasts, like those of the Jews and Christians, being regulated, for the most part, by the lunar revolutions, they employ on this account, exclusively of the solar astronomical reckoning, a lunar year. This they make to consist of 12 months, and each semi-lunation is distinguished into 15 equal portions, or lunar days, which are somewhat shorter than the natural day. In order to preserve its general correspondence with the solar year, "they reckon twice that lunation during which the sun does not enter on any new sign," or, in other words, which falls completely within a solar month; and the obvious reason for this mode of intercalating is, that as the lunar months take their denomination from the solar month in which the change happens, if 2 new moons fall within the same month, they naturally take the same name, and no irregularity is observable. This opportunity of increasing the number of lunar months, without embarrassing the reckoning, presents itself, on a medium of a few years, just as often as is requisite to effect the compensation. It cannot happen in all the solar months indifferently, their lengths being unequal, and some of them shorter than the synodical lunar month.

The commencement of the solar day is usually estimated from sunrise, and the space between that and the sunrise of the following day is divided into 60 parts, the length of which must vary with the sun's unequal course through the ecliptic; but for the purposes of calculation it is supposed to be ascertained at the solstices, and is equal to 24 of our minutes. The subdivisions, in like manner, follow the sexagesimal scale. There is also a mechanical division of the day and night into 8 parts, of which 4 are allowed to the interval from sunrise to sunset, and 4 to that from sunset to sunrise. The proportion of length of these parts respectively depends therefore on the season of the year and the latitude of the place, and the division is consequently inapplicable to general astronomy.

The days of the week are denominated from the 7 planets, and their arrangement is the same with that adopted in the western parts of the world, proceeding from the sun and moon to Mars, Mercury, Jupiter, Venus, and Saturn. Friday, or the day of Venus, appears as the first of the week in their calculations, and probably because the Kalee Yoog began on that day; but in common the week is considered by the Hindoos as beginning with Sunday. Having thus briefly touched on such points of their astronomy as are immediately connected with the measurement of time, Mr. M. proceeds to the comparison of their eras, which he first brings into one general view, and afterwards considers separately.

Table exhibiting the correspondence of the several Hindoo eras with each other, and with the Julian period and Christian era.

	Julian period.	Kalee Yoog.	Era of Bikramajit.	Christian era.	Era of Salabân.	Bengal era.	Cycle of 60 years.
Kalee Yoog, or grand era. .	1612	0					13
Era of Bikramajit	4657	3045	0				58
Christian era	4713	3101	56	0			54
Era of Salabân	4791	3179	134	78	0		11
Present cycle of 60 years . .	6460	4848	1803	1747	1669	1154	1
Year 1790 of J. C. (from April)	6503	4891	1846	1790	1712	1197	44

For better understanding the above table, it is necessary to remark, that when the Hindoos quote the year of an era, they do it by the number of the elapsed or complete year; whereas, the common European mode is to date by the number of the current or incomplete year; therefore what we should term the first year of an era, is with them the year zero, and their year 1 that which follows; excepting in the cycle of 60, of which the year 1 immediately succeeds the last complete year of the former cycle. The difference in years between any 2 eras is expressed by the number appearing at the intersection of the horizontal and perpendicular lines, to which the names of such eras are prefixed; or it may be found by subtracting the less number from the greater, as they stand in any of the horizontal lines, under their respective names at the top of the perpendicular columns; thus, the years intervening between the eras of Salabân, and that of the Kalee Yoog, are denoted by the number 3179, at the intersection of the 2 lines, or equally by deducting 1712 from 4891, in the lowest horizontal line.

In a comparison of the dates of the earlier Hindoo eras with the Christian era, there occurs a difficulty which it is proper to consider apart. This arises from an ambiguity in our manner of reckoning the years before Christ. It is most usual to pass immediately from the year 1 after to the year 1 before Christ, making the interval of time only 1 year; but some of the best chronologists pass from the year 1 after to the year zero, and thence to the year 1 before; by which means the interval between any number of years before and after Christ is equal to the sum of those numbers; and as this method is used in almost all astronomical tables, it may, without impropriety, be called the astronomical, and the other the common method. As the Hindoo year begins in our month of April, we must observe, in reducing any Hindoo date to the Christian era, that when it happens between the commencement of our year and theirs, the number of their year must be increased by 1, and the subtraction or addition then made.

The Kalee Yoog, or principal chronological era, began in the year before specified, when the sun's mean place was in the first point of the constellation Aries of the Hindoo zodiac; which happened on the 18th of February, at sunrise, under their first meridian, called the meridian of Lanka. At that period, it is said to be asserted by their astronomers, that the sun, moon, and all the planets, were in conjunction, according to their mean places. The reality of this fact, but with considerable modification, has received a respectable sanction from the writings of an ingenious and celebrated member of the French Academy of Sciences, who concludes, that the actual observation of this rare phenomenon, by the Hindoos of that day, was the occasion of its establishment as an astronomical epoch. Though M. Bailly has supported this opinion with his usual powers of reasoning, and though abundant circumstances tend to prove their early skill in this science, and some parts of the mathematics connected with it, yet we are constrained to question the verity or possibility of the observation, and to conclude rather that the supposed conjunction was, at a later period, sought for as an epoch, and calculated retrospectively. That it was widely miscalculated too, is sufficiently evident from the computation which M. Bailly himself has given of the longitudes of the planets at that time, when there was a difference of no less than 73° between the places of Mercury and Venus. But 15 days after, when the sun and moon were in opposition, and the planets far enough from the sun to be visible, he computes that all, excepting Venus, were comprehended within a space of 17°; and on this he grounds his supposition of an actual observation.

