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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[*] It will be seen that in plate IV there are duplicates of fig. 5 and 6; but from the nature of the subject the reader will easily distinguish them.

[†] In page 276, line 3, *for* fig. 1, pl. 6, *read* fig. 10, pl. 5.

[‡] In page 543, line 3, *for* fig. 11, *read* fig. 6.

THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON;

ABRIDGED.

Experiments concerning the Degrees of Heat of boiling Liquors. By M. Fahrenheit, F. R. S.* N° 381, p. 1. *Abridged from the Latin. Vol. XXXIII.*

M. Fahrenheit finding, in the history of the Royal Academy of Sciences, that M. Amontons had, by means of a thermometer of his own invention, discovered that water boils with a fixed degree of heat, was very desirous of making such another thermometer, to view with his own eyes this curious phenomenon of nature, and be convinced of the truth of the experiment. And recollecting what sagacious inquirers into nature had written about the manner of rectifying barometers, viz. that the height of the column of mercury, was sensibly affected by the different temperament of the mercury; from this M. Fahrenheit imagined, that perhaps a thermometer might be made of mercury, whose structure would not be so difficult, and yet it might discover the experiment he so much desired.

Having made such a thermometer, though still imperfect in several respects, the event answered his expectation, to his no small satisfaction. The issue of the experiments is exhibited in the following table: the first column shows the several liquors used; the second their respective specific gravities; the third the degree of heat each liquor acquired by boiling.

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* Gabriel Daniel Fahrenheit, an ingenious experimental philosopher, was born at Dantzic in 1686. It was about 1720 when he improved the thermometers, as above, by substituting mercury in the tube, instead of spirits of wine, which had hitherto been used, and by forming a new scale for the instrument, founded on very accurate experiments, in which the freezing point for water is at the number 32, and the boiling point at 212, the space between the two being divided into 180 equal parts or degrees. This scale has generally been adopted by the English, while the French, &c. have used that of Reaumur. Mr. F. published a treatise on thermometers in 1724; and he died in 1736.

Liquors.	The specific gravity of liquors heated to 48 degrees.	The degrees of heat acquired by boiling.
Spirits of wine	8260	176
Rain water	10000	212
Spirit of nitre	12935	242
Lixivium of pot-ash	15634	240
Oil of vitriol	18775	546

Volatile oils begin to boil with any degree of heat; but their heat continually increases by boiling; the reason of which may probably be this, viz. that the more volatile particles fly off, while the resinous ones, that have a greater attraction, remain behind.

But fixed oils require so great a degree of heat, that the mercury in the thermometer begins to boil at the same time with them; and hence their degree of heat can scarcely be found with certainty in the manner abovementioned.

Excepting spirits of wine and water, probably the degrees of heat in the other liquors, abovementioned, may likewise vary; especially, if they be taken in larger quantities, and boil for some time.

Observations made in 1723, *on the Barometer, Thermometer, Hygrometer, &c. By Nicolaus Cruquius, Mathematician, and F. R. S.* N° 381, p. 4. *Abridged from the Latin.*

In 1723 M. Cruquius made some accurate observations at Leyden, Delft, and Rheinburg, on the height of the barometer, thermometer, and hygrometer; also the quantity of rain, dew, snow, hail, and the quantity of water exhaled, &c. viz. Of the barometer, the greatest height 29 inches 8 lines, the least height 27 inches 7 lines, and the mean 29 inches: of the thermometer, the greatest degree of cold, or least height 1000 degrees, the freezing point of water at 1070, the boiling point of the same 1510, and the mean 1113: the water exhaled or evaporated 28 inches 11 lines: the rain, dew, snow and hail, at Delft 20 inches, and at Rheinburg 21.

An Account of a Dropsy in the left Ovary of a Woman, aged 58, *cured by a large Incision made in the Side of the Abdomen. By Dr. Robert Houstoun.* N° 381, p. 8.

The subject of this history was a poor woman aged 58. In her last lying-in, at 45 years old, the midwife having violently pulled away the burthen, she was so sensibly affected by a pain, which then seized her in the left side, between the umbilicus and groin, that she has scarcely ever been free from it since, but it has troubled her more or less, during 13 years. For 2 years past she has been extremely uneasy, her belly has grown very large, and a difficulty of breathing has increased continually on her; so that for the last 6 months, she has breathed with the utmost difficulty. In all that time, she has scarcely eaten as much as would nourish a sucking child; and for 3 months together she has been forced to lie constantly on her back, not daring to move at all, to either side.

I found this tumor grown to so monstrous a bulk, that it engrossed the whole left side, from the umbilicus to the pubes, and stretched the abdominal muscles, to a great degree. It drew towards a point. From being obliged to lie continually on her back, she was grievously excoriated, which added much to her sufferings, and together with want of rest and appetite, had greatly emaciated her.

The operation of puncturing the abdomen being proposed, she consented: accordingly with an imposthume lancet I laid open about an inch, but finding nothing issue, I enlarged it 2 inches, and even then nothing came forth but a little thin yellowish serum, so I ventured to lay it open about 2 inches more: I was not a little startled, after so large an aperture, to find only a glutinous substance bung up this orifice. The difficulty was, how to remove it; I tried my probe, and endeavoured with my fingers, but all was in vain; it was so slippery that it eluded every touch, and the strongest hold I could take.

I wanted in this place almost every thing necessary, but bethought of a very odd instrument, yet as good as the best in its consequence, because it answered the end proposed. I took a strong fir-splinter, such as the poor in that country use to burn instead of candles; I wrapped about the end of this splinter some loose lint, and thrust it into the wound, and by turning and winding it, I drew out above 2 yards in length of a substance thicker than any jelly, or rather like glue fresh made and hung out to dry; its breadth was above 10 inches; this was followed by 9 full quarts of such matter, as is met with in steatomatous and atheromatous tumors, with several hydatides, of various sizes, containing a yellowish serum, the least of them larger than an orange, with several large pieces of membranes, which seemed to be parts of the distended ovary. I then squeezed out all I could, and stitched up the wound in three places, almost equi-distant: I was obliged to make use of Lucatellus's balsam, to cover a pledget, the whole length of the wound, and over that laid several compresses, dipped in warm French brandy; and because I judged that the parts might have lost their spring, by so vast and so long a distention, I dipped in the same brandy a large napkin 4 times folded, and applied it over all the dressings, and with a couple of strong towels, which were also dipped, I swathed her round the body, and then gave her an opiate medicine, which was ordered to be repeated at intervals.

Next morning I found her in a breathing sweat, and she informed me, with joy, that she had not slept so much, nor found herself so well refreshed, at any time for 3 months past. I carefully attended her once every day, and as constantly dressed her wound in the same manner as above, for about 8 days together; I kept in the lower part of the wound a small tent, which discharged some serosities at every dressing for 4 or 5 days. But business calling me elsewhere, I left her, having first instructed her two daughters (both women, who carefully attended her) how to dress her wound, and told them what diet I thought most proper, enjoining them strictly to observe what I ordered.

She afterwards mended apace to the admiration of every one, and lived in perfect health from that time, which was in August 1701, till October 1714, when she died in 10 days sickness.

That this tumor, or rather dropsy of the ovarium, proceeded from the midwife's rashness in pulling away the placenta, not knowing how to separate it from the uterus skilfully, seems to me plain from what the woman herself told me, and what fell out afterwards.

Method of preparing Prussian Blue, communicated from Germany, to John Woodward, M. D. Gresham Professor of Physic, and F. R. S.* N° 381, p. 15. *Translated from the Latin.*

Take of crude tartar and crude nitre dried, each ℥iv. Let them be reduced to a fine powder and mixed together; then detonate them in a charcoal fire, and

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* This is the first account that was made public of the method of preparing this blue colour, the process having, until this time, been kept a secret. The discovery of this useful pigment was wholly accidental. It happened (as Macquer relates from Stahl) in the following manner. A manufacturer of colours, named Diesbach, who usually prepared a lake of cochineal, by mixing a decoction of this substance with alum and some green vitriol, and by precipitating the mixture with a fixed alkali, being one day in want of fixed alkali, borrowed from Dippel, in whose laboratory he worked, some salt of tartar from which that chemist had several times distilled his animal oil, and observed that the lake precipitated by means of this alkali instead of being red, was of a fine blue colour. Dippel, to whom he related the appearance, knew that it must have been caused by his alkali, and attempted to produce the same effects by giving the same quality to fixed alkali by an easier process. In this he succeeded. This blue has been called Prussian or Berlin blue from the place where it was first made. An account of it was published in the Berlin Memoirs for 1710, but no description was then given of the process for making it; of which Dr. Woodward in 1724 first published the particulars in the above communication.

you will have ℥iv of salt of tartar. While this salt is yet hot let it be well pulverized, and let there be added to it ℥iv of bullock's blood thoroughly exsiccated, and reduced to a fine powder. Having mixed them well together, put them into a crucible, so that a third part of it may be left empty. After applying a lid[*] to the crucible, let it be placed in the fire, surrounded with coals, and be gradually heated, so that the contents may be kindled into a flame, but not too violently. Let the materials be kept in this degree of heat until the flame and combustion abate; after which let the fire be increased until the matter in the crucible becomes red hot, and scarcely emits any more flame. Then take the crucible out of the fire, empty its contents into a mortar, and reduce them to a fine powder; having at hand 4lb. of rain water, made boiling hot, into which the pulverized materials, while they are yet hot, are to be thrown, and to be boiled therein for the space of $\frac{1}{2}$ an hour. The decoction is to be strained through a linen cloth, and upon the black residuum is to be poured a fresh quantity of water, to be again boiled and strained. This is to be repeated until the water ceases to acquire any saline [or lixivial] taste. All the moisture which is retained by the residuum left on the strainer is to be squeezed from it; the different decoctions are then to be mixed together, and the collected liquor is to be placed on the fire, and to be evaporated until it is reduced to 4lb. It is then to be kept for use, and marked N° 1.

In the next place, take of English vitriol[†] slightly calcined[‡] to whiteness ℥j. Dissolve it in ℥vj. of rain water, filter it through blotting paper, and let it be marked N° 2.

Lastly, take of crude alum ℥viij. Dissolve it in 4lb. of boiling water, and when the solution is completed pour it into a sufficiently capacious broad earthen vessel, and mix with it the vitriolic solution N° 2, made boiling hot, and the lixivium N° 1, likewise of a boiling heat. [The vitriolic solution and the lixivium to be boiled in separate vessels.] A great ebullition [effervescence] will instantly take place, and a colour resembling that of mountain-green, or chrysocolla, will be produced. At different times during the ebullition, or effervescence, the mixture should be poured from one vessel into another; and when the effervescence is over, the liquor should be left at rest. It should afterwards be poured out upon a linen strainer, that all the water may run off,

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[*] Either not fitting tight, or being provided with an aperture to give exit to the flame during the combustion and calcination of the matter contained in the crucible.

[†] Green vitriol, or vitriol of iron.

[‡] Exposed to a gentle heat, and only for a short time, so as to be exsiccated rather than calcined; for by calcination, in the proper acceptation of the term, vitriol of iron is converted into a red powder.

leaving the sediment (i. e. the colour) upon the strainer, from which it is to be scraped off with a wooden spatula into a smaller earthen vessel. Then pour upon it ℥ij or ℥iij of spirit of salt, and immediately a most beautiful blue colour will be produced. To this mixture, after it has stood at rest for a night, add a large quantity of rain water, and stir it well with a spatula. When the [blue] matter has settled, let this water be poured off and fresh water be added; and let this be repeated until the water, with which it is washed, ceases to have any taste. Then empty out the fine blue sediment upon a linen cloth, that the water may drain from it, and dry it by a gentle heat, for use.

N. B. It is of great consequence in the preparation of this blue colour, to regulate the calcination of the blood with the salt of tartar properly; beginning with a gentle, proceeding to a moderate, and ending with a strong heat.

The lixivia should be mixed together as hot as possible, all at once, and with the utmost expedition.

Observations and Experiments on the foregoing Preparation. By Mr. John Brown, Chemist, F. R. S. N° 381, p. 17.

Dr. Woodward having lately communicated a paper to the R. S. containing a process for making the Prussian blue; Mr. Brown went through it exactly, according to the proportions there prescribed; and observed, that by a calcination of ℥4 of blood dried, with ℥4 of sal tartari, in 2 hours time that part of the operation was over, and a black spongy substance remained in the crucible weighing ℥4, a solution of which being made in boiling water, and afterwards filtered, the remainder, when dried, weighed 9 drachms, avoirdupois, the former having been weighed by the same kind of weight.

The loss in the solution and filtration of the vitriol and alum, is not worth noticing, having both been very clean before they were dissolved. The mixtures being made as prescribed, with the addition of the sp. salis, the product was a very fine blue, which when well edulcorated by frequent washings, and after that thoroughly dried, weighed ℥1, or a little more, and entirely answered the character the author gave of it.

Among the several experiments made with these liquors, the solution of vitriol, the solution of alum, and the spirit of salt, though they always produced a blue, yet that blue differed in degrees of colour, according to the varied proportions of the vitriol and alum, and the colours produced from these several propositions, were each of them improved by the addition of the sp. salis.

The differences in colour, arising from the several proportions of the vitriol and alum, confirm the truth of the author's prescript, as being the most exact and best proportioned to produce the finest colour. The only misfortune he takes notice of, as attending his prescript, is what may happen in the calcination.

It would be curious to know what gave the first hint for the production of so fine a colour, from a combination of such materials; especially when we come to consider, that the blood has the greatest and principal share in this surprising change. Doubtless blood of any kind, or flesh of any kind, would produce the same effects, but the latter would not produce so beautiful a colour as the former. Having dried some beef freed from its skin and salt, and pursued the same course as with the blood, there was a sensible difference to be observed during the calcination, and a very manifest one in the beauty of the 2 colours, when finished.

To prove the share the blood has in this change, the following experiments may be convincive.

The solution of alum mixed with that of the vitriol, produces no alteration of colour: if to these there be added the sp. salis, the appearance is the same; but if to the whole you put the lixivium with blood, there precipitates a blue.

If you substitute, instead of the lixivium with blood, a lixivium made with the same salt of tartar only, which then becomes an ol. tartari; and after the mixture of the solution of alum, with that of the vitriol, you pour on this ol. tartari, there follows indeed a precipitation, but of no colour; and if you add the spirit of salt, it so strongly attracts what is precipitated, as to render the muddy mixture perfectly clear.

The very same effect will follow, if any volatile alcalious spirit is made use of as a precipitant, or any volatile salts dissolved in water; nor can the blood itself be supposed to communicate this change from any such properties, the heat of fire it undergoes in the calcination, being sufficient to throw them off.

In the calcination of the dried blood and salt of tartar, it was observed, that there was a loss of just half. It is difficult to determine exactly what quantity of either was lost by this calcination, but it will easily be granted, that there was lost a far larger quantity of the blood, than of the salt of tartar; and that is obvious from an experiment, by which, when the salt of tartar was calcined by itself, with the same degree of heat, it lost less than $\frac{1}{8}$, whereas, when the dried blood was calcined by itself, it lost more than $\frac{6}{8}$.

The blood, in calcination with the salt of tartar, communicates its tinging quality to the salt, or that quality is extracted from it by the salt, and passes with it in its dissolution in the boiling water.

To prove this, some dried blood was calcined by itself, and a strong decoction was made of it in water, and afterwards filtered: this, when mixed with the former solutions, produced little or no alteration; but on the addition of the sp. salis, changed to an amber colour, without any precipitation.

When this liquor was mixed with the ol. tartari, and poured to the former solutions, it caused a precipitation, but no colour; and the sp. salis, as in the other experiment, made the liquor clear again, but left this also of an amber colour.

The change of colour is not effected in any of the materials, except in that of the solution of vitriol; so that the alum seems only to be of use in fixing the colour, as it is often used by the dyers for that purpose; and the sp. salis gives it a deeper dye. For if the lixivium with blood be poured to the solution of alum alone, there will fall a sediment a little on the purple; to which if you add the sp. salis, it changes the colour, and the sediment is brown.

So much the same changes will be produced, if you pour the sp. salis to the lixivium, but not the least appearance of a blue: whereas, as is beforementioned, when the lixivium is poured to the solution of vitriol, there immediately follows the blue, which is still heightened by the addition of the sp. salis.

It will not be improper to take notice, that as the author orders all the liquors, except the sp. salis, to be boiling hot when mixed, so it is certain the colour is thereby more immediately produced, and looks more beautiful; but most of the experiments here mentioned were made with the liquors cold, and the colours came to their beauty with a little washing. In one of the experiments with the liquors cold, after the lixivium with blood had precipitated the blue in the mixture of alum and vitriol, by pouring in a little more of the lixivium, the blue all disappeared, and an ugly muddy colour was left; but the addition of the sp. salis soon discharged that, and the blue returned.

In calcining the beef and salt of tartar, the matter left in the crucible weighed just half of the whole mixture, as in that with the blood; but after boiling it in water, the residuum in the filtre when dried, was very near a third less in proportion than the other. From whence may be reasonably inferred, that the salt of tartar holds a larger share of the beef in the one operation, than of the blood in the other.

Having, in the former part of this account of the Prussian blue, proved. by the experiments there mentioned, that the solution of vitriol was the only subject among those ingredients, that the lixivium of blood produced this change of colour in it; and having since considered that the vitriol used in this preparation, is no more than iron dissolved by a liquor running from the pyrites, when exposed to the weather, which is afterwards boiled up and shot into crystals; it seemed to follow, as a natural consequence, that this metal is the subject on which the lixivium of blood produces the change; and this thought gave occasion to the following experiments on metallic bodies, in order to observe if the same change of colour could be produced in any of them.

To a solution of silver in aquafortis was poured the lixivium of blood, which occasioned a coagulum of a pure flesh colour. The lixivium made with flesh produced a whitish coagulum, and the ol. tartari a much whiter. By the addition of the sp. salis to each of these, the bloom of the flesh colour was taken off in the first, but suffered no other change. In the 2d the coagulum was a little tinged with blue; and in the 3d the white was manifestly improved. The bluish tinge in the 2d of these experiments cannot entirely be assigned as the effect of the lixivium with flesh, because silver, when thus dissolved, whether precipitated with salt-water, or ol. tartari, will, after it has stood some time, contract a bluish tinge, and this from an alloy of copper, from which it is not entirely freed.

The same liquors were used to precipitate the mercury in the merc. sublim. corr. dissolved in water; the consequence of which was, that the lixivium with blood produced a pure yellow; the lixivium with flesh an orange colour; and the ol. tartari a dingy red. The addition of the sp. salis to these, made some very odd alterations; for the first changed its yellow colour for an orange; the 2d its orange for a blue; and the 3d became quite clear again without any colour. The blue colour in the mixture of the lixivium with flesh, and solution of sublimate, may be accounted for from the vitriol in the composition of the sublimate; but it will not be so easy to give a reason why the same colour should not have been produced from the lixivium with blood, and the same solution.

Copper, when dissolved in aquafortis, tinges the water of a green colour; and if to this you pour the two lixivia of blood and flesh, the coagula are much alike, viz. a white tinged with green; but when you add the sp. salis, they both change, and become of a colour not unlike the copper itself before it is dissolved in the aquafortis. If the ol. tartari be poured to a solution of the copper, the coagulum is a pale green, which coagulum the sp. salis dissolves, and leaves the liquor clear, but green, as before precipitation.

Tin-glass,* an imperfect metal, dissolved in aquafortis, and mixed with the lixivium of blood, made a milky coagulum; and by the addition of the sp. salis, after some time standing, its upper surface changed to a light blue. The lixivium of flesh and the ol. tartari produced both white coagula, which the sp. salis scarcely alters.

Lead, dissolved in spirit of vinegar, produces much the same white coagulum, when mixed either with the lixivium of blood, flesh, or the ol. tartari, nor does the sp. salis make any alteration.

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* Bismuth.

By all these experiments it is pretty evident, that not any of these metallic bodies were affected by the lixivium of blood, so as to produce this fine blue. The two metals untried are gold and tin, the latter of which, when dissolved in spirit of vinegar, has so near a resemblance to lead dissolved in the same menstruum, that in all probability the experiments would answer much alike in both. What may be expected from gold, I am not yet so well assured of, as from iron, which when dissolved in sp. vitrioli, will answer all the experiments that have been tried with the solutions of vitriol, and produce as fine a colour; nor can this be owing to any property in the dissolvent itself, which, though drawn from the same kind of vitriol all along used in these experiments, yet is so altered by the violent fire in its production, as not to answer in any trials to the vitriol itself.

May we not therefore hence conclude, that iron is the metal, that is the subject of this beautiful colour, produced by means of the lixivium with blood?*

The Remainder of the Bills of Mortality, &c. of the several Towns in Europe. Extracted from the Acta Breslaviensia. By Dr. Sprengell, F.R.S. N° 381, p. 25.

A List of those that were Born and Buried in Breslaw, in the Year 1720, i.e. *from the 25th of Dec.* 1719 *to the 24th of Dec.* 1720.

Buried 1816.—Christened, males 564; females 556; total 1120.—Married 460 pair.—Among the dead were married men 385, married women 186, widows and widowers 285, bachelors 113, maidens 113: children to 10 years of age, boys 345, girls 300: stillborn, boys 53, girls 30; total 1810.

Of the year 1721—In Breslaw—Buried 1482.—Christened, males 610, females 585; total 1195. Married 405 pair.—Among the dead were, married men 301, ditto women 157, widows and widowers 208, bachelors 92, maidens 82: children to 10 years of age, boys 324, girls 248: stillborn, boys 42, girls 28; total 1482.

In Jauer: buried 114—Christened 124.

In Vienna: buried 6490—Christened 4104. So that there died 2386 more than were christened.

Among the dead were 43 casualties. Besides eight persons of 90 years old;

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* Mr. Brown was very right in concluding that iron is the metal which is the subject or basis of the blue pigment, produced in these experiments; but the colouring matter itself, viz. the prussic acid, contained in the lixivium of blood, was at this period of time unknown; and was not discovered till about half a century afterwards, by the celebrated Scheele.

one 91, two 92, three 93, three 94, three 96, one 98, four 99, two 100, one 105.

In Dresden: buried 1850—Christened 1396—Married 404 pair. Among the buried were, married men 274, ditto women 206, widowers 42, widows 238, bachelors 128, maidens 93, boys 479, girls 400: stillborn, males 50, females 28.—Among the baptized were, boys 701, girls 690: bastards, males 40, females 51.

In Leipzig: buried 1300—Christened 760—Pairs married 268.—Among the dead were, 218 from 60 to 69; 82 from 70 to 79; 16 from 80 to 88; 2 from 90 to 91 years old.

Among the christened were, 8 posthumous, 14 twins, 63 bastards, and 2 Jews baptized.

So that in this year there died 1300. Born and christened 760. Hence there are 540 fewer born than died

In the Marck.—Christened 16086. Among which were 596 bastards. Married 4613 pair. Buried 13511—More born 2575 than buried.

Among the buried were 28 persons of 90 years old and upwards; besides 3 of 100, 1 of 101, 1 of 102, 2 of 104, 2 of 107, and 1 of 112 years.

In the whole Royal Prussia: born 75275. Buried 58017. So that there are more born 17258 than buried.

In Ratisbon: Christened males 130, females 120; total 250. Among which were 3 pair of twins, viz. 2 boys and 4 girls. Besides 6 bastards, i. e. 3 boys and 3 girls.

Buried 220, viz. Married men 41; married women 43; young men 11; young women 15; children 110, i. e. 67 boys and 43 girls; among which, widows 23; lying-in women 2; stillborn 2; married 67 pair.

In Nurnburg.—Buried 1063. Christened, males 541, females 543; total 1084.—21 more born than died; among the christened, were 16 pair of twins.

In Copenhagen: born 2630. Buried 2247.

In Amsterdam.—The following List contains 7 *Years, viz. from* 1715 *to* 1721. In the year 1715 died 7633; in 1716, 7078; in 1717, 7451; in 1718, 8644; in 1719, 9726; in 1720, 7820; in 1721, 7632 persons.

In Epperies: born 214. Buried 142.

In Dantzic: born 1833. Buried 1435. Married 457 pair.

An Account of the Dissection of an Eye with a Cataract. By Mr. John Ranby, Surgeon.* N° 381, p. 36.

Sept. 21, 1723, one William Sollars, aged 50, complained of a decay in his sight. On examining his eyes, there were two cataracts, that in his right eye almost ripe, the other just forming. Before the operation however was performed, the man fell ill of a fever, and died the 2d of March. Having procured the right eye, in which the cataract was most confirmed, in order to make an exact dissection of it: in examining it, the aqueous and vitreous humours were in their natural state, but the crystalline humour was opaque, and of a foul pearl colour, and more solid substance than in its natural state. The aqueous humour had its natural transparency, nor could there be observed any thing preternatural, either on the iris or uvea, except too great a contraction of the pupil. This very much strengthens the opinion of Maitrejan, Brissé, Heister, and Valsalva, who have severally asserted, that a cataract is only an opacity of the crystalline humour, and that it naturally proceeds from a serous acid, which so far astringes and corrodes its substance, as to destroy its transparency. This Maitrejan confirms by an experiment, of immerging the crystalline humour in a composition of 3 parts water and one of aquafortis, by which he says it may be rendered hard and opaque; but in this point I cannot help siding with the learned Dr. Pitcairn, who has sufficiently proved, that there is no such serous acidity in an animal body. To me nothing seems more easy than to deduce this opacity of the crystalline humour from an inflammation in the blood, or an increased momentum in the fluids, with which it is supplied: for in that case, grosser particles, inconsistent with the transparency, may be impelled into the lymphatic vessels of which it is composed; and that there is an inflammation is sufficiently demonstrated from hence; first, the patient feels often a pungent pain in the eye, which, as it is generally the forerunner of a cataract, so it certainly indicates an inflammation of the part. 2dly, Those maculæ, which appear as it were swimming in the air, plainly prove that there are opaque particles already entered the lymphatic vessels, which compose the vitreous humour. 3dly, The iris, whose colour arises from the blood vessels, as it changes from a lighter to a darker colour, shows the violence of the inflammation, and is therefore esteemed a symptom of the worst consequence.

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* Mr. John Ranby bestowed considerable attention on human and comparative anatomy, on which he published several papers in the 33d, 34th, 36th, and 37th Vols. of the Phil. Trans. He was in high repute as a practising surgeon, and was among the most active and most successful promoters of the inoculation of the small-pox, soon after its introduction into this country.

Observations on the Comet that appeared in Oct. Nov. and Dec. 1723. *By the Rev. Mr. Bradley,* M.A. F.R.S.* N° 282, p. 41.

This small comet was first observed in England by Dr. Halley, Octob. 9, between 7 and 8 o'clock in the evening; it appearing then to the naked eye not much unlike a star of the third magnitude. About 9 he again viewed the comet, and found it considerably moved from its former station, having now passed a small star, which at the time of the first observation was on the other side of it. Comparing the two situations of the comet together, he perceived that its apparent motion at that time was about 8 or 9 minutes in an hour, in a direction towards Sagitta; and that the comet passed very near, if it did not wholly eclipse the forementioned small star, whose place he afterwards found to be in ♒ 7° 22′ 15″ with 5° 2′ N. latitude.

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* Dr. James Bradley, an eminent astronomer, was born at Sherborne, in Gloucestershire, in 1692, and educated at Baliol college, Oxford, where he took his degrees in arts, and then entered into orders. In 1719 he obtained the living of Bridstow, and afterwards that of Landewy Welfry. Becoming also curate to his uncle Mr. James Pound, at Wansted in Essex, with that gentleman he principally gained his knowledge in mathematics and astronomy. In 1721, on the death of Dr. John Keil, Mr. B. succeeded him as Savilian professor of astronomy at Oxford; on which occasion he resigned his church livings, agreeable to the rules of the founder. In the course of Mr. B.'s numerous observations, he discovered and settled the laws of the alterations in the fixed stars, from the progressive motion of light, combined with the earth's annual motion about the sun; as also the nutation of the earth's axis, arising from the unequal attraction of the sun and moon on the different parts of the earth: the former of these effects is called the aberration of the fixed stars, the theory of which he published in 1728; and the latter the nutation of the earth's axis, the theory of which appeared in 1747, deduced from 20 years assiduous observations; by which he communicated to the world two of the finest discoveries in modern astronomy.

In 1730 Mr. B. succeeded Mr. Whiteside as lecturer in experimental philosophy, in the Museum at Oxford. And, on the death of Dr. Halley, he succeeded him as astronomer royal at Greenwich, in 1742; and at the same time the University of Oxford presented him with his degree of D.D. In 1748 the King ordering £1,000 to defray the expence of better furnishing the observatory with instruments, Dr. B. employed those excellent artists, Mr. Graham and Mr. Bird, who provided an ample supply of them, to his satisfaction, of which he afterwards made the most advantageous use during the rest of his life: of his observations an immense number was found after his death, in 13 folio volumes; which were presented to the University of Oxford in 1776, on condition of their being printed and published, which however it seems has not yet been accomplished.

During Dr. Bradley's residence at the royal observatory, the vacant living of Greenwich church was offered to him; which however, he thought fit to decline, alleging "that the duty of a pastor was incompatible with his other studies and necessary engagements;" on which the King was pleased to grant him a pension of £250 over and above the astronomer's original salary from the Board of Ordnance; a pension which has been regularly continued to the astronomers royal ever since. Dr. B. died at Chalfont in Gloucestershire, of a suppression of urine, in 1762, at 70 years of age. His communications in the Philos. Trans. extend from the 33d to the 62d volume.

The night following Mr. Bradley observed it at Wansted; when the result of his observations showed that the comet's place was 307° 6′ 40″ of right ascension, and 11° 8′ 15″ south declination.

Oct. 12, and several nights following, the comet was observed by Mr. B. assisted by his uncle, the Rev. Mr. Pound; and the results of their observations, together with those of Dr. Halley and Mr. Graham, were collected as below, in the following table, which contains the longitudes and latitudes of the comet deduced from the observations, together with the places of the comet calculated from the theory of gravity, for the times of observation on the several days, as also the differences between the observed and computed places. Those differences, not exceeding one minute, show that the observations are not only consonant to each other, but that the places of the stars are likewise near the truth, since the comet's places deduced from them are found all along to agree sufficiently near with the theory of gravity; the truth of which has long since been established by its great author Sir Isaac Newton, and by Dr. Halley.

Temp. Æquat. 1723.	Comet. Long. Observat.	Lat. Bor. Observ.	Comet. Long. Comput.	Lat. Bor. Comput.	Differ. Long.	Differ. Latit.
Octob. 9^d 8^h 5^m	♒ 7° 22′ 15″	5° 2′ 0″	♒ 7° 21 26′	5° 2′ 47′	+ 49″	— 47″
10 6 21	6 41 12	7 44 13	6 41 42	7 43 18	— 30	+ 55
12 7 22	5 39 58	11 55 0	5 40 19	11 54 55	— 21	+ 5
14 8 57	4 59 49	14 43 50	5 0 37	14 44 1	— 48	— 11
15 6 35	4 47 41	15 40 51	4 47 45	15 40 55	— 4	— 4
21 6 22	4 2 32	19 41 49	4 2 21	19 42 3	+ 11	— 14
22 6 24	3 59 2	20 8 12	3 59 10	20 8 17	— 8	— 5
24 8 2	3 55 29	20 55 18	3 55 11	20 55 9	+ 18	+ 9
29 8 56	3 56 17	22 20 27	3 56 42	22 20 10	— 25	+ 17
30 6 20	3 58 9	22 32 28	3 58 17	22 32 12	— 8	+ 16
Nov. 5 5 53	4 16 30	23 38 33	4 16 23	23 38 7	+ 7	+ 26
8 7 6	4 29 36	24 4 30	4 29 54	24 4 40	— 18	— 10
14 6 20	5 2 16	24 48 46	5 2 51	24 48 16	— 35	+ 30
20 7 45	5 42 20	25 24 45	5 43 13	25 25 17	— 53	— 32
Dec. 7 6 45	8 4 13	26 54 18	♒ 8 3 55	26 53 42	+ 18	+ 36

To determine the orbit of this comet, Mr. B. supposed it to describe a parabola, agreeable to what is delivered in the third book of Sir Isaac Newton's Princip. Math. and then he found the inclination of the planes of the orbit and ecliptic 49° 59′: the place of the ascending node ♈ 14° 16′: the place of the perihelion ♉ 12° 52′ 20″: the distance of the perihelion from the node 28° 36′ 20″: the logarithm of the perihelion distance 9.999414: the logarithm of the diurnal motion 9.961007: the time of the comet's being in its perihelion, Sept. 16^d 16^h 10^m equal time. In its orbit thus situated, the motion of the comet was retrograde, or contrary to the order of the signs.

From these elements, by the help of Dr. Halley's general table for comets, to which they are adapted, Mr. B. computed the places in the foregoing table: which agreeing with the observed places as near as the observations themselves agree with one another, show that it would be vain to attempt to determine the true ellipse in which this comet moves, or its periodical revolution, from so small a part of its orbit as that was, which it described between the first and last of the observations; this therefore must be left to posterity, especially since it is certain, that this comet is not one of those of which observations have hitherto been transmitted to us, sufficient to determine the situation of their orbits.

The nucleus of this comet was very little, for it appeared but of a small diameter when first seen, though it was then above 3 times nearer to the earth than the sun is at his mean distance. Its tail was then hardly discernible with the naked eye, but through a telescope one might perceive a faint light extending itself above a degree from the body.

The comet was in opposition to the sun Oct. 1, when it had near 74° S. Lat. and altered its longitude 2 signs in a day. About Oct. 3 it was in its perigee, or nearest distance to the earth, being then almost 10 times nearer to it than the sun is at his mean distance; and its apparent motion was then about 20° in a day, and when Mr. B. last saw it, it was above twice as far off as the sun.

Observations on the same Comet, made at Witham in Essex. By Lord Paisley.
N° 382, p. 50.

His lordship first discovered this comet Octob. 11, about 7 in the evening. It then appeared as a star between the 4th and 5th magnitudes, but a haziness round the head, and some light streaming from it on the side opposite to the sun, induced him immediately to consider it as a small comet; which his observation the next evening abundantly satisfied him of. The tail was visible on the 11th to near a degree distance from the body; it was of a dusky light, not unlike a cloud growing darker and darker towards its extremity. The tail appeared sharper, and not so much spread in the two following observations, and in the last did not exceed one-third part of the first length; it was then of a much darker colour, which made the difference between that and the head more observable, the head yet appearing sufficiently bright. For some following nights his lordship's observations were interrupted by cloudy weather, after which the comet was so far diminished, as only to be known by its motion, its appearance being no ways distinguishable from that of a small nebulous star.

An Observation of the same Comet at Albano. By Sig. Francisco Bianchini. N° 382, p. 51. *Abridged from the Latin.*

Sig. Bianchini perceived this comet Oct. 17. It appeared as a very thin nebulous globe, with a small bright nucleus in the middle. Besides the nebula or atmosphere of a comet, it had a short tail directed to the east.

Sig. B. traced its path through the heavens by several nights observations, by which he found that its path was through the plane of a great circle, intercepting the ecliptic in 9° of Aquarius, at an angle of 80° nearly. Its place, Oct. 21, was in 6° 45′ of Aquarius, with 8° 5′ north latitude. After several trials, he could not find any sensible parallax, consequently its distance from the earth was very considerable.

Observations on Wasps, and the Difference of their Sexes. By the Rev. Mr. Derham, F. R. S. N° 382, p. 53.

In the beginning of July, 1723, Mr. D. observed several wasps flying about on the top of the collegiate chapel in Windsor castle, and particularly frequenting a covering of deal boards, and the pieces of timber lying on the leads. Most of these wasps were of a larger sort than usual. On July 6, he observed a cluster of only three wasps closely embracing each other, one of which was a large female, the other two of a smaller sort. And soon after he found eight or ten wasps closely hanging together, and divers other such like parcels. In the midst of all these was constantly a queen wasp, and only one, the rest being always of a different sort from either the queen or the common wasps. Therefore examining another company of them with greater strictness, he found the queen wasp, in coitu, with one of the other wasps, so closely joined tail to tail, that it was some time before they were parted. After this he caught all the wasps he could on the top of the chapel, but could not see one of the common labouring wasps among them; but all were for the most part male wasps, with now and then a queen, or female, among them, and she generally in coitu.

Hence it appears, that there are three sorts of wasps, the queens, or females; the kings, or males; and the common labouring wasps; each of them very distinct. The queen, or female wasp, by many called the king wasp, is much longer in the body, and larger than any other wasp. The male wasps are less than the queens, but as much longer and larger than the common wasps, as the queen is longer and larger than these. These males also have no stings, which the queens and common wasps all have. And these are those which Moufet says, authors call 'Ακέντρας, and take to be females, though he is of another opinion, imagining all wasps to have stings; on examining a wasp's nest, at Ham, Anno 1587, in which he found no wasps without a sting. But Mr. D. wonders how that curious inquirer missed of these stingless male wasps. Surely he was too hasty in his examination, and not being aware of the difference, he thought the males, which are but few in number to the labouring wasps, were the same, and had stings as well as the rest; or else he made his inquiry at a time when perhaps the males had deserted the nest, which probably they may do, as the male or drone bees are forced to do. In all the nests that Mr. D. searched, he constantly found male wasps, either many or few, according to the size of the nest, and the number of wasps in it. And the part of the nest where these males were found, was chiefly the two uppermost cells, or partings between the combs, but one.

Another thing by which the males may be known from other wasps is their antennæ, or horns; which are longer and larger than either those of the queen, or common wasps; and with them they seem, in running, to feel more than the others do.

But the grand and chief difference are the parts of generation of these male wasps, quite different from other wasps. Mr. D. found, on dissection, that they had a sheath-like penis, with a bulb in the middle and a hooked extremity.

As for the parts of generation in the queen, or female wasps, nothing was to be seen so remarkable as in the male; but those parts are very like what we see in the common labouring wasps; indeed, with the most accurate observations he could make with microscopes, he could not perceive any difference at all. For which reason he supposed it is that most of the writers on wasps and bees, have been very confused and wavering about the sexes of these two tribes of insects.

Two remarkable Observations in Physic, communicated by John Huxham, M. D. to James Jurin, Secr. to the R. S. An Abstract from the Latin. N° 382, p. 60.

Observ. I. *A large Omentum.*—In the first of these observations we have an account of a large omentum, occurring in a soldier's wife, in whom a very bulky hard swelling had formed in the abdomen, preceded and accompanied by excessive pain and violent vomiting, insomuch that not only quantities of black bile, but even the alvine fæces, were discharged by the mouth. The pain was greatest in the left hypochondrium, and the swelling was as hard as a board. From this swelling there arose several excrescences, one or two of which were as large as a child's head, and another about the size of a man's fist. As the swelling increased the woman became affected with dyspnœa. After lingering for nearly 14 months, a period was at length put to her sufferings by death.

On opening the abdomen, which was swelled to a prodigious size, while the rest of the body was greatly emaciated, an immense mass presented itself, resembling suet, except that it was not quite so white; it filled the whole cavity of the abdomen, so that neither stomach, liver, nor intestines could be seen. It had several adhesions, especially in both the hypochondria. It descended almost into the pelvis, was attached to the liver, and oppressed by its weight the stomach, duodenum, colon, and jejunum. It was intimately connected with the adipose membrane of the kidneys, especially the left. The colon, a little above where it terminates in the rectum, was so completely envelloped in this suety mass, as to have no passage for the fæces remaining; hence for many days preceding the patient's death, no fæces were discharged per anum, either spontaneously or by means of clysters.

This enormous omentum, after being taken out of the body, was found to weigh 16½ lb. avoirdupois, not including what was left adhering to the different viscera, and which was supposed to be at least 2 lb. more; a most astonishing mass, if it be considered that even in a very corpulent, full-grown subject, the omentum scarcely weighs 3 lb. Mention, however, is made by Gregory Horstius, and in the Ephemerid. German. ann. x, of even a larger omentum than this.

The blood-vessels in the interior of this substance were exceedingly dilated; some were as large as a goose-quill, others terminated in a sort of aneurisms. From the largest of these aneurisms, as the author terms them, about 6 oz. of black blood were extracted, intermixed with some white lumps. This mass of omentum seemed to be composed of a number of lobes intimately connected with each other. Some of these lobes were about the size of a small apple, and nearly of the same shape. The middle part was harder than the rest, and could scarcely be cut with a knife.

With regard to other particulars observed in this body; the lower part of the liver was become scirrhous, and in the gall-bladder were found several stones of a black colour, like coal, friable, and so light as to swim upon water. The glands of the mesentery were scirrhous; indeed some of them had acquired almost a stony hardness. The small intestines were inflamed; nearly the whole of the colon, with the cæcum, was in a gangrenous state. The right kidney had two ureters. The pelvis of this kidney was divided by a thick septum of the same texture with the rest of the parenchyma of the kidney; thus in the right lumbar region, there was a double kidney, a double pelvis, and a double ureter. In the lower part of the abdomen there were about 2 lb. of serum tinged with blood.

OBSERV. II. *Saliva of an unusual colour.*—In this second observation we have an account of a man aged 40, of a thin, bilious habit of body, who had an attack of jaundice, followed by colic, which last was occasioned by drinking too great a quantity of cyder. An emetic was first given, then a terebinthinate clyster, and an anodyne mixture; afterwards a cathartic bolus, composed of jalap. ℈j, calomel. gr. viij. spec. diambr. gr. vj. laud. solid. gr. j.; and along with this bolus, a draught consisting of tinct. sacr. ℥ij. Copious stools ensued in the space of 12 hours, after which an anodyne was administered at night. In the morning the patient complained of pain and swelling of the fauces, and spat up some thick, brownish spittle; soon after the saliva followed in great quantity, and of a deep green colour, like porraceous bile, only thinner. This flux of green saliva continued for about 40 hours, during which the quantity discharged amounted to two sextarii. The colour of the saliva then changed to yellow, like a solution of gamboge, with an increase rather than a diminution of the quantity. It continued of this colour for the space of 40 hours more, after which it gradually became pellucid, and the salivation ceased as suddenly as it had come on.

Before this illness the patient had suffered two or three attacks of jaundice, within the space of 2 years. Ten years ago he had a very profuse salivation, which came on spontaneously, and brought him into great danger. He was then cured by Dr. Pyne. But at that time not a particle of mercury was given, nor was the saliva at all coloured.

Dr. H. remarks that this appears to be a most extraordinary case, whether the salivation be considered as having come on spontaneously, or as having been excited by the single dose, (gr. viij) of calomel; which he knew to have been rightly prepared, and of which he had prescribed in many other instances larger doses, without producing any effect upon the salivary glands. During the flow of saliva in this patient, the teeth and fauces were as green as if they had been stained with verdigrise. The teeth retained this colour for the space of a fortnight after the patient's recovery. The author further remarks that this salivation proved critical both of the jaundice and colic; for from the moment that it took place, the pain of the bowels ceased, and the greenish colour of the skin began to go off, while the urine was secreted more abundantly, but of a blackish colour. It is further remarked that though the patient was before extremely languid and almost dying, yet he bore this profuse salivation well. Dr. H. considers the large quantity of cyder which the patient drank, as the procatartic cause of the green coloured saliva. The sour cyder, he remarks, like all acids, would turn the bile green, &c. &c.

On the Effects of Inoculation; the Eclipse of the Sun in November 1722; *and the Venom of Spiders. By Mr. Tho. Robie, Physician in New England.*

N° 382, p. 67.

They had not hitherto observed any ill effects of inoculation in New England; the inoculated being as well, and some of them a great deal better than ever. As for the ill consequences that have been in England, Dr. R. can hardly think they are the genuine effects of inoculation, but may arise from some previous disposition to distempers, or for want of due evacuations after inoculation, and too soon healing the places of incision.

As to the great eclipse of the sun in November last, Dr. R. observed it Nov. 27, 1722, at $7^h\ 27^m$ morn. He saw the sun rise eclipsed, on its supreme vertex to the south, about 4 digits, though some on the top of the new college saw it 2 or 3^m before. The sun's true rising this morning was $7^h\ 30^m$; hence the refraction is about 6′. From this time, till about $8^h\ 30^m$ or 40^m, he saw no more of the sun, but then he judged it was eclipsed 6 digits or more.

At $9^h\ 25^m\ 45^s$ he saw the moon go off the sun.

At $9^h\ 25^m\ 45^s$, Mr. Danforth, in a room just by, saw the shadow go off the paper about 30° from its lower vertex to the east.

At $9^h\ 25^m\ 20^s$, Mr. Appleton saw the shadow go off the paper fixed to the college brass quadrant at his house.

Mr. Owen Harris, an ingenious schoolmaster in Boston, says, he observed the end at about 26^m past 9.

At Boston the eclipse was observed, allowing for its distance, as Dr. R. observed it at the college. And at Barnstable, on Cape Cod, there was but a little left of the sun, and nearer the head of the cape there was a ring of light quite round the moon.

Dr. R. gives an account of a remarkable accident relating to the venom of spiders. Sept. 13, 1722, one Nat. Ware of Needham was bit by a small spider, which he could not give an exact description of, crushing it to pieces between his stocking and leg. The account he gave is this, viz. that getting up early in the morning, and putting on his stocking, he presently felt something bite his left leg a little above his ancle, about half an hour after he felt a pain in that leg, and about half an hour from his first perceiving pain in his leg, he felt a pain in his groin, and at the same time a creeping pain in the calf of his left leg; and about one hour after it got into the small of his back, and then round him, and in his stomach, and in his right thigh, and afterwards a numbness in his head. The pains were not constant and fixed, but erratic and very acute. His pulse was very low and heavy.

Sept. 14. In the morning the man came to Dr. R. and was much better, though he had but little sleep in the night. The means the Doctor used were only sp. cor. cerv. et sal vol. corn. cerv. with vinum viperin. and onions or garlic, externally applied to the place where the wound was. These things raised his pulse, and so it seems assisted nature to throw off the venom.

Observations made in Italy of a Lunar Eclipse, on Sept. 8, 1718.

N° 382, p. 71.

I. Observations made by Sig. Giovanni Poleni and Giovambatista Morgagni, at Padua, the house of Signor Pietro Bembo, noble of Venice.

The beginning not seen for clouds.
At 7^{h} 49^{m} 4^{s} an almost total immersion.
9 36 4 beginning of the emersion.
10 41 2 the apparent end of the shadow.
10 42 57 the end of the penumbra.
All by apparent time.

II. Observations made in the palace of the Istituto delle Scienze, at Bologna; by Signor Geminiano Rondelli, Giuseppe-Antonio Nadio, and Giulio-Cæsare Parisi.

The beginning not observed.
At 7^{h} 47^{m} 35^{s} total obscuration, true time.
9 33 40 beginning of the emersion.
10 37 39 end of the eclipse.

III. Observations made in the suburbs of Bologna southwards; by Signori Eustachio and Gabbrielo Manfredi.

At 6^{h} 42^{m} 13^{s} beginning of the eclipse, apparent time.
7 47 50 the total immersion of the moon.
9 31 20 the beginning of the emersion.
10 38 5 the end of the true shadow.

In the Ephemerides published in the year 1715, from M. Cassini's Tables, for the use of the Istituto Bolognese delle Scienze, the beginning of these eclipses was marked at 6^{h} 41^{m}, the total immersion at 7^{h} 46^{m}, the beginning of the emersion at 9^{h} 33^{m}, the end at 10^{h} 38^{m}, which times scarcely differ one or two minutes from the times observed.

IV. Observations made by the Marquis Antonio Ghisilieri, at Bologna, on the observatory in his own house.

At	6^h	40^m	23^s	the beginning, doubtful; apparent time.
	7	46	37	the total immersion of the moon.
	9	33	50	the beginning of the emersion.
	10	37	42	the end of the eclipse.

Experiments and Observations on the Freezing of Water in Vacuo. By M. Fahrenheit. N° 382, p. 78. *Abridged from the Latin.*

Among the several surprising phænomena of nature, M. Fahrenheit always thought the freezing of water not the least considerable; hence he was often desirous of trying what would be the effects of cold, on including water in vacuo; and because the 2d, 3d, and 4th of March, 1721, O. S. were favourable for that purpose, he made the following experiments and observations.

Before he comes to the experiments themselves, he premises some things on his thermometers, the division of the scale, and his method of exhausting them. His thermometers were chiefly of two sorts; one filled with spirits of wine, the other with mercury; and their lengths were different, according to the uses they were designed for; but they all agreed in having the same number of degrees, and in the fixed limits of their variations. The scale of thermometers for meteorological observations only, begins below at 0, and ends at 96 degrees. There are three fixed limits in this division; the first is in the lowest part or beginning of the scale, and produced by the commixture of ice, water, and sal-ammoniac, or even sea salt; if into this mixture the thermometer be put, it descends to 0. This experiment succeeds better in winter than in summer. The second limit is when water and ice are mixed together without the abovementioned salts; for putting the thermometer into this mixture, it is at 32 degrees, which M. Fahrenheit calls the limit of initial congelation; for stagnant waters are covered with a thin crust of ice in winter, when the thermometer is at that degree. The third limit is at the 96th degree, to which the spirits are dilated, while the thermometer is held in the mouth or under the armpits of a healthful person, till it perfectly have acquired the same degree of heat with the body. The scale of the thermometers for determining the degrees of heat in boiling liquors, also begins at 0, and contains 600 degrees; for, the mercury itself, with which the thermometer is filled, begins to boil at about the same degree.

That the thermometers may be the sooner affected with all the changes of heat, they are provided with glass cylinders, instead of balls; for, by reason of the greater quantity of superficies, they are sooner penetrated by the different degrees of heat.

M. Fahrenheit's method of exhausting was thus; a glass ball, furnished with a small tube, 3 or 4 inches long, and tapering at the extremity, is heated over a fire; after which the extremity of the tube is immersed in water, till by the cooling of the air, contained in the ball, it become filled with some drops of the water; and then it is held with a pair of tongs over the broader flame of a lamp, or over live coals, till the water in the ball begin to boil, and the vapour forcibly burst out; this boiling is continued for some time, after which the ball is removed from the fire, and the flame of a candle applied to its extremity. As the ball cools, the vapour rarefied by the fire, is successively condensed, and its egress gradually diminished; and as soon as it entirely ceases, the extremity of the tube melts, and the ball is hermetically sealed and exhausted. Whether all the contained air be well exhausted in this manner, may be tried by breaking the extremity of the tube under mercury; for then the ball will be entirely filled with mercury, if it be carefully broken without admitting the external air. You may also break it under water; but though it be done ever so carefully, the ball will not be completely filled with water; for, while the water enters the exhausted ball, the air, which is always mixed with water, separates from it into very small bubbles, which after uniting, form a larger one in the ball. In the same manner the ball may be exhausted, if the third, the half, or a greater part of the ball be required to be filled with water; for, it is first filled with the desired quantity of water, and then, after boiling the water, it is hermetically sealed.

The experiments were as follow: March 2, 1721, he exposed to the cold a glass ball, about an inch in diameter, exhausted in the manner abovementioned; and filled with rain-water almost half full; the temperature of the air in the thermometer was marked at 15 degrees. In an hour after, he found the water still fluid in the ball. He then left the ball exposed all night in the open air, and next day, viz. the 3d of March, at 5 o'clock in the morning, he found the water still fluid, and the thermometer at the same degree; the cause of which unexpected phenomenon he attributed to the absence of the air. To discover the truth of this conjecture, he broke the extremity of the tube; that the exhausted ball might be again filled with air; on which the whole mass of water was suddenly mixed with very thin lamellæ of ice. He broke the ball, and putting some of the ice into some water in a glass cup, he observed it floated.

A little time after, he observed all the water mixed with icy lamellæ; yet the greatest part of the water still continued fluid between the interstices; the thermometer, put into this mixture, stood at 32 degrees. On repeating the experiment with two other balls, and after preparing them in the manner abovementioned, he exposed them for an hour in the open air, the thermometer being then at 20 degrees; an hour after, he found the water still fluid in both the balls, but after the exhausted ball was again filled with air, the water, as in the former experiment, was very soon mixed with icy lamellæ; and their production was so instantaneous, that it could hardly be observed with the eye. Before he broke one of the balls, he separated the water in the said cup from the icy lamellæ, on which he broke the ball, and threw the ice into water; the ice, it is true, floated on the water, but he in vain expected the production of the lamellæ in the cup. M. Fahrenheit postponed the further trial of these experiments till 11 o'clock at night, when he exposed three balls in a keen frost; two of these balls were again filled with water about half full: and the third ball was filled about three-fourth parts full; the temperature of the air was by the thermometer marked at 26 degrees; at 4 o'clock in the morning he found the temperature of the air the same by the thermometer, and the water in the two balls, which were only filled half full, still fluid; but in the third ball the water was frozen and the ball broken. The ice was intermixed with very small bubbles, but few in number, and its transparency was very much interrupted, and resembled the confused crystallization of some salts.

M. Fahrenheit, being still desirous of viewing the production of the lamellæ in the glass cup, took the said vessel into the room where the former experiments were made, and happening to stumble a little, the water in the glass was agitated, and in that very instant the whole mass appeared intermixed with several icy lamellæ. By this accident he discovered, that ice could be produced in pretty cold water by agitating it, and this put him on trying whether water would freeze in vacuo by agitation. Therefore, after shaking the ball a little, he observed the same phenomenon, and at the same time found his mistake in attributing the fluidity of the water to the absence of the air.

Concerning the Difference in the Height of a Human Body, between Morning and Night. By the Rev. Mr. Wasse, Rector of Aynho in Northamptonshire. N° 383, p. 87.

Mr. Wasse having measured a great many sedentary people and day-labourers, of all ages and shapes, found the difference in their height between the morning and night to be near an inch. He tried himself when sitting, and found it in like manner; particularly, August 21, 1723, he sat down, at 11 in the morning, and fixed an iron pin so as to touch it, and that but barely. Afterwards fatiguing himself for half an hour with a garden-roller, the consequence was, that at $12^h\ 30^m$ he could not reach the nail sitting, by about $\frac{5}{10}$ of an inch. At 2 the same day he wanted near $\frac{6}{10}$ of an inch. On the 21st, at $6^h\ 30^m$ in the morning, he touched the nail fully; and after the abovementioned exercise for only a quarter of an hour, at $7^h\ 14^m$ he fell short almost as much as before. On the 27th, having sat up late with some friends, he was faint, and felt himself heavy on the ground, and without any spring, and at 9 next morning he did not reach the nail, though he had used no exercise. He rode out, but could not reach it that day. On the 28th he rode about 4 miles; and whereas at 6 that morning he reached the nail, he had lost $\frac{6}{10}$ of an inch by 8. Sept. 19th he came from Oxford a little tired, and next morning at 8 wanted $\frac{1}{2}$ an inch. After studying closely, though he never stirred from the writing-desk, yet in 5 or 6 hours he lost near an inch All the difference between labourers and sedentary people is, that the former are longer in losing their morning height, and sink rather less in the whole than the latter. When the height is lost, it can be regained by any rest that day, or by the use of the cold bath.

The alteration in the human stature, it seems proceeds from the yielding of the cartilages between the vertebræ, to the weight of the body in an erect posture.

Some Remarks on the foregoing Observation. By Mr. William Beckett, Surgeon, F. R. S. N° 383, p. 89.

The remarkable difference in the stature of human bodies, in the space of a few hours time, Mr. B. found to be fact, by several experiments. He further observed, that in young persons the alteration has been more considerable than in those that are aged. The trials equally succeeding in a sitting as in a standing posture, will naturally lead us to believe, that it must necessarily be from the trunk of the body, or some of its parts, that this remarkable alteration is brought about.

There is something so surprising in the structure and disposition of the spine, that nothing but such a peculiar contrivance could so curiously have fitted it for the respective uses and purposes it was ordained for. The thickness and shortness of the bones, with the intervening cartilages, assisted by the boney processes, dispose it to a motion peculiar to itself. Whereas had the bones been of any considerable length, on bending the body, the articulations must have made a large angle on their inmost edges, and the spinal-marrow have been continually liable to be injured; or had the cartilages been entirely wanting, it would have been as useless as if it were but one bone; so that being rendered incapable of bending the trunk of the body, it must have always remained in an erect posture. But by the present disposition of its parts, it is not only absolutely secured against any such inconveniencies, but, though so small a pillar, it is capable of supporting, without hazard, such prodigious weights, as we find it does.

Another particular, which bespeaks the utmost wisdom and design in the contrivance of this part, is the remarkable difference there is in the thickness of the cartilages, placed between the bones of the spine. The vertebræ of the breast requiring but little motion, the cartilages are there but thin, in comparison of those of the loins, which being very thick, the lowest more especially, the motion is there vastly greater; and the cartilages being abundantly thicker before than behind, this is the reason that we bend our bodies so much more forward than backward. And by this admirable method of disposing of the thicker parts of the cartilages forward, it is, that in all violent exercises, the parts contained in the belly and breast are, in a great measure, secured from any damages they might have been liable to, because by the pliableness and elasticity of these cartilages, they break the violent shocks the viscera must otherwise have necessarily sustained on such occasions.

From what is here remarked, in regard to these peculiar properties of the cartilages placed between the bones of the spine, we may reasonably suppose them to be certain compressible, dilatable, elastic bodies, which like other bodies, endued with the same qualities, will naturally yield to any incumbent weight, sufficient to force the particles of matter of which they consist, into a more strict and close union, and that when this compressive power is removed, they will of themselves recover that state they enjoyed before they were obliged to give way to that pressure. Now the lowest of all the cartilages of the loins is the thickest, and so consequently it contains a greater quantity of matter than any of the rest; by which means it becomes more disposed to have its thickness diminished, and that all of them gradually become thinner, even to the top of the spine. Now all superior bodies, if they come to an immediate contact, pressing on their inferior, it must necessarily follow, that the whole weight of the body, except the lower limbs, must press upon and be sustained by the lowest vertebræ and their cartilages; but these cartilages, as has been observed, being much thicker in this part than the other, and the incumbent weight bearing harder upon them, they must be compressed more than the other; and so consequently, when this weight is removed, their expansion, from their natural elasticity, will be greater also.

This being the natural state and disposition of these parts, during the whole space of time we are usually employed about our necessary avocations, till we dispose ourselves to rest, the cartilages of the spine will, by their compressible and yielding properties, become more close and compact from the perpendicular pressure they sustain, and so consequently the spine, the only support of the trunk of the body, will become shorter. But when this superior weight shall be entirely removed, by placing the body in a horizontal posture, as it always is when we are in bed, the compressed cartilages will, by their natural elastic power, begin gradually to enlarge themselves, till they recover the same expanded state they enjoyed before they were forced to give way, and yield to the incumbent pressure; thus producing a considerable alteration in a person's stature, as mentioned in the preceding letter. For if we only consider, that the beforementioned compressive power will lessen the thickness of all the cartilages, in proportion to the quantity of matter they contain; and that there are usually reckoned about 24 in number, it will be no difficult matter for us to apprehend, that their natural expansion being recovered by our customary repose, the aggregate of the whole of the expansions may amount to about an inch. Now if the alteration be so considerable as this, occasioned only by the bare incumbent weight of the superior parts of the body, without any additional force applied to compress the cartilages yet closer, how much more may we reasonably imagine it would be, were the experiment tried on those persons, whose usual employment it is to carry heavy burthens. This alteration is not to be expected to be the same in aged persons, as in those that are younger; because the cartilages, as we advance in years, gradually grow harder and harder, till many of them arrive to the solidity of a bone; that is, by degrees they lose their spring or expansive power, and at length continue in a compressed state of rest. And this is doubtless one principal cause, why old people not only seem to have lost somewhat of their former height, but are actually shorter.

A Catalogue of 50 *Plants from Chelsea Garden, presented to the Royal Society for the Year* 1723, *by the Company of Apothecaries of London, pursuant to the Direction of Sir Hans Sloane, Bart.* N° 383, p. 93.

Observations on the Variation of the Horizontal Needle at London, in the latter Part of the Year 1722, *and the beginning of* 1723. *By Mr. George Graham, F. R. S.* N° 383; p. 96.

The figure of the three needles, with which the experiments were made, was prismatic; their lengths were nearly 12.2 inches; their ends, which pointed to the divisions, being filed to an edge, which made a fine line perpendicular to the horizon. The caps of two were of crystal, the other of glass; they were well polished on the inside, in that part which touched the pin they moved on. The box was brass, and of a breadth sufficient to admit of 20° on each side the middle line, and covered with a piece of ground glass. The circular arches at the ends were raised so much above the bottom of the box, as to have their upper surfaces, on which the divisions were cut, lie in the same plane with the needle, and at such a distance from each other, that the needle might play freely between them. A few of the degrees at the north end were divided into six equal parts, each division being 10′. It was easy, by the help of a convex glass, to determine the pointing of the needle to less than a quarter of these divisions, or to about 2′ of a degree. The pin on which the needle moved, was of steel hardened, and ground to a fine point; and by a spring placed in the box, the needle might be raised from off the point, and let down again at pleasure, without removing the glass, or disturbing the box. By this means both the sharpness of the point, and polish of the cap were better preserved from injury, when there was occasion to move the box. A small piece of brass was made to slide on that end of the needle which pointed to the south, for readily bringing it to an horizontal position; for according to the different strength of the touch, the north end of the needle will dip more or less.

For determining the quantity of the variation, a meridian line was stretched on the top of the house, between the rails of the leads, which were above 15 feet asunder. The compass-box was placed on a wooden stool, with 3 feet, that had nothing of iron about it, and its top set level by a plumb-rule. The first needle was a little above $\frac{3}{10}$ of an inch broad, about .06 in thickness, and weighed about an ounce Troy; the cap of crystal. After some trials with this needle, it was made narrower, not to exceed half a tenth of an inch, and it then weighed 5 dwt. 5 grains. The 2d needle was at first about $\frac{3}{10}$ of an inch broad, and .04 thick, the cap of glass; and after several trials, it was made so much narrower, that its breadth was a little less than its thickness, and it weighed 2 dwt. and 5 grains. The 3d was nearly of the same dimensions with the 2d, and weighed 2 dwt. 3 grains. The reason for making the first two needles so heavy, was to try whether they would return more constantly to the same situation than lighter ones. But notwithstanding each of them would settle very exactly in the same place, for a great number of trials made immediately one after another, they would differ at times considerably from their former directions.

This occasioned the making them narrower, fearing their breadth had been some way concerned in this irregularity. But after the alteration, the same thing happened, though nothing of it seemed to proceed from any friction on the point. This made the lighter needles to be preferred, as less apt to injure the point they moved on, and as exact in returning to the same situation. After many trials, Mr. G. found all the needles would not only vary in their direction on different days, but frequently at different times of the same day;

and this difference would sometimes amount to upwards of half a degree in the same day, sometimes in a few hours. Mr. G. found it of no consequence, whether the needle was drawn aside or let alone, the shaking of the floor by walking upon it, or the trembling of the house by the coaches in the street, was sufficient to overcome the small friction on the point.

The box was placed in the room more than 6 feet from the nearest wall, and above 13 feet from the grate in the chimney, and no iron was at any time brought near it. The needles were all touched by that excellent loadstone presented to the Society by the Right Hon. the Lord Paisley. It may not be improper to take notice, that the needles were not touched on the naked stone, but with its armour on, generally on that part of the capping nearest the poles; but there was no difference in the direction, by touching on another part. When the needle was observed increasing, or decreasing in its variation, Mr. G. has very frequently, with a key, drawn it the contrary way several degrees, and then, by letting it return very gently, till it has been within a degree, or less, of the place it stood at immediately before, it stopped there for some time, by holding the key at a proper distance; and withdrawing his hand gradually, tried to make it stand short of its former place, but could never succeed. By this method, and several others made use of, he was well assured these changes in the direction are owing to some other cause than the friction of the needle on the pin; but what that cause is he cannot say, for it seems to depend neither on heat nor cold, a dry or moist air, clear or cloudy, windy or calm weather, nor the height of the barometer. The only thing that has any appearance of regularity, is, that the variation has been generally greatest, for the same day, between the hours of 12 and 4 in the afternoon, and the least about 6 or 7 in the evening.

With these needles, and their apparatus, Mr. G. made numerous experiments in many days, from Feb. 5, to May 10, 1722, for many times each day, in all varieties of circumstances. Indeed, from Feb. 6, 1722, to the 10th of May following, he made above 1000 observations in the same place; and the greatest variation westward, was 14° 45′, and the least 13° 50′. It was seldom less than 14°, or greater than 14° 35′.

Observations on Dr. Eaton's Styptic. By Dr. Sprengell, F.R.S. N° 383, p. 108.

The method of curing fresh wounds in a few days, without suppuration, where neither nerves, large vessels, bones, nor any of the viscera were concerned, has been long ago observed. Purman, a famous surgeon of Breslaw, in his Chirurgia Curiosa, tells us of a mountebank, who gave himself 13 wounds, by incision, in the upper part of his left arm, and then applied his nostrum, and with the help of a good roller, was cured in 2 days time. Next he mentions a martial styptic, which stopped bleeding incomparably, and healed fresh wounds he says in 2 days time, especially if the patient took a few drops inwardly. This has likewise been mentioned by Blegny near 30 years before.

When Dr. S. was in France, he found that several trials were made there also, with a styptic ball mixed with French brandy, by striking a cock through the head, opening the crural-artery of a dog, or chopping off a dog's leg, &c. but he found it did not amount to any thing of consequence; yet he had still a ball, made above 20 years before, of filings of iron, and an equal quantity of tartar, mixed well with French brandy on a marble. This, with some alteration, was afterwards published by the famous Helvetius, physician to Lewis the 14th of France, in a book called, Recueil des Methodes pour la Guerison des diverses Maladies, which was reprinted in Holland in the year 1710.

The recipe for his medicinal ball, translated, is as follows: Take 4 lb. of the filings of steel, and 8 lb. of tartar, well powdered; mix these well together, and put them in a new earthen pot, and pour on them as much French brandy as will make it into a poultice. Let this stand fermenting in a cellar for 4 days, and stir it between whiles. Then put it in balneo mar. and distill it s. a. with a moderate fire, to draw off the brandy. When you find that nothing but the phlegm comes over, then take it from the fire, and take out the mass, stamp it very fine, that not the least lump may remain; then mix it again as before with a sufficient quantity of brandy, and put it in the cellar to ferment as before, and then distill it a 2d time. This operation may be reiterated 7 or 8 times; but the last time mix the mass well on a marble, and form it into 2 ounce-balls. One of these balls is steeped in a pint of good French brandy, a little warmed, and hung only in it by a wire, till the brandy has received the colour of the ball. But if you are in haste, grate a sufficient quantity of the ball in some brandy, stir it well, and you may use it that very instant.

Doubtless the author thought, by often grinding, fermenting, and distilling this mass, to comminute and subtilize its particles, so as to make it more fit to contract the fibres and vessels of a wound, and to prevent stagnations of the fluids, both within and without, on contusions; but the success did not answer, and therefore it was laid aside. Neither did Helvetius ever recommend it as an universal styptic, astringent, or consolidating medicine, but merely in fresh wounds, and that only for a first dressing, and where people lived at a distance, and could not get immediate assistance from a surgeon. Besides, he makes several exceptions where it should not be used; and, in general, advises it where chalybeate medicines may be made use of according to experience.

But last year a balsamic styptic was published by one Dr. Eaton, good to stop all manner of bleeding without or within, and that without any manner of exceptions. This made Dr. S. desirous to see it, and he had soon after an opportunity to examine it. He presently found that this was the same old medicine, which was also got into England, after other countries had discarded it.

Finding this remedy recommended in an extraordinary manner, by so eminent a physician as Sir Rd. Blackmore, Dr. S. began to think that possibly he might have been mistaken, and therefore desired an apothecary immediately to prepare the recipe, as described by Helvetius. When this was ready, Dr. S. sent for a bottle of Dr. Eaton's styptic, and tried them both with galls, before several gentlemen; the tincture was the same, a deep purple. He then precipitated the contents with old hock, and found the precipitated matter to be the same in both. Not contented with his own inquiry, he sent several small quantities to others, and went to Mr. Godfrey, a famous chemist in Covent-Garden. They all told him, that there was no difference between them.

Next to try these two on the crural-artery, Dr. S. got a good middle-sized dog, laid the artery bare, and opened it with a lancet the length way of the artery, for near half an inch. The old trick used to be, to cut the artery crosswise, and then there was no necessity of a styptic at all, nor indeed here neither. But at first Helvetius's tincture was applied, and stopped the bleeding; then we opened the artery again, and tried Dr. Eaton's, with the same success. Dr. S. then had the artery opened in the other thigh, and tried it only with French brandy, which did as well as the other two. He opened the artery again, and had dissolved in French brandy a little sal martis and saccharum saturni, and applied that, and it answered in the same manner. This made him conjecture that there was but little virtue in either of them, but only that the brandy, by its great heat, merely contracted the fibres of the artery, which might be a little assisted by the chalybs; but this could not be much. Dr. S. then reflected on the smallness of the crural artery in a dog, and that a little pledget of lint alone might stop the blood, as well as the temporal artery when opened with a lancet, which we did, and the pledget of lint stopped it. Thus far as to its outward use, and Dr. S. could wish it were as harmless within. If only, according to Helvetius, it had been ordered to be taken inwardly, in fresh wounds and contusions, it might have been let pass; but when, without exception, Dr. Eaton recommended it, as also even Sir Richard, in all inward bleedings, it was high time to make some animadversions upon it.

Thus it is shown, that the styptic quality of these tinctures is very inconsiderable; and that Helvetius's tincture is rather better than Dr. Eaton's, if if there is any difference at all; though that might be owing to the brandy, which was stronger than Dr. Eaton's.

The Specific Gravities of several Bodies. By M. Fahrenheit, R. S. S.

N° 383, p. 114. *Translated from the Latin.*

Gold	19081
Mercury	13575
Lead	11350
Silver	10481
Swedish copper	8834
Japanese copper	8799
Iron	7817
Malacca tin	7364
English tin	7313
White marcasite	9850
Regulus of antimony	6622
Brass	8412
Rock-crystal	2669
Homogeneous Pyrites	2584
Pot-ash depurated from the filth, and a neutral salt, in which it more or less abounds	3112
The above neutral salt	2642
Sea salt	2125
Nitre	2150
Alum	1738
Very white sugar	1606½
Oil of vitriol	1877½
Lixivium of pot-ash, fully saturated with salt	1563
The same	1571⅓
Good aquafortis	1409
Spirit of nitre	1293½
Rain water	1000
Rape-seed oil	913
Alcohol of wine	826
The same purer	825

M. Fahrenheit made the experiments several ways; for, he weighed fixed bodies, as is commonly done, first in a nice pair of scales in air, and then in rain water: he discovered the weight of salts first in air, and then in a proper liquor, after which he compared it with the gravity of water; he discovered the gravities of the liquors, sometimes by the arometer, described in a following Transaction, and sometimes in proper vessels.

The most simple method of finding the difference of gravity, which arises from the different temperament of fluids, is, first to fill some vessels with a liquor not very hot, but whose degree of heat should be discoverable by the thermometer, and then to weigh it; and afterwards to fill the said vessel again with hotter liquor, and weigh it as before. If the degree of heat in this second experiment be again marked, you have the difference of the gravity of the liquor, caused by an intermediate degree of heat, which may then be easily calculated for every other degree.

The experiments were made in air; its gravity therefore is to be added to each number, in order to have the gravity of the liquors in vacuo. But the specific gravity of air is to that of water, as 1 to 1000 nearly.

Some Considerations about the Cause of the universal Deluge, laid before the Royal Society, Dec. 12, 1694. *By Dr. Edmund Halley, F. R. S.* N° 383, p. 118.

The account we have of the universal deluge is no where so express as in the Holy Scriptures; and the exact circumstances as to point of time, show that some records had been kept of it more particularly than usual in those things derived from remote tradition, wherein the historical minutiæ are lost by length of time. But it must be allowed, that length of time may have added, as well as taken away many notable circumstances, as in most other cases of the story of remote times and actions.

This we may however be fully assured of, that such a deluge has been; and by the many signs of marine bodies found far from and above the sea, it is evident that those parts have been once under water: or, either that the sea has risen to them, or they have been raised from the sea; to explain either of which is a matter of no small difficulty.

To effect this, says Dr. Halley, a change of the centre of gravity, about which centre the sea is formed, seemed not an improbable conjecture, till it appeared that this centre of gravity was the necessary result of the materials of which our globe consists, and not alterable while its parts remained in the same position: and besides, this supposition could not drown the whole globe, but only that part towards which the centre of gravity was translated, leaving the other hemisphere all dry.

I shall say nothing of Dr. Burnet's Hypothesis, nor of its many insufficiencies, as jarring as much with the physical principles of nature, as with the Holy Scriptures, which he has undertaken to reconcile. Dr. Hook's Solution of this Problem, as he has not fully discovered himself, I cannot undertake to judge of; but his compression of a shell of earth into a prolate spheroid, thereby pressing out the waters of an abyss under the earth, may very well account for drowning two extreme opposite zones of the globe; but the middle zone, being by much the greater part of the earth's surface, must by this means be raised higher from the centre, and consequently arise more out of the water than before; and besides, such a supposition cannot well be accounted for from physical causes, but require a preternatural digitus dei, both to compress, and afterwards restore the figure of the globe.

But the Almighty generally making use of natural means to bring about his will, Dr. H. thought it not amiss to give the society an account of some thoughts that occurred to him on this subject; wherein, says he, if I err, I shall find myself in very good company.

In N° 190 of these Transactions, he proposed the casual shock of a comet, or other transient body, as an expedient to change instantly the poles and diurnal rotation of the globe; at that time only aiming to show how the axis of the earth being changed, would occasion the sea to recede from those parts towards which the poles did approach, and to increase upon and overflow those parts from which the poles were departed; but at that time he did not consider the great agitation such a shock must necessarily occasion in the sea, sufficient to answer for all those strange appearances of heaping vast quantities of earth and high cliffs upon beds of shells, which once were the bottom of the sea; and raising up mountains where none were before, mixing the elements into such a heap as the Poets describe the old Chaos; for such a shock impelling the solid parts, would occasion the waters, and all fluid substances that were unconfined, as the sea is, with one impetus to run violently towards that part of the globe where the blow was received; and that with force sufficient to rake with it the whole bottom of the ocean, and to carry it upon the land; heaping up into mountains those earthy parts it had borne away with it, in those places where the opposite waves balance each other, miscens ima summis, which may account for those long continued ridges of mountains. And again, the recoil of this heap of waters would return towards the opposite parts of the earth, with a less impetus than the first, and so reciprocating many times, would at last come to settle in such a manner as we now observe in the structure of the superficial parts of the globe.

In this case it will be much more difficult to show how Noah and the animals should be preserved, than that all things in which was the breath of life should hereby be destroyed. Such a shock would also occasion a differing length of the day and year, and change the axis of the globe, according to the obliquity of the incidence of the stroke, and its direction, in regard to the former axis. That some such thing has happened, may be guessed, as the earth seems as if it were new made out of the ruins of an old world, wherein appear such animal bodies as were before the deluge, but by their own nature and defences from the weather, have endured ever since, either petrified, or else entire in statu naturali. Such a shock may have occasioned that vast depression of the Caspian Sea, and other great lakes in the world; and it is not unlikely but that extreme cold felt in the north-west of America, about Hudson's Bay, may be occasioned by those parts of the world having once been much more northerly, or nearer the pole, than now they are; by which there are immense quantities of ice yet unthawed in those parts, chilling the air to such a degree, that the sun's warmth seems hardly to be felt there, and of which the Poet might justly say, Frigus iners illic habitat pallorque tremorque—Ac jejuna fames.

Some further Thoughts upon the same Subject, delivered on the 19*th of the same Month. By the same.* N° 383, p. 123.

Having been advised since the last day, by a person whose judgment Dr. H. had great reason to respect, that what he then advanced, ought rather to be understood of those changes which might have happened to the earth in times before the creation, and which might possibly have reduced a former world to a Chaos, out of whose ruins the present might be formed, than of the deluge whereby mankind was in a manner extinguished about 4000 years since; that being much more gradually brought to pass, and with some circumstances that this hypothesis cannot admit of, which abler pens, perhaps, may account for: what Dr. H. has advanced, he desires may be taken for no more than the contemplation of the effects of such a shock as might possibly, and not improbably, have befallen this lump of earth and water, in times of which we have no manner of tradition, as being before the first production of man, and therefore not knowable but by Revelation, or else a posteriori by induction from a convenient number of experiments or observations, arguing such an agitation once, or oftner, to have befallen the materials of this globe. And perhaps, in due periods of time, such a catastrophe may not be unnecessary for the well-being of the future world; to bury deep from the surface those parts, which by length of time are indurated into stony substances, and become unapt for vegetable production, by which all animals are either immediately or mediately sustained: the ponderous matter in such a mixture subsiding first, and the lighter and finer mould remaining for the latter settling, to invest the exterior surface of the new world. This may, perhaps, be thought hard, to destroy the whole race for the benefit of those that are to succeed. But if we consider death simply, and how that the life of each individual is but of a very small duration, it will be found that as to those that die, it is indifferent whether they die in a pestilence out of 100,000 per ann. or ordinarily out of 25,000 in this great city, the pestilence only appearing terrible to those that survive to contemplate the danger they have escaped. Besides, as Seneca has it,

Vitæ est avidus quisquis non vult
Mundo secum pereunte mori.

N. B. The foregoing papers having been read before the Society 30 years since, were then deposited by their author in their Archives, and not published; he being sensible that he might have adventured ultra crepidam; and apprehensive lest by some unguarded expression he might incur the censure of the Sacred Order. Nor had they now been printed, but at the desire of a late Committee of the Society, who were pleased to think them not unworthy the press.

Here the reader is desired to observe, that Mr. William Whiston's Book, intitled, A New Theory of the Earth, was not published till about a year and a half after the date hereof, and was not presented before June 24, 1696, to the Royal Society.

Some new Experiments concerning the different, and sometimes contrary Motion of the Sap in Plants. By Tho. Fairchild, Gardener at Hoxton. N° 384, p. 127.

Some years since, Mr. Fairchild showed some experiments before the Royal Society, which were allowed to be new and useful. This encouraged him to try further experiments, in order to show the course of the sap; which he finds by experience, will be so useful, that he can make barren trees fruitful, and decaying trees healthful, and render the system of gardening and planting more useful to the public.

Mr. F. showed the laureola, grafted on the mezereon, and the evergreen oak of Virginia, on the common English oak; both which hold their leaves all the winter. and are in good state and flourishing, though grafted on plants that drop their leaves in winter; which plainly proves that the juices rise upwards, in winter, in those plants that drop their leaves, otherwise the evergreens that are grafted on them would soon perish.

Mr. F. believes that by grafting the variety of foreign oaks on the English, we might make the timber more firm and lasting, than it is in its own nature, when raised from foreign acorns: for as the crab stock makes the wood of the apple-tree more firm and lasting than that on the apple-stock, and the peaches and almonds, budded on plums, are more lasting than those on peach-stocks; so by the contrary rule, all firm timber, grafted on spongy stocks, would be made worse than it would be on its own bottom. For example, if that which is called the English elm, should be grafted on that which is called the Dutch, it would partake of abundance of the spongy juices of the stock, by which the timber would become unfit for the purposes it is now used for.

The first experiment Mr. F. offered, was on the New England cedar, or rather juniper, grafted on the Virginia; and it is remarkable, that the branch, which is grafted, is left several inches below the grafting, which part continues growing as well as the upper part above the grafting.

The second was on the viburnum, with the top planted in the ground, which is become roots; and the roots turned up, which are become branches. But whether the same vessels, which fed the branches, have changed their course, or whether the juices go up and down in the same vessels, he leaves undetermined; but the plant was in as good state of growing, as in its natural state.

The third experiment was on a pear-tree, which he enarched on two pear-stocks, in March 1721-2, which was in a good flourishing state, with a branch in blossom, and receives no nourishment but by the two enarched branches, the root being out of the ground; and though it was done above 2 years since, it is now shooting suckers out of the root; which proves that the branches are as useful to support the roots, as the roots the branches; and it is therefore no wonder that so many trees miscarry in planting, when there are no branches left on the head.

The fourth experiment was on the cedar of Libanus, grafted on the laryx, which drops its leaves in the winter; yet maintains the cedar in as flourishing condition, as if it had been on a tree that held the leaves all the winter; and the part of the graft, left below the grafting, is in as good health as the part above it.

Of an extraordinary Cure by Sweating in Hot Turf; with a Description of the Indian Hot-Houses. By the Hon. Paul Dudley, F.R.S. N° 384, p. 129.

In the year 1704, Peter Coffin, Esq. of Exeter, in New England, being then 74 years of age, had taken a great surfeit, as it was thought, by drinking cold water in a very hot day, after having heated himself in the woods. It settled principally in his right side, but gave him a racking pain all over his body, and particularly deprived him of the use of his right arm: in this condition he kept his house and bed for 9 weeks, and his recovery, considering his age, was despaired of; when a son of his making a visit to his father, proposed the sweating of him in turf: the father readily agreed to it, having used many medicines without any effect. Immediately orders were given to cut a large oven full of turf, the pieces might be about 18 inches square. The turf itself was of English grass, and only the sward, or top of the earth, with the grass. Before the turf was put into the oven, the doctor rubbed the grassy side of the turf, with some spirit, or oil, and then doubled the grass-sides together, and so set them in. When they were well baked, which was in about 2 hours, he took them out, and made a bed of them on the floor, the place for the head raised; as soon as that was done, he ordered his father to be taken out of his bed without his shirt, but wrapped up in a sheet, and laid upon the hot turf, and then covered him over with the rest of the turf, more especially on his side, where the seat of his pain was; but they laid none on his breast or head; they then covered him with blankets, to keep the heat in. While the father was in this bath, the son gave him warm cordials, to prevent fainting, which he was in great danger of. After he had lain thus about $\frac{3}{4}$ of an hour, which was as long as he could bear it, he was put naked into the bed very well warmed, where, in a few minutes, he fell asleep, and sweat to that degree, that it ran through his pillow and bed, upon the floor. After about 2 hours sleep, they dried him, and put him on warm cloaths, and the old gentleman found himself much eased and refreshed. This was in the morning; and before night, he walked about the house comfortably, his pain being in a manner all gone. The next day the doctor repeated his cordials, and the 4th day he sweated his father a 2d time, in the same manner as above; and the next, viz. the 5th day, he went abroad about his business, and lived 11 years afterwards in perfect health, and free from pain. The doctor tells me, great care must be taken that the patient do not lie too long in the turf, and that even a quarter of an hour may be sufficient for some persons; and whenever the patient begins to fetch his breath short, or faint, he must be put to bed immediately, and the physician, or operator, must by no means omit his cordials.

Houses to sweat in were common among the aborigines, when the English first came into New England, though now but little used. A gentleman of the island of Nantucket, where the Indians sometimes practise it, even at this day, or did very lately, gives the following relation.

The cave was usually 4 feet high, and about 8 feet diameter; the roof supported with sticks or boards, covered with earth; it was dug in the side of a hill, and as near as could be to some river, pond, or place of water: the entrance into this cave was small, and the door, when any person was sweating, was covered with a blanket or skin; near the cave they make a good large fire, and heat a parcel of stones, to the quantity of 5cwt. and roll them in red-hot, piling them up in the middle of the cave; when this is done, the Indians go in naked, and sit round the heated stones as many as please; as soon as they begin to grow faint, which may be in a quarter of an hour, they come out, and plunge themselves all over in the water for a minute or two, and then in again, as long as they can well bear it, and so in the water a 2d time, and then dress themselves. This has been used with success for colds, surfeits, sciaticas, and pains fixed in the limbs; and even the English have often found relief by it. The Indians often used it before, and after long journies, hunting or voyages, to strengthen and refresh themselves.

An Attempt to account for the rising and falling of the Water of some Ponds near the Sea, or ebbing and flowing Rivers; where the Water is lowest in the Pond, at the Time of high Water in the Sea or River; and the Water is highest in the Pond, at the Time of low Water in the Sea or River. As also for the Increase or Decrease of the Water of such Pools and Brooks as are highest in the dry Seasons, and lowest in the rainy Seasons: With an Experiment to illustrate the Solution of the Phænomena. By the Rev. J. T. Desaguliers, LL. D. and R. S. S. N° 384, p. 132.

Hero, and other hydraulic writers, have described a cup, called a tantalus, from its effect, which will hold any liquor very well, when it is not filled above a certain height marked in the cup; but if it be filled higher, not only the liquor above the mark will run out, but the whole liquor that is in the cup. This is performed by a syphon in the cup, which is sometimes concealed to make the effect the more surprising.

The cup, AB, fig. 1, pl. 1, has a visible syphon CED in it; the cup fig. 2 has the same, concealed by the figure of a man, to represent Tantalus in the Fable; and the cup of fig. 3 has its syphon more concealed, as it is carried up into the handle. Any of these cups will hold water very well, provided they are not filled up above the line FG; for then, not only the liquor that is above FG will run out, but all the liquor in the cup as low as D, the orifice of the short leg of the syphon.

Exper. 1, fig. 4. In the vessel abcd is placed an open wooden box ABCD, filled with water as high as the line LM. Another box or plug EFGH made tight, and containing weights to sink it, is made to let down into the water between the partition IK and the end AB of the former box; but when it is not to press the water up to IO, as it does when let down, it is drawn out of the water by the weight m, which pulls it up by the bar ik, fastened to a leaver moving round the centre l.

When by means of the plug, the water in the space ABKI is pushed up to IO, by passing under K; it runs out through the spout PQ, whose passage is gauged by a little sluice Pp, and falls into the vessel RS, made of an oblong figure like a fish-pond, and having a syphon at s, so as to make it a tantalus, or in the nature of the cups abovementioned.

Let the weight m draw up the plug EFGH; then the water, having filled RS, will run down below the orifice P to M. The tantalus RS, beginning to run out as soon as full, will, for the reasons above given, continue to run till it is all emptied; and as it discharges itself into another tantalus TV, whose syphon is at V, this last tantalus will also, when full, begin to run out, and its water go down to XYO.

If the plug be let down gradually, as soon as the water begins to run out of the last tantalus TV, and the first tantalus RS be covered so as to be concealed from sight, it will appear to the spectators, that the cavity TV, representing a pond near an ebbing and flowing river (as it is said there is such an one at Greenhithe in Kent, between London and Gravesend) always rises, while the water at NO, or the tide, falls to LM; and always sinks while the water at LM, or the tide, rises to ON.

Exper. 2. Let the water in the box ABCD not be made use of; only the vessel Z be filled every half hour: it will empty itself in the space of a quarter of an hour, falling like rain, and dropping also through the leaden platform ef into the hidden tantalus RS, which will not begin to run till this artificial rain is over: then in a quarter of an hour more, the tantalus RS will have emptied itself into the visible tantalus TV, which will be filling all the time after Z has done running, or in the dry season, and as soon as TV is full, it will begin to run out through its syphon V, at the end of the half hour, when the vessel Z or sieve runs again; that is, at the return of the rainy season.

This last experiment may easily be applied to those ponds, or those brooks, that are high in dry weather, and low in wet weather; of which kind it seems there is a brook at Lambourn in Berkshire.

If it be objected, that such ponds are full for some time, which a tantalus cannot be, because it begins to run out as soon as full; that may be easily solved, by supposing the hidden tantalus, or intermediate cavity between the river and pond, to contain more water than the visible one, provided it does not contain so much as not to be emptied, before the return of the tide. The same solution will serve for wet and dry seasons, only supposing the cavities larger.

If it be asked, where the water of the visible tantalus, near a river, can run; it may be answered, that all this may happen, though the second, or lowest tantalus, should have its bottom higher than low water-mark in the river. And for the syphons, which are of a particular make in the cup; though such be not supposed in the earth, yet any long passage, rising in the middle, will answer the end. See fig. 5, where ABCD represents the channel of a river, AD high water-mark, and GH low water-mark; ZI a passage from the river to the cavity IKLMN, the first or hidden tantalus; LMQ the syphon of the first tantalus, running into the second tantalus, or visible pond OQRP, which by its syphon RSV runs out into low grounds that may be above the low water-mark

GH; and the bottom KL of the first tantalus may be above the top of the last, whose level is the line WW.

ABCDYOQRPVH is the section of the surface of the earth.

An Account of a Human Body found 2 Years before in a Copper Mine at Fahlun; by Adam Leyel, Coll. Reg. Metall. Assessor. From the Acta Literaria of Sweden, for 1722. N° 384, p. 136. *An Abstract from the Latin.*

In this account it is stated that in the month of Dec. 1719, there was found in one of the copper mines at Fahlun, in an uncorrupted state, and converted into a horny consistence, the body of a man, who had been killed by the falling in of a part of the mine, in the autumn of 1670, i. e. upwards of 49 years before. Both his legs, with his right arm and head, were fractured; but his face and the rest of his body were unhurt. His flesh and skin felt rough and hard; they were not, however, in a petrified state, but only of the hardness of horn or hoof, for they could be cut with a knife.

When the body was exposed to view it was recognised (for the features still remained perfect) not only by several of the miners, but also by an old woman to whom the unfortunate man had been married, to be the body of Matthew Israel, called, on account of his height, Big or Tall Matthew, who it was well remembered had gone down into the mine at the date beforementioned, and had been missing ever since.

The preservation of this dead body from putrefaction for so many years, and the conversion of the skin and flesh into a substance as hard as horn, is attributed by the author of this account to the vitriol dissolved in the water of the mine, in which vitriolic water the body was found.

The Description and Use of a New Areometer. By M. G. Fahrenheit, F. R. S. N° 384, p. 140. *Translated from the Latin.*

The two tubes CD and EF, fig. 6, pl. 1, are joined to the ball A, which is pretty large, the larger the better, with a receptacle G to the smaller tube EF; and the middle of the tube is distinguished by a very small point a, yet sufficiently discernible. The other extremity of the tube CD is furnished with a ball B, which serves instead of a receptacle to the inferior weight, with which the instrument is charged. Let the distance of the ball B from the centre of the ball A be 3 times the distance of the receptacle G from the same centre. The instrument being thus prepared, let the ball B be filled with so much mercury, that if the areometer be immerged into the lightest liquor, as for instance, spirits of wine, well dephlegmated, or spirits of turpentine, it may descend therein almost to the point a; after which, the tube is hermetically sealed near E, and the instrument weighed in a nice pair of scales; and the weight of the instrument will be the very same with that of the liquor, excluded by the instrument, as is well known from hydrostatics. But if the weight of heavier liquors, as water, lixivia, or acid spirits, be sought, their difference of gravity is found, when the instrument is charged with so much weight in the receptacle G, as may make it descend again to the point a. On adding this weight to that of the instrument, the specific gravities of these liquors, if the weights be very small, will be had sufficiently exact: and so of the rest.

M. Fahrenheit affirmed that the instrument should descend in the abovementioned spirits almost to the point a; but it will be better that the liquor do not exactly reach that point, and that the inconsiderable difference be made up by very small weights; for, in this manner, were there still lighter liquors proposed, or were the gravity of the abovementioned liquors rendered specifically lighter by heat, they might still be discovered by the instrument, which otherwise would not happen, should it descend in the said liquors exactly to the point a.

In making the experiments, care is to be taken, that neither the superficies of the instrument, nor of the liquors, have any fat, or other heterogeneous particles adhering to them; else the experiments can never be accurately made.

On the preternatural Structure of the Pudenda in a Woman, described in Philos. Trans. N° 379. *By Mr. John Bonnet, Surgeon at Fowey, in Cornwall.* N° 384, p. 142.

As there appears to be some difference in the accounts of the remarkable structure of the pudenda, &c. of the woman of Lanteglass, near Fowey, published in the Philos. Trans. N° 379, Mr. B. gives this particular account of what he observed with respect to this matter.

The woman was about 23 years old when she was married, and some time after she conceived. As she was conscious of the preternatural structure of the parts, and her mother apprehensive of the danger that would attend the delivery under such unhappy circumstances; they applied to Mr. B. about the 7th month, in order to engage his assistance. On viewing the abdomen, he found no sign of the umbilicus; but about 3 inches lower than the regular place of it, there was a spongy, fleshy extuberance, nearly of the shape and size of a hen's egg; not, as is said in Mr. O.'s account, composed of many lobules enveloped by distinct membranes, but much resembling that luxuriant flesh which is thrown forth in ill-digested wounds, commonly called proud flesh. This was exceedingly tender, and on it she could not bear the least touch. On the lower part of this excrescence were two small orifices, the one about an inch from the other. Through these the urine dropped continually, nor was she able to retain it; but by violent efforts could make it spout out near a foot. What is said in Mr. O.'s account of its being rendered multis rivulis, is certainly erroneous; the two orifices, by which it is indeed discharged, being now very evident, and will easily admit a small probe.

About ¼ inch below this protuberance, was a transverse orifice, much resembling the anus of a cock. Through this the menstrua regularly flowed, and by this she was impregnated. It was with some difficulty Mr. B. thrust his finger into this orifice, to try to reach the os tincæ, which however he could not feel, it lay so deep; but he plainly felt a thick transverse membrane, separating this passage from an orifice, situated about 2 inches below that already described. This lower orifice seemed to be situated exactly where the symphysis of the ossa pubis is, in women regularly formed, somewhat above the place where the natural hiatus should have been. He could but just enter the tip of his finger into this. There were a few hairs scattered up and down irregularly about this orifice. The anus terminated as usual, with a sphincter about 2 inches below this lower orifice, much more forward than usual.

So that the upper orifice, which may be properly called the orifice of the vagina, was about ¼ inch below the umbilical excrescence; the lower oblong orifice, or another passage to the womb, was about 2 inches below that of the vagina. The woman had no os, or ossa pubis, indeed there was an apophysis jutting out from the lower part of each os ilium, but they were far from being joined, as usual, by synchondrosis.

July 18, 1722, Mr. B. was sent for late at night. He found the woman with true travail pains upon her. The throws were excessively violent, and the continued agony had almost quite exhausted her spirits; but the orifice of the vagina was nowise sensibly dilated, though the anus, through the violence of the throws, opened extremely wide. In vain were all endeavours to relieve her, by thrusting up the child, and putting the mother in a proper posture. Vain were her own throws and agonies. Convulsions now had seized her, and nature seemed to have denied a longer life to the mother, or an entrance into it to the child.

Mr. B. was in the utmost perplexity what to do under these circumstances. On the one hand he considered, that if there was not a passage made for the child, and that by incision, both mother and child must speedily perish. On the other hand, he foresaw the danger and hazard of an incision, and the unavoidable censure of having killed the woman, if she should die under the operation. At last humanity so far prevailed with him, as to try a doubtful method of preserving life, rather than none. He told her mother, and the other persons in the room, that death was inevitable, without making the passages wider by incision, and so attempting a delivery. When they saw her, as they thought, just expiring, they delivered her into his hands, to do with her what he thought fit.

He immediately thrust his scalpel into the inferior oblong orifice, and directly cut into the orifice of the vagina, so brought them into one; then presently with his scissars snipped the transverse membrane. This being done, he easily introduced his hand, felt the head of the child, and with his finger thrust into its mouth, drew forth a female infant, living and well formed.

Ever after her delivery she has suffered a prolapsus uteri, on the least standing or walking. Mr. B. proposed to remedy this by a suture, as is practised in the case of the vulva breaking into the anus; but she would by no means admit of it. So that she almost continually laboured under a procidentia uteri, and the body of the womb and vagina were so corroded by the acrimony of the urine, that four or five ulcers formed upon them. Besides this inconvenience, some of the thinner parts of the excrement were discharged at the bottom of this large cleft; and by introducing his finger at the bottom of it, he could easily thrust the top of it through the anus.

A partial sight of Objects. By Dr. Abraham Vater. N° 384, p. 147. *Translated from the Latin.*

A middle aged woman, was in one night seized with a black cataract, or gutta serena, by the retropulsion of a coryza, by bathing and a subsequent cold. For, getting up in the morning, she found herself deprived of her sight, without any external blemish in her eyes. After taking several laxative medicines and purifiers of the blood, and at the same time applying vesicatories, and using a regular diet, her sight gradually returned; yet with the following particular phænomena: at first, all the people she met appeared without heads, their bodies only without the head presenting to view. In process of time she saw objects entire, but through a mist or net, as it were; and after this, she observed spots or motes before her eyes; at length this disorder degenerated into a partial sight; and lastly, she saw an object entire, with both her eyes open and directed towards it; but shutting either of them, part of the object seemed to be covered with a round spot or mist, which appeared so much the larger, as the eye was farther removed from the object; but as either the right or left eye was shut, the middle, though different, part of the objects was obscured. Thus, for instance, if she viewed with her left eye only these three words, ego sum cœca, the middle word sum, when the pupil was directed towards it, was not seen, but only the words ego—cœca; and when the pupil was directed towards ego, it was not seen, only the words sum, cœca; but if on the contrary she shut her left eye, and viewed an object with her right, then the middle of the object was not seen; yet in such a manner, that only a fourth part of the object escaped the sight, while the other three parts were distinctly seen. Thus, for instance, should she view with this eye these four words ego opto esse sana, then the pupil being directed towards the middle, she saw all the words but opto, which was obscured by a round spot, and with difficulty she saw ego esse sana.

Since, therefore, it appears from this case, that the round spot, by which part of the object was obscured, was really fixed, though changing its place according to the different direction of the pupil, it seems doubtful whether the defect was in the crystalline humour, or in the retina; in the cornea there appeared no spot or obscurity.

The abovementioned phenomenon, viz. where she could see the whole body of a man, excepting only his head, seems to be of greater consequence, and more difficult to be explained.

Dissection of two Eyes which had been affected with Cataract; communicated in a Letter from ..... to Samuel Molyneux, Esq. Secretary to the Prince of Wales, and F. R. S. N° 384, p. 149. *An Abstract from the Latin.*

An old soldier, named John Wright, having both eyes affected with cataract, underwent the operation of couching; by which the sight of the left eye was restored; but the operation on the right eye was unsuccessful. He was afterwards taken into the Royal Hospital of Invalids, near Dublin;* and died in 1722, 8 or 9 years after the operation had been performed, being choaked by a bit of cheese sticking in his throat, an accident which had nearly proved fatal twice before, owing to a constriction of the œsophagus.

Both eyes, after being detached from their sockets, were sent for examination to Tho. Molyneux, M.D. Fellow of the Royal College of Physicians in Dublin, &c. On removing the cornea with a portion of the sclerotica of the left eye, the sight of which had been restored, no traces of a pellicle floating

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* The Kilmainham Hospital.

in the aqueous humour, or adhering to the edge of the iris, could be seen, as said to have been observed by Wodhouse. But what was still more surprising, no remains whatever of the crystalline humour were found. The vitreous humour exhibited nothing preternatural; the tunica choroidea and retina were of a brown colour both internally and externally. In like manner no traces of a pellicle, nor any remains of the crystalline humour, were found in the right eye. The cornea of this eye was wrinkled and flaccid; for the aqueous humour, when once let out, is never renewed. Hence the use of this eye was irrecoverably lost. No doubt this man had a crystalline humour in each eye before he underwent the operation of couching. It is therefore inferred, that in consequence of its displacement in the act of couching, and at the same time of its disengagement from its connection with the ciliary ligaments and vessels, by means of which nourishment is conveyed to it, this humour, (the crystalline humour,) wasted away and disappeared.* It is conjectured that this always happens after the operation of couching. This case very clearly proves, contrary to the opinion of the Parisian oculist Wodhouse, that vision may take place where the crystalline humour is wanting.

An Account of a Book entitled, Dominici Bottoni, de immani Trinacriæ Terræ Motu Idea Historico-Physica, in qua non solum Telluris Concussiones transactæ recensentur, sed novissimæ Anni 1717. *Messanæ* 1718, *8vo. By J. G. Scheuchzer, M. D. R. S. S. Coll. M. L. Lic.* N° 384, p. 151.

This treatise contains an accurate historical account of the several violent earthquakes, which happened in the kingdom of Sicily, in the years 1693, 1694, and 1717, interspersed with some philosophical digressions concerning the causes and effects of earthquakes in general. An account of the first of these was communicated to the Royal Society by the late Malpighi, and is inserted in the Phil. Trans. N° 207.

The summer of 1692 was exceedingly hot and tempestuous, with frequent thunders, lightnings and rains. About the middle of September fell such profuse showers, that all the rivers and torrents increased to such a degree, as to overflow their banks in several places, and cover large tracts of ground with water. This joined to the continual blowing of southerly winds, during the autumn, put the inhabitants under great apprehension of future mischiefs. And indeed, the disastrous fate, which afterwards befel Sicily, the beginning of 1693, proved that this ominous fear was not groundless. For on January 9, about the 5th hour, according to the Italian way of counting, after a warm,

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* Being absorbed.

serene and calm day, the earth began suddenly to tremble, chiefly about Catania, and in some neighbouring places. This first shake was accompanied as usual by a hollow, thundering noise, and succeeded by another small trembling, observed on Saturday, early in the morning. These two succussions, though violent enough, were but a prelude to the third, which happened the 11th of the same month, at 4 in the afternoon. This last was stupendous beyond imagination; the fiery eruption of the burning Etna throwing out a prodigious quantity of flames, stones, and ashes; the terror and confusion of the distracted inhabitants running up and down the streets, uncertain where to provide for their safety, or how to escape the fury of all the raging elements, which seemed to have conspired their ruin. There was scarcely one place all over the kingdom without some particular misfortune, Catania, Syracusa, Agosta, Messina, Noto, Ragusa, Leontini, Ibla Chiarumonte, Carleontino, Caltagirone, Soctino, Francofonte, Bontello, Militello, Occhiali, Aydono, Motica Mascali, were all, if not entirely destroyed, at least miserably shattered, many churches and stately buildings, throughout the country, violently thrown down, and above 60,000 inhabitants buried under the ruins, of which about 16,000 perished at Catania only.

In many places the earth gaped prodigiously, by which people were swallowed up, and some places even in the bottom of the sea. Out of all these openings sprung forth a great quantity of water, which drowned the neighbouring places. This water was in some places hot, with a strong sulphureous smell, which lasted even after the earthquakes were over, and induced some of the inhabitants, not without success, to make use of it in curing of ulcers, and other cutaneous diseases, for which chiefly a hot well near Lazaretto became very famous. Out of some of these gapings of the earth issued a thick stench and smoke.

Just at the time of the second shock, the sea retired from the land all along the coasts, leaving its bottom dry for a considerable distance, and in a few minutes it returned again with great fury, and overflowed the shores.

And it seemed that the earth itself was in some places considerably lowered, and the tops of the mountains depressed. Of this they had a remarkable instance at Paternione. The hills, between this city and the shore, hindered it from having any view of the sea, which since the earthquake discovers itself towards the east very plainly.

In other places the earth actually sunk down, and instead of it appeared great lakes, some of which were large enough to become navigable. By the breaking forth of such a lake between Noto and Syracusa, a large piece of ground was transported for about 50 paces, where it now stands as firm as if it had always stood there.

There remains still one thing worth observing, and that is the very rise and progress of this terrible succussion. It arose in the south, and proceeded from thence towards the north. For it was first observed in the island of Malta; then in the southern parts of Sicily; and last, always with some difference as to the time, in the northern parts of the same kingdom. But the shakes were less violent the more it approached to the north. For the rest, it extended itself so far, that not only the island of Malta, but also Calabria, and some parts in the kingdom of Naples, participated of its fury.

Nor was this the end of all the miseries which befel this noble kingdom: for the earth continued trembling for several months after, during the whole year of 1693. In the remaining part of January, and from that time to the beginning of the summer, the shakes came strong and thick, with hollow terrible noises, and frequent eruptions of Etna. Towards the end of the summer, the shakes were observed to lose a great deal of their force, and Etna to throw out flames and ashes in less quantity; when on the 4th of September, this mountain trembled and cracked all of a sudden, with so loud and thundering a noise, as if some thousands of guns were fired all at once. This was succeeded by a new opening, about 1000 paces from the old mouth, out of which immediately issued a thick stench and smoke, followed by a great flame. The same mountain opened itself in two other places, with the like noise, and eruption of smoke and fire, the 25th of September, 1693, and the 1st of April, 1694. After that time the shakes became visibly weaker and weaker, and at last entirely ceased.

As to the earthquake which happened April the 22d, 1717, early in the morning, and of which the author has given a short account by way of appendix, it need not be insisted on, being much the same with the former, though far inferior as to the degree of violence.

The remaining part of the book is employed in examining the opinions of all the ancient and modern philosophers about the causes of earthquakes, and establishing the author's own; which is, that the earth is shaken by the violence of subterraneous fires, occasioned by the fermentation of the combustible minerals hid in its entrails; and that the effects of the earthquakes may in all respects be compared to the effects of mines. By the way, he observes, that the causes of thunder, lightning, and winds, may be derived from the same principle.

An Account of the Scarabæus Galeatus Pulsator, or the Death Watch. By Mr. Hugh Stackhouse. N° 385, p. 159.

A very full account of this insect by Mr. Derham having been inserted in some of the preceding Vols. of the Phil. Trans. (see Vol. iv. p. 576, and Vol. v. p. 134 of these Abridgments) it seemed unnecessary to reprint Mr. S.'s observations on the same subject.

Observations of the Eclipses of the first Satellite of Jupiter, communicated by William Burnet, Esq. Governor of New York, F.R.S. N° 385, p. 162.

These observations were made in the Fort of New York, for determining the longitude of that place.

The latitude of the fort was formerly determined to be 40° 40′.

The observations were made on several days in the years 1723 and 1724. And the times of the observed eclipses being compared with the times at London, as computed from Mr. Pound's tables, give the several differences of times in the margin, the mean of all being $4^h\ 58^m\ 30^s$, or 74° 57′ 30″ nearly, which is taken as the mean longitude of New York from London. But as this determination is from the times observed at New York, compared with those *computed* at London, instead of times *observed* at this place, the result cannot be expected to be accurate, but must involve at least the same error as the tables from whence the computations were made. In fact, by later observations it is found that the longitude of New York is 74° 4′ west from London, or 74° 10′ from Greenwich.

	4^h	58^m	42^s
	4	58	33
	4	58	22
	4	58	21
mean	4	58	30

The variation of the magnetic needle was observed, this year, to be 7° 20′ west. Philip Wells, surveyor general of this province, in the year 1686, observed it to be 8° 45′; by which, it appears to decrease about 1° 25′ in 38 years, or a little more than 2 minutes in a year.

A New Contrivance for taking Levels. By the Rev. J. T. Desaguliers, LL.D. R.S.S. N° 385, p. 165.

That the air thermometer is also a barometer, has long been observed; and, because the liquor in it will rise and fall, as well by the change of the weight of the air, as by its rarefaction by heat and cold, this instrument has been disused as a thermometer, and in its stead spirit of wine thermometers, hermetically sealed, have been used ever since.

But, because the errors of the air thermometer, or its difference from the spirit thermometer, depend only on the change of the weight of the atmosphere, from what it was when the two thermometers were set at the same degree of their respective scales; the late Dr. Hook contrived an instrument, that he called a marine barometer, made of a combination of the two abovementioned thermometers; in such a manner, that a third scale being used to observe the difference of the two thermometers, from which the change of the air's gravity, and consequently storms, rains, and fair weather, might be foretold at sea, where the quicksilver barometer becomes useless by the motion of the ship.

Dr. Halley, some years since, published two tables, to show how much the mercury in the barometer would subside, when the instrument is carried up to determinate heights, above the level of the place where the first observation was made; but as he makes $\frac{1}{10}$ of an inch of fall of mercury to correspond with only 90 feet in altitude, which is rather of the least, it is evident that only very high hills and mountains can have their heights determined by this method. The same learned professor has lately, in the Philos. Trans. proposed Mr. Patrick's pendent barometer for taking the level of distant places, because the mercury in that barometer will sometimes rise and fall a foot, or a foot and a half; if therefore the motion of the mercury in this barometer, be 5 times more sensible than in the common one, a 10th of an inch of fall of the mercury will answer to a height of 18 feet; and therefore such an instrument might be of use in taking the levels of distant places. But it is known by many experiments, that this will not answer in practice; because, as the tube of such a barometer is of a very small bore, the attraction of cohesion, between the mercury and tube, will disturb the motion of the mercury caused by the different pressure of the atmosphere; so that setting up this barometer several times successively in the same place, it will often differ a 10th of an inch, or more; and if it be shaken, as is commonly done to set it right, the mercury will sometimes part, and a drop of it fall from the rest: so that it is less to be depended on for this use than the common barometer.

Mr. Stephen Gray has often made a very sensible barometer in the following manner. Into a bottle CB, fig. 7, pl. 1, he fixes a tube AB, of a very small bore, open at both ends, and cemented tight to the neck of the bottle at C; then having warmed the bottle with the hand to drive some of the air out of it, he immerges the end A into water, tinged with cochineal; so that as the air cools in the bottle CB, some of the red water is forced into the bottle; then setting the bottle upright again, as in the figure, the liquor in the bottle will stand at B, above the end of the tube, and that in the tube at D; but if it should stand higher or lower than D, it may be brought to that place by sucking or blowing at A. The instrument, thus prepared, being first set on the ground, and a springing ring of fine wire slipped on the tube down to D, by way of index, and then set upon any table, or other place, scarcely a yard higher, one may observe that the liquor is risen sensibly. Dr. D. has seen it rise a quarter of an inch, when the bottle was set but a yard higher than where it stood before; so that the column of atmosphere, that pressed down the tube, while the machine was on the ground, being shortened only 3 feet, was so overbalanced by the expansion of the air in the bottle at B, that the liquor rose a 10th of an inch above D. There is, indeed, a great uncertainty in this instrument; for since it is a thermometer, as well as a barometer, the warmth of the hand that touches it, or even comes near it, will make it rise, if the air in the bottle was cold before. Mr. Gray therefore contrived to put the bottle CB, into the vessel FE, which he filled with sand; that in raising the instrument, and moving it up and down, the air in CB might continue in the same state, and the machine be only a barometer during the experiment.

This seems to bid fair for an instrument, by which the different levels of places may be taken; but on a nice examination, it will be liable to error. For, in the first place, though sand is not suddenly altered in its heat or cold; yet in 2 or 3 hours, as it is carried into a warmer or a colder place, it will become hotter or colder, and the least degree of heat or cold, communicated to the air CB, will alter the height of the liquor at D, when the instrument is made so sensible as beforementioned. Then if, in carrying the instrument, it should be accidentally inclined, as in fig. 8, so that the liquor in the bottle should not cover the bottom of the tube at B, some liquor may fall out of the tube at B, or some air may get into it: each of which accidents will quite spoil the experiment. But if this machine be made portable, without any inconveniency, and be secured against the action of heat and cold; or, which is the same, if the alterations by heat and cold be exactly allowed for, it will be of very great use and certainty, in taking the levels of distant places; provided they be not so far distant from each other, that it requires above 6 hours time to carry the instrument from one place to another; nay very distant places, even at 2 or 3 days journey from each other, may be taken tolerably well with two instruments, nicely adjusted to each other, if they be noticed by two observers at the same hour, in fair and calm weather.

Now such an instrument Dr. D. thinks he has contrived, by which the difference of level of two places, which could not be taken in less than 4 or 5 days with the best telescope levels, may be taken in as few hours.

To the ball C, fig. 9, is joined a recurve tube BA of a very fine bore, with a small bubble at top at A, whose upper part is open. It is evident from the make of this instrument, that if it be inclined in carrying, no prejudice will be done to the liquor, which will always be right, both in the ball and the tube, when the instrument is set upright. If by heat, the air at C be so expanded, as to drive the liquor to the top of the tube, the cavity A will receive it, which will come down again and settle at D, or near it, according to the level of the place where the instrument is, as soon as the air at C returns to the same temperature. To preserve the same degree of heat, when the different observations are made, the machine is fixed in a tin vessel FE, filled with water up to gh, above the ball; and a very sensible thermometer has also its ball under water, that the liquor at D may be observed in each experiment, when the thermometer stands at the same height as before. The water is poured out when the instrument is carried, which may conveniently be done by means of the wooden frame of fig. 10, which is set upright by means of 3 screws, such as S, and a line and plummet PP. The back part of the wooden frame is represented by fig. 11, where, from the piece at top K, hangs the plummet P, over a brass point at N: MM are brackets to make the upright board KN continue at right angles with the horizontal one at N. The 12th figure likewise represents the wooden frame and screws. Figure 13 represents the machine seen in front, supposing the forepart of the tin vessel transparent. And here the brass socket of the recurve tube, into which the ball is screwed, has two wings at II, fixed to the bottom, that the ball may not break the tube by its endeavour to emerge, when the water is poured in as high as gh.

As the tube is of a very small bore, if the liquor should rise into the ball A, in carrying the instrument from one place to another, some of it would adhere to the sides of the ball A, and on its descent in making the experiment, so much might be left behind, that the liquor would not be high enough at D, to show the difference of level; therefore, to prevent that inconveniency, a blank screw is contrived to shut up the hole at A, as soon as one experiment is made, that in carrying, the air in A may balance that in C, so that the liquor shall not run up and down the tube, whatever heat and cold may act upon the instrument, in going from one place to another.

Now, because one experiment being made in the morning, the water may be so cold, that when a 2d experiment is made at noon, the water cannot be brought to the same degree of cold that it had in the morning; therefore in making the first experiment, warm water must be mixed with the cold; and when the water has stood some time, before it comes to be as cold as it is likely to be at the warmest part of that day, observe and set down the degree of the thermometer at which the spirit stands; and likewise the degree of the water in the barometer at D; then screw on the cap at A, pour out the water, and carry the instrument to the place whose level you would know; there pour in the water, and when the thermometer is come to the same degree as before, open the screw at top, and observe the liquor in the barometer.

The Doctor's scale, for the barometer, is 10 inches long, and divided into 10ths; so that such an instrument will serve for any heights not exceeding 10 feet, each 10th of an inch answering to a foot of height.

N. B. The Doctor has not made any allowance for the decrease of density in the air, because he did not propose this machine for measuring mountains, though with proper allowance for the decreasing density of the air, it will do very well, but for heights to be known in gardens, plantations, and the conduct of water, where an experiment, that answers to 2 or 3 feet in a distance of 20 miles, will render this a very useful instrument.

Intestinum Parturiens, or a very uncommon Case wherein the Bones of a Fœtus came away per Anum; communicated by John Lindelstolpe, M. D. Reg. Coll. Med. Stockholm Assessor. From the Acta Literaria of Sweden for the Year 1723. *An Abstract from the Latin.* N° 385, p. 171.

Under the quaint title of intestinum parturiens an account is here given of a woman, aged 41, who, after having been married 4 years, became pregnant in July 1720, and continued enlarging for 7 months (during which time the menses occasionally appeared in small quantity) expecting in due time to be delivered of an infant; but after the 7th month the enlargement disappeared, a weight only remaining in the right side. She became pregnant again, and in Dec. 1721 she was delivered of a dead child. She was confined to her bed until the month of June following. But in the month of May, as she went to stool, she felt so great a pain in the anus, that she thought the intestinum rectum had entirely fallen out. On applying her fingers to relieve herself, she brought away part of a cranium, of the size of a Swedish crown piece (called a dubbel carolin) and afterwards found in the close-stool 2 ribs. In the course of a fortnight there came away, by the same exit, the remainder of the bones. She afterwards recovered her health, and has since had 3 children, all of whom are living.—Several instances are cited from different authors of fœtal bones coming away by abscesses, from the navel (Albucasis Chirurg. lib. 2, cap. 7, and Marcellus Donatus Hist. Med. Mirab. lib. 4, cap. 22); from the hypochondrium (as mentioned by Wepfer) and, as in the present case, from the intestinum rectum (Marsilius Cognatus Obs. lib. 4, cap. 9, and Joh. Langius Epist. Med. lib. 2, epist. 39). Reference is also made to the case related by Littre, in the Memoirs of the Royal Academy of Sciences for 1702.*

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* In the case above related by Dr. Lindelstolpe, it is probable (as indeed the author himself

An Account of the Aurora Borealis observed at Upsal. By M. Burrman. N° 385, p. 175. *Abridged from the Latin.*

M. Burrman relates the observations of such appearances that occurred March 17, 1716, and Sept. 20, 1717. Those appearances were much of the same nature as several that have been already described in these volumes.

M. B. adds, that hence we may take occasion to consider further the optical reasons of the phænomena, with Descartes, cap. 7, parag. 18, de Meteoris. Yet M. B. does not think that an accension of a more subtile sulphureous matter, in the lower region of the atmosphere, is entirely to be rejected in this matter. For he himself observed a much greater variety of colours, with a hissing noise, like the flame of a fire, at several other times, but especially in the chasma of March 17, 1716, which was more remarkable in Sweden, for a whole night together, than in England, France, Germany, or any where else.

Description of a New Barometer. By M. Fahrenheit. N° 385, p. 179. *Translated from the Latin.*

To the cylinder AB, represented fig. 14, pl. 1, is joined the tube BC, to which an oblong ball CD is added, and to this the tube DE of a very fine bore. The cylinder, which can bear the heat of boiling water, is filled with a certain liquor; the sensible degrees of heat in the air are measured in the tube BC by means of the scale bc. If this thermometer be put into boiling water, the liquor will not only fill the ball CD, but likewise rise to the different limits of the tube DE, according to the degree of heat, which the water, at the time of making the experiment, will acquire from the gravity of the atmosphere. So that, for instance, if, at the time of making the experiment, the height of the mercury in the barometer be 28 London inches, the liquor in this thermometer will reach the lowest place in the tube DE: but if the gravity of the atmosphere be equal to the height of 31 inches of mercury, the liquor will be raised by the heat of the boiling water to the highest part of the tube DE; by means of the annexed scale de, the different limits of the heat of the boiling water will be denoted, not by degrees, but by the number of inches, by which the height of mercury in barometers is commonly measured.

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suggests) that the fœtus had been lodged in the Fallopian tube; which, as it increased in size, it distended until it burst the tube; when it (the fœtus) descended into the lower part of the abdomen, and from thence passed, by the formation of an abscess, into the rectum.

An Eclipse of the Moon observed Nov. 1, 1724, at Lisbon, by J. B. Carbone and D. Capasso. N° 385, p. 180. *Abridged from the Latin.*

At 1^h 38^m 0^s true time, the penumbra was sensible.
1 47 45 the shadow began.
4 20 36 end of the true shadow.
4 28 0 end of the sensible penumbra.

Eclipses of Jupiter's First Satellite observed at Lisbon. By the same. N° 385, p. 185.

Emersiones.				*Emersiones.*			
1723.				Die 2 Sept.	9^h	36^m	57^s
Die 23 Julii	7^h	47^m	00^s	Die 9	11	34	26
Die 7 Sept.	8	21	48	Die 25	9	59	21
1724.				Die 4 Oct.	6	26	44
Immersiones.				Die 18	10	21	20
Die 8 Jun. mane	2	3	28	Die 3 Nov.	8	42	30
Die 15	3	56	27				
Die 30	2	8	51				

This Difference of the Meridians between Lisbon, Paris and London, from the observed Phases of a Lunar Eclipse, &c. By F. J. B. Carbone. N° 385, p. 186.

From the observed phases of a lunar eclipse, Nov. 1, 1724, the differences of the times of the same phases, observed at Lisbon and at Paris, were in several instances taken, and the medium among them was nearly 45^m 50^s.

Again, by the observed eclipses of Jupiter's satellites, at several times, at Lisbon and Paris, the medium among the several times came out 45^m 48^s for the difference of the meridians of those places, in time, and nearly agreeing with the former. And hence F. C. infers that the difference in time between Lisbon and London is 36^m 7^s.

An Excretory Duct from the Glandula Renalis. N° 385, p. 190.

The anatomist, Valsalva, already known by his treatise De Aure humana, has lately made a considerable discovery. He has found the excretory ducts of the glandulæ renales, or, renes succenturiati, which discharge themselves into the parts of generation; that is to say, into the epididymides in men, and into the ovaria in women. He has read a learned dissertation relating to this discovery, before the Academy of Sciences in Bologna, in which he undertakes to prove, that those renes succenturiati, are to be reckoned among the principal organs of generation.*

Of the Currents at the Straits Mouth. By Capt. - - - - - . Communicated by Dr. Hudson. N° 385, p. 191.

Cape Spartel, and Cape Trafalgar, from the western ocean, are known to make the Straits mouth; from whence a current, in the middle of the channel, which is about five leagues broad, between the Barbary and Spanish land, runs at least 2 miles each hour, as far as Ceuta Point: and there the two coasts opening about 18 leagues distant from each other, the current does not run above 1 mile an hour, and so continues as far as Cape de Gat, which is 70 leagues up the Mediterranean. Our mariners observe a current to set to the western sea, or the great ocean, from Ceuta, along the Barbary shore; and from Gibraltar, along the Spanish shore; but that on the Barbary shore is generally their common route, not only as being the freest from rocks and less dangerous, but because the tide is much stronger than on the other side, which the sooner helps the ships out of the Straits, which are the narrowest between the points of Gibraltar and Ceuta; at which last place a neck of land extends a considerable way into the sea; and it is probable, that the current thus runs 2 miles an hour against this neck of land, the water there meets so violent an opposition in its course, as occasions it to rebound so forcibly, that part of it returns back along the same coast, and so out of the Straits mouth; which, with the small tide that sets out on the Spanish shore, it is believed may exhaust a considerable part of the current which continually sets in to the eastward, at the rate abovementioned. It is very remarkable, that in the year 1712, Mons. du L'Aigle, that fortunate and generous commander of the Privateer called the Phœnix of Marseilles, giving chase, near Ceuta Point, to a Dutch ship bound for Holland, he came up with her in the middle of the gut, between Tariffa and Tangier, and there gave her one broad-side, which directly sunk her, all her men being saved by Mons. du L'Aigle; and a few days after, the sunk ship, with her cargo of brandy and oil, arose on the shore near Tangier, which is at least 4 leagues to the westward of the place were she sunk, and directly against the strength of the current; which has persuaded

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* It will be seen in a subsequent part of this Vol. that Mr. Ranby has rendered it probable that what Signior Valsalva conceived to be excretory ducts of the glandulæ renales, were arteries.

many men that there is a recurrency in the deep water, in the middle of the gut, that sets outwards to the grand ocean, which this accident very much demonstrates; and possibly a great part of the water, which runs into the Straits, returns that way, and along the two coasts beforementioned; otherwise this ship must of course have been driven towards Ceuta, and so upwards. The water in the gut must be very deep, several of the commanders of our ships of war having attempted to sound it with the longest lines they could contrive, but could never find any bottom.