Calculating from the rules laid down by the Brahmans, as given by M. Le Gentil, it appears, that 4891 mean years of this era expired on Monday, 12th April, 1790, at $4\frac{1}{2}^h$, and that the true place of the sun came to the first of Aries, and consequently that the 4892d year, or 4891 complete as the Hindoos express it, actually began on the 10th, at 1^h; or on Sunday, 11th April, in their civil way of reckoning. Thus we see that, during this long period of time, the Hindoo account has lost on the Julian 42 days, allowing for the change that took place in our stile. The year of the former exceeding the latter by 12^m 30^s, falls continually later and later on our old stile year, at the rate of a day in about 115 years; and from this the commencement of any future year may be readily computed. The annual irregularity observable, which is independent of the almost imperceptible change, arises only from our mode of intercalating a day at the end of every 4th year, to compensate the fractions that have accumulated during that time. The Hindoo astronomical year, admitting of no intercalation, cannot preserve an annual correspondence, but began at nearly the same time, with respect to our year, in 1786 as in 1790; in 1775 and 1787, 6 hours later; in 1781 and 1785, 6 hours sooner; and in 1784 and 1788, being leap years, 18 hours sooner than in 1787. The civil year begins at the sunrise immediately preceding the calculated commencement of the astronomical, when that happens during the day; but if in the night-time, it begins at the sunrise following, and will consist of 366 days as often as the excess of the astronomical year above 365 amounts to a whole day.

The next era that presents itself and the first that has pretensions to be considered as historical, is that of Bikramajit. This prince, whose paternal kingdom was Malva, and his capital Ougeim, waged war against Saka king of Dehli, probably his lord paramount, and having overcome and slain him, ascended in his stead the principal throne of India. Authorities differ widely as to the length of his reign; but he likewise is said to have fallen in battle, fighting with a king who invaded him from the southern provinces. He appears, from this account, in no other light than that of an unfortunate usurper, yet his fame in the memory of the Hindoos has eclipsed that of his predecessors, his adventures are a favourite subject of romance, and an era has been distinguished by his name. It is doubted whether we ought to consider his accession, or his death, as constituting the proper epoch; but with regard to the time from which the reckoning dates there is no uncertainty, and the nature of the event is not essentially connected with our present object. It is placed in the year 56 before Christ, or in the 4657th of the Julian period, and corresponds with the 3045th year of the grand era.

Mr. M. remarked, when treating of the Hejerà, that this Mahometan era was computed, not from the day of the prophet's flight, as generally supposed, but from the ordinary commencement of that year in which the flight happened; and thus we find, on comparing the Hindoo eras, that though some of them are professed to be counted from the deaths of their kings or other historical events, they yet all begin from the same point of the sun's annual course through the zodiac. The numerical reckoning of the years can well be conceived more liable to arbitrary change, as being less interesting to the bulk of the people, than the observance of a particular day, whose periodical return is every where marked with popular ceremonies and superstitions.

The era to which Salabân has given name dates from the 78th year of Christ, or 4791st of the Julian period, commencing with the 3179th year of the grand era. As this is no less than 134 years later than that of Bikramajit, it seems a bold anachronism to make them cotemporaries, or to suppose, what is commonly asserted, that the one prince was the conqueror of the other. Fortunately for the present investigation, it is history rather than chronology which suffers from this want of accuracy or discrimination in the annalists of India, or their Persian translators. Salabân, who is said to have reigned many years over the ancient kingdom of Narsinga, in the northern part of the peninsula, is described as a liberal encourager of the sciences. There is reason to think, that astronomy experienced a reformation and considerable improvements under his auspices; and the professors appear to have attached the celebrity of an era to his death, in respect for his talents and gratitude for his protection.

As the era of Bikramajit prevails chiefly in the higher or northern provinces of India, so does that of Salabân in the southern, but more exclusively. In their current transactions, however, the inhabitants of the peninsula employ a mode of computation of a different nature, which, though not unknown in other parts of the world, is confined to these people among the Hindoos. This is a cycle, or revolving period, of 6 solar years, which has no further correspondence with the eras above-mentioned than that of their years respectively commencing on the same day. Those that constitute the cycle, instead of being numerically counted, are distinguished from each other by appropriate names, which, in their epistles, bills, and the like, are inserted as dates, with the months, and perhaps the age of the moon annexed; but, in their writings of importance and record, the year of Salabân, often called the Saka year, is super-added; and this is the more essential, as it is not customary to number the cycles by any progressive reckoning. In their astronomical calculations, they sometimes compute the year of their era by multiplying the number of cycles elapsed, and adding the complement of the cycle in which it commenced, as well as the years of the current cycle; but from hence we are led to no satisfactory conclusion respecting the origin of this popular mode of estimating time. The presumption is in favour of its being more ancient than their historical epochs. The present cycle, of which 43 complete years were expired in April 1790, began in the year 1747, with the year of Salabân 1669, and of the grand era 4848. M. Le Gentil, to whom Europe is chiefly indebted for what is known of Hindoo astronomy, has fallen into an unaccountable error with regard to the years of this cycle, and their correspondence with those of the Kalee Yoog, as appears by the comparative table he has given of them, and other passages of his work. He seems to have taken it for granted, without due examination, that the year 3600 of the latter must have been produced by the multiplication of the cycle of 60 into itself, and consequently that the first year of this grand era must also have been the first of the cycle; but this is totally inconsistent with the fact; the Kalee Yoog began with the 13th year of the cycle of 60, and all the reasoning founded on the self-production and harmony of these periods must fall to the ground.

It now remains to take notice of a mode of reckoning peculiar to the province of Bengal, and thence denominated the Bengal era. The circumstances of its institution are involved in obscurity, and I do not find that even a conjecture on the subject has yet been offered to the world. It is admitted however to have been imposed by the Mahometan conquerors, and is therefore of no very remote antiquity. The most obvious consideration that presents itself, in examining the date of this era, is its proximity to the year of the Hejerà; the Bengal year 1196 complete ending on the 10th April, 1790, and the Hejerà year 1204 on the 10th September following. The difference has plainly arisen from the inequality of the solar and lunar reckonings, and its accumulation since a certain period when they must of necessity have coincided; and it is no improbable supposition, that the time of such coincidence was also that of introducing the mode of computation which has since prevailed. By ascertaining the amount of this difference, and the number of years required to produce it, we may expect to arrive at a knowledge of the period in question, or to approximate it at least. As the Hindoos compute from the elapsed year, and the Mahometans by the current, the difference between the two dates should be 6 years and 7 months; but as this correction of 1 year may be presumed equally applicable at the supposed time of the coincidence, and therefore unnecessary in this instance, it will follow that the real difference should be found by a simple subtraction of one date from the other, and consequently be 7 years and 7 months. The annual excess of the Hindoo or sydereal year above the Mahometan or lunar being $10^d\ 21\frac{1}{4}^h$, an interval of 254 years is required to produce this difference, or 220 years, to produce that of 6 years and 7 months. The former number being deducted from 1790, carries us back to the year 1536, in the reign of the Mogul emperor Humaioon, and the latter to 1570, in that of Akbar. About one or other of these periods we should seek for some record of the institution, but the histories we possess throw no light on the subject. Several gentlemen, conversant in the affairs of Bengal, to whom I have referred it, confirm the general idea as above given, but disagree as to the political circumstances. By one, the regulation is ascribed to Sheer Shah, who wrested the empire from Humaioon, and governed it for some years with wisdom and energy, when his death, and the distractions that ensued, restored it to the former possessor; and, by another authority, to Akbar, deservedly named the Great, who was next in succession.

The most obvious way of accounting for the peculiar mixture of Hindoo and Mahometan observances in this reckoning, appears to be, that the zeal of the monarch for establishing the era of his prophet had effected only a partial or temporary innovation; and that his new subjects, who were constrained to adopt as an epoch that year of the Hejerà in which the royal edict bore date, could not ultimately be forced to change their accustomed solar year for one that rapidly inverted the order of the seasons. But this attempt must be referred to a period somewhat earlier than the reigns of these particular princes, which were distinguished by a liberal policy; and the gentlemen above alluded to attribute to them respectively, not the interdiction but the restitution of the solar reckoning; that of the Mahomëtans, imposed on the province by their more bigotted predecessors, being found inconsistent with the periodical collection of the revenue, which depends on the harvests. It is, in truth, extremely difficult to conceive how a mode of computation, so much at variance with the rational concerns of civilized people, can possibly subsist in any state of society above that of the pastoral and predatory tribes with whom it originated.

As it appears, that the people of Siam, in the farther India, have borrowed their knowledge of astronomy from the Hindoos, it will not be thought inconsistent with the present subject, to add some account of the chronological eras in use among them. Of these, one has been termed their civil, and another their astronomical era. The civil reckoning is by lunar years, consisting of 12 months each, with an intercalation of 7 months in the period of 19 years, and commencing with the new moon that precedes the winter solstice. This era is computed from the supposed time of the introduction of their religion by Sommona-codom, 544 years before Christ, or in the year of the Julian period 4169; and consequently 2333 years of it were expired in the month of December 1789; but by a custom which, though not without its parallel, wants to be satisfactorily explained, they do not change the date, or count the succeeding year 2334, till it meets the astronomical reckoning in the month of April following.

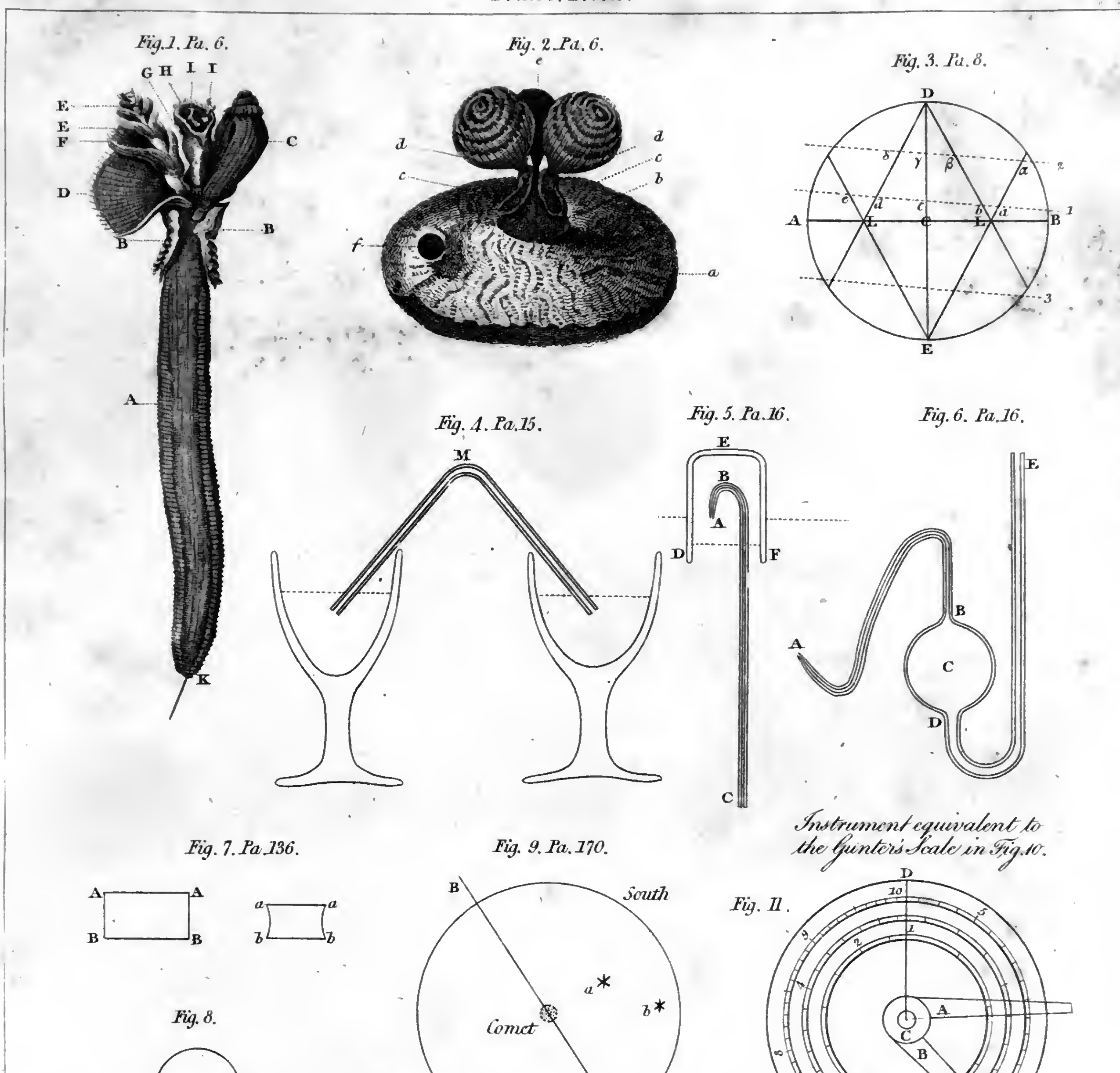
The astronomical era is founded immediately on the tables and modes of calculation adopted from the Hindoos. The French astronomer, Dom. Cassini, by an ingenious deduction from no very circumstantial data, inferred that it must have had for its epoch a mean conjunction of the sun and moon, which happened on March 21, 638 of the Christian era. This preceded by a few hours the commencement of the established Hindoo year, to which it was evidently meant to be accommodated, though it is by him referred to the vernal equinox, which took place 2 days earlier. The length of the Siamese solar year he found to be 365^d 6^h 12^m 36^s, and consequently 1152 years of the era should expire on the 11th April 1790, when the sun enters the Indian zodiac, being 560 years later than the era of Salabân. For want of corroborating facts, this determination of an epoch by M. Cassini was considered as speculative and uncertain; but Mr. M. was in possession of a date which, though not precise, may serve generally to authenticate it. "In 1769, the king of Pegu (a country bordering on Siam, and formerly conquered by it) dates his letter to the French at Pondicherry, the 12th of the month Kchong 1132." This makes 1790 to correspond with 1153 instead of 1152; but when we consider the vague manner in which notices of this kind are given, a difference of 1 year can scarcely be urged as an objection.