Ambergris found in Whales. Communicated by Dr. Boylston of Boston in New-England. N° 385, p. 193.

The learned have been at a loss about the origin of ambergris, till the whale fishermen of Nantucket, in New-England, 3 or 4 years since, made the discovery. Their account is this.

Cutting up a spermaceti bull whale, they found accidentally in him, about 20lb. of that drug. After which the fishermen became very curious in searching all the whales they killed; and it has been since found in lesser quantities, in several male whales of that kind, and in no other, and that scarcely in one of a hundred of them. They further add, that it is contained in a cyst, or bag, without any inlet or outlet to it, and that they have sometimes found the bag empty, and yet entire. This bag is no where to be found, but near the genital parts of the fish. The ambergris is, when first taken out, moist, and of an exceedingly strong and offensive smell.*

Observations on some of the Plants in New-England, with remarkable Instances of the Nature and Power of Vegetation. By the Hon. Paul Dudley, F. R. S. N° 385, p. 194.

The plants of England, as well those of the fields and orchards, as of the garden, that have been brought over into New England, suit very well with the soil, and grow to perfection.

The apples are as good as those of England, and look fairer, as well as the pears; but they have not got of all the sorts.

The peaches rather excel those of England, and there is no trouble or expence of walls for them; for the peach trees are all standards, and Mr. Dudley has had, in his own garden, 7 or 800 fine peaches of the rare-ripes, growing at a time on one tree.

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* This account of the origin of ambergris has been confirmed by the subsequent observations of Dr. Schwedianer and Mr. Champion, inserted in Phil. Trans. vols. 73 and 81.

The people, of late years, have run so much upon orchards, that in a village near Boston, consisting of about 40 families, they made near 3000 barrels of cyder in the year 1721: and in another town, of 200 families, in the same year, they made near 10,000 barrels. Some of their apple trees will make 6, some have made 7 barrels of cyder; but this is not common; and the apples will yield from 7 to 9 bushels for a barrel of cyder: a good apple tree will measure from 6 to 10 feet in girt. A fine pearmain, which at a foot from the ground measured 10 feet 4 inches round, has borne 38 bushels of as fine pearmains, as ever were seen in England. A Kentish pippin at 3 feet from the ground, 7 feet in girt; a golden russetin 6 feet round. The largest apple tree, that Mr. D. could find, was 10 feet 6 inches round; but this was no graft.

An orange pear tree grows the largest, and yields the fairest fruit. He observed one of them, near 40 feet high, that measured 6 feet and 6 inches in girt, a yard from the ground, and has borne 30 bushels at a time; and he measured an orange pear fruit, that was 11 inches round the bulge. He had a warden pear tree, that measured 5 feet 6 inches round. One of his neighbours had a bergamot pear tree, that was brought from England in a box, about the year 1643, that measured 6 feet about, and has borne 22 bushels of fine pears in one year. About 20 years before, the owner took a cyon, and grafted it on a common hedge pear, but the fruit proved not quite so good, and the rind is thicker than that of the original.

The peach trees are large and fruitful, and commonly bear in 3 years from the stone. Mr. D. had one in his garden, of 12 years growth, that measured 2 feet and an inch in girt, a yard from the ground, which 2 years before bore near a bushel of fine peaches. The common cherries are not so good as the Kentish cherries of England; and they have no dukes, or heart cherries, unless in two or three gardens.

Some years before, Mr. D. measured a platanus occidentalis, or button wood tree, as they are called, that was 9 yards round, and it held its thickness a great way up. This tree, when cut down, made 22 cord of wood. A gentleman informed him, that in the forest, he met with a straight ash, that grew like a pillar, to a great height, and free from limbs, that measured 14 feet 8 inches round, near a yard from the ground; and Mr. D. met with a sassafras tree, that measured 5 feet 3 inches in girt. Among the trees of quick and easy growth, the button wood beforementioned, and the locust tree, are the most remarkable: the locust tree may be called the American manna. Mr. D. has known a seed of it blown off from the tree into his garden, that took root of itself, and in less than 2 years was got above 6 feet high, and as thick as a common walking cane. The platanus he frequently propagated by cutting off sticks of 5 or 6 feet long, and setting them a foot deep into the ground in the spring of the year, when the season is wet: they thrive best in a moist soil.

An onion, set out for seed, will rise to 4 feet 9 inches in height. A parsnip will reach to 8 feet; red orrice will mount 9 feet; white orrice 8. In the pastures, he measured seed mullen 9 feet 2 inches in height, and one of the common thistles above 8 feet.

Among the remarkable instances of the power of vegetation, Mr. D. had a well attested account of a pumpkin seed, from Mr. Edwards of Windsor, as follows: that in the year 1699, a single pumpkin seed was accidentally dropped in a small pasture where cattle had been foddered for some time. This single seed took root of itself, and without any manner of care or cultivation, the vine ran along over several fences, and spread over a large piece of ground far and wide, and continued its progress till the frost came and killed it. The plant had only one stalk, but a very large one; for it measured 8 inches round; from this single vine were gathered 260 pumpkins; one with another as large as a half peck; enough in the whole, to fill a large tumbrel, besides a considerable number of small and unripe pumpkins. The Philos. Trans. give an account of a single plant of barley, that by steeping and watering with salt-petre dissolved in water, produced 249 stalks, and 18,000 grains; but then there was art, and even force in that case; whereas in the other, there was nothing but pure nature and accident.

The Indian corn is the most prolific grain that we have, and commonly produces 1200, and often 2000 grains, from one: but the fairest computation is thus; 6 quarts of this grain will plant an acre of ground; and it is not unusual for an acre of good ground to produce 50 bushels of corn.

The Indian corn is of several colours, as blue, white, red, and yellow; and if they are planted separately, or by themselves, so that no other sort be near them, they will keep to their own colour, i. e. the blue, will produce blue, the white, white, &c. But if in the same field, you plant the blue corn in one row of hills, as they are called, and the white, or yellow, in the next row, they will mix, and interchange their colours; that is, some of the ears of corn in the blue corn rows, will be white, or yellow; and some again, in the white or yellow rows, will be of a blue colour. The hills of Indian corn are generally about 4 feet asunder, and so continued in a straight line, as far as the field will allow; and then a second line or row of hills, and so on; and yet this mixing and interchanging of colours has been observed, when the distance between the rows of hills, has been several yards; and Mr. D. has been assured, that the blue corn has thus communicated, or exchanged, even at the distance of 4 or 5 rods; and particularly in one place, where there was a broad ditch of water between them. Some of our people, but especially the aborigines, have been of opinion, that this commixtion, and interchange, was owing to the roots, and small fibres reaching to and communicating with one another; but this must certainly be a mistake, considering the great distance of the communication, especially at some times, and cross a canal of water; for the smallest fibres of the roots of the Indian corn cannot extend above 4 or 5 feet. Mr. D. is therefore of opinion that the stamina, or principles of this wonderful copulation, or mixing of colours, are carried through the air by the wind; and that the time or season of it, is when the corn is in the earing, and while the milk is in the grain for at that time, the corn is in a sort of estuation, and emits a strong scent. One thing which confirms the air's being the medium of this communication of colours in the corn, is an observation, that a close, high board fence, between two fields of corn that were of a different colour, entirely prevented any mixture or alteration of colour, from that they were planted near.

An apple tree in the town bears a considerable quantity of apples, especially every other year, which never had a blossom:* for three years he went in the proper season, to observe it; and when all the rest of the orchard was in the bloom, this tree had not one blossom. Not content with once going, he went repeatedly, till he found the young apples perfectly formed. In 1723 he went early, not knowing but that it might blow sooner than the other trees, but found no blossoms; and the owner, with many of his neighbours, assured him, they have known the tree these 40 years, and that it never had a blossom. Mr. D. opened several of the apples, and observed but very few seeds in them; and some of them lodged single in the side of the apple. The tree was no graft, and the fruit but ordinary for taste. He could not perceive, by his observation, but that in all other respects it fructified like other apple trees.

I would just mention, what is frequently observed in our gardens, as to the winding or running vines, more especially the hop, and the French or kidney beans; how contrary they are to each other in their climbing, and yet how steadily they observe their own laws: the hop vine winding about the pole with the sun, and the bean against the sun; and this course they keep with such obstinacy, that though an attempt has been made, over night, to force the hop vine to wind against the sun; yet in the morning it has got back again to its natural course; and the bean again has done the same in her way. In like

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* The flowers of this tree were destitute of petals, but possessed every other essential organ, and were consequently fruitful.

manner, the Indian corn has always an equal number of rows of grain on the ear, as 8, 12, &c.

A Dissertation concerning the Figure of the Earth, by the Rev. John Theophilus Desaguliers, LL.D. F.R.S. N° 386, p. 201.

That the earth is of a spherical figure, or nearly such, has been often proved. But as a little variation from a true sphere, besides the irregularity of high hills and deep valleys, does not hinder us from calling the earth a globe; so to determine what that variation may be, since modern philosophers are divided about it, may be a subject not unacceptable.

M. Cassini says, that the earth is an oblong spheroid, higher at the poles than the equator, making the axis longer than a diameter of the equator about 13 French leagues; which he deduces from comparing his father's measures of the meridian, from Paris to the Pyrenean mountains, with those of M. Picard; of which an account may be seen in the Memoirs of the Royal Academy for 1713. But having afterwards continued the meridian, which is drawn through France, from Paris to Dunkirk, he still draws consequences to prove the earth an oblong spheroid; but then he makes the axis exceed the equatorial diameter 34 leagues.

Sir Isaac Newton makes the earth higher at the equator, and consequently flatted towards the poles, reckoning its equatorial diameter 34 English miles longer than the axis; which he proves from the principles of gravity, and the centrifugal force arising from the diurnal rotation of the earth; and to confirm this, mentions several experiments on pendulums, which have been made shorter to swing seconds near the equator, than in greater latitudes.

These are the two opinions which have divided philosophers, and which Dr. D. proposed to examine.

M. Cassini, taking the measures abovementioned to be exact enough, not only to determine the magnitude of a degree of the earth, corresponding with a degree of the great circle of the heavens, but also to show the difference in the degrees of the earth; reckoning those that were measured in the south of France, to exceed those towards the north, by a certain number of toises and feet, demonstrates, that if the degrees of the earth are longer towards the equator than the poles, the plane of the meridian must be an ellipse, whose long axis is that of the earth. This he demonstrates in the French Memoirs for the years 1713 and 1718.

If M. Cassini's measures of terrestrial degrees, decreasing from the equator towards the pole, were grounded on observations liable to no error, he would have fully proved his figure of the earth. But since those measures are not built on a mathematical certainty, his premises may be called in question, and his conclusion, though mathematically drawn from these premises, is only probable. But Dr. D. first endeavours to show, from undoubted phænomena, that as his conclusion will lead to an absurdity, his measures must be false, because his reasoning from them is just; which therefore disproves his figure of the earth. And he afterwards points out some of the errors which he supposes to have occasioned the mistake in the measures.

M. Cassini, as well as the English astronomers, believes that the earth makes one revolution about its axis once in $23^h\ 56^m$. Let H be taken in any parallel of latitude, fig. 15, pl. 1, as for example, that of 51° 46′; a plumb line, LH, will be perpendicular to the curve BH, at H, and produced pass through the zenith of the point H, if the earth had no diurnal rotation; but since the earth moves round its axis, all bodies on its surface endeavour to fly from the axis of their motion, with a force proportionable to their distance from it, in a direction along the plane of their parallel. Let that force, explained by M. Huygens, and called a centrifugal force, be represented by the line Hl, or its equal and parallel Lh; now a plummet placed at L, if the earth stood still, would descend in the line LH; but as it is at the same time acted on by the force Hl in the direction Lh, it will move in the direction Ll, the diagonal of the parallelogram Hl, according to the known laws of mechanics; and the plumb line LH, instead of being perpendicular to the curve at H, will in the latitude 51° 46′ make an angle of 5′ with HL. This angle will be less towards the poles, till at the very pole it quite vanishes, as it also does at the equator. Now since there is no such angle observed, but in all water levels we find the plumb line always perpendicular to the line of level, the surface of the earth must be depressed towards G, and rise farther from the axis towards I, in order to become perpendicular, that is, to have its tangent perpendicular to the line Ll, in which the plumb line must descend.

But to say, that those gentlemen could observe the latitude so nicely, as to find a difference in the length of the terrestrial degrees, and that only of 12 toises, when they made it the least, or of 31 toises when they made it the most, is attributing to them an exactness so far beyond the nature of the instruments which they made use of, that it would be rather a dispraise than a commendation to insist upon it.

For in the first place, the instrument with which they took observations for the latitude at the two ends of their meridian, was a 10-foot sector, where the 200th part of an inch answers to 8 seconds of a degree: now the 200th part of an inch being one of the least visible parts we can see in a divided line; they could not take an angle nearer than that; and their instrument was divided only to every 20 seconds. Now they allow that 16 toises on the surface of the earth answer to one second in the heavens; and they pretend not to observe nearer than to about 3 seconds, which therefore cannot determine a difference less than 48 toises; whereas the degrees are only supposed to decrease at most 31 toises each, from Collioure to Dunkirk. But an error of 8 seconds would make a difference of 128 toises on the surface of the earth; above ten times greater than the difference of degrees in the first supposition, and four times greater than that difference in the last. Besides, the latitude was not observed in the intermediate places between Paris and Collioure, with the abovementioned instrument of 10 foot radius; but they used a quadrant, whose radius was only 39 inches, and sometimes an octant of 3 feet radius; and they say themselves, that it is not the observations made at the ends of the meridian that we are to deduce the difference of the length of a degree from, but the altitudes taken at several places between the extremes; and if we grant, that they can take an angle very well to 4 or 5 seconds, with the great instrument, they cannot come nearer than 12 or 15 seconds with the quadrant or octant, which we must depend upon for the difference of the measure of degrees: so that on the whole, we are to determine a length of 31 toises, by an instrument which is liable to err above 200.

But there is another error, which might considerably mislead the French gentlemen, and make the degrees appear longer in the south of France; that is, the error in taking the true height of several mountains in Auvergne, Languedoc, and among the Pyreneans; for if they have allowed too much for the air's refraction, which, by the observations of travellers, is greater towards the northern regions, and diminishes as we go southward, the heights of those mountains will be taken too little, and their bases consequently longer, which will make the degrees appear greater than they are.

Now one such mistake, in one degree, will give a difference above twice as great as the supposed difference of degrees in that latitude, which they make of 31 toises. And that there was a mistake of this kind in taking the height of that mountain, Dr. D. shows.

The vapours, that generally float in the air about the tops of high hills, make it so difficult to take their height exactly, that experiments made with the barometer will, by observing the fall of the Mercury, show the height nearer than any thing else we know of. There were indeed several experiments made with the barometer, where the differences of the height of the Mercury, from the heights at which it stood at the Royal Observatory, are said to answer to so many toises; but of nine observations mentioned by M. Cassini, there are not two where the number of toises, said to correspond to the heights of the barometer, do agree together.

A sight of the table they give, for the altitudes answering to the fall of the barometer, show that these observations are not to be depended on, for determining the height of the mountains in the south of France; for the differences are not small, such as might happen in making the experiments; but such as render the observations useless for the purposes abovementioned.

And it was wrong to compare the height of the mercury in the south of France, with the height that the mercury was at in the barometer of the Royal Observatory at the same time. The way to have made the experiments with the barometer exactly, would have been to have observed the height of the mercury at the bottom and at the top of the mountain, and that with a tube of a pretty large bore.

This error will increase the measure of the 44th degree of latitude on the earth, and, by observing what was done in the next degree, we shall find that that degree was taken too short, as Dr. D. shows by a calculation. And though the 45th degree of latitude may be 13 toises more than the 44th, it might by this means appear to be considerably less.

Such a mistake might be the occasion of making the hypothesis of the earth an oblong spheroid, especially because, in this hypothesis, the degrees differ most in length from each other about the 45th degree; and when once an hypothesis is set on foot, we are too apt to draw in circumstances to confirm it; though, perhaps, when examined impartially, they may rather weaken than strenthen our hypothesis.

Having thus given his reasons for disapproving of M. Cassini's opinion, concerning the figure of the earth, Dr. D. next considers Sir Isaac Newton's, who makes it higher at the equator than at the poles.

Dr. D. then continues the dissertation in the next N° p. 239, as follows.

How the figure of the earth is deduced from the laws of gravity and centrifugal force, is very well shown by Dr. John Keill, in his book against Dr. Burnet's Theory of the Earth; Dr. D. from this takes his ideas; where they may be seen at large.

After which the Doctor concludes thus: Let us now in a few words compare the experiments and observations employed to confirm each of the opinions abovementioned.

To prove M. Cassini's figure of the earth, we must take the altitude of a star nearer than to 2 seconds, because 2 seconds answer to 32 toises on the surface of the earth, and the difference of the length of degrees is but 31. And what is more, we must take this angle with an instrument of 39 inches radius, because the 10-foot sector was only used at the ends of the two parts of the meridian.

To disprove M. Cassini's hypothesis, we need only observe whether a plumb line makes an angle of 5 minutes with a perpendicular to the surface of stagnant waters, or lines of level.

To prove M. Cassini's opinion, the height of a great many mountains must be accurately measured by trigonometry, which mathematicians have always found very difficult.

To prove Sir Isaac Newton's opinion, we are only to measure, to about one-tenth of an inch in a rod of 39.129 inches; and to know what to allow for the lengthening of the same rod by the summer heat, when it is shut up in a case, and carried towards the equator. For though the experiments on pendulums, made by several persons that travelled southward, differ among themselves, yet they all agree in this, that the observers were obliged to shorten their pendulums, in order to make them swing seconds, as they went towards the equator. And when we come to compare them together, in order to have the exact proportion of length in different latitudes, we must rely on the most exact experimentor, which we may very well do on M. Richer; because when he found a difference, he was so careful to find out how much it was, that he caused a simple pendulum to swing, and compared it with a good pendulum clock, which he did several times every week for 10 months together; and when he returned to France, he compared it with the length of the pendulum at Paris, which is of 3 feet 8$\frac{3}{5}$ lines, or 39.129 English inches, and found it to be shorter by 1$\frac{1}{4}$ line.

Dr. D. in another N° resumes the discourse as follows: Since his paper concerning the figure of the earth was read before the Royal Society, M. Mairan, in the Memoirs of the Royal Academy of Paris, for the year 1720, has a dissertation, where he has taken a great deal of pains to reconcile the observations made on pendulums, found to be shorter at the equator than at Paris, when they swing seconds, with the oblong spheroidical figure of the earth, deduced from M. Cassini's measures. And though on a strict examination of his conjectures, and what he gives for demonstrations, there is no reason to alter the opinion concerning the oblate or flatted spheroid, which Sir Isaac Newton has shown to be the figure of the earth; yet since it might be thought by some, who have read M. Mairan's treatise, and afterwards may read the Doctor's, that he had not considered all the circumstances, he shows where he thinks M. Mairan is mistaken, and gives other additional proofs of his assertions.

First then, M. Mairan says, "that it is as reasonable to suppose the earth, if it was once fluid, to have been an oblong spheroid at first, as a sphere; and that, in such a case, the centrifugal force of the several parts of the earth, arising from its revolution about its axis, which might convert a sphere into an oblate spheroid, would only change an oblong spheroid into one less oblong." But if the earth was at first a fluid, supposed homogeneous, and of any given form, and left to those laws which we find to obtain at present, it must put on a spherical figure; for the same reason that drops of mercury, of water, and other fluids, put on such a figure. And to suppose any change made in that figure from the pressure of an external fluid, filling up all space, is contrary to what has been demonstrated by Sir Isaac Newton, in his Principia, lib. 2, prop. 19, where he shows, that if any portion of a fluid be compressed by the same or any other homogeneous fluid, that portion will not have its figure altered by that pressure.

Now, without considering the unreasonableness of the supposition, let us imagine the earth to have been an oblong spheroid at first, and then to have a diurnal revolution given to it, which should by degrees shorten its axis, to bring it to what Messrs. Cassini and Mairan suppose it at present to be. If in such a case the earth be supposed fluid enough to change its figure by the revolution about its axis, why should it stop when the equatorial diameter comes to want just $\frac{1}{96}$ part of the length of the axis? since two powers act upon it to shorten its axis, viz. gravity, and the centrifugal force; the first of which has already been shown capable to reduce it to a sphere, and the centrifugal force is acknowledged by M. Mairan to be, as Sir Isaac Newton has proved it, at the equator equal to $\frac{1}{289}$ part of the gravity there. Certainly the alteration of figure would not have stopped before the earth came to be a sphere; nay, and it must have risen at the equator, and how much, has been already shown in the former paper.

Again, if we suppose the earth of an heterogeneous fluid, before the diurnal revolution, the heaviest parts would go towards the centre, and the lighter towards the surface; and that way the terraqueous globe would also become a sphere. Then if, when the central parts are fixed, and the superficial strata are still fluid, the earth receives a diurnal motion; it will rise at the equatorial parts, and that to a greater height than what has been shown in the former paper, where the earth is supposed of uniform matter. And that something like this must be the case, appears from what Sir Isaac Newton has said on this subject. For after having shown, from supposing the earth of uniform matter, that the centrifugal force of all its parts would bring it to be $17\frac{1}{6}$ English miles higher at the equator than at the poles, and after having given a table of the proportionable decrease of the length of the degrees of a meridian of the earth, going from the poles to the equator, in such a figure of the earth, with the lengths that pendulums must have to swing seconds in several latitudes; from a comparison of the lengths of pendulums, observed by different persons to be shorter towards the equator than in greater latitudes, when they swing seconds, he shows that the earth must be $31\frac{7}{12}$ miles higher at the equator than at the poles; and therefore that it must be denser towards the central than the superficial parts, to produce a flatted spheroid, where the equatorial diameter must exceed the axis so much more; that is, be longer something more than $\frac{1}{125}$ part.

Lastly, let us suppose the earth, at its first creation, to have been made of land and water, the first as solid, and the last as fluid as it is now, but of M. Cassini's figure, and examine the consequence. Since in that figure the axis is $\frac{1}{96}$ part longer than the equatorial diameter; the gravity will be so much greater at the equator than at the poles, that the waters will all flow to the equatorial, and leave the polar regions; which will happen still more by the centrifugal force, which the earth in its diurnal motion will give to the fluid; and therefore the sea would be $43\frac{85}{100}$ miles higher at the equator than at the poles, which must overflow all the torrid zone, and leave the polar regions dry.

Now if we suppose the same figure of the earth, but the land, at its first creation, as firm as it is now, it will in that case follow from M. Mairan's principles, that the sea must rise and overflow all the equatorial regions, though the earth had no diurnal revolution; and much more so, when the centrifugal force, arising from the diurnal motion, helps to carry the water the same way.

Having shown that M. Mairan's account of the action of gravity, on several places upon the earth's surface, can be of no service for reconciling the experiments made on pendulums, with the figure of the earth deduced from M. Cassini's measures; Dr. D. proceeds to show that his demonstrations are founded on wrong principles. And first, in regard to gravity.

This gentleman has followed Sir Isaac Newton, in saying, that gravity increases in a duplicate reciprocal proportion of the diminished distance from the centre of the force, and so vice versâ; but he has followed Sir Isaac Newton no farther than served his present purpose; otherwise he would have known, that in respect to a central body, as a planet, towards which others are urged by gravity, this law obtains only, as bodies attracted are removed from the surface of the planet to greater distances from the centre compared with that distance, or as from greater distances they approach nearer to the planet; that the greatest action of gravity is at the surface of the planet; that afterwards in advancing towards the centre, the force of gravity on the body attracted continually grows less, decreasing directly as the distance; and that this holds true in a spheroid as well as a sphere; that on different parts of the surface of the earth, the gravity on bodies is reciprocally as their distance from the earth's centre: that though at a considerable distance we consider the earth, or any planet, or even the sun, as a point endued with an absolute force, proportional to its quantity of matter; yet when we come so near the body as to consider the space it takes up, we are to take notice, that the whole gravity of the body is made up of the sum of the attractions of all its parts properly combined; and therefore, that when a corpuscle, or body attracted, comes to be within the planet, or body attracting, the matter above it draws it back in such a manner, that it leaves it only a force to go on towards the centre, which is directly as the distance, as before said; just as if a body concentric to the planet had its surface just where the corpuscle is, and all the exterior crust or shell was annihilated.

Further, Mons. Mairan demonstrates, that in an oblong spheroid, the diminution of gravity, by the centrifugal force, increases faster in going from the poles to the equator, than it would do in a sphere, and faster in a sphere than it would do in a broad spheroid; and therefore would show, "That though the surface of the earth is nearer to the centre in M. Cassini's figure than in Sir Isaac Newton's, yet the centrifugal force will diminish the gravity so fast in going from Paris to the equator, that the shortening of pendulums, to make them swing seconds at the equator, may very well be accounted for that way."

Now let us examine into this matter, to see whether the cause is adequate to the effect.

If the distance from the surface of the earth at the pole to the centre be 96, and the distance of the surface at the equator be 95, the distance of the surface at Paris, in the latitude of 48° 50′, will be 95.562, by the property of the ellipse. Now since the force of gravity, in different places on the earth's surface, is reciprocally as the distance from the centre, and the lengths of pendulums, that perform their vibrations in the same time, are directly as the force of gravity; therefore the length of pendulums at Paris, will be to their length at the equator, at 95 to 95.562, that is, as 440.555 to 443.165, and consequently they must be lengthened 2.61 lines. But as, from M. Mairan's principles, the diminution of gravity by the centrifugal force is greater at the equator than at Paris, hardly $\frac{1}{410}$ part of the whole gravity at the equator, the pendulums must be shortened in that proportion; so that then the length of a seconds pendulum will be 440.555 + 2.61 — 1 lines. But as that quantity is greater than 440.555, therefore the pendulums on the whole must be lengthened: nay, though we should allow a shortening of two lines; since by observation pendulums are found to be about 2 lines shorter at the equator, the oblong spheroidical figure of the earth cannot be consistent with the experiments on pendulums.

And now, Dr. D. thinks he has answered all that relates to the figure of the earth in M. Mairan's Dissertation; in showing, that his conjectures can neither be supported by those physical principles, which Sir Isaac Newton has mathematically deduced, from unquestioned observations and experiments accurately made; nor even by those principles which M. Mairan has assumed to serve his intended purpose:—That his demonstrations relating to the difference of the action of the centrifugal force, are of no service to him, for reconciling the experiments made on pendulums, with M. Cassini's measures;—because, when applied to Sir Isaac Newton's principles, they will make pendulums longer at the equator than at Paris, and when applied to M. Mairan's own principles, they will make them a whole inch shorter at the equator than at Paris, contrary to all observations, which at a medium, make pendulums but about 2 lines or $\frac{166}{1000}$ of an inch longer at the equator than at Paris:—That he has built his demonstrations on a wrong notion of gravity:—And that he has not considered what is most material in the effect of the centrifugal force, acting on bodies descending by their gravity, between the equator and the poles, namely, the alteration of their line of direction, which would make them fall out of the perpendicular towards the equator.

To conclude, the Doctor proposes a method of observing the figure of the shadow of the earth in lunar eclipses, by which the difference between the diameters in the oblong spheroidical figure, if there be such a one as M. Cassini affirms, viz. of 96 to 95, may be discovered.

If therefore those astronomers who have instruments nice enough, and sufficient skill in the management of them, to take angles to 3 or 4 seconds of a degree, will observe what has been mentioned in total and partial eclipses of the moon; by such observations they will easily convince us that the figure of the earth is such as M. Cassini supposes it, or convince him that he has been mistaken.

Some Observations on an Ostrich, dissected by Order of Sir Hans Sloane, Bart. By Mr. John Ranby, Surgeon, F. R. S. N° 386, p. 223.

Having separated the muscles of the abdomen, which in this subject were only 2 oblique pair, they observed, between their tendons, which were very strong, and the peritoneum, which was exceedingly thin, a thick layer of sevous fat, whose office, considering the smallness of the epiploon, and the few adipose vesicles of the mesentery, with the thinness of the peritoneum, might probably be to supply the part both of epiploon and mesentery in other animals, as to lubricating the intestines.

There were two distinct ventricles, contrary to the observation of the Royal Academy at Paris The first, and in its natural situation the lower, was considerably larger than the 2d, and uppermost muscular one; besides, that it had strong muscular fibres, both circular and longitudinal: the duodenum comes immediately out of the 2d ventricle.

Both ventricles were distended beyond their usual form, and filled up with so large a quantity of food of different kinds, as stones, bones, sticks, grain, and other food, that it was almost impossible for them to perform their office of digestion, which very likely was one of the chief causes of the animal's sickness and death; and indeed the contents of both seemed to have undergone but little or no alteration. The epiploon partly covered the first ventricle, but it was no ways proportionable to the size of the animal.

The spleen was fastened, by a membrane, to the right side of the second ventricle, and was very small, considering the size of the animal.

The glands of the mesentery were hardly visible, but the veins and arteries very conspicuous.

The cæcums were near 3 feet in length, the diameter 1 inch 8 lines; they were fastened to the ileum, and not to the colon, as the gentlemen of the Royal Academy assert.

To their description of the kidneys there is nothing to add, except that the two ureters lay upon their surface, as they do in other birds, and that their different branches, coming from all the parts of the kidney, of which the superior was very conspicuous, entered the kidney about its middle, and formed there a very large pelvis.

The liver was in one cavity with the heart, of which it covered near one half: it had no gall-bladder, and but one ductus bilarius, inserted into the duodenum, about 2 inches below the pylorus, which seemed to have an immediate communication with the vena portæ, because, by blowing into it, this latter was also distended. The heart and liver were separated from the intestines by a membranous diaphragm. Both heart and liver were suspended by one common mediastinum, by the help of its several membranes, and 8 strong muscles on each side, arising from the upper part of the ribs, going from thence over the lungs, and ending in a very strong tendinous membrane, which is inserted into the spina dorsi.

The liquor, contained in the pericardium, was small in quantity, and perfectly transparent.

The lungs lay under the diaphragm and its muscles, in a deep cavity, formed by the 5 true ribs. They were pretty thick about the middle, and exceedingly thin and sharp towards the extremities.

The eye externally, somewhat resembled the human eye, except that it was less convex, with a free and moveable upper eye-lid, with eye-lashes, as most terrestrial animals have, besides a tunica nictitans, as in other birds. Besides the 7 muscles of the eye, as they are in brutes, it had 2 more, one arising from the forepart of the sclerotica, which soon formed a small tendon, obliquely surrounding the optic nerve, and then joined to another muscle, which arises opposite to the former, from which the tendon continues its way, and is inserted in the tunica nictitans. The aqueous humour was in greater quantity than common. The crystalline was of an uniform substance, but less convex on the inside, than without. The vitreous was small in quantity, considering the size of the eye; the choroides was entirely black, without that variety of colours at its bottom, which is common to most brutes. The fore part of the sclerotica, where it is annexed to the cornea, was bony, consisting of 15 bony scales joined to one another, so as to make one circular bone round the cornea.

As for a more particular description, I refer to the anatomical account given by the Royal Academy at Paris, in their Natural History of Animals, and to Vallisneri, professor at Padua, in his Notomia del Struthio.

An Account of the Appearance of Mercury, passing over the Sun's Disk, Oct. 29, 1723; *determining the mean Motion, and fixing the Nodes of that Planet's Orbit. By Dr. Edmund Halley.* N° 386, p. 228.

The transit of the planet Mercury, over the sun's disk, being one of the most curious and uncommon appearances that the heavens afford, astronomers, both at home and abroad, made due preparation to observe, with the utmost exactness, that which happened on the 29th of October, 1723, which the Doctor had predicted in the year 1691 (Phil. Trans. N° 193) would be in part visible in England. And the sky proving very favourable at that time, the ingress on the sun's limb was observed with the greatest accuracy.

Accordingly, the same day, Oct. 29, O. S. at Greenwich, in the Royal Observatory, the Doctor first perceived, with the 24-foot tube, the planet making a small notch in the sun's limb, at $2^h\ 41^m\ 23^s$ apparent time. And at $2^h\ 42^m\ 26^s$ he was wholly entered, making an interior contact, the light of the sun's limb just beginning to appear behind his dark body; which, notwithstanding the slowness of the motion, was in a manner instantaneous. Then, applying the micrometer to the said 24-foot tube, Dr. H. opened it so as to take in 16′ 15″, equal to the sun's semidiameter at that time; and causing the northern edge of the sun to move exactly along one of the pointers, he waited till the centre of Mercury came to move along the other, as he found it to do at $3^h\ 1^m\ 16^s$. But refraction contracting this difference of declination about 5 seconds, the sun being then but about 11° high, he concluded that the centres of the sun and Mercury were truly in the same parallel of declination, at $3^h\ 3^m$ nearly.

At Wansted in Essex, Mr. Bradly, Savilian Professor of Astronomy, observed with the Hugenian telescope, of above 120 feet long, the total immersion, or interior contact of the limbs, at $2^h\ 26^m\ 45^s$ æq. time, that is $2^h\ 42^m\ 38^s$ app. time, or 12 seconds later than I found at Greenwich; most of this difference being due to the difference of our meridians. And applying the micrometer to that vast radius, he measured the diameter of the planet 10″ 45‴. At $2^h\ 48^m\ 57^s$ he found the difference of declination between the southern limbs of the sun and planet by the micrometer, in a 15-foot tube, to be 15′ 19″. Therefore, allowing the observed semidiameter of the planet, and the refraction, the said difference was nearest 15′ 30″, and consequently Mercury more southerly than the sun's centre in respect of declination, 0′ 45″.

Mr. George Graham, in Fleet-street, London, observed the first impression on the sun's limb at $2^h\ 41^m\ 9^s$ app. t. and at $2^h\ 42^m\ 19^s$ Mercury was entirely within the disk. At $3^h\ 6^m\ 41^s$ he measured with a micrometer, in a 12-foot tube, the distance of his centre from the nearest limb of the sun, 2′ 13″. And again, at $3^h\ 25^m\ 24^s$, their distance was found 3′ 57″. At $3^h\ 34^m\ 43^s$ he measured the difference of delination, from the northern limb of the sun, 14′ 57″, which, corrected by refraction, becomes 15′ 4″, that is, 1′ 11″ more northerly than the sun's centre.

In the Observatory at Paris, Sig. Maraldi observed the first appearance of Mercury on the sun's limb at $2^h\ 50^m\ 13^s$ app. t. and the interior contact at $2^h\ 51^m\ 48^s$. And Mr. de Lisle, observing apart, concluded the same at $2^h\ 51^m\ 37^s$, but suspects it might have been some few seconds later. This gentleman has communicated his observation at large, from whence we shall only borrow the following observed latitudes.

At 2^h	56^m	20^s	Latitudo Borea Mercurii	3′	36″
3	00	40	....	3	42
3	10	20	....	3	46
3	16	12	....	3	55

At Bononia, in Italy, Sig. Manfredi observed Mercury indenting the sun's limb at $3^h\ 26^m\ 22^s$; and that he was gotten entirely within, at $3^h\ 27^m\ 45^s$. And these are the observations most to be depended on that have been received from abroad.

In order to deduce from this phenomenon, so accurately observed, what may contribute to perfecting the theory of Mercury's motion, which seems to need but very little correction; Dr. H. carefully computed, from his tables, the motion of the planet in 5 hours, and found his apparent motion on the sun, to be in longitude 29′ 21″ retrograde, and that his latitude increased northerly 4′ 17½″ in the same time; whence the horary motion in longitude was 5′ 52″, and in latitude 0′ 51½″; and thence the angle of the visible way with the ecliptic 8° 19′, and the horary motion in that way 5′ 56″. Again, the angle of the ecliptic with the meridian, being in this place 73° 24′, the visible way of Mercury made an angle of 65° 5′, with the meridian passing through the sun's centre; whence the horary change of declination becomes exactly 2′ 30″.

These data Dr. H. chooses rather to take from the theory, than from immediate observation; because there is always an unavoidable, though small uncertainty, in what we observe, yet greater than there can be in the computation for so small a space of time, especially now the theory is so very near the truth.

This premised, let us now inquire the true time of the central ingress, and the latitude of the planet at that time. And first, by Dr. H.'s account, Mercury was in the parallel of the sun's centre at 21½ minutes after the central ingress, in which time he ascended to the northward 54″; and so much therefore was he more southerly than the sun's centre at his ingress. Mr. Bradley, 7½ minutes after the said ingress, in which the planet ascended 19″, found his declination 45″ south, and therefore at the ingress his declination was 1′ 4″ south. And by Mr. Graham's observation, Mercury was more northerly than the sun's centre 1′ 11″, 53ᵐ 20ˢ after the central ingress; but in that time Mercury ascended 2′ 13″; therefore, according to him, at the ingress the planet had 1′ 2″ south declination. We shall not therefore err above a semidiameter of Mercury, if we assume his declination, at that time, to have been precisely one minute.

Now the sun's semidiameter being then 16′ 15″, one minute is the sine of 3° 32′ in the arch of the sun's limb; and consequently, the point of this ingress was 13° 4′ more northerly than the ecliptic; whence the latitude of Mercury was then 3′ 40″ north, and difference of longitude 15′ 50″, by how much he at that time followed the sun's centre.

If therefore, to the arch of 13° 4′, we add the double of 8° 19′, or of the angle which the visible way made with the ecliptic, we shall have 29° 42′ for the point on the sun's western limb, at which the planet made his exit, likewise to the north of the ecliptic. Hence the chord described in the whole transit, was of 137° 14′, and the chord itself 30′ 16″; and the nearest distance to the sun's centre 5′ 56″. Now the horary motion, in this chord, being 5′ 56″, the whole duration of this mercurial eclipse becomes $5^h\,6^m$ in respect of the planet's centre; and therefore the nearest approach of their centres was at $5^h\,14^m\,30^s$ at Greenwich, and the exit at $7^h\,47\frac{1}{2}^m$, both visible in our American plantations, had there been any curious person there qualified to observe them.

It follows likewise, by the observed diameter of Mercury, 10″ 45‴, that he was very little less than 2 minutes of time in passing the limb; and, by the given nearest distance to the sun's centre, it is concluded that he was in conjunction. in point of longitude, at $5^h\,23^m\,15^s$, having then precisely 6′ 00″ north latitude. Nor can it be doubted, but that all this would have been found exceedingly near to truth, had not the too early setting of the sun deprived all Europe of the desirable sight.

There being a very remarkable period of the motion of Mercury in 46 years, in which time he makes 191 revolutions about the sun; this transit is found to have been preceded by two others at that interval: the first, in the year 1631, when Gassendus at Paris, on the 28th day of October, old style, was the first that ever observed this appearance of Mercury within the sun's disk, and found him to pass off at $10^h\,28^m$ mane. The second was, Oct. 28, 1677, when Dr. H. had the good fortune to observe both the ingress and egress of the planet, in the island of St. Helena; the middle time, when he was nearest to the sun's centre, being there but $3^m\,50^s$ past noon, and the visible duration of the transit of the centre of the planet, $5^h\,14^m\,20^s$; which was some small matter contracted by parallax, and most likely might have been $5^h\,15^m\,00^s$ without it. Now in $5^h\,15^m$ Mercury described the chord of 146° 52′ in the sun's limb, being 31′ 9″, and consequently the nearest distance to the centre was 4′ 38″, or the sine of 16° 34′, the sun's semidiameter being radius; that is, 1′ 18″ less than we found it in 1723. Hence also it follows, that the true conjunction in longitude was 7 min. of time later than the nearest approach of the centres, viz. at $0^h\,10^m\,50^s$ at St. Helena, or at $0^h\,35^m$ past noon at Greenwich: and that the north latitude of the planet, at that time, was 4′ 41″.

Supposing therefore the nearest distance of the centres, in the transit of 1631, to have been 3′ 20″, that is, 1′ 18″ less than in 1677, we shall find that Mercury then described a chord of 156° 20′, traversing the sun's disk in $5^h\,21^m\,30^s$; so that supposing his exit at $10^h\,28^m$ at Paris, that is $10^h\,18^m\,40^s$ at Greenwich, he entered on the sun at $4^h\,57^m\,10^s$ in the morning; and was nearest his centre at $7^h\,38^m$ app. time, but in the same longitude with him at $7^h\,43^m$, or Oct. $27^d\,19^h\,43^m$ app. time, having then 3′ 22″ north latitude.

And here the Dr. observes, that above 30 years before, viz. in Philos. Trans. N° 193, for the month of March 1690-1, he predicted, by help of the two former, this last transit, with a surprising exactness, even beyond his hopes, making the time of the middle, or nearest approach of the centres of the sun and Mercury, Anno 1723, Oct. 29^{d} 5^{h} 19^{m} app. time, which was found by observation at 5^{h} $14\frac{1}{2}^{m}$, only $4\frac{1}{2}^{m}$ sooner; and, in latitude, Mercury was but 6 seconds more southerly than he had computed it; the error in longitude being little more than 2 diameters of this exceedingly small planet; and in latitude only a single semidiameter. So that for the future astronomers may trust his table of these transits, in Trans. N° 193, to a few minutes of time, and not wait with the uncertainty of hours, nay days, as has lately been done.

But, in order to obtain a yet further degree of exactness by help of this observation, it may be most expedient to compare with it the ingress observed at St. Helena; because in that, as well as in this, the latitudes of the planet being very small, a little error in them will not so much affect the longitudes. Supposing therefore, that Anno 1677, Oct. 27^{d} 21^{h} 26^{m} 15^{s} at St. Helena, or 21^{h} 50^{m} 15^{s} app. time, at Greenwich, the centre of Mercury entered on the sun, and that at that time he was $8\frac{1}{4}$ degrees on the sun's limb, to the north of the ecliptic, according to what is above concluded, it follows, that he had then 2′ 20″ north latitude, and 16′ 5″ greater longitude than the sun's centre; as in this present transit, Oct. 29^{d} 2^{h} 41^{m} 30^{s} app. time, at Greenwich, he had 3′ 40″ north latitude, and 15′ 50″ more longitude.

Now the apparent geocentric differences of longitude, are to the real heliocentric differences, as the planet's true distance from the sun, to his distance from the earth; that is, in both cases, as 313 to 676; therefore, in 1677, Mercury wanted 34′ 45″ of the conjunction with the sun; and, in 1723, but 34″ 13‴, at the times of his apparent ingress on the disk. And, equating the times, he finds that the sun, Anno 1677, Oct. 27^{d} 21^{h} 34^{m} 20^{s} æq. time, was in ♏ 15° 36′ 55″, and consequently Mercury's heliocentric place ♉ 15° 2′ 10″: and, Anno 1723, Oct. 29^{d} 2^{h} 25^{m} 30^{s} æq. time, the sun was in ♏ 16° 39′ 43″, and therefore Mercury at that time in ♉ 16° 5′ 30″.

Mercury therefore, in 46 years, with 11 intercalations, and besides 1^{d} 4^{h} 51^{m} 10^{s}, has made 191 revolutions to the equinoctial points, and over and above 1° 3′ 20″. But, by the Scholion to Prop. XIV. Lib. 3. Nat. Philos. Principia Math. the motion of the Aphelion of Mercury, from the equinox in that time, is 40′ 18″; so that there remains 23′ 2″ of true anomaly to be reduced to the mean: now the mean anomaly of Mercury, in both cases, being 5 sig. 12° 23′ 2″ of true anomaly, gives 15′ 24″ mean anomaly; which added to 40′ 18″, becomes 55′ 42″, for the mean motion above so many revolutions: and this is to be increased by 8″, to reduce it to the plane of Mercury's orb, in all 55′ 50″.

Hence, doubling the interval, in 92 Julian years $1^{d}\ 9^{h}\ 42^{m}\ 20^{s}$, the mean motion of Mercury from the equinox is 0^{s} 1° 51′ 40″, from which taking 5° 44′ 50″, the motion in $1^{d}\ 9^{h}\ 42^{m}\ 20^{s}$, we have his motion in 92 Julian years 11^{s} 26° 6′ 50″, and in 100 years, 2^{s} 14° 2′ 13″, which is but 20″ more than the Dr. had some years since printed it, in his astronomical tables, shortly to be published, and differs but one hour's motion from them in 3000 years.

The forementioned proportion of the distances, viz. 313 to 676, is also between the latitudes seen from the earth and the inclinations, or heliocentric latitudes of the planet: so that 2′ 20″, at the ingress of 1677, gives 5′ 2″; and 3′ 40″ in 1723, becomes 7′ 55″ for the latitudes at the sun. And the inclination of the orbit of Mercury to the plane of the ecliptic, determined by accurate observations near his northern limit, being 6° 59′ 20″, we compute the distance of the planet from his node, in the former 0° 41′ 7″, and in the latter, 1° 4′ 37″; which being deducted from his heliocentric places respectively, leave the place of the ascending node, in 1677, ♉ 14° 21′ 3″; and in 1723, ♉ 15° 0′ 53″: so that in 46 years, the node is found 39′ 50″ forwarder in the ecliptic; which is but 1′ 30″ more than the precession of the equinox in the same time. We may therefore safely assume the plane of the orbit of Mercury to be immoveable in the sphere of fixed stars, and its ascending node to be 0^{s} 15° 41′ from the first star of Aries. Nor can so very slow a motion (supposing such to be) be fully defined, but by the utmost care and diligence of future astronomers, after the observation of many ages.

As to the rest of the theory of this planet's motion, the Dr. makes his mean distance from the sun, 38710 such parts as the mean distance of the sun and earth is 100,000; and his greatest equation 23° 42′ 37″. The epocha of his middle motion, ineunte Anno 1723, vet. styl. from the equinoctial point, he makes ♐ 19° 9′ 31″; and that of his aphelion to the same time ♐ 13° 3′ 34″: the aphelion moving according to the order of the signs, 7 minutes in 8 years. And these numbers may represent the motion of Mercury, with an exactness equal to that of the other planets; perhaps as near as the sun's place by any tables, or those of the fixed stars by any catalogue yet extant.

It were to be wished, that some good observation, like this, had been made of the like transit of Mercury at his other node in April, where he was seen indeed April 23, 1661, but so imperfectly, that neither ingress nor egress was any where observed; and though it be certain that he traversed the sun on April 26, 1674; and again April 24, 1707; yet we were so unfortunate, that the conjunction in both happened so near midnight, that he escaped unseen by all the astronomers of Europe, excepting singly Mr. Roemer at Copenhagen, whose observation, received by the favour of Mr. de l'Isle the astronomer, communicated in the words of the manuscript Journal of Observations of the said Mr. Roemer. "Hodie sexto Maii (Anno 1707) hora matutina 4^h 19^m, spectabatur Mercurius in extremo margine solis jamjam exiturus; altus supra imum solis marginem $\frac{1}{4}$ diametri solaris, et ad sinistram in tubo (sc. invertente) accuratius hæc determinare non licuit ob moram nimis brevem." It was great pity, that he did not at least estimate, how many diameters of his body he was distant from the sun's limb, or what part of a diameter, if so near: but having examined this observation, the sun, at that time was but just risen, or rather rising, and soon after entered into a cloud, so that the sun's limb could not be distinctly seen, it always undulating and sparkling much, when so near the horizon; in which circumstance, a just observation could hardly be made.

Let us now see how our numbers, corrected as above, will represent this observation. Anno 1707, April 24^d 16^h 19^m at Copenhagen, is 15^h 28^m at Greenwich; but 15^h 24^m 20^s æq. time. To this time, the sun's true place is ♉ 14° 50′ 1″, and his distance from the earth 101005. The correct epocha of Mercury's mean motion, for the year 1707, is 3^s 13° 18′ 45″, to which adding, for the rest of the time, 3^s 19° 9′ 28″, we have his middle motion at the time of the observation ♏ 2° 28′ 13″; and taking his aphelion in ♐ 12° 49′ 49″ from it, we have his mean anomaly 10^s 19° 38′ 24″, and thence the equation to be added 12° 39′ 41″, and the place of Mercury in his orb ♏ 15° 7′ 54″. But the correct place of the descending node is ♏ 14° 46′ 25″, and therefore Mercury, being 21′ 29″ past the node, had 2′ 36″ south latitude at the sun; and his place, reduced to the ecliptic, was ♏ 15° 7′ 45″, that is, 17′ 44″ past the conjunction of the sun; which diminished in the proportion of 5567 to 4533, or of the distance of the planet from the earth to his distance from the sun, becomes 14′ 27″; and by so much was he past the conjunction as viewed from the earth. Again, by the same proportion, his geocentric latitude at that time, was 2′ 7″ south; and therefore his apparent distance from the sun's centre, was 14′ 37″; that is, but 1′ 18″ from his western limb; so that he might well be said to be, jamjam exiturus.

But that Mercury should at that time be so far northerly, as Mr. Roemer's words import, was absolutely impossible; and probably so acute an astronomer as Mr. Roemer was, could not himself be the observer, but some person less acquainted with these matters; which the words spectabatur Mercurius, instead of Mercurium vidi, seem to import. If he had then had north latitude, he must needs have been seen in the sun in April 1720, which we are assured he was not.

Lastly, it may not be amiss to advertise, that on the last day of Oct. 1736, Mercury will again traverse the northern part of the sun's disk, both ingress and egress being visible to all Europe.

An Essay on the Natural History of Whales; with a particular Account of the Ambergris found in the Sperma Ceti Whale. By the Hon. Paul Dudley, F. R. S. N° 387, p. 256.

The following account respects only such whales as are found on the coast of New England. And of these there are divers sorts or kinds.

As first, the right, or whalebone whale, is a large fish, measuring 60 or 70 feet in length, and very bulky, having no scales, but a soft fine smooth skin, no fins, but only one on each side, from 5 to 8 feet long, which they are not observed to use, but only in turning themselves, unless while young, and carried by the dam on the flukes of their tails; when with those fins they clasp about her small, and so hold themselves on. This fish, when first brought forth, is about 20 feet long, and of little value, but then the dam is very fat. At a year old, when they are called short heads, they are very fat, and yield to 50 barrels of oil; but by that time the dam is very poor, and termed a dry-skin, and will not yield more than 30 barrels of oil, though of large size. At 2 years old, they are called stunts, being stunted after weaning, and will then yield generally from 24 to 28 barrels. After this, they are termed scull-fish, their age not being known, but only guessed at by the length of the bone in their mouths. The whale-bone, so called, grows in the upper jaw on each side, and is sometimes 6 or 7 feet in length. A good large whale has yielded a thousand weight of bone. It is thought by some, that the hairy part of the whale-bone, and which is next to the tongue, serves in the nature of a strainer of their food.

The eye of a whale is about the size of an ox's eye, and situated in the hinder part of the head on each side, and where the whale is broadest; for his head tapers away forward from his eyes, and his body tapers away backward; his eyes are more than half way his depth, or nearest his under-part; just under his eyes are his two fins beforementioned; he carries his tail horizontally, and with that he sculls himself along.

The entrails of this whale are made and situated much like those of an ox, and the scalp is sometimes found covered with thousands of sea-lice. One of these whales has yielded 130 barrels of oil, and the tongue near 20. The whalebone whale is the most valuable, excepting the sperma ceti whale.

The scrag whale is near akin to the fin-back; but instead of a fin on his back, the ridge of the hinder part of his back is scragged with half a dozen knobs; he is nearest the right whale in figure and for quantity of oil; his bone is white, but will not split.

The finback whale is distinguished from the right whale, by having a large fin on his back, from 2½ to 4 feet long. He has also two side fins, as the whalebone whale, but much longer, measuring 6 or 7 feet. This fish is somewhat longer than the other, but not so bulky; much swifter and very furious when struck, and held with great difficulty; their oil is not near so much as that of the right whale, and the bone of little profit, being short and knobby. The belly of this whale is white.

The bunch or humpback whale, is distinguished from the right whale, by having a bunch in the place of the fin in the finback. This bunch is as large as a man's head, and a foot high, shaped like a plug pointing backwards. The bone of this whale is worth but little, though somewhat better than the finback's. His fins are sometimes 18 feet long, and very white; his oil much as that of the finback. Both the finbacks and humpbacks are shaped in reeves lengthwise, from head to tail, on their bellies and their sides, as far as their fins, which are about half way up their sides.

The sperma ceti whale is a fish much of the same dimension with the other, but of a greyish colour; whereas the others are black; he has a bunch on his back like the humpback, but then he is distinguished by not having any whalebone in the mouth; instead of which, there are rows of fine ivory teeth in each jaw, about 5 or 6 inches long. They are a more gentle fish than the other whales, and seldom fight with their tails; but when struck, usually turn upon their backs and fight with their mouths. The oil of the body of this fish is much clearer and sweeter than that of the other whales.

The sperma ceti oil, so called, lies in a great trunk about 4 or 5 feet deep, and 10 or 12 feet long, near the whole depth, breadth, and length, of the head, in the place of the brains, and seems to be the same;* it is disposed in several membranous cells, and covered not with a bone, but a thick grisly substance below the skin, through which they dig a hole, and lade out the clear oil. Not but that the head, and other glandulous parts of this fish, will make the sperma ceti oil; but the best, and that which is prepared by nature, is in the aforesaid trunk, which will afford from 10 to 20 barrels. Besides the sperma ceti oil, this fish will yield from 20 to 50 barrels of common oil.

Whales generate much like neat cattle, and therefore they are termed bull, cow, and calf. They bring forth only one at a time, and that every other year. When the cow takes bull, she throws herself upon her back, sinking her tail, and so the bull slides up, she then clasps him with her fins. A whale's penis is 6 feet long, and at the root is 7 or 8 inches diameter, thence tapering away till it comes to about an inch diameter; his testicles would fill half a barrel, but

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* The sperma ceti is *not* "in place of the brains," nor "the same." There is a cerebral substance distinct from it.

his genitals are not open or visible, like those of the true bull. The calf, or young whale, has been found perfectly formed in the cow, when not above 17 inches long, and white; yet, when brought forth, it is usually 20 feet, and of a black colour; it is supposed they go with young about 9 or 10 months, when they are very fat, especially when they bring forth. When the female suckles her young, she turns herself almost upon her back, on the surface of the water; she has two teats of 6 or 8 inches long, and 10 or 12 inches round. The milk is white, like that of a cow; and on opening a young sucking whale, the milk is found curdled in its bag, just like that in a calf.

Their care of their young is very remarkable; they not only carry them on their tails, and suckle them, but often rise with them for the benefit of the air; and however they may be chased or wounded, yet as long as they have sense, and perceive life in their young, they will never leave them, nor will they then strike with their tail; and if, in their running, the young one loses its hold and drops off, the dam comes about, and passing underneath, takes it on again. And therefore care is taken by those who kill these mate fish, as they are called, only to fasten the calf, but not to kill it, till they have first secured the cow. For as soon as ever the calf is dead, the cow perceives it, and grows so violent, that there is no managing her.

Whales are very gregarious, being sometimes found 100 in a scull, and are great travellers. In the fall of the year, the right or whalebone whales go westward, and in the spring eastward. But the several kinds of whales do not mix with each other, but each sort by themselves.

Their way of breathing is by two spout holes in the top of the head. The sperma ceti has but one, and that on the left side of the head. Once in a quarter of an hour, when not disturbed, they are observed to rise and blow, spouting out water and air, and to draw in fresh air; but when pursued, they will sometimes keep under water half an hour or more; though when a cow has her calf on her tail, she rises much oftener, for the young one to breathe, without breathing herself. Out of their breathing holes they spout great quantities of blood, when they have received their death wound.

For the first year they all suck the dam. After they are weaned, the right whales, it is supposed, live upon some ouzy matter, which they suck up from the bottom of the sea. The triers, that open them when dead, say, that they never observed any grass, fish, or any other sort of food in the right or whalebone whale, but only a greyish soft clay, which the people call bole ammoniac; and yet an experienced whaleman says, that he has seen this whale, in still weather, skimming on the surface of the water, to take in a sort of reddish spawn or bret, as some call it, that sometimes lies on the top of the water, for a mile together. Though the body of this whale is so very bulky, and so exceedingly fat; yet when cut open, they are seldom found to have much more draught than that of an ox, and they dung much as neat cattle do. Their swallow is not much wider than an ox's; but the finback whale has a larger swallow; for he lives upon the smaller fish, as mackarel, herring, &c. great sculls of which they run through, and with a short turn cause an eddy or whirlpool, by the force of which the small fish are brought into a cluster, so that the whale with open mouth will take in some hundreds of them at a time. The sperma ceti whale, besides other fish, feeds much on a small fish that has a bill, which the fishermen call squid fish. The small pieces of these squid bills are plainly to be discerned in the ambergris, and may be picked out of it; they appear glazy, and like little pieces of broken shells.

Mr. Harris, in his Bibliotheca Navigantium, &c. has given a very particular account of the method of taking whales at Greenland; but our way in New England differs very much from that. The boats our whalemen use in going from the shore after the whale, are made of cedar clapboards, and so very light, that two men can conveniently carry them, though they are 20 feet long, and carry 6 men, viz. the harponeer in the fore part of the boat, 4 oarmen, and the steersman. These boats run very swift, and by reason of their lightness can be easily brought on and off, and so kept out of danger. The whale is sometimes killed with a single stroke, and yet at other times she will hold the whalemen in play near half a day together, with their lances; and sometimes they will escape after having been lanced and having spouted blood, with irons in them, and drags fastened to them, which are thick boards about 14 inches square. Our people formerly used to kill the whale near the shore, but now they go off to sea in sloops and whale-boats, in the months of May, June, and July, between Cape Cod and Bermudas; where they lie by in the night, and sail about in the day, and seldom miss of them; they bring home the blubber in their sloops. The true season for taking the right or whalebone whale is from the beginning of June to the end of May; for the sperma ceti whale, from the beginning of June to the end of August. And it has been observed by the fishermen, that when a sperma ceti whale is struck, he usually, if not always, throws the excrements out of the anus.

The prodigious strength of this animal lies principally in the tail, that being both their offensive and defensive weapon; many instances are related. A boat has been cut down from top to bottom with the tail of a whale, as if cut with a saw, the clap-boards scarcely splintered, though the gunnel on the top is of tough wood. Another has had the stem, or stern-post, of about 3 inches through, and of the toughest wood that can be found, into which the ends of the cedar clap-boards are nailed, cut off smooth above the cuddee, without so much as shattering the boat, or drawing the nails of the clap-boards. An oar has been cut off by a stroke upwards, and yet not so much as lifted up out of the thole-pin. One person had an oar cut off, while in his hand, and yet never felt any jarring.

A few years since, one of the finback whales came into a harbour near Cape Cod, and towed away a sloop of near 40 ton, out of the harbour into the sea. This accident happened thus: it is thought the whale was rubbing herself on the fluke of the anchor, or going near the bottom, got the fluke into her nisket, or the orifice of the uterus, and, finding herself caught, tore away with such violence, and towed the sloop out of the harbour, as fast as if she had been under sail with a good gale of wind, to the astonishment of the people on shore; for there was nobody on board. When the whale came into deep water, she went under, and had like to have carried the sloop with her, but the cable gave way, so the boats that were out after her recovered it. This whale was found dead some days after on that shore, with the anchor sticking in her belly.

The fish that prey upon the whales, and often kill the young ones, are by the whalemen called killers. These killers are from 20 to 30 feet long, and have teeth in both jaws that lock into each other. They have a fin near the middle of their back 4 or 5 feet long. They go in company by dozens, and set upon a young whale, and will bait him like so many bull dogs; some will lay hold of his tail to keep him from threshing, while others lay hold of his head, and bite and thresh him, till the poor creature, being thus heated, lolls out his tongue, and then some of the killers catch hold of his lips, and if possible of his tongue; and after they have killed him, they chiefly feed upon the tongue and head; but when he begins to putrefy they leave him. This killer is doubtless the orca, that Dr. Frangius describes in his Treatise of Animals. His words are these: "When an orca pursues a whale, the latter makes a terrible bellowing, like a bull when bitten by a dog." These killers are of such strength, that when several boats together have been towing a dead whale, one of them has come and fastened his teeth in her, and carried her away down to the bottom in an instant. And sometimes again, they have bitten out a piece of blubber of about 2 feet square, which is of that toughness, that an iron with little barbs being struck into it, will hold it till it draws the boat under water. The killers are sometimes taken, and make good oil, but have no whalebone. The carcases of whales in the sea serve for food for gulls, and other sea fowl, as well as sharks.

Many and various have been the opinions, even of the learned world, as to the origin and nature of ambergris. Some have reckoned it a bitumen, and to issue from the earth; others, that it was produced from some insect, as honey, silk, &c. Mr. Boyle, in a former Transaction, communicated an account of ambergris, from a Dutch merchant, who first denies it to be the scum or excrement of a whale, and then gives it, as his opinion, that it is a fat gum that issues from the root of a tree, and that you may raise it in quantities by planting those trees by the shore; and so the stream will cast it up to great advantage. But it is now found, that this occultum naturæ is an animal production, and bred in the body of the sperma ceti whale, analogous to what is found in some land animals, as the musk hog or Taiacu, the musk deer, the bezoar sheep, and some amphibious animals, as the musquash, &c. which have their valuable scent in a particular cystis or bag. Probably what first gave occasion to the notion of ambergris being the production of the whale, was because it was found in considerable quantities on the shores of the summer islands, and among the Bahamas, where the dead whales are frequently wrecked and broken up with the sea, and the ambergris found floating, or on the shore. But here again the ingenious, till very lately, were at a loss, and divided in opinion; for though they agreed that it came from the whale, yet some took it to be the true and proper semen, being found only in the bull, at the root of the penis, near the testicles; others again took it to be the ordure or excrement of the whale.

The best and most exact account of ambergris, has been very lately received from one Mr. Atkins, an inhabitant at Boston in New England, who used the whale fishery, for 10 or 12 years together, and was one of the first that went out a fishing for the sperma ceti whales, about the year 1720, and then began to discover the ambergris. His account, which agrees with that of several others, is to the following purport.

The ambergris is found only in the sperma ceti whales, and consists of balls or globular bodies, of various sizes, from about 3 to 12 inches diameter, and will weigh from a pound and a half to 22 pounds, lying loose in a large oval bag or bladder, of 3 or 4 feet long, and 2 or 3 feet deep and wide, almost in the form of an ox's bladder, only the ends more acute, or like a blacksmith's long bellows, with a spout running tapering into and through the length of the penis, with a duct, or canal, opening into the other end of the bag, and coming from towards the kidneys; this bag lies just over the testicles, which are above a foot long, and is placed lengthwise at the root of the penis, about 4 or 5 feet below the navel, and 3 or 4 above the anus. This bag or bladder is almost full of a deep orange coloured liquor, not quite so thick as oil, and smelling strong, or rather stronger of the same scent as the balls of ambergris, which float and swim loose in it. The inside of the bag is very deeply tinged with the same colour as the liquor; which may also be found in the canal of the penis; the balls seem to be pretty hard while the whale is alive, inasmuch as there are many times found, on opening the bag, large concave shelves, of the same substance and consistence, that have scaled off from them, and the balls themselves seem to be composed of several distinct coats inclosing each other, something like the coats of an onion.

Mr. Atkins never found above 4 of these balls in a bag; and in the bag where he found one that weighed 21 pounds, which was the largest he ever saw, there was no other in the bag besides that one.

He further says, That to one sperma ceti whale that has any of these balls, there are two that have only the deep orange-coloured liquor in their bags. This remark confirms what another whale-man said, that the ambergris was found only in such sperma ceti whales as are old and well grown. It is the general opinion of the whale-men, that the ambergris is produced only by the male or the bull sperma ceti whale. As to this particular, Mr. Atkins says, he never saw, nor certainly heard of a sperma ceti female taken in his life, the cows of that species of whales being much more timorous than the males, and almost impossible to be come at, unless when haply found asleep on the water, or detained by their calves. This is certain, the boats can never come near them when awake, they are so very shy and fearful.

Mr. Atkins's method of getting the ambergris out of the whale was thus: after the fish is killed, he turns the belly upwards, and fixes a tackle to the penis, then cuts a hole round the root of the penis, through the rim of the belly, till he comes to the entrails, and then searching for the duct or canal at the further end of the bag, he ties it pretty near to the bag, and cuts the duct off beyond it, on which he draws forth the penis by the tail, when the ambergris bag entirely follows it, and comes clean and whole out of the belly.

The Rev. Mr. Price of Boston, supposes the bag to be the urinary bladder, and the ambergris ball to be a certain concretion, formed out of the greasy odoriferous substance of the liquor contained within it.

An Enquiry into a Discovery, said to have been made by Sig. Valsalva of Bologna, of an Excretory Duct from the Glandula Renalis to the Epididymis. By Mr. John Ranby, Surgeon, F. R. S. N° 387, p. 270.

The late Sig. Valsalva, having some time ago discovered a vessel, which he took to be an excretory duct going from the glandula renalis, to the epididymis in men, and the ovarium in women; an account whereof was inserted in the Phil. Trans. N° 385, Mr. Ranby procured a human body, in order to search for it, which he did with all possible diligence. He was not so happy as to discover any duct of this kind; but having injected the aorta, he found the arteries going to the glandulæ renales, disposed as represented in fig. 16, pl. 1. Now whether that branch of the artery, which from the glandulæ renales goes down on both sides towards the testicles, without supplying any of the neighbouring parts, might not have been mistaken by the learned anatomist for an excretory duct, all arteries being generally in dead bodies free from blood, and of a whitish colour, Mr. R. will not at present determine, for want of further experiments.

In the figure, AA represents the right kidney; BB the left kidney; CC the descending trunk of the aorta; D the right emulgent artery; E the left emulgent artery; F the right glandula renalis; GG the right spermatic artery; H the left glandula renalis; II the left spermatic artery; KK the ureters.

aa A small artery arising out of the descending trunk of the aorta, a little above the right emulgent artery: it sends two branches bb upwards to supply the right glandula renalis: a third branch cc goes downwards towards the right spermatic artery GG, and then farther in company with the same to the right testicle.

dd A small artery arising out of the left emulgent artery, near the descending trunk of the aorta, and going directly upwards to the left glandula renalis H.

ee A small artery arising out of the descending trunk of the aorta, a little below the left emulgent artery. It divides into 2 branches; one, ff, goes upwards between the emulgent artery and vein, to the left glandula renalis; the other gg downwards, towards the left spermatic artery, and in company with the same to the left testicle.

An Account of a Book entitled, Prodromus Crystallographiæ. De Crystallis improprie sic dictis Commentarium. A Mauritio Antonio Cappeler, M. D. et Centumviro Lucernensi. Lucernæ 1723, 4o. By J. G. Scheuchzer, M. D. Coll. Med. Lond. Lic. R. S. S. N° 387, p. 272.

A Catalogue of the 50 Plants, from Chelsea Garden, presented to the Royal Society, for the Year 1724, by the Company of Apothecaries, pursuant to the Direction of Sir Hans Sloane Bart. Pr. Coll. Med. S. P. V. Pr. By Mr. Isaac Rand, Apothecary, F.R.S. N° 388, p. 305.

Observations on the Height of the Barometer, at different Elevations above the Surface of the Earth. By Dr. Nettleton. N° 388, p. 308.

Having measured a hill of a considerable height, on a clear day, and observed the mercury at the bottom and at the top, it was found according to that estimation, that about 90 feet, or upwards, were required to make the mercury fall $\frac{1}{10}$ of an inch; but coming afterwards to repeat the experiment on a cloudy day, when the air was somewhat gross and hazy, the small angles were so much augmented by refraction, as to make the hill much higher than before, though they were taken carefully with very good instruments, both at that time and before.

The Doctor afterwards frequently observed at home, by pointing the quadrant to the tops of some of the neighbouring mountains, that they would appear higher in the morning before sun-rise, and also late in the evening, than at noon, in a clear day, by several minutes: particularly, one morning in December, when the vapours lay condensed in the vallies, and the air above was very pure, the top of a mountain, at some distance, appeared more elevated, by above 30′, than it had done in the beginning of September about noon, on a very clear day. From whence it appears, that the refraction is at some times greater than at others; but probably it is always very considerable; and as there is no certain rule to make allowance for it, it seems likely, that all observations made on very high hills, especially when viewed at a distance, and under small angles, as they commonly are, are uncertain, and scarcely to be depended on, generally erring in making the heights greater than they really are.

The Doctor then proceeded to observe, as near as possible, the alteration of the mercury in some smaller perpendicular elevations, which we could measure with a line, and also on the tops of some hills of a moderate height, whose altitude could be observed most commodiously, and, by taking the angles large, avoid the danger of any considerable refraction.

At the bottom of the tower of Halifax church, the mercury stood at 29.78 inch. At the top it subsided to 29.66. The height of the place, where the observation was made, was found to be 102 feet.

At the bottom of a coal-mine, near this place, the mercury stood at 29.48. At the top, it fell to 29.32. The depth of the mine, being measured, was found to be 140 feet.

At the bottom of another mine, the mercury was observed to stand at 29.50. At the top it fell to 29.23. The depth of this mine was 236 feet.

At the foot of a small hill, whose height could be measured very exactly, the mercury stood at 29.81. At the top it fell to 29.45. The height of the hill was 312 feet.

At the bottom of Halifax hill, commonly called the bank, the mercury was observed to stand at 30.00. At the top, it fell to 29.41. The height of this hill was found to be 507 feet.

Mathematicians demonstrate, that the density of the air decreases in a geometrical progression, as the elevation increases in an arithmetical one; and consequently, that the logarithms of the densities are as the elevations reciprocally. But the weight of the air being as its density, and the height of the mercury in the barometer being always proportional to the air's weight, it follows, that the logarithms of the heights of the mercury are reciprocally as the elevations. Whence, having found by observation, what elevation is required to make the mercury stand at any given height, it will be easy to determine how much is requisite to reduce it to any other height proposed. If we make 30 inches the standard height of the mercury, equal to unity, and suppose an elevation of 85 feet be required to make it fall $\frac{1}{10}$ of an inch from that height, as by these observations it is very nearly; then as the logarithm of $\frac{30.0}{29.9}$ is to 85, so is the logarithm of $\frac{30.0}{29.5}$ to the number of feet required to make it fall half an inch; and so of the rest. When the mercury stands above 30 inches, the numbers will be negative, and show the spaces descending; by which method were computed the following tables.

The latter, which contains the differences of the numbers in the former, was of very great use, when in these experiments the mercury stood at any other height in the tube, besides 30 inches, and fell any number of tenths, or parts of a tenth, by adding the numbers answering thereto, or proportionable parts of them, to find the elevation required in the table, to make the mercury fall so much, and thereby readily to compare the heights found by observation with them. And though some small errors, in the observations, make them vary a little from each other, yet in the main they agree as near as possible with the numbers of the table; as did also several other experiments; which makes it probable those numbers are not far from the truth.

That the air is colder, as well as more light and rare, in places that are situated high, than it is in the vallies and low grounds, is generally known; and in order to learn how much it is so, Dr. N. got a friend, who lives higher than himself, to observe the portable barometer and thermometer, at his house, for some days, being placed as near as possible in the same circumstances with his own; and his barometer was found to stand at a medium, for 20 days, $\frac{3}{10}$

lower than the Doctor's, and the thermometer $3\frac{1}{5}$ degrees lower; allowing for the difference of the instruments, which had been observed before.

At another place the barometer, at a medium for 14 days, stood lower by 4.46, and the thermometer was lower by $4\frac{2}{5}$ degrees. At another place, which was very high upon the moors, the barometer, at a medium for 10 days, stood lower by 0.65 and the thermometer fell 7°.

A Table showing the Number of Feet ascending, required to make the Mercury fall to any given Height in the Tube, from 30 to 26 inches. As also the Number of Feet descending, required to make the Mercury rise, from 30 to 31 Inches.

Inc.	Feet.	Inc.	Feet.
31.0	834.79	27.9	1847.55
30.9	752.53	27.8	1938.97
30.8	670.01	27.7	2030.72
30.7	587.21	27.6	2122.80
30.6	504.15	27.5	2215.21
30.5	420.82	27.4	2307.95
30.4	337.21	27.3	2401.02
30.3	253.32	27.2	2494.44
30.2	169.10	27.1	2588.20
30.1	84.72	27.0	2682.33
30.0	00.00	26.9	2776.80
29.9	85.00	26.8	2871.62
29.8	170.29	26.7	2966.79
29.7	255.87	26.6	3062.32
29.6	341.73	26.5	3158.21
29.5	427.89	26.4	3254.46
29.4	514 34	26.3	3351.07
29.3	601.08	26.2	3448.05
29 2	688.11	26.1	3545.41
29.1	775.44	26.0	3643.14
29.0	863.08		
28.9	951.01		
28.8	1039.25		
28.7	1127.80		
28.6	1216.66		
28.5	1305.83		
28.4	1395.32		
28.3	1485.13		
28.2	1575.26		
28.1	1665.70		
28.0	1756.47		

A Table showing the Number of Feet required to make the Mercury fall $\frac{1}{10}$ of an Inch from any given Height in the Tube, from 31 to 26 Inches.

Inc.	Feet.	Inc.	Feet.
31.0	82.26	27.9	91.42
30.9	82.53	27.8	91.75
30.8	82.79	27.7	92.08
30.7	83 06	27.6	92.41
30.6	83.33	27.5	92.74
30.5	83 61	27.4	93.07
30.4	83.89	27.3	93.41
30.3	84.16	27.2	93.76
30.2	84.44	27.1	94.12
30.1	84.72	27.0	94.47
30.0	85.00	26.9	94.82
29.9	85.29	26.8	95.17
29.8	85.58	26.7	95.53
29.7	85.86	26.6	95.89
29.6	86.16	26 5	96.25
29.5	86.45	26.4	96.61
29.4	86.74	26.3	96.98
29.3	87.03	26.2	97.36
29.2	87.33	26.1	97.73
29.1	87.63	26.0	98.10
29.0	87.93		
28 9	88.24		
28.8	88.55		
28.7	88.86		
28.6	89.17		
28.5	89.49		
28.4	89.81		
28.3	90.13		
28.2	90.45		
28.1	90.76		
28.0	91.09		

A Barometrical Experiment; by M. And. Celsius. N° 388, p. 313. *Translated from the Latin.*

For observing the variation of the column of mercury in the barometer, according to different heights in the atmosphere, the deep mines in Swedlen may be reckoned peculiarly adapted. For not only their depth may be measured with great accuracy, but also the whole observation performed in a short time: an advantage often wanting in making the like experiments on high mountains. If therefore a great many experiments were made in different mines, no doubt but the true progression, by which the density of the air decreases, would at length be discovered.

Aug. 28, 1724, M. Celsius made the following experiment in the Salan silver mine, about 7 miles to the west of Upsal. At the entry to the queen Christina's shaft, he observed the height of the mercury at 30.38 inches, or the $\frac{3038}{1200}$ of a Swedish foot; he was then let down with the barometer in a vessel by a rope, to the depth of 636 feet, where he observed the mercury ascend to 30.98 inches: from thence being drawn up again to the mouth of the shaft, he observed the column of mercury at the same height as before, viz. 30.38 inches. So that the mercury raised to the height of 636 feet in the air, falls 6 lines or $\frac{6}{120}$ parts of a foot; and consequently, if the air were supposed of equal density every where, the variation of one line in the column of mercury would answer to 106 feet perpendicular height. During the time of the observation, there was a little rain and wind; yet no sensible alteration could be observed at the same time in the column of mercury in another barometer, fixed to a wall above the mine.

Next day, the sky being serene and calm, the mercury stood at 30.36 inches at the foot of the church of Sale, not far from the mine; but going up 145 feet high in the tower of the said church, he found the mercury at 30.23 inches: so that the height of 111 feet and $\frac{7}{13}$ parts answers to the descent of one line in the barometer.

That this observation may be duly compared with the experiments of this kind, made by others, it is to be noted, that the ratio between the Swedish and Paris foot royal is nearly that of 1000 to 1096, or 125 to 137, as Mr. Celsius accurately observed by comparing them together.

Remarks on the Observations made on a Chronological Index of Sir Isaac Newton, translated into French by the Observator, and published at Paris. By Sir Isaac Newton. N° 389, p. 315.

Nov. 11, 1725, a small tract in print was delivered to me, as a present, from Mr. Wm. Cavelier, jun. a bookseller at Paris, a person unknown to me, entitled, Abregé de Chronologie de M. le Chevalier Newton, fait per lui même, et traduit sur le Manuscript Anglois. And the bookseller has prefixed an advertisement, in which he endeavours to defend himself for printing it without my leave, saying, that he had written three letters to me for my leave, and in the third had told me, that he would take my silence for a consent; and that he had also charged one of his friends in London to speak to me, and procure my express answer; and that having long expected my answer, he thought that he might take my silence for a sort of consent, and so procured a privilege, and printed it, and then received my answer from his friend, which was as follows:

"I remember that I wrote a Chronological Index for a particular friend, on condition that it should not be communicated. As I have not seen the manuscript which you have under my name, I know not whether it be the same. That which I wrote was not at all done with design to publish it. I intend not to meddle with that which hath been given you under my name, nor to give any consent to the publishing of it. "I am,

London, May 27, 1725. "Your very humble servant,

St. Vet. "ISAAC NEWTON."

The privilege was granted May 21, and registered May 25, Old Style, my letter was dated May 27, and the Chronological Index, or Abridgment, as he calls it, was printed before the arrival of my letter, and kept ever since to be published at a convenient time. The bookseller knew that I had not seen the translation of the Abridgment, and without seeing it could not in reason give my consent to the impression. He knew that the translator was unknown to me, and was against me; and therefore he knew that it was not fit that I should give my consent, nor be asked to do it. He knew that the translator had written a Confutation of the paper translated, and that this Confutation, under the title of Observations, was to be printed at the end of it, and he told me nothing of all this, nor so much as the name of the Observator, and yet asked my consent to the publishing; as if any man could be so foolish as to consent to the publishing of an unseen translation of his papers, made by an unknown person, with a Confutation annexed, and unanswered at their first appearance in public.

After the recital of my letter, he adds, that the author of the translation, and of the observations upon it, pretends to have an entire certainty that this Index, or Abridgment of Chronology, is the same with the writing owned by me in my letter, and is persuaded that the manuscript, which has been communicated to him, has been copied from that of this friend, that is, from that of the particular friend abovementioned in my letter. And therefore the manuscript, which has been communicated to him, is that of Abbè Conti, a noble Venetian now at Paris. He, being about 7 years ago in England, gave me notice, that the friend abovementioned desired to speak with me. And this friend then desired a copy of what I had written about Chronology. I replied that it was imperfect and confused, but in a few days I could draw up an abstract thereof, if it might be kept secret. And some time after I had done this, and presented it, this friend desired that Signor Conti might have a copy of it. He was the only person who had a copy, and he knew that it was a secret, and that it was at the desire of this friend, and by my leave, that he had a copy, and he kept it secret, while he staid in England; and yet, without either this friend's leave or mine, he dispersed copies of it in France, and got an Antiquary to translate it into French, and to confute it; and the Antiquary has got a printer to print the translation and the confutation; and the printer has endeavoured to get my leave to print the translation, without sending me a copy thereof to be perused, or telling me the name of the translator, or letting me know that his design was to print it with a confutation unanswered and unknown to me.

The translator, near the end of his observations, p. 90, says, I believe that I have said enough concerning the Epocha of the Argonauts, and the length of generations, to make people cautious about the rest. For these are the two foundations of all this new system of Chronology. What he says, concerning the Epocha of the Argonauts, is founded on the supposition that I place the equinox in the time of the Argonautic expedition, 15° from the first star of Aries, p. 75, 79. I place it in the middle of the constellation, and the middle is not 15° from the first star of Aries. The Observator grants that the constellations were formed by Chiron (p. 70, 71, 79), and that the solstices and equinoxes were then in the middle of the constellations (p. 65, 69, 75), and that Eudoxus, in his Enoptron or Speculum, cited by Hipparchus, followed this opinion, p. 62, 63, 65, 69, 79. And Hipparchus (see Hipparchus published by Petavius, Vol. 3, p. 116, 117, 119, 120) names the stars, through which the colures passed in this old sphere, according to Eudoxus, and says expressly that Eudoxus drew one of these colures through the middle of Cancer and the middle of Capricorn, and the other through the middle of Chelæ and the back of Aries. And the colures, passing through the back of Aries, passes through the middle of Aries, and is but 8° from the first star of Aries. I follow Eudoxus, and, by doing so, place the equinoctial colure about 7° 36′ from the first star of Aries. But the Observator represents, that I place it 15° from the first star of Aries, and thence deduces that I should have made the Argonautic expedition 532 years earlier than I do. Let him rectify his mistake, and the Argonautic expedition will be where I place it.

As for the length of generations, the Observator says, that I reckon them one with another at 18 or 20 years a piece (p. 52, 55), which is another mistake. I agree with the antients in reckoning 3 generations, at about 100 years. But the reigns of kings I do not equal to generations, as the antient Greeks and Egyptians did; but I reckon them only at about 18 or 20 years a piece one with another, when 10 or 12 kings, or more, are taken in continual succession. So the first 24 kings of France (Pharamond, &c.) reigned 458 years, which is one with another 19 years a piece. The next 24 kings of France (Ludovicus Balbus, &c.) reigned 451 years, which is one with another 18¾ years a piece. The next 15 kings (Philippus Valesius, &c.) reigned 315 years, which is one with another 21 years a piece. And all the 63 kings of France reigned 1224 years, which is 19½ years a piece. And, if the long reign of Lewis XIV. be added, the 64 kings of France will reign but 20 years a piece. And they, that examine the matter, will find it so in other kingdoms: and I shorten the duration of the antient kingdoms of Greece, in the same proportion that I shorten the reigns of their kings, and thereby place the Argonautic expedition about 44 years, and the taking of Troy about 76 years, after the death of Solomon, and find Sesostris contemporary to Sesac.

So then the Observator has mistaken my meaning, in the two main arguments on which the whole is founded, and has undertaken to translate and to confute a paper which he did not understand, and been zealous to print it without my consent; though he thought it good for nothing, but to get himself a little credit, by translating it to be confuted, and confuting his own translation.

The Observator says, that I suppose that the Egyptians began, about 900 years before Christ, to form their religion, and deify men for their inventing of arts, notwithstanding that it appears by the scriptures, that their idolatry and arts were as old as the days of Moses and Jacob, p. 82, 83. But he is again mistaken. I deny not that the kingdom of the Lower Egypt, called Mizraim, had a religion of their own, till they were invaded and subdued by the shepherds, who were of another religion: but I say that, when the Thebans expelled the shepherds, they set up the worship of their own kings and princes. I say also, that arts were brought into Europe principally by the Phœnicians and Curetes, in the time of Cadmus and David, about 1041 years before Christ; and do not deny that they were in Phœnicia, Egypt, and Idumea, before they came into Europe.

The Observator says also, that, 884 years before Christ, I place the beginning of the canicular cycle of the Egyptians on the vernal equinox, though that cycle never begins in spring, p. 84, 85. But he is again mistaken. I meddle not with that cycle, but speak of the Egyptian year of 365 days.

The Observator represents, that I have a great work to come out: but I never told him so. When I lived at Cambridge, I used sometimes to refresh myself with History and Chronology for a while, when I was weary with other studies: but I never told him, that I was preparing a work of this kind for the press.

Abbè Conti came into England in spring 1715, and, while he staid in England, he pretended to be my friend, but assisted Mr. Leibnitz in engaging me in new disputes, and has since acted in the same manner in France. The part he acted here may be understood by the character given of him in the Acta Eruditorum for the year 1721, p. 90, where the editor, excusing himself from repeating some disputes which had been published in those Acta, subjoins: "Let it therefore suffice to say, that when the Abbè Conti, a noble Venetian, (of whom M. Leibnitz acknowledges M. Herman gave a good character) came over from France into England, he undertook to be mediator in the disputes between Sir I. Newton and M. Leibnitz; and took the care of transmitting their letters to each other." And how Mr. Leibnitz, by this mediation, endeavoured to engage me, against my will, in new disputes, about occult qualities, universal gravity, the sensorium of God, space, time, vacuum, atoms, the perfection of the world, supramundane intelligence, and mathematical problems, is mentioned in the preface to the second edition of the Commercium Epistolicum. And what he has been doing in Italy, may be understood by the disputes raised there by one of his friends, who denies many of my optical experiments, though they have been all tried in France with success. But I hope that these things, and the perpetual motion, will be the last efforts of this kind.

On Camphor. By Mr. Caspar Neuman, Regius Professor of Chemistry, at Berlin, and F. R. S. An Abstract from the Latin.* N° 389, p. 321.

In this communication the author states, that he had obtained from the dis-

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* Caspar Neumann was a celebrated German chemist, and member of various learned societies. He was born in 1682. He travelled into Holland, England, France, and Italy; and on his return in 1724, he was appointed professor of chemistry in the Royal Coll. of Physic and Surgery at Berlin; where he delivered a course of chemical lectures, annually. He died in 1737. Besides the above and other papers inserted in the Phil. Trans. he published at different times select parts of his lectures; which were reprinted after his death with large additions, in the German language. This

tilled oil of common thyme, by letting the oil stand at rest in the bottle into which it had been received, a quantity of camphor, differing in no respects from the genuine oriental camphor, except in its odour. This camphor had shot into crystals of various sizes, some as large as filberts, and mostly of a cubic form, like sugar candy, at the bottom of the bottle. This substance (he observes) is very different from that obtained by Hoffman, (Dissert. de Camph. et Obs. Physico Chym.) from roses and the lignum aloës, as well as from that which Mr. Boyle (Treatise on Solidity and Fluidity) obtained from the ol. anisi. The product in these instances is merely unctuous, or at most only of the consistence of butter, but not in the form of solid, friable crystals, as in the case of the oil of thyme.—In the further part of this paper, the author shows that camphor is a substance differing in its properties from oils, resins, volatile salts and gums; that it is in fact a sui generis substance; and that it is obtainable from thyme and perhaps from some other vegetables, besides the so called camphor-tree.

Observations of the Dipping Needle, made at London, in 1723. *By Mr. George Graham, F. R. S.* N° 389, p. 332.

About the time Mr. G. was observing the variation of the horizontal needle, he made also some experiments with the dipping needle, to try if the dip and vibrations were constant and regular. The needle he made for this purpose, was $12\frac{1}{10}$ inches long, half an inch broad in the middle, but not above $\frac{1}{10}$ near the ends; the ends themselves being filed to fine edges; and in thickness it was about $\frac{1}{3}$ of a tenth. The ends of the axis, on which the needle turned, were very smooth, and not thicker than was necessary for the support of the needle, which weighed 9^{dwts} 21 grains, or about half an oz. Troy. The ends of the axis were placed on the edges of two thin plates of steel, that were hard and well polished, and parallel to the horizon, that the needle, when vibrating, might roll, and not slide on the edges of the plates, to avoid the friction of moving in holes. A brass semicircle was provided, and from the lowest point

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posthumous edition, which contains the whole of the author's lectures, consists of 3 vols. 4to. and was printed at Züllichau 1749. An English translation of his chemical works, abridged and methodized by Lewis, was published first in 1 vol. 4to. 1760, and afterwards in 2 vols. 8vo. 1773. They have also been translated into French and other Europæan languages. These lectures exhibit a better view than had before been given by any other writer, of all that was then known in chemistry. They moreover contain many new analyses made by the author himself of certain medicinal and alimentary substances; such as amber, opium, castor, tea, coffee, &c. It has been before stated in a note at p. 554 of the first vol. of these Abridgments, that he examined with much attention the acid juice of ants, and showed (what has been confirmed by subsequent experiments) that it coincides in its leading properties with the acetic acid.

graduated each way, and a few of the degrees, about that part of it which answered to the dip, were divided into 6 equal parts. By means of screws, the semicircle could be brought to a due situation; and by two spirit levels, placed at right angles to each other, any change of situation was easily perceived, and by the screws it could be readily restored to its former position; all was inclosed with glass to secure the needle from being disturbed by the motion of the air. He notices the great difficulty of poising the needle so exactly, before it is touched with the loadstone, as to take any position indifferently: for, when it is pretty near the truth, it is extremely troublesome to place it at rest in the position desired, in order to try which way it is inclined to move. It cannot be done in the open air; for the least motion of it will disturb the needle; and when it is shut up, it is no easy matter to settle it in the place intended. And that there will be a sensible difference of the dip, on shifting the sides of the needle, whatever pains be taken to prevent it, fully appears from the following experiments.

Exper. 1. *March* 20, 1722. Mr. G. touched both sides of that end of the needle, which intends to point south, on the north-pole of a small terrella; after which he caused it to vibrate in an arch of 10 degrees, and counted the time by a pendulum clock, showing seconds, till the needle had performed 50 vibrations.

It performed the first 25 vibrations in 2^{m} 58^{s}; the next 25 vibrations in 2^{m} 27^{s}; the 50 in 5^{m} 25^{s}; which gives for each vibration at a medium $6^{s}.5$. The needle dipped 73° 15′.

Exper. 2. He then shifted the needle, so as that side which before respected the east, was now turned west, and causing it to vibrate in the same arch, as before,

It performed the first 25 vibrations in 2^{m} 49^{s}; the next 25 in 2^{m} 39^{s}; the 50 vibrations in 5^{m} 28^{s}; that is, each vibration in $6^{s}.56$. The dip was 73° 50′.

Exper. 3. He now touched the same end of the needle, a second time, on both sides, on the same stone, and suffering it to vibrate, as before,

It performed 25 vibrations in 2^{m} 49^{s}; that is, 1 vibration in $6^{s}.76$. The dip being 73° 20.

Exper. 4. The needle was now shifted, and stood as in the 2d experiment.

It performed 25 vibrations in 2^{m} 41^{s}; that is, 1 vibration in $6^{s}.44$. The dip being 73° 45′.

Exper. 5. The same end of the needle being now touched twice on each side, with the loadstone presented by Lord Paisley to the Royal Society, in the armour,

It performed the first 25 vibrations in 1^{m} 58^{s}; the next 25 in 1^{m} 46^{s}; the 50 vibrations in 3^{m} 44^{s}; that is, each vibration in $4^{s}.48$. The dip being 73° 55′.

Exper. 6. The needle being turned, and standing as in the 2d and 4th experiments,

It performed the first 25 vibrations in $2^m\ 00^s$; the next 25 in $1^m\ 57^s$; the 50 vibrations in $3^m\ 57^s$; that is, each vibration in $4^s.74$. The dip being 74° 10′.

Exper. 7. He now touched the needle at both ends with the same stone, with which it was touched in the 5th experiment, after which

It performed the first 25 vibrations in $1^m\ 35^s$; the next 25 in $1^m\ 34^s$; the 50 in $3^m\ 9^s$; that is, each vibration in $3^s.78$. The dip was 74° 20′. The dip repeated with the needle taken off and replaced 74° 20′+.

Exper. 8. On shifting the needle, it performed

The first 25 vibrations in $1^m\ 33^s$; the next 25 in $1^m\ 34^s$; the 50 in $3^m\ 7^s$. The dip being 74° 25′. The dip repeated 74° 30′—.

N. B. The needle had the same side to the east in the 1st, 3d, 5th, and 7th experiments; and had that side turned westward in the 2d, 4th, 6th, and 8th; and Mr. G. began to count the vibrations, when he observed it to vibrate just 10°. All these experiments were made with sufficient care in every particular, excepting the quantity of the dip, which requires the divisions of the semicircle to be very equal, and the 90° to be perpendicularly under the axis of the needle; this last he found was a little faulty, the dip being in reality greater than the semicircle showed it. After rectifying this error, and now touching the needle, upon that part of the armour to which iron is applied, when it is to be lifted by the stone, it performed the same number of vibrations in less time than in any of the former trials. He now determined to observe, for some space of time, both the dip and vibrations, without fresh touching the needle.

The observations follow, by which it appears there is a very considerable difference, both in the quantity of the dip, and in the quickness of the vibrations.

In all these experiments, the needle was placed, so as to vibrate exactly in the plane of the magnetic meridian; and sufficiently distant from all iron that could affect it, as far as could be perceived, till he had occasion to put up a very large iron rod in the room above it, which immediately altered the dip of the needle, and thereby put an end to these trials.

The experiments are then recorded, as made at several hours of every day, from March 29 to May 2, 1723; the quantities of dip differing from each by only a very few minutes of a degree. The greatest was 75°, and the least 74° 20′: the medium of all, nearly 74° 40′.

The vibrations of the needle, also observed at the same times, were, the 100 vibrations in all intermediate intervals of time, between $6^m\ 12^s$ the greatest, and $4^m\ 58^s$ the least; the medium of all being nearly $5^m\ 35^s$, the time of performing 100 vibrations.

An extraordinary Case of Tumours. By Mr. Joseph Atkinson, Sen. Surgeon.

N° 389, p. 340.

A maiden, about 20 years of age was brought to Mr. A. about Christmas 1723; having a tumour on the inside of her right thigh, extending from the groin to the knee, which was so large, that it seemed to contain at least the quantity of a gallon: the cutis was exceedingly distended, but of the natural colour, only, the capillary veins appeared varicous, and very numerous. She had also a large tumour on the buttock of the same side, of the size of a quartern loaf; but when the tumour on the thigh was pressed, the tumour above very much increased, which showed a communication, and it proved afterwards to be so. She had also another tumour on her right side, stretching from the left side of the vertebræ of the back to the hypochondrium, about the size of a penny loaf. Her body was very much emaciated, and she could hardly breathe, and the little victuals she eat very difficultly passed out of the stomach. She had had the menses but twice or thrice, viz. about 12 months before the beginning of those tumours ; and it is to be remarked, that the tumour of her thigh began first, and increased to near its full magnitude, before the tumour of the buttock and hip began; after that, the tumour of her back began, which, as it increased, brought on great difficulty in breathing. She had been with several other persons, who advised against opening the tumour of her thigh, most of them being of the opinion it was from blood, and that her case was incurable. Mr. A. was of a contrary opinion; but being told what so many others had said, he declined meddling with it at that time, though her parents and herself wished it. At this time he dismissed her, saying, that if she lived, a little time would discover more of her case.

About 2 months after, he was desired to visit her again, when the tumours were so monstrously increased, and her body so wasted, that he wondered she could live under such circumstances. The tumour of the thigh was every way yielding to the pressure of the finger; nor was there the least hardness about its extremities; so that it might be easily mistaken for an aneurism, had it not wanted the grand characteristic, pulsation, which some say is not to be felt when those are very large; the middle of this then looked a little red, and shining, and seemed to point a little. He told them it would probably break with a small orifice, and show what was contained, willing them to notify it to him, if such a thing happened. Three days after they called him in haste to the patient, saying, the swelling of her thigh was broke. He found there had been discharged a small quantity of purulent substance, much like what is contained in a meliceris, but the opening was so small or closed, that he could not enter a probe; however, though she seemed ready to expire, yet, at the desire of her parents, he opened this tumour with a lancet, making an incision about 1½ inch long, through which poured 3 pint basins full of matter, besides several smaller, which together contained about 5 quarts. It was very fœtid, and bloody towards the latter end of this discharge; upon this, the tumour wholly subsided, insomuch that the thigh instantly became as small as the other. He put his finger into the wound, and found the fascia lata quite consumed; the muscles lay all loose, so that he freely touched the thigh-bone between them.

Immediately on the discharge of the matter, the tumour on the buttock was considerably abated, but there followed about 2 or 3 spoonfuls of florid blood. He dressed it up for this time with a proper digestive, and a suitable bandage. The day following he found she had slept pretty well, and was much refreshed, and not the least faintness had attended her; which shows the imaginary synocope, that is feared to follow such evacuations, to be groundless. The day following, taking off the dressings, the limb was larger than the other. At the third dressing there appeared a small hard swelling a little below the orifice, which was made by some grumous blood that lay there, which he turned out with his finger, in quantity about 4 oz.; this was followed by a florid blood. He judged this proceeded from some hypogastric vessel that supplied this tumour. He then laid open the sinews to the groin; and, though he could not discover the vessel, yet he so successfully applied an astringent, that from that time it bled no more; however there was for a week a great flowing of a serous matter, which wholly sunk the tumour of the buttock and hip, and, by boulstering and compressing with suitable bandage, the so long separated cutis closed with the muscles, and all things, in about a fortnight, seemed to be in a fair way of healing, yet it was near 3 months, before this cure was completed.

But still, for a fortnight after the opening of the thigh, the tumour on her back continued, and she was much straitened for breath; saying, if that was opened she should be presently relieved; this tumour Mr. A. then opened, and there issued out about 2 quarts of matter, or rather more. He entered a probe, and found it penetrate into the cavity of the thorax, between the second and third spurious rib, reckoning from beneath, on which she respired with all freedom; but there was a halitus at this wound. He continued to dress this, and believed, before this tended towards healing, not less at times than a gallon, or rather 5 quarts of matter, was discharged. And when he thought all was over, it filled again, the external tumour became almost as large as before, and her breathing as difficult as ever; so that he now thought all his labour had been in vain; yet he opened it again with a larger orifice, and from that time dressed it successfully to the perfect healing. The menses returned, and the patient continued after in a good state of health.

An Experiment to illustrate what was said in N° 386, 387, 388,* *concerning the Figure of the Earth. By the Rev. J. T. Desaguliers, L. L. D. F. R. S.* N° 389, p. 344.

On an axis of iron made to turn swiftly, by means of a wheel, whose string went round a pulley fixed to the said axis, Dr. D. slipped on two iron hoops, whose planes intersected at right angles, representing two colures, which, being of a spring temper, sprung in such manner as to be $\frac{1}{96}$ part longer in that diameter that coincided with the axis, than in their equatorial diameter; this proportion being the same that Mr. Cassini supposes to be between the axis and equatorial diameter of the earth. Two circular plates, to which the said hoops were rivetted, had square holes, through which the axis passed, so that the two poles of the oblong spheroid, which the hoops describe in their revolution, might approach together in such manner, as to let them put on the form of a true sphere, when, by the whirling, the equatorial diameter of the machine swelled and overpowered the elasticity of the hoops. A greater degree of swiftness turned the sphere into an oblate spheroid of Sir Isaac Newton's figure. A velocity still greater makes the disproportion of the diameters, such as those of Jupiter; and still the equatorial diameter increases with the centrifugal force.

Another hoop with a catch, representing the equator, shows the increase of the equatorial circumference; and an index, applied to the frame, shows the increase of the diameter.

As soon as the revolution of the machine ceases, the colures, meridians or hoops return to their elliptical figure, whose longest diameter is the axis of revolution.

If the force, by which the hoops endeavour to keep their figure, be considered as the gravity that keeps together the parts of the earth; from this experiment, compared with what has been said in the translations abovementioned, it will appear that the earth cannot preserve its figure, unless it be an oblate spheroid.

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* An abstract of all the papers will be found at p. 61, of this vol.

Some Experiments concerning the Cohesion of Lead. By the same. N° 389, p. 345.

Dr. D. took the leaden balls A and B, fig. 1, pl. 2, the first weighing 1 lb. and the other 2 lb.; and having from each of them cut off a segment of about $\frac{1}{4}$ inch in diameter, he pressed them together by his hand, with a little twist, to bring the flat parts to touch closer. The balls stuck so fast, that when the hand H, by means of a string, sustained the upper ball A, the lower one B, was sustained by the contact at C, though loaded with the scale S, and weights E, which amounted to 16 lb. A little more weight added separated them, and, on viewing the touching surfaces, it appeared that they did not exceed a circle of $\frac{1}{10}$ inch diameter; but this surface can hardly be measured exactly, on account of its irregularity. The experiment was repeated several times, and the cohesion of the balls was different every time.

At another time he made the experiment another way, as follows: On the upper pin or bar of the wooden frame DDIH, Dr. D. suspended the steel-yard EF, whose hook held up a leaden ball A, of 2 inches in diameter, having a hole through it, at A, to receive a string; the lower ball B equal to, and prepared in the same manner as the first, received the pin oo through its string, so that G, the weight of the steel-yard, was made use of to separate the balls, which happened when it was applied at the number 20, in the first experiment; but in the three following experiments, the balls were not separated till the weight was removed to the numbers 25, 37, and 45, expressing pounds on the steel-yard.

Lastly, the balls being applied together as before, always cleaning the surface of contact with a knife, and never making a contact sensibly greater than as mentioned before; the weight G removed quite to the end F, where it weighed 47 lb. was not able to separate the balls, so that he was obliged to make use of another steel-yard. He made several other trials, but could not again bring the force of the contact to be equal to 47 lb. How much greater than 47 lb. the force of the contact was in the 5th experiment, he could not determine, by reason of an accident; but the surface was much as before.

An uncommon Nævus Maternus, or Mole. By Dr. Steigertahl, Physician to his Majesty, and F. R. S. N° 389, p. 347.

Jeremias Rudolph von Walthausen, a captain of the garrison at Danneberg, near Lunebourg, was born Oct. 24, 1680, with a very singular mole on his right arm, shoulder, and hind part of his side, not unlike the branch of a vine, with its leaves and grapes. It has been affirmed and attested, both by the deceased himself and several of his relations and friends, that his mother, when big with child, had an earnest desire for grapes, and impatient to stay till they were full ripe, went down into the garden to pluck off some of those unripe; on which a whole branch with its leaves and grapes suddenly fell down upon her right arm, at which she was much frightened. Some time after, she was brought to bed, and the child was observed to have several reddish or bluish spots, beginning from behind his shoulder, and from thence extending over the same, down the right arm to the fingers. The captain's whole right side was larger than the left by more than an inch, and so continued to his death. The veins of the right arm were much raised, lying almost immediately under the cuticula, which made them very discernible; they were also much distended, chiefly between the elbow and hand, where they were almost as thick as a man's thumb. On the inside of the fore finger the vein was extended into a small tumour, of a reddish or purple blue colour, about the size of a nutmeg, corrugated with some lenticular protuberances, which made it in some measure resemble a grape. The like tumours, but not so large, were observed in several other parts of the arm, in the spring time; and as they thought, when the sap began to enter the vines, as also when the vines flowered, and in autumn, when the wine was fermenting, the captain was taken ill, with violent and itching pains in the affected arm for some days. The whole right side then swelled more than usual, and the veins and tumours were so distended with blood, that at last a serous matter was forced out of the pores of the tumours, which as it gave the patient some relief, so he promoted it, by scraping the tumours with the edge of a penknife. If the captain held up his affected arm, the running of the blood backwards, in the distended veins, was very visible. If he held his arm down again, the blood returned with some noise, and sensibly filled up again the vessels, which by the preceding action had been emptied; for which reason, when in bed, he was obliged to lay his arm upwards.

An Account of a Book, entitled, Historia Cœlestis Britannica, tribus Voluminibus contenta, Authore Joanne Flamsteedio, Astron. Reg. N° 389, p. 350.

The first volume contains the observations of Mr. William Gascoigne, the first inventor of the way of measuring angles in a telescope, by the help of screws, and the first that applied telescopical sights to astronomical instruments, taken at Middleton, near Leeds, in Yorkshire, between the years 1638 and 1643, excerpted from his letters to Mr. Crabtree; with some of Mr. Crabtree's observations of the same years; as also observations of the sun's and moon's diameters; configurations and elongations of Jupiter's satellites from him, small distances of fixed stars, with appulses of the moon and planets to them, observed with a telescope and micrometer at Derby, by Mr. Flamsteed, between the years 1670 and 1675; with the larger intermutual distances of fixed stars, and of the planets from them; eclipses of the sun, moon, and Jupiter's satellites, spots on the sun, comets and refractions, taken with a sextant of near 7 feet radius, a voluble quadrant, and the abovementioned instruments, between the years 1675 and 1689, at his majesty's observatory, disposed under proper heads, with the places of the moon, Saturn, Jupiter, Mars, Venus, and Mercury, deduced from the observations, and also necessary tables to be used with them.

The second volume contains his observations, made with a mural arch of near 7 feet radius, and 140 degrees on the limb of the meridional zenith distances of the fixed stars, sun, moon, and other planets, with the time of their transits over the meridian, also observations of the sun and moon's diameters, eclipses of the sun, moon, and Jupiter's satellites, variations of the compass from 1689 to the end of the year 1719, &c.

Tables to render the calculation of the stars and planets places, from the observations, easy and expeditious; to which are added the places of the moon, at the oppositions, quadratures, and on her limits, &c. and the places of Saturn, Jupiter, Mars, Venus, and Mercury, derived from the abovementioned observations.

The third volume contains a catalogue of the right ascensions, polar distances, longitudes, and magnitudes of near 3000 fixed stars, with variations of the right ascensions and polar distances, while they change their longitudes one degreee, by which their right ascensions and distances from the pole may be determined for 200 years past, or to come, sufficiently exact. Large tables, by which the right ascensions and polar distances of the stars and planets, being given, their longitudes and latitudes may be found by inspection. To this volume is prefixed a very large preface; containing an account of all the astronomical observations made before his own time, with a description of the instruments made use of; as also an account of his own observations and instruments, together with a new Latin version of Ptolomy's catalogue of 1026 fixed stars, from the Greek, and Uleg-beig's places annexed on the Latin page, with the corrections; small catalogues of the Arabs, Tycho Brahé's of about 780 fixed stars, in a proper order; the Landtgrave of Hesse's of 386; Hevelius's of 1534, in a proper order. A catalogue of some of the southern fixed stars, not visible in our hemisphere, reduced to right ascension, polar distance, longitude, and latitude, with variations of the right ascensions and polar distances, calculated from observations made by Dr. Halley, at St. Helena, and Mr. Flamsteed's stars' places, and fitted to the year 1726.

An Account of the Scythian, or Vegetable Lamb. By Dr. Breyne, of Dantzic, F. R. S. N° 390, p. 353. *From the Latin.*

The pretended agnus scythicus, or vegetable lamb is (it is now well known,) nothing more than the root of some large fern, covered with its natural villus or yellow down, and accompanied by some of the stems, &c. in order, when placed in an inverted position, the better to represent the appearance of the legs and horns. The fern itself is generally supposed to be the polypodium aureum of Linnæus. Dr. Breynius, the author of this paper, very properly treats the whole as a fable, and suggests the real state of the case, viz. that it is nothing more than the root of a fern. He also adds, that the down or villus of the root is the poco sempic, or golden moss, so much esteemed by the Chinese for the purpose of stopping hæmorrhages.

On Camphor. By Mr. John Brown, Chemist, F. R. S. N° 390, p. 361. *An Abstract from the Latin.*

Mr. Neuman's observations on camphor, and its extraction from the distilled oil of thyme, gave occasion to this paper of Mr. Brown's. The lastmentioned chemist is of opinion, that the coagulated or crystalline substance, which Mr. N. obtained from the aforesaid oil, cannot properly be called camphor, seeing that the same effects do not in all instances result from the action of the same chemical agents on both. Thus, although neither the one nor the other is soluble in water, but both in spirit of wine; yet it is otherwise when spirit of nitre or oil of vitriol are employed. With spirit of nitre genuine camphor forms a pellucid liquor; but on the affusion of the same menstruum, the coagulated oil of thyme puts on a resinous or gummy form, and never becomes fluid. Again, if the solution of camphor in spirit of nitre be diluted with water, a separation of the camphor is effected; whereas under the same treatment the coagulated oil still retains its resinous or gummy form.

In like manner camphor is wholly dissolved in oil of vitriol, which is not the case with the coagulated oil of thyme, &c. &c. Mr. Brown adds, that Dr. Slare had mentioned, 30 years before, the obtaining of a similar substance to that described by Mr. Neuman, in the distillation of thyme, and that another chemist, Mr. Manel, had shown him a similar product from oil of marjoram.

On the Effects of Lightning. By the Rev. Mr. Jos. Wasse, Rector of Aynho in Northamptonshire. N° 390, p. 366.

We are told by Mr. Jessop, in a former N° of the Transactions, that what the common people call fairy circles, are occasioned by lightning; but I think it has not yet been observed, that they continue visible 50 years, and that no composition of use in fire-works will produce near so lasting an effect as I have experienced. There seems to be something here, which sulphur and nitre will hardly account for. Does it depend on the great quantity of the matter discharged, or the violence with which it is impelled? The ground is nowise torn up, and the grass is only a little blasted; whence it would seem its force is nearly spent: whereas, when the burst is near us, the effect is like that of a petard, as appears from the following instance.

At Mixbury, on July 3, one William Hall, about 60 years of age, was found dead in a hard gravelly field, with 5 sheep, which lay round him at about 30 yards distance; of which that only which lay nearest him had a visible wound through the head. The shepherd lay partly on his side; the upper part of his head was terribly fractured, and his right knee was out of joint; he had a wound in the sole of his foot, towards the heel; his right ear was cut off, and beaten into his skull, and blood flowed out of that part upon the ground. All his clothes and shirt were torn into small pieces, and hung about him; but from the girdle downwards they were carried away entirely, and scattered up and down the field; particularly the soles of a pair of new strong shoes were rent off. His hat was torn to pieces, a hand-breadth of it was full of irregular slits, and in some few places cut as with a very sharp penknife, and a little singed in the upper part. His beard and the hair of his head were mostly close burnt off. The iron buckle of his belt was thrown 40 yards off, and a knife in the right side pocket of his breeches was broken in pieces, not melted, and the handle split. Near his feet were two round holes, about a yard deep, and 5 inches diameter, like the perforation of a mortar shell fired perpendicularly upwards, when it falls down again. About the time this accident happened, a tradesman of the town observed a sort of fire-ball, as large as a man's head, to burst in four pieces near the church. Two persons at Aynho were a little hurt at the same time, and one of them struck down to the ground, who says, he thought he was knocked down with a beetle. Mr. Wasse himself heard the hiss of a ball of fire, almost as large as the moon, which flew over his garden from S. E. to N. W.

Both the abovementioned holes were almost perpendicular for half a yard, and after that grew narrower; in both of them, the matter divided into two parts, and formed horizontal cavities about 3 inches diameter. In one was found a very hard glazed stone, of about 10 inches long, 6 wide, and 4 in thickness, cracked in two; others it could not pierce, but was turned here and there out of its course, but left not the least blackness, or other discolouring any where. As to the knife, it was not the blade but the handle and the hinge which were shivered in pieces. Near the wounded sheep, the ground was torn up near 2 yards round.

One James Marshal of the same town said, that in the middle of the same storm he received a blow on his hat, which rattled like shot through the branches of a tree; it beat in the crown a little, without penetrating it: he staggered, and was giddy for two days afterwards. Two of his sons were at the same instant both struck down to the ground, and stunned a little, but presently came to themselves, and had no wound. Query, whether this may not be accounted for, by supposing the flame to rarefy the air, and make a sort of vacuum about one, into which when it returns again, it gives the likeness of a stroke with a beetle, as he expressed it. Perhaps a wind-gun, with compressed air, would have the same effect, and might easily be tried on a dog, or such like animal.

*Of Magnetical Powers. By M. Muschenbroek.** N° 390, p. 370. *From the Latin.*

M. Muschenbroek wished to try, whether loadstones operate on each other at different distances, according to a certain proportion; and he saw in the Phil. Trans, N° 335, that Mr. Hauksbee had thought of the same thing; but that he had made the experiments with a loadstone and needle, that could not satisfy accurate inquirers; whence, though he concludes in these words: 'I see no reason to doubt, but the proportions of this power will be regular, and correspond to the different distances;' yet Dr. Taylor, Phil. Trans. N° 334, repeated the same experiments, and made observations differing from those.

M. Muschenbroek attempted the same thing in a quite different manner; he thought, that if he took two magnets, and hung one of them by a thread, at different distances above the other, and if he tied the end of the thread to a balance, he might weigh the quantity of the force with which the magnets would act on each other; which succeeded accordingly. He took a very nice

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* A celebrated Dutch philosopher, born in 1699 at Utrecht, where he became professor of mathematics. He afterwards accepted the professorship of natural philosophy at Leyden, and died in 1761. He was author of several works: as, 1. Elements of Physico-Mathematico, 1726; 2. Elements of Physics, 1736; 3. Institutions of Physics, 1748; 4. Introduction to Natural Phylosophy, 1762. Besides several tracts in the Memoirs of the Paris Academy, for the years 1734, 35, 36, 53, 56, and 60. Also several papers in the Philos. Trans. vols. 33, 37, 38.

balance, and put a scale to one arm, and a thread several feet in length to the other: to the lower end of the thread he tied an unarmed magnet; he made the thread very long, that the experiment might not be disturbed by any action of the magnet on the iron balance; and he likewise pitched on a place in the house, where there was as little iron as possible. He took two very good magnets, perfectly spherical, which Mr. Gilbert calls terrellæ, whose poles were exactly in each extremity of the axis of the sphere; and consequently he could very accurately measure the distances of both poles; he first counterpoised the magnet, by means of a weight in the scale, and afterwards he put the magnets under each other: and as the balance was moveable, by means of a cord over a pulley, he let it down to different distances at pleasure; and when the upper magnet was attracted downwards by the force of the lower, he always laid so much weight in the scale, till the force of the magnet and the weight were in equilibrio. The following table contains the experiments made at the different distances of inches and lines; and corresponding to them are columns, with the number of grains which counterpoise the attractions at these distances.

Distance. inches.	lines.	Grains of attraction.
13	6	0
12	0	$0\frac{1}{20}$
11	0	$0\frac{1}{8}$
10	0	$0\frac{1}{4}$
9	0	$0\frac{1}{2}$
7	6	$1\frac{1}{2}$
7	0	$2\frac{1}{2}$
	12	$70\frac{1}{2}$
	11	$78\frac{1}{2}$
	10	87
	9	94
	8	106
	7	114
	6	131
	5	146
	4	172
	3	190
	2	215
	1	250
	$0\frac{1}{2}$	290
In the very point of contact or	0	340

M. Muschenbroek made use of Rhinland inches, and the grains were apothecary's weight, very accurately adjusted, in order to have them true, and of equal weight.

After having proceeded so far, he suspected whether the suspended magnet were not in some measure heterogeneous, and whether another, substituted in its stead, might not render the event more successful, and from which he might at least receive more light; for, these experiments were too tedious, to have reaped so little advantage from them. The following table exhibits the observations he made with another very good small magnet, while the lower terrella was the same as before, and firmly fixed on a table; these experiments were made in the same manner as the former.

Distance, inches.	lines.	grains of attraction.	lines.	grains.
5	10	1¼	7	33
4	6	2¼	6	38½
3	9	3	5	43½
2	4	9	4	50½
1	9	12	3	62
1	0	23	2	79
	11	23½	1	140
	10	26¼	0½	186
	9	29	0	340
	8	30¾		

But here again occur great irregularities, from which nothing can be concluded: this only is surprising, that though the magnet used in the second experiment was smaller than that in the first, yet in the point of mutual contact it was attracted with equal forces, namely, 340 grains, while in other distances the attraction was much less, as appears from comparing both tables; but besides, this smaller magnet was more vigorous and much better at raising the iron than the magnet in the first.

He repeated these experiments with other magnets, and particularly with one whose force was so great, as to affect a magnetic needle at the distance of 14 Rhinland feet. But from all the experiments he could only conclude, that there is no assignable proportion between the forces and distances.

Since both the declination and inclination of the magnetic needle vary almost every year, he wished to observe, whether the force of the magnet was the same every day, or greater or less in summer than in winter; but he found by several experiments, that the force is less in summer than in winter, at least in the summer of 1725; whether it will be the same in 1726, must be then discovered.

He therefore took the two magnets used in the first experiment, and on July 25, 1725, when the barometer was at $29\frac{9}{24}$ inches, he made experiments with them, altogether in the same manner, and in the same part of his house as before. M. Fahrenheit's thermometer was at 62 degrees, the wind N. W. the sky serene, and the weather dry.

Distance, inches.	lines.	grains of attraction.	lines.	grains.
12	0	0	7	106
9	0	1⅛	6	111
8	0	1½	5	132
7	6	2	4	149
7	0	2½	3	173
	12	70½	2	205
	11	75⅛	1	240
	10	85	0½	270
	9	92	0	300
	8	100		

Philosophers are agreed, that both poles of the magnet do not act with equal force; but that the north poles are stronger than the south: this has been asserted indeed, but no where accurately demonstrated: and because M. Muschenbroeck's method of estimating the magnetic forces, was sufficiently easy, and that this might be thus accurately determined; he turned both the poles of each magnet in such a manner, that the corresponding poles might be opposite to each other; the observations he made on the magnets in this last experiment, were as follow:

Distance. lin.	Grains of attraction.	lin.	Gr.
12	57	5	101
11	63	4	113
10	66	3	124
9	70	2	148
8	79	1	168
7	83	0	228
6	90		

Hence it evidently appears, that both poles of the magnet do not act with the same force; the quantity of the difference may be seen by comparing both these tables together.

Since M. Muschenbroek had hitherto been persuaded, that the action of the magnet depends on effluvia, or at least on some impelling fluid without the magnet; and since he had observed, that the most learned philosophers were of the same opinion, he had a mind to try whether he could confirm this by any experiment. While he made the former experiments with magnets, therefore, placed at different distances from each other, he interposed very thick pieces of lead, tin, silver, copper, and a pretty large mass of mercury, in order to see whether the magnetic effluvia would not be intercepted; and if not entirely, yet if in some measure at least: glass is pellucid and transmits the rays of light, but not in such quantity as with glass intervened; in the same manner he supposed that the magnetic effluvia, if they were not quite intercepted, yet in some degree would hinder the magnets from attracting with such force, if a piece of lead of a cubical foot, or a piece of lead 2 inches thick, the tin of the same thickness, and afterwards copper or a large mass of mercury, intervened between them; but he observed, that whatever bodies he interposed, the magnetic forces were always the same, as if no such bodies at all intervened; which he thinks indeed a thing surprising and not to be understood: for we are not to suppose that these bodies are so porous, as to have no solidity; if therefore they have some solid parts, as they have a great deal, shall not these hinder the approach of a foreign fluid, or its egress from the magnet, or some of it at least; but experiments show, that the magnetic forces are nowise hindered; or shall these effluvia be much more subtile than the rays of light? Besides, that this again is but an hypothesis, the above difficulty is not removed: fire is intercepted by bodies, and light does not immediately penetrate all bodies; and thus it is with all fluids, they meet with resistance from solids; but it is not so with the magnetic effluvia, they meet with none from a solid body; and this is the grand difficulty.

But he takes the strongest argument from the repelling forces of magnets, which are much weaker than the attracting forces, as appears from the experiments below; so that a fluid must necessarily come from without towards the magnet, which meeting the other magnet, impells the one fluid towards the other, and which enters the magnet; and because the magnetic attraction is much stronger than the repulsion, a greater quantity of the fluid enters into the magnet than passes out from it: whence the magnet must necessarily be soon filled with this fluid, so as to be no longer porous; nor can it be supposed, that this fluid is emitted from all parts of the magnet, as it were; for, the attraction is in every point of the magnet, but the repulsion is only in the poles. In order to show that the magnetic repulsion is less than the attraction, the following table contains the experiments, made with the last mentioned magnets.

Distance.		Grains of repulsion.		Distance.		Grains of repulsion.
inch.	lin.			inch.	lin.	
13	0	0		1	11	16
11	11	½		1	10	17
10	9	¾		1	4	17
9	9	1		1	0	24
9	0	1			10	24
8	0	1¼			7	25
7	0	1¾			6	25½
6	1	2			5	27½
5	1	3½			4	29
4	0	6½			1	34
2	9	11½	in the very point of contact		0	44
2	3	13				

Hence it appears that no proportion can be deduced from these experiments on the repulsion of magnets; but that magnets are indeed very surprising bodies, of which we hitherto know but very little.

An Account of the Anomalous Epidemic Small-Pox; which began at Plymouth in August 1724, *and continued to June* 1725. *By Dr. Huxham.* N° 390, p. 379.

The small-pox were preceded by the usual symptoms of that distemper; but the pains of the limbs and back were generally more severe than common, as were likewise the nausea and vomiting. Numbers were seized with violent colic pains, which would leave them on the eruption, or after a clyster or two with a gentle anodyne: the stools were commonly bilious. It sometimes happened that the symptoms would not seem very severe before, and at the eruption; and yet the pox would prove very confluent and fatal.

The pustules were very small, and did not regularly fill; but, in a day or two after the eruption, would flat and be depressed in the middle. This was observed even in the distinct kind. In some persons they appeared in less than 24 hours from the seizure: when they broke out so very soon, they were always of the confluent kind, as is commonly observed. The eruption was attended with prodigious sneezing, especially in children. One child about five years old, sneezed incessantly for more than 30 hours, nor could it be allayed but by anodynes. This child had the confluent pox, and died the 13th day. In some patients, both at and after the appearance of the pustules, they would itch most intolerably; this was generally a bad symptom; as it was an argument of the great acrimony of the morbific matter.

In some few, a day or two after the eruption seemed to be completed, there would appear in the interstices of the pox, several miliary pustules, some of a dark red, others filled with a limpid serum: these never came to suppuration, as the secondary crop of small-pox, which sometimes do; nor were they as large. Though this is a bad symptom in general, yet in a girl of 7 years old, her fever and delirium went totally off, on this eruption, and the urine immediately settled.

Some had abundance of purple petechiæ appear among the pox at the eruption, and the pustules would look of a livid hue: in others, the purples would not discover themselves, till the maturation. Of these, some died the 5th or 6th day, some dwindled on till the 10th or 11th. One only survived.

During the suppuration, the pox would become very sessile, and the coherent kind would enlarge their bases exceedingly; so that, though they seemed for some time after the eruption to be very distinct, they would now flux together. A purple speck would often appear in the centre of the pustules, which would spread, and grow blacker and blacker by degrees. The interstices would also sometimes turn pale, sometimes livid: Symptoms which are very bad. The pustules without the purple speck did not incrust yellow, but appeared of a dead, ash colour, and by degrees took a dark black crust.

The salivation, which constantly ought to accompany the maturation in the confluent small-pox, was in several very inconsiderable, in some none at all, excepting a very small quantity of extremely viscid matter, which was got off by syringing. Dr. H. had 2 adult persons, and some children, labouring under the confluent sort, who neither salivated, nor purged, except when some lenient cathartics were given them; and yet they got over the distemper. Indeed it was very rare to find children have that gentle diarrhœa, which Sydenham and others justly think supplies the salivation in persons of more advanced age. Some very young children, on the contrary, drivelled exceedingly through the course of the distemper. In 2 children, one of 5, the other of 7 years old, no salivation came on till after the 13th day, and then it was so profuse, and continued so long, that it was with difficulty stopped by purges first, and then by the bark, astringents, &c. To the younger of these, indeed the Doctor had given calomel, gr. iv, but it was soon purged off.

Where the swelling of the face and throat was very hard, painful, and tense, with a strong vibration of the carotid arteries, and little or no salivation, the patients generally became delirious at the state of the distemper. These symptoms frequently proved fatal. The maxillary and parotid glands, of those that recovered, would remain swoln and indurated, for a considerable time after the entire desquamation of the pox, though that were very slow, nor would these tumours go off, but after repeated purging, and that with calomel, &c.