The Siamese were also accustomed to make use of a cycle of 60 years, expressed by a repetition of 12 names of certain animals, which are for the most part the same with those employed, for the same purpose, by the Chinese and Mogul Tartars, from whom we may conclude it has been borrowed; but the meagre and unsatisfactory examples of its application, furnished by M. Loubere and P. Tachard, do not afford us the means of determining at what time they began to reckon their cycle. It appears only that the year 1687 was the 10th of the lesser or constituent cycle of 12 years.

END OF THE EIGHTIETH VOLUME OF THE ORIGINAL.

END OF VOLUME SIXTEENTH.

C. and R. Baldwin, Printers,
New Bridge-Street, London.



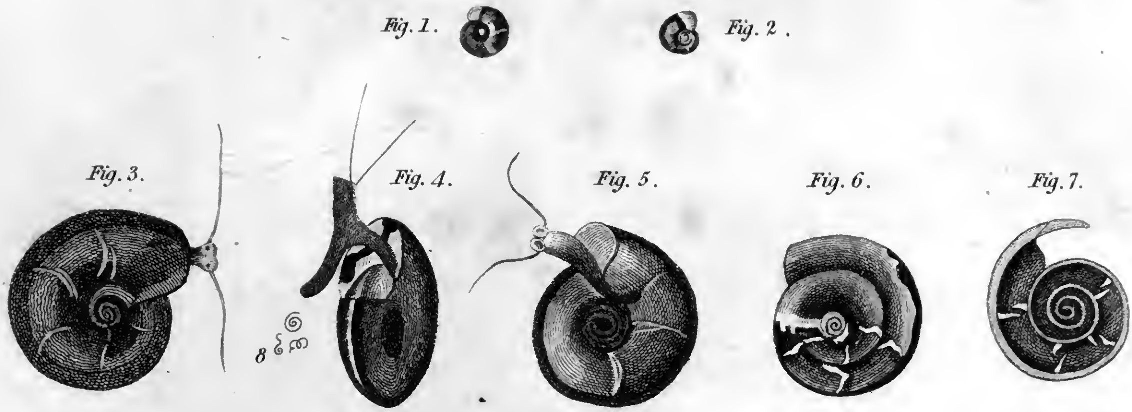
Mutlow Sc. Russell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1808.

Nautilus Lacustris.

Shell A. Pa. 80 &c.

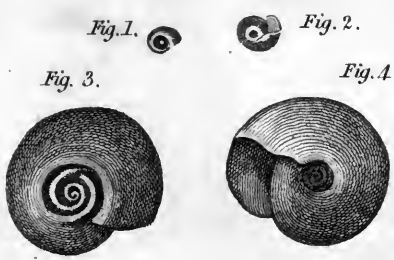
Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5. Fig. 6. Fig. 7.



Helix Fontana.

Shell B.

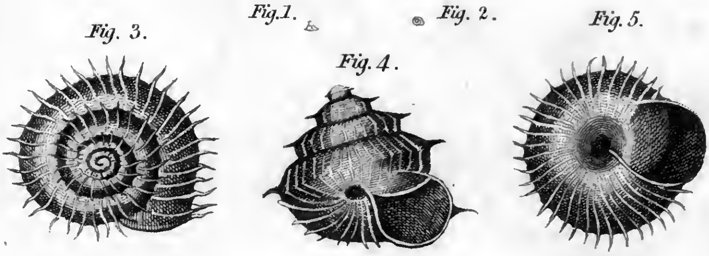
Fig. 1. Fig. 2. Fig. 3. Fig. 4.