Those tumours were undoubtedly the consequence of a very viscid matter obstructing those glands, which hardened the swelling of the face, hindered the salivation, and in some measure the circulation, through the external carotids. Under these circumstances, bleeding, emollient clysters, eccoprotics, plentiful dilution, were absolutely necessary.

On this occasion it may be asked, whether, the salivation being very viscid and defective, the tumour of the face hard and tense, some mercurial, as a duly prepared calomel, might not be given with advantage, even in the state of maturation? The Doctor has frequently given cinnabar to good purpose. There are some instances that would seem to justify such a practice; and he knows but one material objection to it, which is, that the weight of the mercury would by increasing the momentum of the blood, augment the fever: but surely we have given calomel after the incrustation, when the secondary fever has subsisted, without any ill consequence, indeed with great success.

Nothing so certainly fuses viscous tough humours, being joined with plentiful diluting liquors, as this, and so prepares them to be discharged by proper outlets. As to oxymel scillit. syringing, and the like, in a defective salivation; the former by vomiting, sometimes irritates the glands of the membrana schneideriana to discharge their contents; syringing barely deterges the mouths of the ductus salivares: either have little certain effect further; whereas the viscous obstructing matter is lodged in the inmost glands, and even in the blood itself.

This method seems peculiarly adapted to such an epidemic small pox as now described, in which are all the indications imaginable of a very viscid state of humours. The blood, when drawn, was always excessively viscous, especially at the state of the disease: frequently there was little or no salivation; generally it was extremely glutinous; so that the nurses were often obliged to pull the matter out of the patient's mouth with their fingers; and without drinking very plentifully, it would soon cease. A diarrhœa very seldom happened to children. The blisters soon dried up. None were known to make bloody urine. Where that dreadful symptom happens, the crasis of the blood seems to be dissolved, as Lyster well observes; on the contrary, the recited symptoms argued a too compact and viscous diathesis of the blood.

This state of the humours, during this constitution, might partly depend on the extraordinary driness of the season, and the almost constant northerly and easterly winds, which prevailed in the months of October, November, February and March. From the middle of January to the middle of April, was a drier season than ever was known in this county, where we have generally more continued rain, than in most places in England, Plymouth being infamous for wet weather.

This remarkable change of the temperature of the air must needs have some considerable effect on human bodies: a very cold wind suffering only the thinner part of the blood to pass off by perspiration: nor in such seasons does the body imbibe so much of a diluting humidity from the air, as Keill observes. Hence the necessity of drinking plentifully of thin diluting liquors, which, as it is always proper in this distemper, so in such a season it is highly necessary. And probably M. Andry's method of bathing in warm water and milk, or warm milk, before the eruption, may, upon many accounts, be proper in such a constitution of the air. There can be no objection against it, but its not being in fashion.

The Doctor took particular notice, that while, and just after the easterly winds blew excessively strong for 7 or 8 days together, in the months of October and November, the patients in the small-pox scarcely salivated at all. Then particularly, an adult person, who had the confluent pox very severely, did not spit the least through the whole course of the disease; she was seized with a violent pleurisy the 18th day, but was relieved by bleeding. The blood was the most viscid he had ever seen. It is remarked by Lancisi, that people expectorate very little in disorders of the breast, when cold, dry, easterly winds blow; and Dr. H. has frequently observed the same. And this may be one reason, why some asthmatic persons generally suffer a paroxysm at such seasons.

The swelling of the hands did not so regularly succeed the detumescence of the face, during this constitution, as in other epidemic small-pox. Some had very small, or rather no tumours at all. It was very rare that the legs and feet swelled, till after the patients sat up, and then they had much pain in the parts.

Perhaps the succession of the tumours of the hands, to those of the face, might partly depend on the later inflammation and suppuration of the pustules of those parts: the pain and inflammation being a stimulus determining the humours to the pained part: and it is particularly to be observed, that the greatest pain of the hands and arms commonly happens at the time when the salivation begins to cease: so that the tumour of the hands may in some measure prove a succedaneum to the spitting. It is the common observation, that the pustules of the arms and hands inflame and maturate a day or two later than those of the face; and those of the legs and feet latest; which may also be the reason that the tumour of the legs succeeds that of the hands. Dr. H. has been the rather inclined to this opinion, as he sometimes observed a considerable swelling of the hands, the pustules being very painful and inflamed, and that too in the distinct kind, when there has been little or none in the face. Generally the more painful a bile is, the greater the tumour around it; and consequently the tumour of a part is in proportion to the painfulness of the biles, and their number.

From this he would inforce the use of epispastics applied above the wrists, a little before the time the tumour of the hands is expected to rise, especially when symptoms are threatening, as they are stimuli to be depended on, not only attenuating and deriving the humours to the parts, but also discharging them, and so proving a convenient outlet to the morbific matter, which before was thrown off by the now partly suppressed salivation.

Blisters applied to the neck frequently relieve the extreme pain of the throat, and difficulty of swallowing, which are sometimes exceedingly troublesome to the patient in the third stage of the small-pox, by drawing the humours another way. Nay, in some, where vesicatories have been early applied, and continued to run extremely, there has been less swelling, and less salivation, than seemed proportionate to the vehemence of the distemper, but without any disadvantage to the patients; the running of the blister supplying the defect of the spitting. It seems then but reasonable, when we expect the translation of the noxious humour to the hands, which is what nature itself affects, to endeavour to promote its flux thither, and give it vent.

How advantageous discharges of this nature may be, the Doctor had occasion once to observe in the case of a lady; where through the prodigious discharge of blisters, applied to her neck, ears, and arms, as also a plentiful flux of urine, she neither swelled nor salivated, through the whole course of a very dangerous, confluent small-pox, and yet recovered.

Any person, that has been conversant in practice, cannot but have observed translations of the morbid matter, from one part to another, sometimes of the greatest service, especially where it has had a discharge. Indeed, all critical evacuations are of this nature. But he means, how often has a bile, an imposthume, or swelling of the limbs, been the evident means of terminating a fever? This he experienced particularly in himself, several years before at Paris, when labouring under a violent inflammatory fever, with delirium, the 9th day towards night, he was seized with excessive pain in the arms and hands, upon which he bathed his hands a long time in warm water, by the persuasion of 2 gentlemen of the faculty. In a little time his hands began to swell, and in 4 or 5 hours the delirium and fever went off entirely, though the hands remained swoln and pained for some time.

If nature therefore, in some cases, take such extraordinary methods to free herself from diseases, how intent ought we to be in promoting her operations, in a distemper, where the metastasis of the morbific matter to the hands and feet is generally regular and salutary. It is doubtless on this view, that Baglivi orders sponges soaked in a warm emollient decoction, to be applied to the hands and feet in the small-pox: and this, he says, he has done with great success. The Doctor has seen no less from blisters maturely applied to the arms and legs; ordering the patients to drink plentifully of a thin whey, or the like, which takes off, in great measure, the acrimony of the cantharides.

The Doctor observes, that the delirium, attending the eruption of the small-pox, is very much alleviated by the application of emollient cataplasms to the feet, in children especially; and in many cases it has been the means of deriving the variolous matter that way; and by making the eruptions more copious in the lower parts, the face and breast have suffered less than otherwise might have happened. The great tenderness of the feet, which happens after their application, is a trifling disadvantage, in comparison of the benefit that may be received by them; and so are those shooting pains, which often affect the legs on the use of those cataplasms: not to say, that these are rather an argument of the benefit arising from their use.

In the confluent kind, generally a micturition and dysury came on about the 12th or 13th day; and that when no blisters were applied. If a large quantity of turbid urine followed, it was soon succeeded by urine, which deposed a very large sediment; but if it proved thin and limpid, and in small quantity. a delirium, tremor, subsultus tendinum, and other convulsive symptoms soon followed.

There were no symptoms so certainly fatal, at the turn of these small-pox, as a delirium; and, what is of constant ill omen in all kinds of eruptive fevers at the state of the disease, a dyspnœa, or the anhelosa respiratio: bleeding on the first appearance of them, frequently saved the patient; the omission of which a few hours, made the case irrecoverable.

It was very common in persons afflicted with these pox, that 8 or 10 pustules would run together, and form a large vesication, full of a limpid. crude matter, which would continue so several days after the incrustation. In one that died, mortifications were seen under these bladders. It was necessary to let out this matter with a lancet, or needle, as soon as possible, lest it should cause an ulceration, as it did when left to itself.

In two patients, several of the pustules were filled with a bloody sanies: it was surprising to find one of them get so easily over the distemper, though she laboured also under the flux kind.

The desquamation was very slow; the black crusts adhering several days, nay weeks, after the turn, while abundance of purulent matter gleeted from under them. These left very ugly cicatrices. No application seemed to have a better effect, in this case, than frequently fomenting the parts with warm milk, or milk and water; this diluted the acrid salts, washed them off, and softened the skin: oily liniments, by stopping the pores, are frequently hurtful.

In a case or two a repullulation of pustules under the crusts in the face and hands, when thrown off, were observed. This particularly in the boy, that recovered with purple spots. The latter were distinct, though the former were in the greatest degree confluent.

Nothing so certainly abated, and took off the secondary fever after bleeding, if indicated, as gentle cathartics; such as rhubarb, manna, tartar, infus. senn. and the like. The hot, scammoniate, aloetic purgers, seem not so proper, at least, to begin with. These were given the 10th, 11th, 12th, or 13th days, if the patient had a quick pulse, feverish heat, dry tongue, head-ach, restless anxiety, and other symptoms of the putrid fever. Some one, or other of these, being once or twice repeated, calomel was given, and purged off. This was our general method, and the most successful.

The Dr. thinks the world highly obliged to Dr. Friend, and the other noble ornaments of our faculty, that have introduced, and written in favour of, this method. In the beginning of the Dr.'s practice, relying on the authority of Morton, he gave the cortex to check the secondary fever; especially when he found it, as is very frequent, evidently intermit; but not with a success any way answering expectation: not but that after due purging, the bark is very proper to extinguish the hectical disposition of the blood, which is frequently the consequence of the small pox; to which, if a cool regimen, and asses milk, where no idiosyncracy forbids it, be subjoined, we have done, perhaps, as much as lies in the power of physic.

This surely is the only way of cleansing the primæ viæ, stuffed with a load of fœtid, acrid impurities, thrown off by the glands of the guts, which cannot be supposed to cease from their office, during the course of this distemper: and as the pores of the skin are at this time very much constipated by the incrusted pustules, it is reasonable to believe that the glands of the guts rather separate more than usual; it being an allowed maxim in physic, that the lessening one evacuation, is the increase of another; especially where there is such a peculiar consent, as between the skin and the guts.

If so, the excrement being retained for a week, or more, by its weight pressing on the great artery, hinders the blood from passing freely to the lower parts, and so deluges the brain. Hence those deliriums, comas, &c. so frequently threatening at this stage of the disease. Further, can we imagine, that the putrid recrement of the now putrid blood, joined perhaps with the pus of the internal pox, and having also the addition of some part of the morbific matter separated by the glands of the fauces, which is accidentally swallowed, must not be greatly hurtful, by remaining in the intestines? where growing more and more acrimonious, as is the nature even of our most balsamic juices, when they are extra aream circulationis, and exposed to the constant heat of the body, it contaminates the chyle, or liquors that are drunk, is reabsorbed into the mass of blood, and becomes a pabulum to the very fever, which nature endeavours, even this way partly to throw off.

And indeed what horribly offensive fœtid, large stools do we observe in this distemper, on the use of clysters, and more especially after a purgative? So that this very putrid matter lying long in the guts, and growing more and more so, becomes at last so virulent, as to corrode them, and brings on that very diarrhœa, or dysentery, which it is so vainly feared would arise from a gentle cathartic; since we are always easily able to check its force, if need be, with an opiate, &c.

How often do we find feverish, hectic heats proceed from a cacochymy of the first passages, in children especially? In which case, a little rhubarb, or a few gentle stomachic purgers, will do more to remove the feverish disposition, than a pound of the cortex. Indeed it is common enough to find the bark itself purge gently, on its first administration: and in some cases it has the better effect. Have not we seen some intermittents cured by one seasonable vomit? And that not barely by the shock and agitation given to the blood-vessels, genus nervosum, &c. and so acting as an attenuant; but by throwing off the saburra from the stomach, which fed the feverish paroxysms: and this is more particularly evident in the fever frequently following a surfeit. Hence it is, that a small quantity of the bark sometimes does more after a vomit, than a much greater could before it. Why therefore, when there is a lodgment of putrid matter in the primæ viæ, which partly feeds the secondary fever, should not we attempt to carry it off by either gentle vomiting or purging, as may be judged most convenient?

It has been, and may be objected to this practice, that it tends to draw the noxious humours from the circumference to the centre; but to this it has been answered, that the purging is more especially pleaded for, when the incrustation is begun, and the matter too thick to be absorbed.

If nature, neither by her own effort, nor the help of art, is capable of keeping the morbific matter from falling on the more vital parts; but, by an unfortunate translation of it, is likely to sink under its weight; as on a sudden retrocession of the tumour of the face and hands; a premature suppression of the salivation, or the like; does it not seem necessary to endeavour to carry off the offending matter by some other outlet? As, in the present case, by the guts, which are much more easily solicited to a discharge, than either the pores of the skin, the urinary passages, or the salivary ducts.

Indeed, when the salivation of course ceases, it seems necessary to promote some other evacuations in its stead. At the same time, cardiac, or alexipharmic medicines, are in nowise contraindicated by his method, if judged needful. This way nature affects in children, to whom a gentle diarrhœa is commonly of the greatest service, as proving a happy substitute to the salivation in older persons.

The following history will evince, how necessary it may be sometimes to evacuate an offending acrimonious matter lodged in the guts, and that too, even in the midst of the suppuration. About 3 months since, the Dr. had under his care a person about 30, ill of the small-pox. It happened, that the

4th day from the eruption, he was seized with a violent bilious colic, to which he had been formerly subject: this threw him into the utmost agony. The pox flatted and grew pale, as also the interstices: his pulse was extremely languid, and he had a prodigious tremor, with clammy sweats. The Dr. ordered two clysters to be thrown up; one as soon as the other was rendered: these gave him 5 large bilious stools: after the third stool, he was tolerably easy: however, he was ordered Laudan. Solid. gr. iſs. Croc. Anglic. gr. iv. Theriac. Andromach ʒſs. 4^tis^, vel 6^tis^ horis, to be washed down with a testaceous julep. He took the Laudan. 3 times, and slept sound all night. The next morning the pustules were round, florid and turgid. The man got over the distemper, though he relapsed into his colic some days after the turn, which on purging with calomel, &c. and the use of opiates, soon left him. This person, before, and at the eruption, complained of a great difficulty of breathing, with a short importunate cough, and a violent pain under his sternum; for which reason ℥xvi of blood were ordered to be drawn, which was very sizy.

The major part of the adult persons, seized with this distemper, died; among whom fell an old gentlewoman of 72.

It was a remarkable instance of the extraordinary virulence of these small-pox, that the women, though they had had the small-pox before, and some very severely, who constantly attended the patients of the confluent kind, whether children, or grown persons, had generally several pustules broke out on their face, hands, and breast, exactly resembling the pocky pustules; which undoubtedly arose from the matter of the crushed pox infecting the skin in those parts. Those pustules arose, maturated, and scabbed off, entirely like the true pox. The Dr. knew one woman, (a nurse) that had more than 40 on one side of her face and breast; the child she attended frequently leaning on those parts on that side. Those which had the tenderest skins, and who attended those ill of the worst sort, had most of these eruptions. Instances of this nature were very frequent.

An Account of the Strata in Coal-Mines, &c. By John Strachey, Esq. F. R. S. N° 391, p. 395.

Mr. Strachey here corrects a typographical error in his account of the several strata of earths and minerals, found in some of the coal-works in Somersetshire, printed in the Phil. Trans. N° 360: for whereas he said, that in those parts they never meet with freestone over the coal; it is, by mistake, called fire-stone; whereas the latter is always found in those mines, contrary to the works in Staffordshire, Newcastle, and Scotland, where freestone does lie over the coal. He further observed the strata of stone, clay, and marl, of the interjacent hills, where, under the black marl. lies a spongy yellowish earth; all this lies above the red soil, which is generally the surface of the vallies, where the coal is found. And as this red mould on the surface degenerates into marl or loam, so towards the north west, beyond or without the veins of coal, about Winford, in the same county, it turns to ruddle, or red-ochre, used chiefly for marking sheep, and for ground colours or priming, instead of Spanish brown; and often counterfeits bole ammoniac.

But as he never heard that any coal was found to the west or south of Mendip hills; so Cotswold, to the north-east, and the Chalk-Hills of Marlborough-Downs and Salisbury Plains, seem to set bounds to the coal country, to the east and south-east; of which fig. 3, pl. 2, may be supposed a section from south-east to north-west, viz. from the dip to the rise; and fig. 4, at right angles, from south-west to north-east, on the drift or level.

Mr. S. mentions this by way of correction and addition to his former observations of the coal-works in Somersetshire. He has since had opportunities of being under ground, and viewing several coal-works in Scotland and Northumberland, and observing their several strata. At Widdrington they have 4 fathom clay, then a seam of coal, about 6 inches thick, not worth working; then a white freestone; then a hard stone, called whin; then 2 fathom of clay; then a white soft stone; and under that a vein of coal 3 feet 9 inches thick. This is a small coal of the same nature, but not so good as the Newcastle coal which comes to London market. These veins dip to the south-east, one yard in 20. Near Tranent, in East-Lothian in Scotland, the coal dips also to the south-east, in the same proportion; but at Baldoe, in the parish of Campsy, 3 miles from Kylsith, it dips to the north-east; and at Madestone, near Falkirk, to the same point, and in the same proportion. The strata of earth sand minerals, at these places, agree very nearly: they have, as the ground rises or falls, 1, 2, or 3 fathom of clay; then 11 fathom of slate, or coal-clives; 1 fathom of limestone; under that 2 fathom of slate, earth and stone; and then coal. And all these agree in this, that the pits generally need no timber, and have a good roof, which is supported by pillars of coal, which they leave in the working. At Baldoe, the coal is commonly 45 inches thick; and all along, for some miles eastward thence, on the sides of the hills, are crops of coal and limestone; and the tenants often spit up as much as will serve them for a winter's burning, just under the surface; for there wants a market, and it is scarcely worth working for sale. And to the north-west and north, in the drift of the coal in higher ground, and consequently lying over it, there appear, in the sides of the hills, seams of spar and lead, the drift of which is north-east, and lies almost perpendicular; but what obliquity there is, pitches to the south-east. At Auchenclaugh, 6 miles east from Kylsith, there is a coal 18 feet thick; this dips 1 foot in 3, and is not pursued, by reason of water; and, for want of a market, will not quit the cost of draining. At Madestone, the coal is 4½ feet thick, and above 3½ fathom deep: they land it on girls backs. Near Tranent three different veins are wrought; the undermost is about 18 fathom from the surface, called the Splenty coal, 4½ feet thick; it is a hard, but not large coal; it makes a clear and strong fire; lies 10 fathom under the main coal, which is 9 or 10 feet thick, and comes out very large. Its roof is of freestone, under which Mr. S. walked backward and forward 2 hours; but had no opportunity to make any other observations on the upper vein, than that it is about 4 feet thick, and neither so hard nor large as the other.

As fig. 3 and 4 represent the different strata which Mr. S. observed, on a supposed plane, as they there lie; fig. 5 and 6 represent them in a globular projection, supposing the mass of the terraqueous globe to consist of the foregoing, or perhaps of 10,000 other different minerals, all originally, while in a soft and fluid state, tending towards the centre. It must mechanically, and almost necessarily follow, by the continual revolution of the crude mass from west to east, like the winding up of a jack, or rolling up the leaves of a paper-book, that every one of these strata, though they all reach the centre, must, in some place or other, appear to the day; in which case there needs no specific gravitation to cause the lightest to be uppermost, &c. for every one in its turn, in some place of the globe or other, will be uppermost; and were it practicable to sink to the centre of the earth, all the strata, that are, would be found in every part, and according to the Poet, Ponderibus librata suis. Add to this, that in all places within Mr. S.'s knowledge, the observation of Dr. Stukely has held good, that the precipices of all hills are to the westward, whereas the ascent to the east is more gradual.

The common Experiment for proving French Brandy, shown to be both false and fallacious. By M. Neuman. N° 391, p. 398. *Translated from the Latin.*

Some merchants in Holland, England, Hamburg and Dantzic, who chiefly deal in French brandy, boast of a certain probatory experiment, as a very curious arcanum; they firmly persuade themselves, that by means of this experiment, they can distinguish not only French brandy from malt spirits, but likewise what is genuine from adulterated brandy; and that consequently they cannot be imposed on in buying the genuine sort. Hence it is still considered as an infallible proof, and as a grand secret: so that M. Neuman could not procure, either to smell or taste this proof-liquor.

This liquor is of a duskish yellow tincture; and with it the experiment is made in the following manner; first they fill a glass with the brandy to be proved; and into this glass they put 1, 2 or 3 drops of that liquor, more or less, according to the quantity of brandy used; and if the brandy be good and genuine, there immediately appears at the bottom of the glass, a very beautiful blue colour; which, if stirred, and well mixed with the rest of the brandy, tinges it entirely of an azure; but if it be malt spirit, there is no such tincture to be seen in the glass, this retaining its pristine tincture, though 20 times the number of drops be put into the glass. Therefore as this method of proving does, according to the opinion of the merchants, generally serve to distinguish pure malt spirits from pure French brandy; so, depending on this hypothesis, they judge of the different degrees of the adulteration of brandy and malt spirits; for, they pretend to judge, from observing the blue colour, unless it be obscure, but provided it be a bluish gray or a bluish green, that the brandy is not only adulterated, but likewise with what proportion of malt spirits, more or less, it is mixed.

At first Mr. N. took this to be a certain and infallible method of proof.—But considering that no demonstrative experiment had hitherto been made, by which we might show, or at least suspect, the peculiar constituent parts of pure French brandy, distinct from malt spirits, equally rectified; but that both consist of the same essential parts; to which common quality they are simply reduced by necessary fermentation; he therefore thus reasoned with himself; that if any remarkable difference happened in the spirits, it was owing, neither to the intimate composition that constitutes the brandy itself, nor to the nature of the wine from which it is distilled, but necessarily arises from some heterogeneous additament, no ways constituting brandy, as such; whether it be added in fermentation or distillation; or whether it happen by tinging, extracting, or even by the commixture of other liquid or soluble bodies.

M. Neuman would not acquiesce in such thoughts alone, for the most part precipitate and fallacious; but made several experiments to that purpose; by which he easily had a confirmation of his conjecture, namely, that that apparent difference, by means of the above experiment, is in no manner a true and essential distinction of spirits; but that the production of the blue tincture, by which the difference is judged, is owing to some heterogeneous additament, not requisite to the peculiar constitution of brandy; and consequently, that the whole experiment, however plausible it may appear, is false, fallacious, and useless; which he will farther show below.

M. Neuman understood by a friend, who had tasted this proof-liquor at Dantzic, that it was of a styptic savour: therefore, directing all his experiments this way, he at length found, that it was no other than a mere solution of iron, in a vitriolic acid; whether it consist of iron dissolved in spirit of vitriol, and diluted with water, or of English vitriol, or some venereo martial vitriol, prepared by precipitation, or of an extract of some iron-ore or earth, as the Hessian, though the most elegant blue colour be produced from the last mentioned, namely, with the liquor of Hessian earth; and the more saturated the solution is, the less of it is required for making the experiment.

That additament, which first communicates a yellow tincture to French brandy, and then a blue, in the course of the experiment, is oak-wood, or the shavings or chips of it, infused in brandy, or when the brandy is kept in a new oak-cask, till it has extracted a yellow tincture from the wood; and the more yellow it is, the more blue it becomes by the martial liquor in the experiment, unless it be tinged with saffron, or some other yellow body.

That the whole is owing to nothing but the oak-wood, M. Neuman confirmed by the following experiments: he took malt-spirits, the same to which, when the liquor is put, they neither exhibit a blue tincture, nor undergo any other change; and therefore, reckoned pure malt spirits by the merchants; in these he infused oak-chips, till the spirits had been almost as yellow as French brandy; and after filtration, he poured into them some of the vitriolic liquor, as is done into French brandy, and it produced the same elegant blue tincture, without any the least apparent difference; which abundantly shows, that the change is entirely owing to the oak-wood.

In like manner, may any other substance, resembling oak, as for instance, galls, be infused in malt spirits; the experiment also succeeds in some measure with pomegranate rind, and other astringent vegetables; yet best of all with oak, to which the pomegranate rind is far inferior, as exhibiting rather a violet than a blue colour, and when stirred, appearing somewhat green.

It is worth observing, that a very small quantity of the liquor, extracted from oak, is sufficient to give a blue tincture, as the abovementioned liquor does, to a large quantity of malt spirits; for, with a single drop of that infusion, M. Neuman made the experiment answer in half an ounce of malt spirits.

That the liquor, or solution, should consist and be prepared of pure iron vitriol, and not of that of copper, appears hence. 1. Because the experiment, made with Goslar, Hungary, Dantzic, and all such mixed vitriols, that contain any copper, more or less, does not succeed so well, and they produce a very diluted blue, or grey tincture. 2. Because the experiment does not at all succeed with pure vitriol of copper, nor strike any blue tincture; as M. Neuman found by several experiments to this purpose, as will be shown below.

It should still remain to explain the causes and origin of this blue tincture: but because M. Neuman takes the composition to be the same with new or diluted ink, whose chief ingredients, that produce the tincture, are the same, namely, vitriol of iron, and an astringent vegetable, on which M. Lemery has written a learned and copious dissertation (vide Hist. de l'Acad. des Sciences pour l'An. 1707) to him he refers; and as to the optical and other philosophical reasons, consult Mr. Boyle, Sir Isaac Newton, and other authors who have written on colours.

In fine, M. Neuman briefly observes, that no other difference should be sought for between French brandy and malt spirits, namely, such as are pure and carefully distilled, than the peculiar flavour of French brandy; though the same flavour may be several ways communicated to malt spirits, and thus be adulterated into French brandy: so that the most skilful might take them for genuine brandy, or at least, not for malt spirits; whence it appears, that the abovementioned, and other probatory experiments on spirits, are of no use, or at least insufficient.

An account of the experiments here follows: I. M. Neuman prepared eight solutions of vitriol, taking for each solution 2 drachms of vitriol, and 1½ oz. of common distilled waters. The solutions were, 1. Of Goslar vitriol. 2. Dantzic vitriol. 3. Hungary vitriol. 4. Cyprus vitriol. 5. English vitriol. 6. Of iron prepared with oil of vitriol. 7. Of iron obtained by precipitation from venereo-martial vitriol, and 8. A solution of Hessian earth; of which last he only took 2 drachms to the abovementioned quantity of waters.

II. He made 3 infusions of astringent vegetables; to each ounce of the vegetable putting one pound, apothecary's weight, of malt spirits, which received no blue tincture from the proof-liquor: the infusions were, 1. Oak. 2. Turkish galls. 3. Pomegranate rind.

III. With these three well saturated infusions, he made from malt spirits 3 sorts of adulterated French brandy, at least resembling it in the yellow tincture: and with each 8 oz. of malt spirits, he mixed an ounce of the infusion.

IV. He took the common yellow French brandy, and also common malt spirits, newly distilled, and with both, and the solutions of vitriol, he successively made experiments, in the manner just mentioned.

V. At each time he poured half an ounce of French brandy into a clean glass, narrowing downwards, and from 1 to 4 drops of the solution of vitriol, more or less, as was found sufficient to produce the blue tincture; and carefully viewing it, he observed that the said French brandy had a pale blue tincture communicated to it, from the solution of Goslar, and that of Dantzic vitriol; as also a bluish tincture from Hungary vitriol; from the solution of the Cyprus vitriol it had not a blue but a greenish tincture; from the solution of English vitriol, and from that of vitriol of iron, prepared with oil of vitriol; and likewise, the solution of vitriol of iron, made by precipitation, a very elegant tincture; but it had the most beautiful of all, from the solution of Hessian earth. The reason of this diversity of tinctures has been given above.

VI. Further, for each probatory experiment he took also $\frac{1}{2}$ oz. of malt spirits, and put to them 1, 2, 3, to 10 drops and upwards, of each solution of vitriol; yet he could not observe the least bluish tincture produced.

VII. Afterwards for each experiment he took $\frac{1}{2}$ oz. of the adulterated French brandy of N° III, and dropping into it, as also into common French brandy, some of each of the solutions of vitriol of N° V, the experiments agreed in all respects; only that, on account of the combination of the extracts of the various vegetables, there happened some, though a very inconsiderable, difference, as shall appear from what follows:

VIII. The malt spirits became somewhat black, on dropping into them some of the solution of Goslar and Dantzic vitriol, with an extract of galls; and with the solution of Hungary vitriol, at first a bluish tincture, but on mixing, they entirely lost it; the solution of Cyprus vitriol strikes no tincture at all; they had an elegant blue tincture from the solution of English vitriol; and from that of iron, both ways prepared, they at first had a blue tincture, but on mixing and stirring them with a quill, they got a violet tincture; in fine, a single drop of the solution of the mineral earth of iron gave them a beautiful blue tincture.

IX. Malt spirits, impregnated with an extract of pomegranate rind, and mixed with the solutions of vitriol in the order abovementioned, had little or no blue tincture; and in the four former trials they appeared almost in the same manner, as the abovementioned malt spirits, impregnated with the extract of galls; and in the latter, they immediately appeared of a somewhat green tincture, and presently after somewhat resembling ink.

X. On the contrary, malt spirits, with an extract of oak, somewhat resemble French brandy; and with the solutions of pure martial vitriols, but especially with that from Hessian earth, the most beautiful blue is produced. With the first four solutions of vitriol the appearance was the same as in other counterfeited malt spirits, nay, as in French brandy itself.

XI. At length being sufficiently persuaded, that the experiment succeeded very well with oak shavings, M. Neuman wished to try the least quantity of the infusion of oak requisite to produce some degree of the blue tincture; he tried several quantities, from ½ drachm to 20, 15 drops and downwards; so that one single drop, with a drop at least of the solution of the mineral earth of iron, was sufficient to impregnate the whole half ounce of malt spirits, and to produce a blue tincture, though not so greatly saturated.

Of a Fork put up the Anus, and afterwards extracted through the Buttock. Communicated by Mr. Rob. Payne, Surgeon at Lowestoff. N° 391, p. 408.

James Bishop, an apprentice to a ship-carpenter in Great Yarmouth, about 19 years of age, had violent pains in the lower part of the abdomen, for 6 or 7 months; it did not appear to be any species of the colic; he sometimes made bloody urine, which induced Mr. P. to believe it might be a stone in the bladder. He was very little relieved by physic; at length a hard tumour appeared in the left buttock, on or near the glutæus maximus, 2 or 3 inches from the verge of the anus, a little sloping upwards. A short time after he voided purulent matter by the anus, every day for some time. The tumour at length broke, and shortly after the prongs of a fork appeared through the orifice of the sore, above ½ inch beyond the skin, on which his violent pains ceased. Mr. Payne divided the flesh between the prongs, and made a circular incision about the prongs; then with a strong pair of pincers extracted it, not without great difficulty, handle and all entire. The end of the handle was besmeared with the excrement, when drawn out. It was 6½ inches long, a large pocket-fork; the handle ivory, but died of a very dark brown colour; the iron part is very black and smooth, but not rusty. The patient's account of the matter was, that being costive, he put the fork up his fundament, thinking by that means to help himself, but unfortunately it slipped up so far, that he could not recover it again.

He further says, he had no trouble or pain, till a month, or more, after it was put up.

Two Cases of Insects voided by the Urinary Passage. By Dr. Daniel Turner, Coll. Med. Lond. Lic. N° 391, p. 410.

Nov. 24, 1725, a poor woman came to crave Dr. T.'s advice for a child about 16 months old, bringing with her a worm, which she said the apothecary had just then drawn out of the child's penis, who had for several days before laboured under great uneasiness, which she called convulsions of the bowels.

The child was continually drawing up the lower limbs, and straining at both sphincters; the urine seemed to pass with difficulty for some days, till at last there came on a total suppression, and the worm advancing, showed itself at the extremity of the urethra, when the apothecary was called up to help them. The insect measured above 4 inches, resembling the worms usually voided by the anus, of the earth-worm kind, but whiter. The apothecary said, that, when he came to the child, he saw a preternatural body, which at first he knew not what to make of, hanging $\frac{1}{2}$ an inch out of the glans, and lying double in the passage: perceiving it farther advancing, he took hold of it, and with little difficulty drew it forth.

The summer before the last, a woman showed Dr. T. an insect of the maggot species, with a crusty red galea over the snout, and a crescent or forked tail, which she had just then voided by the urinary passage.

An Account of a New Machine, called the Marine Surveyor, contrived for measuring a Ship's Way in the Sea, more correctly than by the Log, or any other Method hitherto used for that Purpose. By Mr. Henry de Saumarez, of the Island of Guernsey. N° 391, p. 411.

The primum mobile of this machine is in the form of the letter Y, and is made of iron, or any other metal; at each end of the lines, which form the angle, or upper part of that letter, are two pallets not much unlike the figure of the log; one of which falls in the same proportion as the other rises. The falling or pendent pallet meeting a resistance from the water, as the ship moves, has by that means a circular motion under water, which is faster or slower according as the vessel moves. This motion is communicated to a dial within the ship, fixed either in the master's cabin, or any other place, by means of a rope of any convenient length, fastened to the tail of the Y, and carried to the dial. The motion being thus communicated to the dial, which has a bell in it, it strikes exactly the geometrical paces, miles, or leagues, which the ship has run. Thus is the ship's distance ascertained; and with equal ease may the forces of tides and currents be discovered by this instrument.

In fig. 1, pl. 3, AKCL and BHDI are the pallets, worked from the legs DE and CE into the form they appear, to a breadth of about $4\frac{1}{2}$ inches. The length of the pallets BD and AC is 8 inches. The branches or legs, DE and CE, are each $15\frac{1}{2}$ inches long, and 2 in circumference; and the angle CED contained between them, is 45 degrees. The shank EF is of the same thickness as CE and DE, and is 27 inches long. At the point F is a ring, where one end of the rope FG is hooked to the machine, the other end G being fixed to the dial within the ship or vessel. This rope may be about 5 fathoms, more or less, according as the dial is fixed high or low, in respect to the surface of the water.

In the figure this machine has but two branches; however, it may be formed of three, if not four, and adjusted to the same standard or measure; but as three or four branches would be more subject to entangle in sea-weeds, and so prevent the regular motion of the instrument, if not in some measure impede the ship's way, Mr. S. approves of two only; for, in an experiment at sea, he observed those made in this form have been so far from being choaked by weeds, that if they encountered any, they always cleared themselves again, without the trouble of hauling the engine into the ship to do it.

This instrument may be regulated several ways: first, by opening or closing the angle CED; secondly, by lengthening or shortening the branches, or turning or bending more or less the pallets AKCL and BHDI; by which the machine is brought to any standard or measure, to cause the hydraulical revolution to answer either to a geometrical pace of 5 feet, or to 10, 12, 14 feet, &c.

The machines of this kind, which he tried at sea, in all sorts of weather, weighed some 4, others 5, and others 6 pounds; their weight not at all affecting the peculiar property of the instrument, nor hindering the regulation of them. They may be made of tin, as well as iron, and so light as not to weigh above 2 or 3 lb. which may serve for any boat, wherry, barge, &c. without any hindrance to their rowing or sailing. The manner of fixing them to a ship or boat is represented in fig. 2.

Mr. S. explains three several dials, any one of which may be used with this machine. The first had three indexes, one of which marked ten revolutions of the engine, each revolution 10 feet; so that the whole round of the circle was 100 feet. As five of these revolutions make 50 feet, by holding the half-minute glass in one's hand, which is always used with the log-line, by inspection is seen how many times 50 feet the ship runs in half a minute, and of course how many miles an hour, without the trouble of employing four or five persons as usual in heaving the log. The second index on this dial marked 100 revolutions, which makes 1000 feet, as the third index did 1000 revolutions, which is equal to 10,000 feet; and then a little bell struck, signifying when the ship had sailed that distance, which may be also fitted to strike to any other measure.

The second dial had the circle on its plane divided into 12 parts; so that as the index passed each division, the ship had run 1 mile, and consequently 12 miles when it had measured the circumference. On one side of this dial was fixed another plate, which was graduated in such manner, that by the half-minute glass it would tell what the vessel ran in that space of time, &c.

On the third dial were 3 circles: the first was so divided as to show when the ship had run 60 leagues; the second, when the ship had run the same distance in miles; and on the third was marked 120 knots; so that, computing each knot at 50 feet, the circumference was 6000 feet, the standard of an English maritime mile, or the 60th part of a degree on the equator, in running which length, the instrument has just 600 revolutions; to which distance a little bell strikes, to give notice to the man at the helm, of the distance sailed in that time.

Besides the several circles on this dial, were also two plates on each side, having two circles, one divided into 100 leagues, and the other into 300 miles; so that, without hearing the bell strike to every mile or league, one might at any time see by them, what number of miles or leagues the ship had run, from the time she had left her port. As to the materials within the dial, there is little more than common clock-work.

As by this machine Mr. S. undertakes to correct the errors of the log, he states the particulars of a comparison between that instrument and his invention. He remarks on the inaccuracy of estimating the time by the half-minute or quarter-minute glass; on the uncertainty of the log floating quite upright, and dragging, as well as in heaving it over and veering out the line; then the uncertainty from the line itself, being at times longer or shorter, from drought or moisture: also the uncertainty arising from the changeable velocity of the ship, faster or slower, in the intervals between the times of heaving the log, by the variable state of the winds, &c. From all which irregularities, Mr. S. asserts his instrument is quite free and exempt. He concludes with subjoining the testimonials of its accuracy and usefulness by several experienced seamen, who had made trial of his machine.

END OF VOLUME THIRTY-THIRD OF THE ORIGINAL.

De Salibus Alcalino-fixis, Auctore C. Neuman, Chem. Prof. Reg. Berolin, R. S. S. N° 392, p. 1. *Vol. XXXIV.*

In this, and the subsequent dissertation, the author treats at great length of the production, properties, and uses of fixed alkaline salts; but as the fullest information respecting these particulars is to be found, not only in this author's chemistry translated into English, but in every modern system of chemistry, it is deemed unnecessary to insert even an abstract of these dissertations.

Observations of the Eclipse of the Moon, Oct. 10, 1725, *made at Bristol. By John Burroughs, Esq.* N° 392, p. 37.

The cloudy weather prevented seeing the beginning of the eclipse, or of total darkness, but Mr. B. observed, pretty exactly, the first appearance of light after the total darkness, and the end of the eclipse: and their respective times are as follow, viz.

Beginning of light	7h	31m	20s	apparent time.
End of the eclipse	8	29	30	

Some small time before the renewal of the true light, there appeared a remarkable brightness on the eastern limb of the moon, which was also diffused about the edge of the moon, to a sensible distance. If others, who are more skilled in these affairs, have made the like observation, Mr. B. will no longer doubt of the moon's having an atmosphere.

Part of two Human Skeletons petrified. Communicated in a Letter from John James Scheuczer, M. D. F. R. S. to Sir Hans Sloane, Bart. Vice Pres. of the Royal Society. N° 392, p. 38. *An abstract from the Latin*

Of these two petrifactions, which were dug out of a quarry at Oeningen, one exhibited part of a human skull, with the seven vertebræ of the neck; the other a portion of another skull, part of the bones of the face, and 16 vertebræ in a continued series, with the transverse processes to most of them; also fragments of both clavicles.

A Contrivance to avoid the Irregularities in a Clock's Motion, occasioned by the Action of Heat and Cold on the Pendulum Rod. By Mr. George Graham, Watch-maker, F. R. S. N° 392, p. 40.

As several persons, who have been curious in measuring time, have taken notice that the vibrations of a pendulum are slower in summer than in winter; and have very justly supposed this alteration has proceeded from a change of length in the pendulum itself, by the influences of heat and cold upon it, in the different seasons of the year; with a view therefore of correcting, in some degree, this defect of the pendulum, Mr. G. made several trials, about the year 1715, to discover whether there was any considerable difference of expansion between brass, steel, iron, copper, silver, &c. when exposed to the same degrees of heat, as nearly as could be determined; conceiving it would not be very difficult, by making use of two sorts of metal, differing considerably in their degrees of expansion and contraction, to remedy, in great measure, the irregularities to which common pendulums are subject. But although it is easily discoverable, that all these metals suffer a sensible alteration of their dimensions by heat and cold; yet he found their differences, in quantity from each other, were so small, as gave no hopes of succeeding this way; and made him leave off prosecuting this affair any farther at that time. In the beginning of December, 1721, having occasion for an exact level, besides other materials Mr. G. made trial of, quicksilver was one; which, though he found it was by no means proper for a level, yet the extraordinary degree of expansion observed in it, when placed near the fire, beyond what he had conceived to be in so dense a fluid, immediately suggested the use that might be made of it, by applying it to a pendulum.

In a few days after, he made the experiment, but with much too long a column of quicksilver, the clock going slower with an increase of cold, contrary to the common pendulum; however, it was a greater confirmation of the advantage to be expected from it, since it was easy to shorten the column in any degree required. The only doubt entertained, was, lest there should not be a proportional expansion and contraction between the quicksilver, and the rod of the pendulum, through the various degrees of heat and cold, from the one extreme to the other. To make this experiment the more convincing, he placed the clock in a part of the house the most exposed of any to the changes of heat and cold, the room having no fire in it in the winter, and exposed to a south sun, with leads above it, which in the summer made it extremely hot. He hung a thermometer by it, and had also another clock at no greater distance from it than was necessary to keep the cases from touching each other. This clock had been made some years before, with extraordinary care, having a pendulum above 60 pounds in weight, and not vibrating above one degree and a half from the perpendicular; and which, in a more temperate situation, had not altered above 12 or 14 seconds in 24 hours, between winter and summer; but in this place it altered 30ˢ a day, between the hottest and coldest weather, in the year 1722, a year no way remarkable for either extremes. But this great alteration was owing to the situation abovementioned, and which was chosen to make the experiment the more sensible.

The two clocks being firmly screwed to a party-wall, Mr. G. began to make the first trial of this kind of pendulum, December 18, 1721; and by Jan. 3, perceiving the column of quicksilver considerably too long, he procured a shorter glass, which he made use of till the beginning of June following: by which time he was well satisfied of the advantage of the contrivance, though both these pendulums were but rudely executed, and this last had the pillar of quicksilver too short, but much nearer the true length than the first. This encouraged him to provide another glass, a little longer than the last, and to bestow more care on all the parts of the pendulum that required exactness. This being finished, by the 9th of June, he began then to observe the motion of the clock by the transits of the fixed stars, as often as the weather permitted, making use of a telescope which moved in the plane of the meridian; with this instrument he could be sure of not erring above 2 seconds in time. The clock was kept constantly going, without having either the hands or pendulum altered, from the 9th of June, 1722, to the 14th of October, 1725, being 3 years and 4 months.

For the first year, Mr. G. wrote down every day, the difference between the two clocks, with the height of the thermometer, not omitting the transits of the stars, as often as it was clear. The result of all the observations was this, that the irregularity of the clock with the quicksilver pendulum, compared with the transits of the stars, exceeded not, when greatest, a 6th part of that of the other clock with the common pendulum; but for the greatest part of the year, not above an 8th or 9th part; and even this quantity would have been lessened, had the column of mercury been a little shorter; for it differed a little the contrary way from the other clock, going faster with heat, and slower with cold; but no alteration was made in the length, to avoid an interruption of the observations. To confirm this experiment the more, about the beginning of July, 1723, Mr. G. took off the heavy pendulum from the other clock, and made another with quicksilver, but with this difference, that instead of a glass tube, he made use of brass, and varnished the inside, to secure it from being injured by the mercury. This pendulum he has made use of ever since, and finds it about the same degree of exactness as the other. The reason why this kind of pendulum is more exact than the common sort, will be evident to any one who considers, that as heat lengthens the rod of the pendulum, at the same time it increases the length of the column of quicksilver, and its centre of gravity is moved upwards: and when by cold, the rod of the pendulum is shortened, the column of quicksilver is likewise shortened, and its centre of gravity carried downwards: by this means, if the column of quicksilver be of a proper length, the distance between the point of suspension and the centre of oscillation of the pendulum, will be always nearly the same, on which the exact motion of a clock principally depends. Were the pendulum of a clock to remain invariably of the same length, yet some little inequalities would appear in its motion, from the difference of friction, arising from the imperfections of the materials, as well as different degrees of foulness: on which account, the force communicated to the pendulum would not be constantly equal, which would cause some small alteration. But when the pendulum is very heavy, and vibrates in a small arch, and the workmanship of all the parts is well performed, there will be very little inequality in the motion, besides what proceeds from heat and cold.

In making use of quicksilver for a pendulum, by varying the diameter of the vessel that contains it, or the thickness of the rod of the pendulum, whether it be of brass or steel, they may be reduced nearly to an equality as to the receiving, or retaining the impressions of heat or cold, on which the regularity of the motion chiefly depends; and particular care ought to be used to free the mercury from all blebs of air, otherwise their great and sudden expansion, or contraction, may cause a considerable disorder; but the air may as easily be excluded in this way, as in a barometer, and the great specific gravity of quicksilver renders it a proper material for the weight of a pendulum.

De Salibus Alcalino-fixis, Auctore C. Neuman Chym. Prof. Reg. Berolin, R.S.S. Pars reliqua. N° 392, p. 45.

See remark at p. 128 of this Vol. of these Abridgments.

Several Celestial Observations maae at Southwick, in the County of Northampton. By George Lynn, Esq. N° 393, p. 66.

The following observations were made at Southwick, long. west from London, 30′, lat. 51° 58′ nearly, with a 13-foot telescope, whose aperture was 2.4 inches, and charge 2.5 inches; all by apparent time.

In 1724, Nov. 8, 7^h 37^m 7^s, the first satellite of Jupiter began to emerge: the same day at 6^h 24^m 20^s, the third satellite began to immerge.

In 1725, July 21, 10^h 43^m 20^s, the third satellite immerged, that is Mr. L. lost sight of it, at a little above a semidiameter from Jupiter; but it began sensibly to abate of its light above 3 minutes before.

August 9th, 11^h 51^m 20^s, Mr. L. lost sight of the second satellite; but it began sensibly to abate of its light about 2 minutes before.

August 18th, 25^m 50^s, the first satellite immerged very near Jupiter's body.

Oct. 11th, 6^h 21^m 45^s, the third satellite began to emerge, and was full 3½ minutes before it was at its greatest lustre. It came out of the shadow about half a diameter from Jupiter's edge.

Dec. 26th, 5^h 51^m 12^s, the second satellite began to emerge.

1725-6, Jan. 5th, 6^h 28^m 30^s, the third satellite began to emerge.

1724, June 23d, 10^h 15^m, Saturn followed a star (in Senex's Zodiac, but without any distinguishing mark) 51½^s of right ascension in time, and declined from it south 40″.

June 25th, 10^h 0^m, Saturn followed the same star, 13^s of right ascension in time, and declined from it south, 3″ or 4″ only.

1725, Dec. 17th, $8^{h}\ 0^{m}$, Jupiter preceded φ Aquarii $4\frac{1}{2}^{s}$ of right ascension in time, and declined from it south 11′ 45″.

An Extraordinary high Tide in the River Thames, observed by Capt. Tho. Jones.

N° 393, p. 68.

March the 8th, 1725-6, the tide in the River Thames, at New Crane in Shadwell, flowed 20 feet $5\frac{1}{2}$ inches, taken by a level, from that high-water mark, to low-water the next morning, and was 4 inches higher than has been known these 40 years.

Observations on the Tides in the River Thames. By Mr. Henry de Saumarez.

N° 393, p. 68.

That the use of Mr. S.'s instrument called the marine surveyor, described in N° 391, may yet further appear, he here gives some experiments, made with it on the River Thames, in order to determine the strength of the tides of flood and ebb. Were the same to be done in the channel, and on the sea-coast of Great Britain, and marked in the charts, it would be of no small advantage to commerce, and a sufficient recommendation of the marine surveyor, if that alone were the use of it.

A TABLE showing the strength and gradual increase and decrease of the Tides of Flood and Ebb in the River Thames, as observed in Lambeth Reach, off Manchester Stairs, and in the middle of the river, with a new instrument called the Marine Surveyor, on the 9th of June, 1720; it being then full moon, and consequently a spring tide. The movement of the machine 14 inches under water.

FLOOD.											
The Time of Flood.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times express'd in the first Column.	The same reduced to Statute Miles of 5280 feet, or 528 Revolutions of the Machine.			The Reduction into English maritime Miles of 6000 Feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
0	15	5	0	110	110	0	0	11	0	0	11
0	30	6	0	590	700	0	0	70	0	0	70
0	45	6	9	1100	1800	0	$\frac{1}{4}$	48	0	$\frac{1}{4}$	30
1	0	7	0	1490	3290	0	$\frac{1}{2}$	55	0	$\frac{1}{2}$	29
1	15	8	0	1870	5160	0	$\frac{3}{4}$	120	0	$\frac{3}{4}$	66
1	30	9	0	2230	7390	1	$\frac{1}{4}$	79	1	0	139
1	45	10	0	2500	9890	1	$\frac{3}{4}$	65	1	$\frac{1}{2}$	89
2	0	11	6	2660	12550	2	$\frac{1}{4}$	67	2	0	55
2	15	13	0	2730	15280	2	$\frac{3}{4}$	76	2	$\frac{1}{2}$	28
2	30	14	0	2740	18020	3	$\frac{1}{4}$	86	3	0	2
2	45	14	9	2720	20740	3	$\frac{3}{4}$	94	3	$\frac{1}{4}$	124
3	0	14	9	2570	23310	4	$\frac{1}{4}$	87	3	$\frac{3}{4}$	81
3	15	14	10	2220	25530	4	$\frac{3}{4}$	45	4	$\frac{1}{4}$	3
3	30	14	9	1820	27350	5	0	66	4	$\frac{1}{2}$	35
3	45	14	0	990	28340	5	$\frac{1}{4}$	62	4	$\frac{1}{2}$	134
3	50	13	9	130	28470	5	$\frac{1}{4}$	75	4	$\frac{1}{2}$	147

EBB.											
The Time of Ebb.		The Depth of the River.		The Run of the Current in every 15 Min.	The whole Run of the Current to the Times expressed in the first Column.	The same reduced to Statute Miles of 5280 feet, or 528 Revolutions of the Machine.			The Reduction into English maritime Miles or 6000 Feet, or 600 Revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
0	15	12	9	280	280	0	0	28	0	0	28
0	30	12	3	1140	1420	0	¼	10	0	0	142
0	45	11	10	1900	3320	0	½	68	0	½	32
1	0	11	4	2080	5400	1	0	12	0	¾	90
1	15	11	2	2120	7520	1	¼	92	1	¼	2
1	30	10	9	2120	9640	1	¾	40	1	½	64
1	45	10	4	2170	11810	2	0	125	1	¾	131
2	0	10	0	2130	13940	2	½	74	2	¼	44
2	15	9	6	2060	16000	3	0	16	2	½	100
2	30	9	4	2040	18040	3	¼	88	3	0	4
2	45	9	0	2020	20060	3	¾	26	3	¼	56
3	0	8	9	1910	21970	4	0	85	3	½	97
3	15	8	6	1900	23870	4	½	11	3	¾	137
3	30	8	3	1910	25780	4	¾	70	4	¼	28
3	45	8	0	1860	27640	5	0	124	4	½	64
4	0	7	9	1810	29450	5	½	41	4	¾	95
4	15	7	3	1780	31230	5	¾	87	5	0	123
4	30	7	0	1690	32920	6	0	124	5	¼	142
4	45	6	6	1620	34540	6	½	22	5	¾	4
5	0	6	3	1570	36110	6	¾	47	6	0	11
5	15	6	3	1570	37680	7	0	72	6	¼	18
5	30	6	0	1570	39250	7	¼	97	6	½	25
5	45	6	0	1560	40810	7	½	121	6	¾	31
6	0	5	9	1550	42360	8	0	12	7	0	36
6	15	5	6	1500	43860	8	¼	30	7	¼	36
6	30	5	3	1460	45320	8	½	44	7	½	32
6	45	5	0	1450	46770	8	¾	57	7	¾	27
7	0	4	9	1430	48200	9	0	68	8	0	20
7	15	4	6	1400	49600	9	¼	76	8	¼	10
7	30	4	3	1380	50980	9	½	82	8	¼	148
7	45	4	3	1340	52320	9	¾	84	8	½	232
8	0	4	0	1270	53590	10	0	79	8	¾	109
8	5	3	10	420	54010	10	0	121	9	0	1
8	10	3	11	410	54420	10	¼	30	9	0	42
8	15	4	0	400	54820	10	¼	70	9	0	82
8	20	4	0	380	55200	10	¼	108	9	0	120
8	25	4	2	300	55500	10	½	6	9	¼	0
8	30	4	2	270	55770	10	½	33	9	¼	27
8	35	4	3	130	55900	10	½	46	9	¼	40
8	40			Stagnant	Stagnant						

A TABLE, showing the strength and gradual increase of the tides of flood and ebb in the river Thames, as observed in Lambeth Reach, off Manchester stairs, and in the middle of the river, with a new instrument called the marine surveyor, on the 18th of June 1720: it being then the last quarter of the moon, and consequently a neap tide. The movement of the machine 14 inches under water,

The time of flood.		The depth of the river.		The run of the current in every 15 minutes.	The whole run of the current to the times expressed in the 1st co	The same reduced to statute miles of 5280 feet, or 528 revolutions of the machine.			The reduction into English maritime miles of 6000 feet, or 600 revolutions.		
H.	M.	Ft.	In.	Feet.	Feet.	M.	Pts.	Rev.	M.	Pts.	Rev.
				FLOOD.							
0	15	4	0	220	220	0	0	22	0	0	22
0	30	4	3	520	740	0	0	74	0	0	74
0	45	4	9	900	1640	0	¼	32	0	¼	14
1	0	5	3	1030	2670	0	½	3	0	¼	117
1	15	5	9	1020	3690	0	½	105	0	½	69
1	30	6	1	1160	4850	0	¾	89	0	¾	35
1	45	7	0	1450	6300	1	0	102	1	0	30
2	0	7	9	1640	7940	1	½	2	1	0	44
2	15	8	1	1830	9770	1	¾	53	1	½	77
2	30	9	0	1920	11690	2	0	113	1	¾	119
2	45	9	6	2070	13760	2	½	56	2	¼	26
3	0	10	0	2170	15930	3	0	9	2	½	0
3	15	10	4	2070	18000	3	¼	84	3	0	0
3	30	11	3	1960	19960	3	¾	16	3	¼	46
3	45	11	4	1890	21850	4	0	73	3	½	85
4	0	11	9	1700	23550	4	¼	111	3	¾	105
4	15	11	6	1300	24850	4	½	109	4	0	85
4	30	11	0	730	25580	4	¾	50	4	¼	8
4	35	11	0	70	25650	4	¾	57	4	¼	15
4	40	11	0	stagnant	stagnant						
4	45	10	10	ditto	ditto						
4	50	10	9	ditto	ditto						
The time of ebb.				EBB.							
0	15	10	6	610	610	0	0	61	0	0	61
0	30	10	0	1340	1950	0	¼	63	0	¼	45
0	45	9	9	1520	3470	0	½	83	0	½	47
1	0	9	3	1650	5120	0	¾	116	0	¾	62
1	15	9	0	1750	6870	1	¼	27	1	0	87
1	30	8	6	1730	8600	1	½	68	1	¼	110
1	45	8	0	1700	10300	1	¾	106	1	½	130
2	0	7	9	1710	12010	2	¼	13	2	0	1
2	15	7	3	1710	13720	2	½	52	2	¼	22
2	30	7	1	1710	15430	2	¾	91	2	½	45
2	45	6	9	1710	17140	3	0	130	2	¾	64
3	0	6	7	1680	18820	3	½	34	3	0	82
3	15	6	4	1670	20490	3	¾	69	3	¼	99
3	30	6	0	1570	22060	4	0	94	3	½	106
3	45	5	9	1500	23560	4	¼	112	3	¾	106
4	0	5	8	1480	25040	4	½	128	4	0	104
4	15	5	3	1440	26480	5	0	8	4	¼	98
4	30	5	2	1430	27910	5	¼	19	4	½	91
4	45	5	0	1420	29330	5	½	29	4	¾	83
5	0	5	0	1430	30760	5	¾	40	5	0	76
5	15	4	10	1420	32180	6	0	50	5	¼	68
5	30	4	6	1430	33610	6	¼	61	5	½	61
5	45	4	4	1420	35030	6	½	71	5	¾	53
6	0	4	1	1380	35410	6	¾	77	6	0	41
6	15	3	11	1360	37770	7	0	81	6	¼	27
6	30	3	11	1340	39110	7	¼	83	6	½	11
6	45	3	10	1230	40340	7	½	74	6	½	134
7	0	3	10	1070	41410	7	¾	49	6	¾	91
7	15	3	11	530	41940	7	¾	102	6	¾	144
7	20	4	0	20	41960	7	¾	104	6	¾	145
7	25	4	0	stagnant	stagnant						
7	35	4	3	ditto							

A New and exact TABLE, collected from several Observations, taken in four Voyages to Hudson's Bay in North America, from London: showing the Variation of the Magnetical Needle, or Sea Compass, in the Way to the said Bay, according to the several Latitudes and Longitudes, from the Year 1721, to 1725. By Mr. Christopher Middleton. No. 393, p. 73.

Lat.	Long.		Varia.		Lat.	Long.		Varia.		Lat.	Long.		Varia.		Lat.	Long.		Varia.	
50°	12°	0′	14°	0′	59°	21°	0′	20°	15′	58°	32°	15′	25°	0′	58°	43°	30′	30°	0′
51	12	0	14	15	50	23	15	19	0	59	32	15	25	15	59	43	30	30	15
52	12	0	14	30	51	23	15	19	15	50	34	30	24	0	51	46	0	29	0
53	12	0	14	45	52	23	15	19	30	51	34	30	24	15	52	46	0	29	15
54	12	0	15	0	53	23	15	19	45	52	34	30	24	30	53	46	0	29	30
55	12	0	15	15	54	23	15	20	0	53	34	30	24	45	54	46	0	29	45
56	12	0	15	30	55	23	15	20	15	54	34	30	25	0	55	46	0	30	0
57	12	0	15	45	56	23	15	20	30	55	34	30	25	15	56	46	0	30	15
58	12	0	16	0	57	23	15	20	45	56	34	30	25	30	57	46	0	30	30
59	12	0	16	15	58	23	15	21	0	57	34	30	25	45	58	46	0	30	45
50	14	15	15	0	59	23	15	21	15	58	34	30	26	0	59	46	0	31	0
51	14	15	15	15	50	25	30	20	0	59	34	30	26	15	52	48	30	30	0
52	14	15	15	30	51	25	30	20	15	50	36	45	25	0	53	48	30	30	15
53	14	15	15	45	52	25	30	20	30	51	36	45	25	15	54	48	30	30	30
54	14	15	16	0	53	25	30	20	45	52	36	45	25	30	55	48	30	30	45
55	14	15	16	15	54	25	30	21	0	53	36	45	25	45	56	48	30	31	0
56	14	15	16	30	55	25	30	21	15	54	36	45	26	0	57	48	30	31	15
57	14	15	16	45	56	25	30	21	30	55	36	45	26	15	58	48	30	31	30
58	14	15	17	0	57	25	30	21	45	56	36	45	26	30	59	48	30	31	45
59	14	15	17	15	58	25	30	22	0	57	36	45	26	45	53	51	0	31	0
50	16	30	16	0	59	25	30	22	15	58	36	45	27	0	54	51	0	31	15
51	16	30	16	15	50	27	45	21	0	59	36	45	27	15	55	51	0	31	30
52	16	30	16	30	51	27	45	21	15	50	39	0	26	0	56	51	0	31	45
53	16	30	16	45	52	27	45	21	30	51	39	0	26	15	57	51	0	32	0
54	16	30	17	0	53	27	45	21	45	52	39	0	26	30	58	51	0	32	15
55	16	30	17	15	54	27	45	22	0	53	39	0	26	45	59	51	0	32	30
56	16	30	17	30	55	27	45	22	15	54	39	0	27	0	60	51	0	32	45
57	16	30	17	45	56	27	45	22	30	55	39	0	27	15	54	54	0	32	0
58	16	30	18	0	57	27	45	22	45	56	39	0	27	30	55	54	0	32	15
59	16	30	18	15	58	27	45	23	0	57	39	0	27	45	56	54	0	32	30
50	18	45	17	0	59	27	45	23	15	58	39	0	28	0	57	54	0	32	45
51	18	45	17	15	50	30	0	22	0	59	39	0	28	15	58	54	0	33	0
52	18	45	17	30	51	30	0	22	15	50	41	15	27	0	59	54	0	33	15
53	18	45	17	45	52	30	0	22	30	51	41	15	27	15	60	54	0	33	30
54	18	45	18	0	53	30	0	22	45	52	41	15	27	30	61	54	0	33	45
55	18	45	18	15	54	30	0	23	0	53	41	15	27	45	54	57	0	33	0
56	18	45	18	30	55	30	0	23	15	54	41	15	28	0	55	57	0	33	15
57	18	45	18	45	56	30	0	23	30	56	41	15	28	15	56	57	0	33	30
58	18	45	19	0	57	30	0	23	45	57	41	15	28	30	57	57	0	33	45
59	18	45	19	15	58	30	0	24	0	58	41	15	28	45	58	57	0	34	0
50	21	0	18	0	59	30	0	24	15	59	41	15	29	0	59	57	0	34	30
51	21	0	18	15	50	32	15	23	0	50	43	30	28	0	60	57	0	35	0
52	21	0	18	30	51	32	15	23	15	51	43	30	28	15	61	57	0	35	30
53	21	0	18	45	52	32	15	23	30	52	43	30	28	30	55	60	0	34	0
54	21	0	19	0	53	32	15	23	45	53	43	30	28	45	56	60	0	34	30
55	21	0	19	15	54	32	15	24	0	54	43	30	29	0	57	60	0	35	0
56	21	0	19	30	55	32	15	24	15	55	43	30	29	15	58	60	0	35	30
57	21	0	19	45	56	32	15	24	30	56	43	30	29	30	59	60	0	36	0
58	21	0	20	0	57	32	15	24	45	57	43	30	29	45	60	60	0	36	30

Lat.	Long.	Varia.	Lat.	Long.	Varia.	Lat.	Long.	Varia.	Lat.	Long.	Varia.
61°	60°	37° 0′	63°	66°	39° 40′	62°	75°	43° 0′	62°	86°	35° 0′
57	63	35 0	60	69	41 0	62	75	45 0	59	88	28 0
58	63	35 30	61	69	41 40	63	81	43 0	60	88	28 40
59	63	36 0	62	69	42 20	64	81	46 0	61	88	29 20
60	63	36 30	60	72	40 0	62	82	39 0	57	90	24 0
61	63	37 0	61	72	42 0	63	82	44 0	58	90	24 30
62	63	37 30	61	72	42 40	61	84	33 45	59	90	25 0
59	66	37 0	62	78	43 0	62	84	40 0	57	94	23 0
60	66	37 40	63	78	44 0	63	84	42 0	58	95	22 30
61	66	38 20	63	78	46 0	60	86	30 0	59	95	21 0
62	66	39 0	61	75	38 0	61	86	33 0			

From Long. 68 Deg. to 81, is in Hudson's Straits, where is the greatest Variation, and the Compass will hardly Traverse.

An Account of several Experiments concerning the Running of Water in Pipes, as it is retarded by Friction and intermixed Air: some of which were made before the Royal Society. With a Description of a new Machine, by which Pipes may be cleared of Air, as the Water runs along, without Stand-Pipes, or the Help of any Hand. By the Rev. J. T. Desaguliers, LL. D. F. R. S. N° 393, p. 77.

Having found by several experiments in small, that through a long pipe, water would not be discharged in the same quantity, by a great deal, as it would be through a shorter of the same bore, the orifice being at the same depth under the surface of the water in a reservoir. Dr. D. made an experiment on a pipe above 1000 yards in length, and of 1¾ inch bore; and found that the quantity of water given was much less (he thinks $\frac{11}{12}$ less) than it ought to have been according to Mons. Mariotte's rules; and that something more than the friction, on account of the length of the pipe, had retarded the water; which he since found to be air confined in the eminent parts of the pipe. A full account of this experiment the Dr. published in his Notes on Mariotte's Mouvement des Eaux, in the English translation, some years since.

Considering this matter again lately, the Dr. made the following experiment. In fig. 3, pl. 3, A is a vessel containing a cubic foot in the inside, and kept always full by means of the pipe B running from a larger vessel. CD is a short pipe, of ¾ of an inch bore, 2 feet in length, opening into the bottom of the cistern A, and whose orifice D is always 10 inches below the bottom of A. And OGEEEHF is another pipe, of the same bore, whose orifice F is likewise 10 inches below the bottom of A. This pipe is 113 yards long, lying along the ground 5 feet below A, except the depending part OG, and the ascending part HF.

When F is stopped, and (A being kept full) the water runs out at D, the quantity of water given is 19 times more than when D is stopped, and the water runs out at F. The air confined in several parts of the long pipe is the chief reason of this difference.

In order to get rid of the air which, lodging in the pipe, contracts its bore, and thereby lessens the quantity of water, which is to be delivered at the issue, the Doctor made several experiments to find whereabouts the air lodges, the more easily to let it out; one of which was as follows. He took a glass pipe AB, fig. 4, of about one inch in diameter, 12 feet in length from P to P; only the parts AP and PB, at the other end, were of lead. Then pouring in water at A, till it came up to B, stopping the end G, the air lodged in the eminent parts of the pipe at the places marked CC, DD, and EE: but when the water was suffered to go out at G, the air came forward towards G, and took up the spaces cc, dd, and ee, contracting the bore of the pipe as before, but stood forwarder in the pipe, so that it generally happened that the space of air began on the upper part of the eminence of the pipe.

The glass pipe may be made of several pieces joined to each other, and to the leaden pipes and funnels, by brass ferrels and elbows, turning in all manner of angles. These are not represented here. When the velocity of the water is very great, the air will go even beyond the eminence of the pipe.

To let out the air from the conduct pipes, which obstructs the running of the water, Dr. D. recommends the experiments which he made, and the apparatus he applied to a wooden conduct pipe, of 9 inches bore, which runs a mile and a half from the water engine at York-Buildings, to a reservoir near Cavendish-square; the surface of the water in the cistern at the water-house being sometimes 15, and sometimes 20 feet, above the issue at the reservoir.

On a part of the pipe, such as AB, fig. 5, the Doctor fixed a leaden pipe DF of 2 inches bore, by means of 3 ferrels, or short communication-pipes, the first at D, just beyond the beginning of the space CC, that used to be filled with air in the running of the water, the second in the middle of the leaden pipe, and the third at the end of it; the length of the pipe itself being from 12 to 24 feet, according to the steepness of the descent, the shortest pipe being sufficient where the descent is very quick. From the middle of the leaden pipe abovementioned, called a rider, from its being laid along on the main or conduct pipe, there goes another leaden pipe as EH, of the same diameter, rising all the way very gently from E to the cock H, and so on to I; because, if there was the least descent, water would lodge in it.

Now when the water runs from A to B, the first ferrel D will catch the air as it runs, so as to let it out at I, if the cock H be open, sometimes without going to G or to C. But if the cock had not been opened, till the water had passed through the part AB of the pipe, the air would lodge in the space CC, and be discharged on the opening of the cock. After the cock has been shut, when no more air comes, and water succeeds, after some time, air will extricate itself out of the water, and come up to CC; or if it comes from the parts of the pipe towards B, it will rise contrary to the current of the water, quite up to C, and so go out at the pipe EH, when the cock is opened again.

As, after the first discharge of the air, it cannot be known when more air is got into the pipe, unless by opening the cock, which would require one man to attend each cock constantly, and occasion a waste of water at every turn of the cock, unless when air happens to be in the pipe; it was proposed to contrive a valve that should open to let out the air, and shut again when the water came; and an inverted brass clack or valve shutting upwards, and falling down by its own weight, with cork fixed to the under-side of it to help it to rise when the water came, was mentioned as fit for the purpose by some of the persons present. But we rejected that proposal; because, when such a valve has been shut some time, if air should extricate itself from the water, it would be dense air, whose force being equal to that of a column of water 30, 60, 80 or more feet in height, it would keep the valve shut as well as the water did before, though the air at first could not shut the said valve.

At last, after several thoughts, we contrived a machine which exactly answers the purpose, and is very simple; therefore it will be of general use. The description of it is as follows: in fig. 6, G is a section of the main or conduct-pipe, with water up to G, and air above it, AB being a horizontal line touching the top of the said pipe: EHI is the leaden pipe described above, and marked with the same letters as in fig. 5, reaching from the pipe in the street to the side of a house, or to the side of one of the posts set up to keep off coaches from the foot-way. The machine is the box K, made of cast-iron, fixed to the leaden pipe at I, with a thin door of plate-iron, moving on hinges, and made to lock at D. This box stands in the street out of the way of passengers, with its bottom fixed to a plank in the pavement, so as not to be damaged by a small shock or any chance blow.

The several parts of the machine are the following. In fig. 7, NN is an iron plate, about an inch thick, with 4 holes at 1, 2, 3, 4, of about an inch diameter, quite through the plate, to let through 4 screws, such as a; OO is a face, or flat ring, raised out of the whole stuff, and prominent about $\frac{1}{4}$ of an inch, ground, or turned to a true flat. 5. Is a hole of about 1½ inch diameter, to receive the nose of a cock, which is put through it, stopping with a shoulder or flaunch screwed within the circle oo by 4 other screws, marked with large points round the hole 5.

In fig. 8, NN is the same plate seen edge-wise. M is the air-cock, screwed to the said plate through the flaunch of its pipe at mn, having its key 6, 10, fastened to a rod of about ½ an inch diameter, of the figure 6, 7, 8, 10, having a shank one foot long, 8, 9, joined to a buoy or hollow copper ball L, which ball, when the said shank is in an horizontal position, keeps the cock shut; but falling by its own weight, when not sustained by the water, opens the cock by means of the rod 8, 9, as may be seen in fig. 9, where the plate NN is screwed to the box, and the pricked line ML shows the surface of the water coming into the box through the great cock and leaden pipe HI, so as to make the ball L float with its shank in the horizontal position 8, 9; but when more air comes in to drive the water down the pipe I, the buoy will fall to l, and its shank coming down to 10, 11, will open the air cock M, and let out the air, be its density what it will, till it be all discharged, and the water is again got up to ML, and has raised up the buoy to L. NN is the fore-part of the box, with its hole, to which the plate of fig. 7 is screwed.

It is easily conceived, that the cock H must always be left open; that the end of the pipe I is screwed to a hole in the bottom of the box by means of screws at rr; that there are oiled leathers at the heads of all the screws, and likewise on the plate NN, to make the face oo of fig. 7, apply itself close to the fore-part of the box K, fig. 10, which has a hole at oo, to take in the buoy and cock of figure 8, the screws at 1, 2, 3, 4, which have their heads within the box, and their nuts, such as b, fig. 7, screwed on, when the plate NN is applied; and that the whole box thus fitted, is made air-tight.

D in fig. 6, and DD in fig. 9, represent an iron door, to cover the mouth of the air-cock from external injury, and is punched full of holes to let out the air freely.

This machine, which, from its make, was called a jack in a box, will be useful wherever water is to be conveyed a great way in pipes; and since the Doctor was not the sole contriver of it, in justice to those who joined their thoughts with him, he acquaints the public, that the box is the joint invention of Mr. Richard Jones, Mr. James King, Mr. Thomas Newcomen, Mr. Joseph Horn blower, his operator, and himself.

The Longitude of Lisbon, and the Fort of New York, from Wansted and London, determined by Eclipses of the First Satellite of Jupiter. By the Rev. Mr. James Bradley, M. A. Astron. Prof. Savil. R. S. S. N^{o} 394, p. 85.

The immersion of the first satellite was observed by Mr. Pound at Wansted, with Mr. Hadley's reflecting telescope, August 4, N. S. 1725, about 45^s after the time of the immersion, as calculated from his tables. By another observation made August 29, N. S. the true immersion preceded the calculation from the same tables $1^m\ 10^s$. So that in 25 days the satellite's motion was accelerated as much as answered to $1^m\ 55^s$ in time. Supposing therefore the acceleration to have been in the same proportion between July 28, and August 4, N. S. then the true immersion July 28, N. S. would have happened at Wansted about $1^m\ 15^s$ after the time by the tables; which make the immersion at $12^h\ 48^m\ 45^s$ app. time. The true immersion therefore was at Wansted, July 28, N. S. $12^h\ 50^m\ 0^s$ app. time; and at Lisbon it was observed at $12^h\ 12^m\ 26^s$ app. time, the difference being $37^m\ 34^s$.

Sept. 28, N. S. the first satellite was seen emerging in the reflector at Wansted $3^m\ 50^s$ sooner than the tables make the emersion; and by the mean of two more observations made at the same place, and with the same telescope, on the 14th and 16th of October, N. S. the true emersion preceded the calculation $4^m\ 30^s$. We may therefore from hence conclude, that on Sept. 21, N. S. the true emersion at Wansted preceded the calculation by the tables about $3^m\ 35^s$, and that the true emersion there was at $12^h\ 1^m\ 15^s$ April 1; but this emersion was observed at Lisbon at $11^h\ 24^m\ 55^s$, the difference being $36'\ 20''$.

The observations at Wansted being made with Mr. Hadley's reflecting telescope, by which one may see the first satellite near $\frac{1}{4}$ of a minute sooner when emerging, than in a refracting telescope of 15 feet, and the contrary when immerging, there ought to be some allowance made on account of different telescopes used at Lisbon and Wansted, by deducting 10 or 15^s from the difference of time collected from the immersions, and adding as much to the difference deduced from the emersions. Such correction being made, the difference of meridians by the immersion observed July 28, will be $37^m\ 20^s$, and by the emersion, Sept. 21, $36^m\ 35^s$.

The emersion observed at Lisbon, Dec. 8, N. S. at $8^h\ 32^m\ 40^s$ apparent time, was likewise seen at Wansted in a 15-foot tube at $9^h\ 10^m\ 5^s$ apparent time, the air being a little hazy, which may probably make the difference $37^m\ 25^s$ a little too great.

The emersion seen at Lisbon, Jan. 16, 1726, N. S. at $6^h\ 51^m\ 10^s$, which seems accompanied with circumstances that argue its exactness, was likewise very well observed at Wansted in a 15-foot tube, at $7^h\ 28^m\ 22^s$ apparent time, the difference being $37^m\ 12^s$.

These are the only observations among those which were last communicated, that Mr. P. could compare with any degree of certainty with his own. But others were printed in the Phil. Trans. N° 385, which were likewise made by the same curious persons, who observed an emersion of the first satellite at Lisbon, Sept. 2, 1724, N. S. at $9^h\ 36^m\ 57^s$. This was seen also at Wansted in the reflector at $10^h\ 13^m\ 28^s$ apparent time. Hence, allowing for the different telescopes, the difference of meridians is $36^m\ 45^s$.

This emersion at Wansted preceded the calculation by the tables $4^m\ 40^s$: and another emersion observed with the same telescope on Sept. 18, N. S. preceded the calculation $5^m\ 10^s$. We may therefore suppose that on Sept. 9, N. S. the true emersion at Wansted preceded the computed about $4^m\ 52^s$. The emersion that day by the tables was at $12^h\ 15^m\ 34^s$ app. time; therefore the true emersion at Wansted was at $12^h\ 10^m\ 42^s$. At Lisbon it was observed at $11^h\ 34^m\ 26^s$. So that, allowing for the difference of telescopes, the difference of meridians by this observation is $36^m\ 30^s$.

The mean of all these differences is about $36^m\ 58^s$, from which subtracting 28^s for the difference of meridians between London and Wansted, the remainder will be the difference of meridians between London and Lisbon, viz. $36\frac{1}{2}^m = 9^\circ\ 7\frac{1}{2}'$, Lisbon being so much to the westward of London; which is about $5\frac{1}{4}'$ greater than what is determined in the forementioned Transaction.

The same Transaction containing some observations of eclipses of the same satellite made in the Fort of New York, communicated by his Excellency William Burnet, Esq. governor of New York. Mr. Pound takes this opportunity of determining the longitude of that fort more exactly than it can be supposed to be there done, by the bare comparison of the observations with the tables; having two observations made at Wansted, which tally with two made at New York, on Aug. 25, and Sept. 10.

By the observation made Aug. 25, 1723, O. S. which is esteemed the most distinct and best, the satellite emerged at $9^h\ 35^m\ 14^s$ by the clock, which went about $1\frac{1}{4}^m$ too fast for the apparent time at the emersion, as appears by the altitudes of the sun's limb taken the morning before and after the observation; so that the emersion at New York was at $9^h\ 34^m$ apparent time, that is, $9^h\ 32^m\ 20^s$ mean time.

Aug. 27, $8^h\ 57^m\ 40^s$ mean time, the satellite was seen emerging at Wansted in the reflector; and Sept. 12, $7^h\ 17^m\ 15^s$ mean time, it was seen emerging again in the same telescope; so that in 15^{d} 22^{h} 19^{m} 35^{s} there were nine emersions; and the interval between each was about 1^{d} 18^{h} 28^{m} 50^{s}. This subtracted from the time of the emersion observed at Wansted, Aug. 27, will give the true emersion at Wansted on Aug. 25, 14^{h} 28^{m} 50^{s} mean time, that is, 4^{h} 56^{m} 30^{s} later than it was observed at New York.

Sept. 10, 8^{h} 0^{m} 10^{s} by the clock, another emersion was observed at New York. From the altitudes of the sun's limb taken the morning before, Mr. P. computed the error of the clock at the time of the emersion to be 1^{m} 10^{s}, and that the emersion happened at 7^{h} 59^{m} apparent time, that is, 7^{h} 51^{m} 52^{s} mean time at New York. But subtracting the forementioned interval of 1^{d} 18^{h} 28^{m} 50^{s} from the time of the emersion observed at Wansted Sept. 12, 7^{h} 17^{m} 15^{s} mean time, we shall have the time of the true emersion at Wansted on Sept. 10, at 12^{h} 48^{m} 25^{s} mean time, which is 4^{h} 56^{m} 33^{s} later than it was observed at New York. The difference therefore of meridians between Wansted and New York, allowing about 15^{s} for the difference of telescopes, is about 4^{h} 56^{m} 45^{s}, and between London and New York, 4^{h} $56\frac{1}{4}^{m}$. So that the true longitude of New York from London is 74° 4′ west.

Eclipses of Jupiter's first Satellite, observed at Lisbon in the Years 1725 *and* 1726. *By Fa. Carbone.* N° 394, p. 90. *From the Latin.*

		True time.			
1725.	July 28^{d}	12^{h}	12^{m}	26^{s}	. . the satellite immerged into the true shadow.
	Sept. 12	15	0	10	. . it emerged out of the true shadow.
	14	9	28	7	. . it began to emerge out of the shadow.
	21	11	24	55	. . began to emerge out of the true shadow.
	Oct. 23	8	11	10	. . the beginning of the emersion.
	Nov. 8	6	30	4	. . the beginning of the emersion.
	15	8	24	50	. . the beginning of ditto.
	Dec. 8	8	33	30	. . ditto.
1726.	Jan. 9	4	58	50	. . emersion.
	16	6	51	10	. . emersion.

On the Latitude of Lisbon. By Fa. Carbone. N° 394, p. 92.

From a number of the sun's altitudes, accurately observed at the Jesuit's college at Lisbon, accompanied with proper allowances and calculations, Father Carbone determines that its latitude, from a medium of the whole, is 38° 42′ 30″.

Astronomical Observations made at Toulon, By Fa. Laval. N° 394, p. 100. *From the Latin.*

Emersions of Jupiter's first satellite; true time.

1725.	Sept. 23, emersion	6^h	56^m	42^s	Dec. 17, emersion	5^h	56^m	34^s	
	Oct. 16, emersion	7	15	17	Dec. 24, emersion	7	49	18	dub.
	Nov. 8, emersion	7	31	33	1726.				
	Nov. 15, emersion	9	26	52	Jan. 9, emersion	6	2	3	dub.

Jan. 18, 1726, an occultation of Mars by the moon, temp. ver. 7^h 23^m, rather doubtful. Emersion of Mars, 8^h 21^m 34^s certain.

Apparent meridian altitudes of Venus.

1725.	March 20	36°	34′	30″	Dec. 7	23°	59′	30″
	April 21	51	43	0	21	28	21	0
	May 8	59	35	0	24	29	30	0
	Sept. 8	44	30	30	1726.			
	21	37	57	0	Jan. 9	36	29	0
	24	36	26	30	19	41	19	0
	Oct. 18	26	28	45	31	47	14	0
	Nov. 8	21	50	0	Feb. 3	48	40	30

By many accurate observations, the latitude of Toulon is 43° 6′ 55″. And the difference of longitude between Lisbon and Toulon, by many accurate observations, is 1^h 0^m 9^s, or 15° 2′ 15″.

Of some remarkable Appearances observed on opening the Body of a Person who had laboured under Calculous Complaints. By Abraham Vater, M. D. Professor of Anatomy at Wittemberg, and F. R. S. N° 394, p. 102. *From the Latin.*

A young student was frequently affected with a strangury for the space of 2 years, during which time he voided above 50 stones, most of which came away inter mingendum, without much pain; some of the largest however, about the size of large peas or kidney beans, stuck in the urethra, and could not be brought away without either being broken or being extracted by incision. Thus harassed, the patient gradually fell into a marasmus, accompanied with a dry cough and asthma. To these was superadded an œdematous swelling of the feet; and this complication of symptoms proved fatal on the 2d day of the patient's confinement to his bed.

On opening the thorax, the lungs were found to adhere in many places to the pericardium, diaphragm, and ribs; and the right lobe was in a scirrhous state. Some very large polypous concretions, which extended into the trunks of the blood-vessels, were found in each ventricle of the heart. These Dr. V. considers as the principal cause of the patient's asthma and his sudden death.

The liver and spleen appeared to be free from disease; in the ileum there were several livid spots; and the colon, together with the rectum, was so contracted in its whole course on the right side, where it rests upon the liver, that it was scarcely as thick as one's finger, and its cavity was nearly destroyed.

The kidnies and ureters exhibited nothing preternatural; but three stones were found in the bladder. They were as large as kidney-beans, not loose but inclosed within a strong membrane, and adhered to the fore part of the bladder, near the sphincter. How the membrane, in which these stones were enveloped, was formed, Dr. V. thinks it difficult to explain; but the situation of the aforesaid stones, in the part of the bladder just mentioned, readily accounts, as it appears to him, for the frequent strangury with which the patient was affected, and also, in consequence of the perpetual irritation the stones would occasion, for the preternatural constriction of the colon and rectum.

Concerning Equations with impossible Roots. By Mr. Colin Mac Laurin, Professor of Mathematics at Edinburgh. F. R. S. N° 394, p. 104.

The following is a very easy and simple way of demonstrating Sir Isaac Newton's rule, by which it may be often discovered when an equation has impossible roots. This method requires nothing but the common algebra, and is founded on some obvious properties of quantities demonstrated in the following lemmata, without having recourse to the consideration of any curve whatsoever, which does not seem so proper a method in a matter purely algebraical.

Lemma 1.—The sum of the squares of two real quantities, is always greater than twice their product. Thus $a^2 + b^2$ is greater than $2ab$; because the excess $a^2 + b^2 - 2ab$ is equal to $\overline{a - b}^2$, and therefore is positive; since the square of any real quantity, negative or positive, is always positive.

Lemma 2.—The sum of the squares of three real quantities, is always greater than the sum of the products, that can be made by multiplying any two of them. Thus $a^2 + b^2 + c^2$ is always greater than $ab + ac + bc$; for the excess $a^2 + b^2 + c^2 - ab - ac - bc = \frac{2a^2 + 2b^2 + 2c^2 - 2ab - 2ac - 2bc}{2} =$ $\frac{a^2 - 2ab + b^2 + a^2 - 2ac + c^2 + b^2 - 2bc + c^2}{2} = \frac{\overline{a - b}^2 + \overline{a - c}^2 + \overline{b - c}^2}{2}$, that is, half the sum of the squares of the differences of the quantities a, b, c: but since these squares are positive, it follows, that the excess of $a^2 + b^2 + c^2$ above $ab + ac + bc$ is positive, and that the sum of the squares of three quantities must be greater than the sum of the products made by multiplying any two of them.

Lemma 3.—The triple sum of the squares of four quantities is greater than the double sum of the products, that can be made by multiplying any two of them; for $3a^2 + 3b^2 + 3c^2 + 3d^2 - 2ab - 2ac - 2ad - 2bc - 2bd - 2cd = a^2 - 2ab + b^2 + a^2 - 2ac + c^2 + a^2 - 2ad + d^2 + b^2 - 2bd + d^2 + b^2 - 2bc + c^2 + c^2 - 2cd + d^2 = \overline{a-b}^2 + \overline{a-c}^2 + \overline{a-d}^2 + \overline{b-c}^2 + \overline{b-d}^2 + \overline{c-d}^2$, the sum of the squares of the differences of the four quantities a, b, c, d. Therefore $3a^2 + 3b^2 + 3c^2 + 3d^2$ is greater than $2ab + 2ac + 2ad + 2bc + 2bd + 2cd$, the excess being always positive.

Lemma 4.—Let the number of the quantities a, b, c, d, e, &c. be m, the sum of their squares A, and the sum of the products made by multiplying any two of them B. Then shall $\frac{m-1}{2} \times \text{A}$ be always greater than B.

For by adding together the squares of the differences $a - b$, $a - c$, $a - d$, $b - c$, $b - d$, $c - d$, &c. we add a^2 as often to itself as there are quantities more than a; the same is true of b^2, c^2, d^2, e^2, &c. But the rectangles $-2ab$, $-2ac - 2ad - 2bc - 2bd$, &c. arise but once each. Therefore the sum of all the squares $\overline{a-b}^2$, $\overline{a-c}^2$, $\overline{b-c}^2$, $\overline{b-d}^2$, &c. $= \overline{m-1} \times a^2 + \overline{m-1} \times b^2 + \overline{m-1} \times c^2$, &c. $- 2ab - 2ac - 2bc$, &c. $= \overline{m-1} \times \text{A} - 2\text{B}$. But $\overline{a-b}^2 + \overline{a-c}^2 + \overline{a-d}^2$, &c. is always a positive quantity; therefore $\overline{m-1} \times \text{A} - 2\text{B}$ is positive, and consequently $\frac{m-1}{2} \times \text{A}$ greater than B.

Cor.—It appears from the demonstration, that the excess of $\overline{m-1} \times \text{A}$ above 2B is always equal to the sum of the squares of the differences of the quantities a, b, c, d, &c. and that when the quantities a, b, c, d, &c. are all equal, then $\overline{m-1} \times \text{A} - 2\text{B} = 0$, and with this restriction the preceding lemmata must be understood.

It is to be observed, that though we have supposed, in these lemmata, the quantities a, b, c, d, &c. positive, they are, a fortiori, true of negative quantities, whose squares are the same as if they were positive, while the sum of their products is either the same, or less than it would be, were they all positive.

PROP. I.—In a quadratic equation that has its roots real, the square of the second term is always greater than the quadruple product of the first and third terms.

Let the roots of the quadratic equation be represented by $+a$ and $+b$; and if x be the unknown quantity, then shall $\begin{array}{l} x^2 - ax + ab = 0, \\ - bx \end{array}$

Now since $a^2 + b^2$ is greater than $2ab$, by lemma 1, therefore $a^2 + b^2 +$

$2ab$ is greater than $4ab$; therefore $\overline{a+b}^2 \times x^2$, the square of the second term, will be greater than $4ab \times x^2$ the quadruple product of the first and third terms.

Prop. II.—In any cubic equation, all whose roots are real, the square of the second term is always greater than the triple product of the first and third.

If the cubic equation has all its roots real, they may be represented with their signs by a, b, c, and the equation will be expressed thus:

$$\begin{aligned} y^3 - ay^2 &+ aby - abc = 0.\\ - by^2 &+ acy\\ - cy^2 &+ bcy \end{aligned}$$

But by lemma 2, $a^2 + b^2 + c^2$ is always greater than $ab + ac + bc$; and consequently, adding $2ab + 2ac + 2bc$ to both sides, $a^2 + b^2 + c^2 + 2ab + 2ac + 2bc$ ($= \overline{a+b+c}^2$) will be greater than $3ab + 3ac + 3bc$; and therefore $\overline{a+b+c}^2 \times y^4$ must be greater than $\overline{3ab+3ac+3bc} \times y^4$, that is, the square of the second term must be greater than the triple product of the first and third terms.

Cor. 1.—In general, it appears from the demonstration, that the square of the sum of three real quantities, $\overline{a+b+c}^2$ is always greater than the triple sum of all the products that can be made by multiplying any two of them into each other.

Cor. 2.—It follows from the proposition, that when the square of the second term is not greater than the triple product of the first and third terms, the roots of the equation cannot be all real; but two of them must be impossible: and this plainly coincides with one part of Sir Isaac Newton's rule for discovering when the roots of cubic equations are impossible. He desires we may write above the middle terms of the equation the fractions $\frac{1}{3}$, $\frac{1}{3}$ as in the margin; and placing the sign + under the first and last term, he multiplies the square of the second term by the fraction $\frac{1}{3}$ that is above it; and if the product is greater than the product of the adjacent terms, he places + under the second term; but if that product is less, he places − under the second term, and says, there are as many impossible roots as changes in the signs. Now by this proposition, if $p^2 x^4$ is not greater than $3qx^4$, or $\frac{1}{3} p^2 x^4$ greater than qx^4, the roots cannot be all real. The same supposition makes two changes in the signs, whatever sign we place under the third term, since the signs under the first and last are both +; and therefore this proposition demonstrates the first part of Sir Isaac Newton's rule, as far as it relates to cubic equations.

$$\begin{array}{cccc} & \frac{1}{3} & \frac{1}{3} & \\ x^3 + & px^2 + & qx + & r = 0 \\ + & - & * & + \end{array}$$

Cor. 3.—If the second term is wanting in a cubic equation, and the third is positive, two of the roots of the equation must be impossible: for the square of the second term (equal to nothing in this case) will be less than the triple product of the adjacent terms. But this will better appear from considering that, when the second term vanishes in an equation, the positive and negative roots are equal, and when added together, destroy each other. Suppose the roots to be $+ a$ and $- b$, $- c$; then in this case $a = + b + c$, and the coefficient of the third term will be $- ab - ac + bc = - b^2 - 2bc - c^2 + bc = - b^2 - bc - c^2$, and consequently negative. Or, if we suppose two roots positive and one negative, let them be $- a$, $+ b$, $+ c$, then the coefficient of the third term will be still $- b^2 - bc - c^2$. Therefore when the roots are real, the coefficient of the third term is negative; and if the coefficient of the third term is not affected with a negative sign, it is a proof that two of the roots are impossible.

PROP. 3. In any cubic equation, all whose roots are real, the square of the third term is greater than the triple product of the second and fourth terms.

In the same cubic equation, whose roots are a, b, c, the square of the third term is $\overline{ab + ac + bc}^2$, the product of the second and fourth terms is $a^2bc + ab^2c + abc^2$, as is plain from the inspection of the equation; and it is obvious that $a^2bc + ab^2c + abc^2$ is the sum of the products of any two of the terms ab, ac, bc; and therefore, by Corol. 1, Prop. 2, the square of the sum of these terms, that is, $\overline{ab + ac + bc}^2$, must be greater than $3a^2bc + 3ab^2c + 3ac^2b$. So that $\overline{ab + ac + bc}^2 \times y^2$ must be greater than $\overline{3a^2bc + 3ab^2c + 3ac^2b} \times y^2$; that is, the square of the third term must be greater than the triple product of the second and fourth terms.

Cor. 1. It follows from the demonstration, that $\overline{ab + ac + bc}^2$ is always greater than $3abc \times \overline{a + b + c}$.

Cor. 2. If the square of the third term is found to be less than the triple product of the second and fourth terms, then the roots of the equation cannot be all real quantities; and this agrees with the second part of Sir Isaac Newton's rule for finding when the roots of a cubic equation are

impossible: for this case gives $-$ to $x^3 + \overset{\frac{1}{3}}{px^2} + \overset{\frac{1}{3}}{qx} + r = 0$
be placed under the third term, and $+ \quad * \quad - \quad +$
consequently two changes of the signs, whatever sign is placed under the second term.

Schol. After the same manner it may be demonstrated, that in a cubic equation, whose roots are all real, if the second term is wanting, the cube of the third part of the third term taken positively, is always greater than the square of half the last term. Suppose that the roots of the equation are $+a$, $-b$, $-c$, or $-a$, $+b$. $+c$, and that $a = b + c$, then the second term in the equation will be wanting, and the other terms will be expressed thus:

$$\begin{array}{l} y^3 * - b^2y \pm bc \times \overline{b+c} \\ - bcy \\ - c^2y \end{array}$$

The square of $b - c$ is always positive, since b and c are real quantities. Suppose it, (viz. $b^2 - 2bc + c^2$) equal to D, then $b^2 + bc + c^2 = \mathrm{D} + 3bc$, and $\overline{b+c}^2 = \mathrm{D} + 4bc$. Therefore $\frac{\overline{b^2 + bc + c^2}^3}{27} = \frac{\mathrm{D}^3}{27} + \frac{\mathrm{D}^2bc}{3} + \mathrm{D}b^2c^2 + b^3c^3$, and $b^2c^2 \times \frac{\overline{b+c}^2}{4} = \frac{\mathrm{D}b^2c^2}{4} + b^3c^3$. Now it is obvious that $\frac{\mathrm{D}^3}{27} + \frac{\mathrm{D}^2bc}{3} + \mathrm{D}b^2c^2 + b^3c^3$ is greater than $\frac{\mathrm{D}b^2c^2}{4} + b^3c^3$, since D is positive, and bc also positive, b and c being roots having the same sign. Therefore the cube of $\frac{1}{3}$ of the third term having its sign changed $\left(= \frac{\overline{b^2 + bc + e^2}^3}{27}\right)$ is always greater than the square of half the last term $\left(= b^2c^2 \times \frac{\overline{b+c}^2}{4}\right)$. In the cubic equation $x^3 * + qx + r = 0$, if q be positive, or if it be negative and $+ \frac{1}{27}q^3$ be less than $\frac{1}{4}r^2$, it appears that two roots of the equation must be impossible, from this corollary, and from Cor. 3, Prop. 2, taken together.

PROP. 4. In a biquadratic equation, all whose roots are real quantities, $\frac{3}{8}$ of the square of the second term, is always greater than the product of the first and third terms; and $\frac{3}{8}$ of the square of the fourth term, is always greater than the product of the third and fifth terms.

1. Let the equation be $x^4 - px^4 + qx^2 - rx + s = 0$; and since the roots are supposed to be all real, let them be represented by a, b, c, d, then $p = a + b + c + d$, and $q = ab + ac + ad + bc + bd + cd$. But it is plain from Lemma 3, that $3a^2 + 3b^2 + 3c^2 + 3d^2$ is greater than $2ab + 2ac + 2ad + 2bc + 2bd + 2cd$; and consequently, by adding $6ab + 6ac + 6ad + 6bc + 6bd + 6cd$ to both, we shall find that $3 \times \overline{a + b + c + d}^2$ must be greater than $8ab + 8ac + 8ad + 8bc + 8bd + 8cd$; that is, $3p^2$ greater than $8q$; and therefore $\frac{3}{8}p^2x^6$ greater than qx^6.

2. Since $r = abc + abd + acd + bcd$, and $s = abcd$; and since qs is equal to $a^2b^2cd + a^2c^2bd + a^2d^2bc + b^2c^2ad + b^2d^2ac + c^2d^2ab$, which are the products that can be made of any two of the quantities abc, abd, acd, bcd, whose sum is r, multiplied by one another; it follows, that $3r^2$ is always greater than $8qs$: so that $\frac{3}{8}$ of either the square of the second term, or of the

square of the fourth term, must always be greater than the product of the terms adjacent to them.

Cor. Multiply either the square of the second term, or the square of the fourth term, of a biquadratic equation, by $\frac{3}{8}$, and if the product does not exceed the product of the adjacent terms, some of the roots of that equation must be impossible.

PROP. 5. In an equation of any dimension expressed by m, the coefficients of the second, third, last, last but one, and last but two terms, being respectively A, B, E, D, C, if the roots of the equation are all real, then shall $m - 1 \times A^2$ always be greater than $2mB$, and $\overline{m - 1} \times D^2$ greater than $2mCE$.

1. For supposing the roots to be a, b, c, d, e, &c. then by Lemma 4, shall $\overline{m - 1} \times a^2 + \overline{m - 1} \times b^2 + \overline{m - 1} \times c^2$ &c. be greater than $2ab + 2ac + 2ad$, &c. and adding $\overline{2m - 2} \times ab + \overline{2m - 2} \times ac + \overline{2m - 2} \times ad$, &c. to both, the sum $\overline{m - 1} \times a^2 + \overline{2m - 2} \times ab + \overline{m - 1} \times b^2 +$ &c. ($= \overline{m - 1} \times \overline{a + b + c, \&c.}^2$) must be greater than $2mab + 2mac + 2mad$, &c. that is, $\overline{m - 1} \times A^2$ must be greater than $2mB$.

2. In general, it follows from this demonstration, that the square of the sum of any quantities whose number is m, multiplied by $m - 1$, must be greater than the sum of all the products can be made by multiplying any two of them, multiplied by $2m$. But it is easy to see, from the genesis of equations, that CE is the sum of the products that can be made by multiplying any two of the terms whose sum is D: from which it follows, that $\overline{m - 1} \times D^2$ must be always greater than $2mCE$.

Observations on the Dissection of an Ostrich. By Mr. George Warren, Surgeon in Cambridge. N° 394, p. 113.

Dr. Brown has so well described the parts of the ostrich he dissected (Philos. Collect. N° 5*) that Mr. W. thinks there is not much to be added. But the Dr. affirms it has no epiglottis; yet in this subject that cartilage was plainly visible; and indeed the rimula appeared too open not to require one. The os hyoïdes is 3 inches long from the basis; the musculi directòres asperæ arteriæ were very plain, large and strong; the ring composed of 3 cartilages at the divarication of the aspera arteria very bold; the 2 glands on the carotid arteries, as large as small eggs. There was nothing in the lungs or heart, but what it has in common with other birds. The 2 stomachs, viz. the crop and gizzard, were filled with half-digested grass, in which were some nails, some

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* Vol ii. p. 534, of these Abridgments.

stones of the size of walnuts, and about 14 or 15 pieces of silver and copper-money. The 1st stomach, or crop, was exceedingly tender, and contained, crammed as it was, between 3 and 4 quarts. The glands on the top of the crop were very large and numerous, in the order described by Dr. Brown, and of the size of little oculi cancrorum, and of a watery-brown colour; which being so different from the colour of the stomach, that added to the pretty order they are placed in, makes them very remarkable. The crop lay within the thorax, but so that the gizzard lay higher. The looseness and likeness to flannel, of the inner coat of the gizzard, mentioned by Dr. Brown, was very remarkable in this bird; but the texture in its muscular part did not seem proportionably strong to that in other birds, being broader, thinner, and more flaccid. The guts were about 26 yards long. The 2 cæcums, which are about 34 inches long each, and which have beautiful spiral valves, were appendages of the very beginning of the colon. The testicles lay as in other fowls, very high, and less than pigeons eggs, but longer. The liver had 4 lobes, but a gall-bladder appeared to be only the membrane of the liver raised by some accident from its inner substance. The gland under the stomach, which Dr. Brown supposes to be the spleen, and the pancreas and kidneys answer his description; and the ureters were, as he says, firm, strong, white, long, and opening into the rectum. The eye is said to be exactly like the human eye, but is indeed a perfect goose-eye for its colour, and probably for the rest of its parts, as they are well described by Mr. Ranby: it was flatter than the human eye, as that of all birds are; and it had that simple look so peculiar to the goose. The bony circle described by Mr. Ranby, this bird has in common with other fowls, both of the water and land, with this difference only, that the ring in water-fowls consists of 15, and in land-fowls only of 14 bones. They are so disposed, that one bone lies over the ends of 2 others, then 3 or 4 lie over one another, like the scales of fish; then 1 bone lies under the ends of 2 others; and then 2 or 3 more follow again like the scales of fish: but unless there be a lusus naturæ, Mr. Ranby's icon seems not to express it so very justly, as it might be done. There was no musculus suspensorius oculi in this animal, nor probably is it to be found among birds, and indeed there seems to be no reason for it.

The crop was so stuffed with grass or other greens, proper food for a goose, or one of that kind, that probably the bird could not have digested it off, if there had been no other reason for its death. The gizzard was not so stuffed as the crop, and what was therein seemed undigested. The guts contained a thick deep-green juice, even to the cloaca. The money, both of silver and copper in the gizzard, was very remarkably worn away, the edges in particular were made round, and the bust and reverse scarcely perceptible in some pieces, and quite obliterated in others. The ærugo and sulci in many of the pieces would make one believe, that besides the attrition, there may be a menstruum in their gizzards not unfit to dissolve metals. Within an inch of the end of the rectum was the cloaca, or expansion of that great gut, which was thinner than the other part of the gut, in proportion to its expansion, and would hold above half a pint. The end of the rectum, from the cloaca, opened into a cavity large enough to hold my two fists; and for want of another name, it may be called the receptaculum penis, because the penis was always lodged in it when flaccid. That part is called by Dr. Brown, a kind of præpuce; but upon dissection, it appeared plain enough to be a very strong muscle composed of circular fibres, and to be designed for a sphincter of that part in which the penis was to be lodged, and to be a sphincter of the rectum too; round which the same muscle was traced above an inch; and this being but one muscle, must be the reason that the penis always came out some inches when it muted, as reported. The penis, flaccid as it is, is 5½ inches long from the skin of that receptaculum, and, as Dr. Harvey says, not unlike a hart's tongue. Mr. W. did not find a cartilage in it, as Dr. Brown suggests; but at its origination it is so hard a body, that he believed if the bird had lived some years, it might have become cartilaginous. There are two bodies that are joined to the crura penis, which perhaps may be the vesiculæ seminales, and the rather, as there are two vessels enter them, which seem to be the vasa deferentia; but of this Mr. W. was not certain; for though he found semen in the urethra, he was not able to trace a passage from these supposed vesiculæ seminales, or those vessels, or any other part into the urethra. He calls it urethra, because there is no other term for it, though the urine does not pass that way; but, as in other birds, is mixed with the grosser excrements in the cloaca. The urethra then, is only a sulcus, or gutta, from one end of the penis to the other; which sulcus, as the penis lies flaccid in the receptaculum, lies on one side; but on erection, the penis turns towards the belly, and the sulcus is then at the top, and lies conveniently enough for conveyance of the semen. If those two bodies are not the vesiculæ seminales, they must be elongations of the crura penis; but they seem of much too loose a contexture to serve that purpose. Whether the vena cava, dividing into two branches to go into the kidneys, and uniting again when it comes out, is singular to this bird, or is in common with geese and other water-fowl, is not known; but so it was in the ostrich. The cæcums of the ostrich, which are so much taken notice of, are no more than what it has in common with other fowls; and that a chicken has two, as large, and as long in proportion as the ostrich.

The omentum, on the stomachs and guts, was 6 inches thick at the top, and decreasing gradually, was nearly 2 inches thick at the vent, and was divided into two parts in the middle from the top to the bottom. The basis of the os hyoïdes is of this figure , and the round part at the top is lodged in a proper cavity in the top of the tongue. Partly under the basis of the os hyoïdes, lies a cartilage, in the front and very beginning of the aspera arteria, which is not unlike the thyroïdes; but it has no other cartilages in that part besides what forms the rimula. The first 28 cartilages of the aspera arteria are not annular; the rest, being about 226, are entirely annular, but as soon as it divaricates to go into the lungs, they are not so again.

An Account of a Person killed by Lightning at Worcester. By R. Beard, M.D. F. R. S. N° 394, p. 118.

We had June 10, 1724, continued lightning in the east from 8 o'clock to 12, the weather for some time before having been very sultry, the wind at N.E. and the barometer at settled fair. This morning the mercury sunk, and the sky became more cloudy and temperate, except a few hot gleams; at 2 in the afternoon, several hard showers fell, attended with flashes of lightning and claps of thunder, that still approached nearer us; between 2 and 3, a flash came so violently, succeeded so very quick by a low, unusual, dreadful sound, that Dr. B. immediately went to the door, fearing some mischief near. He was soon called to an officer's lady, aged about 18, and breeding, killed by it in the adjoining street. He found her yet warm, and that she had survived the stroke for 6 or 7 minutes. The fire marks were streaks of a copper colour, branched from the left shoulder all over the thorax, and interspersed here and there with irregular spots. This sad accident happened in a parlour window next the street, that could contain about two persons; the lady, it seems terrified with the repeated lightning and thunder, it having formerly been fatal to her brother, desired an officer to change places with her, that she might be near her husband; but she was no sooner seated by his side, than she inclined sideways, and spoke some words; after she was carried to another room, she said, she was gone, and then, that she was blind, and asked for water. The husband was thrown along, together with the fortunate gentleman that had just resigned his seat, and a large looking-glass was lifted off the hooks. The landlord's daughter, at work near the lady, perceived such an impulse on the side of her head, that her hearing was much impaired, and upon every peal of thunder since, she is affected in like manner, though not so strongly. The gentlemen complained that they were stupified, forced down they did not know why, unless it was for want of breath, and of pains and numbness in their limbs. They had likewise on different parts of their bodies such reddish wheals as were seen on the lady's breast; but these symptoms vanished the next day. The other two persons at the further end of the room were untouched, they were all sensible of a sulphurous smell. The pane of glass exactly behind the lady's waste was perforated by a round hole of 1½ inch diameter, as if done with a diamond, or rather a wind gun. On a more nice examination of the body, in the presence of the friends, that evening, Dr. B. discovered on the left loin, taking in part of the spine of the os ilium, which was somewhat swelled, a deep contusion of the same dimension with the breach in the glass; the skin was neither indurated nor pierced; the blood in the capillaries all round, but chiefly up the back, settled, the colour of which was easily distinguished from that of the streaks, and the circular impression.

An Account of the Strata met with in digging for Marl, and of Horns found under Ground in Ireland. By Mr. James Kelly. N° 394, p. 122.

The marl here is found only in the bottoms of low bogs, where it is searched for with augres, and found at the depth of 7, 8, or 9 feet; this in many places occasions great expence in draining off the water. For the first 3 feet they meet with a fuzzy sort of earth, called moss, proper to make turf for fuel; then a stratum of gravel about half a foot; under which, for about 3 feet more, is a more kindly moss, that would make a more excellent fuel; this is altogether mixed with timber, but so rotten, that the spade cuts it as easily as the earth; under this, for the depth of 3 inches, are leaves, mostly of oak, that appear fair to the eye, but will not bear a touch. This stratum is sometimes interrupted with heaps of seed, that seem to be broom or furze seed; and in one place there appeared to be gooseberries and currants; in other places in the same stratum they find sea-weed, and other things as odd to be at that depth; under this appears a stratum of blue clay, of half a foot thick, fully mixed with shells; this is esteemed good marl, and thrown up as such; then appears the right marl, commonly 2, 3, or 4 feet deep, and in some places much deeper, which looks like buried lime, or the lime that tanners throw out of their lime-pits, only that it is full mixed with shells, being the small periwinkles, called fresh water wilks; though there are among them abundance of round red periwinkles, such as are often thrown out on the sea-shore. Among this marl, and often at the bottom of it, are found very great horns, which, for want of another name, are called elk-horns: where they join the head, they are thick and round; and at that joining there grows out a branch of about a foot long, that seems to have hung just over the beast's eyes; it grows round above this for about a foot or more; then spreads broad, which ends in branches, long and round, turning with a small bend. One of these horns is represented fig. 11, pl. 3. They have also found shanks and other bones of these beasts in the same place.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1725, *pursuant to the Direction of Sir Hans Sloane, Baronet, Pr. Coll. Med. S. P. V. Pr. By Mr. Isaac Rand, Apothecary, F. R. S.* N° 395, p. 125.

An Account of an Aurora Borealis, seen in Ireland, Sept. 24, 25, 26, 1725; *with a Solution of the Phenomenon. By Arthur Dobbs, Esq.* N° 395, p. 128.

The theatre of light, forming an irregular variable curve, was, as at most times before, from E. N. E. to W. N. W. the horizon and whole hemisphere serene, little or no wind, what there was seemed northerly. The seeming dawn, or scene of light, generally continued in an irregular curve; the one point in the first two nights began near the horizon, near N. N. E. the other point was at W. N. W. the height of the arch not exceeding 20 degrees, in which there seemed to be a continual dawn; under that field of light seemed to be a dark cloud, which however was a clear sky, not filled with that luminous vapour; because all the stars appeared distinctly and twinkling through it. Whenever that light rose about 10 degrees higher, or to about 30 degrees, then flashes or coruscations followed alternately, and seemed to be columns or beams of light, which followed or succeeded each other, and by that means seemed to move and change by the succession of light and darkness, according to the flashes. When the lighted vapour rose higher, to about 40 or 45 degrees, then the appearance altered, and instead of the beams or columns of light, as when lower, there were flashes like those attending explosions, in which faint colours of red, green, and yellow appeared, but not very vivid; and on each explosion it would spread upwards towards the zenith, in the appearance of thin enlightened clouds, and immediately disappear. On the 26th, about 9 at night, one of these irregular arches of light had reached the zenith, the lower points being near E. N. E. and W. S. W. the lower part being a constant fixed light, equal to the light of the edge of a white cloud in the day-time, when the edge shines on it. As it rose higher it was somewhat weaker, and the motion of the columns or beams of light after each flash, seemed by that means to move. Somewhat higher again, at about 40 degrees, the flashes were like explosions of great guns, with the faint colours observed as before; but the coruscations or flashes from thence to the zenith, expanded at every flash, like a broad, thin, white cloud, of which some faint view could be seen after each explosion for some time; and after all the explosions were over, there remained a thin duskish vapour in and near the zenith, and all along the arch from east to west, from 14 to 20 degrees broad, which undulated and moved like a stormy sea, the motion coming from the S. S. E. and so lessened till it appeared no brighter than the milky way, but more like a very thin cloud or mist, through which the stars appeared. At the same time there was another thin cloud, having the same appearance, arch ways, to the southward, at about the height of 40 degrees, which Mr. D. supposed had been another, which had been over, and had moved thither from the northward before he went out; and during the whole time there were lesser lights towards the north, but dispersed here and there, and not forming any large body of light. During the whole time, the hemisphere was clear, except a few very small clouds near the horizon; and when any moved into the enlightened arch they broke the connection, so that the light was above them, at the same time it froze hard each night.

From these observations, Mr. Dobbs supposes, that the aurora borealis is a thin nitro-sulphureous vapour, raised in our atmosphere considerably higher than the clouds, which is discontinued in several places by the interspersed air, and which by pressure and motion is kindled; and perhaps the explosion of one may by its shock and motion contribute to kindle the next; by which means they go off one after another, till the whole vapour within their influence is discharged, and then the light disappears, and the thin smoke appears, and undulates, according to the motion in that part of the atmosphere. Hence he thinks most of the appearances may be solved; for first, as to the continued light near the horizon, they being at a great distance from us, and nearly in a line, all these explosions may seem as a continued light; when these approach nearer to us, and so appear higher in our hemisphere, we observe the motion in each flash, and still seeing them laterally, yet somewhat breaking the continuity of the light; they, by the reflection of the vapour floating in the atmosphere, and being not reflected, where the air between them is free of those vapours, may appear as columns; and as the flash below and beyond them moves, as it kindles and expands, so they seem to move, and perhaps are shocked at the same time by the motion; but afterwards, when they are nearer and raised to the altitude of 40 degrees, we get somewhat under them, and see the expansion of the explosion, which appearing somewhat globular, gives the faint colours observed above, the light not being intense enough to make them vivid; and afterwards when they rise to, or near the zenith, they are nearest to us, and then they expand very wide at each flash, like little clouds; and the great objection of their appearing in the northern part of the hemisphere, and seldom or never in the southern, is in some measure answered by the appearance on the 26th; since at least half the arch was in the southern part of the hemisphere; and perhaps the reason why the light is not seen near the horizon, in the southern part of the hemisphere, may be this, that in clear serene weather, the wind being generally near the north, objects from thence are much more distinctly viewed, and at a greater distance than from the south; and it is generally known, that lands at a greater distance are most distinctly seen when the winds blow from them.

And perhaps a cold northerly freezing air may be needful to kindle the vapours, when a contrary motion above, higher in the atmosphere, may carry the sulphureous vapour, which falling down from the nitrous vapour may be kindled. Which, I suppose, from the undulations of the smoke after the explosion, which seemed, as above, like a stormy sea moving from the S. S. E. Note, the barometer was low for some days before and after it.

An Account of the Aurora Borealis that appeared Oct. 8, 1726. *By the Rev. Dr. Langwith, Rector of Petworth in Sussex.* N° 395, p. 132.

After describing several of the phænomena, much as usual, Dr. L. says the north-easterly stream suddenly expanded itself every way; all its parts began to be in a violent commotion, and its brightness increased to such a degree, that he remembered nothing like it in the former great meteor of this kind. All above it was of a bright flame-colour; but below it was edged with the prismatic colours, which were full as strong as ever seen in the brightest rainbow; they were not indeed so distinct, as he could only distinguish the red, the yellow, and a dusky bluish green.

This surprising sight did not last above a minute or two; but when the colours vanished here, they began to appear in the north-westerly arch, which was now become a portion of a larger circle than before, and was not elevated so high above the horizon. The colours extended themselves from the north towards the west for about 15 or 20 degrees; and though they were not so bright as in the other place, yet they were more steady, and so as easily observed. Their order was the same as before, the red lowermost, and so on.

In the mean time the streaming lights began to appear in all parts of the heavens, and to form a corona and canopy, which were in all respects like those of the great meteor of 171$\frac{5}{6}$. The streams continued their direction upward towards a point of concourse for a long time after, and formed by fits imperfect circles of pale light about it; this point however was not fixed; for at first it seemed to be in, or very near our zenith; but shifted afterwards.

Hence, 1. It plainly appears, from the position of the arches, that they could not owe their figures to the sun: they seem to have been partly optical, and partly to have depended on the different heights of the luminous vapours; but for want of sufficient data, it will be no easy matter to determine how far each of these causes concurred. 2. The prismatic colours, wherever they appeared, seem to have been caused by the sun. 3. None of the streams proceeded directly from the horizon. They were nearest it towards the north, where there were some weak irregular lights in the confused parts of the arch before described.

An Account of the same Phenomenon at Plymouth. By Dr. Huxham, Physician at Plymouth. N° 395, p. 137.

As the particulars of this description are nearly the same as in the foregoing articles, it is needless to repeat them.

Dr. Huxham at last adds, as to the weather, the morning was fair, though the air was thick, and we had a great dew: the mercury was at 30 inches, Hauksbee's thermometer at 50, little or no wind. The day was pleasant and warm, and the air grew much thinner. The evening was serene; a very soft breeze from N. and by W. About 5 the next morning, there were several clouds formed, and the air was very thick and hazy, at 7 it was all cloudy, and a few drops fell.

Though I had before seen several faint appearances of the aurora borealis; yet this, for beauty, lustre and duration, vastly exceeded any thing of that nature I had ever seen. Indeed, I saw not that of March 6th, 1716, not being then in England.

On the same Subject. By Dr. Hallet, Physician at Exeter. N° 395, p. 143.

Exon, Oct. 12, 1716.

Oct. 3, 1726, at 9 in the evening, I saw an aurora borealis, in which there was nothing different from former appearances, excepting that from the luminous arch which appeared in the north, were frequently shot off parts of arches towards the zenith, which vanished there.

After describing the appearances more particularly, Dr. Hallet concludes with the following reflections as to the cause of the phænomena.

The best account I can give of this phenomenon is this: I imagine a thin cloud composed of a sulphureous exhalation, hanging over us in the air, at a considerable height, parallel to the horizon; the length of it being very great from east to west nearly; the breadth of it at first not so great, but that we might see the star's from under it to the north and south. The north side of it I suppose first took fire, and shot its streams or flames perpendicularly upwards, which being undisturbed by winds, must appear straight and pointed at the top. The bases must make an arch by the rules of perspective: for I think a horizontal right line, of a vast length, and at a great distance from us, such as I take the northern edge of this luminous cloud to have been, seen at a considerable height in the air, must appear bent down into an arch. On a sudden the fire propagated itself to all parts of this vapour. The whole heaven must then appear covered with the same streams, which though really parallel to each other, must appear bent into a cupola. The shooting and darting of these flames, and their concourse, together with a smoke proceeding from them, must give that confused cloud which was observed in the centre of this canopy. I think the red appeared at the right hand in all of the colours, which were regularly disposed in every stream. Somewhere in the Philos. Trans. I have met with an observation of an aurora, in which the streams were coloured only where they met, or crossed each other. Whether the light of one stream passing through another, may not be separated into colours by refraction, I will not determine.

The same Appearance described by John Hadley, Esq. F.R.S. N° 395, p. 146.

This description of the appearances is much the same as the preceding ones.

The same Phenomenon observed at Geneva. By J. L. Calandrini, Professor of Mathematics there. N° 395, p. 150.

The description is much the same as the foregoing. M. Calandrini, in conclusion says: it is affirmed, that those northern lights are produced by the reflection of the sun's light from the northern frozen parts of the atmosphere; but he does not see how such remarkable flames can be explained. If this phenomenon be supposed to arise from the accension of exhalations, the aurora borealis that accompanies the phenomenon, the columns, the duration of the appearance, and its continuing in the same place, will be the grand difficulty.

An Account of a Preternatural Bony Substance found in the Cavity of the Thorax. By Mr. William Giffard, Surgeon: and a further Account of it, by Dr. Rutty. N° 395, p. 152.

May the 10th, 1726, Mr. Giffard opened the body of a person who died of a peripneumonia. In the right side of the thorax was found an osseous substance, about $\frac{1}{4}$ of an inch thick, 6 inches long, and 3 broad, extending itself under the 3d, 4th, 5th and 6th ribs, closely and strongly connected to the periosteum of the ribs, and the tunica propria of the inner intercostal muscles, by fibres which shot from a strong and thick intervening membrane, which closely adhered to its outer surface or back, and was continued over its inner likewise, thereby forming a capsula for this preternatural substance. Its upper edge lay immediately below, and was contiguous to that part of the ribs, where they become cartilaginous: the membrane that adhered to, and covered it, continued thick some distance from it, and gradually grew thinner, and was at length lost in the pleura; so that this extraneous body seemed to have been formed between the two lamellæ of that membrane. The lungs so strongly adhered to its inside, that on separating them, part remained to it: on cutting into which, the cells were stuffed with a thin, but somewhat digested pus.

On the same side, towards the back, was another substance, but perfectly bone, invested as the former, with a strong and thick membrane, and by the fibres which shot from it, tied to the body of the vertebræ, and the 4th, 5th, 6th, and 7th ribs, and intercostal muscles. Its back or outside was convex, according to the concave surface of the ribs, which had made indentations on that part of it where they pressed: its inside was concave as the ribs; the right lobe of the lungs strongly adhered; part of which for that reason remained to it after separation: its length was about 7 inches; breadth about 3, from edge to edge; its thickness, in some parts $\frac{1}{3}$, in others $\frac{1}{4}$ of an inch. It had, as the former, a thick membrane running from it, which gradually thinning was at length lost in the pleura; from whence Mr. G. judged this extraneous substance to have been formed as the forementioned.

" Since Mr. Giffard has not inserted the particulars of this gentleman's indisposition, Dr. Rutty adds, that on inquiry, he has since been informed, that the patient was troubled for some years with a short cough, which latterly was accompanied with a difficulty of breathing, and great labour in inspiration; and some time after, with a weight and pain on the right side; which symptoms increasing more and more, brought on the peripneumonia, of which he died in a few days. The rise and succession of those different symptoms proceed so naturally from the formation and growth of this preternatural body, as well as the particular part of the thorax where it was lodged, that it is entirely needless to say any thing more upon them.

" W. RUTTY."

On the Leaf of a Plant lodged in a Piece of Amber. By Dr. Breynius. N° 395, p. 154. *Translated from the Latin.*

Whoever examines the museums of the curious, and their amber rarities, and peruses the authors who have treated this subject, he will, Dr. Breynius thinks, acknowledge with him, that among natural bodies included in amber, those from the vegetable kingdom are the most uncommon; and of these, the parts of the more perfect plants, as the leaves, pods, flowers, &c. if any such be found, are the most so. The reason of this doubtless is, that, according to the opinion of the moderns, amber is naturally prepared in subterraneous places, to which the parts of vegetables, as growing on the surface of the earth, do with difficulty, and but accidentally, or very rarely reach; while insects, though inhabitants of the air, in order to defend themselves from the cold, and other inclemencies of the weather, or for some other reason, frequently and spontaneously retire into chinks, hiatuses, and subterraneous cavities, and there hasten to their entombing; and where they are entangled, involved and suffocated, in the amber, which is still in a liquid state, and with which they harden, and so are embalmed as it were for ever.

One Philip Benlows showed the Doctor a glebe of this kind, which included a leaf, and which he valued at upwards of 30 florins. It was almost of an oval, but compressed figure, and of the size represented fig. 12, pl. 3; it was $\frac{1}{4}$ of an inch thick, and of that kind, which, from its resemblance to the colour of the wine of that name, is called Falernian amber; it was pretty clear and pure, and without affording the least suspicion of a cheat; quite through the middle lengthwise, it included an expanded leaf of the pennate kind, or according to others of the alate, though not so properly; obscure, it is true, but of a shining gold colour, it yielded a very agreeable sight by the reflection and refraction of the rays of light. This leaf was not entire, but broken off at both extremities, as represented in the figure, and it consisted of 5 oblong small leaves, somewhat pointed on both sides, in conjugations, almost equally distant from each other on the common rib; some of these small leaves were worn off and broken. The leaf itself had an horizontal position, which is common in that kind of leaves, only that the small leaves by their obliquity deflected a little from it: but the conjugations of the small leaves did not at all seem posited across; though that always obtains in such as botanists call the conjugate. So that the Doctor no longer doubted, but that this leaf was of the compound pennate kind: but of what particular species of plants was hard to determine; because several species of this family have leaves so much resembling each other, that though fresh, it is difficult to distinguish them: add to this, that even the little veins of the small leaves did not appear to the armed eye, as having been obliterated by the amber in its liquid state, and as it were incrustated with it. It nearly resembled securidaca secunda clusii, or coronilla herbacea, &c. Tournefortii, which is pretty common among the thickets of Prussia. On one side a spider was pretty distinctly seen between two of the small leaves; and on the other a small fly, but these could not be seen without a glass.

The Doctor does not remember to find in any author mention made of a leaf of the pennate kind included in amber, excepting in Michael Mercatus's curious work, entitled Metallotheca Vaticana, published by Joh. Maria Lancisi; where among other figures of elegant glebes, containing a frog, a smaller fish, lizard and other insects, there is one, that includes such a pennate leaf somewhat less, but more curious and elegant, because entire, and of 8 conjugations, the last small odd leaf terminating the extremity of the rib; and this greatly resembles the small and tender leaf of coronilla herbacea, flore vario Tournefortii; though it may very well be referred to Onobrychis secunda Clusii, which is likewise a native of Prussia.

The same Mercatus in the same place delineates another glebe, which includes a small jagged leaf of some plant, probably of the umbelliferous kind.

However, since the Doctor had not seen the said glebes, though very accurately delineated in the abovementioned treatise of Mercatus; and besides, since the figures of a large frog, a small lizard and fish, gave him no slight grounds of suspecting some cheat; he would not take upon him to answer for their being genuine, because it is very well known, that such bodies may be so artfully included in amber, as to impose on the less knowing, and the cheat not be discovered but by the most skillful and accurate observers.

A Solar Eclipse observed at Padua, Sept. 25, 1726. *N.S. By Sig. J. Poleni, Astron. Prof. F.R.S.* N° 395, p. 157. *Translated from the Latin.*

The beginning was not seen for clouds, nor the end; only some phases as below.

At 5^h 29^m 5^s...... 1 dig. was eclipsed.
5 34 27 ...... 2 digits.
5 40 43 ...... 3 digits.
5 47 15 ...... 4 digits.

A Lunar Eclipse, observed at Padua, Oct. 10, 1726. *By the same.* N° 395, p. 158. *Translated from the Latin.*

The end of this eclipse was not seen by reason of clouds. But the beginning was observed, as follows:

At 16^h 16^m 44^s A very thin penumbra.
16 21 19 The beginning of the true shadow.

Two newly discovered Arteries, going to the Ovaria in Women. By Mr. Ranby, Surgeon to his Majesty's Household. F.R.S. N° 365, p. 159.

In the Philos. Trans. N° 387, Mr. R. presented the Royal Society with a description of two arteries, which arising from the aorta, and sending branches to the glandula renalis, then descend to the testicles. And he has since found the same arteries in women, descending in the same common capsula with the spermatic artery and vein, to the ovaria. These arteries very probably are what the late excellent anatomist, Valsalva, took for excretory ducts of the glandulæ renales, the disposition and progress of these being very much alike to what has been ascribed to those supposed ducts.

A short Account of Negativo-affirmative Arithmetic. By Mr. John Colson, F.R.S. N° 396, p. 161.

The usefulness of this arithmetic consists in this, that it performs all the operations with more ease and expedition than the common affirmative arithmetic, especially in large numbers: and it differs from the common arithmetic chiefly in this, that it admits of negative figures promiscuously with the affirmative. These negative figures are distinguished from the affirmative, by the sign - placed over them. Thus $3\bar{7}09\overline{28}65\bar{7}3\overline{961}47\bar{2}$ is one of these numbers, which may be converted into its equivalent common number 2308726432039468, in this manner:

$$\begin{array}{r}3009006503000470\\ \underline{0700280070961002}\\ 2308726432039468\end{array}$$

1. Write down all the affirmative figures by themselves, putting a cypher in the place of every negative figure. 2. Write down all the negative figures by themselves, putting a cypher in the place of every affirmative figure. 3. Subtract the last number from the first, and the remainder will be a common number, equivalent to the given negativo-affirmative number. See the operation above.

But the readiest practical way of performing this reduction in any given number, is in this manner: begin at the left hand; and going over all the figures in order, observe these rules. 1. An affirmative figure before a negative must be diminished by a unit. 2. A negative figure before an affirmative, must be changed into its complement to 10. 3. A negative figure before a negative, must be changed into its complement to 9. All other figures must remain unchanged, and a cypher is always to be understood where there is no significant figure. The sign of the cypher is neglected; but where there is occasion to consider it, it is always supposed the same as the sign of the following figure. Thus the negativo-affirmative number $72\bar{9}\bar{5}86\bar{4}\bar{5}\bar{5}98\bar{2}00\bar{1}\bar{7}30$ is immediately reduced to 710585545977998330; and so of all others.

But on the contrary, common numbers may be reduced to negativo-affirmative numbers a great variety of ways, by substituting instead of the figures 1, 2, 3, 4, 5, 6, 7, 8, 9, their respective values $1\bar{9}$, $1\bar{8}$, $1\bar{7}$, $1\bar{6}$, $1\bar{5}$, $1\bar{4}$, $1\bar{3}$, $1\bar{2}$, $1\bar{1}$, in any places at pleasure. But the most useful reduction of this kind is what Mr. C. calls a reduction to small figures, which consists in throwing out all the large figures, 9, 8, 7, 6, out of any given number, and introducing in their stead the equivalent small figures $1\bar{1}$, $1\bar{2}$, $1\bar{3}$, $1\bar{4}$, respectively. Thus 182937462 may be reduced to $2\bar{2}3\bar{1}4\bar{3}5\bar{4}2$, consisting only of small figures. But this reduction may be performed more readily by these rules following.

1. A small figure before a large figure must be increased by a unit. 2. A large figure before a large figure must be changed into its negative complement to 9. 3. A large figure before a small figure must be changed into its negative complement to 10. Other figures are not to be changed; and 5 will be ambiguous, being to be esteemed either large or small, according as the figure following is either large or small. Some examples of this reduction are as here follow, both in whole numbers and decimal fractions.

$$37068259764 = 4\bar{3}1\bar{3}\bar{2}3\bar{4}0\bar{2}\bar{4}4$$

$$729528960739957 = 1\bar{3}3\bar{1}53\bar{1}0\bar{4}1\bar{3}400\bar{4}\bar{3}$$

$$9260872395{,}87294 = 1\bar{1}3\bar{4}1\bar{1}\bar{3}240\bar{4}{,}\bar{1}\bar{3}3\bar{1}4$$

$$\text{Or } (9)\ 926087239587294 = (10)\ 1\bar{1}3\bar{4}1\bar{1}\bar{3}240\bar{4}\bar{1}\bar{3}3\bar{1}4$$

$$(m)\ 387916407953\ \&c. = (m)\ 4\bar{1}\bar{2}\bar{1}2\bar{4}41\bar{2}\bar{1}53\ \&c.$$

It is to be observed, that in this last example the numbers are what Mr. C. calls interminate, or approximations only; that is, the first and most valuable figures are expressed, and all the rest (whether finite or infinite in number, whether known or unknown) are omitted as inconsiderable, and insinuated by the mark, &c. Also the index m before the number stands for some integer, expressing the distance of the first figure 3 or 4 from the place of units; which integer is either affirmative or negative, according as the said first figure stands in integral or fractional places. The example immediately before is a particular instance of this.

After which Mr. Colson shows how the common operations in arithmetic is to be performed with these numbers. But as it is not likely the method will ever be brought into practice, the above specimen may suffice.

A Lunar Eclipse observed at Rome, Oct. 31, 1724, *N. S. By Sig. Bianchini.* N° 396, p. 174. *From the Latin.*

At	15^h	15^m	40^s	the beginning of the eclipse, or true shadow.
	17	48	50	the end of the same : true time.

Eclipses of Jupiter's Satellites in 1724. *By the same.* N° 396, p. 176.

In 1724, near Bolsena in Italy, by true time.

June 8^d	14^h	48^m	30^s	the 2d sat. emerged out of Jupiter's shadow.
	15	0	30	the 3d sat. enters Jupiter's limb.
	15	27	10	the 1st sat. enters the shadow.

At Rome, by true time.

June 24,	1	39	0	the light of the 2d sat. began to weaken.
	1	40	20	it was entirely immerged.
	1	41	50	the light of the 1st sat. began to diminish.
	1	42	50	it was entirely immerged.
June 30,	15	23	34	the light of the 1st began to weaken.
	15	24	29	it was entirely immerged.
Aug. 18,	0	40	45	the 1st sat. emerged from the shadow.
Sept. 25,	11	25	55	the 1st sat. began to emerge.
	11	27	5	it was entirely emerged.
Oct. 11,	9	53	8,	at Albano, the 1st sat. began to emerge.

An Eclipse of the Moon observed at Albano, Oct. 21, 1725, *N. S. By the same.* N° 396, p. 179.

The beginning was not seen for clouds.

At	6^h	15^m	6^s	the shadow touched the moon's centre.
	6	45	0	the moon was entirely immerged.
	8	27	0	the beginning of the emersion.
	9	25	0	the end of the true shadow

Observations on the Spot Plato, in the Moon, August 16, 1725, *N. S. By the same.* N° 396, p. 181.

The raised edges in the border of the spot were enlightened by the sun, and shone with the usual brightness. The bottom of the spot was dark, the sun's rays not having yet reached it: but a reddish kind of light was projected over the middle area of the spot, as in fig. 13, pl. 3, as if some hole was in that part of the edge A opposite to the sun, through which a ray of the sun entered.

It is imagined that this appearance may be either owing to a hole in the border, as just mentioned, or to the refraction of some solar ray in the summit of that edge, whence the ray might pervade the internal parts of the spot itself: so that it would seem that the moon has some sort of an atmosphere about her.

A Remark on the new Opinion relating to the Forces of moving Bodies, in the Case of the Collision of Non-Elastic Bodies. By Mr. John Eames, F.R.S.* N° 396, p. 183.

The antient opinion, and most generally entertained, is, that the forces of bodies in motion are as their quantities of motion, i. e. as their quantities of matter multiplied by their respective celerities.

The new opinion is, that the forces of bodies in motion are as their quanti-

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* Mr. John Eames, was a very useful member of the Royal Society, besides a general and profound scholar, being deeply skilled in all branches of learning. He was a native of London, and educated at Merchant-Taylors' school. He afterwards pursued a course of academical studies, with a view to the ministry among the Dissenters; but he never preached more than one sermon, when, from excessive diffidence, he was so exceedingly agitated and confused, that he was scarcely able to proceed. There was also a great defect in his organs of speech, and his pronunciation was exceedingly harsh, uncouth, and disagreeable. Hence, quitting the pulpit entirely, Mr. E. devoted himself to the instruction of young men whose education for the pulpit, among the Protestant Dissenters, was patronized and assisted by the Independent Fund. His department included the languages, mathematics, with moral and natural philosophy; to which were afterwards added divinity and the oriental languages. Dr. Watts, who dedicated to Mr. Eames his treatise on Geography and Astronomy, said of him that he was the most learned man he ever knew. His scientific learning procured him the esteem and friendship of Sir Isaac Newton, who introduced him to the Royal Society. Mr. E. was one of those learned men who wrote in defence of Newton's doctrine of moving forces; and he was also usefully employed by the R. S. with other learned members, in preparing and publishing an Abridgment of the Philos. Trans. Among those persons who were formed under Mr. E. for the stations which they afterwards filled with reputation and honour, were Dr. Furneaux, Dr. Price, and Dr. Savage. Archbishop Secker also received part of his academical education under this learned man, who died suddenly, June 29, 1744. Mr. Eames's contributions to the Philos. Trans. are in vols. 34, 35, and 40.

ties of matter multiplied by the squares of the velocities; so that in equal masses the moving forces will not be as the velocities themselves, but as their squares.

The latest experiments, brought to prove the truth of the new opinion, are made on soft or yielding substances. Now these have been already observed to be a little complicated, and unfit for this purpose. The proper use of experiments of this kind being rather to discover, and settle the laws, which such kind of substances observe in the resistance they make to bodies moving in them, than the forces themselves, with which the bodies move; which ought to be determined before hand by some simple experiment, fit to determine that matter.

A variety of experiments have been made, and reasoning used in England and France, to prove the truth of the common opinion; but they do not entirely satisfy all the gentlemen on the other side of the question. The present professor of mathematics and philosophy at Utrecht (Muschenbroeck), tells us in the preface to his Epitome Elementorum Physico-Mathematicorum, published this year, anno 1726, "In computing the forces of moving bodies, I have embraced the opinion of M. Leibnitz, Huygens, Poleni, and Gravesande, and have rejected the old opinion, which I formerly maintained and taught; and that notwithstanding the arguments of very learned men, both in France and Britain, who maintain it. And when the experiments, described by Poleni and Gravesande, are examined, they so plainly prove the forces of striking bodies to be in a ratio compounded of the square of the velocities, and of the simple ratio of the masses; that we must submit to them, unless we would contradict the plainest evidence."

Mr. Eames examines the truth of the new opinion in the case here proposed, viz. as to the forces of striking bodies; and endeavours to show, from their own principles, that it cannot be true in all the cases of non-elastic bodies.

It is allowed, that the common rules of finding the velocities of non-elastic bodies after the stroke are true: for thus the ingenious Mr. Gravesande says in paragraph 251, of his Supplementum Physicum: "From this principle, i. e. of multiplying the mass by the velocity, philosophers have deduced these very rules, N° 234 and 237, which I have several ways deduced from my principles: but it was surprising that in this case one error destroyed another, and a double error led me to discover the truth: they followed a false principle in computing the forces, and supposed what is not at all consonant with truth, viz. that bodies lose nothing of their force by the intropression on the parts, and overcoming their cohesion."

Now the rule for finding the common velocity of non-elastic bodies, moving the same way after the collision, is, to divide the sum of the quantities of motion in the two bodies, by the sum of the quantity of matter. It is also granted, "that these bodies cannot mutually act on each other with the quantity of motion common to both, sect. 215. The stroke therefore depends on the relative velocity, which remaining, the intensity of the impulse will be the same, in whatever manner the absolute velocities may vary. On this intensi depends the intropression of the parts, which will therefore be always the same, if two bodies impinge with the same relative velocity, whatever be the velocities they move with."

These principles furnish us with an argument against the new opinion. For if it be true, then equal causes may have unequal effects, and that in their own sense of an effect: the proof shall be taken from instances of the effects of the collision of non-elastic bodies, whose respective velocities shall be always equal.

Let A and B denote two non-elastic bodies, of equal quantities of matter; and let B be at rest, while A moves towards it with 8 degrees of velocity. Here the common velocity after the stroke will be half the velocity of A before the stroke, i. e. 4 degrees. Consequently the force in B thus communicated by the stroke, will be as its square, or 16.

Let B move forward with 2 degrees of velocity, and A follow it with 10 degrees; the respective velocity will be 8 as before; consequently the strokes in both cases are equal. The velocity in B after the stroke will be half the sum of the velocities before the stroke, or 6 degrees.

According to the new opinion, the forces being as the squares of the velocities, the force of B before the stroke will be to its force after the stroke, as the square of 2 is to the square of 6; i. e. as 4 is to 36. Subduct the force in B before the stroke, from the force it has after the stroke, and we have the degrees of force communicated by the stroke: which, if this opinion were true, would be 32, i. e. just double the number of degrees communicated by the same force in the former instance, which was but as 16. Thus equal strokes produce unequal effects in our sense of effects.

The following table gives several other instances. In the first three columns are the velocities of the two bodies both before, and after the stroke; in the two next, are the forces in B, both before and after the stroke; and in the sixth, the difference of those forces, or the different degrees of force effected by the same stroke; and in the last column, the proportion of those forces, or effects of the cause or stroke.

The Velocity in			The Forces in.		Force communicated by the Stroke.	Proportion.
A	B	B	B			
8	0	4	0	16	16	1
10	2	6	4	36	32	2
14	6	10	36	100	64	4
18	10	14	100	196	96	6
22	14	18	196	324	128	8
26	18	22	324	484	160	10
Before		After	Before	After		
the stroke.			the stroke.			

If it be said, that Mr. E. has not considered the other part of the entire effect of the stroke, the intropression of the parts; he replies, this will make but a small alteration in the matter; since the intropressions in all these cases are equal, the relative velocities being by supposition the same: so that notwithstanding, upon the whole, one and the same, or equal causes, will produce unequal effects.

Remarks on a supposed Demonstration, that the moving Forces of the same Body are not as the Velocities, but as the Squares of the Velocities. By the same. N° 396, p. 188.

The demonstration runs thus: "I conceive that the body C, fig. 14, pl. 3, impinges obliquely on the spring L with the velocity CL, as 2, the angle of inclination CLP being of 30 degrees, whose sine CP is half the radius CL. I suppose the resistance of the spring to be such, that to bend it, there is precisely required one degree of velocity in that body, should it impinge perpendicularly; what then, shall be the consequence after an oblique impulse of C against the spring L? since the motion in CL is compounded, as is well known, of the two collateral motions in CP and PL; and since CP, according to which the body directly impinges on the spring L, represents half the velocity of the body through CL, this motion through CP will be destroyed, when the spring is bent; for it would be the same thing, as if the body C should with the velocity CP, perpendicularly impinge on the spring, which by the hypothesis might destroy that velocity, the velocity of the body and the direction PL continuing the same; producing therefore, PL to M; so that LM be $= PL = \sqrt{3}$, for CL is supposed $= 2$, and applying in M another similar spring, forming with LM the angle LMQ, whose sine $LQ = CP = 1$; by the same reason it is manifest, that the body C, after the bending of the spring L, will bend the spring M, losing the motion through LQ, and retaining that through QM; producing therefore QM to N, that MN may be $= QM = \surd 2$; and putting there a third similar spring, forming with MN half a right angle, as MNR; so that MR be again $= CP = 1$; in like manner it is evident, that the motion through MR is entirely expended on bending the spring N, the body in the mean time continuing to move with the direction and velocity $RN = 1$. In fine, if with this remaining velocity, it should perpendicularly impinge on the spring O, it will entirely communicate the rest of its force in bending it; and therefore the body itself will be brought to a state of rest. From these premises it now appears, that the force of the body C was so great, that by itself alone, it could precisely bend 4 such springs, to bend each of which apart, there is required half the velocity of a body equal to C itself, consequently, since the effect of the former is 4 times greater than the effect of the latter, it is likewise evident, that the force of a body of 2 degrees of velocity, is 4 times as great as the force of the same, or an equal body of one degree.

In much the same manner I might demonstrate, that the body C with a velocity of 3 degrees may bend 9 springs, to bend one of which there is one degree of velocity required in that body; and in general, that the number of bent springs is always the square of the number of the degrees of velocity; whence therefore it will follow, that the forces of equal bodies are in a duplicate ratio of their velocities. Q. E. D."

This argument is founded entirely on the commonly received doctrine of the composition and resolution of forces, and not on any decisive experiments, that have been actually made on this occasion. All that is proved from this doctrine is, that a body moving with 2 degrees of velocity, may be made to bend 4; with 3 degrees of velocity it may be made to bend 9 similar springs, each destroying one degree of velocity in a perpendicular direction, before its force is entirely spent, provided care be taken to alter the directions of the motion in every stroke but the last, after a certain manner; that had the same body moved but with one degree of velocity in one direction, and that in a perpendicular one, it would have lost all its force at once, and bent but one of those springs; which is far from proving the thing in question.

To make the reasoning on this head conclusive, the two bodies should not only be equal in quantity of matter, but alike in that material circumstance, the direction of their motions; so that if one of the bodies move in a perpendicular direction, the other should do so too; or if the one strikes in an oblique direction, the other should do the same, and that in the same degree of obliquity; and lastly, if one moves in several directions, the other should do the same. But in the case before us, one is supposed to move only in one direction, perpendicularly, and the other to move in three oblique directions, and but one perpendicular.

Let therefore the same body move always in the same directions; and, with a small alteration, the argument used in this demonstration will be so far from proving that side only of the question for which it was brought, that it will equally serve to prove the truth of the other, namely, that the forces of the same body moving with different velocities are as those velocities. Let therefore the same body, instead of moving with 2 degrees of velocity, move with only one, and in the same directions as above; only let the springs be capable of destroying but half a degree of velocity in a perpendicular direction; then by the same steps of reasoning it will follow, that this body will now also bend 4 similar springs, before its force is spent; so that the same body moving with half the velocities, and in the same directions as before, bends the same number of springs; only now the springs make but half the resistance, that the springs in the former case made; therefore the effect in this case, according to our way of estimating an effect, is only half the former effect; consequently the forces producing these effects are as 2 to 1; but in this ratio are the velocities, with which the body moved in the two cases, therefore the forces are as the velocities.

Let the body move with 3 degrees of velocity, and it will bend 9 similar springs, each destroying one degree of velocity in a perpendicular direction, before the whole force is consumed. So also by the same way of arguing, it is as certain, that if the same body move with one degree of velocity, it will bend 9 similar springs, each destroying a third part of one degree of velocity in a perpendicular direction, before its force is extinguished; so that still the effects, or resistances overcome, in the same directions, are, according to our way of computing, as 3 to 1; and so also their forces must be but in the same ratio of 3 to 1, as were the velocities; consequently the forces are as the velocities.

Since therefore this proof, drawn from the doctrine of composition and resolution of forces, equally proves both sides of the question, it proves too much, or in reality nothing at all; and is therefore far from deserving the name of a demonstration.

Brevis Commentatio de Cobalto, Auctore Viro Clarissimo Joh. Henr. Linckio, Lipsiensi, Acad. Cæsar. Leopold. Carolin. Nat. Curios. et Soc. Reg. Anglic. Sodale. Lipsiæ, mense Quintil. A. R. S. 1726. N° 396, p. 192.

There is nothing in this account of Cobalt that is in the least degree interesting at the present day, when the natural history, chemical properties and uses, of this, and many other metallic bodies, are so much better known than they were at the abovementioned date. Indeed Mr. Linck's experiments threw very little light upon this colouring material, the true metallic nature of which was not ascertained till about 7 years after, by the Swedish chemist Brandt.

Remarks on some Dissertations lately published at Paris, by the Rev. P. Souciet, against Sir Isaac Newton's Chronology. By Dr. Edmund Halley, F. R. S. N° 397, p. 205.

There having been lately put into my hands a book published at Paris by Father Souciet, Jesuit, against Sir Isaac Newton's Chronology, without waiting till the book be published, and without knowing its contents, otherwise than by a short extract, made at the desire of a very great person, and without intention that it should be publicly seen. However, a copy of it having been, as I suppose, surreptitiously obtained and carried over into France, the same was first translated into French, and then printed at Paris, with a pretended refutation, by the same P. Souciet. Since that time, Sir Isaac having answered,* as he thought, his objections, has thereby given him a handle to publish five other dissertations against the new system of chronology, as he calls it; the first and last of which, being chiefly astronomical, since the great author is no more, seem properly to fall under my examination, both on account of the post in which I have the honour to serve his majesty in quality of his astronomer, as also from the long acquaintance and friendship that subsisted between the deceased and myself.

And first, I observe, that P. Souciet readily allows what seems to be the most exceptionable part of the whole system, viz. that Chiron the Centaur fixed the colures, in the ancient sphere of fixed stars, in the same places as Hipparchus tells us they had been supposed by Eudoxus, many centuries of years after Chiron. His words are these, ἐν δὲ τῷ ἑτέρῳ κολȣ́ ᾧ φησὶ κεῖθαι τȣ͂ κήτȣς τὴν κεφαλὴν, καὶ τȣ͂ κριȣ͂ τὰ νῶτα κατὰ πλάτσος. This, undoubtedly, was the position of the colure of the vernal equinox many ages before Eudoxus; but whether so old as Chiron, and the Argonautic expedition, I shall not undertake at this time to inquire; but only observe, that P. Souciet, in his Fastes du Monde, or abridgment of his chronology, prefixed to these dissertations, makes the Argonautic expedition 1467 years before our æra of the birth of Jesus Christ; and the taking of Troy 1388 years before it; which date is 120 years sooner than the Parian Chronicle, read and published by our learned Selden, in his Marmora Arunde-

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* See p. 89, of this volume.

liana, makes it; and above 500 years earlier than the time assigned by Sir Isaac Newton.

Now both of them making use of the same premises, it may seem strange that their conclusions should be so widely distant; and indeed on a prepossession that the Argonautic expedition, and the siege of Troy, could not have been less than 1000 years before Christ, I must own, I was at first somewhat prejudiced in favour of P. Souciet, taking his calculations for granted, and not having seen Sir Isaac's work. But observing that he quotes Sir Isaac, as saying, that in consequence of what Hipparchus has recorded from Eudoxus, the equinoctial colure in the old sphere was about 7° 36′ from the first star of Aries, I was resolved to examine the matter with due attention, especially since the good Father seems to triumph over his adversary, and to treat a man of his figure in the commonwealth of learning in a very ludicrous manner, notwithstanding the several fine things he says of him to palliate it.

I find the dispute to be chiefly over what part of the back of Aries the colure passed; the words of Hipparchus, as from Eudoxus, are simply, that it passed over the back, without saying over what star, or over what part of the back it passed. And the same Hipparchus shows, that if it passed over the star in the middle of the back, it greatly differed from its situation in his time; and conceiving thence that the equinoctial points might have a regressive motion, he was the first that attempted to define their motion; but having no observations older than those of Tymocharis, made within less than 200 years of his own time, and very coarse withal, he was not able to determine its quantity, but guessed it to be about a degree in 100 years; which length of time, and the more curious observation of the moderns, has now proved to be 1° 24′, or rather 50″ per annum.

In a word, Sir Isaac takes the colure to have passed over the middle of the constellation of Aries, and very near the star in the middle of the back, ν Bayero. And P. Souciet will have it, that it passed over the middle of the sign of Dodecatemorion of Aries, reckoning the sign to begin with the first star of the constellation; and consequently his colure must pass about midway between the rump and first of the tail of Aries, ε and δ Bayero, which situation could never be said to be over the back; but while Sir Isaac makes the colure but 7° 36′ from the first star of Aries, which P. Souciet makes 15 degrees from it, the difference 7° 24′, at 50″ per annum, makes 533 years difference in the result.

Let us now examine when the stars in question did actually pass under the colure of the vernal equinox, assuming their places as they are in Mr. Flamsteed's British Catalogue, fitted to the beginning of the year 1690. He places the first star of Aries in 28° 51′ of Aries, with 7° 9′ north latitude. And sup posing the obliquity of the ecliptic 23° 29′, it will be, as radius to the tangent of 23° 29′ :: so the tangent of 7° 9′, to the sine of 3° 7½′, the difference of longitude between the star and the point in the ecliptic which passed under the colure at the same time with the star; so that this point was the beginning of the year 1690, in ♈ 25° 43′ 30″, and therefore, allowing 50″ per annum, the star was under the colure 1852 years before the epocha of the British Catalogue, that is, 162 years before our æra of the nativity of Jesus Christ; in which very year Hipparchus began to observe the equinoxes recorded by Ptolemy, lib. 3, cap. 2.

If therefore, with Sir Isaac, we add 7° 36′ to the long. of the first star of Aries, as it was in 1690, we shall have 36° 27′, which the colure moves in 2624 years, and deducting 1690, we shall have 934 years before Christ for the Argonautic expedition. And if to 7° 36′ we add 3° 7½′, we shall have 10° 43½′, that is, 772 years before the first star of Aries passed the colure.

Next let us inquire when the star in the middle of the back of Aries, ν Bayero, passed the colure. Its longitude anno 1690 ineunte, was 9° 48′ 35″ of Taurus, with north lat. 0° 8′; but by the foregoing analogy, the point in the ecliptic, over which the colure passed at the same time with it, was 2° 40½′ before it, that is, in ♉ 7° 8′. Now 37° 8′ give 2674 years nearly, or 984 years before Christ, when that star was under the equinoctial colure, being but half a century earlier than Sir Isaac places the Argonautic expedition; and shows that he took the middle of Aries over which the colure is supposed to have passed, to be the middle of the constellation, and not of the Dodecatemorion, and in so doing, no doubt, had reason to place this colure 7° 36′ in consequence of the first star of Aries, instead of 8° 17′, as it was when the star in the middle of the back of Aries was under the colure.

But if, with P. Souciet, we make the colure to intersect the ecliptic 15 degrees from the first star of Aries, or 43° 51′ from the equinoctial point, as it was anno 1690, we shall have the time nearly 1470 years before Christ; but then the colure will be very far from the middle of the back of Aries, and leave only his tail to the eastward, as it leaves the head of the whale to the westward, so as by no means to agree with the description we have of it from Hipparchus; which it were to be wished had been more definitive, and as well circumstanced as what Hipparchus has left us of the position of the colures in his own time, which on examination I find to be very consistent, and the observations made with sufficient care.

Thus I hope, I have shown P. Souciet, that there was no affectation of mystery in Sir Isaac's placing the colure 7° 36′ from the first star of Aries, nor any occasion to drole as he does p. 131, 132, on that account; as also that he ought to have deducted $3^\circ\ 7\frac{1}{2}'$ out of the 15 degrees he assumes for the distance of his colure from the first star of Aries, which will bring him 225 years nearer to Sir Isaac Newton's time. He is likewise entreated, in the next edition of his dissertations, to be a little more careful of his numbers, than he has been p. 134, 135, and to inform himself in the spherics, so as to give us the right ascensions of the stars truly, from their given longitudes and latitudes.

Lastly, I would inform him, that the star in the Centaur which Hipparchus describes, as being in his time very near the autumnal colure, was not Ψ of Bayer, but certainly Φ, and that anno ineunte 1690, its longitude was Scorpio 8° 43′ 40″, with south latitude 27° 59′. But the colure passing through that star, by the proportion given above, cuts the ecliptic 13° 20′ 50″ in antecedence of the star, that is in Libra 25° 22′ 50″. But 25° 22′ 50″ give 1827 years; therefore the time this star was in the colure, was 137 years before Christ, when Hipparchus flourished, and might very well observe it.

An Account of a large Stone voided through the Urinary Passage, by a Woman. Communicated by Dr. Richard Beard, F. R. S. Physician at Worcester. N° 397, p. 211.

A poor woman in the parish of Fladbury in Worcestershire, aged 63, about 3 years since, was afflicted with the usual symptoms of a stone in the kidneys, and afterwards in the bladder. The fits of pain occasioned by it increased with its bulk, till she was so emaciated, that her case was judged desperate. Finding relief, towards the end of last summer, by a plentiful use of mallow-tea, she persisted in it for a while; when on a sudden, in the presence of some women, she perceived an uncommon weight and force within, which assisting with all the strength and breath she had left, a stone came away with a noise that very much surprised the whole company, and with less pain and effusion of blood then, or soreness afterwards, than might have been expected. She is since easy and in health, and feels no other inconvenience now, but that unavoidable one, an incontinence of urine.

The stone is of the same colour and texture with others of this kind that have been formed in human bodies. Its weight at present, is, ℥ij. ʒj 55 gr. avoird. When first voided, it was considerably more, several pieces having been rubbed off on both sides. The greatest circumference is $7\frac{1}{2}$ inches; it is $4\frac{3}{4}$ inches round at the thickest place, and the length on the convexity is $4\frac{7}{8}$ inches.

Observations on the Comet that appeared in October 1723, *made at Bombay; and on an Eclipse of the Moon, Oct.* 21, 1724, *at Gomroon in Persia. By Mr. William Saunderson. Communicated by Dr. Halley, Astron. Reg. F. R. S.* N° 397, p. 213.

In the month of October 1723, Mr. Saunderson riding at Bombay, a brightness in the heavens appeared in a right line, or but very little to the eastward of one, with Lyra and the bright star in the Eagle, being about 50° distant from the last; and on Monday the 7th following, it had advanced 10° toward the Eagle, moving towards it in the forementioned direction, from the S. E. quarter. He took the annexed distances between 9 and 10 at night.

	October. Days.	Distance from the Eagle's Heart.		
1	7	40°	00′	South.
2	10	23	50	
3	11	20	30	
4	13	17	40	
5	15	14	40	
6	19	11	40	

At first it looked only like one of the white spots called the magellanic clouds, the space filling the field of a 6-foot glass. Afterwards he saw the head in the centre of the illuminated space, which did not look with much brightness; but appeared largest on the 10th of October, decreasing gradually both in its bulk and motion from that time, until the 25th, at which time he could find no appearance of it with the forementioned glass. From the 20th to the 25th it had nearly the same place in the heavens, seeming to move directly from the earth.

October 21, 1724, being at Gomroon in Persia, the moon entered into the dark shadow or umbra of the earth, at $5^h\ 11^m\ 33^s$, ante meridiem.

Concerning the Propagation of Misselto. By the Rev. Mr. Edmund Barrel, Rector of Sutton in Kent. N° 397, p. 215.

The berries of misselto have within their viscid pulp, a kernel covered with a thin whitish skin; the inner substance of which is deeply green, and harder than the substance of a pistachio-nut's kernel. It is flattish, and shaped sometimes like a heart, sometimes oval; both are as truly seed, as any plant can have. Those of the oval shape put out but one germen; those like a heart, have two, which prove two distinct plants.

Sir John Colebatch recommends the sowing this seed by way of inoculation: accordingly in Feb. 171$\frac{8}{9}$, Mr. Barrel endeavoured to place the berries within the bark of oak, ash, beech, pear, and apple-trees, by making several cuts and gashes in the upright sides of the trees. The whole berries would not stay in any of them; and when he broke them, the seed always slipped out to the edge of the cut, and then it stuck to the bark, by the slimy substance with which it is encompassed. Mr. B. also stuck one seed on the bare bark, without any cutting at all: this succeeded best, and being the heart-like shape, it gave him two plants. For about the 28th of March 1719, this, with two more on the apple-tree, and one on the pear-tree, began to shoot; and the growth was in this manner:

The viscous matter having stuck the seed on, and, as it dried, drawn the seed close and flat down to the bark of the tree, there began, in March and April, to spring out of that end of the seed, which had been toward the eye of the berry, a small deep green shoot or twig, very like a short piece of a little clasper of the vine. At first it arose upward from the bark, and then turning again, as it approached the tree, it swelled out somewhat thicker round about the end; yet leaving the very tip or bottom quite flat, forming as it were a foot to stand on; not unlike the bottom of some brass pestles. This foot, when it came to the bark, which was about May or June 1719, fixed itself on it. Being thus fastened at both ends, it made a little arch, whose diameter was as long as the seed, or about $\frac{1}{10}$ of an inch.

In this condition it remained all that year, till about March or April 1720, and then that part or end of the little seedling, which was joined to the bark, at the place where the seed first shot forth, let go its hold, and raising itself upward, put forth leaves, and became the head of the plant: and the other end, which sprung out first, and had taken footing in another place, became the root of the plant.

It is no uncommon thing, for seeds of evergreens to be 2 years before they spring out of the ground. And the change of the ends, first one of them shooting out, and then the other, was what surprised most at first; but on further reflection he found that nature, even in this strange plant, is uniform to her other productions; in carrying the sap first one way to form the root, and then turning the course of it back again to send out the upper parts of the plant. The strangest and most wonderful part is, that the rooting end should make its first shoot into the open air, and then turn itself down, to find a proper place to fix on. Who could have supposed that a plant, whose berry is the most orbicular of any, and therefore the least likely to lie quiet in any situation, and whose proper place of growth is a round and wavering bough, or the upright side of a tree, should after it is once fixed, leave its first footing, and seek out a new point in the bark to grow upon.

This is indeed the great secret of the matter, and seems to be the very thing that has kept the world in ignorance, about the growing of this seed. For by requiring a new smooth place of the bark, on which to fix the rooting part, it has frustrated all attempts of sowing it in the usual way of other seeds.

Theophrastus, about 2000 years since, seems to endeavour at a reason, why this seed could not grow in the earth: but all that he, or any one since, has said upon it, is only to agree, that in fact it does not, and to wonder why so perfect a seed should not grow in the earth. That ancient author rationally concluded, from its having a seed, that the plant must come from that seed: whereas latter times have been so fond of allowing chance a share in the productions of nature, that Scaliger has not only experimentally confuted the common notion of misselto's being sown in the dung of the thrush; but argues also very strenuously, against the possibility of this plant's growing from its seed. Even the great Lord Bacon, Sir Thomas Brown, Lobel, and the inquisitive Mr. Ray, so late as 1673, all give into the notion, that this plant has a spontaneous and equivocal, rather than a seminal and univocal generation.

Scaliger's strongest objection is, that misselto shoots from branches where it is not possible either for compost or seed to stand. Lobel objects against it, because of the imperfection of the berry, acinulo illo pallido pellucido. Mr. Ray's argument is, viscus innatus etiam in pronâ ramorum parte.

It is the property of true experience to clear up doubts, and answer objections: and if nature had been well examined, it would have appeared, that this seed is of a substance equal to other kernels; and that the pulp of the berry, with which the seed is surrounded, is of a more clammy sticking nature, than the pulp of other berries, for this very purpose, that it might be of strength sufficient to fix the seed on any tree, how moveable or upright soever the bough or twig should be, on which it chanced to light.

And doubtless the birds are, though not by their dung, sowers of this, as they are of many other seeds, which they carry away for food; but often drop in places where the seeds could otherwise never have come.

On going to gather some misselto-berries, Mr. B. found a leaf, which had a seed sticking on it; doubtless by a casual fall out of the bill of some bird, that had broken the berry as she was eating it. There was both a dry string of the slime, and a dry spot of the same, on the leaf, that show how the seed was detained there; and how it must be done in like manner any where else.

Mr. Barrel sowed these seeds on near 30 sorts of trees and shrubs, and yet never had above 10 plants that held out the second year; so that we need not wonder at the little success that others have had in their trials of this seed; and may also see the reason why he had not been able to make many other experiments about the growth of this plant. However, some casualties having furnished him with two or three, which he relates, because they somewhat further explain the nature of this plant's growing.

1. One of his little plants, sown in April 1724, which was fixed at both ends in its arch-like form, had in Sept. 1724, the middle part broken off; the two ends keeping still fast to the tree. Which shows how firmly the two ends adhere, while it is in that state; and they both continued green some time, and then withered away.

2. That one seed, which grew on a pear-tree, in $171\frac{8}{9}$, was the next spring $17\frac{19}{20}$, loosened from the tree at one end, as the others were: yet this seedling sprout never put out any leaves at all; but continued in the same state, neither greater nor less, near 6 years, till it was broken off by chance in July 1725. This seems to me a very strange thing: for a seedling plant of any kind is but, as it were, an embryo, till it has put forth leaves.

3. The most thriving pair of plants, of the year $171\frac{8}{9}$, being about 3 inches in length, were on May 21, 1722, struck off, by the falling of a rake-handle against them. They took away with them, only the outmost thin skin of the tree; and there were not any signs of deeper rooting. But as Mr. B. looked now and then on the place where the misselto had grown, he thought he observed the bark to swell up a little; and on the 12th of March $172\frac{2}{3}$, he perceived 3 or 4 little buds putting forth, and another bud was put out by the 18th of March. They all grew on to have leaves that summer; and now February $172\frac{6}{7}$, they are a cluster of boughs, of 4 or 5 joints in height, and bore berries this winter; whereas two others, on the same tree, and which were also sown at the same time, in $171\frac{8}{9}$, and are 6 or 7 joints in height, have not yet born any berries.

The thriving of these plants so well again, after they were broken off, made him reflect on the Druids' way of cutting misselto from the oak, with a golden instrument; a metal not apt to take a good edge, and possibly the bluntness of the instrument might be a means to preserve a future growth, of the same plant; which doubtless they, as well as we, find to be very rarely on the oak. Mr. B. might suggest some reasons for this scarcity, from the nature of that bark, and might observe many mistakes, into which both modern and ancient writers run, when they mention this plant. But he adds only this one observation; that there is almost every year, on most misselto-bushes, a visible proof that the kernel has a vegetative life in it: for when the berries hang on till May or June, the seed will make its little shoot in the berry, as the kernels of lemons, and it may be seen coming out at the eye of the berry.

An Account of a Pair of extraordinary large Horns found in Wapping. By Sir Hans Sloane, Bart. P. R. S. N° 397, p. 222.

Many years since, Mr. Doyly, a great searcher after curiosities, found a pair of extraordinary large and strangely shaped horns, in a warehouse at Wapping, where they had suffered much by worms and otherwise, being eaten pretty deep on their surfaces, in many places. They had lain there so long, that when he bought them, no one could inform him whence they came, or when or how they had been lodged there. They resembled in several things the horns of goats, which made many people think that they had belonged to an animal of that kind, in all likelihood as large as the moose-deer in America is of its kind. The Royal Society being informed of this matter, Mr. Hunt, their operator at that time, made a draught of them, on which Dr. Hook read a lecture at a meeting of the Society at Gresham College. This lecture and the draught are lost; but it is remembered that he suspected them to be the horns of the sukotyro, as the Chinese call it, or sucotario, a very large and odd-shaped beast, mentioned and figured by Nieuhoff, in his Voyages and Travels to the East Indies, where he gives the following description of it: " It is of the size of a large ox, with a snout like a hog, two long rough ears, and a thick bushy tail. The eyes are placed upright in the head, quite different from other beasts; on the side of the head next to the eyes stand two long horns, or rather teeth, not quite so thick as those of the elephant. It feeds upon herbage, and is but seldom taken."

Both are nearly straight for a considerable length, and then turning crooked, they run on tapering towards a small and pretty sharp end. They are not round, but compressed and flattish, and have large transverse sulci, or furrows, on their surfaces, waved or undulated on their under parts. They differ a little in size. Measuring one (fig. 1, pl. 4) from the great end, or basis, AB, where it was fixed to the head, along the outer circumference, he found the length ACD to be 6 feet, 6½ inches, the length by the line BD 4 feet 5⅙ inches, the diameter of the basis AB 6¾ inches, and its circumference 17 inches. This weighed 21 lb. 10 oz. and contained in the hollow part exactly 5 quarts of water. In the other (fig. 2) the length of the outer circumference ACD was 6 feet 4 inches, the line BD 4 feet 7 inches, the diameter of the basis 7 inches, and its circumference 18 inches. This weighed 21 lb. 13½ oz. and contained in the hollow part 4½ quarts; but would have held more, had it not been very much broken at the large end.

The commander of an East India merchant ship, on seeing them, told Sir Hans Sloane, that he had seen such in the Indies on a large buffalo's head. Sir Hans is inclined to think, that they must belong to a very large sort of bull's or cows, who are natives of Ethiopia, and others of the midland parts of Africa, that are mentioned by many of the ancients, perhaps not without some fabulous additions, though, which is strange, very few of the modern writers take any notice of them.*

A Demonstration of the 11th Proposition of Sir Isaac Newton's Treatise of Quadratures. By Mr. Benj. Robins. N° 397, p. 230.

This paper may be consulted with more advantage in Mr. Robins's works, collected and published by Dr. Wilson, viz. vol. 2, p. 168.

Anatomy of the Mus Alpinus or Marmot. By J. Jas. Scheuczer, of Zurich, M.D. F.R.S. N° 397, p. 237. *An Abstract from the Latin.*

When Dr. S. opened the abdomen of this animal, a very beautiful retiform omentum, abounding in fat, presented itself. This fat was more firm and compact than that which will afterwards be mentioned. It seems that the softer or fluid part had been absorbed during the dormant state of the animal in the winter, thus contributing to the secretion of the bile (with which the gall-bladder was quite turgid) as well as to the nourishment of the body.

On each side of the hypogastric region Dr. S. found a large quantity of fat, of a softer consistence than that of the omentum. It extended from the kidneys to the inguina, forming as it were another, and that a double omentum. This fat, as well as that of the mesentery, accompanies the intestines throughout their whole course, and serves both for lubricating the abdominal viscera, and for supplying the body with nourishment.

The pancreas appeared much wasted; as did also the other glands of the body, and especially those which are seated among the muscles. This appearance of the glandular parts Dr. S. attributes to a deficiency of serum or lymph in the blood of this animal.

On cutting open the duodenum, a quantity of frothy bile was discovered, which Dr. S. considers as another proof of a deficiency of serum.

The cornua uteri (for this was a female marmot) were 2 Paris inches in length; the tubes were about half an inch long, but were scarcely thicker than a thread; the ovaria were about 2 lines in length, and 1 line in breadth; they were white; but when viewed through a microscope, they appeared pellucid, containing prominent transparent ovula. The liver of the marmot is pretty

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* They seem to be the horns of the *bos arnee,* or great Indian buffalo. See General Zoology, Vol. ii. p. 400.

large, and is composed of 6 lobes, some of which are subdivided by fissures. The lowermost lobe is connected by a membrane to the right kidney. The kidneys were surrounded with fat. The renes succenturiati are small yellow bodies seated on the side of the vena cava, above the emulgents. Like the other glands they appeared wasted. The shape of the stomach is represented in fig. 3, pl. 4. The intestinal tube is of a singular structure, at the junction of the small with the large intestines. The entrance of the ileum into the colon is only 3 lines in diameter; the colon where it joins the ileum, 2 lines. But the cæcum is remarkably large, being 2 inches in diameter. The so called valve of the colon is worthy of notice; it is of an annular form, remarkably small, and altogether of a very singular structure. The entrance of the ileum is, as it were, between 2 tunicæ [vel membranæ] conniventes, which totally prevent the return of the excrement into the small intestines. The 2 membranes which, by their mutual approximation and contact, constitute the aforesaid valve, are of a rhomboidal figure. Besides these there are other annular, connivent valves which belong to the cæcum. This observation (Dr. S. thinks) throws much light on the use of the cæcum, which in new-born infants is generally very large; viz. it serves as a receptacle or lodgment-place, for the excrements for 9 months, during which time they are collected in the intestines, without being voided. It is the same with dormant animals, in the winter season; during which no excrement comes away from them; yet, notwithstanding the slow circulation and secretion which then take place, and the abstinence from food, fæcal matter is produced; and, that it may not over-distend and plug up either the small or large intestines, it is transmitted to the cæcum, where it is lodged until the next spring; its return into the colon being prevented by the beforementioned valves.

This remarkable structure of the aforesaid intestines is represented at B, and of the valves at C.

[Then follows a description of the muscles in this animal; respecting which Dr. S. remarks, that the platysma myoides is very thick and strong. It covers not only the anterior and lateral part of the neck, but likewise the whole of the masseter muscle, and extends as far as to the articulation of the humerus with the cubitus; it is inserted both into the upper and lower lip.* It contributes greatly to the flexion of the fore feet, as well as to the movement of the lips: for these animals apply their food to the mouth with their fore feet (which thus supply the place of hands) and use their fore feet also for making holes or bur-

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* Dr. S. afterwards mentions that a portion of the platysma myoides is inserted into the spine of the scapula.

rows in the ground.—The masseter muscle is also remarkably strong and tendinous.—In like manner the depressor digastricus of the lower jaw, is exceedingly strong. The thyro-arytænoid muscle, which is very conspicuous under the sphincter of the gula, is subservient, by strongly constringing the arytænoid cartilages, to the production of the peculiar noise which these animals make. There is nothing with regard to the other muscles described by Dr. S. that requires particular notice.]

Explanation of the Figures.—In fig. 3, pl. 4, AB represents the gula; CD the duodenum; E the stomach.

In fig. 4, EH represents a portion of the ileon; GH a portion of the colon; HFI the cæcum.

Fig. 5, aa represents the valve of the colon, almost the same as delineated in human subjects, only that it is nearly rhomboidal; bc the aperture of the ileum into the colon.

Fig. 6, K represents a portion of the cæcum, whose lowest part is open towards the colon, that the valvulæ conniventes ooo may be observed.*

Observations on the Lumen Boreale, or Streaming, on Oct. 8, 1726. *By the Rev. W. Derham, F. R. S.* N° 398, p. 245.

There are two sorts of streamings, which have been noticed; one, by way of explosion from the horizon; the other, by opening and shutting, without shootings up, and swift dartings. Of the latter sort chiefly was that of Oct. 8, 1726, in which, although the streams or spires, or lances, or cones, or whatever else they may be called, were as large and remarkable as in the year 1715-6; yet they exhibited themselves principally by the vaporous matter opening and shutting, as if a curtain had been drawn and withdrawn before them. It began about 8 o'clock, and soon streamed all round in the south, east and west, as much, or nearly as much as in the north. Which was a thing not observed before in these phænomena.

These streams, or cones, were mostly pointed, and of different length, so as to make the appearance of flaming spires, or pyramids; some again were truncated, and reached but half way: some had their points reaching up to the zenith, or near it, where they formed a sort of canopy, or thin cloud, sometimes red, sometimes brownish, sometimes blazing as if on fire, and sometimes emitting streams all round it. This canopy was manifestly formed by the matter

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* The subject of this dissection having been a dormant animal, it is matter of surprise that Dr. S. did not examine the organs of respiration.

carried up by the streaming on all parts of the horizon. This sometimes seemed to ascend with a force, as if impelled by the impetus of some explosive agent below, like that of March 1715, 16. This forcible ascent of the streaming matter, gave a motion to the canopy, sometimes a gyration, like that of a whirlwind; which was manifestly caused by the streams striking the outer parts of the canopy. But if it struck the canopy in the middle, all was then in confusion.

These two particulars, namely, the streaming all round, in all points of the horizon; and the canopy in and near the zenith, are what were observed in all parts of England. But in the more southerly parts of Europe, it seems to have been somewhat different, by the accounts from different places.

One thing observed in most places was, that in some part of the greatest streaming, the vapours between the spires, or lances, were of a blood-red colour; which gave those parts of the atmosphere the appearance of blazing lances, and bloody-coloured pillars. There was also a strange commotion among the streams, as if some large cloud, or other body, was moving behind them, and disturbed them. In the northerly and southerly parts the streams were perpendicular to the horizon; but in the intermediate points they seemed to decline more or less one way or other; or rather to incline towards the meridian.

As for the cause of these phænomena, Mr. Derham takes it to be from the same matter, or vapours, which produce earthquakes: and that for these reasons: First, because some of these phænomena have been followed by earthquakes. As that which Stow gives an account of in his Annals, in the year 1574, on Nov. 14; in which he says, "were seen in the air strange impressions of fire and smoke to proceed forth of a black cloud in the north towards the south. That the next night following, the heavens from all parts did seem to burn marvellous ragingly, and over our heads the flames from the horizon round about rising did meet, and there double and roll one in another, as if it had been in a clear furnace." And after this, he says followed, on the 26th of Feb. great earthquakes in the cities of York, Worcester, Gloucester, Bristol, Hereford, and in the countries about, which caused the people to run out of their houses, for fear they should have fallen on their heads. In Tewksbury, Breedon, &c. the dishes fell from the cupboards, and the books in men's studies from the shelves, &c.

So this last, in October, was preceded by that fatal earthquake at Palermo in Sicily, and succeeded by one in England, on Tuesday, October 25, following. This it seems was perceived in London, and was very considerable at Dor chester, Weymouth, Portland, Portsmouth, Purbeck, and several other places in Dorsetshire, that it caused the doors to fly open, shook down pewter off the shelves, and was felt in some ships that lay in the harbours.

Another reason is, that some gentlemen viewing this appearance, on the tops of their houses at Little Chelsea, plainly perceived a sulphureous smell in the air. Another thing which concurs with what has been said, is, that several persons heard a hissing, and in some places a crackling noise, in the time of the streaming, like what is reported to be often heard in earthquakes.

The same observed at Southwick in Northamptonshire. By George Lynn, Esq. N° 398, p. 253.

This evening appeared an aurora borealis, full as remarkable as that in March 1716, though varying in form; it began about 6 at night to be light in the north, with streaks proceeding from it, and spread gradually both towards the east and west, the south being still very clear; but before 7 it left all the northern parts, except towards the zenith, and covered all the southern. Soon after which, there appeared a white arch proceeding from east to west, passing near the zenith, but more south, which seemed fixed for a time; but about 10 minutes past 7 was dispersed, and immediately succeeded by a kind of glory of an oval form, the longer axis from east to west, rather south of the zenith, with rays shooting up from all parts, and interchanging swiftly, for about 15 or 20 degrees from it; the rest of the heavens, except the north, which still continued very clear, affording various phænomena. In the east there was a quick succession of columns of the iris colours, inclinable to white, the west to purple, and about the south-west, for a good space, appeared almost a blood red coruscation, which continued 5 or 6 minutes.

These appearances in a quarter of an hour became less remarkble; though the aurora continued most of the night, and afforded a light generally equal to the moon in its quadratures. Looking with a telescope at Jupiter, his satellites and belts appeared as plain through the aurora, as if the sky had been perfectly clear.

A Register of Observations of the Aurora Borealis for 4 Years at Lynn. By Mr. Wm. Rastrick. N° 398, p. 255. *From the Latin.*

The appearances of the aurora borealis were observed as follows, viz.

On Feb. 19, 1722, at half past 10 in the evening.
March 15, 1722, from 8 in the evening till midnight.
August 20, 1723.

Oct. 20, 1723, from 6 in the evening till midnight.
Sept. 26, 1725, from 7 till 10 at night.
Oct. 3 and 4, 1726, it continued the whole night.
Oct. 8, 1726, a very surprising one.
Oct. 26, 1726, about 10 in the evening.
March 3, 1727, a very surprising one, from 8 till midnight.
March 5, 1727, it appeared again.

Four Mock Suns, or Parhelia, seen at Kensington, March 1, 1726-7. *By Mr. George Whiston.* N° 398, p. 257.

Walking in a garden at Kensington, about a quarter after 10, Mr. Whiston observed the following appearances. First, a halo about the sun, VM, fig. 5, pl. 4, with its usual circumstances, which are pretty frequent: its upper part was very luminous, having a confused mixture of the rainbow colours in it, and being touched at the vertex with the two other curvatures, OVR, NVT, though the latter arch NVT did not appear till some time after. The bottom part of it also at M, which appeared a little above the horizon, had something of the same nature, but not in so great a degree. The two parhelia, A, B, also soon appeared, whose diameters were pretty large, and their brightness and colour pretty much as the upper part of the halo.

As the halo was at that time not quite perfect, but had some parts interrupted, Mr. W. thought that the two parhelia were in the circumference of its circle, as usual; but after about a quarter of an hour, he directly observed the halo to pass between the parhelion A, and the true sun; and he had no reason to doubt the same of the other B also, though he did not directly observe it.

The parhelia A, B, which were but a little distant from the circumference of the halo, began now to appear with narrow, pale, whitish streaks of light, in the nature of tails, proceeding from them; but soon extended themselves so far, that they met in the point opposite to the sun, and formed the great circle ABCD, parallel to the horizon, whose breadth was about half that of the halo. Viewing it carefully all round, there appeared a third mock sun C, of a plain whitish light, without any mixture of colours, which was also the case of the whole great circle: and presently also a fourth, D, both of them pretty exactly resembling each other, very much inferior to the parhelia A, B, in brightness, though not so much in magnitude.

As Mr. W. had no opportunity of measuring the several angles, he placed the mock suns, C, D, in the scheme, rather in agreement with former observations, than his own guesses; for they appeared to be at a greater distance from each other, and nearer respectively to the first two parhelia, which difference M. Huygens attributes to the different altitude of the sun.

Mr. W. thought he saw plainly at one time likewise, a small portion of a secondary halo, as in the scheme at P. It seemed evidently to be an arch of a circle concentrical with the halo, and tinged with the rainbow colours, whose diameters might perhaps be to that of the halo, as 4 to 3; but as it appeared but for a little time, he would not be positive about it.

This face of the heavens continued, though with an interruption of some parts now and then, till about a quarter after eleven, when Mr. W. left it, and could not return till about twelve, at which time the sky was clouded over, which had been before only hazy, a sure criterion of these appearances, and this phenomenon no longer visible.

A Description of some rare Crystals lately discovered. By Dr. John James Scheuchzer. N° 398, p. 260. *From the Latin.*

On the high cliffs of Grimsul there was a few years since discovered, in the middle of a rock, a vein of crystals, which is since exhausted, very pure, and the largest perhaps ever seen, the greater part of them amounting to about 60 centners. The following is a series of the chief of those crystals.

N° 1, is hexagonal, about 2½ centners, is 2 feet 9½ inches long, and 3 feet 7½ inches round.

N° 2, weighed 136 lb. is 2 feet 3 inches long, 2 feet 9 inches round, with some purple spots on the edge, but elsewhere very pure.

N° 3, of 135 lb. is 2 feet 4 inches long, 3 feet 2 inches round, and is very clear, except at the apex.

N° 4, of 96 lb. is 2 feet long, and 2 feet 9 inches round.

And so on, down to 10 lb. weight, of various qualities and proportions.

Of a Stone taken out of a Horse, at Boston in New England, in the Year 1724. *By the Hon. Paul Dudley, F. R. S.* N° 398, p. 261.

The owner of the horse never observed any thing remarkable about him till within a few days before he died, and then suspected that he might be troubled with the gravel or stone, from the great pain the horse seemed to be in when he staled or dunged, when he would groan and sweat prodigiously. After his death, in the great paunch was found a stone of 5½ lb. weight, almost as round as a globe; for it measured 17 inches round one way, and 17¾ inches the other. The grit was like a Newcastle grindstone; but it was worn smooth in the horse's stomach, the colour somewhat like that of a nutmeg, but more of the ordinary millstone. By the lightness of it, considering its bulk, it seemed to be porous within. How long this stone was generating, or what produced it, is altogether uncertain. The weight of the stone at length made a fracture in the paunch, which proved his death: for before the breach, and while the stone rolled in his stomach, he was very well.

The largest stone found in any animal that the Phil. Trans. give an account of, weighed only 4 lb. 4 oz.

Of a Polypus coughed up from the Windpipe. By Dr. Samber, of Salisbury. N° 398, p. 262.

One Tompson was taken with so violent a flux of blood, that in a short time he lost near 3 lb. of blood. After it he seemed to have something, when he coughed, that stuck in the passage, which he could not get up, and by its rattling seemed very loose. Half an hour after he coughed up something, which on being put into water, was found to be a very remarkable polypus. Dr. S. could find by the blow-pipe, that it was hollow; but its being torn off with such violence, made so many holes in it, that it could not be blown up. It is probable that it lined the bronchia, and that the air had a passage through it, and that a violent fit of coughing had separated the adhesion, and brought on that violent flux of blood, &c. The patient had been tormented with a cough for more than 6 months, and was a gouty man; but after this was coughed up, and so large an ulcer made, he had all the successive symptoms of a fatal consumption; as cough, spitting, hectic, colliquative sweats, diarrhœa, and about a month after he died, aged near 50.

An Account of a Book entitled Vegetable Statics: or an Account of some Statical Experiments on the Sap in Vegetables; being an Essay towards a Natural History of Vegetation. Also, a Specimen of an Attempt to Analyse the Air, by a great Variety of Chymico-Statical Experiments; which were read at several Meetings before the Royal Society, &c. By Stephen Hales, B. D. F. R. S. By the Rev. John Theoph. Desaguliers, LL. D. F. R. S.* N° 398, p. 264.

As the ancients used to say, that geometry and arithmetic are the wings of a mathematician; so a mechanical hand, and a mathematical head are the

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* Dr. Stephen Hales was one of the brightest ornaments of the R. S. during the 18th century. He was born in 1677, and studied at Cambridge. Although divinity was his chief object, yet he showed a marked fondness for natural history and experimental philosophy; and while at the university, we are told that he contrived a machine for representing the motions of the heavenly

necessary qualifications of an experimental philosopher. The first alone may enable a man to make a great many experiments, but not to judge of them:

bodies, similar to that afterwards constructed by Rowley, a mathematical instrument maker (but who is said to have borrowed this invention from Mr. George Graham *). This machine as improved upon by Rowley, received the name of orrery. It will afterwards be seen that Dr. H. exercised his mechanical talents in ways that were conducive to the health and benefit of the human race.

His first publication was the work abovementioned, viz. his Vegetable Statics, 8vo. 1727. It was not until 6 years afterwards that the 2d vol. appeared, under the title of Statical Essays. It will not be wondered by those who consider the vast variety of original experiments relative to animal and vegetable physiology contained in this work, and the important deductions resulting from them, that it should have raised its author to a place in the foremost rank of philosophical inquirers. The subject which he here proposed to himself was one that was every way worthy of his enlightened mind; it was nothing less than to investigate the functions of living organized matter: and never was more ingenuity displayed in planning and contriving experiments, nor greater ardour and perseverance in executing them. This work has been translated into various European languages: into French by two very celebrated writers, viz. the 1st vol. by the Count de Buffon, and the 2d by Boissier de Sauvages.

In 1739 Dr. H. published a miscellaneous vol. under the title of Philosophical Experiments; and in 1758 a treatise on Ventilators, of which he had some years before communicated an account to the R. S. Fresh air being conveyed by means of these ventilators into ships, hospitals, prisons and mines, the health and lives of numbers of persons in such situations were thereby preserved. Before they were fixed in the Savoy-prison from 50 to 100 persons confined there had been known to die of the jail fever, within the space of a year; whereas during 3 years afterwards not more than 4 persons had died, and of these 4, one was destroyed by the small-pox: yet the number of prisoners in one of those years amounted to 240. In like manner the proportion of deaths annually in Newgate was surprisingly diminished after the introduction of his ventilators there. He afterwards extended their application to the preservation of grain. And further he made his mechanical talents subservient to economical purposes by inventing a simple apparatus, which he called a *back-heaver,* for winnowing corn. Of this he published a description in the Gentleman's Magazine, for 1745 and 1747.

All this time Dr. H. continued to send occasional communications to the R. S. These are inserted in the Phil. Trans. from the 35th to the 49th volume inclusive. Some of them relate to medical subjects.

The theological writings of Dr. H. are but few; they consist of a sermon or two, preached on particular occasions. Yet in his clerical character he was actively watchful over the religion and morals of his parishioners; and perceiving the growing habit of dram-drinking among the lower orders of the people, and the complicated evils thence arising, he used his utmost endeavours to dissuade them from it, in a pamphlet entitled an "Admonition to the Drinkers of Brandy, &c." which was printed more than once. He might have had higher church preferment than the livings of Teddington and Farringdon; but, so fond was he of retirement, and so much devoted to philosophical pursuits, that he declined every offer of this kind that was made to him. The career of his long and useful life was closed in 1761, when he had attained his 84th year.

Had Dr. H.'s labours been limited to his Statical Essays, he would still have been regarded as a first-rate experimentalist. But by his other works he has added to the praise of ingenuity, the praise

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* See his Life, Vol. vi. p. 537 of these Abridgments.

for without being able to observe, compare, and calculate the exact quantity of weight, force, velocity, motion, or any other change to be observed in making experiments, effects may be attributed to causes which are not adequate to them, and sometimes expected to be produced even without a cause; as appears by the cost and trouble of those who have attempted to find the perpetual motion. Though such persons may make some discoveries, their philosophy will be at best but conjectural, and their conclusions only guess-work. The mere mathematician, on the other hand, wants postulata in physics; or taking things for granted on the report of others, comes often to wrong conclusions, though he reasons justly; because his premises are false. Men of warm imaginations, who wanted mechanics or mathematics, or would not apply them to physics, have pestered the learned world with philosophical romances, such as the Cartesian system, contrived for the diversion of the lazy and talkative.

But the incomparable Sir Isaac Newton has not only shortened the geometrician's work, by his wonderful discoveries in abstract mathematics; but has also taught us, by his own practice, how to make and judge of experiments and observations, with the utmost accuracy: and as he avoided making hypotheses, he was so cautious as to deliver only by way of queries, several truths which he was convinced of; because he wanted a sufficient number of experiments to make them as evident as those others, by which he has so far improved and advanced natural knowledge. Our author has followed his steps, asserting nothing but what is evidently deduced from those experiments, which he has carefully made, and faithfully related; giving an exact account of the weights, measures, powers and velocities, and other circumstances of the things he observed; with so plain a description of his apparatus, and manner of making every experiment and observation, that as his consequences are justly and easily drawn, so his premises or facts may be judged of by any one that will be at the pains to make the experiments, which are most of them very easy and simple.

His account of every thing is written in such an intelligible manner, that the inquisitive reader is capable of understanding it, without being puzzled with perplexed calculations and complex experiments; which authors have sometimes contrived, in order to be admired for those things, which they themselves found out either by mere chance, or with very little labour. He has illustrated, and put past all doubt, several truths mentioned in Sir Isaac Newton's queries; which though believed by some of our eminent philosophers, were called in

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of utility also. He seems indeed to have been less desirous of exciting admiration for the talents which he possessed, than of meriting gratitude for the manner in which he employed them.

question by others of an inferior class, who were not acquainted with those facts and experiments on which Sir Isaac had built those queries.

Then follows an abstract of the topics discussed in each chapter of the abovementioned work; which abstract it is unnecessary to reprint, the work itself being in the library of every person who possesses the least taste for physiological inquiries.*

END OF VOLUME THIRTY-FOURTH OF THE ORIGINAL.

A Catalogue of the Fifty Plants from Chelsea-Garden, presented to the Royal Society, by the Company of Apothecaries, for the Year 1726; *pursuant to the Direction of Sir Hans Sloane, Bart. By Mr. Isaac Rand, F. R. S.* N° 399, p. 293. *Vol. XXXV.*

Some further Remarks on P. Souciet's Dissertations against Sir Isaac Newton's Chronology.† *By Edmund Halley, LL. D.* N° 399, p. 296.

When I gave my remarks on P. Souciet's Dissertations against Sir Isaac Newton's Chronology, I was obliged to take what he was pleased to give us out of Hipparchus's comment on Aratus, not having then that author himself by me. Since then, having procured the Florence edition of Hipparchus, anno 1567, I find an argument very much ad hominem, which the R. P. must confess will bring the Argonautic expedition full as low as Sir Isaac Newton makes it.

P. Souciet, in his 5th Dissertation, p. 119, 120, finds out a star of the first magnitude, closely adjoining to that we now call the first star of Aries, as it is in the catalogue of Ptolemy, where it is said to be in the horn of Aries, and not in the ear. This star the R. P. supposes long since to have disappeared; but that being of old very considerable, it was from this first star of Aries, the zodiac began, though for argument-sake he is contented to let it begin as Sir Isaac does, with the aforesaid star in the ear or horn; which Hipparchus, in

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* We shall only remark, that although the work is entitled Vegetable Statics, yet in the 6th chap. the author gives an account of a vast variety of experiments relative to the disengagement of air (by distillation, fermentation, chemical solution, &c.) from animal and mineral, as well as vegetable bodies. Also, of the absorption of air in the burning of sulphur, and phosphorus, and in the process of respiration; so that this chap. contains the rudiments or germs of some of the most brilliant discoveries (particularly of those which relate to the constitution of the atmosphere and to gaseous chemistry) which subsequent philosophers have made.

† See the former part of these remarks at p. 170 of this volume.

the last and 54th page, tells us, in his time followed the equinoctial colure the 20th part of an hour: and supposing the star that has disappeared, to have been at that time precisely on the colure, it must differ but 45 minutes of right ascension from it: but how he comes to make the difference of longitude 40 minutes, no way appears, and is gratis dictum.

In page 49 of the said Florence edition, Hipparchus, treating of the rising and setting of the constellations, tells us, that that of Aries began to rise with 18½ degree of Pisces in the ecliptic, and was wholly risen with the 24th degree of Aries, while the zodiac passed the meridian from 23½° of Sagittary to 14° of Capricorn: and again, page 52, he says, that the constellation of Aries began to set with the 29th degree of Pisces, and was wholly set with the 26th degree of Aries, while the zodiac passed the meridian from 29° of Gemini to 29° of Cancer. He tell us also that it was the very same star that both rose and set first in that constellation, calling it ὁ ἐπὶ τȣ̃ ἐμπροσθίȣ ποδός, page 49: and again, page 52, it is named ὁ ἐν τοῖς ἐμπροσθίοις ποσίν, or that in the fore-feet of the Ram.

This certainly is the star P. Souciet would place on the equinoctial colure, and makes it long since to have disappeared; without inquiring whether the aforesaid data were not abundantly sufficient to determine its place in the zodiac at that time; and without regard to the odd uncouth posture he must suppose the constellation of Aries to be in, when he makes one or both of the fore-feet so near to, and above the horn or ear.

Hipparchus expressly says, that it rose when 23½° of Sagittary was on the meridian, and set when 29° of Gemini passed it; and taking the middle between those points, it is plain, that it culminated with about 26° of Pisces, and that it had north declination; the excess above 180° showing that the ascensional difference was about 2¾°. But to give the argument its full scope, the right ascension of 23½ of Sagittary (allowing Hipparchus's obliquity 23° 51′ 20″) will be found 262° 54′. And that of 29° of Gemini will be 88° 54′: so that this star was above the horizon (in the latitude of 36 degrees north, to which Hipparchus has adapted his calculation) 12^h 24^m, or 186 degrees; whence the right ascension of the star is justly concluded 355° 54′, and its ascensional difference precisely 3°; which in that latitude makes its declination 4° 7′ north. We have therefore gotten both the right ascension and declination of this supposed first star of Aries.

Let us now see what longitude and latitude results from the aforesaid right ascension, with 4° 7′ north declination, assuming the obliquity with Hipparchus, to have been 23° 51′ 20″; and we shall by a just computation, find the star at that time to have been in 27° 53′ of Pisces, 5° 24′ north latitude, which therefore was reckoned the place of the star at that time by Hipparchus. Add 2° 40′, for 265 years between Hipparchus and Ptolemy, and we shall have its place, in Ptolemy's account, Aries 0° 33′, with 5° 24′ north latitude. But the 22d star of Pisces, in Ptolemy's catalogue, has the same longitude and latitude, with sufficient exactness, viz. Aries 0° 40′, with north latitude 5° 20′, and is Media trium in Lino Boreo Piscium (η Bayero.) Hence it cannot be doubted but that this star, which P. Souciet takes to have been once a star of the first magnitude, was no other than the said 22d of Pisces, which in the British catalogue, fitted to the year 1690, is set down in Aries 22° $29\frac{1}{3}$′, with north latitude 5° 21′.

How Hipparchus came to reckon this star to be in the fore-foot of Aries, does not at present appear; but it is not unlikely that these commentaries of his upon Aratus, were written some time before he set about making his catalogue of the fixed stars; when he might change his opinion, and replace it in the line of the fishes, to which it seems more properly to belong.

Be that as it may, we will for once, suppose with P. Souciet, this star to have been in the beginning of the zodiac, or of the constellation of Aries, and that at the time of the first fixing the colures, that of the vernal equinox passed 15° in consequence of it. Now anno ineunte 1690, this star being in Aries 22° $29\frac{1}{3}$′, if we add 15°, we shall have Taurus 7° $29\frac{1}{3}$′ for the point in the ecliptic that was then the beginning of the zodiac. Now $37\frac{1}{2}$°, at 50″ per annum, gives 2700 years; from which deducting 1690, we shall have 1010 years before Christ. But this star having 5° 21′ north latitude, the colure, when it passed over it, intersected the ecliptic in 2° 20′ less longitude; which gives the time 168 years later, or but 842 years before Christ. So that malgrè cette grande decouvèrte, the new system of chronology is so far from being refuted, that it seems to be hence very much confirmed, at least in the opinion of the reverent Pere.

I have assumed the latitude to which Hipparchus might have adapted his calculations, to be 36°; because I find in page 14 of the aforesaid edition, that he makes the longest tropical day $14^{h}\ 30^{m}$. And in page 29, he tells us, that the southern star in the left foot of Bootes (υ Bayero) having 27° 20′ north declination, was above the horizon, $14^{h}\ 57^{m}$; whence it follows, that the latitude must be 36° 5′. He also tells us, in the same page 29, that this star set when 22° of Capricorn culminated, and 6° of Taurus ascended; repeating the same thing in page 39, which leaves no room to suspect that those numbers are not the same that Hipparchus had computed. I therefore thought it worth while to inquire in what latitude 6° of Taurus rises when 22° of Capricorn is on the meridian; and with the obliquity of the ecliptic, as now we have it, the latitude resulting is 35½° north; but with the obliquity allowed by Hipparchus, it will be found less than 35°.

This I say, only to obviate any objection that may be made by P. Souciet to the foregoing argument; though if he please to examine it, he will find that an error of a degree in the assumed latitude, will by no means invalidate the proof here given, that this first star of Aries could be no other than the middle star in Lino Boreo Piscium, marked η by Bayer.

An Account of the Lumen Boreale, as seen at several times. By the Rev. Dr. Langwith, of Petworth. N° 399, p. 301.

Jan. 4, 1727, appeared a luminous arch, which extended from N. E. to west. The streams all moved westward.

Jan. 5, exhibited something of the same nature, but hardly enough for observation, and yet this very night the appearances were more remarkable in some parts of the kingdom than those of Oct. 8.

March 2, between 7 and 8, there was an arch on a black basis as before, extending from N. E. to W. Its height variable, pyramidal streams of greenish light moving westward.

March 3, the appearances this night were so extraordinary, that they would require a long description; but I shall chiefly take notice of such particulars as differed from those of Oct. 8.

1. That instead of one luminous arch in the north, here were two, and sometimes three, one above another. They were distinct enough from each other in their upper parts, but blended together towards the horizon. 2. Several of the more permanent streams were bent, at times, into irregular arches of different curvatures and positions. 3. The flashing streams from the east sometimes met with those from the west, and so formed continued arches, of a pale colour, which quickly broke and vanished. No colouring followed on the mixture of these streams. 4. The streams of this kind moved mostly southward, but not to any certain point; for they were inclined to the horizon at all degrees between 5, or less, and 90. There was sometimes such a strange irregularity in their motions as can hardly be described; for the places from whence the flashings were directed seemed to vary every moment.

N. B. Aristotle has given an imperfect account of some of these meteors.

The Description of an Aurora Borealis seen at Liverpool. N° 399, p. 304.

This began about 7 o'clock at night, Jan. 5, 1727. From the northern parts arose several streams of light, as if from behind a black cloud. They were innumerable, and shot upwards to the zenith with a motion not to be followed by the eye. They had also another motion, which seemed to be sideways, their higher ends terminating sometimes in a sharp point, sometimes in two or three points, they appeared from N. W. to N. E. but were brightest in the north. Their colour was pale, like that of Jupiter through a telescope, but not so bright. Most of them reached the zenith, where mixing with each other, they whisked round, and formed an appearance like the curling flame of a glass-house fire; they had a very irregular motion, sometimes turning inwards, sometimes outwards, like the pendulum spring of a watch. This circular light was the brightest, and seemed to occupy near 10° of the highest part of the hemisphere; several strokes of light seemed to dart from it to the south; but died before they got any considerable distance. In the west were two small long clouds, which interposed before the light streams, which appeared above the clouds, and between them, which convinced the observer that this light is far above them. Fig. 6, pl. 4, is a scheme of the whole horizon, as it then appeared. That bright star is Jupiter, whose place then was 17° in Aries, and was about south-west, I guess about 20° high. Some of the brightest stars in Taurus, Orion, and Aries, appeared south and south-east; but they are only placed by guess. After 10 o'clock, the whirling light in the zenith appeared of several colours, as, blue, green, yellow, and reddish.

Concerning a Shock of an Earthquake, felt near Dartford in Kent, in August 1727. *By the Rev. Edmund Barrel, Rector of Sutton.* N° 399, p. 305.

This earthquake was felt very sensibly at a farm on a hill, called Skeat Hill, which is at the west end of Lullingstone park, about 8 miles south-west from Dartford; and the same morning a piece of ground, in a meadow in Farningham, about 5 miles south of Dartford, fell in, so as to leave a pit about 8 or 10 feet over, and near as deep; and being on the same level with the river, it was, when seen that morning, filled with water, within 3 or 4 feet of the top; though that spot of ground was supposed to have been as sound as any about it, carts having often gone over that very place.

On a Subterraneous Fire, observed in the County of Kent. Communicated by Robert Nesbitt, M. D. N° 399, p. 307.

This subterraneous fire was first noticed August 2, 1726, in a marshy field, in the parish of Flinx Hill, about 10 or 12 miles south-west of Canterbury. It began on the side of a little brook near the water, and continued to burn along its bank, without spreading much for some days. Afterwards it appeared on the other side, and extended about 3 acres over the field, consuming all the earth, where it burnt, into red ashes, quite down to the springs, which in most places lay 4 feet or more deep.

It still burnt Sept. 4, when Dr. N. saw it, in many places, and sent forth a great smoke and strong smell, very like that of a brick-kiln. It never flamed but when the earth was turned and stirred. For some space round where it was burning, the ground felt hot, though the grass seemed no more parched than might reasonably be expected from the dryness and heat of the season. The Doctor caused it to be turned up in several places, and found the earth hot and wet near 4 feet deep, and much hotter about 2 feet than near the surface.

When this earth was exposed to the air, though it was very moist, and not hotter than might easily be borne by the hand, the heat increased so fast, that in a few minutes it was all over on fire, like phosphorus made with alum and flower. The soil of the field is of the same nature with that they make the turf of in Holland; its surface is always wet, except in extreme dry seasons. This year it was somewhat more parched and harder than usual. From what has been related, it seems not more difficult to account for this fire than for those fires which often happen in hay-ricks, when the hay is stacked before it is thoroughly made.

Experiments on the Effects of the Poison of the Rattle Snake. By Capt. Hall. N° 399, p. 309.

In South Carolina, May 10, 1720, having got a fine healthful rattle-snake about 4 feet long, Capt. Hall persuaded several gentlemen, besides Mr. Kidwell, a surgeon, to assist in making some experiments on the effects of its poison.

They got 3 cur dogs, the greatest not larger than a common harrier, and the least about the size of the largest lap dog, all of them smooth haired. The snake being tied and pinned down to a grass-plat, they took the largest of the dogs, and having tied a cord round his neck, so that it should not strangle him, another person held one end while Capt. Hall held the other, the length was not more than 4 yards each way from the dog. Immediately on bringing the dog over the snake, the latter raised himself near 2 feet, and bit the dog as he was jumping; the dog yelped, and Capt. Hall pulled him to him as fast as he could, and perceived his eyes fixed, his tongue between his teeth, which were closed, his lips so drawn up as to leave his teeth and gums bare; in short, he was quite dead in a quarter of a minute, and some were of opinion it was in half that time. They could not see where the dog was bitten, nor any blood; on which they ordered some hot water to scald the hair off; when they could find only one puncture, which looked of a bluish green a little round it; it was just between his fore leg and his breast, where, when the legs are distended, the hair is much thinner than in some other places.

Half an hour after the first bite, they took a second dog, which was somewhat less, and in like manner brought him over the snake, which in a very little time bit his ear, so that they all saw it; he yelped very much, and soon showed the signs of being very sick, holding that ear that was bitten uppermost. He reeled and staggered about for some time; then fell down, and struggled as if convulsed, and for two or three times got up, each time wagging his tail, though slowly, and attempting to follow a negro boy, who used to make much of him. We put him into a closet, and ordered the boy to look after him.

About an hour after the second was bitten, they took the third dog in like manner; the snake bit him on the right side of the belly, about 2 inches behind the long ribs; for they saw he had drawn blood there. The dog at first, for about a minute, seemed not to be hurt, so they let him go, being one they could get again when they pleased. For that day they put up the snake, imagining his poison was very near, if not quite expended.

In a little time after, which was just 2 hours after the second dog was bitten, the boy announced he was dead. About an hour after, this dog was opened, when nothing uncommon appeared about the heart. Mr. Kidwell laid open the scull, and was of opinion, that the brain was more red and swoln than any he had ever seen; and a little while after, he said the blood turned very black.

For that day they heard no more of the third dog which was bitten; but the next morning the woman who owned him came to complain of our cruelty for killing her dog. She did not know when he died, but said she saw him at 7 o'clock that evening, which was about 3 hours after he was bitten; and that he was so sick he could scarcely wag his tail. None of these dogs were swoln before they died.

On the Saturday following, which was the 14th, they got two dogs, both as large as common bull dogs. The first dog, which he bit on the inside of his left thigh, died in half a minute exactly, in the opinion of two gentlemen, who kept their watches in their hands all the while; there were two very small punctures in his thigh, which looked livid, though no blood was drawn. This dog did not swell for 4 hours after he was dead.

The second dog was bitten about an hour after the first, on the outside of his thigh, where we perceived the blood at two places; he soon sickened, and died in 4 minutes.

Thinking the snake's poison was not spent, they got a cat, which he bit about an hour after. The cat was very sick, and they put her up in a closet; by some means the cat was let out in less than an hour and a half after she was bitten.

The next morning early she was found dead in the garden, and much swoln; so that nobody cared to examine or search where she was bitten.

About a quarter of an hour after he had bitten the cat, he bit a hen twice; the hen seemed very sick and drooping, and could not, or did not fly up to her usual place of roost among the rest that night; but the next day she seemed very well, and continued so till evening, when being killed, and her feathers scalded off, there were two punctures in her thigh, and a scratch on her breast over the craw, all which looked livid.

About a week after, having got a large bull frog, they brought that over him as usual; he bit it with much force, so that he seemed to fasten for a small space. The frog died in about 2 minutes. In less than a quarter of an hour he bit a chicken, which was hatched the February before, that died in 3 minutes.

About the middle of June, Capt. Hall took him out according to custom, and having got a common black snake, not of the viper kind, about 2½ or near 3 feet long, in good health, just taken, he put them both together, and irritated them both, that they bit each other, and he perceived the black snake had drawn blood of the rattle snake before he took them asunder. In less than 8 minutes the black snake was dead, and Capt. Hall could not perceive the rattle snake at all the worse or sick.

On the last day of June, Capt. H. took the snake out to try whether, if he bit himself, it would not prove mortal to him. Capt. H. hanged him so, that he was not above half his length on the ground; and with two needles at the end of a stick, one to prick, the other to scrach, irritated him so much that he soon bit himself, after having attempted to bite the stick many times. Capt. H. then let him down, and he was quite dead in 8 or 10 minutes.

The snake being cut in 5 pieces, and given to a hog, the head part first, the hog eat up all the snake, and 10 or 12 days afterwards the same hog was alive and in health. And this was no more than Capt. H. had seen before. Indeed he has heard 50 accounts of the same kind, and was told that those hogs which feed in the marshes will run after the common sort of water snakes, which are not poisonous, and will feed on them greedily; and in Maryland in the month of August, he saw a hog eat up the head of a rattle snake just cut off, and while it was gasping very dreadfully; and he was told it was a common thing, and it would do them no harm.

On the 10th of June 1723, Mr. Thomas Cooper, a physician, sent to inform Capt. Hall, he had got a fine rattle-snake, which had been taken not above 4 days, and was about 3½ feet long, and that he designed to try whether he could save some of the dogs after the snake should bite them. He provided a large quantity of Venice treacle, or mithridate, he is not certain which, that he divided into two potions, each about 2 ounces; to one of them he put a large quantity of diaphoretic antimony.

The first dog the snake bit on the inside of the thigh, and died so soon, viz. in about half a minute, that we could not get the potion, which was that without antimony, down his throat soon enough to expect it could have effect.

About an hour after, the second dog was bitten, and had 2 punctures or holes in the fleshy part of the inside of his fore left leg, which bled more than any before; they immediately got down his throat that preparation with antimony. He soon grew very sick, and strove to vomit, but brought up very little, if any; he frothed at the mouth and bit at the grass, which he champed, as if he were mad; and indeed every one was afraid of him. He was therefore put into a room and there kept till next morning, where he seemed to be recovered; they threw him some meat, which he eat, so he was let out and he went home. About a month after that the dog's hair came off, and his master killed him.

The third dog which he bit was a shaggy spaniel, about 1¼ hour after the second. He was bitten on the fore part of his right shoulder. The dog seemed to bite at the place himself, and was very sick for that whole evening, viz. about 2 or 3 hours; but without any means or application he recovered.

Of some remarkable Appearances in the Brains of three Persons who died of Epilepsy, with an Account of the Substance of the Cataract; in a Letter from Mr. Walther Curieusus Rhœtus, to Mr. I. F. Woolhouse, F. R. S.* N° 399, p. 315. *An Abstract from the Latin.*

1. A man aged 35, of a spare habit of body, who had previously been subject to a pain in the forehead and to bleeding from the nose, on the cessation of which he lost the sense of smelling, was afterwards seized with epileptic fits; of which he had frequent attacks for the space of 2 years, when he at length died. On opening the head, the anterior part of the brain, on the right side about the region of the crista galli, was found hard and callous, and strongly adhering to the dura mater; in the anterior part of the brain on the left side, there was found some extravasated blood. In other respects the appearances of the brain were natural.

2. A woman aged 60, who at the same time laboured under anasarca, died after repeated and very violent attacks of epilepsy. On opening the head, a

* Rhœtus, a Grison?

large quantity of extravasated lymph was found between the pia mater and brain, and the anterior ventricles of the brain were almost turgid with the same fluid. In the choroid plexuses were found a number of whitish pellucid eminences, or vesicles, filled with serum, and of different sizes, the largest of the size of a hempseed.

3. In a third epileptic subject, an old woman, there was found some effusion of serum between the pia mater and the brain; and the anterior ventricles of the brain were so much distended with the same fluid, that they seemed ready to burst. Both the choroid plexuses exactly resembled a bunch of grapes. They were studded with numerous round, pellucid vesicles of different sizes, looking like the most beautiful pearls.

Then follows the account of a cataract in the left eye of a woman about 50 years old. During life, the cataract was of a pearly appearance, and of a middling size, occupying the middle part and rather more than the half of the pupil. Previous to the examination of the diseased eye after the woman's decease, Mr. W.'s correspondent was asked by Professor Santorini of Venice, where the dissection took place in the presence of about ten other physicians, what his opinion was respecting a cataract? whether it was owing to the formation of a film (cuticula) or to a fault in the crystalline humour? Having replied that he conceived it to be occasioned by a film or membrane, the Professor, who espoused Heister's doctrine* on this subject, smiled. On opening the eye, it is stated that the crystalline humour was found pellucid, and without spot or obfuscation. It was, however, very slightly and uniformly tinged of a citron colour throughout; but this citron tint was so very slight that it was scarcely perceivable while the woman was living. There was found in the second chamber of the eye a film (cuticula) which was only connected to the uvea by two very slender fibrillæ, being loose every where else. No other morbid appearances were observed; though the pupil was rather larger than common.

Two Surgical Questions, stated and answered by John Douglas, Surgeon, F. R. S. N° 399, p. 318.

Quest. I. Whether it be not possible in some measure to relieve those persons, who by reason of their great age, bad habit of body, &c. cannot submit to any of the great operations for the stone with tolerable hopes of success, by making an artificial fistula in the perinæum?

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* Namely, that the disease of the eye, termed cataract, consists in an opacity of the crystalline humour.

Answ. That many patients offer, both young and old, who are afflicted with the stone in the bladder, whom we cannot, with much hope of success, advise to submit to the great operations for the stone, daily experience shows. It is inhuman to cut them, and it shows a very great imperfection in our art, to say we can give them no relief. Is there no medium yet found out between living in extreme misery, and submitting to a desperate operation? Yes, Thomas Fienús, about 125 years ago, proposed a palliative cure for such patients, where a radicative cure could not be expected; an operation which may be performed with safety on the oldest, the wound being so small, and the parts cut of so little consequence to life; an operation by which we can prevent or alleviate the most lamentable effects of the stone; yet it has been as little attended to as yet by the Hospital Lithotomists as Rosset's most excellent treatise, before Mr. Douglas introduced the hypogastric section in 1719. Though he admires Fienús's design in making this fistula, yet he cannot by any means approve of his way of doing it.

It should be performed thus: place the patient as in Marianus's operation; pass a staff into the bladder, then cut the skin and fat till you lay that part of the urethra bare, which reaches from the prostate gland to the cavernous urethra; then make a small incision into it with the point of the knife; then withdraw the staff, and pass a small flexible canula into the wound of the urethra; then dress the wound S. A. extract the canula, clean it and introduce it again every dressing, that so you may leave a fistula in the room of the wound.

Through this fistula the patient himself, or any one about him, may easily pass an oiled probe, and push the stone back whenever he finds himself attacked with a suppression of urine, or when the stone presses hard against the sphincter on endeavouring to make water, which otherwise could not be done without the ceremony of sending for, and staying in misery till a surgeon comes to pass the catheter, which in such cases is not always to be done, without a great deal of pain, and sometimes danger. By this fistula we can also very easily inject any liquor, that may be thought proper either to prevent or allay the inflammation of the bladder, or cleanse it from the gravel, or any other sort of filth that may collect there, by which the increase of the stone will be prevented, &c.

In females all those advantages are obtained by the natural straightness and shortness of the urethra, whence they never suffer the tenth part that males do, which is an incontestable evidence that when the passage into the bladders of males is made as straight and near as short, as is done by the forementioned fistula, they will reap the same advantages by it.

Therefore artificial fistulas in the perinæum ought to be made for those who, by reason of their great age, and bad habit of body, &c. cannot undergo any of the great operations for the stone, with the hopes of success.

Quest. II. Whether it be not possible to dilate the artificial fistula in the perinæum of males, and the urethra of females, with sponge or gentian tents, gradually increased for some time to such a width, that we may easily pass a pair of forceps into the bladder, with which the stone when small may be extracted, and when large, or of an irregular figure, broken, and the pieces extracted gradually and at different times, when they cannot be extracted at once, without fatiguing the patient too much?

Answ. To prove that both these fistulas may be dilated to a sufficient size by the means proposed, especially if the parts are frequently bathed in a semicupium or otherwise, as the operator shall think proper, and some warm oil injected into the fistula every time the tent is changed, the better to supple and relax the parts, Mr. D. makes use of three arguments, viz. one from common experience in analogous cases; another from the operations of nature on the same parts; and the third from instances of this operation being performed after the method proposed. 1st. Common experience shows to what a great width fistulas in all other parts of the body, though very small at first, may be dilated by sponge or gentian tents.

2d. Nature herself, without any art, has frequently performed this operation on both sexes; in males who have been cut for the stone after the old way, and had fistulas remained in perinæo. It is often found that some considerable time afterwards, stones of no small size have appeared, which had made their way through the sphincter of the bladder into the membranous urethra, and stuck near the orifice of the fistula, whence they were easily and safely extracted. Mr. D. saw a stone as large as a pullet's egg, that was expelled from the bladder of a young woman without any help, and she had no inconveniency afterwards; which certainly would have happened, had it been extracted after the common violent method. In the last Phil. Trans. Dr. Beard, of Worcester, gives an account of a yet larger stone, that passed after the same manner, but the patient had the common inconveniency afterwards, viz. an incontinency of urine, which was owing more to the roughness than the size of the stone, which had lacerated the parts as in the common operation, which might have been prevented had she been assisted in time as above.

3d. Mons. Collet, in his Traité de la Taille, gives an account of a gentleman on whom he performed this operation three different times, and extracted in all ten stones. And Mr. D. knew a gentleman keep a fistula in perinæo open for the very same reasons.

Since then it is evident that fistulas in all parts of the body are dilatable to a great width; since nature is often able of herself to dilate the very parts in dispute to a very extraordinary degree; and since this very operation here proposed has been successfully performed three times on the same person: therefore artificial fistulas in males, and the urethra of females may be dilated, so as to extract any stone without cutting the body of the bladder, or lacerating any of the parts.

The Conclusion of Dr. Desaguliers' Account of Mr. Hales's Vegetable Staticks. N° 399, p. 323.

See note relative to the former part of this account at p. 191, of this vol. of these abridgments.

A Solar Eclipse, observed at Lisbon, Sept. 25, 1726, *N. S. By Father Carbone, a Jesuit.* N° 400, p. 335. *From the Latin.*

At 3^h 59^m 50^s apparent time, the moon's limb touched that of the sun.
4 38 57 just 6 digits were eclipsed.
4 58 30 the greatest obscuration, 7 dig. 45′.
5 56 50 end of the eclipse.

A Lunar Eclipse observed at Lisbon, Oct. 10, 1726, *N. S. By the same.* N° 400, p. 338. *From the Latin.*

At 14^h 37^m 0^s apparent time, a sensible penumbra.
14 57 20 the earth's shadow touched the moon's limb.
16 3 4 6 digits were eclipsed.
16 15 25 greatest obscuration, 6 dig. 10′.
17 33 30 end of the true eclipse.
17 54 0 end of the penumbra.

Remarks on some Experiments in Hydraulics, which seem to prove that the Forces of equal moving Bodies are as the Squares of their Velocities. By Mr. John Eames, F. R. S. N° 400, p. 343.

The result of these experiments is, that the velocities of any fluid, for instance water, issuing out at equal orifices made in the sides of tubes, or vessels filled up to different heights, and kept full at those heights, above the orifices, are found to be as the square roots of those heights respectively. Thus, when the different heights above the orifices are as the numbers 1, 4, 9, 16, &c. the velocities of the particles of water, issuing out, are found to be as the numbers 1, 2, 3, 4, &c.

The argument drawn from these experiments, in favour of the opinion, that the forces of equal masses, or moving bodies, are proportional to the squares of their velocities, runs thus: all the particles of water, being uniform, and of the same nature, every single particle issuing out with 2 degrees of velocity, must move with 4 times the force of any other single particle, that moves but with one degree of velocity; because the force with which it moves, is the effect of a cause 4 times greater; namely, the pressure of a column of water, whose height is 4 times greater.

Thus again, a particle of water running out, with 3 degrees of velocity, must move with 9 times the force of a particle moving with but 1 degree of velocity; because that force is the effect of a cause 9 times greater, viz. the pressure of a column 9 times higher; since no less than a column 9 times higher is found, by experience, necessary to make the several particles of water issue out with 3 degrees of velocity. So that, in these two instances, it seems to be certain, that the forces communicated, are as the squares of the velocities. And that it is so universally, is argued thus: the pressures are as the altitudes, and the altitudes as the squares of the velocities of every single particle; therefore the pressures are as the squares of the velocities; but the pressures are the causes of the forces with which the several particles of water issue out, or move; and therefore, since effects are proportional to their causes, the forces with which the several particles issue out, and move, are as the squares of the velocities.

Remark I. The fault in this reasoning, and which quite runs through it, is the mistaking a part of the effect for the whole. The entire effect of any of these pressures is, not barely a certain number of degrees of velocity, in any single particle, but certain degrees of velocity in a certain number of particles, and that certain number of particles, in a given time, is confessedly as the degrees of velocity.

Remark II. And this leads to a second remark, which is, that the entire effect of these pressures being taken into consideration, seems to overturn this new rule in mechanics for computing the forces of moving bodies, which is, that the forces are as the quantities of matter multiplied by the squares of the velocities. And this Mr. E. makes out thus: the gentlemen who advance this new rule, at the same time that they assert the velocities, in the cases of the experiment abovementioned, to be as the square roots of the altitudes, do also confess, that the quantities of the fluid, pressed out in equal times, are as those velocities. Now if this be true, that the quantities of water, flowing out in equal times, are as the velocities, then the forces cannot be as the quantities of matter multiplied by the squares of the velocities; for then the effects, instead of being proportional, would be more than in proportion to their causes. Thus, the effect of a pressure of a column of any fluid, as water, 9 inches high, instead of being but 9 times greater than that of 1 inch above the orifice, will be no less than 27 times greater. For the velocity being at this height triple, the quantity of matter in a given time will also be triple; which last, multiplied by the square of the velocity, gives 27 for the force communicated by a pressure of 9 inches in altitude, while the force communicated by the pressure of 1 inch, is but as 1. So that the moving forces produced will be as 27 to 1, while the causes producing these forces, are but as 9 to 1, i. e. three times too little for such a purpose.

Thus again, if the velocities be as 1 and 4, the quantities of water issuing out will be as 1 and 4; but the effects, or forces produced, according to the new rule, will be as 1 and 64; though the pressures which communicate them are only as the altitudes, which are as 1 and 16. Whereas, to produce such effects, the altitudes of the latter column ought to have been as 64, i. e. four times greater than by experience it is found to be.

Remark III. Mr. Eames observes, in the last place, that the common rule of estimating the forces of moving bodies, by the quantities of matter multiplied by their velocities, is rather confirmed by these very experiments. For then, according to the old maxim, effects are proportional to their causes, the forces communicated will be as the forces communicating, or pressures. Thus, let the altitude, and consequent pressure of any column of water, be 9 times greater than the altitude of another; then the velocity of every single particle of water pressed out will be triple, and the number of particles issuing out in a given time will likewise be triple; therefore the force resulting from these two multiplied together, according to the common rule, will be 9, proportional to the pressure, as it ought to be. So again, if the altitude be 16 times greater, the velocity will be quadruple, and the number of the particles quadruple, and the force produced the product of these two, i. e. 16, still proportional to the altitude, or pressure.

And universally, the forces communicated, according to the old rule, are in a ratio compounded of two others, one of the quantities of matter, and the other of the velocities; the ratio of the velocities, by the experiments, is the subduplicate ratio of the heights, and the ratio of the quantities of matter is, by confession, likewise the subduplicate of the heights; therefore the compound of these two is the ratio integra, or simple ratio of the heights, in which ratio are the pressures themselves, which produce these moving forces; so that, according to the common rule, the effects are always, as they ought to be, proportional to their causes.

And after the same manner the ingenious Mr. Gravesande once argued, viz. in paragraph 355 of Physices Elem. Mathemat. 1st edition.

On the Efficacy of Camphire in Maniacal Disorders. By Dr. David Kinneir, Coll. Med. Edinb. Soc. N° 400, p. 347.

1. A gentlewoman of 19 years of age, from an obstinate fasting for 2 days, and aversion to see company, in a religious turn before Easter, fell into a deep melancholy; she would not talk, nor answer any question for some time, but moaned and sighed continually; slept very little for 10 days. This happened in the decline of the moon. The night before the change she spoke, and called for some water to drink, which being given her, she immediately fell a starting and laughing, and her eyes sparkled with uncommon briskness; then began to talk wildly, and continued so all that night. She became next morning very furious; on which a physician was called, who bled her 4 times a week the first 14 days, vomited her, purged her, used the Cold Bath, and many other methods common in such cases, all to no purpose. In this condition she continued for 9 months, when Dr. K. was applied to.

He first began her with an antimonial vomit, which had no other effect than that of setting her fast asleep for 12 hours. Next day he gave her ʒss of camphire in a bolus, and as much at night. She continued to rest well all that night, and had a great moistness all over her body, and in the day-time a plentiful discharge by urine. Thus for 4 days he plied her, and afterwards, in the day-time, ordered her pills of Æthiops, Gum-guaiac. Cinnab. Antim. et pulv. de Gutteta; and at night, the dose of camphire. Sensible alterations appeared every day for the better, and in 3 weeks time she enjoyed the full use of her reason; having no extraordinary ailing ever since, after 9 years.

2. A gentleman of a good family, 17 years of age, from some disappointment, became very silly and stupid. He continued in that way for a long time, though he had the best advice. He had a great inclination to chaw and eat every thing that came in his way. He rested pretty well at nights, and was delighted with the sight of every glaring object. He hid every thing he could lay his hands on, as well in company as alone, and was not any ways ill-natured. Thus for half a year he continued, when he became so furious, that two men could hardly hold him from beating every one that came about him. Thus he would continue for 3 or 4 days together, without sleeping; then he would become calmer, after having slept some time; but was a little mischievous. This was the way from new moon till the full; then he became silly and melancholy, speaking little, and looking always down, not caring to look persons in the face. So it was alternately for 14 months.

When the Dr. came to him, he found him full of complaints of his ill treatment; and had he not had a watchful eye upon him, he had suffered from a blow intended at him; but he got him to take the medicines as before, and with allowing him a more plentiful diet, and a great quantity of diluting drinks, he became perfectly well in 6 weeks, and has enjoyed a good state of health and sound judgment these several years.

3. A mercer's wife, 36 years of age, having borne 4 children, and who was naturally of a lively, active disposition, fell so ill, all of a sudden, one day at sermon, that with much ado they could get her out of church with common decency. She tore every thing about her, talked much, and uttered horrid oaths. She had the best advice, that Edinburgh could give her. They failed of success, and left off visiting her. About half a year after, the Dr. was consulted about her. He treated her in the foregoing manner, and in 4 days there was a sensible alteration for the better, and in 4 more she went abroad. He still continued the use of the medicines for 14 days after recovery, and now she is as well as ever she was in her life.

4. A young man aged 20, of a full habit, was so very bad in the sullen, despairing way, wounding himself with his teeth and hands, that there was a necessity of close watching him. The Dr. vomited him twice, gave him some other things common in such cases; then began him with the camphire, which in 10 days brought him to reason. He relapsed after that on the change of the moon; was ill for 3 days, but not so as before. He came out of it again, and stood the change of the next moon with only a little heaviness, as in the hypo. At the height of the same moon, the day before, he was very uneasy, and seemed to resemble a person greatly hypochondriac. The Dr. still continued to him the camphire, and the other alterative medicines, for some time, but in a much smaller dose than in his illness: in 9 weeks he was perfectly cured, and continues in good health.

A Method for determining the Geographical Longitude of Places, from the Appearance of the common Meteors called Falling Stars. By George Lynn, Esq. of Southwick, Northamptonshire. N° 400, p. 351.

On perusing Dr. Halley's account in the Philos. Trans. N° 360, of that extraordinary meteor which appeared all over England, 19th of March, 1718-9, Mr. Lynn observed one very great use he suggests might be made of those momentaneous phænomena, in determining the geographical longitude of places, if we could but have the least notice of their appearing, &c.

Mr. Lynn thinks that some other meteors which are very frequent, though little noticed, might serve very well for the same purpose; viz. those vulgarly called shooting or falling stars, being a kind of natural sky-rockets, discharged at a very great height. Now supposing these to be discharged only at 20 or 30 miles high, they may be seen by different observers at the same moment of absolute time, in very distant places from each other, which is the thing required: for, if in any two places, as the doctor says, any two observers, by help of pendulum clocks duly corrected by celestial observations, do exactly note at what hour, minute, and second, such a meteor is discharged, the difference of those times will be the difference of longitude of the two places; nor does it require so much as the use of a telescope, as in the methods hitherto practised for that purpose. Now these natural rockets are found to be very frequent in every star-light night; but especially after a stormy day, or in a stormy night. If therefore persons who are prepared, as above, to be exact in their time, and also have a moderate knowledge of the several constellations, so as to describe the track of any of those meteors among the stars, would but bestow any determinate hour to be agreed among them, as for instance, from 8 to 9 each such night, to watch and observe those explosions, noting down immediately the time and track of them, it would be easy to determine, on comparing their observations, which of those explosions each of them see at the same time; and thereby the difference in longitude of those places would be exactly had, as above.

An Attempt made before the Royal Society, to show how Damps, or foul Air, may be drawn out of all Sorts of Mines, &c. by an Engine contrived by the Rev. J. T. Desaguliers, F. R. S. N° 400, p. 353.

The engine consists of a triple crank, working 3 pumps, which both suck and force air, by means of 3 regulators, and are alternately applied to drive air into, or draw it from any place assigned, through square wooden trunks; which being made of slit deal, and 10 inches wide in the inside, are easily portable, and joined to each other without.

Exper. 1.—Dr. D. filled a tall cylindric glass with the steams of a burning candle and burning brimstone matches, in such manner, that a lighted candle would go out almost as soon as it was let down into that foul air. Then fixing the trunks, or square pipes, to the forcing hole of the engine, he drove fresh air into the bottom of the abovementioned receiver; so that the foul steam came out at the top of the receiver, which was open.

Exper. 2.—Having filled another receiver, close at top, with foul steams, as before, he placed it in a position almost horizontal, only with the close end something above the open end, that the foul steam might not go out of itself, when specifically lighter than common air. He fixed the trunks to the sucking-hole of the engine; and by working the engine, drew out the foul steams from every part of the receiver, as the trunks were applied to them successively.

Exper. 3.—Having filled with foul steams, and set upright, as in the first experiment, the cylindric open receiver, he applied the trunks to the sucking part of the engine, with their open end near the bottom of the receiver. Then, by pumping, the steams were all drawn downwards, and so out at the top of the trunks at the engine; whereas, in the first experiment, they were driven out at the top of the receiver.

Exper. 4.—Having set a candle in the cylindric receiver abovementioned, without having filled it with steams, and let down the trunks into the receiver, below the flame of the candle, he laid the wet leather over the mouth of the receiver, leaving about half an inch open, for the air to come in; notwithstanding which the candle began to dwindle, and was ready to go out; but working the engine with the trunks joined to the forcing part, the candle revived, and burned, at last, as well as in the open air. On leaving off pumping, the flame of the candle diminished again; but when it was ready to go out, it revived again, on forcing in more air with the engine.

Remarks on the Experiments.—When damps in mines are specifically lighter than common air, they will be driven out of the mine by the first experiment.

When damps are specifically heavier than common air, they may be sucked out by the second or third experiment.

When a sough, or adit, is carried from a mine to any distant valley, to discharge the water, or save the trouble of raising it quite to the top of the pit; then shafts, or perpendicular pits, are generally sunk from the surface of the earth to the said sough, to prevent the workmen from being suffocated as they dig the sough, and that at a great expence; but, by the 4th Experiment, fresh air may be driven down to the workmen, to continue their breathing free and safe, and to keep in their candles; by which means the expence of perpendicular shafts will be saved.

It has been found by several experiments, that a man may breath a gallon of air in one minute, and a candle of 6 in the pound will burn nearly as long in the same quantity of air; therefore the model only is capable of supplying fresh air to one man; and consequently, a large engine will abundantly supply air for the burning of candles, and the working of a great number of men in a mine.

Fire will not do in all cases, though in some, it will draw foul air out of mines with success; because several sorts of damps extinguish fire, and some fulminate, and are dangerous, when fire comes near them; and even in common stagnant air, fire will not keep in long.

An Account of the Norwegian Finns, or Finlanders. By Peter Kinck, Esq.

N° 400, p. 357.

In the confines of Norway which border upon Sweden, live a people called Finns, whose habitation is in the woods and forests, and who are some of them under the Danish, some under the Swedish jurisdiction; of whose origin, nature, and manner of life, the following is some account.

1. Their original was from Swedish Finland, eastward of the sea of Bothnia, from whence, by want of the necessaries of life, they formerly transported themselves into Sweden and Norway, where in the forests they got leave to build and inhabit. Finland formerly was not so well cultivated as at present; so that the produce of the earth was not sufficient to subsist the numbers of people born there; hence many of them, as the Goths and Vandals, were obliged to seek out new quarters: and though these people have for the most part kept to their native language, yet have they made several alterations in their manners and ways of living.

Though the much greater part of them both understand and speak the Norwegian tongue, as well as the Swedish, yet they mostly use their own, which has not the least affinity or resemblance of either the other two. For example, the Swedes and Danes, in numbering, tell 1, 2, 3, een, toe, tree, they in theirs tell yx, kax, kolime; and as the first, for give me bread, say giv mig brod, they say alla mina leip, &c. And on hearing them converse together, it is surprising to conceive how they understand each other, as they speak so very low, that they can scarcely be heard by others.

2. They are generally low of stature, but strong and hardy, and healthy; their eyes are lively, their noses high, and their teeth even and white, their feet short. The women are generally so strong of constitution, that in child-bearing they seldom need any assistance, and soon return to the business of the family again, except here and there a weakly constitution. They are generally ignorant and silly; but this must be attributed to the little converse they have with each other, and the rest of the world; since the men, whose affairs call them often to traffic with their neighbours, and in different provinces, are of good natural parts, sharp, and look well to their interest. They are frugal, parsimonious and humble, fearful of giving offence, and very respectful to their superiors: they will work whole days without any food, if they can only have tobacco to smoke or chew.

3. Their food consists chiefly in a sort of fish which they call oret, and which answers to our salmon-trout; being a very delicious fat fish, which they catch in the rivers that run through the woods, and next to that, in bread and rye meal, which they sometimes get great plenty of, by cutting down and burning whole forests, and sowing the ground with rye, which sometimes produces 30 or 40 fold. But this method of producing has been of late strictly forbidden, under very severe penalties, by the proprietors of those forests, by reason of the damage by the loss of so much timber; and because the fire sometimes lays waste immense tracts of land: so that it is with great difficulty extinguished, burning whole months together, to the great detriment of trade.

They frequently use bathing, at least once a month, thus thinking to prevent sickness, and dissipate all weaknesses from the body: and their method of bathing seems so very particular, as would scarcely agree with any other constitution, or meet with approbation from physicians. The method is this: In the middle of the house, which usually consists of one large room, built all of whole timbers laid across, and notched in at the ends to let them close, and then caulked with moss, as the seams of a ship are with okam, they build an oven with stone without mortar, and without a funnel; the smoke going out at a hole in the roof, which is left open while the wood is burning in the oven, but shut close as soon as it is all burnt to a bright coal, which keeps in all the heat. When the oven is thus made red hot, they then strip, both men and women, without any reserve, and place themselves on benches made near the roof on purpose: then cold water is brought in, which from time to time is sprinkled on the oven; from whence arises a thick steam on the bathers, which makes their bodies so warm, that they sweat profusely. Each person has a rod in their hand, with which they gently beat their whole bodies; and if they find themselves so hot that they cannot well endure it, they call for cold water, which they pour over themselves in so dextrous a manner, that it diffuses itself over their whole bodies, and so cools them again. Thus when they have bathed sufficiently, they go directly out into the air, though in the most inclement season of the year; and will even roll themselves in the snow for a good while together, without any harm from it. And this method of bathing they make use of as their ordinary cure, when they find any indisposition.

In the winter, when the ground is covered with snow, they make use of a sort of long wooden shoe, 3 or 4 ells long, on which they go so swiftly, that in 2 hours time they will run 13 or 14 miles: and, as they are generally good marksmen, with their guns they kill abundance of wild game, both to help to support their families, and sell to buy themselves necessaries.

They are very ignorant about christianity, by reason of their great distance from any towns: yet though they seem so ignorant and barbarous, it is very rare that they are guilty of any considerable crime.

Of a preternatural Perforation in the upper Part of the Stomach. By Mr. Christopher Rawlinson, Surgeon. N° 400, p. 361.

One James Skidmore had complained, for 3 or 4 years, of a violent pain in his stomach and bowels, never being able to rest in his bed at night, until he had vomited up the greatest part of what he had eaten or drank the day before. He would often compare his pain to some great weight lying on the region of the stomach, which he in some measure alleviated, by pressing hard with his hand on that part. When he turned himself in bed from one side to the other, he said he could plainly perceive that some fluid or other fell down with noise to the depending side; which fluid he believed to be the occasion of all his misery: for which reason he often said, he would willingly consent, nay, often earnestly pressed, that the surgeons would cut him open, as he expressed it, and let it out.

He had no apparent tumour on the part, nor was his belly more extended than usual. He had the advice of several able physicians before he came into the hospital, but to no purpose. On opening him after his death, as soon as we had penetrated the peritonæum, there flowed out a whitish liquor, resembling whey, only a little more thick and fæculent; nor did it emit so noisome a smell as might be expected from its long residence in that place. Above 4 quarts of this liquor were contained in the cavity of the abdomen.

The stomach was perforated in its upper part, about the middle space between the 2 orifices, wide enough to contain the end of one's finger. Cutting it open lengthwise, it was pretty full of a thick glutinous matter, inclining to yellow; and to its inner coat, on the lower side, there firmly adhered the stone of a prune, or some other fruit resembling it. On its inside, near the preternatural perforation, it was gangrened for 2 or 3 inches; and on the other side of the perforation there was an ulcer near the same size. The whole stomach was a great deal thicker than usual; but that part next the pylorus was above 4 times thicker than in a natural state. It adhered closely to all the parts about it; and to the pancreas it was so firmly tied down, that it could not be separated without tearing. The spleen did not exceed $\frac{1}{4}$ oz. in weight. The pancreas was scirrhous, though pretty near its natural size. In the liver and kidneys there was no apparent defect; nor had the parts in the thorax received any visible alteration, except that the lungs adhered more firmly to the pleura than usual. The intestines and all the viscera contained in the abdomen were of a whiter colour than usual, by being so long steeped in the liquor they floated in.

On a Human Skeleton of an extraordinary Size, found in a Repository at Repton in Derbyshire; also some Examples of Longevity. By Simon Degg, M.D. and F.R.S. N° 400, p. 363.

Repton, or Repingdon, is a town on the Trent, the burial-place of the Mercian kings, whose chief seat was at Tamworth in Staffordshire. It is also remarkable for its free-school, and its ancient abbey. Having viewed the ruins in this town, and inquiring for antiquities, the inhabitants brought one Thomas Walker, a labourer, 88 years of age, who gave the following account.

About 40 years before, cutting hillocks, near the surface he met with an old stone wall; on clearing farther, he found it to be a square enclosure, of 15 feet: it had been covered, but the top was decayed and fallen in, being only supported by wooden joices. In this he found a stone coffin, and with difficulty removing the cover, saw a skeleton of a human body 9 feet long, and round it lay 100 human skeletons, of the ordinary size, with their feet pointing to the stone coffin. The bottom of this dormitory was paved with broad flat stones, and in the wall was a door-case, with steps to go down to it, whose entrance was 40 yards off, nearer the church and river. The steps are stone, and much worn: it was in a close, on the north side of the church, and over this repository grew a sycamore, planted by the old man when he filled in the earth. This was attested by several old people, who had likewise seen and measured the skeleton.

Instances of Long Life.—At Uttoxeter, in the Moorlands of Staffordshire, 1702, were buried, in 14 days, 3 women; the first 103, the second 126, and the 3d 87: and the same year two sisters and a brother of that parish; the brother 92, the younger sister 95, the elder sister 98. And in the same parish this present year, 1726, were buried in 22 days, the following aged people:

Aug. 7, a woman 94, and a man aged 81. Aug. 4, a man aged 68. Aug. 19, a man aged 87. Aug. 22, a man aged 82. Aug. 23, a man aged 83.

In 1726, at Gravely in Hertfordshire, 31 miles from London, almost every 6th person is upwards of 60. Inhabitants 187; aged above sixty, 29.

A Sequel of the Bills of Mortality, &c. of several considerable Towns in Europe, for the Years 1722 *and* 1723, *extracted from the Acta Breslaviensia. By Sir Conrad Sprengell, F. R. S.* N° 400, p. 365.

1. *A List of those that were born and buried, &c. in Breslaw, &c. in the Year* 1722.—Buried 1791. Christened, males 665; females 670. In all 1335. Married 424 pair. Among the dead were, married men 231; married women 149; widows and widowers 150; bachelors 57; maidens 52; children to 10 years of age, boys 570; girls 499; still-born, boys 53; girls 30. In all 1791.

In Vienna, buried, men 1038; women 942; boys 1551; girls 1430. In all 4961. Among which was, 1 of 105, 1 of 110, and another of 111 years of age. Christened, children 4417.

In Dresden, buried 1519; christened 1514; married 451 pair.

In Leipsic, buried 994. Christened, males 463; females 405. Married 361 pairs.—Among the dead were, 78 from 60 to 69 years old, 50 from 70 to 79, 15 from 80 to 98.

In Weimar, buried 143, christened 190. Among the latter were 107 boys and 83 girls; and among these were 6 twins and 1 bastard; 73 pair were married.

In Berlin, buried 2499. Christened 2701. Among which were 260 bastards. Married 742 pair. So that there were 202 more born than buried.

In the kingdom of Prussia. Christened 20672; pairs married 4420; buried 11316.

In all the king of Prussia's dominions, christened 81770; pairs married 2077; buried 52218.—Among the dead, were 71 above 90 and 100 years of age; and one of 120.

In Nurenberg, buried, married men 194; married women 233; bachelors 42; maidens 61; boys 285; girls 230; still-born 12. In all 1057. Born 1055, viz. males 501; females 554. Among which were 12 twins.

In Copenhagen, born, males 1345; females 1256. In all 2601. Married 785 pair; buried 1999.—So that there were 602 more born than buried.

In Amsterdam, died 8421; in Epperies were born 213; buried 135; in Dantzic were born 2092; married 490 pair; buried 1442: in Rawitz were born 200; buried 74.

The Bills of Mortality of the Year 1723, *from the Acta Breslaviensia.*—In Breslaw, buried 1321. Christened, males 711; females 684. In all 1395.

Married 422 pair. Among the dead were, married men 220; married women 118; widows and widowers 150; bachelors 48; maidens 46; children to 10 years of age, boys 369; girls 306; still-born, boys 38; girls 26. In all 1321.

In Vienna, buried 5443. Among which were 1079 men and 974 women, 1758 boys, and 1632 girls. Christened 4457.

In Lobau, buried 171. Christened 226, and 70 pair married.

In Freyberg, buried 321. Christened 362.

In Dresden, buried 1654. That is, 165 married men; 136 married women; 36 widowers, and 138 widows; 68 bachelors; 64 maidens; 580 boys, and 467 girls. Still-born, boys 54, and girls 46. Christened 1510. That is, 756 boys and 754 girls. Among which were bastards, 66 boys and 41 girls. Married were 396 pair.

In Leipsic, buried 928. Among which were 126 married men, and 88 married women; 53 bachelors and 36 maidens; 199 boys and 164 girls; 21 women in child-bed; new-born boys 60, and girls 46, besides still-born boys 37, and girls 30; also 68 widows and widowers. Christened were 966; as 489 boys, and 477 girls. Among which were 11 pair twins, and 99 bastards. Married have been 306 pair.

In Erfurt, buried 448. Christened 666.

In the elector of Mentz's dominion, in 75 villages are buried 543. Christened 872.

In Berlin, buried 2618. Christened 2770. Among which are 289 bastards.

In the towns and villages of the old, middle, and Uckermark are christened 19058. Among which are 995 bastards. There were married 4943 pair, and buried 13317. Among which were 23 who lived to 90, 100, and 1 to 112 years old. So that there were 5741 more born than buried.

In the rest of the King of Prussia's dominions, born 83515, married 21059, buried 55830.—Among the born are 2157 bastards, and among the buried 77 that were above 90 and 100 years old. So that there were 27685 more born than buried.

In Copenhagen were born 2604; buried 1914; married 701 pair. Therefore 690 more born than buried.

In Amsterdam there died 7119, and were married 2289 pair.

In Epperies, born 203, died 132.

In Dantzic, born 2002, buried 1495, and married 537 pair.

Of a Purulent Discharge from the Mouth of a Boy, which continued for 3 *Years, and was followed by a Caries and Separation of the whole of the Cheekbone. By Peter Hardisway, M. D.* N° 400, p. 374. *An Abstract from the Latin.*

On the 6th of Feb. 1724, Dr. H. was called to a boy 10 years old, who had been afflicted with a slow fever for 6 weeks before. By the use of proper medicines he was cured of the fever in about a week; but a few days after his recovery, a large tumour formed within the right cheek, the boy continuing well in other respects. On causing him to open his mouth, which he did with much difficulty, a quantity of pus made its appearance; but at this first examination Dr. H. could not ascertain from whence the pus proceeded, the boy was so fretful and impatient. A fig boiled in milk was ordered to be put into the mouth, with a view to promote suppuration. The tumour breaking the next morning, there was an almost incredible discharge of extremely fœtid pus. Dr. H. prescribed a detergent gargle. In the evening the discharge was as copious as it had been in the morning. This excited much surprise, and induced the Doctor to examine the mouth more carefully, when he discovered that the upper jaw-bone on the right side was become carious, and that the aforesaid purulent discharge proceeded from the alveoli, the boy himself having easily pulled out the loosened teeth with his fingers. The management of the caries was then committed to a surgeon, who would not (conformably to the suggestions of Dr. H.) use detergent and consolidating applications; but contented himself with applying to the carious jaw a certain tincture, supposed to be tincture of myrrh. This was of no avail; the diseased cheek swelled to twice the size of the other, and at length the whole of the cheek-bone was extracted by the surgeon's instrument. Half of the os palati, with the septum narium, and the lower part of the orbit of the [right] eye, has been removed. The tumour being seated over the processus zygomaticus, the disordered cheek appears twice as large as the other (notwithstanding the removal of the bone) and the [right] eye is almost closed up, while the purulent discharge still continues.

In another letter dated Dec. 24, 1727, Dr. H. mentions that the tumour remained in the same state as before, and that the purulent discharge was undiminished: that in other respects the boy was hearty; that he ate, drank and slept well; and that he joined chearfully in the diversions of those who were of his own age.

The Dissection of the Poisonous Apparatus of a Rattle-Snake, made by the Direction of Sir Hans Sloane, Bart. With an Account of the quick Effects of its Poison. By John Ranby, Surgeon, F. R. S. N° 401, p. 377.

This rattle-snake was sent from Virginia, and delivered to Mr. Ranby, on purpose to make such experiments with it as might inform mankind of the fatal symptoms which attend its bite, and the appearances in the dead bodies of such animals as have been bitten by it. Removing the common integuments of the head, the muscles that raise the poisonous fangs appear; the first of which arises with a short fleshy beginning, from the upper edge of the lower jaw, near the articulation of one of those bones which Dr. Tyson calls maxillarum dilatores, as represented at A, fig. 7, pl. 4, and sends a few carnous fibres to the side of the cranium; then becomes tendinous, and so proceeds to its insertion in the outside of the bone, which receives the poisonous fang. Fig. 8.