Helix Spinulosa.

Shell C.

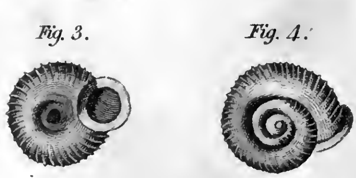
Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5.



Turbo Helicinus.

Shell D.

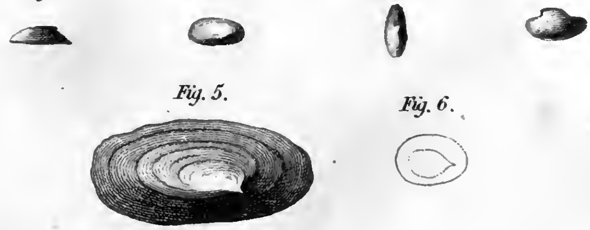
Fig. 1. Fig. 2. Fig. 3. Fig. 4.



Patella Oblonga.

Shell E.

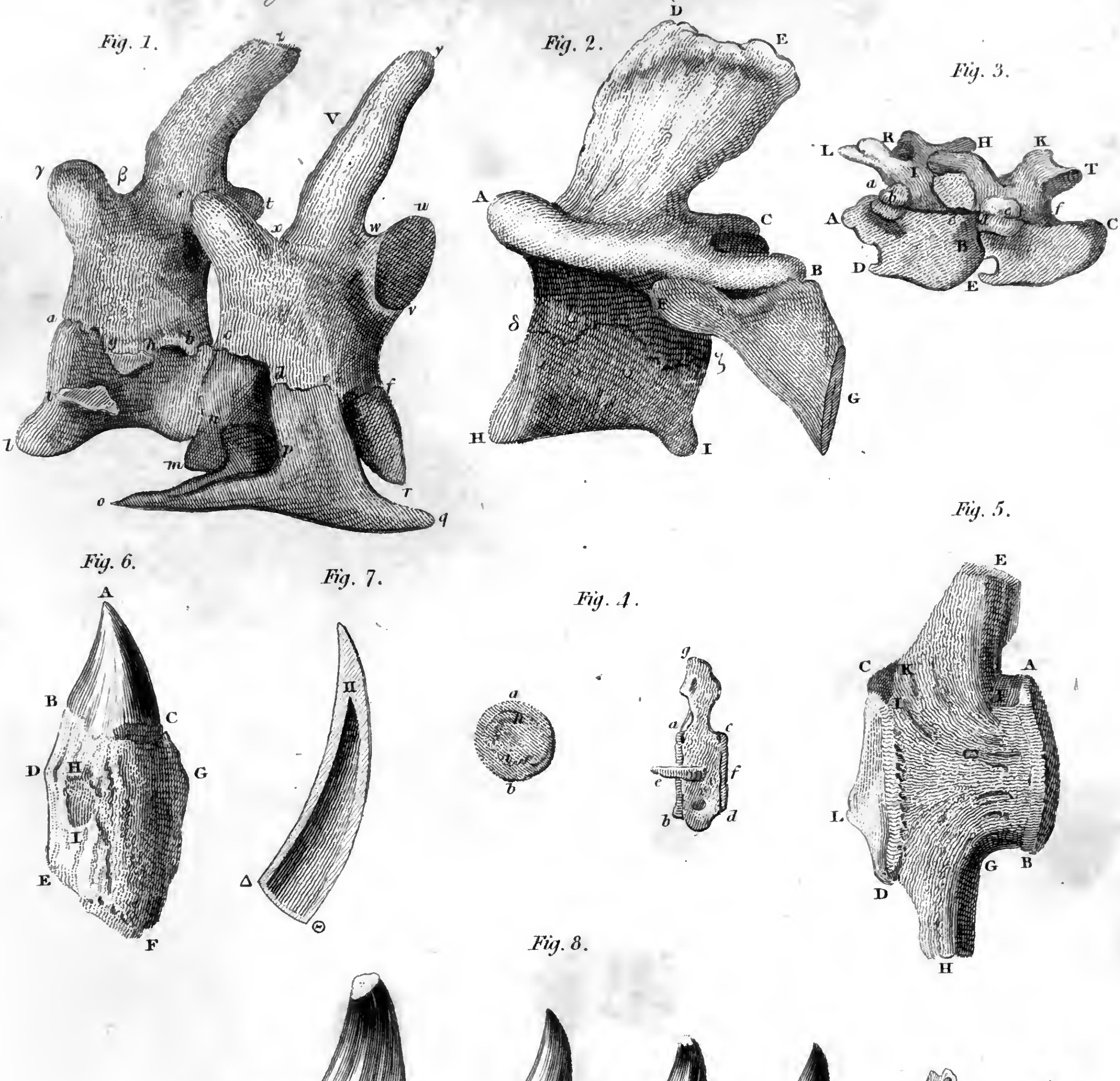
Fig. 1. Fig. 2. Fig. 3. Fig. 4. Fig. 5. Fig. 6.



Mutlow Sc. Russell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1808.

Fossil Bones, reduced from the original, pa. 153 &c.



Mutlow Sc. Russell Co.t

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Fig. 1. pa. 303.

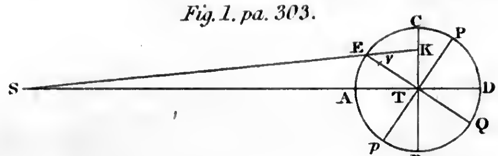
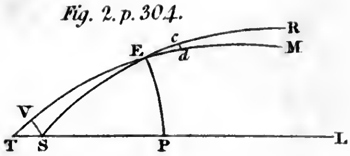


Fig. 2. p. 304.



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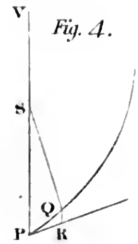
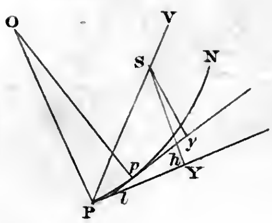


Fig. 3. p. 304. &c.



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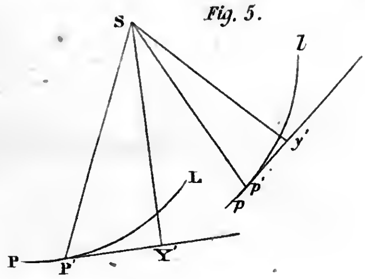

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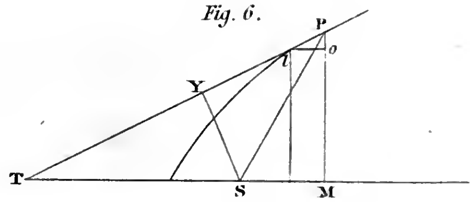

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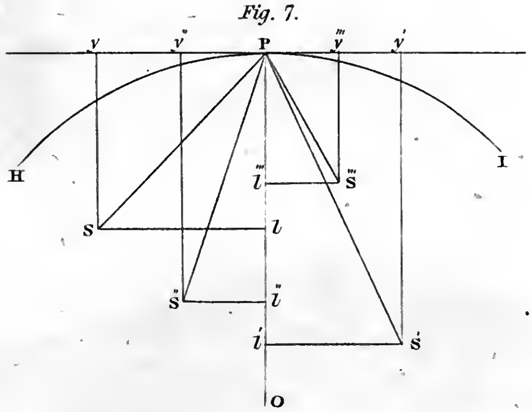

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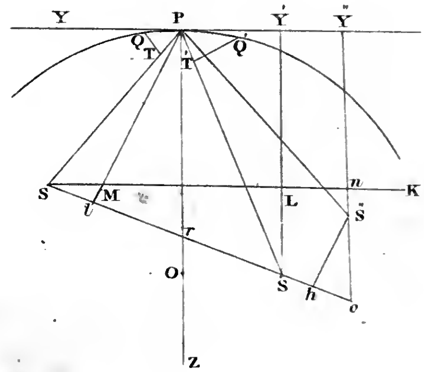

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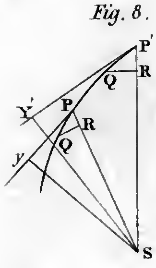

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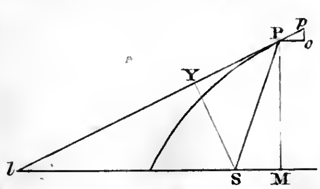

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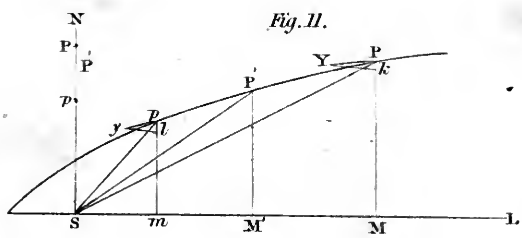

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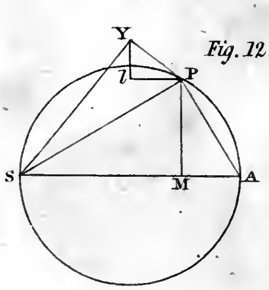

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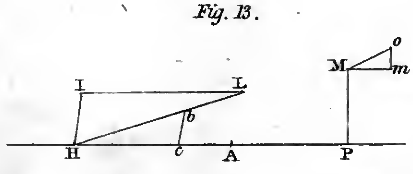

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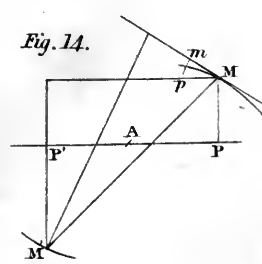