Removing this muscle, there appeared a gland, B, fig. 7, about the size of a small pea, which Mr. R. takes to be one of the maxillary glands, for the following reasons: first, the structure of the parts and its distance from the fang make it unlikely to be designed for separating the poisonous fluid, but rather a saliva to moisten the aliment, in order to make it pass down the œsophagus with ease, the stomach of those animals being but small, and the gullet considerably larger; not without some analogy to the ingluvies or crop of granivorous fowls, where the food stops for some time and is moistened, before it is capable of descending into the stomach. 2dly, These parts are so contrived, that on opening the mouth to receive the prey, when such a fluid is most wanted, the muscle abovementioned pressing on the gland promotes the discharge of its contents into the mouth. The duct of this gland seems to open between the upper lip and the jaw; but as the excretory ducts of so small a gland are rarely to be seen with certainty, Mr. R. pretends not exactly to determine its aperture. Under this gland lies another muscle, smaller than the former, which arises and is inserted near it, at C, fig. 7; these two muscles draw the bone D, in which the poisonous fang is fixed a little outwards and upwards. Between the last described muscle and gland passes a nerve, to the upper part of the bone which receives the tooth E, fig. 7, and B, fig. 8; and it is probable that this nerve has been taken for the excretory duct of the gland beforementioned. Opening the mouth, 2 small eminencies appear in the fore part, on the inside of the upper jaw, being a membrane, raised by the fangs and drawn over them like the mouth of a purse, at AB, fig. 9, and C, fig. 8. This membrane is thick and strong, and placed in a microscope, appears to have a number of glands, some of which are even visible to the naked eye. In a common viper Mr. R. observed one on each side the fang. These membranes prevent the involuntary discharge of the poison out of the fangs into the mouth, as also the killing with the fangs small animals on which they sometimes feed. Putting back this membrane, the fatal fangs appear, which on first view seemed to be only one on each side, till searching further there appeared 4 more; the first and largest is fixed in a bone, which is articulated to the fore part of the upper jaw, F, fig. 7. The 4 others are fastened in and covered with strong tendinous membranes, and lie as it were over each other, as B, fig. 8, and CE, fig. 9. These teeth are crooked and bent in this form , especially the first, and have each 2 perforations, the one on the upper part, the other at the lower part of its convex side; which last comes quite to the point, and resembles the sloping cut of a pen. The upper perforation A, fig. 10, Mr. R. imagines receives the poison, the other transmits it into the wound, B. All these fangs are tubular, the largest of which contained a small quantity of a transparent fluid, of a light yellowish colour, which, on putting the snake into spirit of wine, changed to a beautiful red. Freeing the mouth of the membrane, a muscle appears, about the size of the first described above, which arises from the middle of the maxillarum dilatores, DD, fig. 9, and is inserted on the under side of the largest tooth; for the force required to pull down the fang being less than to raise it, fewer muscles are required.

This animal was in Mr. Ranby's custody about a month, during which time he bit 3 dogs and a cat; the first two were bitten at the College of Physicians, and of these the first died about 2 minutes after the bite, and the moment he was bitten he became convulsed, and lost the use of his limbs. The wounds were exceedingly small, and between the pectoral muscles. On opening the dog, the skin and membrana adiposa, for the breadth of a crown, were livid about the wound, as if from a violent blow. The second dog, bitten at the college, had the same symptoms with the first, but lived near a ¼ of an hour, and had bloody stools. Three days after, Mr. R. carried the snake to bite another dog and cat. The dog was larger than either of the two former, and having been bitten at the extremity of the nose, he was immediately affected, howled, shook, fell down and foamed at the mouth; and in about 10 minutes discharged his excrements involuntarily, tinged with blood: he died in about 2 hours. The next day opening the body, the abdominal contents were very much inflamed, especially the stomach and intestines, which appeared nearly equal to the finest injection; the stomach and intestines contained a mucous matter, the greatest part of which was blood, and the fine villous coat, which is so visible in these animals, was entirely destroyed. About an hour before he was bitten, he had a plentiful meal of coarse beef, of which there was not the least appearance. The pleura and other membranes looked as if injected; the heart was turgid with blood, as were also its vessels. The vessels of the membranes of the brain made a most beautiful figure. from the quantity of blood contained in them; as did likewise the blood-vessels of the nerves; there was a small quantity of water between the 2 hemispheres. The blood contained in the heart, and its vessels, was an even mass, about the consistence of cream.

The cat had nearly the same appearances, and lived about 5 hours.

*On the present Controversy among Mathematicians, concerning the Proportion of Velocity and Force in Bodies in Motion. By the Rev. Dr. Sam. Clarke.** N° 401, p. 381.

It has often been observed in general, that learning does not give men

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* Dr. Samuel Clarke, a learned divine and philosopher, was born in 1675 at Norwich, of which city his father was alderman, and for which he was during several years member of parliament. He studied in Caius college, Cambridge, where he applied himself to the new philosophy with uncommon success. Rohault's physics was then the text book in natural philosophy at that university; and this Mr. Clarke, at 22 years of age, translated into better Latin, accompanied with notes. He applied also to theology; and when ordained he became chaplain to Dr. Moore, bishop of Norwich, who gave him the rectory of Draycot, in Norfolk. In 1701 he published his Paraphrase of St. Matthew's Gospel; which was afterwards extended to the other three Gospels; the whole making 4 volumes in 8vo. In 1704 and 1705 he preached the Boyleian Lectures; sermons which are still generally admired. And about this time, according to Whiston, he embraced Arianism. In 1706 he printed his learned and philosophical letter to Dodwell, on the Immortality of the Soul, in a controversy that continued for some time. The same year appeared his translation of Newton's Optics, into Latin; on which occasion it is said that philosopher complimented him with £500. He was also about this time presented to the rectory of St. Bennet's, Paul's Wharf, London, and appointed chaplain to Queen Anne. And in 1709 he obtained the rectory of St. James's, Westminster, and took his degree of D. D. at Cambridge.

In 1712 Dr. Clarke published an elegant edition of Cæsar's Commentaries. And the same year came out his celebrated book, The Scripture Doctrine of the Trinity; which was written against by many authors, and also complained of by the Lower House of Convocation. The second edition appeared in 1719, much altered and enlarged. In 1715 and 1716, he had a dispute with Leibnitz, on the principles of natural philosophy and religion. And in 1717 he printed Remarks on Collins's Philosophical Enquiry concerning Human Liberty. About a year afterwards Dr. Clarke ventured to make an alteration in the doxology in the singing Psalms, which gave occasion to a number of controversial pamphlets on the subject. And about this time the Dr. was presented with the Mastership of Wigston's Hospital in Leicester. In 1724 he published 17 sermons, preached on several occasions. And in 1727, after the death of Sir Isaac Newton, the Dr. was offered the place of Master of the Mint, which he declined. In 1729 he published the first 12 books of Homer's Iliad, with a Latin version and annotations. The remaining books were published by his son in 1732.

understanding; and that the absurdest things in the world have been asserted and maintained, by persons whose education and studies should seem to have furnished them with the greatest extent of science.

That knowledge in many languages and terms of art, and in the history of opinions and romantic hypotheses of philosophers, should sometimes be of no effect in correcting men's judgment, is not so much to be wondered at. But that in mathematics themselves, which are a real science, and founded in the necessary nature of things; men of very great abilities in abstract computations, when they come to apply those computations to the nature of things, should persist in maintaining the most palpable absurdities, and in refusing to see some of the most evident and obvious truths; is very strange.

An extraordinary instance of this, we have had of late years in very eminent mathematicians, Mr. Leibnitz, Mr. Herman, Mr. Gravesande, and Mr. Bernouilli; who, in order to raise a dust of opposition against Sir Isaac Newton's Philosophy, the glory of which is the application of abstract mathematics to the real phænomena of nature, have for some years insisted with great eagerness, on a principle which subverts all science, and which may easily be made appear, even to an ordinary capacity, to be contrary to the necessary and essential nature of things.

What they contend for, is, that the force of any body in motion, is proportional, not to its velocity, but to the square of its velocity. The absurdity of which notion, I shall first make appear, and then show what it is that has led these gentlemen into error.

In the nature of things, it is evident, that every effect must necessarily be proportionate to the cause of that effect; that is, to the action of the cause, or the power exerted at the time when the effect is produced. To suppose any effect proportional to the square or cube of its cause, is to suppose that an effect arises partly from its cause, and partly from nothing.*

This great man, who had enjoyed a uniform state of health, was suddenly seized with a pain in his side May 11, 1729, of which he died 6 days after. The same year appeared his Exposition of the Church Catechism, and 10 volumes of his Sermons.

Bishop Hoadly's character of Dr. Clarke, though high, is not extravagant. Observing how extraordinary it is that a man should possess an equal degree of excellence in different branches of knowledge, he says, "It ought to be remarked, in how particular a manner, and to how high a degree, divinity and mathematics, experimental philosophy and classical learning, metaphysics and critical skill, all of them various and different, as they are among themselves, united in Dr. Clarke."

* Which is just like the supposition made by those mathematicians, who have taken it for granted, that $\frac{1}{0}$ is equal to infinite; that is, that as 0 to 1, so 1 is to infinite; that is, that infinite multiplied by 0, is equal to 1, or an infinite number of nothings equal to something; which is palpably false. The true proportion is, not as 0 to 1, so 1 to infinite; but as an infinitesimal is to 1, so is 1 to infinite.

In a body in motion, there may be considered distinctly, the quantity of the matter, and the velocity of the motion. The force arising from the quantity of the matter, as its cause, must necessarily be proportional to the quantity of the matter: and the force arising from the velocity of the motion, as its cause, must necessarily be proportional to the velocity of the motion. The whole force therefore arising from these two causes, must necessarily be proportional to these two causes taken together. And therefore in bodies of equal size and density, or in one and the same body, the quantity of matter continuing always the same, the force must necessarily be always proportional to the velocity of the motion. If the force were as the square of the velocity, all that part of the force, which was above the proportion of the velocity, would arise either out of nothing, or, according to Mr. Leibnitz's Philosophy, out of some living soul essentially belonging to every particle of matter.

Whenever any effect whatever is in a duplicate proportion, or as the square of any cause; it is always either because there are two causes acting at the same time, or that one and the same cause continues to act for a double quantity of time.

The resistance made to a body moving in any fluid medium, is in a duplicate proportion to the velocity of its motion; because, in proportion to its velocity, it is resisted by a greater number of particles in the same time; and again, in proportion to its velocity, it is resisted by the same particles singly with a greater force, as being to be moved out of their places with greater velocity.

Light decreases in a duplicate proportion of its distance from the sun; because the rays divaricate according to two dimensions; according to the dimension upwards or downwards, and according to the dimension sideways. But according to the third dimension forwards from the sun, a ray of light undergoes no alteration; because the particles, of which it consists, being emitted all of them with an equal velocity, continue every where at an equal distance from each other.

One and the same cause, acting in a double quantity of time, produces the same effect, as two equal causes acting in a single quantity of time. One and the same force, in two parts of time, will cause a body in motion to describe

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And as the infinitesimal of an infinitesimal (that is, a second fluxion, or the second power of infinitesimal) is to 1, so is 1 to infinity of infinites, or the second power of infinite; that is, for instance, it is as a finite physical line to an infinite surface, or as a finite physical surface to an infinite solid. And as 0 (which is beyond all proportion lower than the infinit'th power of an infinitesimal) is to 1, so is 1 to that which is beyond all proportion higher than the infinit'th power of infinite. Which clearly removes the foundation of all the ridiculous consequences, which have been drawn from the supposition of the forementioned false proportion.—Orig.

the same space, as double the force would do in one part of time. The space described therefore by a body in motion, is not as the force; but as the force and the time taken together. A body, with any the least assignable force, will move through infinite space, if it meets with no resistance, in an infinite time. And in spaces where there is a uniform resistance to motion, the space described before the motion ceases, must needs be as the force and as the time together: because a double force will carry a body twice as far in the same time, and will also cause the motion to be twice as long time in destroying by a uniform resistance. The space described therefore before the motion ceases, is in this case demonstrably as the square of the force. A body thrown upwards with double force, will be carried four times as high, before its motion be stopped by the uniform resistance of gravity; because the double force will carry it twice as high in the same time, and moreover require twice the time for the uniform resistance to destroy the motion. The case is the same in accelerated motion; in bodies accelerated by a succession of elastic impressions, or falling with a motion accelerated by the uniform power of gravity, or by any other uniform power whatever. The space described must needs be as the force, and as the time wherein the force operates.

What I have thus demonstrated concerning any force, considered as the cause producing an effect; and concerning the time, during which the force operates; is on all hands acknowledged to be true concerning velocity. And therefore velocity and force, in this case, are one and the same thing. So that to affirm force to be as the square of the velocity, is to affirm that the force is equal to the square of itself.

Now from hence appears very clearly the ground of the error these gentlemen have fallen into, and of their misapplication of the experiments they build upon.

The effect of a force impressed on a moveable body, is the motion of that body from one place to another. Now forasmuch as the effect cannot but be proportional to its cause, hence Mr. Leibnitz, whom the other gentlemen have followed, contends that the space described by a body in falling, is proportional to the force by which it is impelled during its fall; and that the force acquired by a body in falling, is proportional to the space it has described in its fall. Which space being agreed to be as the square of the velocity, as being proportional to the velocity and to the time taken together, hence they infer that the force likewise is as the square of the velocity.

But from what has been said, it is plain. that the space described in these and all other the like cases, is not as the force only, but as the force and as the time wherein the force acts; that is to say, as the square of the force.

For the cause of the quantity of the space described, is not barely the quantity of the force, but also the continuance of the time wherein the force acts. The force therefore and the time taken together, being necessarily as the space described; as the velocity and the time taken together, are on all hands acknowledged to be; it follows that the velocity and the force are equal, and not the force as the square of the velocity.

When two unequal bodies, fastened at the ends of the arms of a balance of unequal length, counterpoise each other, and vibrate in equal times; as they must necessarily do, being fastened to the arms of the same balance: which is an observation Mr. Leibnitz lays great stress upon: in that case indeed the forces will be as the spaces described. But not therefore as the squares of the velocities. For in that case, the velocities themselves are as the spaces described, because the times are equal.

When a body projected with a double velocity, enters deeper into snow or soft clay, or into a heap of springy or elastic parts, than in proportion to its velocity; it is not because the force is more than proportional to the velocity; but because the depth it penetrates into a soft medium, arises partly from the degree of the force or velocity, and partly from the time wherein the force operates before it be spent.

In the collision of hard bodies, it is I think agreed on all hands, that it is demonstrated by reason, and confirmed by experience; that when a perfectly hard ball, moved with whatever degree of velocity, strikes full upon another hard ball, equal in size and weight, and without any motion in it; if the balls be unelastic, they will both go on together the same way, dividing the motion equally between them, with half the velocity the first ball had originally: but if they be perfectly elastic, the moving ball will communicate its whole motion and velocity to the quiescent ball, and itself lie still in the other's place. Were it true now, that the force of the moving ball was as the square of its velocity; these experiments would then show (which is infinitely absurd) that the force or vis inertiæ in the quiescent ball, the dead force, was always proportional to the square of the velocity (which these gentlemen affect fantastically to call the living force) of the moving ball, whatever its velocity were. Or the force in both might just as reasonably be supposed to be as the cube, or the quadrato-quadrate, or any other power of the velocity of the moving ball. Which is turning the nature of things into ridicule. Mr. Leibnitz, in some letters which he wrote into England, intimated that he had a prospect of a perpetual motion, founded on the notion of a vital principle, or active power in matter. But from the experiments now mentioned, it is evident that if the force of bodies in motion could be exalted even to the infinit'th power of their velocity;

yet since, to answer the phænomena of nature with regard to action and re-action, the same force must necessarily be allowed to all quiescent bodies likewise; it could be of no effect.

Astronomical Observations and Magnetical Variations, made at Vera Cruz. By Mr. Joseph Harris. Revised and communicated by Edm. Halley, F.R.S. N° 401, p. 388.

The latitude of Vera Cruz, by several distant observations, made with a quadrant of 4 feet radius, Mr. Harris found to be $19^\circ 12'$ N.

March 11, 1727, O. S. there happened a considerable eclipse of the sun, the greatest obscuration being about $10\frac{1}{2}$ digits; and having that morning carefully adjusted the pendulum clock, and fixed a telescope to the index of the foresaid quadrant, Mr. Harris observed it to begin in or about the S. E. by S. part of the sun's disk, at $49\frac{1}{2}$ minutes afternoon, apparent time; the altitude of the sun's centre then was $67^\circ 53'$.

The eclipse ended in or about the N. N. E. part of the solar disk, at $3^h\ 59\frac{1}{2}^m$ P. M. at which time the sun's altitude was $28^\circ 34'$.

In the years 1726 and 1727, Mr. Harris observed the magnetic variation several times, and found it to be about $2\frac{1}{4}$ degrees easterly. He also observed the variation several times on the voyage from England towards Vera Cruz, but always found that the best observations, when compared together, differed so much, that he could not depend on them, to much less than 3 or 4 degrees, or sometimes half a point of the compass.

A new Method for composing a Natural History of Meteors. By Mr. Isaac Greenwood, Prof. Math. at Cambridge, New England. N° 401, p. 390.

Mr. Greenwood here recommends it to all navigators to keep regular sea journals of the weather, recording the winds, currents, variations, &c. and in general all sorts of meteors whatever, that may be met with.

This method in general is, that in addition to such observations as should be made on land, there might be some account taken of those also that were made at sea; which already are by far more numerous than what were ever made ashore, or indeed what can be expected thence for some ages still to come.

Some Observations towards composing a Natural History of Mines and Metals. By Dr. Frank Nicholls, Prof. Anat. Oxon. N° 401, p. 402.

Mines in general are veins or cavities within the earth, whose sides receding from, or approaching nearer to each other, make them of unequal breadths in different places; sometimes forming large spaces, called holes. They are filled with substances, which, whether metallic, or of any other nature, are termed the loads. When the substances forming these loads are reducible to metal, the loads are by the miners said to be alive; otherwise they are termed dead loads.

In Cornwall and Devon the loads always hold their course from eastward to westward; though in other parts of England they frequently run from north to south. The miners report, that the sides of the load never bear in a perpendicular, but constantly underlay either to the north or south.

The mines seem to be, or to have been, the channels through which the waters pass within the earth; and, like rivers, have their small branches opening into them in all directions; which are by the miners termed the feeders of the load. Most mines have streams of water running through them, and when they are found dry, it seems to be owing to the waters having changed their course.

The load is frequently intercepted by the crossing of a vein of earth, or stone, or some different metallic substance. In which case it generally happens, that one part of the load is moved a considerable distance to one side. This transient load is by the miners termed a flooking; and the part of the load which is moved is, in their terms, said to be heaved. This heaving the load would be an inexpressible loss to the miner, did not experience teach him, that, as the loads always run on the sides of the hills, so the part heaved is always moved towards the descent of the hill. So that the miner working towards the ascent of the hill, and meeting a flooking, considers himself as working in the part heaved; therefore, cutting through the flooking, he works upon its back towards the ascent of the hill, till he recovers the load, and vice versâ.

Sometimes, though not constantly, the mine is lined with an intermediate substance between the load and itself. This is the wall of the load: though in the common acceptation of that term, it signifies either such intermediate substance, or the side of the mine, where the load immediately unites itself to it. The springs in these parts are always hard, as abounding very much, either in stony or sulphureo-saline particles. From this water, thus saturated with stony particles, we frequently find the passages of the water under ground, either partly or totally stopped up, the stony matter gradually concreting round the sides of the mine, and forming a confused load of spar-stone. At other times this stony matter concretes more distinctly; in which case the stony matter seems to be governed in its concretion by a plastic power.

There are many phænomena observable in the crystals, which are at present passed over, as less relating to the affair of metals; only adding that these crystalline concretions exert a strong attraction on many metallic substances.

The sulphureo-saline particles, with which the waters are frequently saturated, are found to be either of a vitriolic or an arsenical nature; the first constantly, if pure, concreting into white cubes resembling grains of silver, while the arsenical sulphur concretes into yellow cubes like grains of pure gold. Both these are by the miners termed mundick.

These sulphureo-saline substances seem directed in their concretions by a plastic particle, in the same manner as the crystals, and, like them, on the same principles, are found simple or compound. In their sides the concretion forms itself like threads, which in three sides run in different directions, but are always similar in the opposite sides.

The mines are found to contain tin, lead, copper, iron, and a pseudometallic substance, by the miners termed glist; of all which more hereafter.

Astronomical Observations made at Lisbon, in the Year 1726. *By Fa. Carbone.*

N° 401, p. 408.

These observations are of the eclipses of Jupiter's first satellite, which are not now of any manner of use, since the theory of those satellites has since been brought to much greater degrees of accuracy in the more modern tables.

Observations made in the Dissection of three Subjects. By Mr. Ranby, F. R. S.

N° 401, p. 413.

The first, a man aged 70 years, who died of a suppression of urine, occasioned by a stone stopping in the urethra, just within the glans, of the size of a horse-bean. This appearance, with the symptoms that had attended this miserable man, gave reason to expect something remarkable in the urinary passages. The ureters and pelvis were very much distended, which is common where great numbers of stones have descended down them, from the kidneys to the bladder. The bladder contained about 60 stones, the largest of which was about the size of a walnut, the others smaller; and just within the neck, was a hard tumour, as large as a nutmeg, which almost closed the orifice; and indeed the situation of this tumour was such, that it not only made the passing of the catheter very difficult, and prevented feeling the stones, by directing the instrument upwards; but likewise would alone produce the symptoms of the stone in the bladder, by obstructing the free discharge of urine through the urethra. Its inner membrane appeared as if lacerated in several places, and the tube was filled with a glutinous matter tinged with blood. On the back part of the vesiculæ seminales, near the prostata, were several stones, as large as peas, which closely adhered to the adjacent membranes.

The second, a boy aged 10 years, killed by a blow on the skull, whose spleen weighed 2 lb. and possessed almost all the left side of the abdominal cavity. The bladder, when distended to its greatest capacity, would not contain 1 oz.

The third, a man aged 25, who died of a pocky hectick, and some days before complained of a painful swelling in the testicle, which he said came the night before. Mr. Ranby found it to be a hernia aquosa, and would have punctured it, had he not felt, besides the water, a hard body, which he could not reduce. In a few days the patient died. Opening the scrotum, and separating the common membranes to the processus vaginalis, it contained about 4 oz. of water, besides a great part of the omentum; some portion of which adhered to the bottom of the cavity and the albuginea that immediately covers the testicle.

Observations of the Eclipses of Jupiter's Satellites, from 1700 *to the Year* 1727. *By the Rev. W. Derham, M. A. F. R. S. Communicated by Sir Hans Sloane, Bart.* N° 402, p. 415.

These observations are of the eclipses of Jupiter's first four satellites, made at several intervals by Mr. Derham, from the year 1700 to 1727. They were observed with good telescopes, of 16 feet or 12½ feet long, and the times regulated by a meridian instrument. The object was, by a series of good observations, compared with calculations made from the existing tables, to apply the observed differences to correcting the tables. Accordingly the observations are accompanied with calculations of the same eclipses, from two sets of tables, viz. those of M. Cassini, and of the astronomer royal at the British Observatory. But as we possess new tables of those satellites much more exact than those, and made from more regular and more correct observations than the present ones, these are therefore omitted, as no longer of any use whatever.

Of a Roman Pavement found near Grantham in Lincolnshire. By W. Stukeley, M. D. F. R. S. N° 402, p. 428.

In Feb. 1727-8, ploughing in the open fields of Denton, about 2½ miles drom Grantham, they met with a Roman pavement in Mosaic work. It lies partly in the glebe land, and partly in Madam Welby's. It has been a very large room about 30 feet both ways, as was found by digging in divers places;

but being so near the surface, not above a foot, or a foot and half deep, and having been ploughed over time out of mind, most part of it is ruined and imperfect. Besides many fragments of it, there was one piece entire, which was 30 feet long and 6 broad; and this was extremely pretty, the colours lively, the pattern or figure finely designed, as represented fig. 1, pl. 5. There are only three colours, white, red, and blue; but of the middlemost, or most beautiful part of it, which is but 9 feet long and 3 broad, the white and red is double in quantity to the blue. In the outermost part or verge of the work, there is no variety of colour, being entirely blue, and that made of much larger squares than the rest. On the east and west sides this was 6 feet broad, on the north only 3. The red is formed out of Roman bricks, several fragments of which were found about the work; the white colour is made of the common limestone of our country; the blue, of the stone from Benyngton, towards Newark, 5 miles from this place; and these colours wear well together, and produce a good effect. There were found in digging several parts of the foundations of the walls that terminated this room, and seemingly foundations of other rooms adjacent, which foundations were made of the common white stone of the country, set on edge side by side, with here and there a bit of Roman brick. The building was placed parallel with the quarters of the heavens. Some human bones were found, particularly many bones of a hand, which probably belonged to some unfortunate person killed in the ruins, or when the house was demolished.

Reflections on Mr. de Lisle's Comparison of the Size of Paris with London, and several other Cities, printed in the Memoirs of the Royal Academy of Sciences at Paris for the Year 1725. By Peter Davall, of the Middle Temple, Esq. N° 402, p. 432.

Mr. de Lisle, in the account he gives of his method of making an exact plan of Paris, and comparing it with London, and other cities, first shows, by what means he proceeded in determining, and laying down the true situation of the places in Paris: after which he explains his manner of drawing a true meridian line through that city; by which he was enabled to divide it by meridians and parallels, as is practised in a general map.

From his comparisons Mr. de Lisle concludes, that Paris is one-twentieth part larger than London, though he says he has excluded several gardens, contained within Paris, out of this mensuration, which would have made it bear still a greater proportion to London.

On reading this account of Mr. de Lisle's, it immediately occurred to Mr. Davall, that the method which he took of comparing the magnitudes of Paris and London, from whence he infers that the first of these cities is one-twentieth greater than the latter, is founded on a false supposition, viz. that under the parallel of Paris 20 degrees of longitude are equal to 15 of latitude, and consequently that by drawing meridians from 20 to 20 seconds, and parallels from 15 to 15, the figures formed by their intersection will be perfect squares; but Mr. Davall shows they are not.

Mr. de Lisle has therefore, by this account, made the superficial content of each rectangle, and consequently of the whole city of Paris too great by near one-seventh. To confirm which beyond contradiction we have Mr. de Lisle's own testimony, who in the plan he himself has drawn and published of Paris, and which he refers to in this very account, has not made squares of the above-mentioned figures, but has given to their respective sides the proportion of 8 to 7, which is as near the true one as can well be expressed by lines, in a plan of no larger a scale than this.

Now in the account, Mr. de Lisle says himself, that in his measuring of London he drew squares, whose sides contained 15 seconds of a great circle, and of these, he says, London contains 60. Therefore to compare Paris with London, we ought to make an abatement out of the 63 rectangles which Paris contains, nearly in the proportion of 8 to 7; but because that is a little greater than the true one, let us make such abatement only in the proportion of 9 to 8, which is pretty considerably less than the just one. By which abatement the number of squares, whose side is 15 seconds of a great circle contained in Paris, will be reduced from 63 to 56. And consequently, according to Mr. de Lisle's own way of measuring, the magnitude of London will be to that of Paris as 60 to 56, or as 15 to 14; or London will be one-fourteenth larger than Paris.

An Aneurism of the Aorta, dissected in St. Bartholomew's Hospital. By Pierce Dod, M. D. N° 402, p. 436.

The patient, of whom an account is here given, was about 34 years of age, and of a good constitution; but there was a tumour, larger than one's fist, which began from the upper part of the sternum, between the origins of the musculi mastoidæi, and extended itself to the pomum adami, almost up to her chin, and possessed all the breadth between the two carotid arteries.

The account she gave of the occasion of it was, that her husband, being a passionate man, took her by the throat one day as she was crying out upon some occasion or other, and griped her so hard as almost to throttle her. She was then with child, and immediately perceived something of a pain a little above her heart, and a few days afterwards a tumour appeared about the size of the top of her finger, just above the sternum, and so continued without increase or pulsation, till she was brought to bed, when it began to be enlarged, on her having a hard labour; agreeably to what practitioners have observed, that accidents of this nature often happen to women in labour.

This was about 4 years since, and from that time it had continued gradually increasing, till it was arrived to almost the highest pitch of extension; and she had all along been troubled with a palpitation, pain, and straitness within the thorax, great interruptions in her rest, and frequent sinkings, together with a constant beating along the chest up to the tumour; in which likewise there was a pulsation correspondent to the regular pulse, shaking the tumour at every stroke, and manifest to the eye as well as the touch. Yet she was otherwise hearty and healthy.

The apex of the tumour, which was towards the middle, in the prominent part of it, was beginning to mortify, through an over distension, and the common outward integuments were the first that seemed to suffer; but the distension continuing, the mortification increased, and was quickly communicated to the outer coat of the artery likewise, which therefore sloughed off, as well as the other integuments, and being at length worn away, just at the extremity made a sudden aperture, about twice the size of a goose quill. The blood instantly gushed forth, as from a stream or torrent, and the patient died in less than a minute.

On opening the body, in the heart there was little remarkable, except that the left ventricle was somewhat larger, as were likewise the columnæ carneæ, than they naturally should be. There was little observable likewise in the aorta itself, till they came to the curvature, on the upper side of which was the basis of the tumour, forming a cylindrical stem of 4 inches long while in the cavity of the thorax; but extending itself into a circular form of a larger dimension, when it became external. On opening the under part of the aorta opposite to this basis, and carrying the incision throughout its whole extent in the thorax, the trunk retained its usual form and dimensions, and was not at all dilated; but in the upper part above described, just on this side the orifice of the right subclavian artery, which was nearer than usual to the orifice of the left carotid, there was a preternatural circular aperture of half an inch diameter; on dividing this aperture, and carrying on the incision to the apex of the tumour, its whole internal substance appeared. The edges of the aperture at the basis of the tumour were hard, and almost cartilaginous, and seemingly the remains of thick and fleshy fibres; which on a nicer inspection they appeared to be in fact, viz. the broken fibres of the inner, or what is commonly called, the muscular coat of the artery; which terminating here, the tumour immediately increased to 2 inches in diameter, and continued of that dimension, till it came out at the neck, between the clavicles; but then extended itself circularly to a diameter of above 3 inches, the covering of which was nothing else but the outer coat of the same artery all along dilated from the base, even to the extremity of the tumour.

The cavity was for the most part filled with a sort of polypus, or sarcoma; in which nevertheless there were three sinuses, or passages, that were kept open by the constant influx of the blood, and communicated near the apex with each other; that in the middle being the largest, and terminating in one towards the extremity of the tumour, not far from where it broke.

Observations on Aneurisms in general, and on the foregoing in particular. By F. Nicholls,[*] *M. B. Reader on Anatomy at Oxford, and F.R.S.* N° 402, p. 440.

An aneurism is by all authors defined to be a soft circumscribed tumour, in

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[*] Dr. Frank Nicholls was born in London 1699. His father was a counsellor. After receiving his grammatical instruction at Westminster, he was sent to Oxford, where he cultivated with great diligence general philosophy, as well as anatomy and physiology; and was at length appointed reader on anatomy in that university. In this situation he combined, (an object which had been too much neglected by preceding lecturers) physiology with anatomy; for he was not content with a mere exposition of the parts composing the human machine, but threw a new interest upon the subject by making the demonstration of the corporeal structure subservient to the elucidation of the various functions in the animal economy. To quote the words of his elegant biographer, Anatomen 'αλογον et nudam vix inter artes ingenuas numerandam censuit; vix dignam, quæ discipulos studiis liberalibus occupatos morari posset, nisi cum physiologia esset conjuncta. Here it is said that he showed the lymphatic vessels, and explained their uses in the business of absorption, many years before the claims to this discovery were brought into dispute by other anatomists.

When he accepted the appointment of anatomical reader at Oxford, it was his intention to deliver a course of lectures on anatomy and physiology once a year; but not to reside wholly in the university. Accordingly he used to spend part of the year in London, where he resolved some time or other to settle. Yet he once had thoughts of residing in Cornwall, where his father was born, and actually went there with that view; but he soon became disgusted with the sacrifice of time and exposure to fatigue from visiting patients lying wide of one another, to which those who engage in country practice are obliged to submit. Nevertheless, during his short stay in Cornwall, he had an opportunity of making observations on the miliary fever, which was then epidemic in that part of the kingdom; and these observations he some years afterwards laid before the public. It was about this period that he visited the continent, travelling into France and Italy. On his return he established himself in London, where he began a course of physiological lectures, which, as might be expected, were attended by a great number of pupils. In 1729 he took his degree of M. D. at Oxford. He was afterwards admitted a fellow of the College of Physicians, and in 1734 he read the Gulstonian lecture, choosing for his subject, the structure and action of the heart, and the circulation of the blood; this lecture he published some years afterwards with considerable additions. It is a most finished performance; and while it exhibits a comprehensive view of Harvey's discoveries, it contains many new and original reflections relative to the structure and functions of the heart, and the distribution and uses of the which there is a sensible pulsation, cotemporary with the pulsation of the artery, to which it adheres. As it is certain, that any tumour of what kind soever, lying on, or adhering to any considerable artery, must necessarily be moved by every pulsation of such artery, so this pulsation can nowise be admitted as the true diagnostic by which to specify the difference between this kind of tumour and any other.

An aneurism is found most commonly to succeed falls, vomitings, labour-strains, and such other motions or indispositions of the body as, by compressing the great branches of an artery, any ways stop the progressive motion of the blood. It is obvious that, as the section of the artery above the compressure must, in its natural state, be sometimes very incapable of containing at once the whole quantity of blood, which ought only to have passed through it successively; and as the force of the heart may frequently exceed the resistance it may meet with from the coats of the artery; so the consequence of such a stop to the progressive motion of the blood, may occasion either a rupture of the

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sanguineous fluid. Beside this, Dr. N. read at different years two other Gulstonian lectures, one relating to the structure and function of the urinary organs, the other to the causes, symptoms, and treatment of calculous affections. In 1736 appeared his Compendium Anatomico Œconomicum, 4to. In 1739 he delivered before the college the Harveian oration. In 1748 he was chosen reader on surgery to the college. This appointment gave rise to his treatise De Anima Medicâ, which is to be considered rather in the light of a physiological and pathological, than of a metaphysical performance. For so much and such valuable information which Dr. N. had communicated to the college and the world, it was to be expected that no honours would have been thought too great for him by his colleagues; yet, (such is too often the triumph of envy and intrigue over talents and worth,) on the death of one of the elects, a younger physician from among the fellows than himself, much inferior to him, not only in general science, but in every branch of professional knowledge, was chosen to fill up the vacancy. Disgusted at this conduct, he resigned his office of lecturer on surgery, and seldom afterwards attended the meetings of the college. On the death of Sir Hans Sloane in 1753, he had the honour of being appointed physician to George II. and when that monarch died, he was present at the examination of his body, and drew up an account thereof. This account is inserted in the 52d vol. of the Phil. Trans. from which it appears that the immediate cause of that sovereign's death was a rupture of the heart. After this Dr. N. grew weary of practice, and retired to Epsom, where he amused himself with agricultural and botanical pursuits. Although he was always considered to be of a delicate constitution, and was subject to pulmonary complaints, yet the world was not deprived of this ornament of the medical profession until 1778, by which time he had attained his 80th year.

Dr. N.'s various communications to the Royal Society are inserted in the 35th, 36th, 37th, 49th and 52d vols. of the Phil. Trans. His life has been written in elegant latinity by Dr. Lawrence,* who has given a full and connected view of Dr. N.'s writings, and done ample justice to his various merits in philosophy, anatomy, physiology, and medicine. From that life the above memoir has been abstracted.

* Franci Nichollsii M. D. Vita; cum Conjecturis ejusdem de Nat. et Usu Partium Hum. Corp. similar. Scriptore Th. Lawrence M. D. 4to. 1780.

artery, or a distension of it without a rupture, or a rupture of the internal coats of the artery, and a distension of its external coat.

A rupture of the large branches of the aorta necessarily allows so plentiful effusions of the blood, as to occasion immediate death; while the capillaries may be burst without any other injury, but a slight ecchymosis, and the tumour formed by the effusion from them will be diffused and superficial.

A rupture of the mean branches, or such as descend between the tibia and fibula, the radius and ulna, &c. will be attended with a considerable effusion of blood; but as the blood will find a passage between the interstices of the muscles, it will never form a circumscribed tumour. However, the effusion being continued per saltum, through the ruptured artery, will give a faint pulsation, and consequently some resemblance of the aneurism; for which reason it is by some surgeons termed a bastard aneurism.

Whether an aneurism be a tumour formed by the dilatation of the artery, or by a rupture of the internal coats of the artery, and a distension of the external, has for some time been a matter of great dispute; each party protesting, perhaps too unjustly, against the possibility of the other's opinion. As to the possibility of an artery's being dilated, it stands supported by reason and autopsy. We find the uterine arteries constantly increased in thickness and diameter, in proportion as the uterus is distended; and many cases of palpitations of the heart have been attended with great dilatations of the aorta; instances of which have occurred both in human and brute subjects. Such a dilatation will necessarily follow a constant, or frequent pressure on any part of the aorta, provided such pressure does not entirely stop the progressive motion of the blood through the aorta.

But on the other hand, such a dilatation will always retain somewhat of the form of the artery. The resistance will not be every way equal, as in the extravasate tumours; because the quaquaversal pressure of the blood will be controlled by the pressure on the artery, and the resistance from the coats of the arteries, so as necessarily to form a cylindroid. And the consequence of such a dilatation cannot, if considered abstractedly from its pressures, be worse than from a varicose vein, if so bad.

Again, those who conceive an aneurism to be a rupture of both coats of the artery, oppose their opinion, who imagine the internal coat to be ruptured, and the external to be distended, by comparing the two coats in question, and urging, that as the internal coat is so much thicker than the external, it seems impossible the last should be sufficient to resist a force capable of destroying the first. Were these two coats similar as to their structure, we might then compute their strength by their thickness, and this argument would be of much greater force than at present it can be; because the internal coat, being composed of annular fasciculi, whose sides have but a very weak cohesion, their power of resisting will not be measurable by the strength of those annuli; but by the force with which they adhere laterally. And on the other hand, the external coat, being composed of fibres equally interwoven, and of a quite different composition, it may either exert a greater resistance, or be capable of much greater dilatations than the internal.

But that autopsy may evince the truth of this difference in the strength of these coats, it will be found by any one who pleases to try the experiment, that by blowing into the pulmonary artery, the internal coat will soon burst, and the external form itself into aneurismous tumours, as was proved by experiments.

On considering all which, and having, by order of the Society, both privately and publicly examined the aneurism before them, which Mr. N. finds to be round like other extravasate tumours, unless when controlled by any notable pressure, and that the sacculus does not divide into coats, as the artery from whence it arises does; he is induced to think that this aneurism is a tumour formed by the blood being forced through the ligamentous, or what is called the muscular coat, and distending the membranous or outer one. And because the impetus of the blood will, as it were, perpetually press through the aperture into the tumour, and be again in part returned by the elasticity of the external coat; therefore such a tumour will rather have a pulsatile dilatation, than a pulsation, for its true diagnostic.

Of a surprising Shoal of Pumice-Stones found floating on the Sea. By Mr. John Dove. N° 402, p. 444.

On the 22d of March, 172$\frac{4}{5}$, at noon, being in the latitude 35° 36′ south, and longitude 4° 9′ west, with variation 3° 16′ w. they discovered several pumice-stones on the sea; but not expecting any such thing at that distance from the land, the islands Tristan d'Acunha being the nearest, which were judged to bear w. 9° 10′ s. distance 186 leagues, they disputed what it might be; when about 1 P. M. they took up a piece in a bucket, which confirmed Mr. Dove's opinion of its being pumice-stones. Towards night it was spread all round, as far as could be seen. Next morning the pumice-stones were very thick, in drifts, lying N. N. E. and S. S. W. and extended out of sight from the mast's head, increasing as they ran to the eastward.

Wednesday the 24th they continued their course E. S. E. 140 miles, the pumice-stones being thicker; so that for 16 hours some of the drifts were about a cable's length broad, and so thick, they could scarcely see the water between them; and there was much the same breadth between the drifts, with several pumice-stones interspersed. Towards noon, they found the pumice somewhat thinner: latitude 37° 35′ s. and longitude 1° 4′ w.

Thursday the 25th, in the evening, the drifts were near as large as above, but towards next morning they decreased much; so that about noon they were clear of the pumice-stones, several of which were as large as a man's head. They sailed 317 miles since they first discovered them. They lay just in the track for ships outward bound; and all the ships that went out the same year, and since, who go so far to the southward, have fallen in with them. In the morning they tried the current, but found none: and no ground at 130 fathoms. At noon, latitude 37° 54′ s. longitude 0° 38′ E. they judged Tristan d'Acunha bore w. 3° 39′ N. Distance 256 leagues, supposing it to lie in latitude 37° 5′ s. and longitude 15° 38′ w.

Observations made by a young Gentleman, who was born blind, or lost his Sight so early, that he had no Remembrance of ever having seen, and was couched between 13 *and* 14 *Years of Age. By Mr. William Chesselden, F.R.S.* N° 402, p. 447.

Though we say of the gentleman that he was blind, as we do of all people who have ripe cataracts, yet they are never so blind from that cause, but that they can discern day from night; and for the most part in a strong light, distinguish black, white, and scarlet; but they cannot perceive the shape of any thing; for the light by which these perceptions are made, being let in obliquely through the aqueous humour, or the anterior surface of the crystalline, by which the rays cannot be brought into a focus upon the retina, they can discern in no other manner, than a sound eye can through a glass of broken jelly, where a great variety of surfaces so differently refract the light, that the several distinct pencils of rays cannot be collected by the eye into their proper foci; therefore the shape of an object in such a case, cannot be at all discerned, though the colour may. And thus it was with this young gentleman, who, though he knew these colours asunder in a good light, yet when he saw them after he was couched, the faint ideas he had of them before, were not sufficient for him to know them by afterwards; and therefore he did not think them the same, which he had before known by those names. Now scarlet he thought the most beautiful of all colours, and of others the most gay were the most pleasing; whereas the first time he saw black, it gave him great uneasiness, yet after a little time he was reconciled to it; but some months after, seeing by accident a negro woman, he was struck with great horror at the sight.

When he first saw, he was so far from making any judgment about distances, that he thought all objects whatever touched his eyes, as he expressed it, as what he felt, did his skin; and thought no objects so agreeable as those which were smooth and regular, though he could form no judgment of their shape, or guess what it was in any object that was pleasing to him. He knew not the shape of any thing, nor any one thing from another, however different in shape, or magnitude; but on being told what things were, whose form he before knew from feeling, he would carefully observe, that he might know them again; but having too many objects to learn at once, he forgot many of them; and, as he said, at first he learned to know, and again forgot a thousand things in a day. One particular only, though it may appear trifling, Mr. C. relates: having often forgot which was the cat, and which the dog, he was ashamed to ask; but catching the cat, which he knew by feeling, he was observed to look at her stedfastly, and then setting her down, said, so puss! I shall know you another time. He was very much surprised, that those things which he had liked best, did not appear most agreeable to his eyes, expecting those persons would appear most beautiful that he loved most, and such things to be most agreeable to his sight that were so to his taste. They thought he soon knew what pictures represented, which were showed to him, but they found afterwards they were mistaken: for about 2 months after he was couched, he discovered at once, they represented solid bodies; when to that time he considered them only as party-coloured planes, or surfaces diversified with variety of paint; but even then he was no less surprised, expecting the pictures would feel like the things they represented, and was amazed when he found those parts, which by their light and shadow appeared now round and uneven, felt only flat like the rest; and asked which was the lying sense, feeling, or seeing?

Being shown his father's picture in a locket at his mother's watch, and told what it was, he acknowledged a likeness, but was vastly surprised; asking, how it could be, that a large face could be expressed in so little room, saying, it should have seemed as impossible to him, as to put a bushel of any thing into a pint.

At first, he could bear but very little sight, and the things he saw, he thought extremely large; but on seeing things larger, those first seen he conceived less, never being able to imagine any lines beyond the bounds he saw; the room he was in he said, he knew to be but part of the house, yet he could not conceive that the whole house could look larger. Before he was couched, he expected little advantage from seeing, worth undergoing an operation for, except reading and writing; for he said, he thought he could have no more pleasure in walking abroad than he had in the garden, which he could do safely and readily. And even blindness he observed, had this advantage, that he could go any where in the dark much better than those who can see; and after he had seen, he did not soon lose this quality, nor desire a light to go about the house in the night. He said, every new object was a new delight, and the pleasure was so great, that he wanted ways to express it; but his gratitude to his operator he could not conceal, never seeing him for some time without tears of joy in his eyes, and other marks of affection: and if he did not happen to come at any time when he was expected, he would be so grieved, that he could not forbear crying at his disappointment. A year after first seeing, being carried upon Epsom Downs, and observing a large prospect, he was exceedingly delighted with it, and called it a new kind of seeing. And now being lately couched of his other eye, he says, that objects at first appeared large to this eye, but not so large as they did at first to the other; and looking on the same object with both eyes, he thought it looked about twice as large as with the first couched eye only, but not double, that they could any ways discover.

An Explanation of the Instruments used, in a new Operation on the Eyes. By the same, N° 402, p. 451.

A, B, fig. 2, 3, pl. 5, represent two eyes, on which a new operation was performed, by making an incision through the iris, which had contracted itself in both cases so close, as to leave no pupil open for the admission of light.

The perforation in the eye A was made a little above the pupil, the closing of which ensued on the putting down a cataract, which not knowing how low it might be lodged, Mr. C. made the incision a little higher than the middle, lest any part of it should lie in the way.

The eye B was one that was couched not long before, where the patient had been blind but a few years. At first he thought every object further from him than it was; but he soon learned to judge the true distance, the cause of which Mr. C. explains by fig. 4, in which let the circle ABC represent the eye, A the place where an image through the natural pupil B was represented from the place E. Now the artificial pupil being at the place C, the object at D is now painted at the place A, where the object E was also to be perceived; therefore it was, he supposes, that the patient mistook the place D for the place E.

Fig. 5 represents a kind of needle with an edge on one side, which being passed through the tunica sclerotis, is then brought forwards through the iris a a little farther than F. This done, he turns the edge of the needle, and cuts through the iris as he draws it out. The handle of this needle is half black and half white; which, though it is not of much use in this operation, is very much so in couching needles; we being thus able to judge of their position, when we do not see them.

Fig. 6 represents an instrument to keep open the eye-lids. G is a bit of iron; which, as it is moved backward or forward, the instrument opens or closes.

Of several Stones found in the Kidneys of a Person, opened by Mr. John Dobyns, Surgeon and Lithotomist to St. Bartholomew's Hospital and F. R. S. N° 402, p. 452.

Mr. Laurence, a gentleman about 40 years of age, had for near 20 years a complaint in his kidneys; making bloody urine on any extraordinary motion, but free from the great pain, and all other symptoms usually attending nephritic cases. However, on opening the kidneys after his death, there was in each a stone of an extraordinary size and figure, besides 100 smaller.

Fig. 7, pl. 5, shows one of them denudated, as taken from the pelvis of the right kidney: a is that part which had branched into the ureter, and totally obstructed its channel; bbb the eminent parts of it; cc that part which filled the capacity of the pelvis.

Fig. 8 represents the stone taken from the left kidney: a is that part which had protruded itself into the upper part of the ureter, but did not totally plug it up, by which means the urine had a passage; bbbb the eminent parts, which branched into the fistulæ membranaceæ; cccc the body of the stone, which lay in the pelvis of this kidney.

On the Aurora Borealis seen Oct. 19, N. S. 1726. *By several Persons.* N° 402, p. 453.

Mons. Gaudin, in a letter from the Observatory at Paris, dated Oct. 20, N. S. 1726, writes, that he saw it first at half past 7 in the evening, forming at that time a luminous arch, with another somewhat darker under it, which extended itself almost from sun-set to moon rise, and was raised above the horizon about 25 degrees; from whence shot out from time to time luminous streams about 10 degrees above it. At half past 8, the number of these streams vastly increased, covering all the heaven, excepting the height of 20 degrees opposite to it: but towards the zenith there remained a circular space which was never covered by them, though there wanted not a constant succession. These appearances continued very strong till half after 10; when they began to decline, and entirely disappeared about 2 in the morning.

Mons. Maraldi, in a letter dated at Thiers, Oct. 20, 1726, N. S. two leagues to the south of Paris, says, it began there about half past 6, with a constant uniform light in the north; soon after which appeared three or four luminous arches one over another, from whence issued a great number of rays, which shot up a considerable height above the horizon. At 8 o'clock these rays darted quite up to the zenith; half an hour after which they very much increased, spreading with strong undulations all over the sky, and all terminating in the zenith formed a sort of cupola there.

Sign. Francesco Quaranbotti writes from Treggiaia, Oct. 20, 1726, N. S. that he first observed it a little before 8 in the evening, when it extended itself along the north horizon about 80 degrees, and reached above it about 8. After some time, the luminous emissions began to rise perpendicularly, and continued from time to time so to do, from 9 till 11. About 10 it enlarged to 15 degrees farther east, and stretched under the last star in Ursa Major. At 11 it vanished.

An anonymous account in Latin, from Florence, informs us, that it was first seen there at half past 6 in the evening, with a clear expanded light, occupying all the space between the north-east and north-west. At 7 it divided into several spherical triangles near the horizon, which half an hour afterwards united into one large one, whose base was near the horizon, and extended 20 degrees to the west from the north-pole, and whose vertex reached up to Ursa Minor. This continued about half an hour, and then disappeared; but at 10 o'clock it returned much more conspicuously, forming about the pole, a large column which was raised 30 degrees above the horizon. From this time it sent out lucid undulations till midnight, when it entirely dispersed. He afterwards takes notice that the same was seen at Milan and Bologna; the accounts from whence agree, that none of the streams reached beyond the zenith.

Sign. Manfredi writes from Bologna, Jan. 3, 1726-7, that he did not observe this phenomenon himself, but was informed that it was seen every where in the Campagna di Roma, as far as Pesaro and Fano.

From Sweden however it is stated, that though this meteor was seen in Germany, Poland, Switzerland, France, and England, yet at Upsal they could observe nothing but the whole sky beset thick with clouds, of a colour like that of the moon in a total eclipse, and variously agitated as by a wind, but this chiefly towards the south; which continued till 9 at night, a little after which it became quite cloudy.

An Account of Elephants Teeth and Bones, found under Ground. By Sir Hans Sloane, Bart. N° 403, p. 457.

It is observable, that among the vast variety of extraneous substances, lodged and found in several layers of the earth, at considerable depths, where it is impossible that they should have been bred, there are not so many productions of the earth, as of the sea. And again, among those which must have originally belonged to the earth, there are many more remains of vegetables, than of land animals. It appears however, by the histories of past times, and the accounts of many, both antient and modern authors, that bones, teeth, and sometimes almost whole skeletons of men and animals have been dug up, in all ages of which we have histories, and almost in all parts of the world, of which the most remarkable for their unusual size have been also the most noticed. Thus, for instance, in Ireland there have been found the horns, bones, and almost entire skeletons of a very large sort of deer, which is commonly believed to have been the moose-deer, an animal of an uncommon size, some of which kind are thought to be still living in some remote and unfrequented parts of the continent of America.

Sir Hans Sloane, in this paper, chiefly confines himself to the elephant, and such bones, dentes exerti, tusks and teeth of this animal, as are either in his own possession, or have been mentioned by other authors, as having been found under ground. And first, as to those fossil teeth in his own collection, which doubtless once belonged to elephants, he adduces the following:

N° 116 of his catalogue of quadrupeds and their parts, is the dens exertus, or tusk of an elephant, which was taken up, 12 feet deep, from among sand, or loam, in digging for gravel at the end of Gray's-Inn-lane, and preserved by tying it about with whale-bones and tape, to keep it from falling to pieces, by Mr. Conyers, an ingenious apothecary, and a great collector of curiosities of all kinds.

As most part of this tooth was fallen to pieces, nothing could be determined about its length, when entire. The largest piece, and also the most entire, is $5\frac{8}{10}$ inches in length, and $9\frac{6}{10}$ inches in circumference, consequently something more than 3 inches in diameter. This piece belonged to the basis, or bottom of the tooth, where it is articulated with the head, as appears by a cavity in form of a cone, which all these tusks have at bottom, and which was filled with the sand of the gravel-pit.

The condition in which this tooth was found suggests the two following remarks. It shows, in the first place, how far subterraneous steams are apt to calcine substances of this kind, which in this tooth was to such a degree, that it was grown extremely brittle, and ready to fall to pieces; and had also acquired an astringent quality common to calcined substances of this kind, which makes them stick pretty close, when held to the tongue. They had altogether the same effect on the very large skeleton, found near Drapani in Sicily, and mentioned by Boccatius, on that remarkable one found near Tonna, described by Tentzelius; as also on two teeth found in Northamptonshire, described next below. However it by no means follows from hence, that all teeth and substances of this kind undergo the like calcination by lying long under ground, for there are others, as those found in Iceland, and sent to Thomas Bartholin, which were turned to a perfect hard, flinty substance.

It serves, in the second place, to ascertain the structure of these teeth, and consequently of ivory in general, to be layer upon layer, or coat upon coat, like the skins in an onion, or rather the annual circles, or rings in trunks of trees. In this piece, belonging to the basis of the tooth, there appeared very visible marks of 9 coats, some of about $\frac{1}{10}$ of an inch in thickness. Towards the further end of the tooth, where it tapers almost to a point, these several coats also join together into two or three, and those pretty considerably thick. With some care these coats might be further sub-divided into a considerable number of other smaller ones, perhaps no thicker than a common parchment. The very manner of its falling to pieces is an evident proof of its structure, all the fragments being concave within, and convex without, and the lines of convexity and concavity, fragments of concentric circles, which the several coats composed, when entire.

Thomas Bartholin, in his Treatise De Unicornu, observes, that part of a fossil unicorn horn having been calcined by order of Christian the 4th, king of Denmark, it was found to be composed, after the same manner, of thin layers upon layers; whence he infers, that it was not the horn of an animal, as was commonly pretended, but a tooth, viz. the tooth of a sort of whale in the northern seas, called Narvhal, as he had afterwards an excellent opportunity to verify by one of these unicorn's horns still sticking in the skull of the creature, which was sent to Wormius by Thorlacus Scutonius, bishop of Iceland. Nor is this structure by any means to be considered as an effect of the calcination, whether brought about by the subterranean steams, or by a chemical trial, but is natural to the tooth, as appears in some measure by a piece of ivory, marked 1181: but still more plain in another marked 731, where several of these coats are by some disease in the tooth actually separated from each other, like the leaves of a parchment book, the ivory on the other side being still firm and close. This strucure appears likewise from the teeth of the very young elephant which died at London, where the uppermost coat, being very moist, cracked on drying, and broke at the top.

N° 750, is part of another dens exertus, which the Rev. Mr. Morton, in his Natural History of Northamptonshire, gives the following account of: "An extraordinary elephant's tooth, one of those which grow out of the upper jaw, and which for their magnitude and length, have by some writers been accounted horns, was lately taken out of the earth by digging in Bowdon-parva Field. Even the native colour of it has been in great measure preserved; but it is become brittle with lying in the earth; and was broken into three or four pieces transversely by the diggers in taking it up. The two larger pieces of it were presented to me. One of them is somewhat above a yard; the other is 2 feet in length; but the whole tooth must needs have been at least 6 feet long; the thickest part of the larger piece is 16 inches round. The tooth lay buried above 5 feet deep in the earth. The strata, from the surface, downwards to the place where the tooth was lodged, were as follows: 1. The soil 13 or 14 inches. 2. Loam, a foot and a half. 3. Large pebbles, with a small mixture of earth among them, 2 feet and a half. 4. Blue clay. In the upper part of this stratum the tooth was found." That part of this tooth, bears again very visible marks, both of the calcination it underwent by lying in the earth, and of its laminated structure.

N° 1185, is the dens exertus, or tusk of an elephant, remarkable for its large size, and for its being so very entire. It was found under ground in Siberia, and was brought from thence by Mr. Bell, an ingenious surgeon. It is very entire, of a brownish colour, and hollow at bottom, like other elephants teeth, one of which it plainly appears to be. From the basis, measuring along the outer circumference to the small end, it is 5 feet 7 inches long, and along the inner circumference 4 feet 10 inches. Measuring from the inside of the basis to the small end in a straight line, the distance is of 3 feet 10½ inches. At the basis, where thickest, it measures 18 inches round, and is therefore 6 inches in diameter: it weighs 42 pounds. The like tusks, and other bones of the elephant, are found in sundry parts of Siberia to a considerable quantity, and the tusks and teeth in particular, when less corrupted, are used all over Russia for ivory. Henricus Wilhelmus Ludolfus, in the Appendix to his Russian Grammar, mentions them among the minerals of Russia, by the name of Mammotovoikost, and says, that the Russians believe them to be the teeth and bones of an animal living under ground, larger than any one of those above ground. They use it in physic, for the same purposes with the unicorn's horn; and Ludolfus himself having been presented with a piece by one of his friends, who said, he had it from a Russian of great quality, lately returned from Siberia, found it to be true ivory. He adds, that the most sensible among the Russians affirm them to be elephants teeth, brought thither at the time of the deluge. The description of these teeth and bones given by E. Ysbrants Ides, in his Travels from Moscow to China, is still more extensive, and so particular, that his whole passage deserves to be transcribed at length.

" Among the hills, says he, to the north-east of Makofskoi, not far from thence, the Mammuth's tongues and legs are found; as they are also particularly on the shores of the rivers Jenize, Trugan, Mongamsea, Lena, and near Jakutskoi, to as far as the frozen sea. In the spring, when the ice of this river breaks, it is driven in such vast quantities, and with such force by the high swollen waters, that it frequently carries very high banks before it, and breaks off the tops of hills, which falling down, discover these animals whole, or their teeth only, almost frozen to the earth, which thaw by degrees. I had a person with me to China, who annually went out in search of these bones: he told me, as a certain truth, that he and his companions found a head of one of these animals, which was discovered by the fall of such a frozen piece of earth. As soon as he opened it, he found the greatest part of the flesh rotten, but it was not without difficulty, that they broke out his teeth, which were placed before his mouth, as those of the elephant are; they also took some bones out of his head, and afterwards came to his fore foot, which they cut off, and carried part of it to the city of Trugan, the circumference of it being as large as that of the waste of an ordinary man. The bones of the head appeared somewhat red, as though they were tinctured with blood. Concerning this animal there are very different reports. The old Siberian Russians affirm, that the Mammuth is very like the elephant, with this only difference, that the teeth of the former are firmer, and not so straight as those of the latter. They also are of opinion, that there were elephants in this country before the deluge, when this climate was warmer, and that their drowned bodies floating on the surface of the water of that flood, were at last washed and forced into subterranean cavities: but that after this Noachian deluge, the air, which was before warm, was changed to cold, and that these bones have lain frozen in the earth ever since, and so are preserved from putrefaction, till they thaw and come to light, which is no very unreasonable conjecture; though it is not absolutely necessary that this climate should have been warmer before the flood, since the carcases of drowned elephants were very likely to float from other places several hundred miles distant, to this country, in the great deluge which covered the surface of the whole earth. Some of these teeth, which doubtless have lain the whole summer on the shore, are entirely black and broken, and can never be restored to their former condition; but those which are found in good case, are as good as ivory, and are accordingly transported to all parts of Muscovy. The abovementioned person also told me, that he once found two teeth in one head, that weighed above 12 Russian pounds, which amounts to 400 German pounds; so that these animals must be of necessity very large, though a great many lesser teeth are found. By all that I could gather from the heathens, there is no person ever saw one of these beasts alive, or can give any account of its shape."

What E. Ysbrant Ides observes of those teeth that are black and broken, may serve as a comment to the following passage of Pliny, lib. xxxvi. c. 18: Theophrastus autor est, et ebur fossile candido et nigro colore inveniri, et ossa è terra nasci, invenirique lapides osseos. Lawrence Lang, in the Journal of his Travels to China, takes notice of these bones, as being found about the river Jenisei, and towards Mangasea, along the banks, and in the hollows occasioned by the fall of the earth. He calls them maman-bones, and informs us, that some of the inhabitants are of opinion, that they are no real bones, teeth, &c. but a sort of cornu fossile, that grows in the earth, and that others will have them to be the bones of the Behemoth, mentioned in the 40th chapter of Job, the description of which they pretend fits the nature of the beast, whose bones and teeth they are imagined to be, those supposed words, in particular, that he is caught with his own eyes, agreeing with the Siberian tradition, that the maman beast dies on coming to light. The same author affirms, from the report, as he says, of credible people, that there have been sometimes found horns, jaw-bones and ribs, with fresh flesh and blood sticking to them. The same is confirmed by John Bernard Muller, in his account of the Ostiacks, who adds, that the horns in particular have been found sometimes all bloody at the broken end, which is generally hollow, and filled with a matter like concreted blood; that they find, together with these teeth, or horns, as he calls them, the skull and jaw-bones, with the grinders still fixed in them, all of a monstrous size; and that he himself, with some of his friends, has seen a grinder weighing more than 24 lb.; that the inhabitants make divers things of these teeth, and that they are mostly to be met with in the coldest places of Siberia, as for instance, Jakutsky, Beresowa, Mangasea, and Ohder.* He likewise gives the description of one of these animals, from the accounts of several persons, who assured him, that they had seen them in the caverns of the high mountains beyond Beresowa: but as this description has very much the appearance of a fable, it is not inserted here. The author of the present state of Russia observes, that some of the Swedish prisoners

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* On this subject the reader is referred to the accounts given by some late travellers into Siberia, and particularly to the accounts given by Gmelin and Pallas.

banished into Siberia, got their livelihood by turning snuff-boxes out of these teeth; and in another place he mentions them among the Siberian commodities, of which the Czar has the monopoly.

The accounts hitherto given of these maman-bones and teeth, or at least their most essential parts, are confirmed by a letter of Basilius Tatischow, director general of the mines in Siberia, and counsellor of the Czar's metallic council, written to the learned Ericus Tenzelius, now bishop of Gothenburg, and printed in the Acta Literaria Sueciæ (M.DCC.XXV. Trimestre Secundum, p. 36.) where he mentions the following pieces he had in his own possession: a large horn, as he calls it, or tooth, weighing 183 pounds, which he had the honour to present to his Czarish majesty, and is now kept in the Czar's collection of curiosities at Petersburg; another large horn, which he presented to the Imperial Academy at Petersburg; another still larger than either of these two, which he caused to be cut, and carved himself several things of it, the ivory being very good; part of the skull, corrupted by having lain in the ground, and so large, that it seemed to him to be of the same size with the skull of a great elephant; the forehead in particular was very thick, and had an excrescence on each side, where the horns are usually fixed; which excrescence however, as the author observes, was so small, as to make him doubtful, whether there was ever any horns fixed to them. The cavity, where the brain was lodged, was exceedingly small in proportion to the bulk of the skull. He had found also a spongy bone, of 18 inches in length, and 3 inches in breadth, fixed to the skull, and of a conical figure, whence he conjectured, that it served to support one of the horns, which is observed also in other animals that bear horns: lastly a grinder, which was 10 inches in length, and 6 in breadth, besides several of the ribs, shank-bones, and other bones found from time to time, which the author forbore mentioning. The same author has taken no small pains to inquire into the true state of those pits and hollows which the pagan inhabitants of Siberia say these animals make, when they walk under ground, and found that they were nothing but caverns, such as are common in other mountainous countries, and are owing to the force of subterranean rivers and cataracts, which at last eat through and undermine the places where they pass, so as to make the ground above them give way and sink in.

Sir Hans adds one observation of Cornelius le Brun, who in his Travels through Russia to the East Indies, tells us, that in the neighbourhood of Veronitz they had found several elephants teeth on the surface of the ground, which no person could tell how they came there, and that the Czar's opinion about them was, that Alexander the Great, when he passed the Tanais, or Don, advanced as far as Kostinka, a small town 8 wersts from thence, and that probably some of his elephants died there, of which those teeth were the remains.

N° 764 of Sir Hans Slone's collection, is one of the grinders of an elephant, which was likewise found in Northamptonshire; which Mr. Morton thus describes. "Northwards, says he, about 50 yards from this place, where the abovementioned dens exertus was found, was also dug up one of the molares, or grinder-teeth of an elephant, perhaps of the same that the tusk belonged to. The grinder whole, or however all the pieces of it I could find (for it was broken into 3 or 4 in taking it up) being put together as they grew, exhibit 13 or 14 parallel lamellæ; each of which extends the whole length, and almost the whole thickness of the tooth; and of these it is chiefly composed. But in a live, or perfect tooth, these lamellæ do not appear so plainly, being in part crusted over with a white osseous crust, or integument, which in this fossil tooth is almost wholly perished and gone, so that the lamellæ are more exposed to view. From the root to the top in the longest part, which is near the middle, it is just 7 inches long. Its thickness in the thickest part of the root, which is also near the middle, is near 3 inches, and it is a little above 8 inches broad: measuring it this way, we take in the whole pile of the lamellæ. None of the lamellæ are contiguous; there interposes between them a thinner plate of a whiter colour, and a laxer texture. Three or four of the outmost at one end of the pile, appear undulated at the top of the tooth, are near as broad at top as at the root, and have a blunt ending. The rest of them are gradually contracted to a point, and also bend a little over each other. And each of them, as it approaches the top, divides, as it were, into several smaller teeth; and with these the lamellæ of this figure terminate. The above-described tooth was lodged at almost 12 feet depth in earth. Above it were the following strata: 1. The top earth, a blackish, clayey soil, about 16 inches. 2. Sandy clay intermixed with pebbles, 5 feet. 3. A blackish sand, with small white stones in it, 1 foot. 4. A loamy, softer sort of gravel, 1 foot. 5. A sharper gravel, about 2 feet. The tooth was found a foot and a half deep in this stratum of gravel. Below this 5th stratum there was a blue clay." It is very visible, that this grinder also, by lying in the earth, has undergone the same alteration as the tusk above described, found in Bowdon-parva Field.

N° 119 and 120, of Sir Hans Sloane's catalogue, are two pieces of another large grinder, very probably of an elephant too, turned to a very hard, stony, and almost metallic substance.

N° 121 is a piece of the molaris, or grinder of an elephant, where the undulated lamellæ are set very close to each other.

N° 122 is a piece of another grinder, perhaps of an elephant. It has very evident marks of being fossil, as well as the preceding, and is farther remarkable, as a petrifying substance being got between the lamellæ has very considerably separated and divided them from each other, in such a manner, that they appear to have been set very loose.

N° 427, of his collection of quadrupeds and their parts, is part of an elephant's skull, which was found at Gloucester after the year 1630, with some large teeth, some 5, others 7 inches in compass, according to a short inscription written on this very piece.

A Solar Eclipse, and other Astronomical Observations, near Lisbon. N° 403, p. 471. *From the Latin.*

Sept. 15, 1727, N. S. in the morning, Father Carbone observed this eclipse at a place more westerly than St. Anthony's college by 4 seconds of time, and its lat. 38° 42′ 58.″ When the sun rose, about 4 digits were eclipsed. The greatest observation was at $6^h\ 13^m\ 29^s$, when 8 dig. 1′ 48″ were eclipsed; and the eclipse ended at $7^h\ 9^m\ 2^s$, true time.

Oct. 15, with a 22-foot telescope, F. Carbone observed an immersion of Jupiter's nearest satellite, at $9^h\ 10^m\ 54^s$. And Nov. 7, he observed the same at $9^h\ 25^m\ 45^s$.

The same eclipse, of Sept. 15, was observed at Rome, at the foot of the Quirinal hill, as follows:

At $7^h\ 0^m\ 0^s$ true time after midnight, the eclipse had been begun.
7 46 0 greatest obscuration, about 6¼ digits.
8 44 10 the end of the eclipse.

A Solar Eclipse observed Sept. 14, 1727, N. S. *in the Observatory of Bononia. By Sig. Eust. Manfredi.*[*] N° 403, p. 477. *From the Latin.*

The beginning of the eclipse could not be seen for clouds.

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[*] Eustachio Manfredi, a celebrated astronomer and mathematician, was born at Bologna in 1674. His genius and actions were always above his years. Hence he wrote ingenious verses while he was yet a child; and hence while very young he formed in his father's house an academy of youth of his own age, which grew up to be the academy of sciences, or the celebrated institute of Bologna, which still exists there. He became professor of mathematics at Bologna in 1698, and superintendant of the waters there in 1704: also in the same year he was placed at the head of the college of Montalte, founded at Bologna for young men designed for the church. And in 1711 he was appointed to the office of Astronomer to the institute of Bologna. He became member of the Academy of Sciences of Paris in 1726, and of the Royal Society of London in 1729; and he died in 1739. The works of Manfredi are chiefly the following:

1. De Construc. Equat. Differentialium, 4to. Bonon. 1707.
2. Ephemerides Motuum Cœlestium ab anno 1715 ad annum 1750; 4 vols. in 4to. In com-

At 18h 55m 48s the eclipse had sensibly been begun.
19 40 47 5 digits were eclipsed.
20 36 6 the end of the eclipse, very exact.

At the time of this eclipse, many maculæ appeared on the sun's disk.

The same Solar Eclipse, observed at Padua. By Sig. Poleni. N° 403, p. 479. *From the Latin.*

Clouds prevented seeing the beginning.

At 19h 3m 45s true time, eclipsed 0 dig. 10′.
19 41 27 there were eclipsed 4 dig. 30′.
20 38 42 the end of the eclipse.

Some further Observations towards composing a Natural History of Mines and Metals. By Dr. Nicholls, Præl. Anat. Oxon, and R. S. S. N° 403, p. 480.

Of Iron.—Of all the substances concurring to form the terrestrial globe, iron probably bears the greatest share; as it not only abounds in most kinds of stone, showing itself in varieties of crocus, all which gain a more intense colour by fire; but also enters greatly into the composition of common clay; as may be judged from the similitude of colour between clay and dry iron ore; from the easy vitrification of clay; from the resemblance between clay so vitrified and the clinkers of iron; from its deep red colour after calcination; and lastly, from its yielding pure iron, by being burned with oil.

But while iron is thus entangled with other bodies, it rarely employs the care of the miner; who finds the expence of reducing it to metal too seldom balanced by the price it yields: for which reason, though we frequently meet with large and rich loads of iron, yet (the woods having been applied to more advantageous uses) they are there entirely neglected.

When most pure, the ore is found under 3 different appearances. 1. A rich dry ore, whose scrapings exactly resemble an alkohol martis: this kind of iron ore has very nearly the colour of common clay. 2. A rich iron ore, with part of the wall of the load formed by a concretion of yellow crystals. In this stone the iron radiates from points forming segments of spheres, and where

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posing which work it is said his two sisters were greatly assisting to him. The first volume is an excellent introduction to astronomy; and the other three contain numerous calculations.

3. De Transitu Mercurii per Solem, anno 1723; in 4to, 1724.

4. De Annuis Inerrantium Stellarum Aberrationibus: in 4to, 1729.—Besides these he has a number of papers in the Memoirs of the Academy of Sciences; in several volumes of the Philosophical Transactions, and elsewhere.

these spheres leave any interstices, is found a crocus, or ochre. 3. A stone of iron of the kind used for burnishing plate; it is of the species of the hæmatites. Both these last stones scrape into a deep crocus. And from the 2d instance we may conjecture, that the yellow colour in crystals arises from a crocus entangled with the stony salts.

Though the want of wood in Cornwall deprives it of the advantages it might otherwise reap from iron as a metal, we shall nevertheless find it far from being a useless ore, when we consider it as sometimes impregnating the waters with vitriolic salts, thus making them a proper menstruum for dissolving the disseminated particles of metals; sometimes destroying the sulphureous menstrua, which, though they dissolve the disseminated metals, do yet obstruct their new concretions; and sometimes as being itself the magnet by which the metallic particles are attracted into new concretions.

Of Tin.—The next metallic substance found in Cornwall, and from which these islands are supposed to take their name, is tin. It is never found but as an ore; whereas gold is never found but as a metal, at least its ore is unknown, and all other metals are found sometimes as a metal, and sometimes as an ore.

Tin always shoots into crystals, which are of different magnitudes, from 2 oz. in a single crystal, to such as escape our sight. These crystals are for the most part interspersed in loads of other substances. As, 1. Tin crystals interspersed in a load of a kind of clay, in which is observable a considerable quantity of red-ochre. 2. A kind of hard iron-stone, in which are exceeding small crystals of tin. 3. Somewhat larger crystals, interspersed in a dry red-ochre. 4. Tin crystals, interspersed with spar-stone and a sort of marl. 5. Larger crystals, interspersed in a kind of clay and red-ochre, as in N° 1.

When 100 sacks of the load, each containing more than a Winchester bushel, yield one gallon of clean ore, the load is esteemed very well worth working. Sometimes these crystals are so collected into one mass, as to form loads of pure tin ore, and so large as to yield to the value of 100l. every 24 hours. As, 1. Stones of such pure loads, in which the one is black, and the other nearly white.

These crystals concrete sometimes into the form of a parallelopepidon, whose summit is covered by a pyramid; sometimes the angles formed by the sides of the pyramid, and sometimes the summit of the pyramid are as it were planed away. As, 2. A whole crystal, which has none of its angles off; as represented fig. 11, pl. 4. Also a crystal which has only two of its angles planed away; as fig. 12. And a crystal which has all its angles planed away; as fig. 13. Again, a crystal which has all its angles and its summit planed away; as fig. 14.

Sometimes the crystals represent two equal pentelateral pyramids joined at their base. But, under whatever form these crystals shoot, they always carry an exceedingly fine surface; which, when rubbed off, cannot be renewed by any art. In fig. 14, one side of the parallelopepidon is rubbed away, to show its appearance after losing its natural surface.

These crystals are of different colours, from the white, like white sugar candied, to the deep black. The white crystals seem to carry a finer lustre than any other, and are perfectly transparent; so that were they found of equal size with the black crystals, and of a white water, their hardness and weight, in both which they exceed any other fossil, would probably make them preferable to the diamond. However, as the deeper colours of these crystals seem to arise from a greater proportion of iron in their composition, which they throw off in an iron slag on fusion, and which changes by proper degrees of heat into a crocus, thereby changing the colour of the crystal to a brighter red; so the white tin ore is certainly to be esteemed both richest and best, as most free from iron.

These crystals seem to be the heaviest bodies the earth produces, except quicksilver and real metals. Their specific gravity, is to water, as $90\frac{1}{2}$ to 10; to rock crystal in water, as $90\frac{1}{2}$ to 26; to diamond, as $90\frac{1}{2}$ to 34; and to pure malleable tin, as found by repeated trials, as $90\frac{1}{2}$ to 78; from whence appears the possibility of what some miners affirm, viz. That a cubic inch of some tin ores will yield more than a cubic inch of metal.

Having already remarked that the crystals of tin are sometimes so small as to escape the eye, and so disseminated in the load, as not to make above the 800th or 1000th part of the load, one would naturally imagine it an endless labour to cleanse the ore from such a vast disproportion of rubbish. But the great specific gravity of these crystals renders the cleaning of it less troublesome, and less expensive, than in any other ore whatever. It requires no more, than that the whole stuff be stamped to a fine powder, after which it is washed with a water, whose force is so moderated as to wash away only the lightest parts. This stamping and washing is repeated till the ore is left exceedingly clean, and yields in metal from $\frac{18}{20}$ to $\frac{10}{20}$th, according as it is cleansed from the load, and as it is in its own nature more or less free from iron.

A Method of raising some Exotic Seeds, which have been judged almost impossible to be raised in England. By Mr. Philip Miller, Gardener to the Physic Garden at Chelsea.* N° 403, p. 485.

Mr. Miller here gives an account of the methods he has taken to raise the

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* Phillip Miller, author of the Gardener's Dictionary, was born in 1691. His father was gardener

cocoa-nut, with the success of each; which has led him to a sure method for raising such seeds as have hard coats, or shells, surrounding them; and have been judged very difficult, if not impossible to be raised in England.

In the year 1724, Mr. Miller had a parcel of fresh cocoa-nuts given him, which were brought over from Barbadoes: from part of these nuts he stripped off their outer coat, or husk: and the other part he left entire as he received them. He planted both these parcels in large pots, filled with good fresh earth, and plunged the pots into a hot-bed made with tanners-bark; giving them gentle and frequent waterings, as the earth in the pots seemed to require; but had not one, out of the whole number, which made any attempt to shoot, as he could perceive; and on taking them out of the pots, he found they were rotten.

About 4 months after, Mr. M. received another fresh parcel of cocoa-nuts from Barbadoes, which he treated in another manner: from part of these he cut off the outer coat or husk, and the other part he left entire as before: but supposing it was owing to planting the other parcel in pots, that they did not succeed, he made a fresh hot-bed with horse-dung, and covered it over with fresh earth, about 18 inches thick, in which he planted the nuts: observing as before, to supply it with convenient moisture, as also to keep the hot-bed in an equal temperature, by a thermometer graduated for the use of hot-beds: but with all his care he had no better success than before; not one of the nuts making the least shooting.

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at Chelsea to the Company of Apothecaries, in which place his son succeeded him. He raised himself, by his merit, to a degree of eminence but rarely attained by a gardener. It is not uncommon to give the name of botanist to any man who can recite by memory the plants in his garden. Miller was above this class. To the knowledge of the theory and practice of gardening, he added that of the structure and characters of plants, and was early and practically versed in the methods of Ray and Tournefort. Habituated to the use of these from his youth, it was not without reluctance that he embraced the system of Linnæus, but was persuaded at length by the arguments of Sir William Watson and Mr. Hudson. To his superior skill the curious owe the culture and preservation of many fine plants, which, in less able hands, would have failed at that time to adorn the conservatories of England. His attention was not confined to exotics; few have been ever more acquainted with our indigenous plants, the most rare species of which he cultivated with success. He was admitted not only a fellow of the Royal Society of England, but also of the Botanical Society at Florence: he had an extensive correspondence in foreign countries, and was sometimes by foreigners stiled Hortulanorum Princeps. Of his dictionary Linnæus has said, "Non erit Lexicon Hortulanorum, sed Botanicorum." He has also stiled Miller, "Hortulanus omnium Doctissimus." Mr. Miller, a short time before his decease, resigned, on account of his increasing infirmities, his place at Chelsea, and died Dec. 18, 1771, in the 80th year of his age. His capital work, which, as is well known, has passed through a great many editions, (the last of which by professor Martyn of Cambridge, is unrivalled in its kind) is his Gardener's Dictionary, which has been translated into various languages, and which will ever secure his reputation as a scientific cultivator of plants.

The year following Mr. M. had another parcel of cocoa-nuts given him, which, considering his former ill success, he planted in a different manner, as follows. Having a hot-bed, which had been lately made with tanners-bark, and which was filled with pots of exotic plants, he removed two of the largest pots, which were placed in the middle of the bed, and opening the tanners-bark under the place where the two pots stood, he placed the two cocoa-nuts in it, laying them side-wise, to prevent the moisture, which might descend from the pots, from entering the hole at the base of the fruit, and so rot the seminal plant on its first germinating. He then covered the nuts over with the bark 2 or 3 inches thick, and placed the two pots over them in their former station. In this place he let the nuts remain for 6 weeks; when removing the two pots, and uncovering the nuts, he found them both shot from the hole in the base of the fruit, an inch in length; and from the other end of the fruit were several fibres emitted 2 or 3 inches in length. On finding them in such a forwardness, he took them out of the bark, and planted them in large pots, filled with good fresh earth, plunging the pots down to their rims in the tanners-bark, and covering the surface of the earth in the pots half an inch thick with the same: soon after which, the young shoots were above 2 inches long, and continued to thrive very well. Mr. M. communicated this method to some of my acquaintances, who have tried it with the same success; and if the nuts are fresh, scarcely any of them miscarry. This led him to try if the same method would succeed as well, with other hard-shelled, exotic seeds, which he could not, by any method he had before tried, get to grow; as, the bonduc, or nickar-tree; the abrus, or wild liquorice; the phaseolus brasilianus frutescens lobis villosis pungentibus maximus Hermanni, or horse-eye bean; with several others; and he has found it both a sure and expeditious way to raise any sort of hard-shelled fruits, or seeds. For the heat and moisture, which are absolutely necessary to promote vegetation, they here enjoy in an equal and regular manner; the tanners-bark, if rightly managed, keeping to near an equality of heat for 6 months, and the water which descends from the pots, when they are watered, is by the bark detained from being too soon dissipated: which cannot be obtained in a common hot-bed, the earth in such being worked away by the water, and thereby leaving the seeds often destitute of moisture. Some of these seeds Mr. M. has had shoot in a fortnight's time; which would not have so done in a month in their native soil and climate. He has also found this to be an excellent method to restore orange, or any other exotic trees, which have suffered by a tedious passage, in being too long out of the ground: so that he recovered two orange-trees, which had been 10 months without either earth or water.

Of the several Strata of Earths and Fossils, found in sinking the Mineral Wells at Holt, in Wiltshire. By the Rev. Mr. Lewis, Vicar of the Place. Communicated by John Brome, Esq. N° 403, p. 489.

After having passed the upper turf, they came to a blue clay, which held about 3 feet; then they met with a yellow, brittle clay, much resembling ochre, used by painters, about 2 feet in thickness; and next with a loam of a looser texture, which sparkled with a kind of talc, called by the naturalists selenites, and was intermixed with yellow ochre. These selenites, which were plentifully found shot in the clay, were crystals consisting of transparent, shining, brittle flakes, some of a rhomboidal, others of a conical figure, but all hexaedra, or columns of 6 sides. They had no sensible taste of salt, and the clay in which they were found was interspersed with veins of coloured earth, of the colour of sulphur and iron rust.

Below this, at about 10 feet deep, they came to a bed of stones, of a large size and very hard texture, coated with flakes of gypsum of a white and yellowish colour, which ran through and divided them, as it were by various membranes, into different cells, all filled with hardened loam of a grey colour. These stones, which were all of an oval figure, in shape resembling pebbles, weighed from 10 to 60 lb. weight, and lay all on a level, one by another in the bed of clay. Here the springs come in, and below this the clay was darker coloured, and interlaid with small shells of the oyster, escallop, and muscle kind, and with a few belemnites curiously shaped. Here they met with stones of a very close texture, which when washed seemed to be nothing but a mass of shells jumbled and embodied together. And a little lower the clay produced some lumps of a black, bituminous sulphur, interlaid with some small thin laminæ, seeming to be metalline, and bright, like the purest silver; on firing this sulphureous bitumen on a red-hot iron, it emitted a blue flame, and strong smell, like brimstone, but the metal was lost.

From this account of the different strata found in sinking these wells, their impregnation seems to be from alum, vitriol of steel, ochre and sulphur, and from an accurate mixture of all these, which no art can imitate, it seems to derive those admirable qualities with which it is endued. Some conjecture may be made of its nature and qualities, from the tinctures it gives on chemical experiments; with astringent drugs, as galls, oak-leaves, and balaustians, it sometimes tinges red, inclining to purple, and sometimes will not tinge at all; with volatile alkalies, as spirit of urine, and sal ammoniac, it turns milky; with lixiviate salts, as oil of tartar, per deliq. &c. it rises in a white curdle; but acid saline liquors, as spirit of salt, nitre, &c. cause no alteration.

A gallon and half of this water being evaporated to dryness, the remains weighed 3 dr. 1 scr. and 19 gr.; some parts of which were white, and shot into striæ like needles, and others into prisms.

The neighbouring country is chiefly a strong clay; the quarries produce a very hard stone, which seems to be a composition of shells closely cemented and embodied together, and some marcasites which abound with sulphur; in sinking deep pits, they throw up stones like iron ore, and covered with a shining metallic substance, and serpentine stones, &c. and the ploughed fields abound with stones resembling shells of the escallop and cockle kind, striated with some astroites, which are all strong alkalies, and with aquafortis, or spirit of nitre, raise a violent ebullition.

Concerning the Causes of the Gout. By Sig. Michele Pinelli. N° 403, p. 491.

An account is here given of some experiments made upon gouty concretions. Having gotten from a very gouty person, who had died at Rome, about 3½ oz. of that tophaceous substance commonly found about the joints of persons afflicted with gout; Sig. Pinelli took 6 glass bottles, and put 10 gr. of the aforesaid substance into each. He then filled the 1st of these bottles with distilled vinegar, the 2d with sp. of vitriol, the 3d with sp. of salt, the 4th with sp. of sal ammoniac, the 5th with sp. of hartshorn, and the 6th with sp. of urine. After 24 hours he found the aforesaid tophaceous matter totally dissolved in the 3 first bottles, which contained the acid spirits, but in the 3 others, which he had filled with alkaline spirits, it remained entire and untouched, even for some time after. From hence he concluded this tophaceous matter to be of an alkaline nature, as it is the nature of acid spirits to dissolve such substances as are either altogether alkaline, or composed in part of an alkali. And this also he conceived to be the reason why the aforesaid tophaceous substance remained entire in the bottles filled with alkaline spirits, both being of the same nature, and consequently not to be dissolved by each other.

But for further satisfaction, he took the remaining part of this tophaceous matter, being about 3 oz. and put it into a small retort. Then having fixed a recipient to it, he distilled it, according to the rules of art, by a gradual fire, and obtained a spirit, with some few drops of oil, about 2 dr. of a caput mortuum remaining in the retort. This spirit he found to be a perfect volatile alkali, altogether of the same nature with that which is extracted from blood, from urine, and from bones. Hence again it is evident, he thinks, that this tophaceous gouty substance is composed of the same principles* with the other fluid and solid parts of the human body; or, that the cause of the gout is nothing else but a volatile, alkaline, corrosive salt, which by corroding the sensible membranes about the joints, occasions those acute pains, called the gout.

Of Fossil Teeth and Bones of Elephants. Part the second.† *By Sir Hans Sloane, Bart.* N° 404, p. 497.

Here Sir Hans Sloane offers some remarks on divers accounts of bones and teeth found under ground, met with in several ancient and modern authors, and which give him an opportunity of examining into the skeletons, and parts of skeletons, which are shown about as undeniable monuments of the existence of giants.

And first, as many of those bones and teeth, which are kept and shown about for bones and teeth of giants, have been found, on a more accurate inspection, to be only the bones and teeth of elephants or whales, it may from thence very probably be inferred, that others also, which for want of a sufficient description cannot be accurately enough accounted for, must have belonged either to these or to some other large animal. Thus, the fore fin of a whale, stripped of its web and skin, was not long since publicly shown for the bones of a giant's hand; and Sir H. S. has in his own possession, N° 1027, the vertebra of the loin of a large whale, which was brought him from Oxfordshire, where he was assured it was found under ground, and afterwards used as a stool to sit on. Now if a computation had been made from the proportion of this vertebra to that of the other parts of the skeleton, and all had been supposed to have belonged to a man, such a skeleton would have exceeded in measure, all those fabulous skeletons of giants mentioned by authors.

Hence Sir H. S. observes, that it would be an object well worth the inquiries of ingenious anatomists, to make a sort of comparative anatomy of bones; to examine, with more accuracy than has been hitherto done, what proportions the skeletons and parts of skeletons of men and animals bear to each other, with regard either to the size, or figure, or structure, or any other quality. This would doubtless lead us to many discoveries, and is otherwise one of those things which seem to be wanting, to make anatomy a science still more perfect

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* From the experiments of Dr. Wollaston, Phil. Trans. for 1797, it appears that gouty tophi or concretions, are resolvable into uric acid and soda. The deposition of such a saline matter upon the ligamentous and membranous parts of the joints, will readily account for the sharp excruciating pain which accompanies gouty inflammation.

† See the former part at p. 240.

and complete. The very vertebra abovementioned may serve to show the usefulness of such observations. It differs in many things from the vertebræ of men and land-animals, as do the vertebræ of whales, and the fishes of the cetaceous kind in general; and it is a very easy matter to distinguish them from each other. The body of the vertebra is considerably larger in proportion, and also lighter and more porous. The transverse processes arise from the middle of it on each side. The oblique descending processes are altogether wanting; and the arch, or foramen, which the spinal marrow passes through, is made up by the spinal process and the oblique ascending ones only: the body of the vertebra is very rough and uneven on each end, full of small holes and eminences, which receive the holes and eminences of a round bone, or plate, which answers to the epiphysis in a human vertebra, of which there are two between each vertebra, joined together by an intermediate strong and pretty thick cartilage, probably to facilitate the motion, and particularly the flexion of these animals in the sea. But to return.

There are many skeletons, that have from time to time been found under ground, and are mentioned by authors, who speak of them as skeletons of giants, and undeniable monuments of their existence, which he rather takes to be the skeletons of elephants, whales, or some other huge land or sea animal. Of this kind seem to be the pretended skeletons of giants of 12, 20, and 30 cubits in height, mentioned by Philostratus; the skeleton of 46 cubits in height, which according to Pliny was found in the cavity of a mountain in Creta, on the overthrowing of that mountain by an earthquake; the skeleton 60 cubits high, which Strabo says, was found near Tingis, now Tangier, in Mauritania, and was supposed to have been the skeleton of Anteus; the skeleton of Pallas, as pretended, found at Rome in the year 1500, which was higher than the walls of that city; and likewise that, which Simon Majolus says, was found in England in the year 1171: "Long before Fulgosus's time, upwards of 300 years, viz. anno 1171, by the overflowing of a river, a human skeleton was discovered in England, where the bones are still in their proper order: the length of the whole body was 50 feet."

There are others, the description of which concludes more clearly for their having once belonged to elephants, though it could not be positively asserted, that they did. St. Austin, discoursing of the existence and great feats of the giants before the deluge, mentions in proof of what he advances, that he himself, with several others, saw at Utica, on the sea-shore, the grinder of a man so large, that if it had been cut into teeth of an ordinary size, at least 100 might have been made of it. Hieronymus Magius, though himself very much prejudiced in favour of the existence of giants, yet suspects this tooth, mentioned by St. Austin, to have been rather the tooth of an elephant, or else some huge creature of the sea, than that of a man. But Ludovicus Vives, in his commentaries on that passage of St. Austin, takes notice, that in the church of St. Christopher at Hispella, he was shown a tooth larger than his fist, which they pretended was one of the teeth of that huge saint, no doubt, on as good ground as that very large shoulder-bone, which Hieronymus Magius says was shown in a church at Venice, was the shoulder-bone of St. Christopher.

The pretended skeleton of a giant, which was found near Drapini, a castle in Sicily, on digging the foundation of a house, and is described by Joh. Boccatius, is again not unlikely to have been the skeleton of a large elephant. For though the greater part of the bones, through the length of time, and the force of the subterranean steams, were so rotten, that after being exposed to the air, they fell to pieces almost on touching, yet three of the teeth were found entire, which weighed 100 oz. and were by the inhabitants of Drapini hung up in one of their churches, to perpetuate the memory of this fact. They likewise found part of the skull, capacious enough to hold some bushels of corn, and one of the shank-bones, which was so large, that on comparing it with the shank-bone of an ordinary man, it was judged that this giant, whom some took to be Erick, others Ethellus, others one of the Cyclops, and again others the renowned Polyphemus himself, must have been 200 cubits high; according to which calculation, he is figured and represented by F. Kircher, as by far the largest of a whole gradation of giants, whom, after this, he places in the following order:

	Cubits.
The giant of Strabo, whose skeleton was dug up near Tingis in Mauritania, and was found to be	60 high
Pliny's giant, found in a mountain in Creta	46
The skeleton of Asterius, son of Anactes	10
The skeleton of Orestes, dug up by special command of the oracle	7
The giant, whose bones were found under a large oak, not far from the convent of Reyden, in the canton of Lucern in Swisserland	9
Goliath, as described in sacred writ	6½

The case is still less doubtful with regard to those bones which were found in France in 1456, in the reign of Charles the 7th, by the side of a river in the barony of Crussole, not far from Valence. Johannes Marius in libris de Galliarum Illustrationibus, Calamæus in suis de Biturigibus Commentariis, Fulgosus in his Annals, et Joh. Cassanio of Monstroeuil, in his Treatise of Giants, severally take notice of these bones, which were so large, that the whole height of the giant, to whom it was thought they belonged, and who was supposed to have been the giant Briatus, was conjectured to have been of 15 cubits. The skull alone was 2 cubits thick, and the shoulder-bone 6 cubits broad. Sometime after, other bones of this kind were found in the same barony, near the same place, part of which Cassanio saw himself, and gives such a particular description of one of the teeth, as leaves little room to doubt, but that it was the grinder, and consequently the other bones, the bones of an elephant. His words are to this effect: "I saw there several bones, among which was a tooth of a surprising size, 12 inches long, and weighing 8 lb.; it was much longer than it was thick, and had some roots by which it was fastened in the jaw; the part by which the food was ground was 4 inches broad, and rather concave." He adds further, that such another tooth was kept at Charmes, a neighbouring castle; that he measured the length of the place, whence these bones were dug, and found it to be 9 paces; that some time after, more bones were discovered at the same place, and that the country all thereabouts was very mountainous, and such as the giants in all probability delighted to dwell and command in. Sir Hans Sloane has seen some of these bones brought from this place, which he took to have belonged to an elephant, by some large cells between the tables of the skull, which are in the skull of that animal.

Hieronymus Magius gives an account of a very large skull, 11 spans in circumference, and some other bones, probably belonging to that skull, which were dug up near Tunis in Africa, by two Spanish slaves, as they were ploughing in a field. He was informed of this matter by Melchior Guilandinus, who saw the skull himself, when he had the misfortune to be taken by the Rovers, and carried into slavery to that place in the year 1559. Sir Hans Sloane is the more inclined to believe, that this skull and bones were part of the skeleton of an elephant, because a like large skeleton was dug up near the same place some time after, which by one of the teeth sent to Peiresk was made out to have been the skeleton of an elephant.

Sir H. Sloane now comes to those bones, teeth and tusks, or horns, as some call them, which are mentioned by authors to have been dug up in divers parts of the world, and have been made out by them, or otherwise appear by their description and figures, indisputably to belong to the elephant.

Johannes Goropius Becanus, though he lived in an age when the stories of giants were very much credited, and had found their advocates, even among persons eminent for their learning and judgment, yet ventured to assert, that the tooth which was kept and shown at Antwerp, as the tooth of that unmerciful giant, whose defeat, brought about as they pretended by Brabo a son of Julius Cæsar, and king of the Arcadians, was fabulously reputed to have given occasion to the building of that castle and city, was nothing but the grinder of an elephant. However displeasing this assertion might be, as Goropius further adds, to those who are delighted with such idle and ridiculous stories, yet to the judicious it will appear the less surprising, on account of what passed not long before he wrote this book, when the almost entire skeletons of two elephants, with the grinders, and likewise the dentes exerti, or tusks, were found near Wielworda, Vilvorden, as they were digging a canal from Brussels to the river Rupel, to defend that town and country from the incursions of those of Mechlen. Goropius conjectures, that these elephants had been brought thither by the Romans, at the time either of the emperor Galien, or Posthumus.

A very large skeleton, likewise of a giant, as pretended, was dug up near Tunis in Africa, about the year 1630, of which one Thomas d'Arcos, who was then at that place, sent an account, together with one of the teeth, to the learned Peiresk. The skull was so large, that it contained eight meilleroles (a measure of wine in Provence) or one modius, as Gassendus calls it, or a pint and a half Paris measure. Sometime after a live elephant having been shown at Toulon, Peiresk ordered, that he should be brought to his country seat, on purpose to take that opportunity to examine the teeth of the creature, the impressions of which he caused to be taken in wax, and thus found, that the pretended giant's tooth sent him from Tunis, was only the grinder of an elephant. This is the second large skeleton dug up near Tunis in Africa, and it appearing plainly by the tooth sent to Peiresk, that it was the skeleton of an elephant, it may from thence very probably be conjectured, some other circumstances concurring, that the other also, which Guilandinus saw there, must have been rather of an elephant than of a giant.

Thomas Bartholin mentions the grinder, or maxillar-tooth, of an elephant, which was dug up in Iceland, and sent to him by Petrus Resenius. It was turned to a perfect stony substance, like flint, as was also the tusk of a rosmarus, dug up in the same island.

A large tooth, which by its shape appears plainly to be the grinder of an elephant, is described and figured by Lambecius, who had it out of the emperor's library, though he could not be informed where it was found, or how it got thither. It weighed 28 ounces, and was commonly taken to be the tooth of a giant. Antonius de Pozzis, chief physician to the emperor, in a letter to Lambecius, affirms it to be an elephant's tooth, and conjectures, that it was dug up at Baden, about 4 miles from Vienna, where, but a few years before he wrote this letter, they had found also the os tibiæ et femoris of an elephant.

Another tooth, probably of an elephant too, is described and figured by Lambecius, who had it out of the emperor's library. It weighed 23 ounces, and was found in the year 1644 at Krembs, in the lower Austria, on increasing the fortifications of that place.

The year following, when the Swedes came to besiege the town of Krembs, a whole skeleton of a giant, as was pretended, was found at the top of a neighbouring mountain, near an old Tower. The besiegers, in their intrenchments there, being very much incommoded by the water that came down from the mountains, dug a ditch 3 or 4 fathoms deep, to lead it another way. In digging this ditch they found that skeleton, which was much admired for its unusual size. Many of the bones, chiefly those of the head, fell to pieces on being exposed to the air; others were broken by the carelesness of the workmen; some escaped entire, and were sent to learned men in Poland and Sweden. Among these was a shoulder-bone, with an acetabulum in it, large enough to hold a cannon-ball. The head, with regard to its bulk, was compared to a round table, and the bones of the arms, or fore-legs, as thick as a man of an ordinary size. One of the grinders, weighing 5 pounds, was given to the Jesuits at Krembs: another is figured by Happelius (in his Relationes Curiosæ, tom. 4, p. 47, 48,) and it appears plainly by the figure of it, that it is an elephant's tooth. It weighed 4lb. 3 oz. Nuremberg weight.

Again, in Lambecius's Bibliotheca Cæsarea Vindobonensis, are two figures, and the description of a very large elephant's tooth, which weighed 4¾lb. It was sent from Constantinople to Vienna in 1678. They pretended that it was found near Jerusalem, in a spacious subterranean cavern, in the grave of a giant, which had the following inscription on it in the Chaldaic language and characters; "Here lies the giant OG;" whence it was conjectured to have been the tooth of Og, king of Basan, who was defeated by Moses, and who "only remained of the remnants of giants; whose bedstead was of iron, 9 cubits was the length thereof, and 4 cubits the breadth of it, after the cubit of a man."

Hieronymus Ambrosius Langenmantel, a member of the Imperial Academy of Sciences, inserted into the Ephemerides of that Academy, an abstract of a letter to himself, from Johannes Ciampini in Rome, concerning some very large bones, viz. the shank-bone, the shoulder-bone, and 5 vertebræ, one of which was a vertebra of the neck, which were dug up near Vitorchiani, in the bishopric of Viterbo, in the year 1687. They weighed altogether upwards of 180 Roman pounds; and having been compared with the other like bones in several collections at Rome, particularly the Chisian, they appeared to be by far the largest. Most people took them to be the bones of a giant, but Ciampini, and some others, taking them, with more probability, for the bones of an elephant, or some other large animal, and knowing that there was in the Medicean collection at Florence a complete skeleton of an elephant, they procured a copy of it, and found on comparison, the abovementioned bones so exactly to correspond with it, as to leave no room to doubt, but that they had been part of an elephant's skeleton.

The skeleton of an elephant, which was dug up in a sand-pit near Tonna in Thuringen, in 1695, is one of the most curious, and the most complete in its kind; as they found the whole head, with 4 grinders, and the two dentes exerti, or tusks, the bones of the fore and hind-legs, one of the shoulder-bones, the back-bones, with the ribs, and several of the vertebræ of the neck. But the whole has been so accurately described by Wilhelmus Ernestus Tentzelius, Historiographer to the dukes of Saxony, in a letter to the learned Magliabechi, printed in the Philos. Trans. N° 234,* that it is needless to add any thing, the rather, as that gentleman obliged the Royal Society with some pieces of the bones of this elephant, with part of the skull, in which appeared its cells, some of the grinders, and part of the dentes exerti; all which being produced at a meeting of the Royal Society, were found exactly agreeable to his description, and ordered to be carefully preserved in their repository. From the surface of the ground, down to the place where these bones were found, the diposition of the strata was as follows: a black soil 4 feet deep, gravel 2½ feet, the middle of which consisted of osteocolla and stones to the depth of 2 feet, osteocolla and stones half a foot, a sandy clay 6 feet, with about 2 inches of osteocolla in the middle, osteocolla and pebbles 1 foot, gravel 6 feet, a white and fine sand, of unknown depth, in which the bones were found.

In vol. 2, of Count Marsili's Danubius, where he treats of the antiquities he observed along this river, mention is made of several bones and teeth of elephants, which that inquisitive nobleman met with in Hungary and Transylvania, and which are now in his valuable collection of natural and artificial curiosities at Bologna. According to the best information, the people of whom he had them could give him, they were found in rivers, lakes, and pools. One of the vertebræ, a grinder, and a considerable part of the dens exertus, or tusk, were found in the lake, or pool of Hiulca. Two fragments of the os tibiæ, a little corroded on the inside, were taken out of a pool near Fogheras in Transylvania, once the seat of the princes of that country; and the whole lower jaw, with two grinders as yet sticking in it, were found in the standing waters by the river Tibiscus, a little above die Romer skantz, or the Roman fort.

Above was related the opinion of Goropius on the antiquity of those two elephants, whose skeletons were found near Vilvorden, which he traces no

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* Vol. iv. p. 218 of these Abridgments.

highe than the time of the Romans, and their expeditions into those countries, particularly under Galien and Posthumus. Count Marsili is of the same opinion, with regard to the bones and teeth found by him in Transylvania. He takes notice, that whoever is acquainted with the great use the Romans made of elephants in their military expeditions, ought not to be surprised that bones and teeth of them are found in those northern countries, where otherwise there cannot have been any ; and he urges, as a further proof of this assertion, that they are found in pools and lakes, it having been the custom of the Romans, to throw the carcases of dead elephants into the water, as it is still practised to this day with the carcases of horses and other beasts, to prevent the distempers and other inconveniencies, which their putrefaction might otherwise occasion. On the other hand, there are many arguments, taken from the size of the beasts, whose skeletons are thus found under ground, which sometimes far exceeds any that was, or could have been brought alive into Europe, from the condition they are found in, and from the particular disposition of the strata above the places where they are found; by which it appears, almost to a demonstration, that they must be of much greater antiquity, and that they cannot have been buried at the places where they are found, or brought thither any otherwise, than by the force of the waters of a universal deluge. To insist only on one of these arguments: if the skeletons of elephants, which are thus found under ground, and at considerable depths too, had been buried there, either by the Romans, or any other nation, the strata above them must necessarily have been broken through and altered; whereas, on the contrary, several observations inform us, that they were found entire; whence it appears, that what is found underneath, must have been lodged there, if not before, at least at the very time when these strata were formed; consequently long before the Romans.

But there is another argument, which seems to bear very hard against the conjectures of Goropius and Count Marsili. Tentzelius has already mentioned it, and it is urged from the great value of ivory at all times, and particularly among the Romans, which appears by many passages in antient authors; as for instance, by a very remarkable one in Pliny, lib. 12, c. 4, who takes notice, that among the valuable presents, which the Ethiopians were obliged to make to the kings of Persia, by way of a tribute, there were 20 large teeth (doubtless the dentes exerti) of elephants, and then adds, Tanta ebori auctoritas erat. Now it is to be presumed, that the Romans would not have neglected to take away the teeth, and particularly the dentes exerti of dead elephants, before they flung their carcases into the water, whereas there has scarcely been any skeleton, or any part of the skeleton of an elephant dug up any where, but the teeth were found along with them, and even among those figured by Count Marsili, there are three grinders, and a considerable part of one of the dentes exerti.

Dr. Plot, in his Natural History of Staffordshire, says, That he was presented by William Leveson Gower of Trentham, Esq. with the lower jaw of some animal, with large teeth sticking in it, dug up in a marl pit in his ground, and which, on comparison, he found exactly agreeable to the lower jaw of the elephant's skull in Mr. Ashmole's Museum at Oxford.

In the Museum of the Royal Society there are two fossil-bones of elephants: one was given by Sir Thomas Brown of Norwich, the other was brought from Syria for the os tibiæ of a giant; but Dr. Grew proves by an exact computation, that it can never have been the os tibiæ of a human skeleton, by being full 20 times as thick, and but 3 times as long. It is 42 inches long, and 12 in circumference, where it is thinnest. Dr. Grew observes, that by the figure it appears to have belonged to the leg, and not to the thigh, and he conjectures the whole elephant to have been about 5 yards high.

Gessner says, that he was presented by a Polish nobleman with a tooth, four times as large as that which he figured under the title of Hippopotamus, in his book de Aquatilibus. It was found under ground, in digging for the foundation of a house, together with a very large horn, as they called it, which many took to be a unicorn's horn, but erroneously, as Gessner thought, because of its being too thick and too crooked. It is very probable that this pretended horn, was the dens exertus of an elephant. The same author mentions a subterraneous cavern near Elbingeroda, where were found the bones and teeth of men and animals so large, that it was scarcely credible that ever any of that bulky size should have existed.

The grinder of an elephant, petrified, is kept in the king of Denmark's cabinet at Copenhagen, as appears by the catalogue, but no mention is made how it came thither, or where it was found.

In the same collection they show a large thigh-bone, which weighs about 20 Danish pounds, and is above 3 feet in length. It is so old, according to the author of the catalogue, that it is almost become stony. The same author mentions another large bone, then in the collection of Otho Sperling, which weighed 25lb. and was 4 feet long, said to be found in the year 1643 at Bruges in Flanders, where was the whole skeleton, which was 20 yards of Brabant in length.

A piece of ivory was dug up in a field on the river Vistula, about 6 miles from Warsaw, which having been shown at Dantzic to Gabriel Rzaczynski, author of the Natural History of Poland, it seemed to him to be the dens exertus of an elephant.

In the notes on the last edition of Dr. Herman's Cynosura Medica, published by Dr. Boecler of Strasburg, under the title of Unicornu Fossile, mention is made of a remarkable piece of fossil ivory, or rather of an elephant's tooth, in the hands of Jaques Samson de Rathsamhausen de Ehenweyer, an Alsatian nobleman. It was found in the Rhine on one of his estates near Nonneville, and was 3 feet 3½ inches in length, Paris measure. It was near a foot at the basis in circumference, where thickest, and about 8½ inches at the other extremity. It was filled within with a sort of marl; but the outer surface was stony in some places, and bony in others. The bony part scraped, or burnt, smelled like ivory. The scrapings boiled made a sort of jelly. The author of the notes adds, that they find fossil ivory in several parts of Europe, particularly in the Schwartzwald (Sylva Hercynia) in Moravia, in Saxony, and near Canstad in the duchy of Wirtemberg.

A Method for determining the Number of impossible Roots in Affected Equations. By Mr. George Campbell. N° 404, p. 515.

The substance of this paper, as well as of some others in the Philos. Trans. may be found in several other publications: as, in Stirling's Lines of the Third Order; in Maclaurin's Algebra, and in several others.

Observations on the Stomachs of Oxen. By Charles Price, Esq. of Trinity College, Oxon. N° 404, p. 532.

In the stomach of a cow Mr. Price found two things well worth observing. 1. That the villi composing the villous coat, (which in men are so very small as to be scarcely visible when examined separately) are in this animal so very large, as to allow an exact scrutiny into their structure. Each villus is formed by a duplicature of the internal lamina of the vascular coat; from which it receives 3 blood vessels, as represented in fig. 15, pl. 4, which shows one of the villi of the stomach of an ox magnified. Whether the two side-vessels are arteries, and the middle-vessel a vein; and whether those small branches arising from the side-vessels are secretory ducts, carrying a fluid from those arteries into the cavity of the stomach, making a kind of rivus perpetually running through the ductus alimentalis, he leaves others to determine.

The other thing remarkable in the stomachs of these large animals is, that their internal surface is covered by a production of the cuticle, which descends from the lips quite through the alimentary passage. Mr. Price thinks that the cuticle is continued through the intestines, as well in man, as in large animals; though its exceeding fineness may make it less observable.

Eclipses of Jupiter's Satellites, &c. observed at Bologna in 1727. *By Sig. Manfredi, F. R. S.* N° 404, p. 534. *From the Latin.*

Jan.	2,	9^h	45^m	47^s	The 3d satellite was immersed.
		11	53	38	Its emersion; but doubtful.
	5,	6	51	54	Emersion of the 1st satellite.
	7,	8	54	12	Emersion of the same satellite.
Feb.	7,	5	50	5	Immersion of the 3d satellite.
		7	52	54	Emersion of the same.
	8,	8	37	59	Emersion of the 2d satellite.
August	21,	13	34	39	Immersion of the 1st satellite.
Sept.	6,	11	55	15	Immersion of the same.
	17,	10	48	59	Immersion of the 3d; doubtful.
		12	40	50	Emersion of the same; doubtful.
March	9,	8	50	6	Emersion of spica ♍ from the moon's dark limb.

An Occultation of Venus by the Moon, at Bologna, Sept. 18, 1727, *N. S. By Sig. Manfredi.* N° 404, p. 535. *From the Latin.*

Sept.	18,	0^h	27^m	21^s	Venus enters the moon's dark limb.
		0	28	13	She is totally immersed.
		1	16	45	Begins to emerge on the bright side.
		1	17	50	The complete emersion.

The Barometrical Method of measuring the Height of Mountains. Extracted chiefly from the Observations of John James Scheuchzer, M. D. By J. G. Scheuchzer, M. D. F. R. S. N° 405, p. 537.

The height of mountains, and their elevation above the level of the sea, has been at all times thought worthy the attention of inquisitive philosophers. We find in Pliny, that Dicæarchus, one of the old geographers, a disciple of Aristotle, and, as Pliny himself stiles him, a man of great learning, had by particular order of some princes measured the heights of several mountains; and that the highest of them, Mount Pelius in Thessalia, was found by his observations 1250 paces high perpendicularly. Cleomedes also, a Grecian astronomer and geographer, who lived sometime before our Saviour's Nativity, asserts, that the highest mountain cannot be above 15 stadia, or 9375 Roman feet high. But Plutarch fixes the perpendicular height of the highest mountains, as also the greatest depth of the sea, only to 10 stadia, or 6250 Roman feet.

It will appear, by the sequel of this paper, that the height of mountains, as determined by these early writers, does not so very much deviate from truth as one would be apt to suspect from the infant state of arts and sciences in those times. Particularly the 15 stadia of Cleomedes, which make out 9375 Roman, or 10,214 Paris feet, will be found by the following observations to come very near the height of the mountains of Switzerland, which, though the highest of Europe, do not rise above 10,000 Paris feet above the level of the sea; and it may seem surprising that subsequent writers, even such as were otherwise deeply skilled in mathematical learning, have run them up to an extravagant, and altogether unnatural height.

At first, it is not improbable, they went only upon bare conjectures. But afterwards, when geometry came to be more and more improved, quadrants, semicircles, and other geometrical instruments were brought into use, by means of which, and by a trigonometrical calculation, the heights of places could be determined in a more satisfactory manner. And yet, however true the principles be, on which this method is founded, however nice the instruments, and however curious the observer, the method itself must be owned, and has been found by undoubted experiments, to fall far short of that accuracy which it seems to promise; and the more considerable the heights are, the more uncertain it will be. For in the first place, as the state of the air is very different in different seasons and different weather, its refraction also becomes thereby greatly altered, which occasions the tops of mountains to appear higher at some times than they do at others, and at all times higher than they actually are. But besides, there is another inconveniency, which whoever is acquainted with the true state of mountainous countries, must needs be sensible of, and that is the extreme difficulty of meeting, at the bottom of high mountains, with plains large enough for a proper horizontal stand, or basis, to such a triangle, as an accurate and knowing observer would think satisfactory to determine a considerable height, making even proper allowances for the air's refraction.

Among the many improvements in natural philosophy, which are owing to the Torricellian tube, one of the most considerable inventions of the last century, it has been thereby enriched with a new method of measuring the respective heights of places, and their elevation above the level of the sea; a method which, though it must be owned that it has not as yet, and perhaps, considering the inconstancy of the air, hardly ever will be brought to an absolute degree of certainty, is yet in many respects preferable to the trigonometrical one, as it has also been found by experience to come nearer the truth, and leads us, by a new and singular scale, from the very horizon of the sea, to the tops of the highest mountains, a distance far beyond the reach of geometrical instruments. This new method is grounded on that essential quality of the air, its gravity or pressure. As the column of mercury in the barometer is counterpoised by a column of air of equal weight, so whatever causes may make the air heavier or lighter, its pressure will be thereby increased, or lessened, and consequently the mercury rise or fall. Again the air is more or less condensed or expanded, in proportion to the weight or force which presses it: hence it is, that in England, Holland, the maritime provinces of France, and in general all those countries which border on the sea, the mercury stands highest; that the higher you remove from the sea into the midland countries, the lower the mercury will descend, because the air also becomes more rarefied and lighter; and that on the tops of the highest mountains it falls lowest; and these heights of the mercury, in different places, are reciprocally as the expansions of the air. From these principles, supported by a competent number of observations, it has been attempted by several learned men, to derive proper tables, by which the height of any place may be determined, if the height of the barometer be given, or the height of the barometer determined from the given altitude of the place; and likewise the expansions of the air settled, as they answer to every inch, or part of an inch, in the barometer.

Dr. S. passes over the first experiment of this kind, which was made in the year 1648, but a few years after the invention of the Torricellian tube was made public in France by Father Mersenne, by Monsieur Perier, according to the directions of the celebrated Monsieur Pascal, his brother-in-law, on the high mountain Puy de Domme, near Clermont in Auvergne, the height of which was thereby determined to 500 French toises, or 3000 Paris feet. (See the Appendix to M. Pascal's Traitè de l'Equilibre des Liqueurs.) Nor will the Dr.'s present purpose admit a particular enumeration of those made some time after, in 1661, 1665, and 1666, by George Sinclair, professor of philosophy in the university of Glasgow, on the cathedral of that university, and on several high mountains in Scotland, as also in some wells and coal-pits, a particular account of which he inserted in his Ars magna gravitatis et levitatis. Only observing, that these experiments of Sinclair, as well as that of Monsieur Perier, were intended not so much to lay the foundation of a calculation, to determine the differing heights of places, as to prove the gravity and pressure of the air, a problem very much controverted at that time, and to show that the same is much more considerable in valleys than at the top of mountains, and still greater in proportion at the bottom of wells, mines, &c.

But this matter was pursued still further by the members of the Royal Academy of Sciences at Paris, particularly, when by order of Louis the 14th, they drew that extensive meridian line across the whole kingdom of France. M. Mariotte, a celebrated member of that Academy, was one of the first who laid down certain rules for the construction of such tables, as might serve to determine both the elevation of places above the level of the sea, from given altitudes of mercury, and the heights of the air answering to every line of mercury in the barometer, from 28″, or inches, where the mercury was supposed to stand at a medium near the sea. The principles he went upon, and the method he followed, are treated at large, in his second Essay de la Nature de l'Air.

Some time after, in 1686, the ingenious Dr. Edmund Halley went about another calculation, which he derived partly from principles agreeing with those of M. Mariotte, partly from the specific weight of air and mercury, which were found by experiments to be as 1 to 10,800; air being to water as 1 to 800, and water to mercury as 1 to $13\frac{1}{2}$, or very near it. If so, as the column of mercury in the barometer is counterpoised by a column of air of equal weight, a cylinder of air of 10,800 inches, or 900 feet, will be equal to 1 inch of mercury, and 90 feet to $\frac{1}{10}$ of an inch, or 75 to $\frac{1}{12}$ part of it. The height of the air, as it answers to 1 inch of mercury, being thus determined, and the expansions of the air being reciprocally as the heights of mercury, Dr. Halley, by the help of the hyperbola and its asymptotes, calculated two tables, one showing the altitude to given heights of mercury, the other the heights of mercury at given altitudes. These tables, the first that ever were calculated, together with the Doctor's whole method of proceeding, and an ingenious attempt of his to discover the true reason of the rise and fall of mercury on change of weather, were printed in the Philos. Trans. N° 181; and the tables themselves were very lately reprinted, with some observations upon them, by Dr. Desaguliers, ib. N° 386.

In the year 1703, when the meridian line, first begun by M. Picard in 1669, afterwards continued in 1683, was further pursued, several observations of this kind were made, and the heights of several considerable mountains, particularly in the southern parts of France, determined, as well by trigonometrical as barometrical observations. Monsieur Cassini, the younger, took that opportunity to compare these observations with the rules laid down by M. Mariotte, (Mem. de l'Acad. 1705) in order to which, and in conformity to the said rules, he calculated two tables, one showing the height of the atmosphere, as it answers to every line of mercury in the barometer, the other determining the height of the atmosphere above the level of the sea at given altitudes of mercury. But having afterwards, on comparison, found that the observations made in 1703, did not in the main agree with the rules of M. Mariotte, and that the heights of places, as they appeared by those observations, exceeded, generally speaking, the numbers resulting from the tables made by him according to the said rules, he thought it necessary to calculate two new ones, where indeed the results are considerably greater than in the tables framed according to the rules of M. Mariotte; insomuch, that for instance, a place, where the mercury falls to 22 inches, rises above the level of the sea, according to Mariotte, 852 toises, or 5112 Paris feet; and, according to Cassini, 1158 toises, or 6948 feet, which makes a difference of 1836 Paris feet, or 306 toises. Dr. Desaguliers, in his dissertation concerning the figure of the earth, (Philos. Trans. N° 386,) has already shown how far the observations made by the gentlemen, who drew the meridian across the kingdom of France, differ from each other, insomuch that there are not 2 in 9 where the number of toises, said to correspond to the heights of the barometer, agree together; and that consequently the heights of mountains, as determined by these observations, are little to be depended on.

The Doctor's father, Dr. J. J. Scheuchzer, in his journeys over the mountains of Swisserland, as they were more particularly calculated for the improvement of natural philosophy in its several branches, neglected no opportunity, along with his other observations, to make such experiments with the barometer, as might serve to illustrate the qualities of the air, to settle the respective heights of places, and particularly to show how much our mountains rise, as well above the level of the sea, as above other neighbouring mountains, in France, Italy, Spain, &c. Many of these observations are scattered up and down in his writings, particularly his Itinera Alpina, and the several parts of his Natural History of Swisserland, which last work was published in High German. It would be too tedious to mention all the experiments he made at different times, and on different mountains. But the Doctor's design in this paper requires him to be particular in one, which for the height measured both with the line and barometer is, he believes, the most considerable that ever was made, and which enabled him more particularly to examine the two tables made by Cassini the younger, according to the rules of M. Mariotte, and the observations made by him and others, when the meridian line was perfected in 1703.

This curious experiment was made in the year 1709, at Pfeffers, a celebrated mineral water in the county of Sargans, at the bottom and top of a mountain, which rises from a small brook, called the Taminna, to the height of 714 Paris feet, as appeared by letting a line drop down perpendicularly from a tree at top full to the bottom. At the bottom of this mountain, near the Taminna, the mercury was by repeated experiments observed at 25″ 9⅓‴, and at the top it descended to 24″ 11⅓‴, so that it fell just 10 lines for 714 feet, which gives about 71 Paris feet for a line, if the heights answering to every line were supposed to be equal.

Dr. S. here remarks, that in this paper he uses Paris measure, viz. of toises (°) feet (′) inches (″) and lines (‴). Every toise being 6 feet, the foot is divided into 12 inches, and the inch into 12 lines.

The heights of the barometer at the bottom and top of the mountain being thus given, the height of it should be, according to M. Mariotte, 116° 0′ 8″ 11‴, or 696 Paris feet 8″ 11‴, which falls 17′ 3″ 1‴ short of the true height; and according to Cassini 153° 3′ 8″, that is, 921 Paris feet 8″, which exceeds the true height by 207 Paris feet 8 inches. By which it appears, that the table made according to the rules of Mariotte is much preferable to that of Cassini the younger. The same was likewise confirmed by another experiment made in June 1715, on the steeple of the cathedral at Zurich. At the foot of the steeple the barometer stood at 26″ 10‴, and at the top at 26″ 7½‴, and the height of the steeple was found by the line, 241 Paris feet 4 inches, which gives very near 69 Paris feet for one line. According to the table of Mariotte, the height of the steeple should have been 237 Paris feet; according to Cassini, 265; and according to the new calculation following, made pursuant to the experiments above, it comes to 243° 16″ 2‴, or about 2 feet more than the true height.

It appearing by the experiments made at Pfeffers, that from 25″ 9⅓‴ the barometer descends to 24″ 11⅓‴, that is, just 10 lines for the height of 714 feet; and the expansions of the air being reciprocally as the heights of mercury, his uncle, Dr. John Scheuchzer, undertook, pursuant to these principles, and the properties of the hyperbola, to calculate a new table, after the following method.

As the difference of the logarithms of the two given heights of the barometer 25″ 9⅓‴ and 24″ 11⅓‴, that is 309⅓ and 299⅓, or	Is to 1 foot,	So is the difference of the logarithms of the height of Mercury near the sea, 28″ 1‴, to any lesser height, as for instance 28″ 0‴, that is 337 — 336, or	To the height of the atmosphere above the level of the sea, as it answers to one line of Mercury is
928 — 898		1011 — 1008	
142717	714	12906	64′ 6″ 9‴.

Thus the height of the atmosphere at 28″ appears to be 10° 4′ 6″ 9‴; but, according to Mariotte, it is only 10° 3′ or 63 feet; and Cassini supposes it only at 10° or 60 feet.

In like manner, the height of the atmosphere, from 28″ to 27″ 11‴, is found to be 64′ 9″ 2‴. According to the same rule, half the height of the atmosphere, that is, the height of the place, where the mercury in the barometer would descend to 14 inches, appears to be 15060′ 3″, or 2510° 0′ 3″. Still upon the same principle, the mercury will descend to one line at the height of 133,397 Paris feet above the level of the sea, which make 22,232 toises 5 feet, or 11 Paris miles (at 2000 toises the mile) 232 toises 5 feet. But as, in order to determine the whole height of the atmosphere, the logarithm of 1‴ ought to be deducted from the logarithm of 336‴, or 28″; and as that logarithm is 00000, it follows from thence, that beyond the place where the mercury would descend to 1‴, the air is expanded into an indefinite space.

Observations on the difference of Sex in Misleto. By the Rev. Mr. Edm. Barrel.*

N° 405, p. 547.

Mr. Barrel having mentioned being pretty certain, that the plants of misleto were some of them male and some female; he was afterwards informed that Dr. Boerhaave had already told the world of a difference of sex in misleto. But having, since that, seen the Historia Plantarum, which is published as of his dictating; he found that he mentions it in such a manner as makes it seem that he only took his notion from Tournefort, and was not fully apprized of the true nature of these plants: ovarium alio à flore loco natum seems to suppose both flower and ovary to be on the same plant, though in distinct places of it.

Mr. Barrel had, from his own sowing of the berries, 4 thriving plants of misleto growing on one tree in his garden. These, being often in his view, gave him the first apprehension of there being any difference of kind or sex in this shrub. They were not of age to bear flower or fruit till 1726, when one of them bore a berry or two; and expecting that they should all do so the following year, he frequently examined them, and found that two plants had berries, and two had none. He then went and examined the misleto on other trees, which have plants of above 20 years growth. And he found the method of nature to be thus.

Dr. Grew observes, that many plants make a visible preparation in the former year, for the flower and fruit of the next season. This is done also by misleto.

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* The misleto belongs to the class *Dioecia* in the Linnæan system, in which the male and female flowers stand on distinct or separate plants; its order is that of *Tetrandria*.