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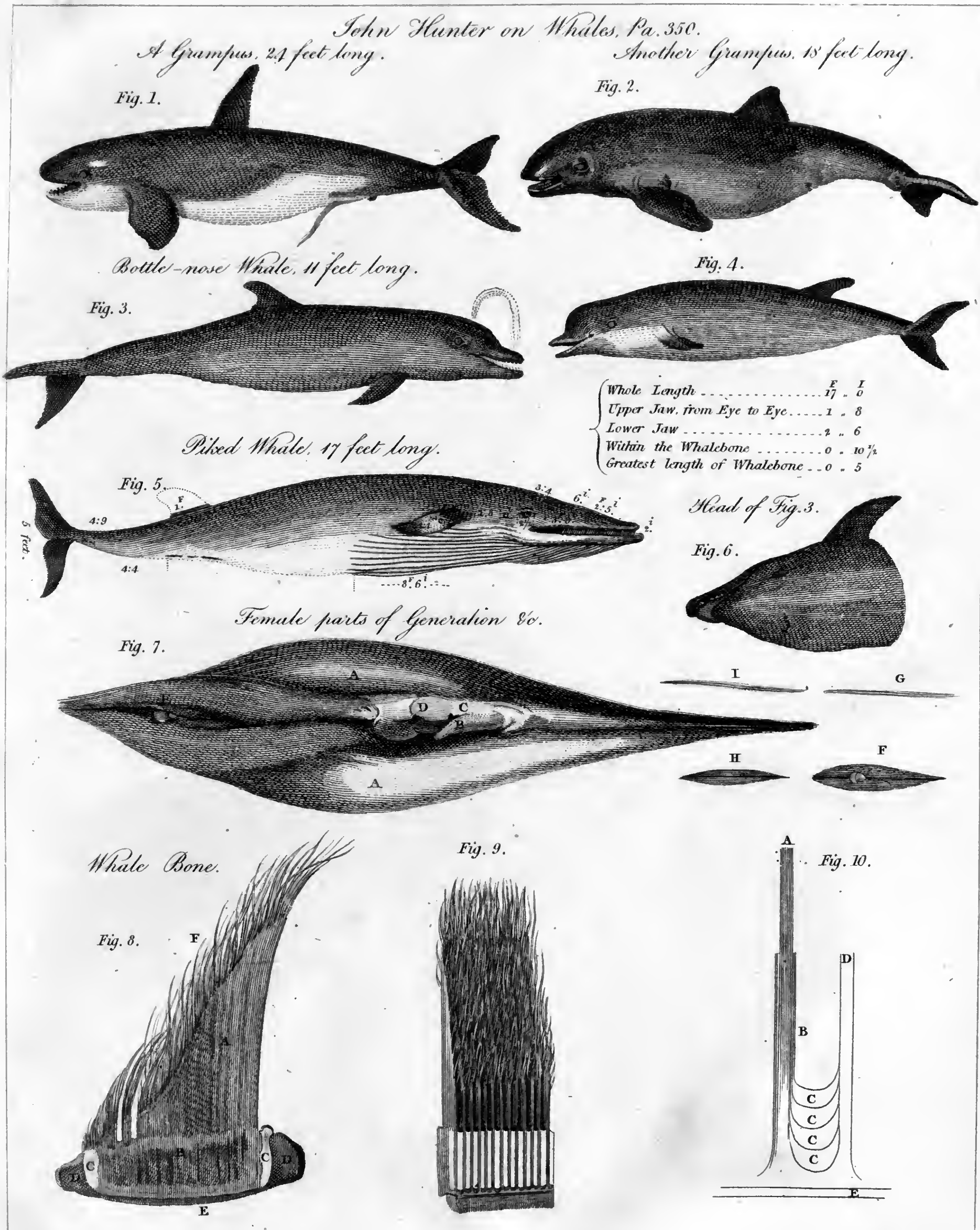

Mutlow Sc Russell Ct.

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Fig. 1. Pa. 450.

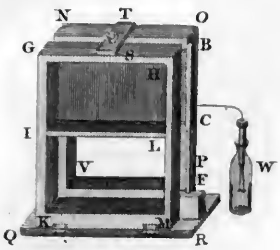


Fig. 2. p. 450.

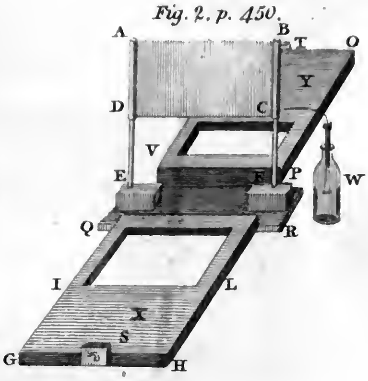


Fig. 4. p. 505.

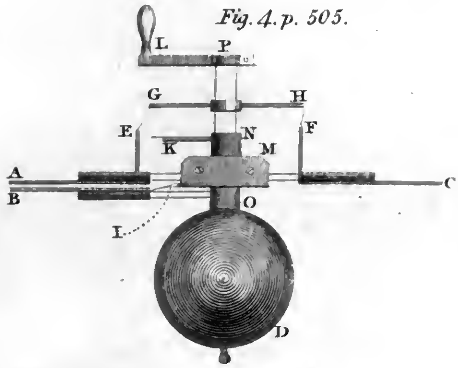


Fig. 3. p. 505.

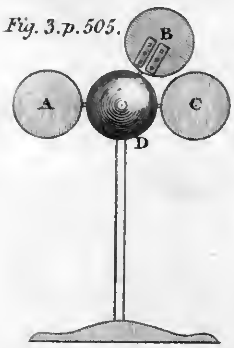
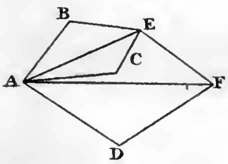


Fig. 5. p. 572.



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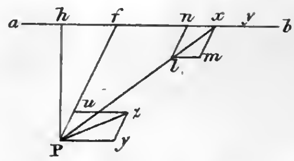

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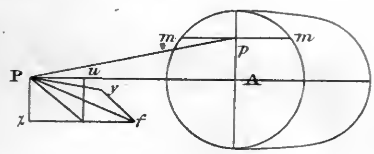

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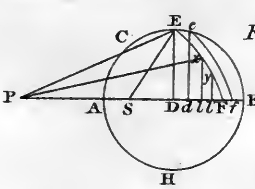

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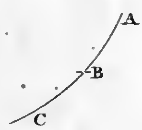

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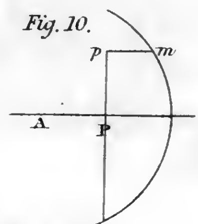

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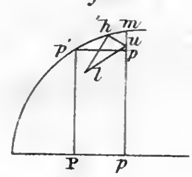

Fig. 14. p. 641.

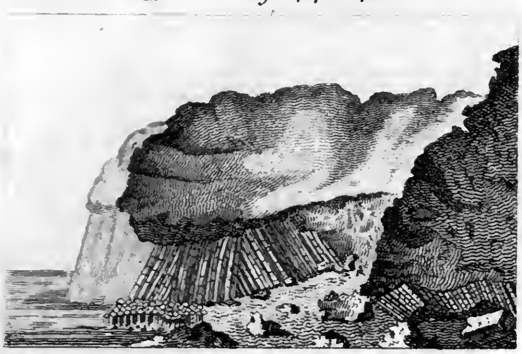

Fig. 12.

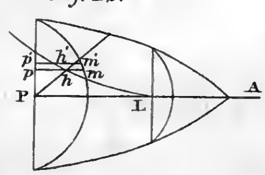

Fig. 13.