At the latter end of May, the male plants put out little knobs, at the joints and tops of their boughs; which at first are not very unlike the young green berries, but they soon appear evidently distinct from them, and being by the latter end of July grown as large as the berries, are then not at all like them; spreading wider upwards, and having 3 or 4, or 5 buds, at the top of each knob. About June, the female plant also makes a like preparation, putting out at the joints and tops of the boughs, knobs, which are more sharp, and shorter than those of the male; with 1 or 2, but most commonly with 3 buds, or small points, at the top of each knob. He calls them buds, because in their season they open into flowers, both in the male and female plants; all the rest of the knob serving only for footstalks to the flowers, in the one sort, and to both flower and fruit in the other. By the latter end of August the berries are grown much larger than the knobs on the male plants. And from thence till late in January, there is little remarkable in either plant; only, the berry grows somewhat larger, and becomes ripe, and the knobs on the male grow more and more yellow; so that one may, at that time, discern a male from a female plant, at a considerable distance. By the 20th of February misleto is in bloom, both male and female. The knobs of the male are open at the top with 3 or 4, or 5 blossoms; which are very well described, though in short, in Boerhaave's Historia Plantarum.

The female plant flowers also now, with a blossom, which Boerhaave calls the ovarium, exactly like the male flower, save only, that the whole female flower is not larger than one leaf of the male flower. They both continue in full bloom till the middle of March, when the male blossoms begin to wither and drop off. And by the 20th of March the young berries begin to show themselves, swelling forth, one under each female blossom; which often adheres to the top of the berry; and being carried up with it, presently withers, and soon falls off again; though some continued on till the 12th of May, when the berries were of the size of a large pin's head.

This completed the year's observation. And Mr. B. wonders, that this plant, the admiration of all ages, should scarcely ever find one observer curious enough to follow its changes through one whole year's revolution. For had this been done with any accuracy, it must have been very evident, that one sort of misleto was very different from the other; one sort bearing very small flowers, with berries succeeding them; the other bearing much larger flowers, not succeeded by any berries; the very footstalk of the male falling off with the flower; whereas the footstalk of the female becomes a footstalk to the berry.

An Uncommon Sinking of the Ground in Kent. N° 405, p. 551.

The sinking of the lands at Lymne in Kent happened about 2 years before, and was the consequence of a very wet season; when it seems the waters that had fallen on the up-lands, and were not carried off by drains, soaked into the ground in such quantities, as to form a quicksand at some considerable depth in the earth, which not being able to bear the weight upon it, broke out at the side of the hill, and raised its lower parts; the brow sinking it seems 40 or 50 feet.

The ground sunk in one night, and was not perceived by the farmer's family, till they found the change in the morning, by their door-cases not suffering the doors to open. The house was strangely rent by this accident; and, had it not been timber built, must have fallen, as a very strong barn near it did, which was built of stone, for one great crack of the earth went through the middle of it, and split a large kitchen chimney from top to bottom.

In fig. 9, pl. 5, abcd represents the profile of the land; a the flat land at bottom, 3 or 4 miles from the sea; d the flat land at top, stiff and rocky ground; * the place of the farm at present, which not only sunk down from d 40 or 50 feet, but was also moved somewhat towards a; b the lower part raised to ☾.

Astronomical Observations made at Pekin. By Fa. Ignatius Kögler, Jesuit Missionary. N° 405, p. 553. *From the Latin.*

In 1724, new style.

Nov.	5,	6^{h}	9^{m}	Jupiter's 3d satellite immerged into his shadow.
	20,	6	44	The 2d satellite emerged.
	30,	6	14	The 1st satellite emerged.
Dec.	23,	6	19	The 1st satellite emerged.
In 1725.				
June	23,	2	29	The 3d satellite immerged.
July	9,	2	$55\frac{1}{2}$	The 1st satellite immerged.
Aug.	9,	11	27	The 1st satellite immerged.
	31,	11	45	The 1st and 2d sat. both immerg. together, like one sat.
Sept.	19,	6	$51\frac{1}{2}$	The 1st satellite emerged.
Oct.	2,	10	45	The 1st satellite emerged.
	10,	12	42	The same.
	11,	7	9	The same.

Oct.	15,	6^h	46^m	The 3d satellite emerged from behind Jupiter.
		7	4	It then disappeared in his shadow.
		10	20	Lastly it emerged from it.
	19,	9	9	The 1st satellite emerged.
	20,	9	6	The 2d satellite emerged.
	25,	11	6	The 1st satellite emerged.
	27,	11	45½	The 2d satellite emerged.
Nov.	3,	7	27⅔	The 1st satellite emerged.
	19,	5	42½	The same.
	20,	6	26½	The 3d satellite began to emerge.

A Total Eclipse of the Moon, Oct. 22, *after Midnight.*

At 0^h	49^m	The beginning of the true shadow, near the node.
2	46½	Total immersion, near the western node.
3	27½	Recovery of the first light at the eastern node.
4	26	About the end of the eclipse.

Other select Observations made at Ingolstad, in 1726. *By the Jesuits.* N° 405, p. 556. *From the Latin.*

Jan.	6,	6^h	40^m	30^s	Jupiter's satellite emerged.
	19,	6	52	0	Mars at the moon's limb.
		6	54	0	Now he was entirely emerged.
		7	54	25	His centre emerges out of the moon's obscure limb.
		7	54	35	Mars entirely emerged.
June	9,	15	4	20	Jupiter's 1st satellite immersed.
	17,	13	24	45	The same, doubtful.
	20,	15	16	40	The 2d satellite immersed.
Aug.	1,	5	25	17	Mars entered the moon's obscure limb.
		6	1	53	His first emersion.
		6	1	59	His total emersion.
	2,	11	41	20	Jupiter's 1st satellite immersed.
	14,	12	25	6	The 2d satellite immersed.
	25,	11	56	19	The 1st satellite immersed.
	26,	11	43	17	The 3d satellite began to emerge.
Sept.	1,	13	51	52	The 1st satellite immersed.
	2,	13	17	32	The 3d satellite totally immersed.
		15	45	9	The 3d satellite emerged.
	9,	9	40	0	The 2d satellite immersed.
		15	50	30	The 1st satellite immersed.

Sept. 9, 17^{h} 20^{m} 30^{s} The 3d satellite fully immersed.
10, 10 19 0 The first satellite immersed.

A Solar Eclipse observed at the same Place, Sept. 25.

At 5 17 22 Beginning of the eclipse.
The sun's diameter measured exactly 16′ 0″.

An Account of a Machine for measuring any Depth in the Sea, with great Expedition and Certainty; contrived by J. T. Desaguliers, LL. D. and R.S.S. and by the Rev. Mr. Stephen Hales, F. R. S. N° 405, p. 559.

There have been several machines contrived for measuring the different depths of the sea, especially such as could not be determined by the lead and line; but as those machines consisted of two bodies, the one specifically lighter, and the other specifically heavier than water, so joined together, that as soon as the heavy one came to the bottom, the lighter should get loose from it, and emerge; and the depth was to be estimated by the time of the fall of the compound body, from the top to the bottom of the water, together with the time of the emersion of the lighter body, reckoned from the disappearing of the machine, till the emergent body was seen again, no certain consequence could be drawn from so precarious and complex an experiment.

For, even in still water, and in the same place, the time will hardly be the same in two experiments: much less will this machine answer in the sea, on account of waves and currents, and many other causes.

But as the pressure of fluids in all directions is always the same at the same depth, a gauge which exactly discovers what the pressure is at the bottom of the sea, will show what is the true depth of the sea in that place, whether the time of the descent of the machine be only a minute or two, or 20 times as long.

The Rev. Mr. Hales, in his Vegetable Statics, describes his gauge for estimating the pressures made in opaque vessels, where honey being poured over the surface of mercury in an open vessel, rises on the surface of the mercury as it is pressed up into a tube whose lower orifice is immersed into the honey and mercury, and whose top is hermetically sealed. Now as, by the pressure, the air in the tube is condensed, and the mercury rises, so the mercury comes down again when the pressure is taken off, and would leave no mark of the height to which it had risen; but the honey, or treacle, which does better, which is on the mercury, sticking to the inside of the tube, leaves a mark, which shows the height to which the mercury had risen, and consequently makes appear what was the greatest pressure.

Dr. D.'s contrivance therefore is a machine, which will carry down Mr. Hales's gauge to the bottom of the sea, and immediately bring it up again: as represented fig. 1, pl. 6. AB is the gauge bottle; Ff the gauge tube, cemented to the brass cap of the bottle at G, with its open end f immersed in the mercury c, which by the pressure of 32 feet of water is carried up to d, with a little treacle or honey d upon it, raised up from D, a small thickness of treacle poured on the mercury. When the pressure of water is from a depth of 64 feet, the mercury and treacle rise up to E, $\frac{2}{3}$ of the height of the tube; and so higher proportionably to the depth.

N. B. A scale may be marked on the tube with a diamond.

K represents a weight, hanging by its shank L in a socket m, fixed to the ring ME, cemented at the bottom of the bottle. When the hole L of the shank is shoved up to m, the catch l of the spring s holds it from falling out of the socket, while the machine is descending. But as soon as K touches the ground at the bottom of the sea, the hole L rising, the catch flies back and lets go the weight, as represented in the figure. Then the empty glass ball I (which at sea may be a hog's bladder) rises up to the surface of the water with the machine; in which observing how high the inside of the tube is daubed, the pressure, and consequently the depth of the sea is known.

HG represents a brass tube to guard the top of the gauge tube.

There are holes at F, G, and E, to admit the water to pass freely every way.

To confirm the use of this sea-gauge, Dr. D. made an experiment in the following manner. Having poured some quicksilver into the bottle of the gauge, he poured on it treacle to the depth of half an inch; then screwed on the brass cap of the bottle to which the glass gauge-tube was cemented; by which means the open end of the tube was brought under the surface of the mercury, the sealed end being upwards. The machine, thus fitted, was immersed in a cylindric vessel of water, which with a plate at top was pressed between two columns, in such manner, that air might be condensed over the water without escaping. Then having forced in so much air with a syringe, as to lay on a pressure equal to what would be in a depth of 40 feet of water, he opened the cock of the upper plate, let out the air, and, on taking out the machine, it appeared how high the quicksilver had risen in the gauge-tube, by the smeared mark which the treacle left within.

Of Two uncommon Cases of Tumours of the Abdomen, from a Latin Tract published at Strasburgh, Anno 1728, *and entitled, Joannis Boecleri, M. D. &c. ad Exteros Medicos Epistola. By W. Rutty, M. D. R. S. Secr.* N° 405, p. 562.

The first case is that of a woman of Strasburgh, 32 years of age, whose belly, after an immature and hasty labour, grew gradually for 10 years together. During the whole time of gestation, she complained chiefly of the weight and heaviness of her belly; and sometimes of a tense pain and a difficulty in respiration: she said also that flatuses would sometimes be discharged from the pudenda, and the more they were so, the less uneasiness she perceived. The menstrua were regular as to time; but in the latter months, towards her death, she grew plainly cachectic. Her countenance was cadaverous; her breast and upper limbs perfectly emaciated; her feet œdematous, and the belly much more turgid and prominent than before; so that at length she breathed with the utmost difficulty, and on taking any nourishment, complained of a great straitness in her chest. On opening the abdomen, 2 days after her death, some water flowed out, of a wheyish colour; but on dividing the uterus, a plentiful quantity of a bloody liquor issued from it, with 72 molæ of different figures and solidity, and chiefly of a black colour. One only adhered to the lower part of the right side of the uterus, contiguous to its internal orifice. These solid substances weighed 64 oz.; as the liquor also filled 15 ancient Alsace measures, so that taken together, the whole weighed 80 lb. apothecaries weight. The skin of the abdomen was very thin, and almost transparent; the navel perfectly obliterated; the fat almost entirely consumed; the muscles pale, flaccid, and very thin also; and the peritonæum in some places so strongly attached to the uterus, that it could not, without the utmost difficulty, be torn from it. The body of the uterus, which is naturally thick, was extenuated to the same degree of rarity and transparency with that of the cutis of the abdomen, and of a surprising capacity. The liver appeared pale, and so flaccid that it might be easily rubbed to pieces. The height of the belly from the vertebræ of the loins to the navel measured 1½ foot; its length from the cartilago ensiformis to the pudenda, 2½ feet; and its circumference at the waist, 4 feet 2½ inches, though the woman was naturally of a small size and stature.

The 2d case is that of a maid-servant in the same city, of 23 years of age, whose belly, from a suppression of the menstrua, grew slowly for 3 years, without any other notable disorder; till on an accidental fall, it increased so much in 6 days, as to obliterate the navel; and not being capable of a farther distension, part of the matter which caused the tumefaction, flowed down to the legs and swelled them likewise; which brought on a difficulty of breathing, a small, frequent, and uneasy pulse, with a total loss of appetite. But, what was more remarkable, the systole and diastole of the heart were plainly felt under the left clavicle, the heart being, on dissection, found thrust up to that part of the thorax. The 14th day from the fall, a diarrhœa came on, which killed her in a few days. On making a small incision in the right hypochondrium, there gushed out from the cavity a liquor, in colour, consistence, and froth resembling well boiled beer; which on enlarging the incision, was followed by a fœtid purulent matter, with entire portions of the putrefied caul; which matter filled 56 Strasburgh pints. On this the belly subsided; but a large solid substance still remained under the containing parts of the abdomen. Opening therefore the whole cavity, there was found, under the left groin, a considerable tumour, nourished by its proper vessels, and every where fixed to the circumjacent membranes; from which being freed, it weighed 6 lb. common weight. This tumour proved a congeries of incysted abcesses (wrapped up in one common covering) of different sizes; the largest equal to a man's 2 fists, the smallest the size of an egg; and each of a different sort of substance: besides which was a great number of hydatides. The peritonæum was as thick as the cutis; the caul almost entirely destroyed; the stomach natural, but perfectly empty; the guts livid, very much thickened and vastly inflated, and connected preternaturally to each other by peculiar membranes. The liver strongly adhered to the right hypochondrium, and its coat parted from its parenchyma almost spontaneously. The left kidney nearly equalled the spleen in bulk, and the pancreas was as hard as a cartilage; but the uterus and bladder were found in statu sano. The cavity of the thorax was much smaller than usual, from the contents of the abdomen pressing up the diaphragm into it; in which cavity also was found the same sort of bloody putrid liquor, as likewise in the pericardium. The right ventricle of the heart was preternaturally soft and flaccid, and being opened, was lined with hydatides. The upper parts of the body were emaciated; the lower much tumefied by the water contained within them.

On the Culture and Management of Saffron in England. By James Douglass, M. D. F. R. S. N° 405, p. 566.

Saffron grows at present very plentifully in Cambridgeshire, and has grown formerly in several other counties of England; but as the method of culture is much the same in all, Dr. D. judged it sufficient to set down the observations which he employed proper persons, in different seasons, to make in the years

1723, 24, 25, and 28, through all that large tract of ground between Saffron-Walden and Cambridge, in a circle of about 10 miles diameter. In that country saffron has been longest cultivated, and therefore it may reasonably be expected that the inhabitants there are more thoroughly acquainted with it than any where else.

The greatest part of the tract just mentioned, is an open level country, with few inclosures; and the custom there is, as in most other places, to crop 2 years, and let the land lie fallow the third. Saffron is always planted on fallow ground, they prefer that which has borne barley the year before.

The saffron-grounds are seldom above 3 acres, or less than one; and in choosing them, the principal thing they have regard to is, that they be well exposed, the soil not poor, nor a very stiff clay, but a temperate dry mould, such as commonly lies upon chalk, and is of a hazel colour. About Lady-day, or the beginning of April, the ground must be carefully ploughed, the furrows being drawn much closer together and deeper, if the soil will allow it, than is done for any kind of corn.

About 5 weeks after, or during any time in the month of May, they lay between 20 and 30 loads of dung on each acre; and having spread it carefully, they plough it in as before. The shortest rotten dung is the best; and the farmers who have the conveniencies of making it, spare no pains to make it good, being sure of a proportionable price for it. About Midsummer, they plough a 3d time, and between every 16 and $\frac{1}{2}$, or pole in breadth, they leave a broad furrow or trench, which serves both for a boundary to the several parcels, when there are several proprietors to one inclosure, and to throw the weeds in at the proper season.

To this head likewise belongs the fencing of the grounds, because most commonly that is done before they plant. The fences consist of what they call dead hedges, or hurdles, to keep out not only cattle of all sorts, but especially hares, which would otherwise feed on the saffron leaves during the winter.

The next general part of the culture of saffron, is planting or setting the roots; the only instrument used for which, is a narrow spade, commonly termed a spit-shovel. The time of planting is commonly in the month of July, a little sooner or later, according as the weather answers. The method is this: one man with his spit-shovel raises between 3 and 4 inches of earth, and throws it before him about 6, or more inches; 2 persons, generally women, following him with heads, place them in the farther edge of the trench he makes, about 3 inches distance from each other. As soon as the digger or spitter has gone once the breadth of the ridge, he begins again at the other side, and digging as before, covers the roots last set, and makes the same room for the setters to place a new row, at the same distance from the first, that they are from each other. Thus they go on till a whole ridge, containing commonly 1 rod, is planted; and the only nicety in digging is to leave some part of the first stratum of earth untouched to lie under the roots; and in setting, to place the roots directly on their bottoms.

The quantity of roots planted in an acre, is generally about 16 quarters, or 128 bushels, which according to the distances left between them, as before assigned, and supposing them all to be an inch in diameter one with another, ought to amount to 392040 in number. From the time that the roots are planted, till about the beginning of September, or sometimes later, there is no more labour about them; but as they then begin to spire, and are ready to show themselves above ground, which is known by digging a few out of the earth, the ground must be carefully pared with a sharp hoe, and the weeds, &c. raked into the furrows, otherwise they would hinder the growth of the plants.

In some time after appear the saffron flowers. These are gathered, as well before, as after they are full blown, and the most proper time is early in the morning. The gatherers place themselves in different parts of the field, pull off the whole flowers, and throw them handful by handful into a basket; and so continue till all the flowers are gathered, which happens commonly about 10 or 11 o'clock.

Having then carried home all they have got, they immediately spread them on a large table, and placing themselves round it, they pick out the filamenta styli,* or chives, and together with them, a pretty long portion of the stylus itself, or string to which they are joined. The rest of the flowers they throw away as useless. The next morning they return into the field again, whether it be wet or dry weather, and so on daily, even on Sundays, till the whole crop be gathered.

The chives being all picked out of the flowers, the next labour about them is to dry them on the kiln. This is built on a thick plank (that it may be moveable from place to place) supported by 4 short legs. The outside consists of 8 pieces of wood, about 3 inches thick, joined in form of a quadrangular frame, about 12 inches square at bottom on the inside, and 22 inches at top, which is also equal to its perpendicular height. On the foreside is left a hole, about 8 inches square, and 4 inches above the plank, through which the fire is put in. Over all the rest, laths are laid pretty close to each other, and nailed

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* The stigmata or summits of the pistils (styles.)

to the frame, and then are plastered over on both sides, as is also the plank at bottom very thick, to serve for a hearth. Over the mouth, or widest part, goes a hair-cloth fixed to 2 sides of the kiln, as also to two rollers, or moveable pieces of wood, which are turned by wedges or screws, to stretch the cloth. Instead of the hair-cloth, many people use a net-work of iron-wire, with which it is observed, that the saffron dries sooner, and with a less quantity of fuel; but the difficulty of preserving the saffron from burning, makes the hair-cloth be preferred by the nicest judges.

The kiln is placed in a light part of the house; and they begin by laying 5 or 6 sheets of white paper on the hair-cloth, on which they spread the wet saffron, between 2 and 3 inches thick. This they cover with other sheets of paper, and over these they lay a coarse blanket 5 or 6 times doubled, or a canvas pillow filled with straw; and after the fire has been lighted for some time, the whole is covered with a board, having a large weight upon it.

At first they give it a pretty strong heat, to make the chives sweat, as their expression is; and here, if they be not very careful, they are in danger of scorching, and so spoiling all that is on the kiln. When it has been thus dried for about an hour, they take off the board, blanket, and upper papers, and take the saffron off from that which lies next it, raising at the same time the edges of the cake with a knife. Then laying on the papers again, they slide in another board between the hair-cloth and under-papers, and turn both papers and saffron upside down, afterwards covering them as above.

This same heat is continued for an hour longer; then they look to the cake again, free it from the papers and turn it; they then cover it, and lay on the weight as before. If nothing happens amiss during these first two hours, they reckon the danger to be over; for they have nothing more to do, but to keep a gentle fire, and turn their cake every half hour, till it be thoroughly dry; for doing which as it ought, there are required full 24 hours.

In drying the large plump chives they use nothing; but towards the latter end of the crop, when these come to be smaller, they sprinkle the cake with a little small-beer, to make it sweat as it ought; and they begin now to think, that using 2 linen cloths next the cake, instead of the 2 innermost papers, may be of some advantage in drying; but this practice is followed as yet but by few.

The fire may be made of any kind of fuel; but that which smokes the least is best, and charcoal for that reason is preferred to any other.

What quantity of saffron a first crop will produce is very uncertain. Sometimes 5 or 6lb. of wet chives are got from one rood; sometimes not above 1 or 2, and sometimes not enough to make it worth while to gather and dry it. But this is always to be observed, that about 5 lb. of wet saffron go to make 1 lb. of dry, for the first 3 weeks of the crop, and 6 lb. during the last week; and, now that the heads are planted very thick, 2 lb. of dried saffron may, at a medium, be allowed to an acre for a first crop, and 24 lb. to the 2 remaining, the 3d being considerably larger than the 2d.

To obtain these, there is only a repetition to be made every year of the labour of hoeing, gathering, picking and drying, in the same manner as before, without the addition of any thing new; except that they let cattle into the fields, after the leaves are decayed, to feed upon the weeds; or perhaps mow them for the same use.

About Midsummer, after the 3d crop is gathered, the roots must all be taken up and transplanted: the management requisite for which, is the fourth thing to be treated of. To take up the saffron heads, or break up the ground, as their term is, they sometimes plough it, sometimes use a forked kind of hoe called a pattock, and then the ground is harrowed once or twice over; during all which time of ploughing, or digging and harrowing, 15 or more people find work enough to follow and gather the heads as they are turned up.

They are next carried to the house in sacks, and there cleaned or rased. This labour consists in clearing the roots thoroughly from earth, and from the remains of old roots, old involucra, and excrescencies; and thus they become fit to be planted in new ground immediately, or to be kept for some time without danger of spoiling.

The quantity of roots taken up, in proportion to those that were planted, is uncertain; but at a medium it may be said, that allowing for all the accidents that happen to them in the ground, and in breaking up, from each acre may be had 24 quarters of clean roots, all fit to be replanted. The owners are sure to choose for their own use the largest, plumpest, and fattest roots, but above all, they reject the long pointed ones, which they call spickets or spickards; for very small round or flat roots are sometimes observed to flower.

Remarks on the Height of Mountains in general, and of those in Switzerland in particular, with an Account of the Rise of some of the most considerable Rivers of Europe. By J. G. Scheuchzer, M. D. &c. N° 406, p. 577.

In a former paper, in N° 405, Dr. S. observed that Dicæarchus found Mount Pelius in Thessalia, to be 1250 paces high, which make 6250 Roman, or 6822 Paris feet, a height which we may well pronounce too great even for the absolute height of Mount Pelius, above the level of the sea. Conformable to the determination of Dicæarchus, the Dr. mentioned, that Plutarch fixes the height of the highest mountains, and the greatest depth of the sea, to 10 stadia; and Cleomedes affirms, that they cannot exceed 16 stadia. The celebrated Galileo is one of the most modest among the modern writers on this head: for he says, (Nuntius Sidereus, p. 14,) that the highest mountains do not rise above a mile, or 8 stadia, or 5000 old Roman Vespasian feet, which make 5458 Paris feet above the level of the sea, which we shall find by and by to agree pretty well with some of the highest mountains in France, and may conjecture to do so with those in Italy. Kepler went rather too far, (Astronom. Optic. p. 129, 135, and Epitom. Astronom. lib. 1. p. 26,) when he assigned the mountains of Rhœtia (thought the highest in Switzerland) a height of 26 stadia, or 10,000 old Roman Vespasian feet, which make 10,916 Paris feet. The opinions of some other antient and modern geographers and mathematicians, will appear better by the following table.

A Table showing the Height of Mountains according to several antient and modern Authors.

	Stadia.	Old Roman Vespasian Feet.	Paris Feet.
Strabo (Lib. 2, Geog.) says, that the highest mountain, called by him Petra Sogdiana, is	30	18750	20468
Pererius (Lib. 12, in Genesin) determines the highest mountains to	32	20000	21832
Leo Bapt. Albertus (Architect. lib. 10, cap. 1,) to		22500	23661
Ath. Kircher, (Ars. magn. luc. et umb. p. 2, Prob. 5) brings them to	43	26875	29337
Fromond (Lib. 1, Meteor. Cap. 2, Art. 1,)	64	40000	43664
Gilbertus de magnete. Lib. 4, cap. 1	128	80000	87328
Pliny (Lib. 3, cap. 44,) according to the explanation of Fortunius Licetus (de Lunæ Luce subobscura, lib. 2, p. 306,) to	400	250000	272900
Ricciolus, Geogr. (Lib. 6) is of opinion, in pursuance of what he imagines to have demonstrated of the mountains Athos and Caucasus, that possibly there may be mountains of	512	320000	349312

Now, in opposition to this table, where the heights on first view must appear romantic and unnatural, let us consider the height of such mountains as have been measured, either by trigonometrical or barometrical observations.

In England, the height of Snowdon-hill, one of the highest mountains in Wales, was measured trigonometrically, by Mr. J. Caswell of Oxford, and found to be of 1240 yards, or 3720 English feet, which makes 3488 Paris feet. At the top of this mountain, the Mercury subsided to $25\frac{1}{2}$ inches, or 24 inches Paris measure. Now in the tables above, the height of the place where the Mercury subsides to 24 inches, is, according to Mariotte, $544\frac{1}{3}$ toises, or 3266 feet above the level of the sea; according to Cassini, 676 toises, or 4056 feet, and according to Dr. John Scheuchzer's calculation $559\frac{1}{3}$ toises, or 3356 feet; so that Mariotte comes 222 feet short of its height, as it was determined trigonometrically, Dr. Scheuchzer but 132, but Cassini exceeds this height by 568 feet, which confirms again, as in the former paper, that the Mariottian table is preferable to that of Cassini, though pretended to have been corrected on the former, and that of Dr. Scheuchzer is an improvement on both. According to the observation made by Dr. Halley, May 26, 1697, the Mercury stood at the top of Snowden-hill, at $26\frac{1}{12}$ inches English, which, reduced as above, would give the height of the mountain something less.

In France, when the meridian line, first begun in 1669, was continued in 1703, the heights of several mountains, particularly in the south of France, were determined trigonometrically by the members of the Royal Academy of Sciences: and we find in their Memoirs, the heights of the following:

	Height in		
	Toises.		Feet.
Mont Clairet in Provence	277	or	1662
La Massane in Roussilion	397	....	2382
The same according to another observation	408	....	2448
Bugarach, a mountain Languedoc	648	....	3888
Mountains in Auvergne.			
Le Puy de Domme, near Clermont	810	....	4860
La Courlande	838	....	5028
La Coste	851	....	5106
Le Puy de Violent	853	....	5118
Le Cantal	984	....	5904
Le Mont d'or	1030	....	6180
In the County of Avignon.			
Le Mont ventoux	1036	....	6216
Pyrenean Mountains.			
S. Barthelemy dans le paix de foix	1185	....	7110
La Montagne du Mousset	1258	....	7548
Le Canigou	1440	....	8640

The heights of these mountains, in general, seem rather too great. This indeed is easily accounted for, as they were measured by trigonometrical observations, which will, as noticed before, because of the refraction of the air, give the heights greater than they really are. But what confirms it still more, is, that according to the tables above, the numbers which answer to the heights of the Mercury, as they were observed at the top of some of those mountains, are considerably less, and that even Mons. Cassini's own numbers, which yet we have by some undoubted experiments shown to be too great, fall often short. It will be enough to mention two or three instances. At the Tower of Massane in Roussillon, the Mercury stood at $25\frac{5}{12}$ inches, and the height of that place was determined trigonometrically, to be .... 397 toises.

Now $25\frac{5}{12}$ inches answer, according to Mariotte, to 342
According to Cassini .... $392\frac{2}{3}$
According to Dr. Scheuchzer 350

At the top of the mountain called la Coste in Auvergne, the Mercury stood, Oct. 9, 1700, at $23\frac{1}{3}$ inches, and the height of this mountain was determined trigonometrically to be .... 851 toises.

Now $23\frac{1}{3}$ answer, according to Mariotte, to ...... $644\frac{1}{6}$ differ. $206\frac{5}{6}$
Cassini .... $826\frac{1}{6}$ .... $24\frac{5}{6}$
Dr. Sheuchzer .... $661\frac{5}{6}$ .... $189\frac{1}{6}$

The difference is still more considerable with regard to the high mountain Mont d'or en Auvergne, the height of which was determined trigonometrically to be .... 1040 toises.

At the top of this mountain, the Mercury fell, according to an observation made by F. Sebastien Truchet, June 8, 1705, to $22\frac{11}{12}$ inches, which answer, according to

Mariotte, to .... $707\frac{5}{6}$ toises, differ. $332\frac{1}{6}$
Cassini, to .... $925\frac{1}{6}$ .... $114\frac{5}{6}$
Dr. Scheuchzer .... $727\frac{1}{2}$ .... $312\frac{1}{2}$

As to the mountains of Switzerland: the barometrical observations made by the Dr.'s father on several of the highest, show, that they rise aloft, above all the neighbouring ones in France, Spain, Italy and Germany. And that it must be so appears further, because from their elevated tops, they dispense their waters to all the European kingdoms and provinces around them. Its valleys and lower parts too, as they are considerably remote from the sea, rise also in proportion above the level of it. It is true, the ascent thither is but gradual, in proportion to the remoteness. At Zuric, for instance, which lies towards the northern borders of Switzerland, the mean height of the barometer has been observed at $26\frac{5}{12}$ inches, which give the elevation of that town, above the level of the sea, according to Mariotte, $205\frac{2}{3}$ toises, or 1234 feet; according to Dr. Scheuchzer, $210\frac{2}{3}$, or 1264 feet; and according to Cassini, $221\frac{1}{3}$, or 1330 feet. This town is distant from the mouth of the Rhine, which is the nearest part of the ocean, at least 375 English miles, or 100 marine French leagues; and from Genoa, which is nearest on the Mediterranean, 225 English miles, or 62 French marine leagues. So that going down from Zuric northward towards the sea, the descent, or fall, is but little more than 12 feet for a marine league of France, if we suppose a straight line to be drawn from Zuric to the sea-shore in Holland; but it is much greater going southward towards the Mediterranean, where it comes at least to 20 feet for 1 league. And if we consider, that the highest mountains of Switzerland lie almost directly between Zuric and the Mediterranean shores, we must allow so much more in proportion, as those mountains are elevated above the horizon of Zuric; and how great and sudden this elevation is, will appear by the following observations.

At Ennen Sewen gen Aweren, in the ascent of the high mountain Freyberg, in the canton of Glarus, which lies south east of Zuric, the mercury was observed Sept. 11, 1710, at $23\frac{5}{6}$ inches; which gives the height of that place above the level of the sea, according to

Mariotte	$569\frac{1}{3}$ toises, or	3416 feet.
Dr. Scheuchzer	$584\frac{2}{3}$	3508 ....
Cassini	$712\frac{1}{2}$	4275 ....

Upon Scherf, one of the branches of the Freyberg, the Mercury fell Sept. 12, 1710, to $21\frac{2}{3}$ inches; which gives the height of that part of the mountain, according to

Mariotte	$906\frac{1}{6}$ toises, or	5437 feet.
Dr. Scheuchzer	$931\frac{1}{3}$	5588 ....
Cassini	$1247\frac{2}{3}$	7486 ....

Still higher on Blattenstock, another part of the same mountain, the Mercury fell on the same day to $21\frac{1}{2}$ inches, which answer, according to

Mariotte	$933\frac{1}{3}$ toises, or	5600 feet.
Dr. Scheuchzer	$959\frac{1}{3}$	5756 ....
Cassini	$1293\frac{1}{2}$	7761 ....

Hence from Zuric to the Blattenstock, near the top of the Freyberg, there is, in less than 3 days journey, a rise of 4366 feet, according to Mariotte, and 4492, according to Dr. Scheuchzer, that is, more than 3 times the elevation of Zuric above the level of the sea.

At Guppen ob Schwanden, in the same canton of Glarus, the Mercury was observed, August 5, 1705, at $23\frac{1}{3}$ inches, which give, according to

Mariotte	$644\frac{1}{6}$ toises, or	3865 feet.
Dr. Scheuchzer	$661\frac{5}{6}$	3971 ....

Omitting the numbers according to the tables of Mr. Cassini, having already shown that they are too great; the height of this mountain is nearly the same with the celebrated Puy de Domme, where Mons. Perier observed the Mercury, Sept. 19, 1648, at $23\frac{1}{6}$ inches.

On Joch, a high mountain in the territory of Engelberg, where it borders on the canton of Bern, full south of Zuric, the Mercury stood, June 23, 1706, at $21\frac{1}{3}$ inches, which gives the height of that mountain, according to

Mariotte	961 toises,	or 5766 feet.
Dr. Scheuchzer	$987\frac{2}{3}$	.. 5926 ....

This mountain, though very high, is far from being the highest in that neighbourhood; for next to it there rises another called the Titlisberg, covered with everlasting snow, which we may, on a moderate computation, pronounce at least 1000 feet higher than the top of the Joch, and consequently one of the highest in the country.

On the Avicula, by the Italians called Monte del' Uccello, and by some S. Bernard's mountain, from a chapel built in honour of that Saint, a high mountain in Rhœtia, towards Italy, the Mercury was observed, July 30, 1707, at $22\frac{1}{12}$ inches, which give, according to

Mariotte	$707\frac{5}{6}$ toises,	or 4247 feet.
Dr. Scheuchzer	$727\frac{1}{2}$	.. 4365 ....

This height must be understood only of that part of the mountain which is passed over by travellers, the mountain itself rising considerably above it, and the Adula, or Διαδȣέλλας of Strabo, (Geog. lib. 3) of which the Avicula is only a part, being still higher. The Rhenus posterior, or Hinder Rhein, and the Moüss, which at last loses itself in the Tesin, near Bellinzone, not much above the entry of the Tesin into the lake of Locarno, arise on this mountain.

At Santa Maria, on the Luckmannier Berg, by some S. Barnaby's mountain, which is also a branch of the Adula, the Mercury stood Aug. 9, 1725, as on the Avicula, at $22\frac{1}{12}$ inches, which shows the height of these two places to be equal.

In the Alp San Porta, near the source of the Hinder Rhein, Rhenus posterior, $5\frac{1}{2}$ hours from Speluga, Splügen in Rhœtia, the Mercury was observed, July 29, 1707, at $21\frac{1}{3}$ inches; where it stood likewise on the abovementioned mountain Joch, whither the reader is referred for the height of this Alp.

At Splügen itself, the Mercury stood the same morning early, at $23\frac{1}{3}$ inches; which gave the elevation of Splügen, according to Mariotte $644\frac{1}{6}$ toises, or 3865 feet, and according to Dr. Scheuchzer, $661\frac{5}{6}$ toises, or 3971 feet. So that the fall of the Rhine from the Alp aforesaid, to Splügen, in $5\frac{1}{2}$ hours, comes, according to Mariotte, to 1091, and according to Dr. Scheuchzer, to 1955 Paris feet perpendicular.

At the Capuchins, on the high mountain S. Gothard, a celebrated passage out of Switzerland into Italy, the Mercury stood, June 30, 1705, at 22 inches; which gives the height of that passage, which with regard to the highest tops of S. Gothard, lies but as it were at the foot of a high mountain, according to to Mariotte 852 toises, or 5112 feet, and according to Dr. Scheuchzer, $875\frac{5}{6}$ toises, or 5255 feet, above the level of the sea.

On the Furca, a high mountain between the Urseren Thal, Ursaria Vallis, and the Upper Vallesia, and one of the branches of the S. Gothard, the height of the Mercury in the barometer was observed, July 31, 1707, at $21\frac{5}{12}$ inches; which give the height of this mountain above the level of the sea, according to Mariotte, $947\frac{1}{6}$ toises, or 5683 feet, and according to Scheuchzer, $973\frac{1}{2}$ toises, or 5841 feet. Near this mountain there are others, which cannot be less than 800 or 900 feet higher.

These mountains, the Avicula, the Luckmannier Berg, the S. Gothard, and the Furca, together with the Grimsula, the Crispalt, the Sempronier, or Sempronius Mons, the Adula, and a chain of others, are the Lepontiæ Alpes of Pliny, (Lib. 3, c. 20,) and the Summæ Alpes of Cæsar (De Bello Gallico, lib. 3). They begin in the Upper Vallesia, then traverse the canton of Uri, and so run on eastward, across the country of the Grisons, towards Tirol. Their greatest height above the level of the sea, may be fixed in round numbers to 7500 or 8000 Paris feet.

It is on these very mountains, that some of the most considerable rivers of Europe take their first rise, within very small distances of each other. The Rhone, for instance, Rhodanus, by Marcellinus called maximi nominis flumen, and by Varro, Fluvius inter tres Europæ maximus, arises from two gletchers, as they are called, or montes glaciales, huge mountains of ice, near the Furca, and thence runs with great impetuosity down Vallesia, the Wallisserland, forming a long valley, surrounded on both sides by huge mountains, till it loses its waters and name in the Lacus Lemannus, or Lake of Geneva, but resumes it again near the town of Geneva, whence it flows with a more gentle descent through some provinces of France into the Mediterranean sea.

The Thecin, Ticinus, by Claudian, in his panegyric on the consulate of the Emperor Honorius, called Pulcher, the handsome, takes its first rise from two small lakes on the S. Gothard, and some lateral sources from the Lago sopra la Cima di Pettine, on a mountain called Pettine, the Lago della Sella, the lake of Rottom on the Luckmannier Berg, the lake of Tom, and the lake of

Fedretto, on a mountain of this name. It descends the Lavinia Vallis, or Liviner Valley, and in its way to the lake of Locarno, receives many brooks and rivulets from the adjoining mountains: it unites its waters with the Po, near Pavia, and loses itself jointly with that river in the Adriatic gulf.

The Rhine, Rhenus, by Cæsar, (de Bello Gallico) termed latissimus atque altissimus, arises in three several branches, which are called Rhenus anterior, posterior, et medius, the further, the hinder, and middle Rhine. The hinder Rhine takes its rise on the high mountain Avicula, Colmen del Occello, part of the Adula, in the alp San Porta, from a Gletcher, or ice-mountain, which extends in length full 2 hours. The middle Rhine, Rhenus medius, arises on the Luckmannier Berg, which is also part of the Adula, in the upper part of a valley called San Maria, opposite to one of the sources of the Thesin. The furthermost Rhine, Rhenus anterior, arises on that branch of the Crispalt, which is called Cima del Badut, Badùz, and soon receives several lateral branches from the Alps Mugels and Cornera. Near the monastery of Disentis, the further and middle Rhine join together, and the united stream falls into the hinder Rhine, near Reichenau. Below Rheineck, the Rhine falls into the Lacus Bodamicus, or Boden sea, and comes out of it near Stein; whence washing the borders of Swisserland, it then traverses great part of Germany in a very irregular course, till at last, in Holland, it loses itself in the great ocean.

The Reüss, Rusa, arises from a small lake called Lago di Luzendro, on the S. Gothard, but soon receives a considerable inforcement from the Furca, and near Urselen, another from a mountainous lake in Oberalp. Near Flüelen, not far from Ury, it enters the Four Waldstetten sea, Lacus quatuor Civitatum Sylvestrium, but resumes its course and name at Lucern, and at last falls into the Aar below Windish, Vindonissa.

The Aar, Arola, Arula, arises on the high mountain Grimsula, in the Upper Vallesia. About 3 hours below that, it falls into the lake of Brientz, and out of that, not far from the monastery Interlachen, into the lake of Thun, which it leaves near the town of Thun, and thence running by Bern, Solothurn, falls at last, after many windings and turnings, into the Rhine near Coblentz, Confluentia.

Gemmius Mons, the Gemmi, is a very high and steep mountain in Vallesia, over which there is a passage, but only in summer-time, from the Fruttinger valley, in the canton of Bern, to the mineral waters at Leük in Vallesia. The descent, on the south side of this mountain, is steep and frightful beyond imagination, being a narrow path, cut on the side of almost perpendicular precipices, sometimes with trembling wooden bridges, or planks over the clefts in the mountain, and here and there supported with low walls. Having been geometrically measured, it was found of 10110 feet in length, or rather height, its many windings and turnings included. At a small cottage, called Zur Dauben, a poor resting place for weary travellers, being the highest part of the mountain which is passable, the mercury subsided July 1, 1709, to $21\frac{1}{4}$ inches, which gives the height of that place, according to

Mariotte .... $974\frac{5}{6}$ toises, or 5849 feet,
And Dr. Scheuchzer .... 1002 .... 6012.

Not far from this cottage, is a small mountainous lake, called the Dauben sea, or the Pigeons lake, encompassed on all sides by high mountains, the tops of which, for their steepness, it would be impossible to reach. At Kandelstag, the first village in the Frutinger Valley, in the territory of Bern, going up to the Gemmi, the mercury rose on the same day to $24\frac{1}{6}$ inches, which give, according to

Mariotte.............................. $520\frac{1}{6}$ toises, or 3121 feet,
Dr. Scheuchzer.......... ....$534\frac{1}{6}$........ 3205.

And at Müllenen, at the foot of the Gemmi, it stood at $25\frac{7}{12}$ inches, which answer, according to

Mariotte to ....$318\frac{5}{6}$ toises, or 1913 feet,
Dr. Scheuchzer ....327 .... 1962.

On the other side of the Gemmi, at Leück, a celebrated place for its mineral waters, the mercury was observed July 2 and July 5, 1709, at $23\frac{3}{4}$ inches, which answers, according to Mariotte, to $581\frac{2}{3}$ toises, or 3490 feet, and according to Dr. Scheuchzer, to $597\frac{1}{2}$ toises, or 3585 feet. So that the cottage Zur Dauben rises above Leück, according to

Mariotte ....2359 feet,
Dr. Scheuchzer ....2427.

Above Müllenen, in the Frutinger Valley, according to

Mariotte..................... ....3936 feet,
Dr. Scheuchzer ....4050.

And the perpendicular height of the Gemmi, above the level of the sea, considerably exceeds 6000 Paris feet.

But high above all the mountains of Swisserland rises the Stella, Piz Stail, a steep mountain in the Schamser valley, in Rhœtia, or the Grisons, the height of which was, by Dr. John Scheuchzer, by some observations made in the year 1709, determined to 9585 Paris feet, above the level of the sea, according to his own calculation, or 9441 according to Mariotte, and 12196 according to Cassini; a height, which the Rupicapræ, or Shamoys themselves scarcely venture to ascend.

Having thus determined the heights of the mountains of Swisserland from barometrical observation, Dr. S. adds a few general observations on the natural history of that country, arising from the height of its mountains. And of the lakes, of which there are several, and very remarkable ones, about the borders of Swisserland. The ascent of the mountains of Swisserland being so very sudden and quick, the rivers, which arise in these mountains, rushing down, in consequence of so quick a descent, with great force and impetuosity, they would often overflow their banks, and cause frequent inundations in the flat countries, if this impetuosity was not in great measure broken, and their waters disposed to a more gentle descent; which is effectually done by those great receptacles of water, the lakes, which are besides of infinite use to the inhabitants around them, supplying them with plenty of fish for their sustenance, and enriching them by the facility with which commerce may be carried on over them.

Again, the extreme smallness of the Alpine plants is another striking observation: as they become less and less, in proportion as the mountains, on which they grow, rise higher. Whether this be owing to the sharpness and purity of the Alpine air, or the decreasing pressure of the atmosphere, which is far less upon mountains than in valleys and lower countries, or to a want of a sufficient quantity of subterraneous heat, to push the nourishment into the roots and vessels of the plants, or rather to a joint concurrence of these and other causes, would require a more leisurely consideration. The thing itself is an indisputable matter of fact, and it extends also to trees and shrubs, which become smaller as they grow higher. Nay, what is still more remarkable, no trees will grow beyond a certain height, which is the reason why the tops of mountains appear so bare and naked, if viewed at a distance, though a curious traveller shall not fail meeting upon their rich pastures with an agreeable variety of beautiful plants. The height, where the trees cease to grow, has been found, by barometrical observations, to be nearly the same in divers parts of Swisserland. Otherwise, the smallness of the Alpine plants is abundantly compensated by the richness of their virtues.

Again, the mountains are much more abrupt and steep, and the precipices greater, to the south than to the north, and westwards than eastwards. Many instances of this might be given in particular mountains in Swisserland, as the Gemmi, the Mons Fractus, &c.; but it is also evidently true with regard to the whole. Those are the highest mountains which separate Vallesia, the canton of Uri, and the several leagues of the Grisons, from Savoy, Piedmont, and the Tyrol, which lie to the south, or south-east. Those very countries are, as it were, one continued set of high mountains, quite to the Mediterranean sea, and the like structure seems to be continued farther on into that sea itself. The Pyrenean mountains also are only a continuation of that vast chain, which begins in the Lepontiæ Alpes, or the mountains in the Upper Vallesia, the canton of Uri and Rhœtia, and from thence spreads itself chiefly west and south. On the contrary, to the east and north they break off by degrees into gentle plains, which appears evidently by the vast tracts of ground, which the Rhine for instance, and the Danube compass, before they lose themselves, the one into the German ocean, the other into the Black sea; whereas the Rhone, on the other side, quickly, and with a proportionable velocity, reaches the Mediterranean. The same observation, with regard to the abrupt steepness of mountains to the south and west, holds true in other parts of Europe, remarkably in England and Norway, more or less in other countries. And so far as our maps and the accounts of travellers go, the same thing is observable in other parts of the world, but most evidently in the high mountains of Peru and Chili in South America, which terminate very abruptly westwards into the Pacific sea, but gradually decline to the east into immense plains, watered by some of the most considerable rivers in the known world, particularly the river of Amazons and the Rio della Plata, which arise in the said mountains.

To conclude, from what has been hitherto said, it appears evidently, that the mountains of Swisserland are the highest of Europe, and the great storehouse, whence all the countries around them are supplied with water.

Whether the depths of the sea correspond with the heights of mountains, must be left to future observations.

Optical Experiments made in the beginning of August 1728, *before the President and several Members of the Royal Society, and other Gentlemen of several Nations; occasioned by Signior Rizzetti's Optics. By J. T. Desaguliers, LL.D. and F. R. S.* N° 406, p. 596.

In 1727, Sig. Rizzetti, an Italian gentleman, published a book on the affections of light, in opposition to Sir Isaac Newton's doctrine, calling in question several of Sir Isaac's experiments, because they did not succeed in the way he tried them; he also denies the consequence of others, and advances new hypotheses contrary to experience.

The president of the Royal Society being acquainted with this, desired Dr. Desaguliers to make some experiments on this occasion. How these were made, and how they succeeded, with the inferences from them, are as follow: first remarking, that some of these experiments are the same as Sir I. Newton's, some also are his, but made in a different manner, and some are entirely the Doctor's own.

Exper. 1.—The Doctor prepared a box, represented fig. 1, pl. 6, like a truncated pyramid, of about 3 feet high, and 1 foot wide within, in the following manner: he painted the inside black, and in the back part, 1 foot above the base, made a square hole of 3 inches in width, whose section is rr, to receive a piece R shutting close with a rabbet or shoulder, whose surface coming through the hole was wholly covered with the painted paper, on which the experiment was to be made. Opposite rr, in the fore part of the box, was a door to open, with a tube in it, 4 inches wide, and 5 inches long, its section e,f,g,h, that two candles set on the places i, k, to enlighten the paper at r,r, might throw no direct light out of the box, whose section is represented at abcd. Then having made the room perfectly dark, he fixed the box on a table, that it might remain in one place; at the distance of 8 feet from rr, he fixed the lens LL, of 4 feet focus, in a frame on another table, with its axis going through the middle of rr: at the distance of about 8 feet beyond the lens, he set up the screen or square of white paper s. Having put into the hole rr a stiff paper, painted with vermilion, and wrapped four times and a half with black silk, as represented by R, that paper enlightened by the candles at i, k, the image of the red paper was projected on the screen at ρ, and when the most distinct place was found, the screen was fixed: then a paper painted with ultramarine being fixed in the hole rr, its image was so indistinct at ρ, that the images of the black silks could not be seen; but holding a piece of paper close to the screen, and bringing it forward, at about ¾ of an inch from it, the representation of the silks began to appear on the blue image; but it was most distinct at 1¾ inch, or at zz: so that there was 1¾ inch between the distinct base of the red, and that of the blue paper.

But what has led several people into an error in making this nice experiment, is the depth of the focus of the rays in both cases; for though the red image was most distinct at ρ, yet the representation of the black silks might just be perceived by a good eye when the screen was moved backwards or forwards ¾ of an inch; the blue image, which was stronger, had its silks visible an inch on either side of zz; so that in a paper half red and half blue, painted with these colours, one might have seen the silks, though faintly, on the two images at once, and have been thereby deceived; but ¾ of an inch beyond the place common to both, the red alone would have appeared distinct; and an inch short of the said place, the blue image most distinct, and distinct alone; that is 1¾ inch nearer the glass. Instead of vermilion, the red paper may be painted with carmine or lake, but it will not do so well, as was then tried, nor does Prussian blue do so well as ultramarine. The best way is to heighten the vermilion with a little carmine, and the ultramarine, which has too much white, with indigo; and then there will be a space between the two distinct bases, where both the images will be indistinct.

Fig. 2 represents the box with one side out, whose place is δdbB, eg is the hole for the tube in the door of the foreside, κδcd; rr the hole in the back to receive the piece R with its painted paper.

Fig. 3 is the box open before, with the candles and paper in it, the same parts being marked with the same letters as in the other figures.

The Doctor made the experiment in this manner, because Sig. Rizzetti attributed the different foci of the colours to different inclinations, which could not be alleged here; the red and blue being, as he had desired, successively fixed in the very same place; so that, as the candles were fixed, the light fell on the painted paper always with the same incidence.

Exper. 2.—Instead of the red or blue paper at rr, fig. 1, 2, 3, the Doctor fixed on the piece R, a paper half red, and half blue, as RB, fig. 4; then over the hole in the fore part of the box, represented by eg, fig. 2, he fixed a square plate κδcd, fig. 4, with an oblong hole in it, 4 inches long in its horizontal position, and 1 inch deep, through which might be seen the party-coloured paper, as if it were only of the size and shape of this aperture, and strongly enlightened by the candles hid in the box, the rest of the room being very dark.

The Doctor made this preparation, because Rizzetti objects to Sir Isaac's first experiment of the first book, that the black cloth beyond the party-coloured paper, was not colourless, and therefore the experiment was not decisive as particularly relating to the paper.

RB, fig. 5, is the paper contracted in length and breadth, by the aperture of the plate, which paper being viewed at the distance of 5 feet, by the prism 1, appeared as drawn at rb. The prism being removed to 2, at the distance of 10 feet, showed the paper as at rb. And when it was at 3, at the distance of 15 feet, the paper appeared as ρβ. In these three cases, the blue, b, b, and β, appeared lower than the red, r, r, ρ, the refracting angle of the prism being downwards. When the refracting angle was held upwards, as at 5, then the blue B was raised higher than the red R; but if due care be not taken, in turning the prism, a reflection may be mistaken for a refraction, as at 4; and then indeed the red and blue will be equally raised, as at T.

This must have been Signior Rizzetti's mistake, when (in page 38) he says that one colour was raised higher than the other by two lines, at 10 feet distance, but not at all at 5 feet; for several of the persons present at the Doctor's experiments, made the same mistake at first, before they could perform the experiment in the manner abovementioned; which they at last did, and found the colours separated most at the greatest, and least at the least distance. This mistaking a reflection for a refraction, has been the occasion of several more errors and difficulties, to be met with in Signior Rizzetti's book.

Exper. 3.—A candle K, fig. 6, reflected from the surface AB of the prism ABC, appeared very faintly to the eye at E, as a weak image at k; because the rays incident at I, pass most of them through the prism, and go on to R, separating from each other according to their different degrees of refrangibility; while a few of them are reflected to the eye in the direction IE.

But if the prism be in the position ACB, fig. 7, most of the rays of the candle K, incident at I, on the plane AB, after having passed perpendicularly through the plane BC, are reflected, and passing perpendicularly through AC, go into the eye at E, which sees a very strong image of the candle at K, while very few rays go down to R to produce colours.

This shows that the rays of light pass with more facility through glass (a dense) than through the air (a rare) medium; contrary to Rizzetti's assertion.

Exper. 4.—To make this more evident, and compare together the facilities with which light passes through the two mediums, the Doctor took a cube of glass, of 3 inches the side, AabBdDC, fig. 8, whose section is ABCD, and viewing it from E, to see by reflection the candle K, he saw two images of it; one at k very faint, and reflected from the upper surface AB, and the other at ϰ very strong, and reflected from the lower surface CD. Now it is evident, that the brightness of the images ϰ, is to that of the image k; as the facility with which the rays, in these circumstances, pass through the glass, or through the air: and those are easily compared, because both the images are seen at once.

Exper. 5.—The line PI, fig. 9, being perpendicular to the reflecting plane AB, of the triangle ACB, the Doctor brought the candle K by degrees so near to P, as to diminish very much the angle of incidence KIP; which made the image, or appearance of the candle at k, become fainter by degrees, and at last as faint as in fig. 6.

Exper. 6.—Having made the experiment as at fig. 7, the Doctor pressed another prism DFG, fig. 10, close to the prism ABC, and when he pressed them together but gently, some of the rays from the candle R, passed through the lower prism, and falling on a paper at R, made a reddish spot; but when he pressed them very hard, the spot became much wider, white in the middle, and only tinged with red about the edges: at the same time the eye saw a black spot in the image of the candle at k; and a stander-by looking obliquely at the place I, where the glasses touched, saw, as it were, a little hole through the prisms, as large as the spot k. But if the prisms be pressed together but gently, then all the other phænomena disappear, except the first little spot at R, as in fig. 11.

When the candle is seen by reflection from the lower surface of a prism, as in the 7th, 9th and 10th figures, the rays pass quite through that surface, and are turned up again, by its attraction, in curve lines, so as to re-enter the prism, and then, going out again through the surface AC, go up to the eye at E. In this case, the most refrangible rays, being the most easily inflected, make the least curves, whose vertices are nearer the glass than those of the greater curves, made by the least refrangible rays. This is proved by experiment 6, where the under prism only attracts down, from the reflection of the upper prism, the red making rays, as in fig. 11, where the plate of air between the prisms is of some small thickness. But when the prisms, whose surfaces are a little convex, are pressed hard together, the lower prism is near enough to attract rays of a great degree of refrangibility; and therefore the spot then becomes white in the middle; and only red about the edges, which are produced by such parts of the lower prism as are not so near the upper.

There are two circumstances in the 6th experiment, which disapprove Rizzetti's assertion (page 125) viz. That there is a sensible reflection even where glasses touch; for when the prisms touch at I, fig. 10, the black spot appearing in the image of the candle k, shows that there is at I a deficiency of those rays, which, coming from the middle of the candle, used to be reflected up to the eye at E, and therefore that AB, the reflecting surface of the upper prism, ceases to reflect in a little space round about I, where the upper surface DF of the under prism touches it; the rays, which before were reflected, now going down to make the spot at R. The other circumstance is this; that whereas a paper at k is invisible to an eye at E, by the interposition of the prism DFG; when another prism ACB is laid over it, and pressed hard, there appears to be a hole, of about $\frac{1}{6}$ of an inch (more or less in diameter as the prismatical surfaces are more or less flat) through which the paper at k becomes visible; this being the place of contact where the reflection downwards, of the surface DF, ceases.

This happens, because those rays, which, coming from the candle K, were bent in curves under the surface AB of the upper prism, about several points near I, are by the nearness of the surface DF of the lower prism, brought down to R, instead of being turned up again to the eye at E; while those rays which, coming from the paper at k, through the surface GF of the lower prism, and passing through its upper surface FD, were bent in curves about several points near I, are prevented from turning down again to R, and are brought up to the eye at E, which consequently must see a round part of the paper at k, just as large as the place of contact, which appears like a hole; or as if the two prisms, being changed to a parallelopiped, were covered with a dark paper that had only a small hole in it.

But to make this more evident, especially to such as are not well acquainted with Sir Isaac's optics, the Doctor explains the manner of the bending of the rays, where they are refracted or reflected.

Of the Bending of the Rays in their Refraction.—Let DD, fig. 12, represent a dense medium, as glass, whose surface is GG; and AA a rare medium, as air. Now let us suppose a power to extend all over the surface GG, acting from AA towards DD, in lines penpendicular to the surface GG, very strong in contact, but insensible at a very small distance from the said surface, which may be called the attraction of the surface GG, without considering whether it be any real virtue in the said surface, or the action of a medium impelling towards it. Let lines 11, 22, 33, such as express the lines in which the attraction exerts itself, and the line MM, extremely near to GG, the limits of the attraction, beyond which it cannot affect a ray of light. Let the ray of light Ra, moving from a rare medium into a dense in the direction Rr, come towards the surface GG in such an angle, that it may be refracted. When the ray comes to a, by the attraction at a it will be acted on in the line ab, and by the known laws of mechanics be turned out of the way into the direction aa, instead of ar: when it is got to b, being acted on in the direction b 4, its new direction will become bb: at c, by the power acting in the line c 5, it will change its direction to cc; and lastly, at d it will go into the glass in the line dd, continuing in that straight line while it moves in that medium.

Now if the lines 11, 22, 33, n, c, b, a, be infinitely near, as they must be supposed to be, the ray, instead of being broken into the several straight lines ab, bc, and cd, will be bent into the curve abcd; and the emergent ray dd will make the same angle with the incident ray Rr, as if the refraction had been made at once at the point n, which point may be considered as in the surface GG, because MM has been supposed extremely near that surface: then also may refractions be considered in gross, and rays traced, in all optical propositions, as if there were no such curve as what we have been describing.

Again, let D, fig. 13, represent the dense medium or glass, and A the rare medium or air; Ra a ray of light coming out of the dense medium into the rare, in the direction Rr, in which it may be refracted, as for example, in an angle of 30 degrees with the perpendicular Pa. Let MM be the line which limits the attraction of the surface GG, which attraction is exerted in lines tending perpendicularly from MM to GG. As soon as the ray of light has emerged at a, it is attracted in the direction aP, and therefore diverted from the line ar, into the new direction aa; at b, it is turned into the line bb; at c, into the line cc;

and at d, into the line dd; so that the emergent ray will be dd, as if the refraction had been performed in the point n, and that point was in the surface GG, without any curve at abcd; and all the rest as we considered it before, with this difference only, viz. That the ray is bent just as it comes out, or rather when it is come out, of the dense medium; whereas before we considered its bending before it came into it.

Of the Bending of Rays in Reflection.—But if the ray RA, fig. 14, coming out of glass into air, should come in such a direction, as to be wholly reflected, as it will do when the angle RAP is of 45 degrees; then the reflection will not be made at the surface GG, nor above it in the glass; but under the said surface, in the air, or even in a vacuum, or any medium less dense, or rather less refractive than glass.

MM represents the limits of the attraction of the glass, exerted in a direction from MM to GG perpendicularly, as beforesaid.

The ray RA, moving in the direction RR, at its emersion at a, is, for the reasons before given, turned into the direction aa; then at b, into the direction bb; at c, into the direction cc; at d, into the direction dd; at e, into the direction ee; and at f, into the direction ff parallel to GG; then at g, the ray is again turned towards the glass, by whose attraction changing successively into all the directions gg, ii, kk, and ll; at last it re-enters the glass in the direction mm, making the same angle with the perpendicular mp, that RA made with AP. Now as the lines perpendicular to GG, drawn from MM, are infinitely near, the line abcdefghiklm must be a curve; and as MM and GG are extremely near, the vertex of the curve, whose tangent is ff parallel to GG, will be so near the point I, as to be considered as coinciding with it, when we compare the angle of incidence with that of reflection; then also will the space between the parallels pm and PA be so far diminished, that those two lines may be considered as coinciding, the angles mmp and RAP being equal, whether the three points m, I, a, coincide or not.

For these reasons, for common use, we may consider the reflection from the under surface of the glass, as made at once in that surface at the point I. But when things are strictly examined, experiments as well as the above reasoning, show that there is such a curve as above mentioned. See experiment 6, fig. 10 and 11; and others are mentioned below.

N. B. If any point of the curve abc, &c. between a and f, fall below, or beyond the line MM, the ray will then go on in a straight line, tangent to the curve in that point where it leaves the line MM.

Now let us suppose MedcbarM, in the same figure to be glass, or any other dense medium, and mPPR air, or any other rare medium, and RA a ray of light moving in the rare, towards the dense medium, in the direction RA towards r; if instead of an attraction at the surface of the glass MM, there be supposed a repellent force, whose limits are GG; then will the ray, by the repulsion of the surface MM, be bent into the curve abcdefghiklm, in the same manner as we showed it would be under the surface GG, when GPPG was considered as a dense medium. Hence it follows, that a ray moving in the air, is reflected from a specular surface of glass, or any other mirror, opaque or diaphanous, without touching the said surface.

N. B. That the same power may, under different circumstances, attract to and repel from the same surface, shall be made out in the remaining part of this paper; but now taking such a power for granted, we will proceed in considering the flexure of rays of light.

Let us suppose a prism ACB, fig. 15, to have the attracting power of its inferior surface extend as far as the line mm; if another prism GDF, the attracting force of whose upper surface extends as far as nn, be brought very near to the first prism; where the attracting powers of the prisms interfere, they will destroy each other, because they act in contrary directions; and thereby the limits of attraction of each of the surfaces will be contracted; the power of AB extending no further than nn, and that of DF no farther than mm, while the space nnmm loses all the force that it had (and would have on the removal of either prism) to turn a ray of light, moving obliquely, out of its direction.

Now in this situation of the prisms, a ray of light entering the surface CB at right angles, will go through the 2d prism also at right angles, not exactly in the same line, but in a line parallel to the direction of the incident ray: for example, let the ray RA, not refracted at, because perpendicular to, the surface CB, emerge from the first prism at a, in the direction ar; its changed direction at a will become aa, and at b, bb, or rather the ray will be inflexed in the curve ab; and at b getting out of the power of the attraction of the surface AB, it will, for the reasons before given, move in a straight line from b to c, where it will be bent again the contrary way in the curve cd, of the same kind as ab, and lastly emerge in the direction dd, parallel to the first direction RR. From hence it follows, that when the prisms are brought so near as to touch, their mutual attractions destroying each other, the rays of light will not be bent, but pass through the two prisms (which in this case perform the office of a parallelopiped) in the same direction with which they came into the first prism, and consequently produce no colours; contrary to what is affirmed by Rizzetti (page 78, 79, &c.); and when the rays RA fall obliquely on the surface CB, the effect of their refraction at their immersion at s, to produce colours, is taken off by the refraction which they suffer at their emersion at z.

Exper. 7.—The Doctor took the cube of fig. 8, and looking obliquely through it at the hole of the window of his dark chamber (the sun shining or not shining) the hole appeared entirely colourless, as did also a candle; both appearing fringed with colours when seen through the prism. Then holding two prisms together, as in fig. 10, if the hole of the dark chamber be at k, it appears white to the eye at E; but if the angles of the prisms at BF be a little separated, while the points AD touch, the hole will appear coloured: when the surfaces are separated at AD, and touch at BF, the colours appear in an inverted order; but if the surfaces AD and BF are parallel, whether they touch or not, the hole will appear white.

N. B. In this case the prisms must be similar, that the surface FG may be parallel to AC; otherwise AB and DF must be so inclined to each other, as to render AC and FG parallel. Indeed if one of the prisms be very far removed from the other, the heterogeneous light which entered in at FG, may be so far spread by the separation of the differently refrangible rays, that the prism ABC will not take it all in; then the eye behind the second prism may see colours, as it seems Rizzetti did. See page 79 of his book.

If the ray of light Rabcdd, fig. 15, changing its direction in the manner abovementioned, makes an angle of about 45 degrees with the perpendicular PA; on the removal of the lower prism, the ray will be turned up again, as in fig. 14. But if the angle PAR be greater, the ray will still be turned up again in a curve, as abcdef, fig. 16, notwithstanding the lower prism is at DFG; but if that prism be brought up closer to the surface AB, the curves will be destroyed where the prisms touch, and all the rays in the place of contact brought down through the lower prism.

The most refrangible rays consist of smaller particles than the least refrangible rays, and therefore must have least momentum, the velocity of all the rays being the same; and consequently are more easily turned out of the way by attraction or repulsion, which makes the curves made by the purple and violet rays, under the surface AB, to be less and nearer the said surface, than the curves made by red and orange rays.

Suppose a violet ray moving in the direction Rr, fig. 16, to be so bent under the surface AB, that at the vertex of the curve, or where its tangent cc is parallel to AB, there still remains a small space between the curve and the line nn, where the limits of attraction, contracted by the proximity of the undermost prism DFG, end, that ray will be turned up again in the curve def, and so reflected in the line ff, the directions having been successively changed, as in fig. 14. But a red ray, with the same inclination, would pass on into the lower prism, as was explained in fig. 15. Because the momentum of the red ray being greater than that of the violet, the same degree of attraction could not give it the same flexure.

This is confirmed by experiment: for when the lower prism is not pressed hard against the upper, as in fig. 11, the rays brought down to R make a spot, of a colour made up chiefly of red and orange rays; but when the prisms are pressed closer, the spot grows larger, and perfectly white in its middle, because all sorts of rays are brought down to the spot; but it is inclosed round with a reddish border, occasioned by the parts of the prism which are very near, but not in contact, or at least not near enough to bring down the green, blue, purple and violet rays. This shows that the reflection is not made from the interior solid parts of the glass, nor from the parts in the surface, as Rizzetti affirms. But this is made more evident by

Exper. 8.—A candle being in the position K, fig. 17, the eye at E, and the prism at ABC; a strong image of the candle was seen at k, as in fig. 7. But lifting up a vessel of water VSSV, till the surface of the water VV touched AB the lower surface of the prism, the image of the candle became almost insensible, as the eye lost all those rays which now were attracted into the water. And for a further proof, that the reflection is made under the surface, and not in it, when the prism was taken out of the water, being wet at its lower surface, or having a stratum of water, whose surface was VV, fig. 18, under AB, the image of the candle again became vivid, the rays being turned up again under VV. Indeed the image, in this case, though strong, did not appear well defined, by reason of the unevenness of the watery surface VV, fig. 18.

I am very well aware that Rizzetti may answer here, that what I have said above, does in some measure favour his notions; and that the rays which, in fig. 7, having passed through AB, the lower surface of the prism, are turned up again to the eye at E, do not suffer a reflection, but a new immersion; for he says, in p. 125, "An English gentleman, (meaning Sir Isaac Newton,) subjoins secondly, that if light, in passing from glass into air, should fall then in an angle of 40 degrees, it is entirely reflected. To which I answer (says Signior R.) that from what I have laid down in Prop. 4, Case 1, Opt. 1, it follows, that this is not a true reflection of the light, but rather a new immersion; and therefore I deny that it follows from that phenomenon, that light is reflected from the solid parts of bodies, at some distance." And a little lower, having quoted what Sir Isaac says, concerning the blue light, which, coming from one prism obliquely on the farther surface of another, is wholly reflected, at the same inclination that the red light is wholly transmitted; he says, "Let it again suffice to answer, that in this case also, what the author calls a reflection, is a new immersion of the light."

But this is only cavilling about words; for if the ray of light, which moving in a dense medium, falls obliquely on the surface common to that and a rarer medium, be turned back again in the dense medium, so as to make the angle in which it returns from the said surface, equal to that in which it came to it; this return of the ray may properly be called a reflection, whether the ray be turned back at the point of the incidence in the surface, or be carried about the point of incidence in a small curve, whose consideration may be omitted in tracing the way of a ray of light in its passage, for making of optical machines. Whoever reads the 8th Prop. of the 2d part, Book 2, of Sir Isaac's Optics, may very easily find that he was not ignorant of the turning back of the ray under the surface of the glass before it returned into it: and though the reflection in that case be not made by impinging on the solid parts of the glass, yet it is owing to them that the light, acted upon at a distance, is turned up again, as has been shown by several of the experiments abovementioned.

Now let us see how Rizzetti's account of the new immersion agrees with phænomena.

Let all above the line PP, fig. 19, be a dense medium, as glass; and all below it a rare medium, as air; ABCD is a beam of light insensible in thickness, but of some breadth, whose rays cohere to one another, and whose section or first line is BC. If the medium in which BC is, did not change, BC would move parallel to itself in the lines BA and cd; but as the end C of the line BC comes out into a rare medium, which being of less resistance to light (for so he supposes), the point C moving with more facility than the point B, describes the curve CFH, while B moving in the dense medium with more difficulty, describes the lesser curve BEG; then the point C, being got to H, is re-immersed, and the line BC, being got to HG, goes on in the direction HK GL parallel to itself, drawing the beam after it in a rectilinear direction, after part of it has been bent within the glass, and part of it without.

Now if this be true, and Ppπ be a prism, Dr. D. asks what becomes of the line at EF, which unites the rays of the beam about the point of incidence I, when water is brought to touch the surface PP, as at AB, fig. 17? If it be said that water making a great resistance, though not so great as glass, the curve BEG deviates so little from the line BA, that the point E comes below I, and the beam is wholly refracted; he asks whence comes the faint image at k? If it be answered, that some part EI of the line EF, fig. 19, is turned up to the eye at E, fig. 17; what becomes of the lateral cohesion of light on which Rizzetti founds his chief proposition, and from which he draws his consequences?

It would be tedious, as well as useless, to be particular in showing all Rizzetti's mistakes; therefore the Dr. only mentions one more experiment from Sir Isaac Newton, which he repeated on account of what is said in Rizzetti's Preface, p. 16, viz. that if, according to Sir Isaac, rays were differently reflexible, colours must be produced by reflection from a plane surface; but this, says our author, is contrary to experience. Now this his assertion is disproved by

Exper. 9.—As this experiment was made exactly in Sir Isaac Newton's manner, and with the same success, the Dr. refers to his own account of it, in Book 1, Part 2, Exper. 16, as illustrated by fig. 20.

If this need any farther explanation, let us suppose CAB, the section of the prism in fig. 20, transferred to fig. 21, at ACB. If RO be a red ray, inclined to a perpendicular to AB, in an angle of more than 41 or 42 degrees, it will, at its emersion under the surface AB, be turned into the curve onmi, and so go up again to the eye at E; but another red ray coming in the direction rn, making an angle with the perpendicular sufficiently less, will after its emersion at n, be only bent so much as to be turned out of the way, and refracted to q, in an angle of refraction agreeable to the refrangibility of red light. But vm a violet ray, with the same inclination as the last red one rn, shall not be refracted, but turned up in the curve miP, and so go to the eye at E. Another violet ray vm, making an angle something less with the perpendicular, will pass through the glass, and be refracted in the line ms. On this account, all that part of the base of the prism, of which AB is the section, between A and p, will be dark or faint; all that part between p and n be tinged with a bluish colour; and all between o and B, of a bright white.

Postscript.—The bending of rays of light, just as they come to be reflected or refracted, may be easily understood by such as are well acquainted with those properties of light, which Sir Isaac Newton calls their fits of easy reflection, and fits of easy transmission; without any hypothesis, but by consequences fairly drawn from experiments and observations. But as Signior Rizzetti does not seem to have the least notion of those properties of light, and the nice observations on which they are founded; and several other persons have not time to read those parts of the optics, with sufficient application, to show how the same power of the surface of a dense medium may both attract and repel under different circumstances —— the Dr. contents himself here with giving the hypothesis, which Sir Isaac does before he comes to that part of his book where he demonstrates the fits abovementioned.

If GG, fig. 22, be the surface of a dense medium GDDG, on which a tremor is caused by the warmth communicated to it by the rays of light, so as to give a wave-like motion to the medium immediately next to the surface GG; as that vibratory motion is performed, the medium alternately pushes from the surface, and returns towards it, as is represented by the position of the darts in the figure, and pushes back the light, so as to reflect it when the vibration is contrary to its direction, but brings it down to be refracted when the vibration conspires with the said motion. See a further account of this in Sir Isaac Newton's Optics, Book 2, Part 3, Prop. 12.

The persons present at the experiments abovementioned, tried them as well as myself, and being satisfied with the success of them, allowed me to mention it, and make use of their names in this account.

The Method of making Tin-Plates, extracted from the Memoirs of the Academy of Sciences, for the Year 1725; *by William Rutty, M. D. R. S. Secr.* N° 406, p. 630.

The making of tin-plates, or latten, as it is called, being not commonly practised in England, though there is so great a consumption of it; either because the method is not sufficiently known, or because that in use to make small quantities, for particular purposes, is much too dear to answer the artificer's expectation in making larger; by which we are obliged to export our own tin to Germany, to receive it back again manufactured: it is thought not improper to describe the method the Germans make use of, as extracted from a Dissertation of Mr. De Reaumur, printed in the last volume of the Memoirs of the Academy of Sciences of Paris, in which also he lays down some improvements, as he thinks, of his own.

He takes notice then, that the making of tin-plates, called in France white iron, properly begins with preparing the leaves or plates of iron to be tinned, which are supposed to be sufficiently thin and flat, and cut into squares: but it is only certain sorts of iron that can be reduced into these leaves, of which those are the most proper, which when heated are easiest extendible, and yet can be forged with a hammer when cold; the more soft and extremely flexible, as well as the more brittle being rejected. These leaves are drawn from bars of iron, about an inch square; which being made a little flat, are cut into thin pieces, then folded together, and being made into parcels containing 40 leaves each, they are beaten all at once with a hammer of 600 to 700lb. weight. After this, the principal part of the whole art is to prepare these leaves; for the lightest dust, or the least rust on their surface, will prevent the tin from uniting with them. This is taken off by steeping the plates in acid waters, for a certain time, and when taken out, scouring them with sand. By this method a woman may clean more plates in an hour than the most expeditious workman can file in many days.

Of these waters the author mentions several; but what the Germans themselves used, and which they make a great secret of, he found to be only common water made eager with rye, which requires very little pains. For after they have ground the grain roughly, and pounded it, they leave it to ferment in common water for a certain time, and with a little patience they are sure to have an eager menstruum.

With this menstruum they fill troughs or tuns, into which they put piles of plates; and to make it grow eager the better, and to have more activity, they keep these vessels in vaults or stoves which have little air, and in which they keep lighted charcoal. The workmen go into these vaults once or twice in a day, either to turn the plates that they may be equally exposed to the action of the acid liquor, or to take out those that are sufficiently cleansed, or to put others in their room: and as the liquor is more acid, or the heat of the vault or stove is more intense, the plates are sooner cleansed; but it requires at least two days, and sometimes a great deal more.

This is the method which the Germans employed in the tin-works in France, constantly made use of, to prepare the iron-plates to receive the coat of tin: but as the author observed, that the constant attendance on them in the stoves was very laborious, the heat therein being almost insupportable to those who are not used to it, he proposes some other methods, which are attended with very little trouble, and as small, if not a less expence; and which on trial succeeded full as well.

Having therefore observed that the iron-leaves or plates are covered with a scale or layer, half vitrified by the fire, on which acids have none or very little effect, he imagined, that instead of dissolving the iron in these acid waters, it would be better to make it rust, and thereby put it in a condition to be easier cleansed from these scales; as rust is accompanied with a sort of fermentation and rarefaction, and the matter which rusts takes up a greater space, and raises up whatever opposes it. To this purpose he steeped iron plates in different eager menstruums, as in water in which alum, common salt and sal-ammoniac were separately dissolved; and others of the same iron he only dipped into the same waters, and instantly taking them out exposed them to the air. These latter were rusted by all of them, but sooner by that in which the sal-ammoniac was dissolved. After two days, during which every plate had been dipped into the menstruum but twice or thrice, he scoured them, and likewise those he had left to steep for that time; and comparing them together, found that those which had been only wetted at different times, cleansed better than those which were steeped; the rust covering all the surface of the latter without raising the scale; whereas in the former, as soon as one part of the metal is detached, it is attracted by the menstruum, and the surface is raised into blisters of rust.

These dissolvents, the author observes, though weak in themselves, yet produce the effect as well as the stronger, which are much dearer: but among the latter he prefers vinegar, which being very plentiful in France, may be used with little cost. For you need only dip each leaf into it, and take it out again immediately, leaving it afterwards in some moist place, and it will be scaled in 48 hours, if care be taken to repeat this 3 or 4 times in a day. The scaling will still be more expeditious, if you dissolve a little sal-ammoniac in the vinegar, a pound or two to a puncheon; for as the vinegar dissolves iron well, so sal-ammoniac rusts it sooner than any other salt: but this must be used very moderately, and the leaf must be left to steep in clean water, to dissolve any particles of it that may stick to its surface, which may otherwise make it rust after it is tinned. If you scale with vinegar, and want to do it at a less expence, you need only plunge the leaves once or twice at farthest, and when the vinegar is dried on the surface, sprinkle it with water; or dip them into it, and take them out immediately.

There are several other ways of making iron rust; as, keeping it in a moist cellar, exposing it to the dew, sprinkling it with simple water, several times in a day, which will still act quicker by dissolving sal-ammoniac in it. In those countries where the pyrites is common, the vitriolic waters will scale them soon enough, which are almost as cheap as common water: you need only heap the pyrites together, and leaving them to moulder in the air, make afterwards a lixivium with them and common water, which lie will have the desired effect.

But as the leaves of iron are sensibly much easier cleansed on one side than the other, the bad side rarely taking the brilliant polish in the tinning, but having always some spots, which happens because in the beating one side is more exposed to the action of the hammer, and is therefore better plained, the author again advises not to steep them, but only to moisten them, in order to make them rust, moistening that side only that wants it most: whereas if you steep them, as the bad side will take double or triple the time of the other, the acid menstruum will dissolve the surface, and occasion a loss of iron.

He next gives two cautions necessary to be followed; the first is in the management of the plates before they come to be prepared; which is in the beating of them, to change the place of each in its turn, that every one may receive the immediate action of the hammer, otherwise they will not extend equally: the second is to steep them in clay or fuller's earth, tempered with water, before heating them, to prevent their soldering with each other.

He then closes this part of the operation with remarking, that whichever of these methods is pitched on, whether the old one, of which he has learnt the secret, or any of the new, which he has here shown, it is absolutely necessary, after the plates are sufficiently scaled, to scour them with sand; and when no more black spots remain on their surface, to throw them into water, to prevent their rusting again, and leave them in it till the instant they are to be tinned, or blanched, as it is called.

This he observes is the very object of the whole art, and is kept as much a secret by the blancher, as the acid eroding menstruum is by the scaler: but the manner of doing it is thus. They flux the tin in a large iron crucible, which has the figure of a pyramid frustum with four faces, of which the two opposite ones are less than the two others. This crucible they heat only from below, its upper border being luted in the furnace quite round. The depth of the crucible always exceeds the length of the plates to be tinned, which are always put in downright, and the tin ought to swim over them.

For this purpose, artificers of different trades prepare the plates in different manners, which are all exceptionable: but the Germans he perceived made use of no preparation whatever, except putting the scoured plates into clean water, as just remarked; but when the tin is melted in the crucible, they cover it with a layer of a sort of suet, an inch or two thick, through which the plate must pass before it comes to the tin: the first use of which is to keep the tin from burning, and if any part should take fire, as the suet will soon moisten it, to reduce it to its natural state again. This suet is a composition, as the blanchers say, and is of a black colour, which the author thought might be given it with soot or the smoke of a chimney, only to spread a mystery over their work; but he found it true so far, that common unprepared suet was not sufficient: for after several attempts, there was always something wanting to render the success of the operation certain. The whole secret then of blanching lies entirely in the preparation of this suet: and this he at last discovered to consist only in first frying and burning it; which not only gives it the colour, but puts it into a condition to give the iron a disposition to be tinned, which it does surprisingly.

The tin itself ought to have a certain degree of heat; for if it be not hot enough, it will not stick to the iron; if too hot, it will cover it with too thin a coat, and the plates will have several colours, as a mixture of red, blue, and yellow, and the whole appear of a bad yellow cast. To prevent this, by knowing when the tin has a proper degree of heat, they might first make an essay with small pieces of the scaled plates, and they would learn from them when the tin is in proper order: but generally speaking, they dip the plates into tin that is more or less hot, according to the thickness they would have the coat to be of.

Some plates they only give one layer to, and these they plunge into tin that has a less degree of heat, than that into which they plunge those plates which they would have take two layers; as also when they give these the second layer, they put them into tin that has not so great a degree of heat, as that into which they were put the first time: besides which, it is to be observed, that the tin, which is to give the second coat, ought to be fresh covered with suet, but only with the common sort without preparation; for melted tin is sufficiently disposed to attach itself to solid tin; and in this case it is to tin itself, to which the new tin is to be joined.

A new Apparent Motion discovered in the Fixed Stars; its Cause assigned; the Velocity and Equable Motion of Light deduced. By the Rev. James Bradley, Savilian Professor of Astronomy at Oxford, and F. R. S. N° 406, p. 637.

The observations described in this paper, were first begun conjointly by Mr. Bradley and Mr. Sam. Molyneux, and after the death of the latter, by Mr. B. alone. They were at first begun in hopes of verifying and confirming those that Dr. Hook formerly communicated to the public, which seemed to be attended with circumstances that promised greater exactness in them, than could be expected in any other, that had been made and published on the same account. And as his attempt was what principally gave rise to this, so his method in making the observations was in some measure that which Mr. Molyneux followed: for he made choice of the same star, and his instrument was constructed on nearly the same principles. But if it had not greatly exceeded the doctor's in exactness, we might yet have remained in great uncertainty as to the parallax of the fixed stars.

This indeed was chiefly owing to Mr. Geo. Graham, to whom the lovers of astronomy are also not a little indebted for several other exact and well-contrived instruments. The necessity of such will scarcely be disputed by those who have had any experience in making astronomical observations; and the inconsistency to be met with among different authors in their attempts to determine small angles, particularly the annual parallax of the fixed stars, may be a sufficient proof of it to others. Their disagreement indeed in this article is not now so much to be wondered at, since it will appear very probable, that the instruments commonly used by them were liable to greater errors than many times that parallax will amount to.

The success then of this experiment evidently depending very much on the accurateness of the instrument, that was principally to be attended to.

Mr. Molyneux's apparatus was completed, and fitted for observing, about the end of November 1725, and on December 3 following, the bright star in the head of Draco, marked γ by Bayer, was for the first time observed, as it passed near the zenith, and its situation carefully taken with the instrument. The like observations were made on the 5th, 11th, and 12th days of the same month, and there appearing no material difference in the place of the star, a further repetition of them at this season seemed needless, it being a part of the year when no sensible alteration of parallax in this star could soon be expected. It was chiefly therefore curiosity that tempted Mr. Bradley, being then at Kew, where the instrument was fixed, to prepare for observing the star on Dec. 17, when having adjusted the instrument as usual, he perceived that it passed a little more southerly this day than when it was observed before. Not suspecting any other cause of this appearance, they first concluded that it was owing to the uncertainty of the observations, and that either this or the foregoing were not so exact as they had before supposed; for which reason they purposed to repeat the observation again, in order to determine from whence this difference proceeded; and on doing it on Dec. 20, Mr. B. found that the star passed still more southerly than in the former observations. This sensible alteration the more surprised them, as it was the contrary way from what it would have been, had it proceeded from an annual parallax of the star; but being now pretty well satisfied that it could not be entirely owing to the want of exactness in the observations; and having no notion of any thing else, that could cause such an apparent motion as this in the star, they began to think that some change in the materials, &c. of the instrument itself, might have occasioned it. Under these apprehensions they remained some time, but being at length fully convinced, by several trials, of the great exactness of the instrument, and finding by the gradual increase of the star's distance from the pole, that there must be some regular cause that produced it, they took care to examine nicely, at the time of each observation, how much it was: and about the beginning of March 1726, the star was found to be 20″ more southerly than at the time of the first observation. It now indeed seemed to have arrived at its utmost limit southward, because in several trials made about this time, no sensible difference was observed in its situation. By the middle of April it appeared to be returning back again towards the north; and about the beginning of June, it passed at the same distance from the zenith as it had done in December, when it was first observed.

From the quick alteration of this star's declination about this time, it increasing a second in 3 days, it was concluded, that it would now proceed northward, as it before had gone southward, of its present situation; and it happened as was conjectured; for the star continued to move northward till September following, when it again became stationary, being then near 20″ more northerly than in June, and no less than 39″ more northerly than it was in March. From September the star returned towards the south, till it arrived in December to the same situation it was in at that time 12 months, allowing for the difference of declination on account of the precession of the equinox.

This was a sufficient proof, that the instrument had not been the cause of this apparent motion of the star; but to find one adequate to such an effect seemed a difficulty. A nutation of the earth's axis was one of the first things that offered itself on this occasion; but it was soon found to be insufficient; for though it might have accounted for the change of declination in γ Draconis, yet it would not at the same time agree with the phænomena in other stars: particularly in a small one almost opposite in right ascension to γ Draconis, at about the same distance from the north pole of the equator; for, though this star seemed to move the same way, as a nutation of the earth's axis would have made it, yet changing its declination but about half as much as γ Draconis in the same time, as appeared on comparing the observations of both made on the same days, at different seasons of the year, this plainly proved that the apparent motion of the stars was not occasioned by a real nutation, since if that had been the cause, the alteration in both stars would have been nearly equal.

The great regularity of the observations left no room to doubt, but that there was some regular cause that produced this unexpected motion, which did not depend on the uncertainty or variety of the seasons of the year. On comparing the observations with each other, it was discovered, that in both the forementioned stars, the apparent difference of declination from the maxima, was always nearly proportional to the versed sine of the sun's distance from the equinoctial points. This was an inducement to think that the cause, whatever it was, had some relation to the sun's situation with respect to those points. But not being able to frame any hypothesis at that time sufficient to solve all the phænomena, and being very desirous to search a little further into this matter: Mr. B. began to think of erecting an instrument for himself at Wansted, that having it always at hand, he might with the more ease and certainty inquire into the laws of this new motion. The consideration likewise of being able, by another instrument, to confirm the truth of the observations hitherto made with Mr. Molyneux's, was no small inducement to him; but the chief of all was, the opportunity he should thereby have of trying, in what manner other stars were affected by the same cause, whatever it was. For Mr. Molyneux's instrument being originally designed for observing γ Draconis, in order, as before said, to try whether it had any sensible parallax, it was so contrived, as to be capable of but little alteration in its direction, not above 7 or 8 minutes of a degree, and there being few stars within half that distance from the zenith of Kew, bright enough to be well observed, he could not, with his instrument, thoroughly examine how this cause affected stars differently situated with respect to the equinoctial and solstitial points of the ecliptic.

These considerations determined him; and by the contrivance and direction of the same ingenious person, Mr. Graham, his instrument was fixed up August 19, 1727. As Mr. B. had no convenient place where he could use so long a telescope as Mr. Molyneux's, he contented himself with one of but little more than half the length of his, viz. of about 12½ feet, his being 24¼, judging from the experience which he already had, that this radius would be long enough to adjust the instrument to a sufficient degree of exactness; and he has had no reason since to change his opinion; for from all the trials he has yet made, he is well satisfied, that when it is carefully rectified, its situation may be securely depended on to half a second. As the place where the instrument was to be hung, in some measure determined its radius, so did it also the length of the arch, or limb, on which the divisions were made to adjust it; for the arch could not conveniently be extended farther, than to reach to about 6¼° on each side the zenith. This indeed was sufficient, since it gave an opportunity of making choice of several stars, very different both in magnitude and situation; there being more than 200 inserted in the British catalogue, that may be observed with it. Mr. B. needed not to have extended the limb so far, but that he was willing to take in Capella, the only star of the first magnitude that came so near his zenith.

The instrument being fixed, Mr. B. immediately began to observe such stars as he judged most proper to give light into the cause of the motion beforementioned. There was variety enough of small ones; and not less than 12 that he could observe through all the seasons of the year; these being bright enough to be seen in the day-time, when nearest the sun. Mr. B. had not been long observing, before he perceived that the notion they had before entertained of the stars being farthest north and south, when the sun was about the equinoxes, was only true of those that were near the solstitial colure; and after having continued his observations a few months, he discovered, what he then apprehended to be a general law, observed by all the stars, viz. that each of them became stationary, or was farthest north or south, when they passed over the zenith at 6 o'clock, either in the morning or evening. He perceived likewise, that whatever situation the stars were in, with respect to the cardinal points of the ecliptic, the apparent motion of every one tended the same way, when they passed the instrument about the same hour of the day or night; for they all moved southward, while they passed in the day, and northward in the night; so that each was farthest north when it came about 6 o'clock in the evening, and farthest south when it came about 6 in the morning.

Though Mr. B. afterwards discovered, that the maxima in most of these stars do not happen exactly when they come to the instrument at those hours, yet not being able at that time to prove the contrary, and supposing that they did, he endeavoured to find out what proportion the greatest alterations of declination in different stars bore to each other; it being very evident, that they did not all change their declination equally. Mr. B. before observed, that it appeared from Mr. Molyneux's observations, that γ Draconis altered its declination about twice as much as the forementioned small star almost opposite to it; but examining the matter more particularly, Mr. B. found that the greatest alteration of declination in these stars, was as the sine of the latitude of each respectively. This made him suspect that there might be the like proportion between the maxima of other stars; but finding that the observations of some of them would not perfectly correspond with such an hypothesis, and not knowing whether the small difference met with, might not be owing to the uncertainty and error of the observations, he deferred the further examination into the truth of this hypothesis till he should be furnished with a series of observations made in all parts of the year; which might enable him not only to determine what errors the observations are liable to, or how far they may safely be depended on, but also to judge, whether there had been any sensible change in the parts of the instrument itself.

On these considerations, Mr. B. laid aside all thoughts at that time about the cause of the forementioned phænomena, hoping that he should the easier discover it, when better provided with proper means to determine more precisely what they were.

When the year was completed, he began to examine and compare his observations; and having pretty well satisfied himself as to the general laws of the phænomena, he then endeavoured to find out the cause of them. He was already convinced, that the apparent motion of the stars was not owing to a nutation of the earth's axis. The next thing that offered itself, was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this, upon trial, proved insufficient. He then considered what refraction might do; but here also nothing satisfactory occurred. At last he conjectured, that all the phænomena hitherto mentioned, proceeded from the progressive motion of light and the earth's annual motion in its orbit. For he perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction, than that of the line passing through the eye and object; and that, when the eye is moving in different directions, the apparent place of the object would be different.

Mr. B. considered this matter in the following manner. He imagined CA to be a ray of light, falling perpendicularly on the line BD: fig. 1, pl. 7: then if the eye be at rest at A, the object must appear in the direction AC, whether light be propagated in time or in an instant. But if the eye be moving from B towards A, and light be propagated in time, with a velocity that is to the velocity of the eye, as CA to BA; then light moving from C to A, while the eye moves from B to A, that particle of it, by which the object will be discerned, when the eye in its motion comes to A, is at C when the eye is at B. Joining the points B,C, he supposed the line CB, to be a tube, inclined to the line BD in the angle DBC, of such a diameter, as to admit of but one particle of light; then it was easy to conceive, that the particle of light at C, by which the object must be seen when the eye, as it moves along, arrives at A, would pass through the tube BC, if it is inclined to BD in the angle DBC, and accompanies the eye in its motion from B to A; and that it could not come to the eye, placed behind such a tube, if it had any other inclination to the line BD. If instead of supposing CB so small a tube, we imagine it to be the axis of a larger; then for the same reason, the particle of light at C, could not pass through that axis, unless it is inclined to BD, in the angle CBD. In like manner, if the eye moved the contrary way, from D towards A, with the same velocity; then the tube must be inclined in the angle BDC. Although therefore the true or real place of an object is perpendicular to the line in which the eye is moving, yet the visible place will not be so, since that must be in the direction of the tube; but the difference between the true and apparent place will be, cæteris paribus, greater or less, according to the different proportion between the velocity of light and that of the eye. So that if we could suppose that light was propagated in an instant, then there would be no difference between the real and visible place of an object, though the eye were in motion; for in that case, AC being infinite with respect to AB, the angle ACB, the difference between the true and visible place, vanishes. But if light be propagated in time, which will readily be allowed by most of the philosophers of this age, then it is evident from the foregoing considerations, that there will be always a difference between the real and visible place of an object, unless the eye is moving either directly towards or from the object. And in all cases, the sine of the difference between the real and visible place of the object, will be to the sine of the visible inclination of the object to the line in which the eye is moving, as the velocity of the eye to the velocity of light.

If light moved but 1000 times faster than the eye, and an object, supposed to be at an infinite distance, was really placed perpendicularly over the plain in which the eye is moving, it follows from what has been already said, that the apparent place of such an object will be always inclined to that plain, in an angle of $89^\circ\ 56\frac{1}{2}'$; so that it will constantly appear $3\frac{1}{2}'$ from its true place, and seem so much less inclined to the plain, that way towards which the eye tends. That is, if AC is to AB, or AD, as 1000 to 1, the angle ABC will be $89^\circ\ 56\frac{1}{2}'$, and ACB $= 3\frac{1}{2}'$, and BCD $=$ 2ACB $= 7'$. So that according to this supposition, the visible or apparent place of the object, will be altered $7'$, if the direction of the eye's motion be at one time contrary to what it is at another.

If the earth revolve round the sun annually, and the velocity of light were to the velocity of the earth's motion in its orbit, which, may at present be supposed a circle, as 1000 to 1; then it is easy to conceive, that a star really placed in the very pole of the ecliptic, would, to an eye carried along with the earth, seem to change its place continually; and, neglecting the small difference on the account of the earth's diurnal revolution on its axis, would seem to describe a circle round that pole, every way $3\frac{1}{2}'$ distant from it. So that its longitude would be varied through all the points of the ecliptic every year; but its latitude would always remain the same. Its right ascension would also change, and its declination, according to the different situation of the sun in respect to the equinoctial points; and its apparent distance from the north pole of the equator, would be $7'$ less at the autumnal, than at the vernal equinox.

The greatest alteration of the place of a star in the pole of the ecliptic, or which in effect amounts to the same, the proportion between the velocity of light and the earth's motion in its orbit, being known; it will not be difficult to find what would be the difference on this account, between the true and apparent place of any other star at any time; and on the contrary, the difference between the true and apparent place being given, the proportion between the velocity of light and the earth's motion in its orbit may be found.

As Mr. B. only observed the apparent difference of declination of the stars, he takes no further notice in what manner such a cause as here supposed, would occasion an alteration in their apparent places in other respects; but, supposing the earth to move equally in a circle, it may be gathered from what has been already said, that a star which is neither in the pole nor plane of the ecliptic, will seem to describe about its true place, a figure insensibly different from an ellipse, whose tranverse axis is at right-angle to the circle of longitude passing through the star's true place, and equal to the diameter of the little circle described by a star (as before supposed) in the pole of the ecliptic; and whose conjugate axis is to its transverse, as the sine of the star's latitude to the radius. And allowing that a star by its apparent motion does exactly describe such an ellipse, it will be found, that if A be the angle of position, or the angle at the star made by two great circles drawn from it, through the poles of the ecliptic and equator, and B be another angle, whose tangent is to the tangent of A, as radius to the sine of the latitude of the star; then B will be equal to the difference of longitude between the sun and the star, when the true and apparent declination of the star are the same. And if the sun's longitude in the ecliptic be reckoned from that point where it is when this happens; then the difference between the true and apparent declination of the star, on account of the cause now considering, will be always as the sine of the sun's longitude from thence. It will likewise be found, that the greatest difference of declination, that can be between the true and apparent place of the star, will be to the semi-transverse axis of the ellipse, or to the semi-diameter of the little circle described by a star in the pole of the ecliptic, as the sine of A to the sine of B.

If the star have north latitude, the time, when its true and apparent declination are the same, is before the sun comes in conjunction with, or opposition to it, if its longitude be in the first or last quadrant, viz. in the ascending semicircle of the ecliptic; and after them, if in the descending semi-circle; and it will appear nearest to the north pole of the equator, at the time of that maximum, or when the greatest difference between the true and apparent declination happens, which precedes the sun's conjunction with the star.

These particulars being sufficient for the present purpose, it may be time enough to enlarge more on this head, when giving a description of the instruments, &c. if that be judged necessary to be done; and when what is now advanced is allowed of, as something more than a bare hypothesis.

This being premised, Mr. B. next proceeds to determine, from the observations, what the real proportion is between the velocity of light and the velocity of the earth's annual motion in its orbit; on supposition that the phænomena beforementioned depend on the causes here assigned. But first he remarks, that in all the observations hereafter mentioned, he has made no allowance for the change of the star's declination on account of the precession of the equinox, on supposition that the alteration from this cause is proportional to the time, and regular through all the parts of the year: having deduced the real annual alteration of declination of each star from the observations themselves; rather choosing to depend on them in this article, because all that have yet been made concur to prove, that the stars near the equinoctial colure, change their declination at this time 1½″ or 2″ in a year, more than they would do if the precession was only 50″, as is now generally supposed. Mr. B. has likewise met with some small varieties in the declination of other stars in different years, which do not seem to proceed from the same cause, particularly in those that are near the solstitial colure, which on the contrary have altered their declination less than they ought, if the precession was 50″. But whether these small alterations proceed from a regular cause, or are occasioned by any change in the materials, &c. of the instrument, he is not yet able fully to determine.

From what has been premised, it will appear that the greatest alteration of the apparent declination of γ Draconis, on account of the successive propagation of light, would be to the diameter of the little circle which a star would seem to describe about the pole of the ecliptic, as 39″ to 40″.4. The half of this is the angle ACB. This therefore being 20″.2, AC will be to AB, that is, the velocity of light to the velocity of the eye, which in this case may be supposed the same as the velocity of the earth's annual motion in its orbit, as 10210 to 1; from whence it would follow that light moves, or is propagated as far as from the sun to the earth, in 8^{m} 12^{s}.

It is well known, that Mr. Romer, who first attempted to account for an apparent inequality in the times of the eclipses of Jupiter's satellites, by the hypothesis of the progressive motion of light, supposed that it spent about 11 minutes of time in its passage from the sun to us: but it has since been concluded by others, from the like eclipses, that it is propagated as far in about 7 minutes. The velocity of light therefore deduced from the foregoing hypothesis, is as it were a mean between what had at different times been determined from the eclipses of Jupiter's satellites.

These different methods of finding the velocity of light thus agreeing in the result, we may reasonably conclude, not only that these phænomena are owing to the causes to which they have been ascribed; but also, that light is propagated, in the same medium, with the same velocity after it has been reflected, as before: for this will be the consequence, if we allow that the light of the sun is propagated with the same velocity, before it is reflected, as the light of the fixed stars. And this will scarcely be questioned, if it can be made appear that the velocity of the light of all the fixed stars is equal, and that their light moves, or is propagated, through equal spaces in equal times, at all distances from them: both which points appear to be sufficiently proved from the apparent alteration of the declination of stars of different lustre; for that is not sensibly different in such stars as seem near together, though they appear of very different magnitudes. And whatever their situations are, if we proceed according to the foregoing hypothesis, the same velocity of light is found from his observations of small stars of the 5th or 6th, as from those of the 2d and 3d magnitude, which in all probability are placed at very different distances from us. The small star, for example, before noticed, almost opposite to γ Draconis, being the 35th Camelopard. Hevelii in Mr. Flamsteed's catalogue, was 19″ more northerly about the beginning of March, than in September. Whence Mr. B. concludes, according to his hypothesis, that the diameter of the little circle described by a star in the pole of the ecliptic, would be 40″.2.

The last star of the great Bear's-tail of the 2d magnitude, marked η by Bayer, was 36″ more southerly about the middle of January than in July. Hence the maximum, or greatest alteration of declination of a star in the pole of the ecliptic, would be 40″.4, exactly the same as was before found from the observations of γ Draconis.

The star of the 5th magnitude in the head of Perseus, marked τ by Bayer, was 25″ more northerly about the end of December, than on the 29th of July following. Hence the maximum would be 41″. This star is not bright enough to be seen as it passes over the zenith about the end of June, when it should be according to the hypothesis farthest south. But because we can more certainly depend on the greatest alteration of declination of those stars, which have been frequently observed about the times when they become stationary, with respect to the motion now considered; Mr. B. set down a few more instances of such, from which we may be able to judge how near it may be possible, from these observations, to determine with what velocity light is propagated.

α Persei Bayero was 23″ more northerly at the beginning of January, than in July. Hence the maximum would be 40″.2. α Cassiopeæ was 34″ more northerly about the end of December, than in June. Hence the maximum would be 40″.8. β Draconis was 39″ more northerly in the beginning of September, than in March; hence the maximum would be 40″.2. Capella was about 16″ more southerly in August than in February; hence the maximum would be about 40″. But this star being farther from the zenith than those before used, he cannot so well depend on the observations of it, as of the others; because he meets with some small alterations of its declination, that do not seem to proceed from the cause now considered.

Mr. B. compared the observations of several other stars, and they all conspire to prove that the maximum is about 40″ or 41″. He therefore supposes it is 40½″, or, which amounts to the same, that light moves, or is propagated, as far as from the sun to us, in 8^{m} 13^{s}. The near agreement he met with among the observations induces him to think that the maximum, as here fixed, cannot differ so much as a second from the truth; and therefore it is probable, that the time which light spends in passing from the sun to us, may be determined by these observations within 5^{s} or 10^{s}; which is such a degree of exactness, as we can never hope to attain from the eclipses of Jupiter's satellites.

Having thus found the maximum, or what the greatest alteration of declination would be, in a star placed in the pole of the ecliptic, Mr. B. next deduces from it, according to the foregoing hypothesis, the alteration of declination in one or two stars, at such times as they were actually observed, in order to see how the hypothesis will correspond with the phænomena through all the parts of the year.

It would be too tedious to set down the whole series of the observations; he therefore makes choice only of such as are most proper for the present purpose, and begins with those of γ Draconis.

This star appeared farthest north about September 7, 1727, as it ought to have done according to the hypothesis. The following table shows how much more southerly the star was found to be by observation, in several parts of the year, and how much more southerly it ought to be according to the hypothesis.

		The difference of declination by observa.	The difference of declination by the hypo.			The difference of declination by observa.	The difference of declination by the hypo.
1727.	D.	″	″	1728.	D.	″	″
Oct.	20	4½	4½	March	24	37	38
Nov.	17	11½	12	April	6	36	36½
Dec.	6	17½	18½	May	6	28½	29½
	28	25	26	June	5	18½	20
1728.					15	17½	17
Jan.	24	34	34	July	3	11½	11½
Feb.	10	38	37	Aug.	2	4	4
March	7	39	39	Sept.	6	0	0

Hence it appears, that the hypothesis corresponds with the observations of this star through all parts of the year; for the small differences between them seem to arise from the uncertainty of the observations, which is occasioned he thinks chiefly by the tremulous or undulating motion of the air, and of the vapours in it; which causes the stars sometimes to dance to and fro so much, that it is difficult to judge when they are exactly on the middle of the wire, fixed in the common focus of the glasses of the telescope.

Mr. B. confesses, that the agreement of the observations with each other, as well as with the hypothesis, is much greater than he expected to find, before comparing them; and it may possibly be thought to be too great, by those who have been used to astronomical observations, and know how difficult it is to make such as are in all respects exact. But if it would be any satisfaction to such persons, he could assure them, that in above 70 observations made of this star in a year, there is but one (and that is noted as very dubious on account of clouds) which differs from the foregoing hypothesis more than 2″, and this does not differ 3″.

This therefore being the fact, he cannot but think it very probable, that the phænomena proceed from the cause he has assigned, since the foregoing observations make it sufficiently evident, that the effect of the real cause, whatever it is, varies in this star, in the same proportion, that it ought according to the hypothesis.

But least γ Draconis may be thought not so proper to show the proportion, in which the apparent alteration of declination is increased or diminished, as those stars which lie near the equinoctial colure; he gives also the comparison between the hypothesis and the obsesvations of η Ursæ Majoris, that which was farthest south about Jan. 17, 1728, agreeable to the hypothesis. The following table shows how much more northerly it was found by observation in several parts of the year, and also what the difference should have been according to the hypothesis.

	The difference of declination by observa.	The difference of declination by the hypo.		The difference of declination by observa.	The difference of declination by the hypo.
1727. D.	″	″	1728. D.	″	″
Sept. 14	29½	28½	April 16	18½	18
.... 24	24½	25½	May. . 5	24½	23½
Oct. . . 16	19½	19½	June. . 5	32	31½
Nov. 11	11½	10½	 25	35	34½
Dec. 14	4	3	July 17	36	36
1728.					
Feb. 17	2	3	Aug. 2	25	35½
March 21	11½	10½	Sept. 20	26½	26½

Mr. B. finds, on examination, that the hypothesis agrees altogether as exactly with the observations of this star, as the former; for in about 50 that were made of it in a year, there is no difference of so much as 2″, except in one, which is marked as doubtful on account of the undulation of the air, &c. And this does not differ 3″ from the hypothesis.

The agreement between the hypothesis and the observations of this star, is the more to be regarded, since it proves that the alteration of declination, on account of the precession of the equinox, is as before supposed, regular through all parts of the year; so far at least, as not to occasion a difference great enough to be discovered with this instrument. It likewise proves the other part of the former supposition, viz. that the annual alteration of declination in stars near the equinoctial colure, is at this time greater than a precession of 50″ would occasion: for this star was 20″ more southerly in September 1728, than in September 1727, that is, about 2″ more than it would have been, if the precession was but 50″.

Mr. B. thinks it needless to give the comparison between the hypothesis and the observations of any more stars; since the agreement in the foregoing is a kind of demonstration, that the hypothesis gives at least the true law of the variation of declination in different stars, with respect to their different situations and aspects with the sun. And if this is the case, it must be granted, that the parallax of the fixed stars is much smaller than has been hitherto supposed by those, who have pretended to deduce it from their observations. Mr. B. thinks he may venture to say, that in either of the two stars last mentioned, it does not amonnt to 2″. He thinks that if it were 1″, he should have perceived it in the great number of observations he made, especially of γ Draconis; which agreeing with the hypothesis, without allowing any thing for parallax, nearly as well when the sun was in conjunction with, as in opposition to, this star, it seems very probable that its parallax is not so great as one single second; and consequently that it is above 400000 times farther from us than the sun.

There appearing therefore after all, no sensible parallax in the fixed stars, the anti-copernicans have still room on that account, to object against the motion of the earth; and they may have, if they please, a much greater objection against the hypothesis, by which Mr. B. has endeavoured to solve the forementioned phænomena; by denying the progressive motion of light, as well as that of the earth.

Postscript.—As to the observations of Dr. Hook, Mr. B. owns, that before Mr. Molyneux's instrument was erected, he had no small opinion of their correctness; the length of his telescope, and the care he pretends to have taken in making them exact, having been strong inducements with him to think them so. And since he has been convinced, both from Mr. Molyneux's observations, and his own, that the Doctor's are really very far from being either exact or agreeable to the phænomena; he is greatly at a loss how to account for it. He cannot well conceive that an instrument of the length of 36 feet, constructed in the manner he describes his, could have been liable to an error of near 30″, which was doubtless the case, if rectified with so much care as he represents.

The observations of Mr. Flamsteed of the different distances of the pole star from the pole, at different times of the year, which were through mistake considered by some as a proof of its annual parallax, seem to have been made with much greater care than those of Dr. Hook. For though they do not all exactly correspond with each other, yet from the whole Mr. Flamsteed concluded that the star was 35″ or 40″ or 45″ nearer the pole in Dec. than in May or July: and according to Mr. B.'s hypothesis it ought to appear 40″ nearer in Dec. than in June. The agreement therefore of the observations with the hypothesis, is greater than could reasonably be expected, considering the radius of the instrument, and the manner in which it was constructed.

END OF VOLUME THIRTY-FIFTH OF THE ORIGINAL.

A Catalogue of the Fifty Plants from Chelsea-Garden, presented to the Royal Society, by the Company of Apothecaries, for the Year 1727, *pursuant to the Direction of Sir Hans Sloane, Bart. by Isaac Rand, F.R.S.* N° 407, p. 1. *Vol. XXXVI.*

An Account of the first Decade of a Book, entitled, Johannis Martyn Historia Plantarum rariorum. London,* 1728. *By Mr. Rand, F.R.S.* N° 407, p. 4.

In this work, Mr. Martyn, has had the plants of their natural size, exactly

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* "At the dawn of learning, (says Dr. Pultney, in his excellent Sketches of the Progress of Botany in England) the seeds of botany had been first sown in England by Dr. Turner, at Cambridge. They can scarcely, however, be said to have germinated until a century afterwards, under the fostering care of Mr. Ray. By his cultivation they took root, although not invigorated by public support. In the mean time, through the munificence of the Earl of Danby, Oxford experienced the benefit of a public institution in aid of this science, and botany flourished under the care of Morison. After his time, to the establishment of Dillenius, it languished; no publication marked its progress; and its history at Oxford is void of interesting facts. Nearly the same languor prevailed after the time of Mr. Ray at Cambridge, and botany attained no strength till the time of Dr. Martyn, who, under the patronage of the University, gave the first public lecture in that department in the year 1727."

"Of this learned Botanist I am now, in the order of time, to present the reader with some account: and here I find myself agreeably anticipated by the relation of his life and writings prefixed designed after the life, and with great accuracy and success painted in their proper colours. This curious invention was never more aptly applied, though

to his *Dissertations on the Æneids of Virgil,* printed in 1700, 12mo. and drawn up by his most respectable son, and successor in the professorship; with whose friendship and correspondence, I have on this occasion a sincere pleasure in acknowledging, I have long been honoured. Hence I shall briefly recite from these anecdotes, only the leading circumstances in the life of Dr. Martyn, as connected with his professional character; and conclude with a short account of his botanical writings."

"John Martyn was born in the city of London, Sept. 12, 1699, and was designed by his father for the profession of a merchant; but his early and strong propensity to learning and science, in the end over-ruled that design. He had from his youth an attachment to botany; and this taste was further excited by his acquaintance with Mr. Wilmer, afterwards demonstrator at Chelsea Garden; and confirmed by an intimacy with, and the countenance of Dr. Sherard, in the year 1719. In the year 1720, he translated from the French, Dr. Tournefort's *History of the Plants growing about Paris*; and having projected a like catalogue of the plants about London, he collected, with unwearied diligence, the native plants of the environs; making for this purpose sometimes very extensive excursions, and almost ever on foot. He had once conceived a method from the *seed-leaves,* and had sown a great number of seeds in order to observe the difference between them. He early became acquainted with Dillenius, and co-operated with him in forming a society of botanists, which consisted of seventeen members. This society kept together till the year 1726. He continued, during the years 1723 and 1724, to make his excursions in search of plants more frequent, and extended them farther, into Middlesex, Surrey, Essex, and Kent. At the same time he studied insects, continued his observations on the seed-leaves, and made many others on the sexes of plants. He had, several years before this time, translated from the Latin, an Ode on that subject, presented to Camerarius, and printed in that author's epistle *De Sexu Plantarum.* The translation may be seen in *Blair's Botanic Essays.*"

"In the summer of 1724 he travelled into Wales, by Bath and Bristol, returning by Hereford, Worcester, and Oxford; by which he extended the objects of his studies, and augmented his collection of English plants; insomuch, that at length it comprehended 1400 specimens."

"In 1725 and 1726 he read lectures in botany in London, and was recommended by Dr. Sherard and Sir Hans Sloane, to exercise the same function at Cambridge, where, on the death of Mr. Bradly, he was chosen Professor of Botany; and continued to give lectures for several years, until the want of a garden, and his long absence from the business of physic, which he had engaged in, rendered it incommodious to him."

"In 1727, Dr. Martyn was admitted a member of the Royal Society; and was so active in the Committee for regulating the Library and Museum, in 1731, that he had his bond for annual payment cancelled by an order of council, as an acknowledgment of his services."

"In 1730, he was admitted of Emanuel College, with an intention to have proceeded regularly with the degrees in physic; but his marriage, and his attention to the practice of the profession, prevented him from finishing his design. In the mean time he read lectures in Botany and the Materia Medica, both at Cambridge and in London, in the years 1730 and 1731. In the beginning of the year 1733, he was elected Professor of Botany by the unanimous voice of the University."

"Dr. Martyn had practised physic for three years in the city, but on account of an asthmatic complaint, removed in the year 1730 to Chelsea, where he continued in the exercise of that profession until his retirement to Streatham in 1752. In 1761, he resigned his professorship; and soon after, in gratitude for the favour of having chosen him, and his son after him, to this post, he presented to the University his botanical library, consisting of upwards of 200 volumes; his Hortus this seems to be the first time it has been used in Botany. By this means, without a long tedious description, a plant may be known by mere inspection. However, that nothing may be wanting, the author has thought fit to give short descriptions, insisting more particularly on those minute parts which cannot be so clearly expressed by sculpture: and has added, where they could be obtained, some account of their uses, &c.

An Attempt to solve the Phenomenon of the Rise of Vapours, the Formation of Clouds, and the Descent of Rain. By J. T. Desaguliers, LL. D. F. R. S.

N° 407, p. 6.

Dr. Niewentyt and some others say, that particles of fire separated from the sun-beams, by adhering to particles of water, make up moleculæ, or small bodies specifically lighter than air, which, by hydrostatical laws, must rise and form clouds, that remain suspended when they are risen up to such a height, that the air about them is of the same specific gravity with themselves.

That rain is produced by the separation of the particles of fire from those of water, which last being then restored to their former specific gravity, can no longer be sustained by the air, but must fall in drops. See Niewentyt's Religious Philosopher, Contemplation 19, from Sect. 13 to Sect. 25.

Now this is liable to several objections. 1st. It is built on a supposition

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Siccus of exotics, containing 2600 specimens; near 250 drawings of Fungi; his collection of seeds and seed-vessels; and his Materia Medica."

He removed to Chelsea about a year before his death, which event took place on the 29th of January 1768.

He was the author of the following publications:

Tabulæ Synopticæ Plantarum Officinalium ad Methodum Raianam dispositæ, 1726, fol.

Methodus Plantarum circa Cantabrigian nascentium, 1727, 12mo.

Historia Plantarum Rariorum Decades Quinque. Fol. max. 1728, 1732. This was the most sumptuous and magnificent work of the kind that had ever been attempted in England.

Tournefort's History of Plants growing about Paris, with their uses in Physic, 2 vols. 8vo. 1732.

The Compleat Herbal of Dr. Tournefort, with large additions from Ray, Gerard, &c. &c. 2 vols. 4to. This is a translation of Tournefort's Institutions, accompanied by plates, copied from the original. It proceeded no farther than the 2d volume. It is a work of great merit; and with the assistance of the plates, must have greatly tended to facilitate the study of Botany in England.

The papers published by Dr. Martyn in the Philosophical Transactions we need not here enumerate, since they will appear in their respective periods of time.

Dr. Martyn was also greatly concerned in preparing the old Abridgments of the Philos. Trans. having, in conjunction with Mr. Eanes, published the Abridgments from the year 1719 to 1733, in 2 vols. being the 6th and 7th; and by himself prepared the remainder, till the year 1752, in 4 other volumes, being the 8th, 9th, 10th, and 11th volumes of the Abridgment.

He was also connected with Mr. Chambers in publishing an Abridgment of the Memoirs of the Academy of Paris, in 5 vols. 8vo. in 1742.

that fire is a particular substance, or distinct element; which has never yet been proved by experiment or observation; and which Mr. Hales, in his Vegetable Statics, has shown to be an ill grounded opinion; making it very plain that, in chemical operations, those bodies which had been thought to become heavier by particles of fire adhering to them, were only so by the adhesion of particles of air, &c. which he has shown to be absorbed in great quantities, by some bodies, while it is generated, or reduced from a fixed to an elastic state, by others; nay, that it may be absorbed and generated successively by the same body, under different circumstances.

2dly. If we should allow the abovementioned supposition, the difficulty will still remain about the production of rain by the separation of the fire from the water; for Dr. Niewentyt ascribes this effect to two different causes. First, to Condensation, (Sect. 23,) Saying, " That when contrary winds blow against the same cloud, and drive the watery particles together, the fire that adhered to them gets loose, and they, becoming then specifically heavier, precipitate and fall down in rain." Then in the very next Sect. he ascribes it to rarefaction, when he says, " That when a wind, blowing obliquely upwards, causes a cloud to rise into a thinner air, i. e. specifically lighter than itself, the fire which, by sticking to the particles of water rendered them lighter, extricates itself from them, and ascending by its lightness, the water becomes too heavy, not only to remain in this thin and light air, but even in a thicker and heavier near the earth, and so will be turned into a descending dew, mist, or rain, or snow, or the like, according as the watery vapours are either rarefied or compressed."

The first of these causes of rain is contrary to experience: for when two contrary winds blow against each other, over any place of the earth, the barometer always rises, and we have fair weather. For then, as Dr. Halley says, in Philos. Trans. N° 183, the air being accumulated above, becomes specifically heavier about the clouds, which, instead of falling into rain, as Dr. Niewentyt supposes, ascend up into such a part of the atmosphere, as has the air of the same specific gravity with themselves.

And if the falling of rain might be attributed to the second of these causes, then every time a cloud is encompassed with air specifically lighter than itself, rain must necessarily follow; whereas one may often see the clouds rise and fall without rain, even when the barometer shows the weight of the air to be altered. For that happens only when, by the great diminution of the specific gravity of the air about the cloud, it has a great way to fall; in which case, the resistance of the air, which increases as the square of the velocity of the descending cloud, causes the floating particles of water to come within the power of each other's attraction, and form such large drops as, being specifically heavier than any air, must fall in rain. For no gentle descent of a cloud, but only an accelerated motion downwards, produces rain.

Not however that the quick descent of a cloud is the only cause of rain: for the shock from a flash of lightning, and the sudden return of the air, after the vacuum made by the flash, will condense the floating vapour into water; and also the same cloud which, in the free air, might be carried horizontally without being turned into rain, meeting with a high hill in its way, will be condensed and fall in drops; especially if, in the day-time, it be driven by the wind out of the sun-shine, against the shaded side of the mountain.

There is another opinion concerning the rise of vapours, namely, that though water be specifically heavier than air, yet if its surface be increased by very much diminishing the bulk of its particles, when once raised, it cannot easily fall; because the weight of each particle diminishes as the cube of its diameter, and the surface to which the air resists, only as the square of the said diameter: that we see this in the dust in summer, and in menstruums that sustain metals dissolved, which are specifically heavier than the menstruums.

But this will not explain the phenomenon; because, though the increase of surface, the weight remaining the same, will in a great measure retard the descent of small bodies moving in the air, by its great resistance to so large a surface; it will for the same reason also hinder the ascent. For the rise of dust is owing to the motion of animals feet in it, or to the wind: whereas vapours rise in calm weather, as well as windy; neither do they, like the dust, always fall to the ground when the wind ceases to blow.

The third opinion, and which is most commonly received, is, that by the action of the sun on the water, small particles of water are formed into hollow spherules filled with an aura, or finer air highly rarefied, so as to become specifically lighter than common air, and consequently that they must rise in it by hydrostatical laws. As for example, if a particle of water, as it becomes a hollow sphere, be only increased 10 times in diameter, its bulk will be increased 1000 times; therefore it will then be specifically lighter than common air, whose specific gravity is to that of water, as 1 to 850; then if the density of the aura, or spirit within the little shell, be supposed 9 times less than that of air, or as 50 to 850, that specific gravity of the shell and its contents, will be to that of air, as 900 to 1000; therefore such an aqueous bubble must rise till it comes to an equilibrium in air, whose density is to the density of that in which it began to rise, as 850 to 945 nearly. But it appears by experiments, that air rarefied by a heat which makes a retort red hot, is only increased in bulk, or dilated, 3 times; by the heat of boiling water only $\frac{10}{14}$ or nearly two thirds; and by the heat of the human body, such as will raise vapours plentifully, only $\frac{10}{39}$ or about $\frac{1}{4}$. The Dr. owns that his objection may be answered, by supposing the spherule of water to be more increased in diameter, as for example 20 times; because then if it be filled with air only $\frac{1}{4}$ rarer than common air, it will be specifically lighter, and capable of rising to a considerable height.

To give this solution all its force, let us express it in numbers. Let A and W, fig. 4, pl. 7, represent a particle of air, and one of water of equal bulk; then will the weight of A be to the weight of W, as 1 to 850, their bulks being equal. If the particle of water be blown up into a bubble (w) of 20 times its diameter, then will its bulk be to its weight, as 8000 to 850, while a sphere of air (a) of the same size, has its weight as well as bulk equal to 8000: now if an air or aura, $\frac{1}{4}$ rarer than common air, be supposed within the watery bubble, to keep it blown, it will be the same as if $\frac{3}{4}$ of the air of a was carried into w, and then the weight of w would be increased by the number 6000; so that the shell of water being in bulk 8000, would be in weight 850 + 6000 = 6850, while an equal bulk of air weighed 8000, and consequently the watery bubble would rise till it came to an air, whose density is to the density of the air next to the surface of the exhaling water, as 6850 to 8000.

This is the strongest way of stating the hypothesis. But to support it, the following queries must be answered.

Query 1. How comes the aura, or air in the bubbles, to be specifically lighter than the air without them, since the sun's rays, which act on the water, are equally dense all over its surface?

Query 2. If it could be possible for a rarer air to be separated from the denser ambient air, to blow up the bubbles, as bubbles of soaped water are blown up by warm air from the lungs, while the ambient air is colder and denser, what would hinder that cold air by its greater pressure, from reducing the bubbles to a less bulk, and greater specific gravity than the air, especially since cold can be communicated through such thin shells, and the tenacity of common water is very small when compared to that of soaped water, whose bubbles, notwithstanding that tenacity, are soon destroyed by the pressure of the outward air, as the air within them cools?

Query 3. If we should grant all the rest of the supposition, yet this difficulty will remain: if clouds are made up of hollow shells of water filled with air, why do not those clouds always expand when the ambient air is rarefied, and presses less than it did before; and also suffer a condensation, as the ambient air is condensed by the accumulation of the superior air?