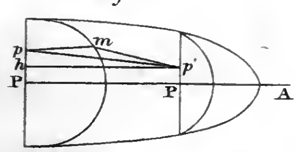

Fig. 16. p. 643.

Fig. 15. p. 641.

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Dr. Herschel on Saturn's Ring and Satellites. P. 613. &c.

Fig. 1. March 17. 1774.

Fig. 2. April 3. 1774.

Fig. 3. June 20. 1778.

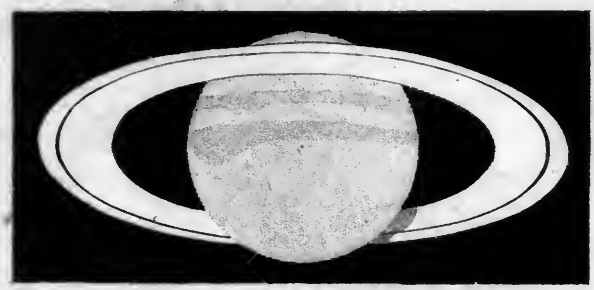


Fig. 4. May 11. 1780.

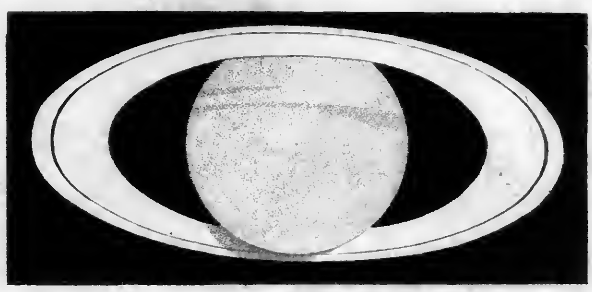


Fig. 5.

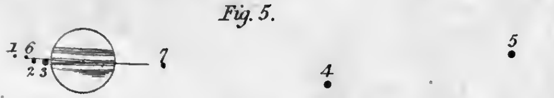


Fig. 6.

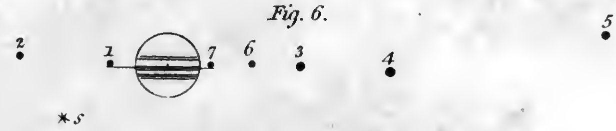


Fig. 8.

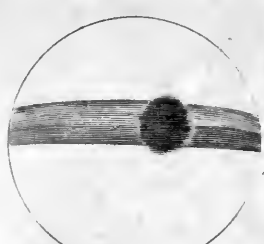


Fig. 9.

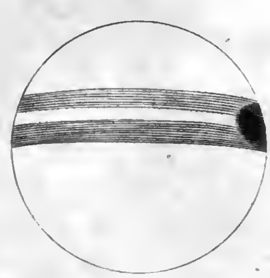


Fig. 7.

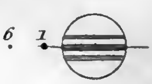
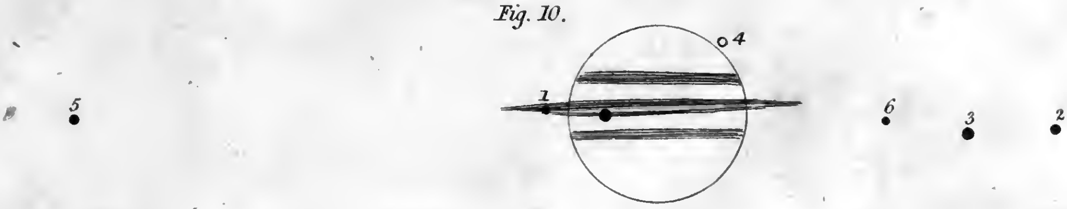


Fig. 10.



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Spikenard.

Fig. 1. Pa. 662.

Fig. 3. p. 667.

Fig. 4. p. 667.

Fig. 5. p. 735.

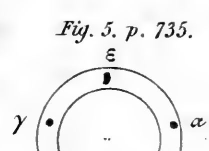


Fig. 6. p. 741.

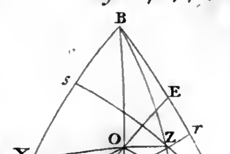
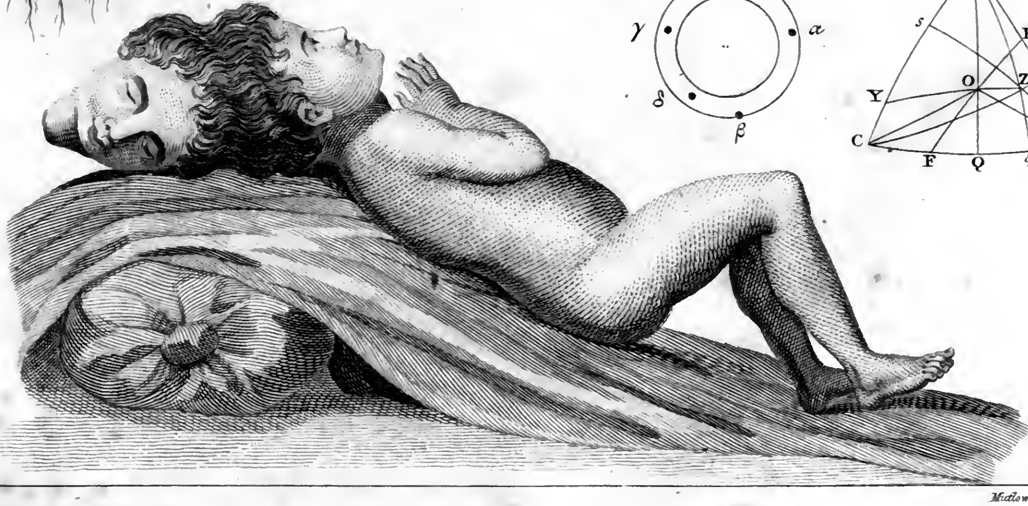


Fig. 2. p. 667.



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