If this condensation and rarefaction should happen to the clouds, they would always continue at the same height, contrary to observation; and we should never have any rain.

From all this it follows, that the condensation and rarefaction of the vapours, which make clouds, must depend on another principle than the condensation and rarefaction of the air: and that there is such a principle, the Dr. endeavours to show.

LEMMA. *The Particles of all Fluids have a repellent force.*—Fluids are elastic or unelastic: the elastic fluids have their density proportionable to their compression; and Sir Isaac Newton has demonstrated (Princip. lib. 2, sect. 5) that they consist of parts that repel each other from their respective centres. Unelastic fluids, like mercury, water, and other liquors, are by experiments found to be incompressible; for water in the Florentine experiment could not by any force be compressed into less room, but oozed like dew through the pores of the hollow golden ball in which it was confined, when a force was applied to press the ball out of its spherical, into a less capacious figure. Now this property of water, and other liquors, must be entirely owing to the centrifugal force of its parts, and not its want of vacuity; since salts may be imbibed by water without increasing its bulk, as appears by the increase of its specific gravity. So metals, which (singly) have a certain specific gravity beyond which they cannot be condensed, will yet receive each other in their interstices, so as to make a compound specifically heavier than the heaviest of them; as is experienced in the mixture of copper and tin.

Scholium.—By increasing the repellent force of the particles, an unelastic or incompressible fluid may become elastic, or a solid, at least a great part of it may be changed into an elastic fluid; and, vice versâ, by diminishing the repellent force, an elastic fluid may be reduced to an unelastic one, or to a solid. That the particles of quicksilver, water, and other liquors, are likewise endued with an attractive force, is evident from those substances running into drops in an exhausted receiver, as well as in the air, and likewise their adhering to other bodies. The attraction and repulsion exert their forces differently: the attraction only acts on the particles, which are in contact, or very near it: in which case it overcomes the repulsion so far as to render that fluid unelastic, which otherwise would be so; but it does not wholly destroy the repulsion of the parts of the fluid, because it is on account of that repulsion that the fluid is then incompressible. When by heat or fermentation the particles are separated from their contact, the repulsion grows stronger, and the particles exert that force at great distances; so that the same body shall be expanded into a very large space by becoming fluid, and may sometimes take up more than a million of times more room than it did in a solid or incompressible fluid. (See the Queries at the end of Sir Isaac Newton's Optics.) Thus is water by boiling, and less degrees of heat, changed into an elastic vapour, rare enough to rise in air; oils and quicksilver in distillation are made to rise in a very rare medium, such as remains in the red-hot retort; and sulphureous steams will rise even in an exhausted receiver, as the matter of the aurora borealis does in the thinner part of the atmosphere. If aquafortis be poured on quicksilver, a reddish fume will rise, much lighter than common air; so also will fumes rise from filings of metals, from vegetables when they ferment by putrefaction; and, as Mr. Hales has shown, several solid substances by distilling, as well as fermentation, will generate permanent air.

That heat will add elasticity to fluids, is evident from numberless experiments, especially from distilling and chemistry: but what is needful to consider here, is only that it acts more powerfully on water than common air; for the same heat which rarefies air only $\frac{2}{3}$, will rarefy water very near 14000 times, changing it into steam or vapour as it boils it: and in winter, that small degree of heat, which in respect to our bodies appears cold, will raise a steam or vapour from water, at the same time that it condenses air.

By a great many observations made by Mr. Henry Beighton, F. R. S. and by the Doctor, on the engine to raise water by fire, according to Mr. Newcomen's improvement of it, they found that the water in boiling is expanded 14000 times, to generate a steam as strong (i. e. as elastic) as common air, which therefore must be near $16\frac{1}{2}$ times specifically lighter. And that this steam is not made of the air extricated out of the water is plain, because it is condensed again into water by a jet of cold water spouting in it; and the little quantity of air that comes out of the injected water must be discharged at every stroke, otherwise the engine will not work well. There is also another experiment to confirm this, as follows.

Exper. 1.—ABCD, fig. 5, is a pretty large vessel of water, set on the fire to boil. In this vessel must be suspended the glass bell E, made heavy enough to sink in water, but put in, in such a manner that it be filled with water when upright, without any bubbles of air at its crown within, the crown being all under water. As the water boils, the bell will by degrees be emptied of its water, being pressed down by the steam which rises above the water in the bell; but as that steam has the appearance of air, in order to know whether it be air or not, take the vessel off the fire, and draw up the bell by a string fastened to its knob at top, till only the mouth remains under water; then, as the steam condenses by the cold air on the outside of the bell, the water will rise up into the bell at F quite to the top, without any bubble above it; which shows that the steam which kept out the water was not air.

N. B. This experiment succeeds best when the water has been first purged of air by boiling, and the air-pump.

We know by several experiments made on the fire engine, in Captain Savery's way, where the steam is made to press immediately on the water, that steam will drive away air, and that in proportion to its heat, though in the open air it floats and rises in it like smoke.

Now if the particles of water, turned into steam or vapour, repel each other strongly, and repel air more than they repel each other; aggregates of such particles, made up of vapour and vacuity, may rise in air of different densities, according to their own density depending on their degree of heat, without having recourse to imaginary bubbles formed in a manner that is only supposed, and not proved, as has been already shown.

Dr. D. owns indeed, that if the watery particles had no repellent force, they must precipitate in the same manner that dust will do, after it has been raised up; but we have too many observations and experiments to leave any doubt of the existence of the repellent force abovementioned. And it cannot be shown by any experiment, how large the moleculæ of vapour must be, which exclude air from their interstices, and whether those moleculæ vary in proportion to the degree of heat, by an increase of repellent force in each watery particle, or by a further division of the particles into other particles still less; but in general we may reasonably affirm, that the rarity of the vapour is proportionable to the degree of its heat, as it happens in other fluids (see Phil. Trans. N° 270) and that, though the different degrees of the air's rarefaction are also proportionable to the heat, the same degree of heat rarefies vapour much more than air.

Now to show that what has been said will account for the rise of vapours and the formation of clouds, we must only consider; —— whether that degree of heat, which is known to rarefy water 14000 times, being compared with several of those degrees of heat in summer, autumn, and winter, which are capable of raising exhalations from water or ice; the rarity of the vapours, estimated by the degree of heat, will appear to be such, that the vapour will rise high enough in winter, and not too high in summer, to agree with the known phænomena.

That the effects are adequate to the causes in this case, the Doctor thinks can be made out in the following manner, viz. The heat of boiling water, according to Sir Isaac Newton's table (Phil. Trans. N° 270) is 34; the mean heat of summer 5; the mean heat of spring or autumn 3; and the least degree of heat, at which vapours rise in winter, or the mean heat of winter, is 2. The rarity of vapour proportionable to these four degrees of heat is 14000, 2058, 1235, and 823. The rarity of air is, in summer 900, in spring or autumn 850, and in winter 800; the density of water, compared with the abovementioned densities, being inversely as one to the said forementioned four numbers. The heights above the earth to which the vapours will rise, and at which they will be in equilibrio, in air of the same density with themselves, will vary according to the rarity of the vapour depending on the heat of the season. For the vapour which is raised by the winter's heat, expressed by the number 2, when the air's rarity is 800, will rise to, and settle at, a height of about the 6th of a mile, when the barometer is above 30 inches high. But if the heat be greater then, the vapours will rise higher, and pretty much higher if the sun shines, though in frosty weather, the barometer being then very high. If the barometer falls, and thereby brings the place of equilibrium, for vapours raised by the heat 2, nearer the earth, then also will the heat be increased, the vapour more rarefied, and consequently the new place of equilibrium sufficiently high. It is to be observed, that in winter, when the heat is only equal to 2, the air is densest close to the earth, which has not any heat sufficient to rarefy it near the ground, as happens in warm weather; therefore the vapour will rise gradually in air whose density decreases continually from the earth upwards; neither will the vapour be hindered of its full rise, by any condensation from a greater cold of the ambient air, the air being then as cold next to the ground, where the vapour begins to rise, as it is at any height from the earth.

The vapour which is raised by the heat of spring or autumn, expressed by the number 3, will rise to the height of 3½ miles, when the barometer is at 30, and the air's rarity is 850. But then, as the air is hotter nearer the ground than at the height of half a mile or a mile, the vapour will condense as it rises; and as the air, when the earth is heated, is rarer near the ground than at some height from it, the place of equilibrium for vapour will, on these two accounts, be brought much lower than otherwise it would be, as for example, to the height of about a mile, which will agree with phænomena.

In summer, the two causes abovementioned increasing, the vapour raised by the heat 5, whose place of equilibrium would be 5½ miles high, if the vapour after it began to rise was not condensed by cooling, and the air was densest close to the earth, will settle at the height of about 1½ or 2 miles, which is also agreeable to phænomena.

Lastly, as the density and rarity of the vapour is chiefly owing to its degree of heat, and in a small measure to the increased or diminished pressure of the circumambient air, when it is not confined; and the density and rarity of the air is chiefly owing to the increased or diminished pressure, by the accumulation or exhaustion of superior air, while heat and cold alter its density in a much less proportion; the clouds made of the vapours abovementioned, instead of conforming themselves to the altered density of the ambient air, will rise when it is condensed, and sink when it is rarefied; and also rise or sink, when the pressure of the air is not altered, and its density very little changed, by their own dilatation, owing to heat or cold; as may be observed often, by seeing them change their height considerably, while the barometer continues exactly at the same degree, and the thermometer's liquor rises or falls very little, and sometimes not at all.

As for the manner how clouds are changed into rain, the Doctor has hinted it in the beginning of this paper: but for further satisfaction, he refers the reader to Dr. Halley's account of it, in the Phil. Trans. N° 183, in which Dr. D. entirely acquiesces, having always found it agreeable to the phænomena.

Since the Doctor has, for brevity sake, only mentioned at what heights from the surface of the earth, vapours of different densities will come to an equilibrium, without giving a reason for settling the place of equilibrium, at those heights; he here gives the method by which they are to be found, viz. As the vapours will settle and rise where the air is of the same density with themselves; it is only required to find the density of the air at any distance from the earth, at several heights of the barometer; which may be deduced from Dr. Halley's two tables, Phil. Trans. N° 386, the first showing the altitude to given heights of the mercury, and the second the heights of the mercury at given altitudes, and knowing the degree of heat by the thermometer, because the density of the vapour depends on the degree of heat of the season; provided that proper allowances be made for the great rarefaction of the air near the earth in hot and dry weather, and the condensation of the vapours in their rise, by reason of the air being colder at a little height above the earth, than just at its surface.

An Account of some Observations relating to Natural History, made in a Journey to the Peak in Derbyshire. By Mr. J. Martyn, F. R. S. N° 407, p. 22.

The Peak in Derbyshire is famous for seven places, which our ancestors have deemed wonders: 1. Chatsworth, a magnificent seat of his Grace the Duke of Devonshire; 2. Mam-tor; 3. Elden-hole; 4. The ebbing and flowing well; 5. Buxton well; 6. Peak's-hole; and 7. Pool's-hole.

The first being a work, not of nature, but art, does not come within the design of this account. Mam-tor is a huge precipice facing the east, or south-east, which is said to be perpetually shivering, and throwing down great stones on a smaller mountain below it: and yet, neither the one increases, nor the other decreases in size. This mountain is chiefly composed of a sort of slate-stone, called in that country black shale, and great stone. The nature of the black shale is such, that though it be very hard before it is exposed to the air, yet it is afterwards very easily crumbled to dust. Thus, on any storm, or melting of snow, this shale is considerably wasted; and as the large stones are gradually disengaged, they must necessarily fall down. That it is only at these times that the mountain wastes, is affirmed by the most intelligent of the neighbouring inhabitants; and that this decay is not perpetual, Mr. M. can affirm of his own knowledge; having not only taken a close survey of it, but also climbed up the very precipice, without seeing any other shivering in the mountain than what the treading of his own feet in the loose crumbled earth occasioned.

Elden-hole is a huge perpendicular chasm of unknown depth. Mr. Cotton says, that he sounded 884 yards, and yet the plummet drew. But he might easily be deceived, unless his plummet was of a very great weight; for otherwise the weight of a rope of that length, would be so great, as to make the landing of the plummet scarcely perceivable. Be that as it may, the depth is doubtless very considerable; and as we have no where in England so good an opportunity of searching the bowels of the earth to so great a depth; it is extraordinary no curious person has ever had the courage to venture down. It is said indeed, that a poor fellow was hired to be let down with a rope about his middle, 200 yards; and that he was drawn up again out of his senses, and died a few days after. But probably if any intelligent and prudent person was to be let down in a proper machine, he would not be much in danger, and his fatigue would be very inconsiderable.

The ebbing and flowing well is far from being regular, as some have pretended. It is very seldom seen by the neighbours themselves; and Mr. M. waited a good while at it to no purpose.

Buxton-well has been esteemed a wonder, on account of two springs, one warm and the other cold, rising near each other. But the wonder is now lost, both being blended together. The spring which is now used for bathing, appears to be 32½ degrees of one of Mr. Hauksbee's thermometers warmer than the common spring water there.* The spring water kept the spirit of wine at 41, the Bath water raised it to 80½.

Peak's-hole and Pool's-hole are two remarkable horizontal openings under mountains, the one near Castleton, the other just by Buxton. They seem to have owed their origin to the springs which have their current through them. It is easy to imagine, that when the water had forced its way through the horizontal fissures of the strata, and had carried the loose earth away with it, the

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* The temperature of the Buxton thermal spring is 82 by Fahrenheit's thermometer.

loose stones must of course fall down; and that where the strata had few or no fissures, they remained entire, and so formed those very irregular arches so much wondered at in these places: which seems a more probable origin, than what others have hitherto proposed. The three rivers, as they are commonly called, in Peak's-hole, are only some parts of the cave deeper than the rest, and receiving all their water from the spring which comes from the farther end of the cave. The water which passes through Pool's-hole is impregnated with particles of lime-stone, and so has incrusted almost the whole cave in such a manner, that it appears like one solid rock.

The lead mines in Derbyshire are very various with regard to their courses. One, into which Mr. M. went down, had two branches; one running to the N. E. the other to the N. W: and, as he was informed, one of the best they ever discovered ran due north. Their breadth and depth are full as irregular. The bodies dug through to come at the vein, are generally lime-stone and black shale. But it is uncertain which of the two is uppermost. Of two mines into which he went down, in one they had dug first through 26 yards of lime-stone, then through one of black shale: in the other, first through 42 yards of shale, and then through 28 of lime-stone. The substances found mixed with the ore, are

1. Chert. This is a kind of flint, which Dr. Woodward, in his Method of Fossils, p. 21, says is so called, when found in thin strata. But in the peak, the strata of chert are often 4 yards thick, or more. They are found in lime-stone, and not always disposed in strata. Those which he took notice of, were generally either black, or of such a colour as the inspissated juice of the buck-thorn berries, which the painters call by the name of sap-green: whence they are called green cherts and black cherts.

2. Spar. This is composed of crystal mixed with other bodies. Those called sugar-spars, have their crystallizations very small; and thus, on crumbling to pieces have the appearance of powdered sugar. There were two sorts of these; white and blue.* Dog-tooth spar is a white pointed spar, in form and colour something resembling teeth.

3. Cauk. This Dr. Woodward says, is a coarse talcky spar. But in that substance which Mr. M. met with in this country, under the name of cauk, he could not discover any flexibility or elasticity, which that learned writer has set down as characteristics of talck and talcky bodies. It seems to be nothing but spar incorporated with a coarse earthy matter.† When this cauk is mixed with

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* Fluor spar, a compound of lime, fluoric acid, and water. Fluate of lime.

† It is now known that cauk consists of the barytic earth and the vitriolic or sulphuric acid. In the new chemical nomenclature, this compound is termed sulphate of barytes.

pellucid crystallizations of spar, it is called bastard cauk. There are other bodies mixed in the mines with lead ore; but Mr. M. did not meet with them.

When the ore is brought up from the mine, it is broken to pieces, that the spar, cauk, or other bodies which adhere to it, may be more easily separated. It is then thrown into a large sieve and washed, and so further purified from extraneous bodies. After this, it is carried to the furnace, to be smelted. The furnace, which Mr. M. saw near Worksworth, was very rude and simple, consisting only of some large rough stones, placed in such a manner as to form a square cavity, into which the ore and coals are thrown stratum super stratum; two great bellows continually blowing the fire, being moved alternately by water. He saw no other fuel used on this occasion, but dried sticks, which they call white coal. Mr. Ray informs us, in his Collection of English Words, ed. 2, p. 174, that they use both white and black coal, or charcoal, in Cardiganshire; perhaps because that ore is harder to flux; the charcoal making a more vehement fire. They generally throw in some spar along with the ore, which it is thought, by imbibing the sulphur, makes it flux more easily. They frequently throw in also some coak, or cinders of pit-coal, because they think it attracts the dross, and so makes it separate easier from the lead. When the ore is melted, it runs out at an opening in the bottom part of the front of the furnace, through a small channel made for that purpose, into a cylindrical vessel, out of which it is laded into the mould. The dross of the ore, on smelting, is called slag. This slag is afterwards smelted again with coak only, and the lead obtained from it is called slag-lead. Their way of making red-lead is the same with Mr. Ray's account, ibid. p. 200; only they use three parts of lead, and one of slag-lead; and think that the red-lead thus made, is better than if made without slag-lead.

The Difference of Longitude, in Time, of diverse Places, computed from Observations of the Eclipses of Jupiter's Satellites. By the Rev. Mr. Derham, F.R.S. N° 407, p. 33.

Rome and Lisbon: $1^h\ 24^m\ 46^s$; 1 25 34; 1 26 34; 1 29 0; 1 26 44; 1 26 54; 1 28 11.—Rome and Paris: $0^h\ 39^m\ 48^s$; 0 40 50; 0 36 16; 0 38 56; 0 40 17.—Rome and Ingolstad: $0^h\ 2^m\ 51^s$; 0 4 1.—Rome and Bologne: $0^h\ 3^m\ 45^s$; 0 2 16; 0 4 45; 0 4 14.—Rome and Kew: $0^h\ 45^m\ 47^s$.—Rome and Wansted: $0^h\ 49^m\ 10^s$.—Rome and Upminster: $0^h\ 47^m\ 28^s$.—Rome and Southwick in Northamptonshire: $0^h\ 47^m\ 58^s$.—Urbino and Lisbon: $1^h\ 28^m\ 57^s$.—Paris and Lisbon: $0^h\ 45^m\ 46^s$; 0 45 44.—Paris and Bologne: $0^h\ 34^m\ 30^s$; 0 34 0; 0 38 32.—Ingolstad and Lisbon: $1^h\ 22^m\ 53^s$; 1 23 21.—Ingolstad and St. Quirico: $0^h\ 1^m\ 20^s$; 0 1 40.—Ingolstad and Bologne: $0^h\ 1^m\ 53^s$.—

Ingolstad and Paris: $0^{h}\ 36^{m}\ 23^{s}$; 0 36 00.——Ingolstad and Upminster: $0^{h}\ 46^{m}\ 10^{s}$.—St. Quirico and Lisbon: $1^{h}\ 22^{m}\ 30^{s}$.—St. Quirico and Paris: $0^{h}\ 37^{m}\ 40^{s}$.—St. Quirico and Upminster: $0^{h}\ 47^{m}\ 50^{s}$.—Florence and Lisbon: $1^{h}\ 19^{m}\ 43^{s}$.—Florence and Bologne: $0^{h}\ 0^{m}\ 31^{s}$.—Florence and Upminster: $0^{h}\ 42^{m}\ 1^{s}$.—Upminster and Bologne: $0^{h}\ 43^{m}\ 43^{s}$.—Upminster and Lisbon: $0^{h}\ 37^{m}\ 42^{s}$.—Bologne and Lisbon: $1^{h}21^{m}28^{s}$.—Bologne and Albano: $0^{h}\ 3^{m}\ 43^{s}$.

Observations of the Eclipses of Jupiter's Satellites, M. Bianchini at Rome, and other Places: with Accounts received by him from other Places. N° 407, p. 35.

These observations are not now of any use.

Observations of the Eclipses of Jupiter's Satellites, from July 10, 1726, *to April* 12, 1728, *at Petersburgh. By M. de Lisle,* at that Place.* N° 407, p. 37.

1726.	d.	h.	m.	s.	What eclipse.		Telescope.	
July	10	12	47	0	Im. of the 1st		15-foot.	a little doubtful.
Aug.	9	14	51	30	Immer.	1	15 and 22	doubtful near 15^{s}.
	18	11	15	46	Immer.	1	15	
		11	15	52	Immer.		20½	
Sept.	10	11	32	51	Immer.	1	15	
		11	32	56	Immer.		20½	
	22	16	13	20	Immer.	2	15	
Oct.	19	12	21	46	Em.	1	15	
	28	8	47	8	Em.	1	15	to some seconds.
Dec.	6	7	11	18	Em.	1	20½	somewhat doubtful
		10	30	31	Em.	2	20½	exact.
		10	30	38	Em.		15	exact.
	29	7	15	36	Em.	1	20½	exact.
		7	15	48	Em.		15	exact.
1727.								
Jan.	2	10	59	46	Immer.	3	15	air foggy.
		11	0	17	Immer.		20½	
	7	10	9	56	Em.	2	20½	
		10	10	4	Em.		15	

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* Joseph Nicolas De Lisle, a celebrated astronomer and philosopher, was born at Paris in 1688. He was invited to Russia in 1726, where he remained till 1747: and he died in 1768, at 80 years of age. He belonged to all the learned academies in Europe, and was the intimate friend of Newton and Halley. He was the inventor of a particular form of mercurial thermometer, which he constructed in 1733. He was author of very numerous memoirs in the Paris Academy and elsewhere; but his chief work is Memoirs of the History of Astronomy.

	d.	h.	m.	s.	What eclipse.		Telescope.	
Feb.	1	7	17	15	Em.	2	20½-foot	exact.
Aug.	5	11	52	23	Immer.	3	22	to a few sec.
	7	10	59	27	Immer.	1	22	♃ was low.
	8	13	37	9	Immer.	2	22	
	21	14	50	30	Immer.	1	22	
	30	11	19	18	Immer.	1	22	
Sept.	2	10	43	57	Immer.	2	22	
	6	13	11	24	Immer.	1	22	doubtful.
	9	13	21	35	Immer.	2	22	
	10	9	34	30	Emmer.	3	22	
	15	9	36	32	Immer.	1	22	
Oct.	31	10	8	48	Immer.	1	15	doubtful.
Dec.	2	8	46	30	Em.	1	22	
	12	14	6	0	Em.	3	22	
1728.								
Jan.	8	12	14	44	Em.	2	15	to some sec.
		12	33	34	Em.	1	13	somewhat doubtful
	10	5	58	7	Em.	3	22	to some sec.
		7	0	12	Em.	1	22	wind incommoded.
	17	7	56	31	Im.	3	13	
		8	53	4	Em.	1	22	exact.
		9	55	14	Em.	3	22	
Feb.	16	10	59	26	Em.	1	22	to some sec.
	18	5	28	20	Em.	1	15	the day not closed.
	27	6	40	5	Em.	2	22	
	29	8	0	29	Immer.	3	22	the satellite appeared and disappeared at different times.
March	10	11	18	19	Em.	1	13 and 15	
April	12	8	16	12	Immer.	3	15	
		10	30	40	Em.	3	15	♃ was low.

Queries, concerning the Cause of Cohesion of the Parts of Matter. By Fr. Triewald, Director of Mechanics in the Kingdom of Sweden. N° 408, p. 39.

Query 1.—Does not the strong cohesion of two balls of lead prove the doctrine of attraction, worthy its great author, Sir Isaac Newton; and that there is a universal attraction between the parts of matter in nature, though some at such small distances as to escape our observations, since we cannot make their parts touch each other close enough, so as to come within their sphere of activity?

Query 2.—Having often found the touching surfaces of such leaden balls, as near as could be measured, much alike; yet the force of cohesion very different: nay, the touching surfaces have been very small, yet sometimes 114 to 126lb. weight has not been sufficient to separate them; when at other times a far less weight, though the measure of touching surfaces far exceeded those mentioned, was more than sufficient to cause their separation. Does it not prove that the cohesion is strongest according to the closeness of the contact, but not as the touching surfaces? for which reason the cohesion is always strongest, when a little twist is given in joining them; since by this means the particles must come closer together, than by squeezing the balls barely on each other, though it was done with a far greater force than could be applied with the bare hands. And since the force, twist, and touching surfaces can never be alike and mensurable when joined by hand, it will be very difficult, if not impossible, to ascertain the forces of this cohesion, which is incredible, and far exceeds magnetical attractions.

Query 3.—Does not this experiment fairly account for the cohesion of the parts of matter; and that this firm cohesion cannot be derived from any glue or cement, any imaginary hooks and funiculus, nor de gravitate etheris: but that the particles of all solid and fluid bodies attract each other by a certain force, which acts most intensely the nearer they touch each other.

Our Dahlkarlians have, time out of mind, practised an experiment, when they have had occasion to remove any unwieldy stones of the hardest rocks, and so large as not to be moved entire by any strength they could apply. They practise the following means, not only to cleave and split them into as many parts and pieces as they please, but they obtain stones with one or more smooth sides, fit for use in buildings. Their method is thus:

They take tallow, grease, train oil, or any other fat substances, with which they draw lines on such large stones, according as they would have them split, and think proper; then they lay either charcoal or wood at top, and round the sides of the stone, so that it is all over covered, and then kindle the fuel; which when burned out, they find the stone divided according to the lines they have drawn on it, with some of the beforementioned fat substances, which seldom or never fails.

May we not account for this odd phenomenon thus? That as the action of heat and fire expands the parts of all hard and solid bodies, and metals themselves; so when the action of the fire about the stone has made the particles of the same recede farther from each other, than when in their natural state, the

the oily substances insinuate themselves more and more between the particles of the stone; by which means, when the stone cools again, and shrinks, they seem to prevent these particles from coming as close, and within their sphere of activity, as the remaining particles may, where no such foreign matter has been applied; by which means they also cannot attract each other so strongly as the rest, and must therefore remain separated. Fat and oily substances seem to be most fit for this purpose, as they are endued with a repelling force.

Of the Nature and Virtues of the Holt Waters, from the Rev. Mr. J. Lewis, Vicar of the Place. N° 408, p. 43.

Experience has proved the Holt waters to be of admirable efficacy in scorbutic and scrophulous cases. They are of an attenuating, astringent, and drying nature: by which qualities they probably perform their cures. This latter quality, it is probable, they derive from the alum and iron supposed to impregnate them. The ingredients, which give them their drying, absorbing and healing quality, are the sulphur and ochre; by which they imbibe the peccant humours, and sheath the sharp salts, that lance and tear the finer glands, and cause blotches, and ulcerations. As they attenuate and astringe, they are a noble diuretic, removing obstructions from the kidneys, and causing the renal glands to make their due secretions, and at the same time dissolving the grosser salts, and fitting them to be carried off through the urinary passages.

A further Account of a new Machine, called the Marine Surveyor, contrived for the Mensuration of the Way of a Ship at Sea, more correctly than by the Log. By Mr. Henry de Saumarez, of the Island of Guernsey. N° 408, p. 45.

In fig. 1, pl. 8, F represents the boat, through the rudder of which a small spindle passes, in an iron pipe, of which HG is the length. To the point G are fastened the 4 iron fins, or flyers, A, B, C, D, in a square form, the bars DB and AC to which they are fixed, lying in an horizontal position. These flyers are so contrived, as to have full play in any motion of the boat. To the point H, which is the upper part of the pipe and spindle, is fixed the dial E. Now the boat being put into motion, the flyers move accordingly, which proportionally affecting the spindle, the motion is thereby communicated to the dial, which may be fitted to strike the miles or leagues the vessel runs.

But to describe the first movement of this machine more exactly, fig. 2, represents it unfixed. The cross, or bars DB and AC, lie flat, or in an horizontal position; the arbor or perp. spindle, screws into the point G, and passes through an iron pipe to the dial, as follows. The flyers A, B, C, D being fitted to move in any motion of the boat, the bars are accordingly affected. This instrument is so contrived, that two of the flyers on one side always resist the water in the motion of the vessel, while the other two give way in their turning. The resisting flyers in this figure are A and B, and D and C will be the same when they come into their position; for they resist and give way alternately so long as the motion continues, which is always circular; and so truly does it revolve, that be the motion swift or slow, in any measured distance, the number of revolutions will be equal.

But considering, that though this projection might be serviceable in barges, pleasure-boats, or other vessels, in fair and moderate gales of wind, yet it might prove useless in boisterous and stormy weather, and in long voyages, when it might be choaked with weeds; Mr. S. therefore fixed to his other invention the fork, which is contrived in such a manner, that it will determine the ship's way in a storm, or when she is scudding before the wind, when the log is incapable of it.

Fig. 3 shows the fork in 5 different positions, or motions of the vessel. This needs no explanation, for it plainly appears, that the pallets will be more or less affected by the resistance of the water, according to the position they are in; and therefore the revolutions in a swift or slow motion, in the same distance, cannot be equal, as had been represented by Dr. Desaguliers, Mr. Samuel Molyneux, and Mr. Gravesande.

Being now fully persuaded that the fork would not revolve equally in the same distance, and in different motions of the vessel, Mr. S. now began to repair this defect by calculating some tables, which render it still a very useful instrument.

Fig. 4 shows a further improvement, in which the objections of the different inclinations of the fork are now entirely removed. AFGH is the fork, in the same form as the iron fork described in the Philos. Trans. Vol. xxxiii, which differs from the other only in the materials of which it is framed; this being contrived of such as to make it equiponderous with the water, and to lie in an horizontal position in all cases. HB is a rope, of a convenient length, fixed to a screw or worm at the point B, which goes about 6 inches into an iron pipe, of which BI is the length: through this pipe an iron spindle passes into the screw or worm to which the dial C is fixed. As soon then as the vessel moves, the fork plays in a horizontal position, which moving the spindle within the iron pipe, the motion is communicated to the dial, which is fitted to strike to the miles or leagues the vessel runs. And whether the vessel move swift or slow, the pallets A and F are equally affected, and consequently must measure the distance sailed to a greater exactness than the iron fork is capable of, in the manner formerly described.

A second Letter from Mr. Colin M^cLaurin, F. R. S. to Martin Folkes, Esq. Concerning the Roots of Equations, with the Demonstration of other Rules in Algebra; the Subject continued from N° 394. N° 408, p. 59.

The chief and most useful part of this paper is printed p. 145 of this volume.

An Account of the Cinnamon Tree in Ceylon, and its several Sorts. Communicated by the Chief Inspector of the Cinnamon Trade and Manufacture in that Island to Albertus Seba,† Druggist at Amsterdam. Translated by the late Dr. Scheuchzer, F. R. S.* N° 409, p. 97.

The best sort of cinnamon, which grows in great plenty in Ceylon, and is peculiar to that island, is called by the natives rasse coronde, which is as much as to say, sharp, sweet cinnamon. This choice sort is exported yearly by the Dutch East India Company, by whom it has been prohibited under severe penalties, that no other sort whatever should be mixed with it.

The 2d sort is called canatte coronde, that is, bitter and astringent cinnamon; for the Ceylonese, in their language, call cinnamon in general coronde, and canatte signifies bitter and astringent. The bark of this tree comes off very easily, and smells very agreeably when fresh, but has a bitter taste. It is an advantage to us, that it grows not in great plenty hereabouts, as one might easily mistake it for a better; as indeed, in general, it requires a good deal of

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* The cinnamon-tree, of which there are probably some varieties, is the *laurus cinnamomum* of Linnæus.

† Albert Seba, an apothecary, at Amsterdam, must have been born towards the decline of the 16th century. He was the collector of one of the most extensive museums ever formed by an individual. This he caused to be described, and published in the most magnificent manner, in 4 volumes large folio. The first volume, which comprises a miscellaneous assemblage of plants, birds, quadrupeds, and amphibia, was published in 1734; the second, which is chiefly confined to snakes, in 1735; the third, containing fishes, mollusca, shells, zoophytes, &c. in 1758; and the last, comprising insects and minerals, in 1765. Some copies of this work, which bears for its title *Thesaurus Rerum Naturalium,* were coloured. It must be confessed to be, on the whole, the most splendid and copious work extant on natural history at the period of its publication. The engravings are executed with great strength and spirit, and many of them have been frequently copied into other publications of a similar nature. The descriptions, which are, (in most copies,) in Latin and French, are rather general than scientific, especially those of the first two volumes: those of fishes were drawn up by Artedi. After the death of Seba, his museum was sold and dispersed; but his executors had been so negligent in the preservation of the insects, that they were nearly destroyed.

skill and attention so to distinguish the cinnamon trees from each other, as not to choose now and then a worse sort for the best. The root of this 2d tree yields a very good sort of camphor.

The 3d sort is called capperoe coronde, which is as much as to say, camphorated cinnamon, because it has a very strong smell and taste of camphor. It grows plentifully enough in the island, but not in the eastern parts of it. However, they find means now and then to send it over privately, and sell it to the Danes and English, who come to trade on the coasts of Coromandel; for as long as there is but one port in the island left open, abundance of this sort of bad merchandise may be exported. Besides, there is a sort of canella, growing on the continent of India, about Goa, which is very like this sort of cinnamon tree, though it has nothing of the true cinnamon. The same sort of canella agrees in many things with the canella malabarica sylvestris, a wild cinnamon tree, growing on the coasts of Malabar. And though with regard to the shape of the tree, and the outward appearance of the bark and leaves, there is very little difference to be observed between these two sorts of canella, and the abovementioned first and good sort of cinnamon, yet the latter is vastly superior in richness, virtue, and sweetness.

The 4th sort of cinnamon is called welle coronde, that is, the sandy cinnamon, because on chewing it, one feels as it were, bits of sand between the teeth, though in fact there is nothing sandy in it. The bark of this tree comes off easily enough, but is not so easily rolled up into a fibular form, as other sorts of cinnamon are, being apt to burst open, and to unfold itself. It is of a sharp and bitterish taste, and its root yields but a small quantity of camphor.

The 5th sort is called sewel coronde, sewel in the Ceylonese language signifying mucilaginous or glutinous. This sort of cinnamon acquires in drying a very considerable degree of hardness, which on chewing of it sufficiently shows itself. It has otherwise but little taste, and an ungrateful smell; but its colour is very fine, and the natives, who are all blacks, mix a good deal of this mucilaginous cinnamon along with the first and best sort, the colour of both being very much alike, excepting only, that in the good sort there are some few yellowish spots appear towards the extremities.

The 6th sort is called nieke coronde, the tree which bears it, having a good deal of resemblance to another tree, called nieke gas, and the fruit it bears nieke. The bark of this sort of cinnamon tree has no taste or smell, when taken off, and is used by the natives only in physic. For by roasting it they obtain a water and oil, with which they anoint themselves, thus thinking to keep off all sorts of noxious fumes and infections in the air. They likewise express a juice out of its leaves, which they say cools and strengthens the brain, by rubbing the head with it.

The 7th sort is called dawel-coronde, that is, drum-cinnamon, in Low Dutch trommel-caneel; the reason of which name is, because the wood of this tree, when grown hard enough, is light and tough, and is that sort of which the natives make some of their vessels and drums, which they call dawel. The bark is taken off, when the tree is yet growing, and is of a pale colour; the natives use it in the same manner as the 6th sort.

The 8th sort is called catte coronde, the thorny or prickly cinnamon: catte, in the Ceylonese language signifying a thorn or prickle; accordingly this tree is very prickly. The bark is in some measure like cinnamon, but the leaves differ very much, and the bark itself has nothing either of the taste or smell of cinnamon. The natives use the root, bark, and leaves of this tree in physic, applying them in form of cataplasms, to tumours and swellings arising from a thick corrupt blood, which they say it cures in a short time.

The 9th sort is called mael coronde, or the flowering cinnamon, because this tree is always in blossom. The flowers come nearest to those of the first and best sort, called rasse coronde, but they bear no fruit, which the other does. The substance of the wood becomes never so solid and weighty in this as in the other cinnamon trees abovementioned, which are sometimes 8, 9, or 10 feet in circumference. If this everflowering cinnamon tree be cut or bored into, a limpid water issues out of the wound, as out of the European birch tree; but it is of no use, no more than the leaves and bark.

The inhabitants of Ceylon say, that there is still another sort of cinnamon, which they call toupat coronde, or the three-leaved cinnamon. It does not grow in that part of the country which the Dutch East India Company is possessed of, but higher up towards Candia.

All the several sorts of cinnamon trees must grow a certain number of years before the bark is fit to be taken off; with this difference however, that some of the trees of the same sort, as for instance of the first and best, will ripen 2 or 3 years sooner than others, owing to the difference of the soil they grow in. Those, for instance, which grow in valleys, where the ground is a fine whitish sand, will in 5 years time be fit to have the bark taken off; others, on the contrary, which stand in a wet slimy soil, must have 7 or 8 years to grow, before they are ripe enough. Again, those trees are later which grow in the shade of other larger trees, by the sun being kept from their roots; and hence also it is, that the bark of such trees has not that sweetness and agreeable taste observable in the bark of those trees which grow in a white sandy ground, where with little wet they stand fully exposed to the sun, but is rather of a bitterish taste, something astringent, and smells like camphor.

For by the heat of the sun's rays the camphor is made so thin and volatile, that it rises up and mixes with the juices of the tree, where it undergoes a small fermentation, and then rising still higher between the substance of the wood and the thin inner membrane of the bark, it is at last so effectually diffused through the branches and leaves, that there is not the least traces of it to be perceived any where. Mean while that thin and glutinous membrane, which lines the bark on the inside, between it and the substance of the wood, attracts and sucks in all the purest, sweetest, and most agreeable particles of the juice, leaving the thick and gross ones, which are pushed forward, and serve to nourish the branches, leaves, and fruit.

For if the bark be fresh taken off, that juice which remains in the tree has a bitterish taste, not unlike that of cloves. On the contrary, the inner membrane of the bark, when fresh taken off, has a most exquisite sweetness, extremely agreeable to the taste; whereas the outward part of the bark differs but very little in taste from the common trees; which shows plainly that all its sweetness is owing only to the inner membrane. But when the bark is laid in the sun, to its being dried and wound up, this oily and agreeable sweetness of the inner membrane communicates and diffuses itself also throughout the whole outward part, and imbues it so strongly, as to make the bark a commodity, which for the fragrancy of its smell, and the sweetness of its taste, is coveted all over the world.

The bark may be taken off from trees which have stood 14, 15, or 16 years, according to the quality of the soil they stand in; but beyond that time they grow thicker, and gradually lose their taste and agreeable sweetness, which makes the bark have more of the taste of camphor; besides, the bark is then grown so thick, that if it be laid in the sun, it will no longer shrink and wind itself up, but remain flat.

And here it may be thought a fit subject of inquiry, how it comes to pass, that, considering what vast quantities of cinnamon have been exported from this island, and sold all over the world, by the Europeans as well as natives, not only for these 200 years last past, there are yet such numbers of good trees fit to be barked remaining in the island and growing there every year? Now in order to solve this question, several authors, who have described the island of Ceylon, have committed a considerable mistake, when they assure their readers, that when the bark has been stripped off the tree, it grows again in 4 or 5 years, and becomes fit to be stripped a second time. For this assertion is entirely contrary to the course of nature and observation: nor is it to be believed that there is any one tree whatever, in any part of the world, which, if it was entirely stripped of its bark, could subsist and grow any longer; that part at least where the bark has been taken off quickly becomes dry, and so dies away; but the root remains entire and in good condition; and this shows the reason why there is such a number of trees fit to be barked every year. For though the cinnamon tree, after the bark has been once taken off, is cut down to the very root, as they do in Europe oaks, birch trees, alders and willows, yet the root quickly pushes forth new shoots, which ripen in 5, 6, 7, or 8 years, some sooner, some later, and then yield their quantity of the bark.

Hence it appears how far the old roots are instrumental to the growth and plenty of cinnamon trees, but the fruit which falls from the trees, contributes very much towards the same end; and it is particularly owing to a certain kind of wild doves, which from their feeding on the fruit of the cinnamon tree, they call cinnamon-eaters, that these trees grow so plentifully in this island; for the doves, when they fetch food for their young ones, flying here and there, disperse vast quantites of the fruit all over the fields, which occasions the rise of many thousand young trees, seen here and there along the roads in such quantities together, that they look like a little wood.

There is hardly any thing so universally grateful, and esteemed by all nations, as true cinnamon. The oil drawn out of it by fire is reckoned one of the strongest cordial medicines; the camphor which comes out of the root, is likewise of great use in several distempers; as are also the oil of camphor, a very costly thing; the leaves of the tree, and the oil distilled out of them; and lastly, the fruits with their oil. In short, there is no part of the cinnamon tree but what is of some singular use or other in physic.

Albertus Seba further adds, that the flowers of the cinnamon are as large as the Italian bean flowers, and of a blue colour. That the oil which is expressed out of the fruit of the cinnamon tree, as also that which is boiled out of them, is of a very good consistence and white, and is by the East India Company called cinnamon wax, because the King of Candia causes candles to be made of it, which for their agreeable scent are burnt only by himself and at his court. However he permits his subjects to express the juice out of another fruit, not unlike the fruit of the cinnamon tree; but this juice being only a thin fat substance, like oil of olives, they can only burn it in lamps.

The Indians use this cinnamon wax also in physic, giving it inwardly in luxations, fractures, falls, contusions and bruises, that in case any inward part be touched or bruised, it may by its balsamic virtues heal them. They give it also in bloody fluxes, to one drachm or a drachm and a half. Outwardly applied, it makes the skin more beautiful, smoother, and softer, than any one known sort of pomade.

The leaves of the cinnamon tree yield also an oil, which is of a bitterish taste, resembling oil of cloves mixed with a little good oil of cinnamon. It is an aromatic, and is reckoned an excellent remedy in headachs, pains of the stomach, and other distempers.

The oil of the root of the cinnamon tree is, properly speaking, an oil of camphor, the roots affording a good quantity of camphor. If this oil be distilled in glass vessels, there comes over along with it, that sort of camphor which the Indians call camphor baros, or camphor of borneo, which shoots in thin transparent crystals, forming a beautiful variety of trees on the recipient, not unlike those which, in very frosty weather, are seen on windows. This sort of camphor is of very great efficacy in physic, and is gathered and kept for the King of Candia's own use, who esteems it an excellent cordial medicine. But not only the camphor of baros, but also the oil of camphor, which is drawn out of the roots of the cinnamon tree, is a very great cordial, if taken inwardly; it strengthens the stomach, expels wind, and has been found of great use in arthritic and gouty disorders: it is also a diuretic. The dose is 10 or 12 drops on a bit of sugar, or in a proper vehicle. Outwardly it is applied in all arthritic pains from cold and obstructions, being rubbed on the affected part with a warm hand, and it will presently lessen the pain, and by degrees take it off.

It is also very good in burns. No sooner is a bandage, or compress, dipped into this spirit, applied to the affected part, but it will give instant relief, and so effectually check the inflammation, that it will creep no farther. But the application of it must be continued till the pain is quite gone, and the ulcus, if there has been any, is dried up. If the exulceration is got deeper, and the wound must be kept open, 2 ounces of camphor dissolved in oleo hyperici, mixed with a pound of the common unguentum cerussæ, applied according to art, will quickly and effectually heal it, as Mr. Seba has often experienced.

The Bills of Mortality in several Parts of Europe, for the Years 1724 *and* 1725. *Extracted from the Acta Breslaviensia. By Dr. J. G. Scheuchzer.* N° 409, p. 110.

For the year 1724. At Breslaw, died, married men 231; married women 148; widows and widowers 154; bachelors 57; maidens 66; children to 10 years of age, boys 417, girls 326; stillborn, boys 36, girls 31. Total 1466. Christened, males 709; females 613. Total 1322. Married 386 pairs.

In Vienna, buried 5524, among whom 3 of 90, 2 of 92, 2 of 93, 5 of 95, 2 of 96, 1 of 97, 3 of 98, 1 of 99, 2 of 100, 3 of 101, 2 of 103, 1 of 106, 27 of 90 and upwards. Christened 4427.

At Lübau, buried 135; christened 166; married 38 pairs.

At Lauban, christened, boys 107; girls 116. Total 223. Buried, married men 29; married women 19; bachelors 7; maidens 15; children, boys 68; girls 61; infants 23; stillborn 17; widows and widowers 18, in all 257. Among which there died 97 children of the small-pox. Married 73 couple.

At Dresden, christened, boys 715; girls 712; bastards, boys 60, girls 70. Total 1557. Buried, married men 161; married women 151; widowers 35; widows 143; bachelors 72; maidens 71; children, boys 522, girls 508; stillborn, boys 58, girls 40. Total 1761. Being 204 more than were christened. Married 413 couple.

At Leipsic, christened in all 913. Boys 491; girls 422. Buried, married men 122; married women 81; bachelors 51; maidens 44; children, boys 234, girls 210; women in childbed 17; infants, boys 59, girls 28; stillborn, boys 33, girls 28; widows and widowers 54. Total 961. Married 276 couple.

At Erfurt, christened 659; buried 612: married 188 couple.

At Salfeld, christened 119; buried 90; married 24 couple.

At Gera, buried, married men 28; married women 15; widowers 4; widows 22; bachelors 6, maidens 4; children, boys 76, girls 42; including 10 stillborn. Total 197. Christened, boys 153; girls 143; including 6 twins, one of which was stillborn. Total 296. Married 82 couple.

At Berlin, christened 2798; buried 2492; married 864 couple.

In all the King of Prussia's dominions, 84910 born; 21182 couple married; 61182 buried. Among the christened were 2215 bastards. Among the dead 66, who lived to 90, and upwards as far as 100.

At Regenspurg, among the protestants, christened, boys 172; girls 126. Total 298. Among which were 5 twins. Buried, married men 34, married women 46, including 13 widows, and 4 that died in child-bed; bachelors 13; maidens 7; children, boys 79, girls 110. Total 289. Married 68 couple.

At Amsterdam, buried 7622; married 2294.

At Venice, buried 4590; born 5046.

At Copenhagen, christened, boys 1306; girls 1183. Total 2489. Buried, men 486; women 343; boys 991; girls 931. Total 2751. Being 262 more than were born, and 837 more than there died in this city the year before; this extraordinary mortality was ascribed to the small-pox being very rife. Married couple 748.

At Dantzic, christened 1999; married couple 488; buried 1872, or 377 more than the year before.

Bills of mortality for the year 1725. At Breslaw. died, married men 259; married women 153; widows and widowers 158; bachelors 64; maidens 58; children to 10 years of age, boys 364; girls 306; stillborn, boys 44; girls 35; in all 1441. Christened, boys 664; girls 675. Total 1339. Married 363 couple.

At Vienna, buried, men 1007; women 1433; boys 1865; girls 1560. Total 5865. Among which, 8 of 90, 1 of 91, 3 of 92, 2 of 93, 1 of 94, 3 of 95, 2 of 96, 2 of 98, 1 of 99, 3 of 100, 1 of 102, 1 of 103, 1 of 106, 29 of 90 and upwards. Christened 4708.

At Dresden, christened, boys 758; girls 714; bastards, boys 68; girls 60. Total 1600. Buried, married men 225; married women 174; widowers 36: widows 65; bachelors 99; maidens 167; children, boys 478; girls 398. Total 1642. Among which, still-born, boys 53; girls 33. Married couple 519.

At Leipsic, christened, boys 478; 1 Jew, 20 years of age; girls 461. Total 940. Among which were posthumous births 6; twins 12; bastards 141; and among them 3 twins. Married couple 260. Buried, married men 113; married women 75; bachelors 49; maidens 34; boys 165; girls 106; women in childbed 10; children, boys 58; girls 51; still-born, boys 45, girls 24; widows and widowers 77. Total 807.

At Erfurt, buried 617; christened 624; married couple 183.

At Coburg, christened, boys 105; girls 101. Total 206. Among which 3 twins and 8 bastards. Buried, married men 35; married women 35; women in childbed 2; bachelors 4; maidens 10; children 96; 6 of which were still-born. Total 182. Married couple 46.

At Regenspurg, among the protestants, christened, boys 142; girls 126. Total 268. Among which 4 twins. Buried, married men 42; married women 39; bachelors 13; maidens 11; children, boys 58; girls 50. Total 213. Married couple 80.

At Frankfort on the Mayn. Christened, boys 346; girls 385. Total 731. Including 10 twins, 4 posthumous births, 1 Jew, 2 foundlings, and 22 bastards. Buried 843. Christened at Sachsenhausen, boys 87; girls 79. Total 166. Including 2 twins and 9 bastards. Buried at Sachsenhausen 168. Married pairs at Frankfort and Sachsenhausen 209.

Marriages at Amsterdam in the reformed church. In 1724, 2294; in 1725, 2249.

At Venice, born 4836; buried 4816.

At Dantzic, christened 2012: buried 1678; married couple 46.

An Account of an Earthquake at Boston in New England. By the Rev. Benjamin Colman. N° 409, p. 124.

This earthquake came suddenly on in the night of Oct. 29, 1727, between 10 and 11, in a very still and fair evening: and the tremblings and rumblings have returned often for some months since the great shake, and at times for 9 months after it.

The town of Newbury, at the mouth of Merrimack river, about 40 miles north east from Boston, is the place that seems to have been the centre of the shock and shakes felt by us. There the earth opened, and threw up many cart-loads of a fine sand and ashes, mixed with some small remains of sulphur. The family nearest to this eruption, it being in that part of the town where the houses lie at a distance from each other, were in the terrors of death; the roar and shock being much more particularly terrible on them. And yet upon us at 4 miles distance, and upon others at double that distance, it was very terrifying and astonishing.

Five or seven small shakes were felt by us, after the first and great one, that night and in the morning following; but these and other following rumbles and tremblings, were louder and greater at Newbury and the adjacent places, than with us; and they felt and heard many times when our parts did not; but yet from week to week, we and the places about us felt and heard some of the greater tremors, both by day and night, and in all varieties of weather.

As to any alterations in the air or water after a shock, Mr. C. could never discern any thing; particularly as to the wind being raised after a shock, when it was calm before, which some reported, he could never perceive the least difference.

A Proposition on the Balance, not noticed by Mechanical Writers, explained and confirmed by an Experiment before the Royal Society. By J. T. Desaguliers, LL. D. F.R.S. N° 409, p. 128.

Theorem.—AB, fig. 5, pl. 8, is a balance, on which is supposed to hang at one end B the scale E with a man in it, who is counterpoised by the weight W hanging at A, the other end of the balance. That if such a man, with a cane or any rigid straight body, pushes upwards against the beam any where between the points C and B, he will thereby make himself heavier, or overpoise the weight W, though the stop GG hinders the scale E from being thrust outwards from C. Also, if the scale and man should hang from D, the man by pushing upwards against B, or any where between B and D, will make himself lighter, or be overpoised by the weight W, which before only counterpoised the weight of his body and the scale.

If the common centre of gravity of the scale E, and the man supposed to stand in it be at k, and the man by thrusting against any part of the beam, cause the scale to move outwards so as to carry the said common centre of gravity to X; then instead of BE, Ll will become the line of direction of the compound weight, whose action will be increased in the ratio of LC to BC. This is what has been explained by several writers of mechanics; but no one has considered the case when the scale is kept from flying out, as here by the post GG, which keeps it in its place, as if the strings of the scale were become inflexible. Now to explain this case, let us suppose the length BD of half the brachium BC to be equal to 3 feet, the line BE to 4 feet, the line ED of 5 feet to be the direction in which the man pushes, DF and FE to be respectively equal and parallel to BE and BD, and the whole or absolute force with which the man pushes, equal to 10 stone. Let the oblique force ED, = 10 stone, be resolved into the two EF and EB, or its equal FD, whose directions are at right angles to each other, and whose respective quantities, or intensities, are as 6 and 8, because EF and BE are in that proportion to each other, and to ED. Now since EF is parallel to BDCA, the beam, it no ways affects the beam to move it upwards; and therefore there is only the force represented by FD, or 8 stone, to push the beam upwards at D. For the same reason, and because action and re-action are equal, the scale will be pushed down at E with the force of 8 stone also. Now since the force at E pulls the beam perpendicularly downwards from the point B, distant from C the whole length of the brachium BD, its action downwards will not be diminished, but may be expressed by $8 \times BC$: whereas the action upwards against D will be half lost, by reason of the diminished distance from the centre, and is only to be expressed by $8 \times \frac{1}{2}BC$; and when the action upwards to raise the beam is subtracted from the action downwards to depress it, there will still remain 4 stone to push down the scale; because $8 \times BC - 8 \times \frac{1}{2}BC = 4BC$. Consequently a weight of 4 stone must be added at the end A to restore the equilibrium. Therefore a man, &c. pushing upwards under the beam between B and D, becomes heavier. Q. E. D.

On the contrary, if the scale should hang at F from the point D, only 3 feet from the centre of motion C, and a post gg hinders the scale from being pushed inwards towards C; then if a man in this scale F pushes obliquely against B with the oblique force abovementioned; the whole force, for the reasons before given, will be reduced to 8 stone, which pushes the beam directly upwards at B, while the same force of 8 stone draws it directly down at D towards F. But as CD is only equal to half of CB, the force at D compared with that at B, loses half its action, and therefore can only take off the force of 4 stone from the push upwards at B; and consequently the weight W at A will preponderate, unless an additional weight of 4 stone be added at B. Therefore a man, &c. pushing upwards under the beam between B and D becomes lighter. Which was also to be demonstrated.

Scholium 1.—Hence, knowing the absolute force of the man that pushes upwards, that is, the whole oblique force, the place of the point of trusion D, and the angle made by the direction of the force with a perpendicular to the beam at the same point, we may have a general rule to know what force is added to the end of the beam B, in any inclination of the direction of the force or place of the point D.

First, find the perpendicular force by the following analogy:

As the radius: to the right sine of the angle of inclination :: so is the oblique force: to the perpendicular force.

Then the perpendicular force multiplied into the length of the brachium BC, minus the said force multiplied into the distance DC, will give the value of the additional force at B, or of the weight required to restore the equilibrium at A.

Or, to express it in the algebraical way. Let of express the oblique force, pf the perpendicular force, and x the force required, or value of the additional weight at A to restore the equilibrium. Then $DE : DF :: of : pf$, hence $pf \times BC - pf \times DC = x$.

The same rule will serve for the second case, if the quantity found be made negative, and the additional weight suspended at B. Or having found the value of the perpendicular force, the equation will stand thus, $-pf \times BC + pf \times DC = -x$, and consequently the additional weight must be added at B; because $-x$ at A is the same as $+x$ at B.

Scholium 2.—Hence it follows also, that if, in the first case, the point of trusion be taken at C, the force at B, required, will be the whole perpendicular force; because CD is equal to nothing: and if the point D be taken beyond C towards A; the perpendicular force pushing upwards, at that point, multiplied into DC, must be added to the same force multiplied into BC, that is $pf \times BC + pf \times DC = x$.

The machine Dr. D. made use of to prove this experimentally, was as follows, represented in fig. 6. The brass balance AB is 12 inches long, moveable on the centre C, with a perpendicular piece Bb hanging at the end B, and moveable about a pin at B, and stopped at its lower end b, by the upright plate

GG, from being thrust out of the perpendicular by the pushing pipe FE, whose lower point being put into a little hole at H, the upper wire or point, when put into another little hole under the beam at D, is by means of the worm-spring EF pressing against the plug E to drive forwards the said wire hD, made to push the said beam upwards with the force of the spring. TSS is a stand, to which is fixed the pillar TC that sustains the balance; and it has also a slit SS to receive a shank of the moveable plate GG, to be fixed in any part of the slit by a screw underneath.

Exper.—Hang on Bb, as in the figure. Then let EF be so applied to the hole H, that its upper wire hDk may go through a little loop at D, so as not to thrust the beam upwards, but be in the same position as if it did, that by hanging on the weight W, the brachium BC with Bb and FE may be counterpoised; and then the action against D and H may be estimated without the weight of the pushing pipe.

Then drawing down the end of the wire k, thrust it into the little hole under D, and B will be so pulled downwards as to require the additional weight of 4 ounces to be hung on at A to restore the equilibrium, when BH is 4 inches, BD 3 inches, and the whole force of the spring equal to 10 ounces.

It need not here be said, that for explaining the second case, Bb is to be suspended at D, with the plate GG fixed, to stop it at the place M, to keep it from being pushed towards T, and that the upper end of GFEDk must push into a hole made under B; in which case the weight P must be hanged at B, to restore the equilibrium.

P. S. To show experimentally that the force which the spring exerts in this oblique trusion, is equal to 10 ounces: take the beam AB, which weighs 4 ounces, from its pedestal CT, and having suspended at each end, A and B 3 ounces, support it under its centre of gravity by the pushing pipe EF, set upright under it; and it will be found that the beam with the two weights will thrust in the wire kh as far as h, the place which the oblique trusion drives it to.

Uncommon Appearances observed in an Aurora Borealis. By the Rev. Mr. Derham, Canon of Windsor, and F. R. S. N° 410, p. 137.

About 8 in the evening of Oct. 13, 1728, at Windsor, was observed a considerable streaming in the north, with such bright lances and columns as usual. But at Redbridge none such appeared, only in the north, Mr. D. observed a great thick, black bank of vapours; the top reaching about 20° above the horizon, without any convexity or curvature, as is usual in most of the streamings; but instead of that, the upper part was indented in many parts, with long black pyramids, somewhat resembling the streams of the lumen boreale, the edges of which were gilded with lucid rays, of the streaming colour: and all over the clouds, or vaporous bank, was a great commotion or disturbance behind them, as if something was rolling, or tumbling behind them. In less than an hour, the clouds, which had been pretty still, began to move to the s. w. and at last obscured the whole hemisphere; which before was all clear enough, except towards the north, to show the stars, though bespread with vapours, like a thin fog, a little inclining to red.

A remarkable Conformation of the Urinary Parts. By Mr. John Budgen. N° 410, p. 138. *From the Latin.*

In 1711, a female child was born, at Ockley in Surry, on whose back, about the inferior vertebræ, appeared an indolent tumour, of the colour of the skin, and size of a large pigeon-egg, that grew up to such a size with the child, that when she was about 9 or 10 years of age, it exactly resembled a calf's bladder, when blown up, but without a neck: in 1728 it was as large as an ox's bladder. On the 29th of Jan. 1728-9, the tumour broke as she lay in bed, from which there issued a large quantity of liquor, like urine. On narrowly examining it, Mr. Budgen found the tunics, the mucous matter on the inside, the ureters, veins and arteries, entirely the same as is common in the bladder; and there was some communication with the internal parts by a foramen in the vertebræ, through which one's little finger might enter into the abdomen, and which received the aforesaid vessels. On the 2d of Feb. 1728-9, the young woman died; and had the body been opened, Mr. Budgen believes he might have found the neck of the bladder in the abdomen, but no bladder. For, after the tumour broke she did not so much as once make water.

An Observation of the Eclipse of the Moon, at Castle-Dobbs near Carrickfergus in Ireland, Feb. 2, 1728-9. By Arthur Dobs, Esq. N° 410, p. 140.

The observation was made by a 9-foot glass. Having adjusted a monthly pendulum clock by a meridian line on the 30th of Jan. and further corrected by the meridian, Feb. 6, 1728-9.—Apparent time.

P. M.	6^{h}	27^{m}	0^{s}	A penumbra observed.
		29	30	The moon's limb immerged.
	7	30	15	The moon totally immerged.
	9	8	30	Moon's eastern limb emerged near mons acabe.

P. M. 10h 10m 0s A penumbra observed, the moon's limb emerging.
10 11 0 The limb evidently emerged.

From the beginning to the end of the eclipse 3h 44m 0s
Totally eclipsed 1 38 15

On the Use of Cold Water in Fevers. By Nicholas Cyrillus, Professor of Physic at Naples, and F. R. S. N° 410, p. 142. *An Abstract from the Latin.*

The use of cold water and of cold liquids, in fevers, is by no means new; on the contrary it was not unfrequent among the most ancient physicians. In ardent fevers, during the height of the paroxysms, they allowed the free exhibition of cold water, or any other cooling liquor, by which means the febrile heat being diminished, the patient often became composed, and a critical sweat succeeded. But to cure fevers by water cooled with snow (aqua nivata*) administered internally in very large quantities for several successive days, withholding at the same time all physic and food, is a practice entirely new, and one which at first appeared to be exceedingly bold. This method was introduced at Naples a few years preceding the date of Dr. C.'s account (1729) from Spain. Although, on the first trials, some fever patients who had been given over, were recovered, by giving them large quantities of cold water; yet for some time the more cautious among the Neapolitan physicians hesitated to adopt this novel and drenching mode of cure: but they afterwards became reconciled to it, especially when they saw that, instead of being practised, as at first, indiscriminately and at random, it was at length reduced to method and rule. The following are the principal rules to be observed in the use of this aqueous regimen. After some hours abstinence from food, so that the stomach may be empty, the patient is to drink as much as one or two pints of cold water, according to his age, strength, and thirst. The same quantity of water is to be repeated every hour or every second hour, day and night without intermission, except during sleep. All sort of food is at the same time to be withheld; for it has been found by experience, that when food is given along with these large doses of water it produces a foulness in the stomach, and so alters the quality of the water, as to render it unfit to pervade the minute vessels and produce its salutary effects. This abstinence from food is to be continued for several days, until a marked abatement or remission of the fever takes place, while at the same time the patient begins to have a craving for food: but if food be given sooner than this, an aggravation of the fever is the certain consequence. Hence in some

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* Snow water or ice water. It is afterwards termed aqua nive refrigerata.

fever-cases all sort of food has been withheld until the 7th or 10th day, and even for a longer period, provided the exhibition of the cold water had not been discontinued. When it appears to be the proper time for indulging the patient with food, the cold water should either be discontinued or be given only in small quantities; and an interval of some hours should elapse, after suspending the use of the water, before the food is offered, that the stomach may act upon it the better. The food should be of the lightest and most digestible kind, such as panada, eggs boiled soft, and the like. At first this sort of food is to be allowed in small quantity once a-day, afterwards twice a-day, till the patient comes by degrees to make rather a hearty dinner, but a very sparing supper; he should however abstain from flesh-meat for a month or longer. When the patients come to this diet, they should not wholly discontinue the use of cold water; but after the food has been digested, they should drink 2 or 3 draughts of water; and this plan should be persisted in, until all remains of fever are removed, and the patient is observed to be in a convalescent state.

A circumstance which requires much consideration during the employment of this method is, whether the water which is administered be freely discharged again from the body, or not? If the quantity of urine be increased, and its colour rendered paler 12 hours after this method has been begun upon, it may be inferred that the water begins to act properly. Sometimes on the very 1st day, or on the 2d or 3d day, the bowels are moved, and there comes away first a quantity of feculent saburra, afterwards a quantity of liquid discharge variously coloured. This again is a favourable occurrence; for after such an evacuation of the bowels, the febrile symptoms abate. Hence, after a lapse of 2 or 3 days, if no evacuation by stool shall have taken place, it will be proper (although the water which is drunk shall come away freely by the urinary passages) to move the patient's bowels either by means of glysters, or by some almond-oil given by the mouth.

It is no objection to a perseverance in this method of cure if the parotid glands should be swelled, or pus should be voided with the stools or urine. Even where symptoms of abscesses forming in the brain or thorax supervene, the internal use of the water should not be discontinued; but if the patient be drowsy or comatose, blisters and other rubefacients should be applied: or, if the patient be affected with difficulty of breathing, some almond-oil should be given, and the water should be exhibited not quite cold.—It not unfrequently happens that a violent vomiting supervenes on the first day's use of the aqueous regimen. The undigested contents of the stomach being thus brought away, the patient is relieved, the vomiting generally ceasing when the stomach is thoroughly emptied. But even if the water alone should be rejected as soon as it is swallowed, yet the patient should persist in drinking it, taking it in larger quantities and more frequently. Its exhibition is also to be continued, although a hiccup supervene. But a sweat coming upon a patient under this aqueous regimen, exhausts his strength, and is attended with great danger; so different are the effects of cold water administered according to this method, from those which follow the use of Hancock's magnum febrifugum.* A sweat, therefore, breaking out during the use of the aqueous regimen, should be checked by giving the water still colder and in larger quantities, as well as by throwing the bed-clothes from off the patient, and by ventilating the chamber.—When a patient is delirious or comatose, it becomes extremely difficult to administer the water in sufficient quantities. Compulsion must then be resorted to, that as much may be got down as possible. On such occasions Dr. C. mentions that he has sometimes crammed snow into the mouths of his patients.

Then follow some remarks concerning the kinds of fever (hereafter specified) and the proper time in those fevers, for subjecting the patients to this treatment; which should not be adopted at the very beginning, but generally in the height and during the utmost violence of the disorder. Hence it has often succeeded when the patient seemed to be in the agonies of death. On the other hand much injury has often been occasioned by some physicians, who have resorted to this remedy too early; with the exception of bilious fevers, in which it has sometimes been employed in the beginning with advantage.

This aqueous regimen is suited to acute fevers and all kinds of malignant and pernicious fevers.—In some cases, however, Dr. C. mentions that he prescribed warm instead of cold water, viz. where inflammations of the lungs and viscera were joined with the fevers. Yet even in these cases he sometimes passed from warm water to water that was cool, when the patients loathed the warm water, and could not be prevailed upon to drink it constantly. Although Dr. C. experienced the best effects from the use of this aqueous regimen in a great number of fever-cases; yet he does not deny that, like all other grand remedies, it sometimes failed.

Thus far Dr. C.'s observations apply to the administration of cold water in fevers; but he adds that he has prescribed it with good effect in a variety of other disorders; such as diarrhœa and dysentery; cœliac and lienteric affection; ischuria renalis and dysury; cardialgia and cholera morbus; hypochondriac

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* Febrifugum Magnum: or Common Water the best Cure for Fevers. By John Hancock, D. D. &c. Lond. 1722. Dr. H. represents cold water to be the best possible sweating medicine in fevers, and ascribes its febrifuge virtues wholly to the perspiration which it excites.

and hysteric affection; and (what he observes is still more extraordinary) in some cases of dropsy. Also in small-pox; viz. in the 3d stage thereof, when the patients have been in a dying state, from abscesses in the brain and chest; pus coming away during the use of the water from the nostrils and mouth. But in all these disorders, food was not withheld during the exhibition of the water; on the contrary, in the chronic disorders here enumerated, it was deemed sufficient to give a large dose of the cold water 4 hours before a light dinner, and another dose 8 hours after dinner.

Dr. C. then remarks, that in the employment of this method of cure, to give too little water is a worse error than giving too much. To produce the desired effects, it must be administered largely and be followed up. This may be done so much the more confidently, if after the first day's exhibition, the water begins to pass off by urine and stool.

Having thus stated the result of his experience at Naples in favour of the aqueous regimen in fevers; Dr. C. leaves it to be proved by the physicians of the northern regions, whether this method shall be equally efficacious in the same disorders in colder countries: He is inclined to think it will, the cold water plan having succeeded at Naples even during the winter season.*

A short Account of the different Kinds of Ipecacuanha.† *By Dr. Douglass, Med. Regin. Extr. et R. S. S.* N° 410, p. 152.

By comparing the several dried pieces as we have them, we may very probably conjecture that a short radical trunk descends from a caulis, and is afterwards divided into several large branches, and these again into smaller ones, in different series, with minute filaments or fibrillæ going out from them.

Each piece is made up of two general parts, an outer or cortical, and an inner or fibrous, which like a white nerve, or smooth compact fasciculus of woody filaments, runs through the centre or axis of the roots, and perhaps encloses within it a small medulla or pith, which however is hardly discernible by the naked eye. The cortical part is corrugated by two sorts of wrinkles, one super-

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* Notwithstanding the praises bestowed both by Dr. Hancock and by this author on the use of cold water *internally*, in fevers; it does not appear that this mode of treatment was much adopted in this country at the time of its first recommendation, or that it came to be adopted at any period afterwards; latterly however cold water has been much and successfully employed *externally* in the cure of fevers, agreeably to the directions given by Dr. Currie of Liverpool, in his treatise on this subject.

† Ipecacuanha has been referred by Linnæus to the genus Viola; but since the death of that celebrated naturalist, it has been found that he never saw a specimen of the genuine plant, and was accordingly mistaken in referring it to the viola. It constitutes a new genus called Callicocca, (Callicocca Ipecacuanha) of which an account by Professor Brotero has been inserted in the 6th vol. of the Linnæan Trans.—In the Spec. Plant. of Willdenow it is referred to the genus Cephaëlis. Cephaëlis Ipecacuanha.)

ficial, consisting either in circular rings or little knots, which do not go quite round; the other penetrating into its substance, being deep. Incisures or fissures reaching all the way to the nerve. What lengths these roots are of, when taken out of the ground, cannot be determined: the Doctor has met with some pieces above 9 inches, many above 6, but the greatest number are still shorter. We find them bent, wreathed, and contorted into all manner of figures; and indeed few pieces are quite straight for any considerable length.

What has been hitherto said, agrees to all the true ipecacuanha-roots; but several other things are still to be taken notice of, in which they differ.

The black is the smallest of the four sorts, very hard, and the fissures wide and numerous. The outer colour of the cortex is not equally black in all the pieces of this kind, and its inner substance, as well as the nerve, is mostly white, though not always in the same degree. The brown sort is larger than the black, the fissures at larger distances, the inner substance of the cortex darker, and the external colour has several degrees of redness in the several pieces. The third or grey sort is sometimes found of a darker, sometimes of a lighter colour, and the inner substance of the cortex is brown, streaked with white. It is much larger than the black sort, many pieces being above a quarter of an inch in diameter, but the nerve is smaller in proportion to the cortical part. The fissures are here still fewer than in the brown sort, and in some pieces scarcely any are to be met with. The superficial corrugations are various in different roots, some being almost wholly smooth, and in others the wrinkles rather longitudinal than circular. The white kind is of very different sizes, some pieces being larger than any of the grey sort, and the rest much less. The whitish colour of the cortex is mixed with a yellowish cast, and the nervous part is very large in proportion to the rest. Very few fissures are to be observed, and hardly any reach so deep as the nerve. The other corrugations are likewise very shallow, and most of them longitudinal; but it seems to be more knotty than the other kinds, and these knots seem to be owing chiefly to the fibrillæ which go out from the larger branches of the roots.

The true places of growth of these different species of ipecacuanha, have not as yet been fully settled. The black sort is hitherto known to come only from Brasil, whence we get it by the way of Lisbon, and some of our druggists for that reason distingush it by the name of the Brasil root. The brown sort is said to grow plentifully at some distance from the city of Cartagena in the kingdom of New Granada; from whence it is frequently sent in saroons or skins, containing 100 weight, to Jamaica, and so to England; where it is certain we have had it of late years in great abundance.

The grey ipecacuanha is with us preferred to all the rest, and by far the most generally used when it can be had. It is said by authors to grow in Peru, from whence it comes by the way of Spain, it being brought from Peru to Porto-Bello, and from thence into Europe, by the Spanish galleons. Some parcels are likewise probably sent from Porto-Bello to Jamaica; as it comes sometimes from that island. This species grows also plentifully in Martinico, where for many years past it has been used by the inhabitants.

The white sort is said by Piso to grow in Brasil; and if we may believe Father Labat, it is likewise found in Martinico.

These are the 4 kinds of true ipecacuanha which have hitherto come to Dr. D.'s knowledge; but he has met with two other roots to which that name has been falsely ascribed, which from their outward colour he calls white and reddish brown.

The white sort agrees pretty much both in colour and surface with the true white, but it is not near so knotty. It is likewise considerably larger in size, straighter and softer to the touch.

The brown sort is of a deeper colour than the true brown, and many pieces have some mixture of red (from whence it has been sometimes called red ipecacuanha) and the inner substance of the cortex inclines to a reddish yellow. The pieces are much longer than any of the former sorts, some of them measuring 16 inches, and they are of a size between the black and grey. The fissures are at greater distances from each other than in the true brown, and the spaces between them much smoother.

Both these false kinds were brought from Maryland in 1725, by one M. Seymour a surgeon, who said they grow there in great plenty, being called ipecacuanha by the inhabitants, and used as a vomit by those of inferior rank.

Sir Hans Sloane informed the Doctor that this false brown kind was the same that was formerly sent to him from Virginia for the true ipecacuanha, and which he afterwards discovered to be the root of a poisonous apocynum described by him in his Natural History of Jamaica; in which island it is very common, and likewise in New Spain, as appeared to him by the specimens sent him by his correspondent Dr. Burnet.

In his introduction to the 2d volume of that excellent history, he has obliged us with a very full and distinct account of what he had learned from his friends abroad, concerning the pernicious effects of the several parts of this plant, and of the great pains he was at to prevent its being brought into use in this country, which was then very much to be apprehended. Helvetius's name will always be mentioned with honour for the great share he had in rendering the use of the true ipecacuanha common in Europe; and Dr. D. cannot think that

Sir Hans Sloane deserves a much less degree of praise for having detected this false kind, which was insensibly creeping into use, the effects of which might otherwise have proved as fatal as the other is found to be beneficial.

This poisonous kind of apocynum is now cultivated by several curious persons about London.

An Account of a Book entitled, Hesperi et Phosphori Nova Phænomena, &c. Auctore Francisco Bianchino. By John Hadley, Esq. R. S. V. Præs. N° 410, p. 158.

The design of this treatise, is to give an account of some new astronomical discoveries relating to the planet Venus, which the author disposes under four heads; viz.

1. The description of the dusky spots observed in her disk. 2. Her rotation round an axis, the position of which is determined by the apparent motion of those spots, with the time of her revolution. 3. The parallelism of that axis to itself in all parts of the planet's orbit. 4. Observations in order to determine the horizontal parallax of Venus, and consequently those of the sun and other planets.

He takes notice of 5 remarkable spots in her whole surface; the two smallest of which are placed, one near each pole, the other 3 lie along the equator, and cover good part of a zone, extended to about 30 deg. of latitude on each side. He represents them to be much like the larger dark spots in the moon, which are usually called seas, but considerably fainter, so as not to be easily discernible, even to a sharp-sighted observer, without the assistance of a telescope, capable of representing distinctly the planet under an angle equal at least to that under which the moon appears to the naked eye, and with an aperture of 3 or 4 inches of the Roman palm.

This revolution he makes greatly different from those of the earth and Mars, both in the position of the axis and time of the period. He places the colurus solstitiorum, or plane passing through the axis of the planet and tropical points of its orbit, about the 20th degree of Leo and Aquarius, and gives the planes of its equator and ecliptic an inclination to each other of about 75 degrees. He determines the time of the revolution to be about 24 days and 8 hours, instead of 23 hours, as it has been generally taken to be from some observations made by Mr. Cassini in the years 1666 and 1667, but which he himself did not seem much to rely on.

The next article of his observations is the continuance of the axis in the same parallelism, through the whole orbit of the planet; which is a necessary and obvious consequence of the established laws of motion.

The 4th article contains an account of some observations made to determine the parallax of Venus in the year 1716. The method he used for this purpose, was to take the several distances of time between the appulse of the limb of Venus and of Regulus, which star she passed by about that time, to a horary circle very near the meridian, and to another about 6 hours after, which he measured by the pulses of a watch, of which 143 went to 1 first minute of time. He likewise observed the alteration of those distances taken at the same hour several days, one after another, and allowing a proportional alteration for the time between the two observations, he computed what the difference of their right ascension ought to have been in the latter of them, if there were no parallax; then comparing this difference with that observed, he concluded the disagreement to be the parallax of right ascension. This method the author seems to depend on so much, as to think that an equal degree of exactness is hardly to be expected from any other yet practised. But if we consider that the whole parallax of right ascension amounts, by his observations, to no more than 4 pulses of his watch: and that he allows a possibility of an error of nearly one of those pulses in taking each of the transits, it is evident that if such an error be actually committed in each of the observations on which the finding of the parallax depends, and all of them happen to conspire the same way, the result of all together may possibly be greater than the whole parallax found. On the whole, he makes the horizontal parallax of Venus at that time to have been 24″ 20‴, and that of the sun 14″ 18‴; but as he takes no notice of the latitude of the place, in deducing the horizontal parallax from that of right ascension, they both ought to be increased on that account by about $\frac{1}{3}$, or in proportion of 3 to 4. If therefore there be no other mistake in his numbers, the horizontal parallax of the sun, as deduced from his observations, should be about 19″.

For a telescope of 100 Roman palms he allows an aperture of 3 or 4 inches of that palm, with an eye-glass whose focal length may be from 7 to 11 of the same; but what he directs in longer instruments, to increase the breadth of the aperture and focal length of the eye-glass in the same proportion with the instrument, must certainly be the effect of some mistake; for in this case, a longer telescope will magnify no more than the shorter, but only have the strength of light in the object increased in proportion to the square of the length.

Observations on a Treatise written by M. Helvetius, to prove that the Lungs do not divide and expand the Blood; but that, on the contrary, they cool and condense it. By F. Nicholls, F. R. S. N° 410, p. 163.

The matter in question between Helvetius and Sig. Michelotti is, whether the lungs cool and condense the blood, according to the opinion of the ancients, or whether they mix, attenuate, and of consequence expand it, according to the system of Dr. Pitcairn?

The author, in order to support the opinion of the ancients, brings several arguments to confute the system of Dr. Pitcairn; the most considerable of which is, that the right auricle and ventricle being considerably larger than the left auricle and ventricle, and the pulmonary artery having a larger capacity than all the pulmonary veins taken together, the blood must evidently occupy a greater space before than after its passage through the lungs; and because the difference in the capacity of these vessels cannot be balanced by any increase of the velocity, he concludes, that the blood is not attenuated and expanded, but must be condensed in its passage through the lungs. And this the author conceives is done by the air, which, as a fluid relatively cold, must cool and condense the blood, to which it is so nearly applied in the action of inspiration.

Had the author of this treatise been contented with supporting the opinion of the ancients, without endeavouring to subvert the system of Dr. Pitcairn, he would probably have found many advocates for his doctrine, and few opposers.

That the blood is cooled by the action of inspiration, is a matter of which Dr. N. believes few physicians doubt, when they consider that in inflammations of the lungs, nothing is more earnestly desired than the breathing cool and fresh air, nor does any thing more evidently conduce to the cure of these and other inflammatory dispositions, than the use of fresh air. But that this is the sole use of breathing, or that this cooling power can overbalance the expansion from the action of expiration, is what he can nowise conceive.

If we consider the state of the blood at its return to the heart, and how careful nature has been, not to use this blood for the nourishment of the lungs before it has passed through the pulmonary vein and artery, though it would in that case have been as effectually cooled in the bronchial arteries as in the pulmonary vessels, we are naturally led to believe, that it is some other quality which has rendered it improper for nourishment, and which is to be destroyed by the action of the lungs.

For this reason, and from the structure of the parts subservient to breathing, it seems evident, that the blood is mixed, attenuated, and consequently re-expanded in the action of exspiration. Dr. N. now considers whether the action of inspiration so far overbalances the action of exspiration, as to condense the blood into a less bulk, than it had before its passage through the lungs.

The accurate Santorini of Venice, in chap. 8, sect. 3, of his observations, has carefully examined the fact as stated by Helvetius, and finding it true in that one subject, as to the auricles and pulmonary vessels, but false as to the ventricles, he proceeds to prove that this difference in the capacity of the pulmonary vessels, could not be designed on account of the blood being condensed in its passage through the lungs; because, if so, the right ventricle ought to have been larger than the left, and the pulmonary artery ought, not only to have been larger than the pulmonary veins, but it ought likewise to have been larger than, or at least equal to, the two venæ cavæ; whereas in his subject, the two venæ cavæ were to the pulmonary artery as 228 to 188.

In the mean time, he recommends repeating the inquiry to other anatomists, as doubting whether the fact is constantly so in healthy subjects.

The first heart is of an adult in which

	the diam.	per. and	areas, are nearly	
Of the vena cava descendens	79	237	4740	
pulmonary artery	115	345	10005	
superior left pulmonary vein	69	207	3519	12477
inferior left pulmonary vein	73	219	3942	
superior right pulmonary vein	49	147	1764	
middle right pulmonary vein	40	120	1200	
inferior right pulmonary vein	57	171	2052	
aorta	110	330	8910	

The ascending cava being tied above the diaphragm, could not be measured in this subject.

The second heart was that of a child nearly a year old. Its lungs appeared perfectly sound, and of a pale clear colour; and therefore the more proper for an examination of this kind.

In this second heart, the	diam.	per.	and areas are,	
Of the aorta above the coronaries	43	129	1419	
pulmonary artery	43	129	1419	
superior left pulmonary vein	29	87	609	2088
inferior left pulmonary vein				
superior right pulmonary vein	26	78	507	
middle right pulmonary vein	17	51	204	
inferior right pulmonary vein	32	96	768	

We may here observe that the aorta, after giving off the coronary vessels, is equal to the pulmonary artery. As to the proportion between the pulmonary artery and veins, the artery in this subject is to the sum of all the veins here measured, as 1419 to 2088, and yet the lower left pulmonary vein is here omitted, as being tied too close to admit of being measured. But if we suppose the inferior left pulmonary vein to be to the superior left pulmonary vein, in the same proportion as in the first heart, we shall then find its diameter nearly 31, and its area at least 700; which will make the pulmonary artery in this heart, to the sum of all the pulmonary veins, as 1419 to 2788; and in that case, the left pulmonary veins will be to the right pulmonary veins, only as 1309 to 1479.

The third heart is of an abortive nearly of 5 months: by its appearance, it was suffocated by too much blood. In this subject the

	diam.	per.	areas are	
Of the vena cava descendens	14	42	197	629
vena cava ascendens	24	72	432	
aorta above the coronaries	16	48	192	
pulmonary artery	20	60	300	
canalis arteriosus	12	36	108	
right pulmonary branch	11	33	99	198
left pulmonary branch	11	33	99	
superior left pulmonary vein	11	33	99	294
inferior left pulmonary vein	9	27	54	
superior right pulmonary vein	7	21	42	
middle right pulmonary vein	11	33	99	

The inferior right pulmonary vein is here cut too close, and otherwise injured, so that its area cannot be measured. Yet we find the remaining pulmonary veins to the pulmonary branches of the pulmonary artery, as 294 to 198.

We may here observe a remarkable difference between the capacities of the two venæ cavæ taken together, and the pulmonary artery; the two cavæ being more than double the pulmonary artery, and the pulmonary artery still one-third larger than the aorta. As this difference could not arise in this case from the blood's being condensed by the inspired air, so it seems a proof, that had the fact been true, as stated by Helvetius, it had still been an insufficient demonstration of his system.

A Lunar Eclipse observed at Rome, Feb. 2, 1728-9. *By Fa. Carbone.*

N° 410, p. 170.

At $7^h\ 44^m\ 22^s$ True time, the beginning of the eclipse; $8^h\ 43^m\ 17^s$ the total immersion; $10^h\ 21^m\ 38^s$ the beginning of the emersion; $11^h\ 20^m\ 41^s$ the end of the eclipse.

The same Eclipse observed at Paris. N° 410, p. 171.

At $7^{h}\ 1^{m}\ 0^{s}$ A dense penumbra; $7^{h}\ 3^{m}\ 0^{s}$ a very dense penumbra; $7^{h}\ 3^{m}\ 0^{s}$ beginning of the eclipse; $9^{h}\ 41^{m}\ 18^{s}$ the beginning of the emersion; $10^{h}\ 41^{m}\ 24^{s}$ the end, doubtful; $10^{h}\ 42^{m}\ 0^{s}$ the end, certain.

The same Eclipse observed at Padua. By S. Poleni. N° 410, p. 173.

At $7^{h}\ 44^{m}\ 40^{s}$ App. time, clouds hindered seeing the beginning; $11^{h}\ 20^{m}\ 56^{s}$ the end of the penumbra.

Total Eclipse of the Moon observed at Wirtemberg, July 9, *N. S.* 1729. *By M. Weidler.* N° 410, p. 174.

At $0^{h}\ 1^{m}\ 30^{s}$ In the morning, the beginning; $1^{h}\ 1^{m}\ 0^{s}$ the total immersion; $2^{h}\ 40^{m}\ 30^{s}$ the emersion; $3^{h}\ 40^{m}\ 0^{s}$ end of the eclipse.

The same Eclipse observed at Padua. By S. Poleni. N° 410, p. 176.

At $0^{h}\ 0^{m}\ 28^{s}$ Apparent time, beginning of the shadow; $0^{h}\ 58^{m}\ 48^{s}$ the total immersion; $2^{h}\ 37^{m}\ 38^{s}$ beginning of the emersion; $3^{h}\ 38^{m}\ 8^{s}$ end of the penumbra.

A Geographical Description of the Kingdom of Tunis. By the Rev. Thomas Shaw, Chaplain to the English Factory at Algiers.* N° 411, p. 177.

From Tunis Mr. Shaw travelled as far westward as Hydra, and from thence he went to Toser, passing from Tegewse through the Lake of Marks, or the Palus Tritonia, to Gaps; from Gaps he travelled all the way on the coast of Biserta. He made use of a small, but very good mariner's compass, and found the variation at Cairwan 10° west, at Biserta something more than 12°, and at Algiers 30° 30′. He carried also a brass quadrant of a foot radius, and took the latitudes of Tunis, Cairwan, Spetula, Gaffsa, Toser, Ebillee, Gaps, Stax, Susa, Lowharia and Biserta, with all the exactness such an instrument would admit of.

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* The Rev. Thomas Shaw, who afterwards took the degree of D. D. was chaplain to the English consul at Algiers, and author of Travels or Observations relating to several parts of Barbary and the Levant first published in 1738. A Supplement to this work appeared in 1746, containing a reply to the strictures of Dr. Pococke, bishop of Ossory. The Travels and Supplement were reprinted in 1 vol. in 1757. After his return from Algiers, Dr. S. became a fellow of the R. S. and was chosen Regius Professor of Greek, at Oxford. He died in 1751, aged 59. Dr. Shaw was a man of considerable erudition, and a diligent inquirer into whatever was most remarkable respecting natural history and antiquities, in those foreign parts which he had opportunities of visiting. Accordingly his Travels were much read at the time of their publication, and it must still be allowed that they afford considerable information on a variety of topics. It is nevertheless true that respecting some of the subjects of which Dr. S. has treated, more satisfactory accounts have been given by later travellers.

The kingdom of Tunis is bounded to the north and east with the Mediterranean sea, to the west with the kingdom of Algiers, and to the south with that of Tripoli. It is 230 miles in length, from the isle of Gerba, in latitude 33° 24′, to Cape Serra, in latitude 37° 16′, and 128 miles in its greatest breadth from Monasteer to Tibesa. Sbeka, its utmost boundary to the west, lies in long. 7° 26′, and Clybea, its utmost boundary to the east, in 10° 47′ from London.

Mr. Shaw, from his observations, corrects most of the geographical writers, both ancient and modern, concerning the situation of places, &c. but of these it is not necessary to enter into a detail here.

A brief Account of some of the Effects and Properties of Damps. By Mr. Isaac Greenwood, Prof. of Mathematics at Cambridge, New England. N° 411, p. 184.

July 19, 1729, two men being employed to repair a pump in this place, uncovered the well; on which one of them immediately attempted to go down, by means only of a single rope, but had not descended above 5 or 6 feet before he was rendered incapable of sustaining his weight, and without speaking, or any signals of distress, slipped down suddenly to the upper part of the joint of the pump, where being supported about a minute, fetching his breath in a very distressed manner, he fell to the bottom, which was about 8 or 10 feet lower, and covered with but a very few inches of water, without discovering any signs of life. On which the other hastily took the rope in his hand, to descend to the relief of the former; but at the same distance from the top, met with the same fatal interruption, and without discovering any signs of distress, was heard to fall to the bottom.

The workmen above prepared a third with a tackle about his waist. On his descent he was rendered speechless, and made no signs at all, though he had agreed to it; on being raised from the well, he appeared as dead: but on the use of proper means was soon recovered, without remembering any thing particularly that had passed. Some hours after this the other bodies were taken up, with all the marks of a violent death upon them.

Next evening several trials were made on descending lights, particularly, by letting down lighted candles uncovered, others enclosed in lanterns, and others with the lantern placed in a pail; but in all these endeavours it was observed, that whatever the circumstances of the descending lights were, it never reached above 6 feet before extinguishing.

July 20, Mr. G. repeated this evening such experiments in the damp as related to flame, and found the effect much the same as before, viz. in about 6 feet below the top of the well, the flame would grow dim, and if not immediately raised would change to a bluish colour, and become more and more contracted or diminished, till in about a minute's time it would be totally extinguished, without any remains or stench accompanying the wick. In these experiments he particularly observed, that the flame in all its changes still continued its pyramidical figure, nor did a quicker or slower descent make any alteration in these circumstances. One experiment was very particular, relating to the flame of a candle. He took a common pail, and having fixed a candle to the bottom of it, erect about 8 inches long, he poured as much hot water into the pail as reached within a quarter of an inch of the blaze of the candle. Then having carefully lowered the pail down the well, the flame, notwithstanding it was defended by the reeking steams of the hot water, went out at the same depth, and in the same time as it did before. After this he immersed burning coals, flaming brimstone, and lighted matches, all which were extinguished with very little difference as to the time, or other circumstances.

Two experiments were made relating to animal life. A large kitten was very much affected in about a minute's time, and after 3 minutes was rendered so weak, that after she was taken out, she could not sustain her weight on her legs. Being at length pretty well recovered, they carefully bound her up in a silk handkerchief, that she might be the more easily suspended; and having let her down about 16 or 18 feet, in 3 minutes she was affected in the like manner as before, making a very distressed noise, and in about 5 minutes was in such extraordinary convulsions as rendered the sight not a little disagreeable; but in these throws she disengaged herself from the handkerchief, falling to the bottom, without making any efforts to swim; whence we concluded they were the last struggles for life, in which she broke loose.

They tried the same fatal experiment on a small bird, which being suspended in the damp about 3 minutes, was found entirely senseless, and according to all appearance past recovery. Mr. G. found it was very cold, nor had it the least motion; however, keeping it close between his hands, which were pretty warm, in about a minute he felt a small palpitation, which presently increased to a stronger pulse, till in about 6 or 7 minutes the bird was restored to a perfect and uninterrupted respiration. About half an hour after this, he again put the bird into the damp, and continued it there about 5 minutes, after which it was past recovery.

July 21, he repeated several of the experiments relating to lights and flame, which succeeded with very little, if any alteration, as before. He then examined the elasticity of the air in the well, by letting down a small bell, the sound of which was as distinct and loud, as in any ordinary well of the same depth.

Then to discover the degree of moisture, he took a large sponge a little wet, which with the silk string, by which it was let down, weighed 278 grains. This being suspended in the damp upwards of 5 minutes, and then raised, was carefully weighed, and found to be of the same weight precisely. After this he dried the sponge, which then weighed but 261 grains, and having applied it to the damp for the space of 10 minutes, found again, that it had not gained the least part that could be perceived in its weight. Also, a large bundle of catgut, weighing 2 oz. 15 dwt. 10 gr. acquired not the least augmentation, by being suspended for a very considerable time.

To these experiments he added one on the hydrostatical balance, to determine whether there was any extraordinary difference as to the density, or specific gravity of common, and this vitiated air. The balance was very large, and accurately poised, and the solid, which was a globe, was $4\frac{8}{10}$ inches in diameter. This with its string weighed in the air 7 oz. 6 dwt. And after being immersed in the damp, it lost nothing of its weight, being then in equilibrio to so great a degree of exactness, that $\frac{1}{2}$ a grain would over-ponderate on either side.

This damp abated more and more by being exposed to the air, till on July the 25th, persons were let down to the bottom without any inconvenience.

The other instance is of a very sudden subterraneous vapour, on May 9, 1729, in a well in School-house-street, Boston. This well had been opened for some considerable time, and not only enlarged in its diameter, but sunk 14 or 15 feet deeper. Two men undertook to lay the stones. They had been employed all the day, till about 6 o'clock in the afternoon, when one perceived a very unusual stench, and by the extraordinary increase, he was apprehensive of some great danger. The other was hitherto insensible of it, but perceiving his partner's visage to change in a very uncommon degree, called up for relief; at which instant, as he afterwards expressed himself, he first perceived a very strong noisome smell, resembling rotten fish, which on a sudden seized his senses, and rendered him unable to sustain his weight. The first had immediately closed his mouth and nostrils with his hand; and when the bucket was lowered with a third person for their relief, assisted in getting the second into it. As the bucket was raising, the latter was taken with very unusual and extraordinary fits; and when he was laid on the ground, till the first was taken out, could scarcely be kept still by the united strength of 3 or 4 persons; but bounding and writhing his body, like a fish newly taken from the water. The first was affected only with fainting fits. After 3 hours the 2d recovered of these extraordinary convulsions, but was disordered in his brain during the whole night: and though the former was sooner relieved of his fits, he continued extremely disordered for a longer time. It was thought remarkable, that neither of them was affected with either vomiting or purging.

This accident happened on Friday, and on the Monday they were both restored to perfect health. The well continued infected for a very little while, and when on the Monday following some other workmen renewed the work, there was nothing noisome that could be perceived.

Abstract of a Letter from the King's Officers at Sheerness and Chatham, to the Commissioners of the Navy, giving an Account of what they met with in opening an ancient Well near Queenborough in Kent, in Sept. 1723. *Communicated by Mr. Peter Collinson,** *F.R.S.* N° 411, p. 191.

On visiting the well near Queenborough, where the castle formerly stood, Sept. 24, 1723, and finding but very little water at the bottom on sounding, a man was let down, who reported that it was cleaned, and the ground sunk 4 feet deeper than the curb at the bottom. They then measured its depth, and found it 200 feet, and artificially steened the whole depth with circular Portland stone, which is all entire, and stands fair, the mean diameter 4 feet 8 inches; but observing, that not one drop of water came into it, they resolved to try whether they could find any by boring; in order to which, they got a piece of timber of about 7 feet long, and bored it through with a 3½ inch auger, which trunk they fixed at the bottom of the well, and fastened it by quarters to the curb at the bottom, to prevent its raising, and filled it all round 3 feet deep with clay, and on that laid 4 course of bricks for a platform for the men to stand on in their boring, and got also an auger of 2½ inches, to bore through the clay; but they could not get all the necessary appurtenances till Sept. 26, when 3 men at a time began to bore, who were shifted every 3 hours. The boring they sent up, was a very close bluish clay, which continuing the same after 3 days and a half boring, they began to despair meeting with water; but on the 30th in the evening, as they were boring, the auger slipped down at once, and the water rushed up violently; so that in an hour's time there was upwards of 4 feet water, which rose so fast, that at 12 o'clock at noon,

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* Peter Collinson, of a respectable family in the North of England, was born about the year 1693. He was early devoted to the pursuit of Natural History, and became early acquainted with the most eminent naturalists of his time, as Derham, Woodward, Dale, Lloyd, Sloane, &c. He was elected a Fellow of the Royal Society in the year 1728, and was, perhaps, one of the most diligent and useful members of the Society; not only supplying that body with many curious observations himself, but promoting and preserving the correspondence of many learned foreigners. Linnæus, when in England, contracted with him a particular friendship, which was reciprocally increased by a multitude of good offices, and continued to the last. To the name of Peter Collinson, says the ingenious Dr. Pulteney, is attached all that respect which is due to benevolence and virtue. In his time England received large accessions of exotic botany from all parts of the globe; to which no one contributed more than himself, through his various correspondence, especially in America. Natural History in all its branches was his delight, and he especially cultivated the choicest exotics, and the rarest English plants. His garden contained, at one time, a more complete assortment of the Orchis genus than had perhaps ever been seen in one collection before. He died Aug. 11, 1768, in the 75th year of his age. His name is perpetuated in a beautiful American plant, belonging to the class Diandria. We are informed by the authors of the Biographical Dictionary, that his person was rather short than tall, and his aspect pleasing and social.

	Feet.	Inch.
On the 1st of October, they found	55	10
On the 2d, at 5 in the afternoon	109	8
On the 3d, at 3 in the afternoon	132	6
On the 4th, at 3 in the afternoon	149	6
On the 5th, at 4 in the afternoon	161	3
On the 6th, at 10½ in the morning	167	8
On the 7th, at 4 in the afternoon	174	0
On the 8th, at 7 in the morning	176	7

and still continued to increase, though slowly, owing to the weight of water which the spring through the hole of the trunk must force up, and the well being wider aloft than below. They bored 81 feet below the foot of the trunk before they met with this body of water, which by computation is 166 feet below the deepest place in the adjacent seas. The water proves excellent, is soft, sweet and fine; on comparing it with the best spring water brought from Milton, the former was found the best. It lathered well with soap, and boiled old pease very well; and they had great reason to believe, that the spring will sufficiently supply his Majesty's ships, as proposed.

Observations on the Crane, with Improvements. By J. T. Desaguliers, F. R. S.

N° 411, p. 194.

When heavy weights are to be raised from a great depth, and laid on carriages very near the precipice, as at the edge of a stone quarry, the crane must be a fixed one, and only the gibbet moveable, from which the weight hangs, as in fig. 1, pl. 9. Here, in the common way, the rope RRR, or chain, which runs over the gibbet, goes between two pulleys, P, Q, fixed within the upper horizontal beam of the crane AQTX, above the axis of the gibbet BGV, so as to be carried easily to the right or left hand, from W to W, when the gibbet turns on its axis, to bring the burthen over the carriage designed to receive it. For this purpose, a small rope, called the guide-rope, is fastened to the weight, or to the upper part of the gibbet near its extremity, g, which a man is to pull, to bring the weight over the place, to which it must be lowered. Now in performing this, the main rope or chain not continuing parallel to the arm of the gibbet, gives the weight a tendency towards that side to which it deviates, and that sometimes so suddenly, that without care, and much force applied, if the weight be very great, the burthen will swing to or from the carriage, so as to break every thing in its way. Sometimes a horizontal piece, like a handspike, is fixed in the upright shaft of the gibbet a little above B, to turn it by; but in that case too the force is unequal, as the weight is carried round; so that the lives of the men that are loading, often depend on the care of the man who guides the weight, by either of the means abovementioned.

But if upon the axis of the gibbet there be fixed an iron wheel, y, with many teeth, to be carried round by a pinion, u, of a few leaves, on the end of whose axis is fastened a wheel, x, with arms (that axis going through the perpendicular piece TZ behind the shaft of the gibbet) a man standing at that wheel is out of harm's way, and has such an advantage of power as to hold the weight steady in any place required, notwithstanding its tendency to swing, which is not felt at the ends of the arms of this last wheel. The first who has made use of this contrivance is Mr. Ralph Allen, Post-master of Bath, at his Stone-Quarry, where the weight raised is 4 or 5, and sometimes 6 or 7 tons. It need not be remarked, that the power to bring up the weight works here by means of a capstan, or upright shaft, RO, drawn round by horses, that the weight may come up more expeditiously, though in the figure the hand-spikes, f, e, b, going in at such a hole as d, show that men may work it on occasion.

The same gentleman has another crane at the river side, where he has a wharf, by which he takes the stone from the carriages, and with great expedition lets it down into the barges or vessels that come to fetch it.

This crane is of the sort commonly called a rat's tail crane, fig. 7, moving round a strong post like a windmill, so that it may turn quite round with all its load. The axle Bb, on which the rope winds, is here horizontal like a winch; but to gain strength, instead of the walking wheel CA, it is carried round by a strong wheel and pinion, fig. 5 and 6; or is in effect a double axis in peritrochio. Now in the common cranes of this kind, there is only a catch, as EKA, fig. 5, to hold the burthen at the height it is brought up to, while the crane is turned round, to have the weight lowered into the vessels, which is done by lifting up the catch, and being ready to let it down again as need requires. Sometimes a half circumference of wood, DHB, fig. 5, is held hard against a wooden wheel ww, on the axle, to regulate and govern the descent of the weight. But as in either of these cases, if the man at the crane is careless, very bad accidents happen, he has made such a contrivance, that the pall or lever, by which the axle is pressed to direct the descending motion, so communicates with the catch, that in case the man that ought to manage it, should carelessly let it go, the catch always takes, and thus all accidents are prevented; as will be shown in the explanation of fig. 5 and 6.

Where goods are to be raised high, as in unloading vessels, and also to be let down deep, as in loading them; if the weights do not exceed 2 or 3 ton, and many hands are not to be had, then an endless screw turned by a handle at each end (in an opposite situation, or with one handle and a balance to it) leading an axis in peritrochio, or as it is commonly called, a worm and wheel applied to a crane, with a gibbet, is most useful: for the teeth of the wheel are pulled by the weight so directly against the thread of the worm in its endeavour to descend, that one may leave the handle in any position, where it will stop, without any catch, or the least danger of the weight falling back again.

But then, if you would have the weight to be let down, to descend pretty quick, which cannot be performed by applying the hand to the handle, which goes through a great space in comparison to the space described by the weight, only give the handle a swing, and if the worm be well oiled, the handle and its counterpoise, or the two handles, will perform the office of a fly in the common jack, turning very fast round, and regulating the motion of the weight, which from that impulse will descend continually, and not too fast, like the weight of a jack.

The way to stop this motion at any time, is to grasp the axis of the screw hard, between the screw and the handle in its round part. The hand is sufficient to do it, and will stop it in 2 or 3 turns.

The worst cranes are those where men walk in a large wheel, by reason of accidents that happen daily on account of the short space between a man's two feet. This may be prevented by using quadrupeds, the length of whose bodies, makes a base of sufficient length to keep the wheel from running back, fig. 7.

An Explanation of the Figures.—Fig. 1, representing a fixed crane with a gibbet moving on an upright shaft or axis. AAQ, the roof of the crane, to preserve the rope RTr from the weather, when the arm of the gibbet VGg being turned towards Y is brought under it. AT, the upper piece of the crane, in a horizontal position, called the plate of the crane. X, Y, Z, the three crane posts, braced at top and bottom. DS, MN, IE, three cills within the stone work, braced with wood, and made fast with an upright plate of iron, pinned to the wood on each side. When the crane is not in stone work, the three cills must be all in one piece, reaching from D to E. HI, hE, are the braces of the main post, which come up above the level of the wharf LWB, which are longer and stronger than the others. Here a cross piece, whose section is &, keeps the main post from twisting.

RO, the capstan, or shaft of the crane, to receive the rope or chain; which shaft is turned here by bars or handspikes, such as bd, fd, or cd, the lower part being strengthened with iron hoops above and below the holes at d, with a pivot or iron axis turning in a hole in a piece, whose section is F. pp, are two pins, which hold on a collar in which the upper part of the shaft turns. CB, the shaft or axle of the gibbet, with pivots and iron hoops at top and bottom, and a wheel of iron, y, having teeth perpendicular to its plane. This wheel is led by a pinion, u, which is on the axis of the wheel x, by whose arms a man standing at H may bring about the end of the gibbet g with the ram-head r, and the weight hanging at it, either to the right or left, and easily hold the gibbet in any position. CTPQ, a strong piece or block, having three pulleys, one vertical, and the other two horizontal, that the rope may run over the first of them, and between the two others.

Fig. 2 represents a horizontal section of the crane in its upper part, or rather a view of it from the plane of the roof, supposing the roof taken off; where the same letters mark the parts which have been described in fig. 1.

Fig. 3 shows the inconveniencies in the motion of the gibbet. LBED represents part of the wharf next the water, or precipice of a quarry. TPQ the block-piece which holds the three pulleys, expressed by the same letters as in fig. 1 and 2. sgrG the arm of the gibbet, represented by vg, fig. 1. T the vertical pulley. P, Q the horizontal pulleys, represented in another situation by p, q, when their centers from m, y, are brought to n and t. c is a point directly over the pivot of the shaft, or axle of the gibbet. c1, c2, c3, c4, c5, represent a line over the arm of the gibbet, or rather a plane going through the middle of it, in several of its positions, when turned towards the right hand, from its direct position cr. c6, c7, c8, cc, represent the several positions of the gibbet towards the left, the last pulley r, at the end of the gibbet, immediately over the weight traversing in the circle 5, 4, 3, 2, 1, 6, 7, 8.

When the gibbet is in the position cg, the rope runs directly over the middle of its arm ; therefore the least force applied to r or r, can keep in its place the greatest weight that can be drawn up by the crane, when suspended to the ram-head. If the pulleys are at p and q, the gibbet loaded will also be without labour retained in the position c2 on the right, and c6 on the left, and with no great trouble in the position c1.

But if the gibbet be brought over the wharf at 4 on the right, or at 8 on the left, the rope will no longer run over the middle of the gibbet, but deviate from it, so as to make with it the angle q4t, or o8n, and raise the weight by the motion of the gibbet in proportion as the line q4, or o8, is longer than t4, or n8 ; and therefore the weight will tend to run back towards g, in proportion to the difference of those lines, which must give a twitch to the person who draws from r, or *r*, by a guide rope.

If, to prevent this inconveniency, the pulley at q be removed back to Q; then indeed the rope will run over the line c4, or t4, and consequently the gibbet will be easily held in that situation; but if there is occasion to remove the weight to 5, the rope touching the pulley at t, will make an angle with c5, and again be subject to the inconveniency abovementioned. Besides, in bringing the end of the gibbet from g to 4, the rope immediately applying itself to the pulley at t, will come forward with a jerk, though it will be twitched back again when at 5.

If the pulley be set backwarder still, as may be seen at P, when you would keep the weight under 8, it will tend to go on towards c, in proportion as the rope at m8 is now shorter than the line n8; for now the weight descending a little, the force of that descent added to the pull of him who draws the guide rope, will cause the weight to swing towards the crane, so as sometimes to do mischief, if the weight he very great, and the men careless.

No position of the pulleys can mend the matter, there being only three situations of the gibbet in its whole traverse, where it can keep its place when loaded. Therefore the wheel, y, and the wheel and pinion, xu, in fig. 1, are of very considerable use when great weights are raised.

Fig, 4, represents the double axis in peritrochio, or wheel and pinion used instead of the walking wheel of fig. 7, c, c, is an axis with handles having a pinion P which leads the wheel PR to wind the rope RZ on the axle R. K, A, part of the catch which stops the rope from running back again. w w, a wooden wheel of some thickness, which, when the catch is up, is kept from turning too swift, as the weight runs down, by pulling up the semicircular part of the pall 101, so as to make it bear hard against the wheel below, to regulate or stop the descent of the weight. cc, the pivots or centres of the axle. LF, part of the lever, by which the pall is drawn up against the wheel ww, by means of the rope FB. Q, the weight to bring down the pall clear of the wheel ww, when it is not pulled up. 101B, the end of the pall which is applied to the wheel, the other end not being represented here.

Fig. 5 shows the manner of letting down the weight swifter or slower as there is occasion, representing that end of the axle on which the catch and pall act alternately. PP and pp are two upright pieces, fixed to the frame of the crane, in any manner that is most convenient for carrying the three centres L, K, and k.

When the rope RrZ, going over a pulley at r, or any where else, draws from the axle in the direction Rr; the catch, if its end is at A, keeps it immoveable. But by pulling at H, the lever GF rises at F, and consequently draws up the end B of the pall BD; which moving on the centre k, by its end D (by means of the bar DE) pulls down E, and raises A of the catch, so as to let the rope run down; but to prevent its running too fast, one must pull a little harder; then the semicircle IOI will press against the wheel, and slacken the descent of the weight; which will be wholly stopped by pulling still harder: then the lever, pall, and catch will be in the position marked by pricked lines and small letters. Now if the person holding H, should carelessly let it go, the weight Q in descending will bring down the pall at B, and raise its other end, so as to throw the catch in again on the teeth of the ratchet, and stop the whole motion without accidents.

Fig. 6 represents the wheel and pinion at the other end of the axis, where the same letters express the same parts.

Fig. 7 represents the crane with the walking wheel, the whole turning round on the strong post or puncheon S, which is fixed steadily upright by means of the braces and cills LLLLLLLL; and when the wheel and pinion is used instead of the walking wheel, all the other parts are the same. fF is a brace and ladder. E, N, M, F, pulleys for the rope to run over, and come to the weight at H.

Of the Meteor called the Ignis Fatuus, from observations made in England. By the Rev. Mr. W. Derham, F. R. S. and others in Italy, communicated by Sir Thomas Dereham, Bart. F. R. S. N° 411, p. 204.

It being the opinion of divers skilful naturalists, particlarly Mr. Fr. Willughby and Mr. Ray, that the ignes fatui are only the shining of a great number of the male glow-worms in England, or of the pyraustæ in Italy, flying together, Mr. D. consulted his friend, Sir Tho. Dereham, about the phenomenon, being informed, that those ignes fatui are common in all the Italian parts. But of the pyraustæ, or fire-flies, he says, he never observed any such effects, though there is an immense number of them in June and July. He also says, that these pyraustæ are called Lucciole, i. e. small lights, and that they are not the farfalls, as Mr. Ray thought, which are butterflies.

But Mr. D. has reason to think, that insects are not concerned in the ignes fatui, from the following observations; the first made by himself, and the others received from Italy, by the favour of Sir Tho. Dereham.

His own observation he made at a place in a valley between rocky hills, which he suspected might contain minerals, in some boggy ground near the bottom of those hills. Where, seeing one in a calm, dark night, with gentle approaches, he got up within 2 or 3 yards of it, and viewed it with all possible care. He found it frisking about a dead thistle growing in the field, till a small motion of the air made it skip to another place, and thence to another, and another.

It is now about 55 years since he saw this phenomenon, but he had as fresh and perfect an idea of it, as if it was but of a few days. And as he took it then, so he is of the same opinion now, that it was a fired vapour.

The male glow-worms Mr. D. knows emit their shining light, as they fly; by which means they discover and woo the females: but he never observed them to fly together in so great numbers, as to make a light equal to an ignis fatuus. And he was so near, that had it been the shining of glow-worms, he must have seen it in little distinct spots of light; but it was one continuous body of light.

As to the communication from Italy, it is observed that these lights are pretty common in all the territory of Bologna. In the plains they are very frequently observed; the country people call them cularsi, perhaps from some fancied similitude to those birds, and because they consider them as birds, the belly and other parts of which are resplendent like our shining flies. They are most frequent in watery and morassy ground, and there are some such places, where one may be almost sure of seeing them every night, if it be dark; some of them giving as much light as a lighted torch, and some no larger than the flame of a common candle. All of them have the same property in resembling, both in colour and light, a flame strong enough to reflect a lustre on neighbouring objects all around. They are continually in motion, but this motion is various and uncertain. Sometimes they rise up, at others they sink. Sometimes they disappear of a sudden, and appear again in an instant in some other place. Commonly they keep hovering about 6 feet from the ground. As they differ in size, so also in figure, spreading sometimes pretty wide, and then again contracting themselves. Sometimes breaking to all appearance into two, soon after meeting again into one body; sometimes floating like waves, and letting drop some parts like sparks out of a fire. And in the very middle of the winter, when the weather is very cold, and the ground covered with snow, they are observed more frequently than in the hottest summer. Nor does either rain or snow in anywise prevent or hinder their appearance; on the contrary, they are more frequently observed, and cast a stronger light, in rainy and wet weather. But since they do not receive any damage from wet weather; and since, on the other hand, it has never been observed, that any thing was set on fire by them, though they must needs in their moving to and fro, meet with a good many combustible substances, it may from thence be inferred, that they have some resemblance to that sort of phosphorus that shines in the dark, without burning any thing. As to the appearance of this phenomenon in mountainous parts, they differ in nothing else but in size; these latter being never observed any larger than the flame of an ordinary candle. In general, these lights are great friends to brooks and rivers, being frequently observed along their banks, perhaps because the air carries them thither more easily than any where else. In all other particulars, as in their motion, the manner of their appearance, their disappearing sometimes very suddenly, their light, the height they rise to, and their not being affected either by rainy or cold weather, they are the very same with the cularsi above described, or the large Will with a Wisp, as observed in the plains.

A young gentleman, a very accurate and skilful observer of natural appearances, travelling sometime in March last, between 8 and 9 in the evening, in a mountainous road, about 10 miles south of Bologna, as he approached a certain river, called Rioverde, he perceived a light, which shone very strongly on some stones that lay on the banks. It seemed to be about 2 feet above the stones, and not far from the water of the river: in figure and size it had the appearance of a parallelopiped, somewhat above a Bolognese foot in length, and about half a foot high, its longest side lying parallel to the horizon: its light was very strong, insomuch that he could very plainly distinguish by it part of a neighbouring hedge, and the water in the river. The gentleman's curiosity tempted him to examine it a little nearer; in order to which, he advanced gently towards the place, but was surprised to find, that insensibly it changed from a bright red to a yellowish, and then to a pale colour, in proportion as he drew nearer, and that when he came to the place itself, it was quite vanished. On this he stepped back, and not only saw it again, but found that the farther he went from it, the stronger and brighter it grew; nor could he, on narrowly viewing the place where this fiery appearance was, perceive the least blackness, or smell, or any mark of an actual fire. The same observation was confirmed by another gentleman, who frequently travels that way, and who asserted, that he had seen the very same light 5 or 6 different times, in Spring and Autumn, and that he had always observed it in the very same shape and the same place; which seems very difficult to be accounted for. He said further, that once he took particular notice of its coming out of a neighbouring place, and then settling itself into the figure above described.

Of a Lunar Eclipse, observed at Bologna, July 28, *O. S.* 1729. *By Sig. Eustachio Manfredi, M. D. F. R. S. &c.* N° 411, p. 215.

At 11^h 56^m 52^s True time, the eclipse had certainly begun.
12 55 54 The total immersion of the moon.
14 34 25 The beginning of the emersion.
15 35 0 The end of the eclipse.

The same Eclipse observed at Rome. N° 411, p. 217.

At 12^h 1^m 0^s The shadow at the moon's limb.
13 0 16 The total immersion.
14 38 24 Beginning of the emersion.
15 38 0 The total emersion.

A Catalogue of the 50 *Plants from Chelsea Garden, presented to the Royal Society, by the Company of Apothecaries, for the Year* 1728; *pursuant to the Direction of Sir Hans Sloane, Bart. P. R. S. & Med. Reg. By Isaac Rand, Apothecary, F. R. S.* N° 412, p. 219.

An Examination of Mons. Perault's new-invented Axis in Peritrochio, said to be entirely without Friction. By J. T. Desaguliers, LL. D. N° 412, p. 222.

Mons. Perault's account of his engine is as follows: "In imitation of the modern crane, says he, I have invented two engines for raising weights. The first is made of that organ which is the most advantageous of any in mechanics, for facilitating motion; because it is free from that inconveniency which we meet with in all others, viz. the friction of the parts of the machine, which renders their motion more difficult. This organ is the roller, which Aristotle prefers to all others, because all these, as wheels, capstans, and pulleys, must necessarily rub in some of their parts. But the difficulty was to apply the roller to an engine that raises weights, its use having only been hitherto to cause them to roll on a horizontal plane. The engine which I propose has a base AAB, plate 8, fig. 7, something like the crane; having in its upper part the horizontal piece B, which clasps an upright shaft CO, supported under its pivot C, on which the whole engine moves in the same manner as the crane, when the weight is to be lowered. The shaft supports on its top a cross piece DD, to which are fastened the ropes EE, which wrap round the barrel, axle, or roller F, which has another rope G, also wrapping round one of its ends. This last rope is that which raises the weight. At the other end of the axle there is a large wooden wheel like a pulley, HH, about which is wound a long rope N.

To work this engine; pull the long rope N, which causing the great wheel to turn, also carries round the barrel, which is made fast to it. This axle, as it turns round, causes the ropes EE to wind about it, by which the axle and the wheel rise, while the rope F, to which the weight is fastened, also winds itself up on the axle the contrary way; and this double winding up of the ropes causes both the burthen and the axle and wheel to rise at the same time. Now it is evident, that all this rise is performed without the friction of any part, and consequently the whole power, which draws the rope N, is employed without any hindrance; which cannot be in other engines.

It may be objected that the power which acts at N, must, besides the weight, raise also the axle and great wheel, and that their weight is one of those obstacles which Aristotle says all engines are liable to; and that this obstacle is equivalent to the friction which is in other organs. But it may be answered, that friction is an obstacle wholly unavoidable in all other organs; but that it is easy to remedy the obstacles of this, which is done by means of the heavy body M, taken equal in weight to the great wheel and axle, which it sustains by means of the rope II, which running over the pulleys LL, is fixed to the ring or collar K, that goes round the axle F. For the axle and the wheel being counterpoised by this weight, the power which acts by drawing the long rope N, acts for raising the weight only. The experiment which was made with this engine has confirmed the truth of this problem, by comparing its effects with those of a crane, in which the proportion of the size of its axle to the circumference of the wheel, was the same as in this machine: for in the crane, a weight of one hanging at a rope going about the wheel, drew up a weight of 7, when it had one half added to it, to make it preponderate, or give motion to the power: and when the weight to be raised, and the weight which served as a power, were proportionably increased, there was also a necessity to increase the additional weight, which made the power preponderate, in the same proportion: so that as it was required to add one half to the power when the weight was 7, the addition to the power became one for a 14lb. weight, 2 for a 28lb. 4 for a 50lb. and so on; because the resistance from friction increases nearly in the same proportion that the weights are increased. But this did not happen to my engine, in which one quarter was always sufficient for the draught, or to make the power preponderate, not only when the weight was 7, but also when it was 14lb. 28lb. 56lb. &c. which evidently shows that this engine acts without friction."

Thus far Mons. Perault. But however plausible this description may appear, a little attention will show, that if this new engine had no friction, yet it is more inconvenient than an axis in peritrochio with the same proportions; and likewise that it has more friction than the same machine in the common use. ACE, fig. 8, is a common axis in peritrochio, which has the wheel AE 5 times larger in diameter than the axle; so that AC, the radius of the wheel, which is the distance of the power, is to CB the radius of the axle, the distance of the weight, as 5 to 1: consequently 1 oz. for example, will keep 5 in equilibrio. Now though the friction of the gudgeon at C is unavoidable, yet it may be diminished by diminishing the diameter of the gudgeon, provided it remains strong enough to sustain the machine and its burthen. Here one penny-weight, or $\frac{1}{20}$ of the power added to it, makes it preponderate, and give the machine motion with a due velocity.

Now this very engine, used in Mons. Perault's way, so alters the distances of the weight and power, that instead of one for our power, we must have 2½ to keep the very same weight 5, in equilibrio, as may appear by a sight of the fig. 9, where, since in the action of the machine, when we pull the rope PA, we make the axle DB to wind itself up on the rope HD, it is evident that D is now become the centre of motion, DB (the whole thickness of the axis) the distance of the weight $= 2$; and the distance of the power is reduced to AD $= 4$. So that if 2 men, having been employed in the common way to raise weights equal to the strength of 10 men, an engineer should alter the manner of working, and fit up the axis in peritrochio in Mons. Perault's way, instead of gaining an advantage, he must call in 3 more men to perform the work. If it be answered, that what is lost in strength, will be gained in time, it may not only be said, that one cannot always call in more help on the sudden, but that even then, though we should not call this an inconveniency, yet there will be still more friction in this than in the common method; for the roller or axle will find a difficulty to wind on the rope, because they are not perfectly pliable, and the less so, the greater the weight is that stretches them. This, together with the friction of the collar of the rope of the counterpoise to the engine, makes the hindrance greater than in the common way. For it appears by experiment, that when the power is become equal to 2½, to keep the weight 5 in equilibrio, there must be added $\frac{1}{5}$ (here 4 penny-weight) to put the power in motion.

And to show that this friction of the ropes is not always the same as Mons. Perault supposes it; when P, or the power, is made only 1 oz. and W, or the weight, 2 oz. then to make the power preponderate, only 2 penny-weight and

18 grains were sufficient. When P is = $2\frac{1}{2}$, and W = 5, the additional weight marked $\frac{1}{5}$, was 4 penny-weight and 2 grains.

It is plain from this, that Mons. Perault's experiments were very inaccurately made, and therefore not to be depended on.

A further Examination of M. Perault's Machine, said to be without Friction. By the same. N° 412, p. 228.

As some have endeavoured to render this engine more useful, by causing it to roll up an inclined plane, instead of making it rise directly up in the manner described, and condemned in the former paper; it is here shown what must be the loss of the power in proportion to the inclination of the plane; which is, that in every inclination of the plane, if the sine of the angle of inclination be taken in parts of the radius of the axle, or roller, the power will be to the weight :: as the radius of the roller + the sine of inclination, to the radius of the wheel — the said sine of inclination; that is, in the fig. 10, P = 1 : W = 3 :: dk : ak.

In the present experiment BE is an inclined plane, on which the roller c is to roll up, touching the said plane at the point c; AM is the wheel behind that plane, another such plane, and equally inclined, being also supposed behind the wheel, to support the other end of the roller.

The lines of direction of the power and weight being aP and dw, through the point of contact, or centre of motion, c, draw AD parallel to the horizon, and perpendicular to aP and dw; through the centre of the engine, c, draw ad parallel to AD. Suppose the angle BCA of the plane's inclination to be 30°, the right sine will then be equal to half the radius; therefore dividing c2, the radius of the roller, into 2 equal parts at k, if you draw kc and cc, the angle kcc will be equal to BCA, and its sine will be ck. Now since it is evidently the same thing to make use of ad for a lever, whose centre of motion is at k, as of AD, equal and parallel to it, with its centre of motion at c; it follows that in this inclination of the plane, the distance of the weight dk is greater than dc (the distance of the weight in the common use of this engine) by the addition of the quantity ck, the sine of the angle of inclination; and ka, the distance of the power, is less than ca (the distance of the power in the common way) by the subtraction of the said quantity or sine ck: consequently that, on an inclined plane, the power is to the weight :: as DC : to CA. Q. E. D.

Corol. 1.—Hence it follows, that the radius of the wheel and the radius of the roller being given, the loss of power may be found in any inclination of the plane. Thus, as here, the power, which in the common way would be but $\frac{1}{5}$ of the weight, must be $\frac{1}{3}$ part of it: and if the angle of the plane's inclination was but 11° 32′, the power would be $\frac{1}{4}$ of the weight, &c.

Corol. 2.—Hence it follows also, that if the plane BE be horizontal, no force of the power will be lost, because cg : cf :: CG : CF.

Scholium.—As the friction of the winding of the ropes, such as BC in the new way, is greater than the friction of the pivot in the old way, besides the friction of the collars of the counterpoise to the engine, so that friction diminishes, as the ropes bear less weight, according to the diminution of the angle of the plane; and when the plane is horizontal, and without a counterpoise, even then the winding up of the ropes, and pressure of the roller against the plane, is equal to the friction in the common way.

Of the Equuleus or Wooden Horse of the Ancients. By Mr. John Ward, Prof. Rhetoric in Gresham College, and F. R. S.* N° 412, p. 213. *Abridged from the Latin.*

For what purpose the equuleus or wooden horse was first instituted, and to what use it was applied, is pretty evident from several passages in ancient authors. But since none of them have described its figure, and the manner of constructing it, learned men have run into various opinions, and those widely differing from each other. And indeed this will not seem surprizing to any one who considers, how difficult very often it is to determine with certainty on things that have been in disuse for several ages, and removed from our view; especially, if the ancient authors, who mention them, do it only slightly, and do not fully describe them. This, then, was the case with the equuleus; nor did any of the learned who, after the revival of literature, undertook to describe this machine, seem to Mr. Ward to have done it with such success, as that their description agreed in every respect with what the ancients deliver about it. So that he had entirely laid aside all hopes of obtaining any greater certainty in this affair: but being favoured with the sight of some papers sent to Dr. Mead from Rome, in which were delineated several figures of an ancient work, still extant there; among others he happened to light upon one, drawn from a marble, in the prince of Borghesi's palace, which, as he conjectured,

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* Dr. John Ward was a very useful member of the Royal Society, and for many years one of the Vice Presidents. He was born at London in 1679; and in 1720 was chosen professor of Rhetoric in Gresham College, where he died in 1758; being also one of the trustees of the British Museum. He wrote Lectures on Oratory, in 2 vols; also the Lives of the Professors of Gresham College; with a great number of learned papers in the Philos. Trans. chiefly on curious subjects of antiquity, contained in most of the volumes from vol. 36 to vol. 49. His writings are replete with learning, and curious remarks; but his manner is rather laboured and tedious.

represented a man suspended on the equuleus: and on more attentively considering the matter, and more carefully inquiring into the passages in ancient authors, where mention is made of the equuleus, and diligently comparing them with this figure, he plainly discovered not only the mistakes of modern writers on that head, but as he thought, the causes of their mistakes.

Some have erroneously confounded the equuleus with the crux or cross: but this latter was a kind of gibbet, to which slaves and others of mean condition were affixed, and punished with death: whereas on the equuleus the torture was applied in order to extort confession.

But among the various opinions, Mr. Ward selects only two, as coming nearest the truth. The one, that of Hieronymus Magius de Equul. c. 1, who rightly judged it to have been made in the shape of a horse. The other, that of Caracciolus, apud Ferrar. Elect. l. 1, c. 5, who, no less rightly, judged it to have been an erect stake. Now Mr. Ward endeavours to show, that both were mistaken (not to mention other errors) in imagining that the equuleus was always of the same form.

In the more early times the equuleus was in some measure made like a horse, with its back flatted and of such length and breadth, as that a man's body might be conveniently extended on it: and he who was to be tortured, did not sit, but lay on his back, with his arms writhed back under the equuleus's breast, his hands bound and feet extended. The equuleus was provided with two pulleys of different sizes; the lesser placed between the buttocks, made hollow to receive it; and the larger, with a handle to it, under the belly. The executioner after tying both feet with cords, passed the cords over the smaller pulley, and fastened them to the larger one; which last as he turned round with the handle, he could stretch the body, till all the joints were loosened, and that with the most exquisite pain.

In the next place Mr. Ward produces the testimonies of ancient authors, that may confirm this description. The very name seems pretty plainly to show that the equuleus was shaped like a horse; as there is at this day among us, such another sort of machine for military punishments, called the wooden horse: and the same thing is manifest from those modes of speech, borrowed from the horse and applied to the equuleus.

The equuleus, as had been said, had not always the form of a horse; but in latter ages was changed into a quite different one; but though it changed its figure, it still retained the name, a thing not uncommon: for, not to mention other instances, that warlike engine, which from its resemblance, to a ram's head, was called aries, had not always the form, from which it originally took its name.

The equuleus, therefore, in these days was an erect stake, at top of which lay a cross piece of timber, incurvated at both ends like horns; and provided, as the former, with two pulleys; the lesser of which was fixed into the lower part of the stake, made hollow to receive it; the larger had a handle to it, and was fastened behind: the person to be tortured, being raised upon the equuleus, hung with his arms bent back on the cross piece of timber, and with his hands bound behind to the stake; his feet were also tied with cords, which, passing over the less pulley, were received into the large one fixed to the back part of the equuleus, by turning of which round, the body was stretched.

And since ecclesiastical writers, who give an account of the exquisite tortures of the martyrs under the Roman emperors, make frequent mention of this sort of equuleus; their testimonies are of especial use in proving its figure.

Mr. Ward quotes a number of passages from ancient writers, having reference to this mode of torture, tending to show the different forms and uses of this instrument; which he illustrates with several engraved figures, not necessary to be here retained.

On the whole then, since the entire proof of this matter chiefly depends on the testimonies of ancient writers; and as Mr. Ward thought it superfluous to adduce any more, though very numerous; so he was of opinion that fewer would not be sufficient to explain it fully. But whatever accounts the ancients give on this head, they may easily be referred to one or other species above described. One, therefore, who attends to the age of the author, the different modes of speaking used at different times, can, Mr. Ward thinks, find no difficulty for the future, what he ought to determine about this kind of torture; in explaining which so many learned men have hitherto perplexed themselves to no purpose.

An Account of a Treatise entitled, Calculations and Tables relating to the Attractive Virtue of Loadstones, &c. 1729. N° 412, p. 245.

The author, the Hon. Lord Paisley, by several experiments very carefully made, has observed, that if two loadstones are perfectly homogeneous, that is, if their matter be of the same specific gravity, and of the same virtue in all parts of one stone, as in the other, and that like parts of their surfaces are capped or armed with iron, then the weights they sustain, will be as the squares of the cube roots of the weights of the loadstones; that is, as their surfaces.

On this principle the tables are formed. The first column of these tables is in common to the four following, and serves to show how many times its weight any loadstone sustains.

The author also explains the use of these tables, by instances under each denomination.

An Account of a Book entitled, Jo. Frider. Wiedleri, Observationes Meteorologicæ et Astronomicæ, Annorum* 1728 *and* 1729, *&c. Wittembergæ, Anno* 1729. N° 412, p. 250.

The author, after dedicating his Tracts to the Royal Society, gives a description of the particular sort of barometer, thermometer, hygrometer, and hyetometer, which he made use of in the subsequent observations. This book gives a diary of the weather, from the vernal equinox of the year 1728, to that of the year 1729; containing the daily state of the barometer, thermometer, wind and weather, with the quantity of rain during that time. To this the author annexes some select meteorological and astronomical observations, which he describes at large.

The first he takes notice of is a remarkable halo round the moon, on Feb. 20, 1728, at $7^h\ 45^m$ in the evening, when the moon was not far distant from the meridian, and about her first quarter. The diameter of the halo occupied about 47 degrees. Its arch was $4\frac{1}{2}$ deg. broad. Within it was red, and towards the extremity pale; exhibiting entire a beautiful spectacle for about 4 minutes. The same day at noon, he observed 13 spots on the sun; the largest equalling $\frac{1}{34}$ of the sun's diameter; and the spirit fell to 90 deg. of the English thermometer.

April 4, 1728, he observed an aurora borealis.

June 20, another, which is described in the Act. Erudit. Lips. Ann. 1728, p. 375.

Oct. 7, a very remarkable one appeared in the N. E. A white arch, extending between the W. and N. E. quickly assumed a black colour, and then divided into three other concentrical arches equally black. From these some radiations arose as usual, but shorter. A little afterwards these likewise ceased, and

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* John Fred. Weidler was professor of Mathematics at Wittemberg. Besides a number of communications to the Royal Society, contained in volumes 36, 38, 39, 40, 41 of the Philos. Trans. he was author of several separate works: as,

1. Institutiones Mathematicæ, in 8vo. 1725. This is a very thick volume, and contains a general, though concise course, of all the mathematical sciences.
2. Observationes Meteorologicæ et Astronomicæ, 1729, above described.
3. Historia Astronomica, in 4to. 1741.

the black arches were converted into luminous tracts, only one remained till eleven o'clock. Then follow several observations of eclipses, with conjunctions and various positions of the planets.

The author next gives 14 astronomical observations, 10 of which are of the eclipses of Jupiter's satellites at different times.

His observation on the declination of the magnetical needle in this and the former year, shows the variation to be 12° 0′ 55″ west at Wittemberg.

These observations are followed by the author's account of the last hard winter. This set in sooner than usual, and the spirit of wine in the English thermometer, on Sept. 21, fell to the 66th degree. After this the frost continually increased; so that on Jan. 20 following the cold was intolerable, and the spirit descended to the 126th degree, very little remaining above the ball of the tube. And very little abatement of the cold was perceived till the last day of March, when it relaxed gradually.

From these observations the author compares this winter with the memorable one of 1709, and proves, both from thermoscopical observations, from its effects on the earth and animals, from its longer continuance, and from the greater extent of the cold into the more southern parts, that this last much exceeded the former, at least in Germany.

An Occultation of Venus by the Moon, observed at Berlin, Sept. 19, *p. m. N. S. By M. Kirch.* N° 412, p. 256. *From the Latin.*

The first contact of Venus with the moon was at $2^h\ 2^m\ 16^s$, and the total occultation at $2^h\ 3^m\ 1^s$. With an 18-foot telescope M. Kirch observed, that as soon as Venus, being nearly in quadrature, approached the moon's disc, she changed her figure, and lost her horns, and assumed an oval figure: which appearance Mr. Kirch thinks may be an argument for an atmosphere about the moon.

An Account of 2 *very extraordinary Cases; in a Letter from Dr. J. Huxham to W. Rutty, M. D. R. S. Secr. From the Latin.* N° 413, p. 257.

1. In the first of these 2 cases an account is given of a very large stone in the urethra, accompanied with strangury and excruciating pain, and giving rise to a tumor in the middle and upper part of the scrotum, and to the formation of 2 or 3 fistulous ulcers in that part (the scrotum); through which the greater part of the urine was voided, sometimes accompanied with pus. After suffering the greatest tortures for several years, the patient was at length admitted into the Plymouth hospital; where, while he was one day straining to make water more violently than usual, he voided through the before mentioned

fistulous opening in the scrotum, a stone weighing 5½ oz. avoird. The lacerated scrotum (then no longer tumefied) was capable of admitting a child's hand. On examination it was found that the stone had come from the urethra. What seemed very extraordinary, this large wound soon healed, with the exception of a small fistula in the upper part of the scrotum; and the patient, who could scarcely stir himself at all before he voided the stone, was afterwards capable of walking about without difficulty.

2. A lady was brought to bed of a daughter perfect in every respect, except that, in the lower part of the lumbar region, there was a tumor or cyst about as large as a child's fist, and that moreover the infant's legs were crossed, and the feet applied to the buttocks after the manner of a tailor sitting at his work; and the child was not able to move them from this posture, to the great astonishment of every person who saw her, and the great grief of her parents.

On examining the tumor, Dr. H. found it to be of the same kind with those mentioned by Tulpius. (Observ. cap. 29 and 30, lib. 3.) He prescribed an aromatic and astringent fomentation; but in 3 days time the tumor was so greatly distended with serum, that the external membrane had begun to break, and fears were excited, lest the tumor suddenly bursting, its contents should be discharged all at once, and so prove fatal. A small puncture was therefore recommended that the lymph might be gradually let out; though it was at the same time represented that whether the tumor was opened or not, it would probably occasion the death of the infant.

On the 4th day the tumor was opened, a larger orifice being made than was intended; so that the contained fluid was discharged in the course of 24 hours. Excessive debility and frequent faintings succeeded for 2 days; but by the assistance of cordial medicines, and good nursing, the child was kept alive for 20 days; at the expiration of which she died in convulsions.

On opening the body, the medulla dorsalis was found to reach not farther than to the last lumbar vertebra; but going out between this vertebra and the os sacrum, it was distributed over the membranes of the tumor or cyst. The os sacrum was solid throughout; and had no foramina for transmitting the nervous propagines of the medulla spinalis (known by the name of cauda equina) to the lower limbs.

An Account of the Imperial Salt-Works of Sóowár in Upper Hungary. By Ernest Bruckman of the Academy of Brunswick, M. D. Communicated by Sir Hans Sloane. N° 413, p. 260.

Sóowár is an Hungarian word, signifying in German salt-burg, composed of so, i. e. salt, and wa, that is, burgh or town where the salt-works are, Eper, a city of the county of Saár entirely peopled with officers of the excise, and miners or wood-cutters, and is situated on the summit of a little hill, with an agreeable prospect.

The 16th of July 1724, we came from Rosenaw to Sóowár with Dr. Poekin, physician to the city and county, to view this celebrated salt-work, which furnishes the finest and most pure salt of the whole kingdom. We communicated our intention to an officer of the salt-works, and having asked his leave to go: we first descended into the works down the well by a rope, seated on leathern dogs (as they term it) about 40 fathom deep; after which we again descended 100 fathom, by clinging perpendicularly against the wall and sides of the wells; and having again continued our journey under ground in the salt-work, we then found ourselves in the cuts, and saw all the allies cut in the finest rock-salt; in the midst of which there were here and there some veins of a dark grey flint. The miners work to cut this rock-salt, which they draw up by a rope, and put it into a reservoir, where they cleanse it with salt-water. They boil it afterwards with the same water, till it becomes of the consistence of crystal, and then put it into vessels, which contain about 268lb. weight each, and then send it into Silesia and other countries.

As to the vegetable or fossil-salt, it is extremely white and transparent; and in such plenty in the salt works of the county of Marmar, near Transylvania, where there are large entire mountains of salt, that one might furnish the whole world for quantity; as also, because as soon as cut, it grows again anew in a very short time. They break and cut it; and though it appear at first black, yet in pounding it becomes extremely white: and so it is with that used in Hungary, for they send all the salt of Sóowár into foreign countries. You find almost in every inn, two stones like to those used to make mustard, between which they pound and break that sort of rock-salt; and one finds also in their stables, large pieces of that mineral, which the cattle lick at pleasure.

But to return to the salt of Sóowár, there are sometimes in the cuts, allies of rock-salt, of the most delicate blue and yellow colours. We observed, that of the first colour being exposed to the sun for some days, lost entirely all that beautiful ultra marine, and became white as the other rock-salt; which did not happen to the yellow, which preserved its colour; but when pounded both together, the salt was neither blue nor yellow, but produced a salt extremely white.

Melissantes, in his new Geography, p. 228, speaking of salt-works, which the Spaniards have in Catalonia, says, that there is rock-salt, the colour of which is so diversified, that it comes near the rainbow, in having green, red, yellow, and blue colours; but that by first preparing, and then grinding it, it becomes white. The same thing happens also to the red rock-salt of Saltsburg, which being pounded becomes white.

There is in these cuts, 4 fountains of salt-water, which they put into buckets made of buffalo's skins sewed together, and draw it up by an engine worked by horses, and convey it by pipes into the boilers, where they put the rock-salt to dissolve, which they afterwards boil till it becomes like crystal. By express mandates of the emperor, no one can sell that fossil salt, neither can the Hungarians employ it for their own use, much less drive any trade in it, but they boil it all, and export it into foreign countries.

They find here also a sort of crystallized salt, like the crust adhering to the pipes of wood: the miners call it salt of crystal; it is very white and transparent, but this appeared to us, nothing else but salt falling drop by drop in its passage in the pipes, and so crystallizing, which they easily also separate.

But what is most curious and remarkable in these subterraneous fosses, are the flowers of salt, which grow as the beard of a goat, with this difference only, that these are much whiter, and much finer. One cannot enough admire these vegetables, yet one cannot find them in all the cuts, nor at all times, but they appear and grow according to the temperature of the seasons, which in those parts is very wholesome, and without any thing noxious. These sort of plumes of salt are very brittle, they met also in moist places, and dissolve into an evaporated oil, but are nevertheless the most pure salt, the finest, the most acid, the most white, and most beautiful; so that it is not without reason they have given it the name of flower of salt.

The salt of Sóowár is esteemed the best of all Hungary, the greatest part of which they export into Silesia, Moravia, and Bohemia, and the Hungarians dare not use any of it themselves, under pain of banishment. They make every year about 50,000 tun, every tun containing 268lb. but by an ordonnance of his Imperial Majesty, they will henceforward boil about 100,000 tun, which they will export as the other.

We saw at Neusol, a statue of rock-salt, as large as life, which serves as the barometer of Neusol; for when it begins to sweat, or grow moist, it presages rain, or wet weather; but when it is dry, it indicates settled fair.

The Natural History of Cochineal; being an Account of a Book, entitled, Histoire naturelle de la Cochinelle justifiée par des Documens authentiques. Amsterdam, 1729. *By W. Rutty, M. D. Sec. R. S.* N° 413, p. 264.

[The history of cochineal being now well known, it is of course, unnecessary to reprint the present paper. The animal is the *coccus cacti* of Linnæus, and will be found described, in a subsequent paper, by Mr. Ellis.]

An Anatomical Description of Worms, found in the Kidneys of Wolves. By Mr. James Theodorus Klein, Secretary of the City of Dantzic, F. R.S.* N° 413, p. 269.

These worms were sent to Mr. Klein from Sewaldia, in Eastern Prussia.

Fig. 1, pl. 10, represents a female worm found in the kidney of a she wolf.

Fig. 2, the kidney of a wolf, resembling a bag, on account of the almost entire consumption of its parenchyma. It contained 8 worms; some of a yellowish, others of a blood colour; 2 of which were females, and 6 males.

The females were more than twice longer and thicker than the males. They were furnished with three very visible holes; the first of which performed the function of the mouth; the second of the anus; the third of the vulva. This last hole is seen under the belly, about 1½ inch from the mouth; as at abc in fig. 3.

The membranous skin was marked with annular fibres, and 7 or 8 chestnut-coloured lines, as at d, running the whole length of the worm. The skin being cut, a limpid humour issued forth, and then appeared the transversal fibres interlaid on every side with the viscera, and are all round about inserted into the skin in the interstices of the vesicles, and at the same time the viscera appeared, which the sole parts destined for nutrition and generation seem to make up.

The alimentary passage is composed of two canals, one of which bb, fig. 4, begins at the mouth, and is about 2 inches long, smooth, fleshy, whitish, and endowed with thick coats, serves for receiving the nourishment. As this duct proceeds with equal thickness, it is once reflected and retorted before it enters the other, cccd, which is of a dark brown colour, much broader and tenderer than the first, flatted, membranous, covered with very fine coats, wrinkled like a swathing cloth, then runs into transversal and winding sinews, and extends in a straight line to the anus. The inner coat of this canal seemed somewhat rough, and as it were strewed with dust. The contained liquor was perfectly fluid, and of a faint sooty colour.

Near the anus was fixed to the skin, the end of a whitish tender vessel, which thence proceeded straight to the beginning of the alimentary canal, where reflecting towards its origin, and again resuming its first way, after being

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* These worms appear to belong to the Linnæan genus *ascaris*.

contorted and implicated in many and various windings and curves, widens and straightens here and there, till at length becoming more and more capacious, it forms a little bag, for which a whitish, fine, smooth canal, about an inch long, covered with pretty thick coats, piercing through the skin 1½ inch from the mouth, prepares an outlet, marked under the belly with a caruncle, as in fig. 3, and h fig. 5, 6. This little canal may, not improperly, be called the oviduct or vagina.

The colour of these parts is not every where the same; for of whitish at the beginning, in the progress it insensibly becomes darker: and at length, where the vessel acquires a greater volume, and especially where it stretches forth into the bag, it is of a chestnut colour. And as far as this chestnut colour continues, the vessel is thick stuffed with myriads of eggs, and therefore is to be called the ovary.

The eggs, whose number is certainly incredible, seen with the naked eye, resemble a magma of a brown colour; but viewed through those microscopes, which in the English apparatus bear the second and third number, they are of the figure marked a and b in fig. 7.

The surface of the inner skin, which inclosed the abdominal contents, was all beset with small whitish bladders, of different figures and sizes, pouring out a lymph when torn. These were in the females.

Though the integument of the male be marked with annular fibres, and as many chestnut-coloured lines, as that of the female, throughout its whole length, yet his external shape differs from that of the female: 1st. Because he is much less. 2dly, Because, the third hole, viz. that under the belly, is wanting in the male. 3dly, Because the anus of the male is surrounded with a thick cartilaginous membrane, of nearly an orbicular figure, about a line broad, externally convex, internally concave; on the middle of which appears a tubercle, divided by a fine slit, which lets out the excrements, and a very small capillary process, k, fig. 8. The cavity of the belly contained a limpid humour, the transversal fibres, the alimentary canals, and the spermatic vessels. The alimentary passages had the same situation and structure as in the female; the anterior canal was of a whitish colour, the posterior, or wrinkled one, of a pale brown.

The spermatic vessels were very white and slender, yielding, when wounded, a milky humour. They are divided into two small branches, hanging out of a vermicular process, scarcely an inch long, which lies in the belly, in that place where the alimentary canals are joined together, and leans on the side of the wrinkled canal, by the help of the transversal fibres. These branches, in their progress hence, creeping above and below the canal of the aliments, are very often reflected, intorted and folded; one at length freed from its windings, stretches away straight towards the anus, into which it is inserted in the shape of a pretty stiff vessel; but the other, at the side of the wrinkled canal, being pressed, collected, and equally inflected, almost through its whole extent, by the transversal fibres, ends in the opposite side, by an extremity pendulous in the belly, not far from the anus.

The inner coat of the skin, just as in the females, is all covered with small whitish bladders, turgid with lymph, but less, in proportion to the less bulk of the worm.

Under the wrinkled canal was found a certain whitish duct, marked with the letters bbb, fig. 8, firmly connected to the aforesaid intestine by its finest part; but whose outlet or origin, the tenderness of the intestine, and fineness of the duct, hindered us from tracing with exactness.

The following Figures represent the Worms, drawn according to their natural Size.—Fig. 3 represents a female worm: a, the worm's mouth; b, the anus; c, the vulva; d, the chestnut-coloured lines, running along its length.

Fig. 4; a, the worm's mouth; b, the alimentary canal, which is white, carnous, &c.; c, the alimentary canal, which is brown and flatted, and whose extremity is in the anus; d, the place where the canals join; eee, the transversal fibres; f, the anus.

Fig. 5 and 6; a, the worm's mouth; bb, the first alimentary canal; cc, the latter alimentary canal; d, the place where these two canals cohere; eee, the transversal fibres; fff, the white vesicles turgid with lymph, with which all the inner skin is thick beset; g, the anus; h, the vagina; s, the oviduct; i, the outlet of the vagina, or the vulva; kk, the ovary filled with innumerable eggs; ll, the vasa præparantia.

Fig. 7; the eggs viewed through a microscope; a, through the microscope, N° 3; b, through the microscope, N° 2.

Fig. 8; a male worm; a, the mouth; bb, the whitish alimentary canal; cc, the wrinkled canal of the aliments; d, the vermicular process of the spermatic vessels; ee, a branch of the spermatic vessels along the side of the intestine, compressed by the transversal fibres, and inflected through its whole extent in a uniform manner; fff, the windings and turnings of the spermatic vessels; gg, the transversal fibres; h, the cartilaginous membrane surrounding the anus; i, the small slit in its middle; k, the very fine capillary process; mm, the small bladders covering the skin.

Fig. 9; a male worm inverted, and dissected about the anus, in order to see with ease the duct lying under the alimentary canal; a, the wrinkled alimentary canal; b, the whitish duct under the wrinkled canal; c, the spermatic vessels.

Fig. 10; a, the vermicular process of the spermatic vessels; bb, the branches of the spermatic vessels, freed from their windings; cc, the same branches dissected.

Observations in dissecting an Ostrich. By Mr. Ranby, F.R.S. N° 413, p. 275.

Mr. Ranby adds two or three more observations, which escaped his notice in his former dissection of the ostrich, in N° 386.

And first, as to the eye, its figure, when taken out of the orbit, he thinks particular, being almost triangular, with some little variation in the bony scales. The contents of the stomach were of such a kind, that they were hardly capable, without very great alteration, of passing the lower orifice, which is very small.

The diameter of the duodenum is much smaller than any of the intestines, and free from valves, as are the jejunum and ileum, except the latter, which has a few valves, as it approaches near the colon. The colon was uneven, with very regular cells: these cells were formed by valves, which were on the inside, and transversely situated, each making more than half a circle.

The parts in other respects agree with the description given by the several curious gentlemen, who have dissected this animal.

A new Kind of Hydrometer, made by Mr. Clarke, and communicated to the Society by J. T. Desaguliers, LL.D. F.R.S. N° 413, p. 277.

The hydrometer, by some called areometer, is an instrument commonly made of glass, consisting of a stem AB, fig. 8, pl. 9, graduated by small beads of glass of different colours, stuck on the outside, a larger ball, B, quite empty as well as the stem, and a small ball, C, filled with quicksilver before the end A, was hermetically sealed, in such manner, as to make the hydrometer sink in rain water as deep as m, the middle of the stem. Such an instrument does indeed show the different specific gravity of all waters or wines, by sinking deeper in the lighter, and emerging more out of the heavier liquors; but as it is difficult to have the stem exactly of the same thickness all the way, which, if it could be had, the same instrument would not serve for water and spirits, sinking quite over head in spirits when made for water, and emerging in water with part of the great ball out, when made for spirits. The hydrometer has only been used to find whether any one liquor is specifically heavier than another; but not to tell how much, which cannot be done without a great deal of trouble, even with a nice instrument. The hydrostatical balance has supplied the place of the hydrometer, and shows the different specific gravity of fluids to a very great exactness. But as that balance cannot well be carried in the pocket, and much less managed and understood by persons not used to experiments, Mr. Clarke was resolved to perfect the hydrometer for the use of those that deal in brandies and spirits, that by the use of the instrument they may, by inspection, and without trouble, know whether a spirituous liquor be proof, above proof, or under proof, and exactly how much above or under: and this must be of great use to the officers of the customs, who examine imported or exported liquors.

After having made several fruitless trials with ivory, because it imbibes spirituous liquors, and thereby alters its gravity, he at last made a copper hydrometer, represented by fig. 9, having a brass wire of about $\frac{1}{4}$ inch thick going through, and soldered into the hollow copper ball, Bb. The upper ball of this wire is filed flat on one side, for the stem of the hydrometer, with a mark at m, to which it sinks exactly in proof spirits. There are two other marks, A and B, at top and bottom of the stem, to show whether the liquor be $\frac{1}{10}$ above proof, as when it sinks to A, or $\frac{1}{10}$ under proof, as when it emerges to B, when a brass weight, such as c, has been screwed on, to the bottom at c. There are a great many such weights of different sizes, and marked to be screwed on, instead of c, for liquors that differ more than $\frac{1}{10}$ from proof, so as to serve for the specific gravities in all such proportions as relate to the mixture of spirituous liquors, in all the variety made use of in trade. There are also other balls for showing the specific gravities quite to common water, which makes the instrument perfect in its kind.

An unusual Aurora Borealis seen at Geneva. By Mr. G. Cramer, Prof. Math. there.* N° 413, p. 279.

This phenomenon occurred Feb. 15, 1730, N. S. And what was chiefly remarkable in it, was a large meridional zone, like a rainbow in its figure, but broader. It was terminated by two parallel arches. The superior insisted with one side on the true point of east, and with the other on the point of south-west, or west-south-west: whence its middle declined about 15° from south to east, and was diametrically opposed to the middle of the aurora borealis. Its

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* Gabriel Cramer was born at Geneva in 1704, and was appointed Professor of Mathematics there at 19 years of age. The public are indebted to him for the edition of the works both of James and John Bernoulli, which he collected and published. In 1746 he finished his L'Analyse des Lignes Courbes, 4to. but which was not published till 1750. Cramer was a member of several academies; as those of London, Berlin, Montpelier, Lyons, Bologna, &c. He was celebrated not only for his mathematical talents, but as a universal genius, a kind of living Encyclopedia, and a man of most exemplary conduct. He died 1752 in Languedoc, where he had gone for the recovery of his health.

altitude varied a little, but never reached higher than the head of Orion, which was 54° high, and never was seen lower than a little under Procyon, which is an altitude of 45 or 46°. The inferior arch was exactly parallel to the superior, and the breadth of the zone varied from 14 or 15° to 18 or 20°.

The colour of this zone was red, scarlet, inclined to purple, pretty lively and changeable by intervals. It was less vivid near the horizon, and also to the meridian, where it seemed now and then interrupted. Some imagined two great arches rising, one from the east, the other from the south-east, and meeting together near the meridian, but immediately afterwards parting, and drawing back, which they repeated very often.

Under this zone was to be seen, but not constantly, one or two lucid and interrupted arches, which comprehended with the horizon a dark segment very like a mist.

It was remarkable, that this Aurora considerably darkened the light of those stars which were seen through it; and that was much more true of the red meridional zone, which died with its reddish colour the stars that appeared behind. When that zone was the highest, it covered Jupiter; and some gentlemen, which at that time had not yet remarked the Aurora, looking at Jupiter through a telescope, affirm they could hardly see it, but that it seemed as intercepted by some dark cloud; and indeed it looked at that time as if it had been seen through a red glass.

This observation confirms what is moreover very probable, that this zone was produced by the light of the opposite Aurora, either by reflection or refraction. But the manner of its production seems difficult to be accounted for. There may be supposed icy particles swimming in the air, and of such figure as to exhibit a great zone, by the reflection and refraction of the light of the Aurora, almost in the same manner as the drops of rain produce the appearance of the rainbow. But this is mere conjecture.

*Of a Spiritus Vini Æthereus; with several Experiments tried with it.** *By Dr. Frobenius, F. R. S.* N° 413, p. 283.

Exper. 1.—The ether of plants appears to be almost destitute of all gross air, from placing it under the receiver of the air-pump; for exhaust the air

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* Bergman (Phys. and Chem. Essays, Vol. III. p. 109, English Transl.) relates, that the method of preparing ether was first described, under the name of Ol. Vitr. dulce, by Valer. Cordus in 1542, and that there are some obscure traces of it in Basil Valentine; but the above appears to be the first distinct account of some of its most remarkable properties. It is uncertain whether Frobenius was a real or a fictitious name, but he was a native of Germany. A particular account of his process for preparing the ethereal liquor was not published until several years after, in the 41st vol. of the Philos. Trans.

ever so accurately, this ethereal liquor remains unmoved, nor does it emit any air-bubbles, which immediately arise in other liquors, and according as their quantity of intrinsic air is greater, so much the sooner are such liquors put into agitation, and emit also more froth, and more vehement ebullitions in proportion to viscidity. Hence it follows, that this ether may be preserved best (because without any diminution) under the receiver in vacuo; whereas, on the contrary, exposed to the open air, its parts soon evaporate, and its whole bulk, but not compressed by the air, vanishes. (This experiment failed remarkably.)

Exper. 2.—A little of it poured on the surface of the hand, affects it with a sense of cold equal to that from the contact of snow; and blow on it, but once or twice with your mouth, immediately the hand becomes dry. Beware however of approaching a lighted candle with your hand thus wet, lest it take fire and burn you. (Succeeded.)

Exper. 3.—It causes such a stridor and hissing, when poured on hot water, as is frequently occasioned by a piece of hot iron thrown into it. Take a lump of sugar, imbibe some of this ethereal liquor, and put it into a vessel full of hot water, the sugar will indeed sink to the bottom, but the ethereal liquor rushing violently forth, excites a great ebullition in the water. And, if one spoonful of this ether be poured into a copper-pot-full of boiling water, without any sugar in it, and approach immediately with a candle or a lighted paper, instantly there issues forth from the water very great lightning. The handle of a spoon, as well as the tongs for holding and applying the lighted paper, must be of a proper length, that the effusion of the ethereal liquor on the hot or boiling water, and the application of the lighted candle or paper, may be performed at the same time; otherwise the ether is immediately dissipated, without any such effect. There is therefore need of an assistant, or of both hands, and also of a room where entrance may readily be given to fresh air, proportionable to the magnitude of the flash of lightning which so rarefies the air, as to endanger the stoppage of respiration. (Succeeded.)

Exper. 4.—Hence it appears, that this ether is both fire and a very fluid water, but so volatile as it soon evaporates, and that it is the purest fire; insomuch, as if kindled in 1000 times the quantity of cold water, it burns inextinguishably. Therefore, if you take an earthen vessel of any magnitude, whose mouth or orifice may be 1 or 2 yards wide, but the interior breadth of the vessel may contain 600, or 6000 gallons of water, the experiment will be the same, pour on the top but 1 oz. or a small vial full of this ether, and apply to it a lighted wax-candle, it takes fire immediately, burns placidly, and is so far from being extinguished by the most profuse superaffusion of common water, that it much increases the vehemence of the flame, which lasts till the subtle parts of the ether are consumed and ventilated by the flame. This experiment should be made in a large and lofty room, not in danger of taking fire. (Not showed.)

Exper. 5.—The sense of touch does not manifest the least oiliness or fatness in this ethereal liquor, though it is the true, natural, and only dissolvent, or menstruum of all fat, oil, rosin and gum whatever: by means of which all sorts of fat, and every kind of fire or flame is extricated by a speedy, safe, and pleasant operation. On these accounts it is that this ethereal liquor will not unite with any kinds of salts whatever; but all sorts of oils, pitch, turpentine, opobalsams, camphor, wax, ambergris, spermaceti, mastic, musk, copal, and the like, it dissolves most readily, and with the greatest ease extracts their best essences.

Exper. 6.—And indeed a wonderful harmony is observable between gold and this ether, even greater than between gold and aqua regia; insomuch as from hence gold appears to approach nearer to the nature of oils than of earths, as shall be proved when we treat in their proper place of the three harmonious menstrua which we have discovered, viz. The corrosive one, for the devoration or solution of earths, minerals, and metals; the aqueous one, for the solution of all kinds of salts; and lastly, the ethereal liquor, or oleous menstruum. If a piece of gold be dissolved in the best aqua regia, and on the solution cold, be poured ½ oz. or what quantity you please of the ethereal liquor, shake the glass carefully, and all the gold will pass into the ethereal liquor, and the aqua regia, robbed of all its gold, will presently deposite the copper at the bottom of the vessel as a white powder, which turning of a green colour, contains the portion of copper with which the gold was adulterated. The ether will swim like oil on the surface of the corrosive waters. The experiment deserves the utmost attention; for here the heaviest of all bodies, gold, is attracted by this very light ether, or the gold, by the force of its gravity, as by an impulse, would descend from thence; or lastly, this phenomenon is owing to a certain harmony and similitude of them both. (Succeeded.)

Exper. 7.—Ether then is certainly the most noble, efficacious and useful instrument in all chemistry and pharmacy, Ubi enim ignis potentialis, ibi actuali non opus est, inasmuch as essences and essential oils are extracted by it immediately, without so much as the mediation of fire, from woods, barks, roots, herbs, flowers, berries, seeds, &c. from animals, and their parts too. Thus, from castor, by a certain manufaction, may be prepared an oil sweeter than that of cinnamon, and also the true oil of saffron, of wonderful efficacy; and all by this particular encheiresis, without the help of fire or distillation. For an example of our method, take mint, sage, or orange-peels, cinnamon,

&c. or all these together, cut and bottle them, pour on them a spoonful or two of the ethereal liquor, and after it has stood an hour in a cold place, fill up the bottle with cold water, and the essential oil will swim on the water poured upon them, easily separable by the funnel, or instrumentum tritorium. Of this essential oil, one drop only on a lump of sugar, manifests to the taste, &c. the medical virtues of the plant, exquisitely drawn out, comprehended in this essence, deservedly named cos, as containing the colour, odour, and sapor or taste of the plant or plants. In like manner the essential oils of exotics are easily prepared. (Succeeded.) But it is not a true essential oil, but an excessive strong tincture, which may be called the essence.

Exper. 8.—Of the like use it is in the animal kingdom, where it produces an essential oil of phosphorus, as also in the mineral kingdom, though not so immediately, because the resolution of earths must precede. Moreover, it is easily proved that the same liquor extracts the purest gold, or every part of the golden system from any, or all the baser minerals; and that this gold, thus extricated, is by this one operation better and sooner purified, than by fusion of minerals with antimony.

Exper. 9.—This water is neither corrosive nor joined with apparent corrosives. Therefore fill as many bottles with ethereal water as there are sorts of salts, and into the first, drop oil of vitriol; put into the second spirit of sea-salt, into the third spirit of nitre, or of alum, or of sal-ammoniac prepared with water, or the lixivium of tartar, or rectified wine-vinegar; all the salts immediately sink to the bottom: besides, it is the lightest of all liquors; for fill any vessel with 20 oz. of oil of vitriol, the same emptied, will contain but 7 oz. of ether. It is the very ens, or being, most pure of flame; therefore neither soot nor ashes are ever found on its deflagration. (Succeeded.)

Thus far Dr. Frobenius; but to make this paper more than a mere harangue, it is absolutely necessary to subjoin two paragraphs out of a paper of that excellent chymist Mr. Godfrey, (Dr. Frobenius's fellow labourer in these experiments) which he delivered in, when this ether was made public before them.

"Feb. 19, 1729-30. That this liquor ethereus, was formerly very much esteemed and inquired into, clearly appears by an experiment I made formerly for my worthy master, Mr. Boyle, by means of a metallic solution, namely, by the solution of crude mercury united with the phlogiston vini, or other vegetables, and this ether swam on the top of the solution which I separated per tritorium. Note, This is what I have done formerly in Mr. Boyle's Laboratory, and Sir Isaac Newton was very well acquainted with it too; which by reason of the shortness of life was not brought to a full end, to do it so readily in quantity. But when Dr. Frobenius, by experiments on this in my Laboratory, produced it in greater quantity, he wanted to see how far Sir Isaac Newton had gone on with it in his book. There we saw that great man's application in fol. 330, that he had done it cum Ol. Vitr. et Sp. Vin."

This of Sir Isaac Newton, is the vini ethereus; only there is a difference in the process: the liquor ethereus, is made with equal parts in measure, not weight. The upper yellow liquor is separated from the inardent sulphureous per tritorium. The inferior liquor is thrown away; and the superior yellow is put into a retort to be distilled with the most gentle heat; and the extraction of the ethereal liquid continued so far, till the superior hemisphere feels cold, and the retort being clapped in the hand, there is found in the receiver a vino-sulphureous gas, very ethereal. Let the sulphur be precipitated by adding an alkali, and gently throwing it in till all ebullition ceases, and the liquor will not farther strike itself against the hand, but will strangely attract it. Then the alkali will go to the bottom of itself, or precipitate itself in the common water.

An Account of the Hermaphrodite Lobster presented to the Royal Society May the 7th, by Mr. Fisher of Newgate Market, examined and dissected, pursuant to an order of the Society. By F. Nicholls, M. D. F. R. S. N° 413, p. 290.

The world has frequently been amused with appearances proper to both the sexes, in persons who have from thence termed themselves hermaphrodites: but such of these as have passed a more strict examination, have proved, that those appearances were either morbid cases, or preternatural formations of the parts proper only to one sex.

But in those animals whose parts of generation are double and independent on each other, as the lobster, crab, and many birds, the parts proper to both sexes may possibly be formed in the same subject.

In order to illustrate this, Dr. N. gives a short account of the structure of the male and female lobster, so far as relates to the difference between the two sexes, and then shows in what manner they were combined in the present subject.

It has already been observed that the lobster, both male and female, has all the parts of generation double, except that the female has one passage only, through which it is probable the ova are emitted out of the trunk, in order to be affixed to the small appendages under the tail.

The penis of the male lobster arises from the testicle, and is no more than a continuation of the vas deferens; it is reflected and retorted once, after which it grows thicker, as to its substance, probably forming a corpus cavernosum, and terminates, not in the last leg but one, as Willis, in his Treatise de Animâ Brutorum, has observed, but at a small perforated tubercle in the first bone of the last leg.

Between the two last legs and the two legs above them are two processes, which from their resembling the nymphæ of women, may be termed nymphæform processes. These processes are covered with hair, and unite at their bases without leaving any passage.

Below the two last legs, towards the tail, are two appendages, which, from their similitude, may be termed the styliform appendages. These in the male are thick, hard, and void of hair.

The tail is continued from the trunk in a gradual decrease of its dimension, and is covered by plates, which extend themselves but little below the substance of the tail, and terminate in acute angles, without anywise diverging. It is to be observed, that sometimes these plates are edged with short and thin hair, and sometimes have no hair.

The female, on the other hand, in the place of the testicle has an ovary, which, like the testicle, extends itself from the stomach to near one half of the tail. From the middle of the ovary a duct descends to the legs, which opens at a round hole edged with hair in the first bone of the last leg but two: this is the uterus.

The two nymphæform processes in the female make a more obtuse angle at the union of their bases, are less hairy, and leave a passage, through which it is probable the ova are emitted, to be affixed to the appendages under the tail.

The two styliform appendages in the female are soft, thin, and edged with long hair. The plates covering the tail, are extended much farther under the tail than in the males: beside which, they diverge, in order to leave a greater space for containing the ova, for the better defence of which they terminate broad, and are edged with thick and long hair.

In the hermaphrodite lobster Dr. N. found all these parts proper to both sexes regularly disposed, but in such manner, that the parts proper to the female were to be found only on the right side, and the parts proper to the male only on the left side. In the antepenultimate leg, the os uteri was very obvious on the right side, as in the females, but had not the least mark of any such passage in the same leg on the left side.

The nymphæform process on the right side made an obtuse angle at its insertion into the body, and was soft and perforated as in the females; while the corresponding process made a less angle, and was more hairy and rigid at its basis, as in the male.

The styliform process on the right side was soft, flat, and edged with hair, as in the female, but on the left side it was stiff, hard, and void of hair.

In the last leg on the left side, the perforated tubercle for the passage of the penis, as in the male, was very conspicuous, but without the least appearance of such tubercle in the corresponding leg on the right side.

The plates covering the tail were extended on the right side considerably below the substance of the tail, and were edged with thick and long hair, and terminated broad, as in females. On the left side, these plates were much less extended below the tail; were almost totally void of hair, and terminated in acute angles. These plates diverged likewise on the right side, as in the females, but not on the left side, as in the males.

On removing part of the great shell, Dr. N. found the internal parts of generation in both sexes exactly corresponding to those externally described. In the right side, adjacent to the heart, the oviduct was regularly disposed, it was full of ova, and sent off its oviduct or uterus, to the antepenultimate leg. In the left side, the testicle was rightly disposed as to its form, substance, and situation; part of which he was obliged to remove, to show the penis, which terminated as in all males, at the tubercle in the first joint of the last leg.

Magnetical Observations and Experiments. By Servington Savery, Esq. of Shilston. N° 414, p. 295.

Precognita.—1. What Mr. Savery calls the magnetical line, is the position of a dipping-needle when it ceases from oscillating, and is at rest in the magnetical meridian of the place.

2. By the word magnet, he means not a loadstone only, but either that, or iron or steel, when they have permanent polarity, or any thing else having a sensible magnetical or polar attraction.

3. Of the magnetical needle, he calls that the north end which, if hung horizontally, naturally turns to the north, and that the south end which turns to the south: but when using the words pole of a needle, he calls that the north pole which turns to the south, and that the south pole which turns to the north.

4. Of touched iron or steel, as well as of the loadstone itself, he calls that the north pole which attracts the north end, i. e. the south pole of the needle, and that the south pole which attracts the south end, or north pole of the needle; or in other words, he calls that the north pole, in all sorts of magnets, which is endued with the same kind of virtue with the north pole of the earth, and consequently is repelled by it: e contra, &c.

5. Mr. S. prepared nails of several sizes, from the smallest sort of bellows-nails to the largest sort of rafter-nails, one or two of each sort, or more of the smaller. He held each of them perpendicularly with its point upwards, and placing on it the plain side of a file horizontally, he filed off a little from its point, more or less, according to the size of the nail, perhaps about the thickness of a sixpence from a sixpenny one. Then on a plain hone, held horizontally,

he placed the nail upright, with its point downward, and so rubbed off the strokes of the file. He then rubbed it a little on a piece of leather.

6. He prepared iron bars of different lengths after the following manner: he made each end in the shape of the lower frustum of a pyramid, cut transverse to its axis about the middle, or a little higher up. Then he filed the ends of the bar as plain and perpendicular to its axis as he could, and polished them with a hone, &c. as he did the nails. See fig. 11, pl. 10.

7. One of the needles he used untouched, for trying experiments, was made thus: he took some iron wire, about the size of a small knitting needle, and about $2\frac{1}{2}$ inches in length. With a hammer he made it just flat enough in the middle, to be able to fix the point of a punch pointed, to as true a cone as he could; in the middle of the wire he punched a hole at least half way through its thickness, and wrought the hole with a drill, pointed like the punch, that it might be truly round, and cleansed off the asperity which the punch and drill had raised round the hole, lest it should injure the top of the pin when placed on it. Then he bended it in the form of fig. 12, taking care to bend it the right way, that the hole might be on the under side. He then marked one end, by flatting it a little with a hammer, that it might be known from the other. Then placing it on a sharp pin, to find which end was heaviest, he made both alike in weight, and deprived it of all fixed magnetism. Then he brought it again to as true a poise as could be, by rubbing the heaviest end on a whetstone, and not a file, which might give it magnetism again. He fitted a pin for it of brass wire, full as small as the middle strings of a spinnet, making the point very thin and round, as well as sharp, and observed it frequently with a lens of 2 inches focus; and if it appeared flat, he mended it on a hone, and took great care in putting on the needle, not to hurt the tender point of the pin. He put a glass over it, to keep off all manner of fanning by the air, the least degree of which would spoil the experiments.

8. A second needle, which he thought better than the former, he made thus: in the middle of such a piece of wire as the former was made of, he wrought a hole perpendicularly through it, and as small as any of those drilled through the pillars of a watch, if not smaller. And having bended the wire in the form of fig. 13, he marked one end, and drove into the hole a small brass pin fitted to it, which was very round and sharp at the point, which rested on a deep plano-concave lens of glass well polished. He fitted a box for it, with a glass over it, which was fastened with a ring of brass wire, as the glasses of telescopes are, to keep out the air. The glass concave was fixed in the large end of a thin brass ferule, just fit for it; and the small end of the ferule was fixed in a hole, made for it in the middle of the bottom of the box; he also put a

ring of thin brass on the top of the lens, not only to keep it in steady, but to prevent the pin from going in between the lens and the ferule, which spoils its point.

The observations made by Mr. Savery, are as follow:

That the two opposite parts of a loadstone attract most vigorously, and are called its poles. The middle between its two poles does not attract at all, and may be called its equinoctial; and from either pole to the middle, the attracting force gradually abates.

That there is no difference, that he could find, between the force of strength of attraction and that of repulsion in the same pole of any loadstone or magnet, unless when a small one approaches so near to a large one, as to have its polarity more or less diminished by it.

These properties convince him, that there is no such thing in nature as magnetical attraction without polarity, which is constituted of attraction and repulsion; and these two powers being always equally strong in the same pole of every magnet, he thinks it a plain contradiction, to say this or that loadstone has a strong attraction, but a weak polarity or direction.

That no interposed body whatever, unless it be magnetical, though the most solid in nature, was ever known in the least to impede or divert any of the effects of a magnet; but it is always found to attract magnetical bodies full as powerfully at the same distance, as if nothing at all was between.

That every frustum of a loadstone is an entire or perfect loadstone, having in itself both poles as the whole stone had; and that the poles in each frustum have their direction, as near as the figure of it will admit, in the same parallel line in which they were directed both in it and the whole stone, before it was separated: for the polarity of every fragment is usually, if not always, before they are separated, parallel to that of the whole stone, and consequently to that of each other: and if ever it is found otherwise, Mr. S. thinks that loadstone wants of perfection. So that the parts of any magnet, when cut in two transversely, or perpendicular to its axis, become complete magnets, having each their poles and axis parallel to the whole magnet; and that, whether the two parts are equal or unequal. And the sum of the weights of iron, lifted by the two parts separately, is greater than the single weight lifted by the whole magnet.

That all magnetical attraction (as also repulsion) is mutual; for iron or steel attracts the loadstone, as that does iron or steel, and they also each other.

That every loadstone communicates virtue to iron or steel, not only by contact, but even by their approach within its attractive sphere, more or less as nearer to, or farther from its body; and likewise its poles, also according to the shape, size and specific virtue, and figure of the iron or steel, and their proportion of magnitude to each other. And that a small magnet communicates nearly as much virtue as a large one. Some authors write, that the loadstone loses none of its virtue by communicating of it to iron or steel, which Mr. S. doubts the truth of, especially if the stone is small in proportion to the steel, in which case he has known touched steel to lose considerable virtue.

That steel is not only more receptive, but more retentive of magnetism, than common iron; and iron or steel hammered hard, than the same while soft; but steel hardened by quenching, than either of them.

That such iron or steel as has magnetic virtue communicated to it, also communicates it to other iron or steel after the same manner as a loadstone does. Which virtue, after ever so many communications, is, as to its nature, perfectly the same with that of the stone itself, having both poles, and will touch other steel, and that a compass, as well as the loadstone itself, and as vigorous, if properly used.

That every loadstone, within its attractive sphere, has a power to keep one piece of iron suspended to another, especially if that to which it is suspended is the larger, and their ends be bright and clean, where they touch each other; and if the suspended iron is not too heavy, the other will draw it up from either pole of the naked loadstone actually touching it, and will also keep it suspended, till at a considerable distance; but will not draw it off in such manner from the armour of unarmed stone, if the armour and iron are both of them bright and clean at their contact. Hence it must follow,

That an armed loadstone can lift more with either of its poles, used singly, than the same can lift naked: That not only steel or iron, regularly touched, but also oblong iron void of permanent virtue, will perform all that any loadstone can, though not with the same degree of power: for either of them will attract, keep one piece of iron suspended to another, and communicate some degree of permanent polarity to steel well hardened, as Mr. S. has experienced, and also to an iron wire.

That, of a soft iron bar void of fixed polarity, as soon as it is in an erect position, the higher part from the middle upward becomes a north pole in north, or a south pole in south magnetic latitude. And, e contra, the lower part from the middle downward becomes a south pole in north, and a north pole in south latitude: but as soon as the bar is inverted, the polarity is shifted in it, and in north latitude the end newly placed upward becomes the north pole, though it was a south one immediately before, and the other end the south pole, though it was its north one just before. The case is the same, if such a bar is placed horizontally in or near the magnetical meridian; for the end directed toward the north will constantly be a south pole, and that which is directed toward the south, a north one; and as soon as the ends of the bar are shifted, the polarity, in respect of the bar, is shifted also, but not in respect of the earth, for which reason this virtue is called transient, and is communicated by the earth's central magnet in such manner as other loadstones are said to do.

That if a bar of iron or steel, not having the least degree of fixed virtues, be placed in any position, except at or near to a right angle with the magnetical line; it will not only for the present receive a transient polarity, but if it remain so long, the said polarity will gradually become fixed or permanent, more or less, according to the hardness or softness of the bar, and the time it has remained in that position, and the angle its length makes with the magnetical line, and the proportion of the length to its magnitude, the longest, cæteris paribus, usually receiving most virtue: and sometimes when all these advantages concur, the polarity will be sensibly permanent in a little time, and not require a very long time to be rendered pretty strong.

That by placing the said bar afterwards in the same position, only with its ends shifted, it will gradually lose its gained magnetism, and at length have its polarity changed.

That magnetism not only in touched iron and steel, but also in the loadstone itself, is soon destroyed by fire.

That though fire destroys fixed magnetism in steel or iron, yet if they are set to cool in an erect position, or rather in the direction of the magnetical line, they will gain more or less fixed virtue by the time they are cold; but especially steel heated to a seasoning height, and in that position cooled suddenly under water.

That while a piece of iron of some magnitude is held at one pole of a loadstone, it will increase the attraction of the other pole thereof, and enable it to lift somewhat more.

That if either pole of a magnet, large enough, touch one end of an oblong piece of steel, not too large and long for the magnet easily to act on; it will transmit its own virtue to the other end of the steel which is farthest off, and make it a pole of its own kind; while the end that touches the stone has virtue of the contrary pole: but the virtue usually is not so strong in the end which is untouched, as in that which is.

That a needle first equally poised, then touched and put to oscillate on its pivots in the magnetical meridian, will in north latitude have its north end (i. e. its south pole) depressed until it directs to the north-attracting point of the central magnet; where, after several oscillations, it will at last rest: and in south latitude the south end will be depressed after the same manner.

Mr. S. endeavoured to procure magnetism in steel, without the assistance of any magnet, except the earth's central one.

Finding that his artificial magnets, rightly used, would communicate more virtue to other steel, than they themselves had; and observing that erect bars had some virtue from the earth's magnet; and having also experienced that iron, which had only transient virtue, would, when in an erect position, or in the magnetical line, give a small degree of fixed polarity; he ordered 9 steel bars, $\frac{3}{4}$ of an inch square, and 16 inches long, to be made. Some of them, through the smith's fault, were a little less; the weight of the heaviest was, after it was finished, 3 lb. avoirdupois. He made them moderately bright by grinding, and filed their ends plain, and transverse to their lengths, by help of a carpenter's square; then marked one end of them, and, when hardened, scoured them bright, and polished their ends very well. He fitted a piece of armour for each end of one bar, and marked the piece which was for the marked end of the bar, and bound fast both pieces of armour to the same bar, one at each end: then standing with his face toward the west, and holding the palm of his left hand upward, he placed in it one of the bars without armour, with its marked end northward, and grasped it fast at its middle, with his fingers on the west side, and the ball of his thumb on the east side, where he also laid along his whole thumb to keep it steady: so that the upper part of the bar was open from end to end. Thus holding it, he elevated the south end till he guessed it was in the magnetical line; and holding with his right hand the armed bar, with the poles of the armour downward, and the marked end toward the north depressed to the magnetical line, he placed the pole of the upper armour about 4 or 5 inches from the top of the unarmed bar, and as soon as it touched the bar, he began to draw it downward till past the middle, and from thence to the bottom gradually slower. When at the bottom, he permitted it to rest there about 1 or 2 seconds. After the same manner, applying the pole of the lower armour to the unarmed bar, about 4 or 5 inches from its bottom, he drew it upward, speedily at first, slower when above the middle, letting it rest a little at the top. Having upwards and downwards alternately repeated the touch on the same side of the bar, he touched the opposite side, next his hand, in the same manner, and afterwards the two other sides. Then holding the unarmed bar erect, he used to see if it had gained any fixed polarity, by holding a small needle at the top and at the bottom of the bar; for if it had gained any virtue by the touch, it would attract the needle stronger, at the same distance, when the marked end of the bar was held downward, than when it was held upward. If he found it had gained any sensible virtue, he took off the armour from the first bar, and bound it to the second which he had touched, and after the same manner touched the first bar with the second, as he had touched the second with the first. And when by trial with the compass needle he found the armed bar had communicated to the other more virtue than was in itself, he took off the armour, and bound it to that which was newly touched, and thus retouched that which he had disarmed. In a few repetitions of changing the armour from bar to bar, and touching the weakest, he procured in both of them, without any other assistance, a fixed polarity to such a degree, as that the north pole, or unmarked end of either of them held downward, would attract the north end of the needle, though much fainter than if the north pole of the bar had been upward, and position did not now change their polarities, but only weaken them: therefore he now calls their virtue perfectly permanent. Four or five repetitions more increased their virtue to such a degree, that the south pole of one of them would lift a ten-penny nail prepared; and after 2 or 3 repetitions more, a common door key of an iron box lock, weight Troy ℥j and above ℈ij, not by the bow, but by its lower end, which was wrought somewhat globular and polished.

In the last place he got a piece of inch deal, above 3 inches broad, and 7 or 8 feet long; in the middle of which, at about 5 or 6 inches from one end, he made a hole through with a large gimlet, into which he drove an iron or steel pin, whose length, besides what went into the wood, was a little less than the thickness of one of the bars. He then placed the largest bar on the said board, with its marked end close to the pin, and its length parallel to that of the board, and with an awl made 4 small holes in the board, one of them on each side of the bar, about an inch from the bottom, and about the thickness of a sixpence, from its sides, and the other two after the same manner, about an inch from the top. He drove into them pins of large wire half an inch long, besides what was in the board, to keep the bars from sliding out of their places in touching. Then removing that, and placing any other bar between the said pins, with its marked end close against the great pin, he placed the marked end of the said largest bar close against the unmarked end of the other, and made 4 holes on its sides, and drove pins in them as before; and so continued to do until the board was full. It held half a dozen bars. He took care to place the marked end of every bar directed towards the great iron pin which was to keep them from sliding down to the ground, when the other end of the board was elevated, to stand in the magnetical line.

The board standing with one end on the ground, and the other leaning against the wall, at the south end of the room, he took the armed bar, which had virtue, and placed its north pole's armour about the middle of the highest bar, keeping the armour of the south pole a little upon one side of the bars, just so far as he might be sure not to touch them with that end, and then immediately drew it from thence downward to the bottom of the lowest bar: after the same manner, placing the armour of the south pole on the middle of the lowest bar, and holding the armed north pole on one side, that it might not touch, he drew it upward to the top of the highest bar, whose top he could reach. And if the end of any bar was a little under that which it rested against, he used to put a sizeable chip under it, that the armour might not hitch in drawing it over the places of their contacts. He usually touched the bars on all their 4 sides, then took out the lowest, and, letting the rest gently slide down to the iron pin, placed it at the top, that those which were first at the top might in their turns take their places in the middle, and be well touched.

When he found those on the board considerably stronger than his armed one, he took out that he thought attracted best, and bound the armour to it, putting the other in its room. After several repeated touchings, the largest of them, being 3 lb. avoirdupois, would be suspended by its north pole to the south pole of one of the best of the others. They did not lift each other, or attract so well when their ends were applied centrally, as when applied to each other a little from the centers, near to their opposite corners. And Mr. S. never saw this communication of magnetism outdone by the loadstone itself, as it is commonly used.

He usually finds the attractive power in square bars, cut plain over transverse to their lengths, to be strongest, not in the middle of their ends, but much nearer to their corners or sides, and to be greater at one corner or side than another; and this not only in such as are of touched steel, but in iron ones having no polarity, but from their position. The same he observed in round bars, if their ends are not convex. In some of his large steel bars, as also in some of the round bars, he found the north pole strongest, in others the south.

The Use of the Bile in the Animal Economy, founded on an Observation of a Wound in the Gall-Bladder. By Alexander Stuart, M. D. F. R. S. N° 414, p. 341.

One Mr. Menzies, of the horse guards, was wounded Oct. 30, 1728, and died Nov. 5 following, being the 7th day after the accident, in the 40th year of his age.

Dr. Stuart was called Nov. 2, being the 4th day after the patient received the wound. The surgeons who had before attended being present, said that his belly had been distended, as then, from the beginning, giving the appearance of a tympany, or ascites, and it continued at the same pitch of distension, neither diminished, nor sensibly increased, to the time of his death. No ructus or flatus, upwards or downwards, nor borborygmi, notwithstanding this distension of the belly. He never once went to stool after he received the wound, though pretty strong purgatives and several clysters had been given, for the 3 days before; and though no opiate (which might have been supposed to have retarded their operation) had hitherto been exhibited: neither had those purgatives nor clysters, ordered afterwards, the least effect. He took what was thought a sufficient quantity of drink and liquid food. He never slept, or but very little, by short slumbers, of about half an hour, or an hour at longest, and that very rarely, though pretty large doses of opiates were given to procure rest, after the doctor came. The wound in the integuments never digested in the usual manner; but looked flaccid, or flabby and pale, almost without pus. The urine in very small quantity, at most 2 or 3 spoonfuls at a time, clear but yellow, as if tinged slightly with saffron, and without sediment. His pulse was full, strong and even, but not quick. No feverish heat to be felt in the skin, on any part of the body. His tongue not hard, rough or black, as in a fever, but of its natural colour, with a silky driness, and very little saliva. He was not in the least delirious, from the beginning to the time of his death. He had some slight fits of the hickup the second day after the Doctor saw him, and some few retchings to vomit; some intermissions in his pulse, sometimes one in 10, 15, 20, or 30 a day before his death.

On opening the body, the abdomen appeared distended as in a tympany, or ascites, and the skin of the belly tinged yellow as saffron in many places. A triangular wound appeared about 2 inches on the right side of the navel, the direction slanting upwards obliquely through the integuments. The belly being opened, discovered the wound to have penetrated through the peritoneum, and the sword had slanted upwards from thence along the omentum, grazing slightly upon it, which was superficially ruffled, but so as to be hardly perceivable. A small triangular wound appeared in the bottom of the gall-bladder, which had penetrated through the membranes into its cavity, but had no where wounded the liver, nor any of the neighbouring parts. The gall-bladder was flaccid or collapsed, containing only a few drops of gall, which, by pressing the cystis slightly, flowed out into the cavity of the abdomen through the wound. The guts throughout their whole tract being distended, so as could be judged to triple the extent of their natural diameters, seemed to fill the whole cavity of the abdomen, so as to give the outward appearance of a tympany, or ascites; which distension disappeared, and the guts collapsed, on making several punctures with a lancet in their sides, to give vent to the air. The rest of the cavity of the abdomen, which was not closely filled up by the distended guts, contained about 3 quarts of a gross muddy water, or serum, intensely yellow, or highly tinged with gall. All the guts and contents of the abdomen were highly tinged with this yellow liquor; but no other part of his body, out of the contact of this liquor, had the least appearance of it. No inflammation appeared in any part of the guts, or in any of the viscera, or contents of the abdomen, which were all sound and healthy. The obliquity of the wound through the integuments, muscles and peritoneum, made it impossible for the external air to enter into the cavity of the abdomen that way.

In order to make some use of this case, it must be observed, that the great apparatus in the liver and spleen, two of the largest viscera in the body, designed for the preparation and secretion of the bile; and the place of the intestines, into which it is immediately deposited, afford indeed a strong argument for its universal use in the animal economy. But this singular case, which must have happened very rarely, if ever before (in which none of the viscera, but the gall-bladder only, was wounded, and by that wound nothing but the gall was lost or misplaced) by showing how many functions in the animal economy were impaired or destroyed by the sole loss or want of it, at the same time points out the use and necessity of it towards health, or the perfection of these functions; and perhaps may lead to some indications of cure, in cases wherein it is known to be deficient, faulty, or redundant.

There was no other apparent or assignable cause for these various symptoms during his life, or of death itself, and of those several appearances in the body dissected after death, but this wound in the gall-bladder: and as this wound could not affect any of the parts, nor produce these symptoms in any other sense, than as it gave vent to the gall into the cavity of the abdomen, and deprived the cavity of the intestines and the blood of it; therefore from this loss and misplacing of the gall, all these symptoms and appearances may justly be concluded to arise, and I think may be accounted for from that cause in the following manner.

1. The abdomen was distended, as in a tympany, or ascites, from the beginning, and the guts appeared inflated to their utmost diameters. It is true, that this inflation and distension happens to most a few hours before death, and to all soon after death. But the inflation and distension here spoken of, was several days before death, and it seems the very next day after he received the wound, though the pulse was apparently strong and equal, and therefore a defect of blood and spirits not to be suspected. Hence it may be justly concluded, that the influx of the gall into the cavity of the guts, is as necessary to the strength of their contraction, and perfection of their peristaltic motion, as that of the blood and spirits into their sides; and that these three are the

conjunct causes of this motion in health, which would be defective by the total want of any of them.

Hence it is, that in scirrhosities of the liver, where the secretion, and therefore the excretion of the bile, is more or less defective; and in the jaundice, where, by some obstruction in the biliary ducts after secretion, a part of it is forced back, and regurgitates into the blood, and very little of it is thrown into the guts: in those cases we observe an uncommon distension in the guts, and costiveness; which, if the case proves incurable, terminates in an ascites, or dropsy, in the cavity of the belly.

It may also be worth while to inquire, whether that which is commonly called an hysteric, or nervous colic, generally attended with a less degree of such like distensions, with flatuses and borborygmi; whether this distemper, wherein the animal spirits are so much, and only accused, does not partly arise from a sluggish secretion and excretion of the bile, occasioning a defect in its quantity; or from its acrimony and great viscosity, occasioned by its stagnation in the gall-bladder; or from both these, as well as from a defective or unequal distribution of the blood and spirits in the parts affected. In confirmation of which, it is generally observed, that at some time or other in the cure, a great evacuation of porraceous viscid bile, brought away either by art or nature, as well as a great profusion of pale urine, finished the cure for that time. The vomiting of porraceous bile, very common in such cases, proves the same; and it is generally allowed, that the ferruginous, porraceous, and black colour of the bile, are owing to shorter or longer stagnations of it, chiefly in the gall-bladder, which the sedentary life of those who are subject to these colics, will sufficiently account for, even if there was no other error in their way of living; and whoever has observed the high yellow colour and contents of the urine in a jaundice, arising from a redundancy of bile in the blood, will readily acknowledge that an uncommon watery paleness in the urine, where no more than the usual quantity of fluids has been taken down to dilute it, shows a defect of bile in the blood. And hence it is, that bitters and steel, known deobstruents of the liver, and correctors of the bile, with gentle cholagogues in very small doses, are of so much use in such cases; though it be certainly true, that all strong stimulating purgatives are very hurtful and improper.

2. But to return to our case, there was no ructuses or flatuses upwards or downwards, nor borborygmi, notwithstanding this distension of the belly and inflation of the guts. This seems to show very plainly that the guts had lost all motion, and were paralytic by a total want of bile only, as much as if their nerves had been totally obstructed: for had any motion remained in them, whether the natural and regular peristaltic motion, or a preternatural convulsive

one, the contraction of them either way, would have propelled the contained air from one place to another, and would have occasioned borborygmi, or would have expelled a part of it upwards or downwards, when nature had so much need of it to relieve the distended guts, and art had contributed to that intention by clysters and purgatives given.

3. The patient never went to stool after he received the wound; and the strongest purgatives and clysters that could be reasonably given, had no effect. This seems also to be owing to the want or total loss of the peristaltic motion; and plainly shows, that the strongest purging stimulus has not the power to restore it, without the assistance of the gall: for had it been in any degree restored, the belly would have fallen proportionally, and some evacuation of what was lodged in the primæ viæ would have followed. If then the power of purgatives depends on the co-operation of the bile, it will follow, that where it is most active or redundant, their operation will be, cæteris paribus, greatest; and where it is unactive or deficient in quantity, they will have proportionally a less effect. Though it be true that a great quantity, or morbid acrimony, of the bile, by a too strong and violent irritation, will bring the intestines into such spasms, as to stop all excretion by stool; and the strongest purging stimulus added to it only increases the spasms and costiveness; as in bilious colics, which are always attended with exceeding costiveness, not conquerable by the strongest purgatives, if they be not joined with opiates, to allay the spasms, and obtund the acrimony of the bile.

He took what was thought a sufficient quantity of liquid food and drink; but if the elater of the guts, and their peristaltic motion were lost, it is easy to prove that none of his food or drink could enter the lacteals for want of the peristaltic motion; and therefore that he died starved. And this will account for all the rest of the symptoms mentioned.

To prove that this was his case.—All that have seen live dissections, intended to show the nature of the peristaltic motion, and the course of the lacteals, must have observed, that the guts have an alternate systole and diastole, or contraction and dilatation, called the peristaltic motion, the superior section contracting itself, while the immediate inferior is dilated; and this motion is carried on in several parts of the guts at the same time; and the contracting part, by expelling the blood and chyle out of its sides, in its contraction looks pale, while the parts dilated look florid, and the vessels full of blood and chyle.

Now the part contracting must necessarily force the chyle from the grosser parts of the food or aliments, towards the inner surface of the guts, where the perforated capillary extremities of the lacteals in the villous coat, are ready to admit, or rather to absorb it by attraction, as far as the larger and visible branches of the lacteals on the coats of the guts, into which it easily flows in the time of dilatation, or diastole, which expands and unfolds these vessels at that time for its easy reception; from which it is further propelled by the next systole, or contraction, into the primary or first order of the lacteals in the mesentery; and by the same repeated impulses of the contracting sections of the guts, is forced further through the second order of lacteals in the mesentery, into the receptaculum commune, and the thoracic duct; assisted by valves, and promoted by the incessant motion of the muscles, and of all the contents of the abdomen and thorax in respiration, it is at last poured into the subclavian vein, for a perpetual recruit of the blood in a healthy state.

But if the first movers in this series fail, that is, if the muscular fibres of the guts have lost their peristaltic motion, as in this case, then the expression, absorption, and progress of the chyle described, cannot succeed, the blood must be deprived of its recruit, and the person die starved; which, as before said, seems to have been this person's case, and will sufficiently account for the rest of the symptoms above recited.

First, His want of sleep, and the inability of opium to procure it, might be owing to a want of recruit of chyle in the blood: as we see that those who live sparingly, sleep very little, and those who feed plentifully, require by so much a greater number of hours to sleep; and in all chronical cases, where the body ceases to be nourished, the sleep also fails, and opiates have but little power; whereas in children, where a great part of their food goes towards both nourishment and growth, the greater part of their time is spent in sleep.

It may indeed seem difficult to conceive how a want of rest should ensue so soon after the accident. But considering that the loss of one meal in a day, especially of supper, to such as have been accustomed to sup, has occasioned fewer hours rest in the following night, it will follow, that such persons require at least some small recruit once in 6 or 7 hours, in order to rest their usual number of hours; and therefore in our case, where all recruit must have ceased soon after the accident, he might be sensible of the impairment of rest in 6 or 7 hours after it, and those about him might well observe the increase of that symptom, at least in the following night.

Another difficulty arises from the observation of swallows and tortoises, &c. which sleep most in winter, when they eat and drink nothing. In answer to which, there seems to be no parity between the natural constitution of their blood and humours, and that of men: to these, and such like animals, with regard to recruit and nourishment, action and rest, the spring and summer are as one day, and the winter as one night; and their blood and humours seem to be fitted by nature, not only to bear, but even to require such long periods of rest and action. And probably there is as little parity between the crasis and constitution of the blood and humours of a healthy person, and of those in soporous and cataleptic diseases, who are reported to have slept for weeks or months without food of any kind: and therefore, where the crasis and consistence of the blood and spirits are nearly the same, that is, cæteris paribus, he who feeds, and is nourished most, will sleep longest, et e contra.

2dly, The want of pus in the wound was probably owing to a want of recruit of chyle in the blood; and the flabbiness and paleness of its lips, to a shrinking of the parts for want of daily nourishment.

3dly, The small quantity of urine was probably also owing to a want of recruit of fluids from the primæ viæ: for these, in a healthy state, find their way to the urinary passages very soon. The slight tincture of yellow, which it had, must have been from the bile spilt in the abdomen, and filtrated through the duplicature of the peritoneum, and bottom of the bladder: for it could not be supposed to derive its colour from the blood, into which no bile could now enter by the common way.

4thly, The want of saliva, and the silky dryness of the tongue, seem to have been owing to the same cause, a want of recruit of fluids in the blood, and a loss of so much of them as fell into the abdomen.

It may reasonably be objected here, that the ductus hepaticus would carry a sufficiency of bile, for the uses of the animal economy, into the cavity of the intestines, though none came by the ductus cysticus; and nature seems to have provided the ductus hepaticus for this purpose, that if any obstruction or defect should happen in any of these secretory channels, the secretion and excretion might go on for the benefit of the economy, in the other: as nature has provided two kidneys, and double organs of sense, for the same reason. But the effect will not be the same in a wound, which is the reverse of an obstruction; because by a perpetual evacuation through it, such a revulsion and derivation is made, as drains and desiccates all the neighbouring parts, and either lessens or entirely frustrates the secretion and excretion by them: and this we find to be true, where the secretory organs and ducts concerned in the different secretions, lie at a great distance from each other; as in the diabetes we generally see a very great desiccation of the salival glands, a defect of saliva, and a perpetual thirst; and sweating and looseness lessen the secretion by urine; an issue drains and emaciates the neighbouring parts; and it is mechanically demonstrated by Bellini, that the flux of the blood, and of all the humours, will be most and strongest towards the part where the resistance is taken off; as in bleeding, to which this perpetual flux of bile, through the wounded gall-bladder, seems to have a great affinity; and therefore would probably promote the afflux of blood and secretion of the bile so much and so strongly towards the vessels, glands, and secretory ducts leading to the cystis, as very much to lessen, or totally to hinder the secretion by the ductus hepaticus into the guts by that channel: and therefore, in this case, the whole of this useful juice seems for this reason to have been totally lost to the animal economy.

Another objection is, that as the guts and other contents, and even the muscles and integuments of the lower belly, were highly tinged by the bile, it is probable that some of it has got into the cavity of the guts, where it might, by its stimulus, keep up the peristaltic motion, and by the lacteals get into the blood, for the use of the animal economy; as it appears that some of it got into the bladder in that manner, and tinged the urine.

It is not unlikely that this might happen when the bile came to be very redundant in the cavity; but in passing through the interstices of the vessels and fibres of the guts, as through a filtre, the grosser, saline and sulphureous particles of it, which are the most pungent and active parts, must have been left behind; which the muddy thickness, as well as deepness of the colour of the liquor found in the cavity of the abdomen, compared with the transparent clearness of the urine of a much lighter yellow colour, without sediment, seems to prove: and it is not likely that such a small quantity of filtrated bile, as may be supposed to have passed that way, deprived of all its active particles, could either in quantity or quality be sufficient to assist in any function of the animal economy, whether natural, vital or animal: and, in fact, if any passed that way, it appeared plainly insufficient to promote the contraction and peristaltic motion of the guts, which remained preternaturally distended, from the beginning to the time of his death.

It has been also objected, that an animal which dies starved, dies delirious and feverish, the experiment having been made on cats and dogs: and therefore this person, who had no fever, nor delirium of any kind, cannot be supposed to have died starved.—The Dr. will not dispute these facts, especially the experiments on cats and dogs. But supposing the facts, these cases will differ very much from this before us: for an animal starved to death purely for want of food, has the gall flowing continually into the cavity of the intestines, unmixed and undiluted with chyle, and from thence by the lacteals into the blood; so that in a few days this acrimonious juice must become more redundant there, than any other humour; which joined with the constant attrition of the globules in circulation, must soon render the blood very acrimonious, rancid and alcaline, that is, must reduce the whole to a mass of putrefaction, capable of stimulating the brain and nerves, so as to produce a fever, delirium or madness. But in the case under consideration, no gall could enter into the blood: and therefore this degree of putrefaction, and its effects, could not happen; though it must be owned, that, through a want of recruit and dilution, a lower degree of putrefaction of the blood and humours must have followed, even in this case, from the continual attrition in circulation; such at least as was sufficient to render the whole mass in a few days unfit for any of the uses in the animal economy, or the functions of life: and therefore may be justly supposed to have been the immediate cause of death. For all the passive principles or materials of putrefaction, being actually in the substance of the blood, and all the active principles of heat and attrition being at work upon it to produce this effect, it could not fail to be brought about in a few days; and the same would happen to all animals, if what is effete, corrupted or altered, so as to be unfit for the use of the animal, was not continually carried off by the emunctories, and a fresh recruit daily supplied from the primæ viæ; which evacuations and supply being kept up in their due quantity and proportion, do effectually prevent all putrefaction and acrimony, and keep the blood and humours in their natural temperature.

It is not then a defect in the quantity of fluids which kills an animal in fasting, but a poisonous acrimony, which the blood and humours naturally contract, for want of a fresh recruit and equal evacuation. Thus in chronical distempers, where the person appears extenuated and exhausted, the quantity of the fluids is certainly very small, yet enough to maintain life for some months or years, being kept in some degree of sweetness or proper temperature, by a certain proportion of recruit and evacuation: but where the recruit is entirely withheld, the evacuations will be proportionally lessened: and therefore the quantity of fluids may remain much the same, but the quality will alter, and putrefaction, for the reasons above assigned, must take place, and be the immediate cause of death, even long before the mass of fluids can be much diminished in quantity, as in the case before us.

Another objection may be, how the pulse should continue full, strong, and equal for several days, while the person was in a starving condition, and the blood had no recruit from the primæ viæ?—This indeed would be very unaccountable, if the waste of the blood and humours were supposed to continue at the same height as before the accident, and the evacuations by the emunctories were the same as in perfect health. In this manner the contents of the blood-vessels would be soon wasted and exhausted; but Sanctorius's observations and experiments show, that the daily recruits and evacuations keep pace with each other, and are nearly equal in 24 hours in a healthy state; and therefore where the recruits are plentiful, the evacuations will be equally so; and where those are sparing, the evacuations are small; or where the balance is cast too much on either side, some indisposition or distemper must follow. There is no exception from this rule but in children, a part of whose nourishment goes to accretion, and the increase of their weight; therefore in the case before us, the recruit being entirely abstracted, the evacuations must have been little or next to nothing; and therefore the quantity of the blood and circulating humours would remain much the same, and keep up the fullness, strength, and equality of the pulse for several days, till the critical putrefaction and colliquation of the blood abovementioned, on the 5th or 6th day, rendered it unfit for a regular circulation, and produced intermissions in the pulse, retchings to vomit, and hickup, all of them being local convulsions, and the effects of corruption, acrimony, irritation, and an unequal distribution of the fluids, which terminated in death the beginning of the 7th day.

The sum of what has been said is, that in this case very little, if any, bile entered into the intestines, and that ineffectual; and none at all into the blood. And as there was no apparent defect in any part of the body, nor any wound that could have been either dangerous or deadly, in any other respect than as it gave occasion to the loss and misplacing of the gall; it is therefore evident, that all the symptoms, and the patient's death, were entirely owing to the loss of this useful juice, which it seems is so necessary to all parts of the animal economy, natural, vital, and animal, that this person could not live above 6 days without it.

The practical inferences that seem to flow by necessary consequences from this observation, are, 1. That the peristaltic motion of the intestines, is as much owing to the influx of the bile into their cavity, as to the influx of the animal spirits and blood into their sides; and therefore that the bile is to be considered as one of the prime movers in the animal economy, by which the elastic springs of the natural motions, viz. the muscular fibres of the guts, are set to work; on whose motion all the subsequent vital and animal motions so far depend, that none of them can be long in perfection where it is imperfect, nor subsist many days where it is totally wanting.

2. This prime motion is totally lost, by a total want of bile, or when it proves sluggish by a defect in its quantity, or becomes irregular or convulsive by a great redundancy or morbid acrimony in it. From whence several distempers that are called nervous may arise, and are more likely to be cured by correcting and evacuating the redundant or faulty bile, and disobstructing the liver, than by most other medicines taken from the common class of nervines.

3. That the power of purgatives depends on the co-operation of the bile;

and therefore it is probable, that the difference of constitutions, at equal ages, with respect to purgatives, depends more on the quantity and quality of the bile than on the bulk or weight of the body, or quantity of the blood, or other circulating humours.

4. It also appears that the nourishment and growth of the body in some measure depend on a due quantity and proper quality of this juice, without which the blood and circulating humours could not be recruited from the primæ viæ; and therefore that defects in it may be frequently the cause of a marasmus or waste of the body, where it is little suspected; which may serve to point out the method of cure in such cases.

5. This observation seems to lead to the knowledge of the immediate cause of natural rest or sleep in a healthy state; viz. a certain quantity or proportion of fresh chyle in the blood; the want of which, from whatever cause, will occasion watchfulness, or some degree of it. And this may serve to point out the immediate effect and consequences of opiates; whence may be gathered how far, and in what cases they may be effectual and useful, and in what circumstances they may be ineffectual, useless, or hurtful. Which may deserve a further illustration.

6. That a due quantity of aliments, at proper intervals of time, is necessary to keep the blood and humours in their natural temperature and sweetness, and to preserve them from acrimony and putrefaction. And this will be true in all distempers as well as in health, and is against the practice of such as pretend to starve away distempers, or to deny a due quantity of drink and liquid food to the sick, especially in fevers, where the want of this recruit will tend to increase the acrimony or putrefaction, whence the malignity of most fevers arise.

7. That pus, or matter, in a wound or ulcer, is the product of chyle, and not of the blood or serum, which has indeed been the received opinion, though supported by no other proof than the similitude of pus to chyle. And as a great redundancy, as well as a defect of pus, sometimes retards the cure of a wound or ulcer, this may serve to show by what means it may be increased or diminished, to answer the intentions of the artist.

This also makes it appear probable, that a great redundancy of chyle disposes the body to purulent, suppuratory, and scrophulous distempers; and seems to indicate the denying such sort of food as afford a rich, gross, or plentiful chyle, and the administering of such medicines as may strengthen sanguification, and the other assimilating powers, to assimilate and so consume it; the sanguification and assimilating powers being manifestly weak, as the chylification seems to be strong in all such cases. And this seems to be the reason why in adults, as the sanguification becomes stronger, and in age, as the voraciousness of the appetite, too common in youth, declines, these distempers often decrease, and at last wear out of themselves. Which shows what assistances art ought to contribute to bring about the same effect in a less time.

A Lunar Eclipse observed at Lisbon, Feb. 2, 1730, *N. S. By Father Carbone.*

N° 414, p. 363. *From the Latin.*

At $13^{h}\ 25^{m}$ true time, the sensible penumbra began; $14^{h}\ 3^{m}\ 45^{s}$ the beginning of the eclipse, doubtful; $14^{h}\ 4^{m}\ 32^{s}$ the same certainly begun; $15^{h}\ 4^{m}\ 16^{s}$ the middle of the eclipse; $16^{h}\ 4^{m}$ the end of the same; 3° 20′ the quantity eclipsed on the north.

Eclipses of Jupiter's Satellites at Pekin, in 1727 *and* 1728. *By the Missionaries.*

N° 414, p. 366. *From the Latin.*

Satellite 1.—Immersions. 1727. Nov. 2, $10^{h}\ 21^{m}\ 10^{s}$ in the evening. $10^{d}\ 0^{h}\ 14^{m}\ 26^{s}$ in the morning; $11^{d}\ 6^{h}\ 44^{m}\ 10^{s}$ in the evening.

Emersions, Dec. $3^{d}\ 2^{h}\ 30^{m}\ 42^{s}$ in the morning; $10^{d}\ 4^{h}\ 22^{m}\ 5^{s}$ in the morning; $11^{d}\ 10^{h}\ 50^{m}$ in the evening; $13^{d}\ 5^{h}\ 17^{m}\ 50^{s}$ in the evening; $19^{d}\ 0^{h}\ 40^{m}\ 44^{s}$ in the morning; $20^{d}\ 7^{h}\ 8^{m}\ 20^{s}$ in the evening; $26^{d}\ 2^{h}\ 32^{m}\ 33^{s}$ in the morning; $27^{d}\ 9^{h}$ in the evening. 1728, Jan. $3^{d}\ 10^{h}\ 51^{m}\ 50^{s}$ in the evening; $5^{d}\ 5^{h}\ 20^{m}$ in the evening; $11^{d}\ 0^{h}\ 45^{m}\ 18^{s}$ in the morning; $12^{d}\ 7^{h}\ 13^{m}\ 27^{s}$ in the evening; $19^{d}\ 9^{h}\ 5^{m}\ 40^{s}$ in the evening; $26^{d}\ 10^{h}\ 59^{m}$ in the evening; $28^{d}\ 5^{h}\ 27^{m}\ 20^{s}$ in the evening; Feb. $4^{d}\ 7^{h}\ 22^{m}$ in the evening; $11^{d}\ 9^{h}\ 16^{m}\ 40^{s}$ in the evening; $18^{d}\ 11^{h}\ 12^{m}\ 30^{s}$ in the evening; $20^{d}\ 5^{h}\ 41^{m}\ 50^{s}$ in the evening; March $21^{d}\ 7^{h}\ 58^{m}\ 55^{s}$ in the evening.

Immersions, Sept. $20^{d}\ 1^{h}\ 12^{m}\ 12^{s}$ in the morning; Oct. $4^{d}\ 5^{h}\ 6^{m}$ in the morning; $13^{d}\ 1^{h}\ 30^{m}$ in the morning; $20^{d}\ 3^{h}\ 26^{m}\ 15^{s}$ in the morning; $27^{d}\ 5^{h}\ 19^{m}\ 30^{s}$ in the morning.

Satellite 2.—Immersion, 1727, Nov. $6^{d}\ 4^{h}\ 5^{m}\ 40^{s}$ in the morning.

Emersions, Dec. $1^{d}\ 3^{h}\ 40^{m}\ 45^{s}$ in the morning; $4^{d}\ 5^{h}\ 2^{m}$ in the evening; $11^{d}\ 7^{h}\ 37^{m}\ 42^{s}$ in the evening; $18^{d}\ 10^{h}\ 11^{m}\ 13^{s}$ in the evening; $26^{d}\ 0^{h}\ 47^{m}\ 39^{s}$ in the morning. 1728, Jan. $5^{d}\ 4^{h}\ 42^{m}$ in the evening; $12^{d}\ 7^{h}\ 16^{m}\ 16^{s}$ in the evening; $19^{d}\ 9^{h}\ 51^{m}$ in the evening; Feb. $13^{d}\ 7^{h}\ 3^{m}\ 45^{s}$ in the evening; $20^{d}\ 9^{h}\ 46^{m}$ in the evening.

Immersion, Oct. $30^{d}\ 3^{h}\ 34^{m}\ 10^{s}$ in the morning.

Satellite 3.—Beginning of the emersion, 1727, Nov. $21^{d}\ 7^{h}\ 57^{m}$ in the evening; $28^{d}\ 11^{h}\ 53^{m}$ in the evening. 1728, total immersion, Jan. $3^{d}\ 5^{h}\ 43^{m}\ 40^{s}$ in the evening; emersion, $7^{h}\ 42^{m}$ in the evening; immersion total, $10^{d}\ 9^{h}\ 42^{m}$

52ˢ in the exening; emersion, 11ʰ 42ᵐ 20ˢ in the evening; immersion total, Feb. 22ᵈ 9ʰ 42ᵐ 30ˢ in the evening; Oct. 9ᵈ 6ʰ 6ᵐ 30ˢ in the morning.

A Lunar Eclipse observed at Pekin, Aug. 19, 1728, *N. S. By the same.* N⁰ 414, p. 368.

At 10ʰ 54ᵐ correct time, the penumbra began; 11ʰ 2ᵐ the true eclipse began; 11ʰ 52ᵐ the moon's centre immerged; 12ʰ 31ᵐ the middle, the quantity 7¾ digits; 13ʰ 10ᵐ the moon's centre emerged; 14ʰ the eclipse ended.

On the Veins and Arteries of Leaves. By Frank Nicholls, M. D. F. R. S. N⁰ 414, p. 371.

By a letter from Dr. Fuller in Holland, the society was informed, that professor Ruysch had observed something, in dissecting leaves, analogous to the veins and arteries of animals; but without explaining in what manner these different vessels were disposed, or by what means they may be distinguished from each other.

When Dr. Nicholls examined the collections of Frederick Ruysch and Albert Seba at Amsterdam, in both which was a great variety of dissected leaves, they made no mention of such a discovery; though in a leaf from the collection of Ruysch he could, with a glass, observe the fibres to be double towards the edges of the leaf, which at that time he imagined to be an unnatural division of the fibres, as in decayed sticks.

In the mean time, Albert Seba having communicated the method of dissecting leaves to the society, Dr. N. separated the pulpous from the fibrous parts of several leaves after his method; when examining them by glasses and in water, he found that each fibre was naturally separated into two distinct fibres, by a thin stratum of the pulpous substance; and that this separation was continued through all the fibres and stem of the leaf, so as to form two distinct planes of similar net-work.

Though this duplication of the vessels in leaves, seems to point out an analogy between them and the veins and arteries of animals, yet the Doctor sees no probable means of guessing which are the arterial and which the venal fibres.

That he might illustrate this matter, he prepared two leaves, the one of an apple, the other of a cherry, in which, both the separation of the fibres and stem, and the pulpous substance, by which they are naturally separated, are very obvious. See plate xi, where fig. 1 shows the cherry leaf, and fig. 2 the apple leaf, their planes being separated.

On some uncommon Anastomoses of the Spermatic Vessels in a Woman. By Cromwell Mortimer, M. D. R. S. Secr. N° 415, p. 373.

Eustachius is the only author who has given any delineation of an immediate communication between the spermatic arteries and veins, during their course along the cavity of the abdomen; which is distinctly laid down in his anatomical tables, Romæ in folio, 1714. Boerhaave, from him, mentions these anastomoses, in his Institut. Med. § 642; and also cites Leal Lealis in his treatise Πεϱι Σπεϱματιζοντων Οϱγανων, Lugd. Bat. 8vo. 1707, p. 18 ad 26; where he fully refutes de Graaf, who denies these anastomoses; but even Leal allows that he never saw them, and only argues for them from the effect, and the close union of the spermatic vein and artery in one covering as they run together. Marchetti in his anatomy, Hardevicæ 12mo. 1656, Chap. of the Parts of Generation in a Man, asserts this anastomosis, p. 58, but it seems he never saw them; nor has Dr. M. conversed with any, even anatomists, the most celebrated for their injections, who had hit on a subject, where these passages were open enough to transmit the subtile matter they inject with.

In the beginning of the year 1723, being at Paris, and at the Hotel Dieu having an opportunity of dissecting various bodies, Dr. M. met with a female subject, where these anastomoses were as large as the spermatic vessels themselves; so that the arteries being injected with a gross mixture of wax, tallow, and vermilion, and the veins with the same, only tinged with smalt, the injection ran out of the artery into the vein, and on the other hand out of the vein into the artery; so that where one vessel entered the other, the matter injected was tinged purple. The arteries were first injected with the red, and the veins afterward with the blue matter.

What appeared most remarkable in this subject was, that on the right side were two spermatic arteries, A and B, fig. 3, pl. 11. One A, arose from the very angle made by the emulgent and the trunk of the aorta descendens C, which, contrary to the common course, ran under the vena cava, and soon after it was got beyond it, sent out a lateral branch, or anastomosis, descending obliquely EF into the spermatic vein G, through which the red matter penetrated into the vein; which being afterwards filled with blue, became purple all about the orifice of this vessel at F; which seems to confirm Eustachius's delineations. This artery A then descended as usual to the right ovary H.

The other right spermatic artery B arose, as usual, out of the trunk of the aorta, but at about half an inch from its rise, it sent out an anastomosis IK, ascending obliquely into the body of the vena cava D, through which a large quantity of the red matter passed, so as to tinge purple a very broad place at K in the vena cava. About an inch below this orifice was another anastomosis LM, through which the blue matter penetrated out of the vein, and made the contents of the artery purple at L. The right spermatic vein had only this one anastomosis ML, being in all other respects as usual. It was rather surprising to see the course of the channel inverted, if the blood ran from the vein M towards L, into the artery; but surely this must be from some accident in the injection, for it could never be so in the person's life; only indeed the oblique rise of this branch out of the vein seems to show, that the blood ascending in the vein would have its natural direction into the orifice of that channel, or at least the blood coming from the artery would meet with a stop, and so the two streams retard each other.

On the left side there was only one spermatic artery N, and one spermatic vein O, which, as usual, inclosed in a common involucrum, made their way to the left ovary P. Only the artery N took its rise out of the body of the aorta near the angle made by it and the left emulgent artery, then ascending between the emulgent vein and artery, turned in an arch at Q, over the left emulgent vein, and so joined the left spermatic vein as usual, which rose out of the left emulgent vein, as it often happens.

There was on this side one thing very uncommon, and not taken notice of by Eustachius himself; which was a short anastomosis RS, about a quarter of an inch in length, from the left emulgent artery S, which making an arch under the left emulgent vein, was inserted into its anterior part at R.

In fig. 3, AB represents two spermatic arteries on the right side; CCC, the aorta descending, and the two iliac arteries; DDD, the vena cava ascending, and the two iliac veins; EF, LM, anastomoses of the spermatic veins and arteries; G, the right spermatic vein; H, the right ovary; IK, an anastomosis of the spermatic artery and vena cava; NQN, the left spermatic artery; OO, the left spermatic vein; P, the left ovary; RS, an anastomosis of the emulgent vein and artery; TT, VV, arteries and veins dispersed on the fat and membranes inclosing the kidneys.

*A new Family of Plants, called Oxyoïdes.** *By M. Garcin. With a Remark, by Mr. John Martyn, F. R. S.* N° 415, p. 377.

The species of this genus are, 1. Oxyoïdes Javanica, sensitiva, caule rubescente, hirsuto, flore majore; fig. 4, pl. 11. 2. Oxyoïdes Malabarica, sensitiva, caule viridi, glabro altiore, flore majore. Fig. 5.

Remarks.—This plant is very sensible of the least cold: it loves warm and

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* These plants belong to the modern Linnæan genus *oxalis.*

moist places. It is found in the island of Java, and probably in the other islands of the Sonde and the Moluccas. When its leaves are touched, they close immediately, and open again by little and little. The more they are warmed by the sun, while their soil is moist, the more impetuously they close against each other. The Portuguese Indians call it dormidera, because, on being touched, it seems to sleep, by shutting up its leaves; or else, because some among them think it procures sleep by being put under the ear; but this soporific quality cannot be ascribed to it, any more than can be recommended the hanging of misletoe of the oak about the neck for the epilepsy. The leaves of this species of oxyoïdes have no acidity in their taste, and give but a faint tincture of red to the blue paper.

The Remark of Mr. John Martyn, F. R. S.—We are obliged to Mr. Garcin for his curious description of this plant, by which its genus is determined. It is however by no means a new species, having been described long ago by Acosta, and other authors, under the name of herba viva. A fair specimen of it is in Sir Hans Sloane's Hortus Siccus, with which M. Garcin's figure agrees very exactly. It was the first sensitive plant known in Europe, and very different from those which are now brought from America, and cultivated in our gardens under that name.

Fig. 6, pl. 11, represents the empalement of the oxyoïdes. Fig. 7, the flower, the petals of which are joined together. Fig. 8, a petal apart.

Remarks on the Family of Plants named Musa. By M. Garcin.

N° 415, p. 384.

Almost all the writers on botany have considered this family as a tree, on account of its size, though it is tender, spongy, membranous, and succulent, not at all hard or woody. Its stalk is slender and supple, not able to keep itself upright, without a great number of thick, membranous sheaths, which clothe its whole bulk, and defend it from the injuries of the weather. Besides, this plant being annual, bears fruit only once, and then gradually perishes.

The trees, on the other side, which are ligneous, hard and perennial, bear fruit several times. So that the size of a plant does not seem to be a character sufficient to distinguish a real tree from a plant that is not one.

Again, the same botanists have placed the musa in the palmaceous class, which are all trees, perhaps on account of this plant's having but one stalk, without any branches; and because the great leaves at the top of it divide, when they grow old, in such a manner as to resemble in some degree a sort of palm.

Having had an opportunity in the Indies to consider this plant better, M. Garcin soon found that it justly belonged to the liliaceous tribe. It is known that the liliaceous plants have several characters, which distinguish them very well. Their roots are either bulbous, tuberous, or consisting of thick, fleshy fibres: their leaves involve the stalk, more or less at their bases. The substance of their flowers is filled wth silver spangles; and lastly, their fruits are always divided into three cells. The musa has all these characters. Father Labat says, in his Travels, that the root of this plant is a thick bulb, round and massy, emitting fibres. Marcgrave, who has given a full description of this plant, under the name of pacoeira, has observed, that, at its first appearance, it sends forth 2 or 3 leaves, rolled up like a horn, which unroll themselves, and grow after the manner of the cannacorus. And, according to M. G.'s observation, the fruit in all its species, is constantly divided into 3 cells, which is sufficient to show that it is a true liliaceous plant.

As Marcgrave, and the authors of the Hortus Malabaricus, have largely described this plant, M. G. only gives a definition of this genus, to make it better known.

The musa is a liliaceous plant, with a monopetalous, irregular flower, incomplete and hermaphrodite, composed of a tube, filled with the ovary, and a pavilion divided into several lobes, and forming a kind of mouth. The ovary, which adheres strongly to the tube, is triangular, and crowned with 5 chives, which grow from the sides of the flower; it has also a stile, which is terminated by a little head. It afterwards becomes a soft, angular, long, crooked fruit, something like a cucumber. This fruit, when ripe, is fleshy, and divided into 3 cells, filled with a mucilaginous pulp; under which the seed is placed along a placenta, which serves as an axis to the fruit.

This seed is small, round, edged with an almost imperceptible leaf. The flowers grow at the end of the stalk, in knots disposed in a spike. Each knot is loaded with two rows of flowers, covered with a membranous, hollow, thick, oval covering, which serves them for a common empalement. In the Hortus Malabaricus there are 3 plates, which give a good representation of the plant, its flower, and its fruit; but there are observed 3 defects in them: 1. That the flower is not represented in its most perfect state, but almost withered, and so ts pavilion too much cleft, which makes the flower seem tetrapetalous; for the flowers of these plants divide when they are old, as well as the leaves. 2. That the 3 cells are not shown distinctly, in the transverse section of the fruit. 3. That the seed is not represented at all.

This genus, or family, comprehends 20 or 25 species, known to the Indians, the differences of which are usually taken from their fruits. This plant does not perish before it has ripened its fruit; whence it might last longer in a temperate climate, cool enough to retard its fruit.

The bark of the fruit is formed of the tube of the flower; and the lobes dry away during the growth of the fruit.

The Fruit of the Musa represented entire, pl. 11.—Fig. 9 represents the fruit half stripped of its bark. Fig. 10, the fruit cut through the middle. Fig. 11, the fruit cut transversely, distinguishing the 3 cells and the seeds. Fig. 12, is another species of musa cut transversely, represented in the Hortus Malabaricus, but having the cells better distinguished here. The six black points are the seed.

*The Hirudinella Marina, or Sea-Leech.** *By M. Garcin.* N° 415, p. 387.

M. Garcin observed on the sea a small insect, shaped like a small worm, which he found in the stomach of a bonite, where it was strongly fastened. Its shape came very near that of a leech; it had all the motions of that animal, besides some of its own.

Fig. 13 represents this insect in its natural size, and according to its most usual dimensions. Its body is round almost throughout its whole length, but a little flatted towards its belly B; so that its circumference, taken according to its thickness, is almost elliptic. It is adorned all along with little circular furrows, parallel to each other, and very close together, but so fine, that they can hardly be perceived without a microscope. It is of a greyish colour, and its body rather transparent. On its back, as well as underneath, two black lines begin by an acute angle towards the neck, and running through the whole length of the body, seem to be terminated towards the anus. These lines are not upon the skin, as might be imagined at first sight, without sufficient attention; they are tubes, or bowels, which serve for nutrition or chylification, which appear through the integuments. M. G. divides the length of this little leech into two parts, distinguished by the centre of a little protuberance C, which is under its belly, and is a muscular body, in form of a spherical bladder. These two parts of the body are in the proportion of 4 to 3. He calls them the fore part and the hind part. This distinction is necessary, both with regard to the different motions of these two parts, and to this protuberance, which separates them at their beginning. This little protuberance, when in its greatest extension that the animal can give it, is of the same form with a little spherical air-pump, and has all the same properties; or it may be compared to the cup of

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* This animal seems rather to be a species of *planaria*, and is probably the same with the planaria clavata, described in the Naturalist's Miscellany.

an acorn, with the mouth a little contracted. The head E, which is the smaller end of this worm, has a hollow body underneath, of a conical, or almost hemispherical form, which seems to serve it for a mouth to suck, as well as to fasten itself on the various bodies that come in its way, after the manner of other leeches.

The belly B is of a dark colour, because several viscera, contained in it, are filled with a thick, black liquor; which makes it look as if the skin was of that colour. The fore part CE is variously shaped, according to its different motions; sometimes it prolongs itself, and then it becomes slender; the diminution being made gradually up to the head; and sometimes it contracts itself, and then the thickness increasing, it becomes equally so throughout. The hinder part CB does not change its figure, because it moves but slowly, and very seldom. The motion of the fore part is of 3 sorts; prolongation, contraction, and the making itself round on all sides. The protuberance also is endued with two motions, extension and undulation, which is made from the centre to the circumference. When this insect stops itself any where, it holds firmly by means of this protuberance. Before applying this, it shortens it, by withdrawing the edges, or the circumference towards its centre; and after applying the orifice of its protuberance on the surface of any body, it lifts up a little the centre or bottom, towards its own body; afterwards it swells, and stretches it on all sides, according to all its dimensions. This protuberance thus applied, stretched and void of air, makes that which endeavours to enter, press it externally on all sides, and holds it so fast, that it is above the strength of the animal to separate it from its place where it is applied. The animal being thus fastened, and detained by its protuberance, its fore part is always in motion, while its hinder part remains almost immoveable. It stretches its head sometimes to the right hand, sometimes to the left, by lengthening and shortening its fore part, which bends and straightens itself very frequently. The extent of all these motions is marked by pricked circles of different sizes, touching each other at one point of their circumference, at the centre of the protuberance, which is as it were the beginning and fixed point of all these motions. By these different motions the insect performs the two functions necessary to it. When the little animal wants to change its place, it makes use of its protuberance and its sucker, which is the little hollow under its head, and seems to serve it for a mouth: it applies this part to the place D, whither it would remove its body, and after being prolonged by its fore part to reach the place, where this application should be made, it draws its protuberance and sucker together, by bending its fore part circularly, after the manner of some caterpillars. Its protuberance being applied, it loosens its sucker, and prolonging itself, applies it to another place more forward: the sucker being fastened, it bends itself circularly again to bring the protuberance up to it, and apply it as before. By this the worm prolongs itself to apply its sucker, and contracts itself to do the same with its protuberance. Thus these motions and applications are made successively, and as often as there is occasion. The hinder part fastens itself to nothing, but is always drawn by the part which goes before it.

This little animal did not live above 2 hours after it was taken out of the place where found. It became languid as soon as it was exposed to the air, and recovered some vivacity as soon as put into a little sea-water, sending out from its mouth a little green, almost imperceptible thread, which kept itself suspended in the water, and was about as long as its body, and was as fine as the finest thread of a spider's web. After this thread was put forth, it emitted also from the same place some little bubbles of air. The body of the worm decreased in bulk gradually as long as it lived, and after its death this diminution either ceased, or became less sensible. Having cut its belly through with a pair of scissars, as soon as it was dead, and squeezed it, there came out a black, thick liquor.

From these facts we can draw but very slender consequences. It is certain that this insect cannot live out of the water; so that we cannot imagine it could live in the stomach of any land-animals, unless they came near the nature of the amphibious; for the worms which grow on, or within the bodies of animals, ought to be of the same nature with them, with regard to the elements in which they live. This worm seems to be incapable of living any where but in the bodies of fish, seeing it kept alive but a very little time in the sea-water in which it was put, having been exposed to the air but one moment at two different times; which was not sufficient to alter its parts, and cause its sudden death. The almost immediate diminution of its bulk in the water is another mark that it cannot live in the sea out of the body of the same fish; for if the water, which was more natural to it than the air, was injurious to it, much more would the air have been prejudicial. The fine fibre which it put forth, and the decrease of its size, were signs that it suffered some uneasiness. The black and thickish juice, which came out of its entrails, could be nothing but some half coagulated blood, which it had sucked in the stomach of the fish.

As the bonite is a fish of prey, living on other smaller fishes, it is probable that this little leech usually fastens itself on those which come into the stomach, and that it lives on their blood. The stomach, in which it was found, was quite empty; so that it was probably as hungry as the bonite could be; for this fish is not easy to be catched but when hungry. However, it was the first time he found it so very empty, though he had seen a great number opened.

A Solar Eclipse observed at Willemberg, July 4, 1730, O. S. By J. F. Weidler, F. R. S. N° 415, p. 394.

At $3^h\ 56^m\ 0^s$ True time, the sun rose eclipsed; $4^h\ 26^m\ 0^s$ the greatest obscuration, being $6\frac{1}{2}$ digits; $5^h\ 13^m\ 30^s$ the end of the eclipse.

The same Eclipse observed at Padua. By J. Poleni. N° 415, p. 396.

At $4^h\ 46^m\ 12^s$ the digits eclipsed were 4; at $5^h\ 6^m\ 8^s$ the eclipse ended.

An Explanation of the new Chronological Table of the Chinese History. By Father John Francis Foucquet, Soc. Jes. Bishop of Eleutheropolis, and published at Rome in the Year 1730. N° 415, p. 397.

This is a translation published by Father Foucquet, Soc. Jes. the present bishop of Eleutheropolis, who has lived near 23 years in China, of a new Chronological Table of the Chinese History, which lately appeared in Latin on a large sheet of paper. The Chinese original table, from which this translation was made, is owing to the learned Nien hi yao, a Tartar, illustrious by birth and merit, and viceroy of Canton in the year 1724: for the Tartars, since their conquest of China, are become well versed in sciences, and especially in the history of the empire they conquered.

Yet this gentleman is not the author of the Chronological system he has here drawn up. He tells us, he has taken it from the most valued historical work in China. What renders this writer praise-worthy, is his ranging his system in a beautiful order, which gives a great facility of seeing at first sight the series of the dynasties, or imperial houses, the names and succession of the emperors, the beginning, end and duration of each of their reigns.

However, this is not the only advantage of this new table: the ancient Chronology of China is there reduced to its true beginnings. The most remote epoque of this Chronology, according to Nien hi yao, does not surpass the first year of a prince called Guei lie wang, who began his reign 424 years before the vulgar æra. Some even think this epoque might still be brought nearer to us; not to fix there the origin of the nation, which, for strong reasons, may be traced back to near the deluge; but because from much later date only, any certainty appears, of whatever is pretended to have befallen this famous people. Se ma quang and Tchu hi, the two gravest historians China has produced, were of this opinion. The first flourished in the year of Christ 1061, the second about the end of the 12th century. They have both omitted whatever is before the time of Guei lie wang, nor would they mention ought of it in their histories: nay, they have not begun them till the 23d year of Guei lie wang, somewhat later than Nien hi yao, who begins with the first of this prince's reign. It is on the example and authority of these two illustrious philosophers, that Nien hi yao has relied in suppressing what precedes.

By fixing this epoque at Guei lie wang, fabulous times, and a thousand errors and absurdities current in Europe, concerning three imperial (absolutely imaginary) families, and reigns anterior to, but no less chimerical than these families, are retrenched. These errors will soon vanish of their own accord; so that the subject of so much laborious, but useless lucubration and study, will at length cease: a worthy motive for congratulating the learned world.

This is not all; we are still particularly obliged to the ingenious Tartar, for having found means to place in his table the cycle of 60 years, called Kia Tse, so much esteemed by the Chinese, that it is as the soul, link and foundation of their whole chronology: a point which requires explanation.

As we mark the incidents of ancient history by the years of the olympiads, so the Chinese mark what has happened in their country by the years of this revolution. According to the Chinese, the prince under whom the great wall was finished, began his reign the 52d year of a cycle, which is found to be the 4th in this chronological table, reckoning from the cycle of the general epoque inclusively. This general epoque is the first year of Guei lei wang.

Every year of the Chinese cycle is marked by two letters, which make up its proper character, and distinguish it from the other 59. Thus the first year is called (1) Kia Tse, and gives its name to the whole cycle. Thus the 52d year of the 4th cycle, in which the prince, who finished the great wall, began his reign, is called (2) y mao. This prince, after bloody wars, became monarch of China; and then abandoning himself to such impious pride, as the philosophers reproached him with, caused himself to be called (3) Chi hoang Ti, as much as to say, the first master, the first emperor reigning of himself; for this is the real signification of these characters well analysed; and those glorious titles belong to God alone in the ancient monuments. This unheard of usurpation happened in the 26th year of his reign, which is the 17th of the 5th cycle, and is there called (4) Keng Chin.

It is thus that all the years of the emperors, for above 2000 years, have names in history common to them with the corresponding years of the cycle: and these names, common to both, are a sort of link, which unites the years of the emperors to the cycle, and so prevents confusion. Hence we see how the cycle among the Chinese is the knot and basis of all their Chronology.

But it is not possible to give these names any translation. Kia Tse will still be Kia Tse in all languages; y mao will still be y mao; Keng chin, Keng chin, and so the other 57 names. These names are composed of two sorts of characters, very famous among the Chinese, who get them by heart in their youth, and employ them on a thousand occasions. Those of the first sort are 10 in number, and are called the year-letters; those of the second, 12, and are named hour-letters. These two sorts of characters are combined, by repeating the 10 year-letters 6 times, and the 12 hour-letters but 5 times; for 6 times 10, and 5 times 12 equally make 60: and from this combination result 60 names for the 60 years that compose the cycle. These three points well comprehended suffice for the use and understanding of the chronological table.

The Chinese pretend that these 22 letters were invented by a very ancient king, whom they name (5) Hoang Ti, in order to determine the beginning, progress, end, and successive periods of a great year; for they have one which includes a certain number of ages, though its total duration be no where distinctly marked. They say the great year is successively at Kia, at y, and at Ping. Now it is no easy matter to determine the extent of these different parts of the great period (for there is room to conjecture that they are unequal) how long, for example, lasts that which commences at (6) Kia, that at (7) y, and so of the rest; nay, it is perhaps impossible, for want of certain principles, the knowledge of which is entirely lost. When the year was at Kia, which seems to signify when it began, this point of time, according to tradition, is called (8) O fong; when it was at y, this is called (9) Tcheou Mong; when at (10) Ping, the name given to it was (11) Jeou Tchao.

Every one of the other 19 letters has in this manner a word for its device; but as it is plain, that all these words are very strange to European ears, and that those which remain are as obscure and barbarous as Kia Tse, Y mao, Keng chin, M. Foucquet omits mentioning them.

Yet it is not easily believed that these words are void of all meaning, or that the letters, whose names they are, are figures made at hazard, or arbitrarily imagined. The inventor of these names must have proposed himself some end. It is already known in general, and is demonstrated elsewhere, that the characters preserved by the Chinese, but much more ancient than them, are true hieroglyphics. It is also known, and strongly demonstrated, that the doctrine veiled under the appearance of these hieroglyphics, is very mysterious and sublime: and it is unreasonable to regard as nonsense, and reject such as we understand not, purely because we do not understand them. And indeed when we closely examine the 22 letters in question, we perceive in several of them something very mysterious, which the Chinese themselves present us with without understanding them.

This so useful cycle, which in the printed history is a certain rule to fix time, the ingenious Tartar has disposed in his table with such art, as renders the relation of the years of the cycle to the years of the emperors very sensible; whence spring great advantages, that are very visible to whoever attentively considers the table, and penetrates into his arrangement.

In the front of the table appears a line written in capital letters, which extends horizontally from right to left: this line contains, according to the order of their succession, the names of 21 dynasties, or imperial families, who have reigned since 4 centuries before Jesus Christ, to this time.

These names placed exactly on the lines, where are the beginnings of the dynasties to which they belong, are as sure guides for easily finding them, and under the direction of which we easily come to the knowledge of the emperors of these imperial families, as well as the incidents of their reigns.

This cycle is placed in the middle, in a perpendicular line or column, which extends from the top to the bottom of the table, and is divided into 60 little square areas, every one of which answers to a year of the cycle, and contains the name of the year it answers. The angles, or empty spaces which surround the name in each of these squares, were coloured black, that the whole may the more readily strike the reader's eye, and be the more easily distinguished.

On the right and left of the cycle thus placed are ranged 20 other columns, divided into 60 squares each, in the same manner as the cycle; and consequently equal to the cycle to which they are parallel.

It is in the squares of these columns parallel to the cycle, that the years of the emperors are disposed in their natural order, for above 2000 years. They are disposed from top to bottom, from the right to the left, after the Chinese custom. And it is essential to remark, that the arrangement is such, that each of these imperial years referred to the column of the cycle by an horizontal line which falls at right angles on this column, answers the year of the same cycle, whose name it bears in history.

The first year of Guei lie wang being placed at the 53d year of the cycle, the 2d year of this prince will necessarily answer the 54th of the cycle, the 8th of the same prince the 60th and last of the same cycle; and consequently the 9th of Guei lie wang, will be the first of the ensuing cycle; and so of the others in a continued series of 21 centuries, down to the present time.

As all the columns of the table are parallel to the cycle, so they are equal to it, and contain 60 years, as that does. Hence flows an easy method to know in a moment the interval of time elapsed between any two years of the table: for it is but multiplying by 60 the number of entire columns between the two years whose interval is sought, adding thereto what remains in the two columns on the right and left, till you reach the two years in question.

By this method a moment's attention will make a judicious reader know that there are 424 years elapsed since the epoque of Guei lie wang, to the year when Denis le Petit places the birth of our Saviour; for this year has been marked on the Latin table, to be as its centre, and serve the European literati for a fixed point to regulate their calculations on. Supposing Petavius's doctrine true, this Dionysian æra is one year before the vulgar æra, which last should be preferred as being most in use, if we had not considered that it coincides with the first year of the emperor Ping Ti.

But if on one hand the epoque of Guei lie wang, placed in the 53d year of the cycle, and once well comprehended, becomes a key that opens the knowledge of the table, and developes its system; on the other, the characteristic names of the 60 years which compose the cycle, do by their connection with the years of the emperors, determine the precise time of incidents. Hence arises clearness and certainty in the Chinese chronology; for these characteristics contribute to the discovery of errors, which either the ignorance and neglect of copyists and printers, or the want of attention in authors, often introduce into chronology.

This cycle removed, the years of emperors might be very easily confounded, by augmenting or diminishing their number. When an emperor is newly come to the throne, if the first year of his reign be reckoned that in which his predecessor died, it is placing two years in one; because, according to the Chinese custom, the year wherein an emperor ends his reign, is wholly attributed to him, though he died in the beginning of the first month: and his successor is held to reign only from the beginning of the ensuing year.

Yet this custom, though very common, is not so universal, but that some emperors have derogated from it. The Tartarian emperor Tchang hoang Ti, founder of the dynasty now reigning, caused the year in which Hoai Tsong had murdered himself, to be taken for the first year of his reign, which was the 17th and last of this last emperor of the Mings.

Another property of this new table, no less remarkable or useful than the foregoing, is, that this table lays before the eye all the names of the particular epoques assumed by the emperors of China for near 2000 years. For Han uou Ti, the first who took this sort of epoque, began his reign 140 years before Jesus Christ.

The emperors of China have a particular custom, little known in Europe, which, if care be not taken, would infallibly spread darkness and confusion over chronology and history. It is not allowed to pronounce the proper name of any emperor during his life, which is held in some measure as ineffable. This respect continues even after their deaths; for then it is not by their proper names they are mentioned, but are as it were consecrated by a surname, which is a sort of character of canonization. And under this title are they received into the burying-place of their ancestors, and afterwards ranked in history. But in their life-time, to supply the name that dare not be pronounced, they themselves choose and determine a term that serves for an epoque to the incidents of their reign. This term we call epoque, because it is from it the years of emperors are reckoned, and to it is referred every thing that falls out during these years.

The names of emperors, and the names of their epoques, are essentially different, and those of the epoques comprehend very instructive meanings, the understanding which must be of great service to the clearing up of history. But there is great danger lest the name of an epoque be made the name of an emperor, which would double the number of emperors, supposing even that each of them had taken but one epoque during his reign. No European writer, that we know, has faithfully given them all; but this table presents us with an exact and entire series of them.

The inconveniency is, that a great number of emperors have often changed these names of epoques. In the more ancient this is a very common disorder. Han uou Ti, the first that introduced the use of epoques, assumed, during his 54 years reign, to the number of eleven very different epoques. Several others have followed his example, which cannot but cause a great deal of confusion in history, if one happened to imagine, as it is natural enough to do, that these names of epoques are the names of so many emperors.

It was of importance to clear up these things thoroughly; this the table does: and to avoid mistake, care has been taken to have the emperor's names or titles engraved in large characters, and those of the epoques in small letters. And when an emperor, not content with one epoque, has taken several, notice is given of it by a star placed on one side of the first.

An Account of Mr. Mark Catesby's Essay towards the Natural History of Carolina and the Bahama Islands, with some Extracts from the first three sets. By Dr. Mortimer, R. S. Secr.* N° 415, p. 425.

The author proposes in this work to give the figures of the birds, beasts,

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* Mark Catesby was born about the latter end of the year 1679. He had an early propensity to the study of natural history, and the residence of some relations in Virginia favoured his inclination to visit that part of the world. He accordingly went there in 1712, where he staid 7 years, collecting various subjects of natural history. On his return to England, he was encouraged by Sloane, Sherard, and many others, to return to America, with the professed design of describing and figuring the more curious productions of nature. Carolina was fixed on in particular, where he arrived in May 1722, and after having carried his researches through this country, as well as Georgia and Flo-

fishes, serpents, insects, and plants, the greatest part of which have never been described by any author, or no good figures given of them. He gives the descriptions in English and French, with observations of air, soil, and water, and an account of the agriculture, grain, pulse, roots, and other productions of the country, with a map of the same. The author was near 4 years in these parts, where he drew every thing from nature in their proper colours; in order to make the coloured prints almost equal to his original paintings, he engraves and colours them with his own hand.

On the preternatural Delivery of a Fœtus at the Anus. By Mr. Giffard, Surgeon. With an Examination of the Parts; by Mr. Nourse, one of the Assistant Surgeons to St. Bartholomew's Hospital; Demonstrator of Anatomy at Surgeon's Hall, and F. R. S. N° 416, p. 435.

About the middle of August 1730, Mr. Giffard was sent for to a woman, who then judged herself to be between 3 and 4 months gone with child; she had all the symptoms preceding a miscarriage, and on touching, he found the os tincæ somewhat dilated; from whence he concluded a miscarriage would ensue, and therefore ordered what he thought proper to promote it. But he was some time after informed by her husband, that though she before believed that she had miscarried, yet that she now thought herself quick, as feeling something to move within her belly, like what she had perceived after former quickenings. Thus it passed on for about 6 or 7 weeks; in which time she grew much larger, and the motion became more perceptible; so that there remained no doubt of her being with child. About the 3d of October she was seized with violent pains in her belly and back; which daily increasing, her sister came to him on the 6th, when he went to her, and found her labouring under very great pains, and other complaints like those preceding a miscarriage or delivery.

rida, he visited the Bahama islands also for the same purpose. On his return he published his work, in 2 volumes, large folio, having etched the plates with his own hand. The date of the first volume of this work is 1731, of the second 1743, &c. The work was however first published in numbers. The whole bears the title of *The Natural History of Carolina, Florida, and the Bahama Islands.* It contains in all 220 plates, which, though not conducted according to the rules of modern accuracy or elegance, are yet, in many instances, both splendid and beautiful. They consist of plants, birds, fishes, and a few insects. At the time of its publication it was considered as the most splendid work which England had ever produced.

The author was elected a fellow of the Royal Society, and continued in habits of friendly acquaintance with many of the most respectable members of that body; being greatly esteemed for his modesty, ingenuity, and good behaviour. Before his death he removed from Hoxton, where he usually resided, to Fulham, and afterwards to London, and died at his house in Old-street, Dec. 23, 1749, aged 70.

But to be better satisfied, he passed up two fingers into the vagina, to examine whether the os tincæ began to dilate. He there felt a large and unusual fulness and tension, which he then judged to be the body of the uterus sunk low into the vagina, and distending it much, and extending backwards, and pressing against the rectum; so that the excrements could not readily pass, neither could she, from its pressure on the neck of the bladder, freely make water. Mr. Giffard could not find the os tincæ, though he very carefully examined all about with the ends of his fingers; he therefore judged that the fundus uteri must have receded from its natural position, and be bent back towards the rectum; in which opinion he was the more confirmed by the fulness he before observed, stretching backwards; and therefore concluded that the os tincæ must be very forward; he therefore endeavoured to pass his fingers between the os pubis and the fulness which pressed against the upper edge of the said bone. This he effected with some difficulty; and at length about 2 or 3 inches above the said bone, he felt the os tincæ with the ends of his fingers. He ordered the patient anodyne and quieting medicines to relieve her pains, which she was obliged to repeat at least every 12 hours, with proper cordials to support nature; and sometimes clysters. Thus matters continued to the 20th of the said month, only that for some days before, a water tinged with blood came away, as she imagined, through the anus, and which she believed proceeded from the piles, with which she was sometimes troubled.

On the 20th her husband came to Mr. Giffard about 6 in the morning, telling him that the midwife had brought away a fœtus, but could not complete her business. On which he went to the midwife, who told him, that a fœtus was protruded through the anus; and on examining, he found the funis umbilicalis hanging out about 2 or 3 inches beyond the anus, and passing up through the same. He therefore passed his two fore fingers by the string into the anus; when about 3 inches up he found an opening, as he then judged, into the uterus, wide enough to admit the ends of 3 or 4 fingers, and the funis umbilicalis passing into it; hence he was assured the fœtus came out that way. With his fingers passed into the opening, he endeavoured to bring away the placenta; but as it was very rotten it tore away between his fingers, so that he was obliged to draw it out in small pieces, and at last to leave a large part of it behind. The septum or partition between the anus and vagina was entirely whole, having no perforation through it.

From these appearances he then concluded that a mortification must have begun in the uterus; and so from its contiguity be communicated to the rectum; so that nature, endeavouring to expel what was contained, and forcing it against this part, already mortified, and consequently ready to give way and separate on any pressure made against it, caused this opening, and the protrusion of the fœtus through it into the rectum, and so through the anus.

There was a large discharge of grumous blood, and other substances, through the anus, which continued coming away till the 26th of the aforesaid month, when the woman died.

It is to be observed, that there was a fulness and hardness very perceptible, to be felt outwardly in the fore part of the belly, some distance below the navel, from the time that the fœtus came away to her death; which, on opening the body, he was well assured, was the uterus forced upwards and forwards by a sacculus, which being large and distended, filled up the pelvis; and by its bulk pressed the uterus forwards. The fœtus was perfect in all its parts; but much wasted and shrunk from its being some time dead; and consequently putrefied.'

The vagina, uterus, ligamenta rotunda, left ovary, Fallopian tube, and ligamentum latum on that side, with the hypogastric and spermatic vessels on the same side, were in a natural state. The Fallopian tube on the right side, was traced from the fundus uteri, almost to the morsus diaboli; where it was confusedly united with, and opened into the sacculus described below. The ovary on this side, with the ligamentum latum, was dilated into a large sacculus of an irregular form, extending itself behind the uterus (to the posterior paries of which it adhered) and passing on towards the left, was connected to that part of the colon that terminates in the rectum, and to the rectum. In this sacculus were found great part of the placenta, and the remains of lacerated membranes, besides the aperture of the Fallopian tube abovementioned; and another, about 4 inches in diameter, into the middle of the rectum. That part of the ureter on the right side, which lies between the ovary and the kidney, was dilated; and so was that part of the rectum between the aperture into it, and the end of the colon; both which were caused from the contents of these canals being obstructed in their passage.

Of a Total Eclipse of the Moon, observed at Barbadoes, July 29, 1729. *By Mr. Wm. Stevenson.* N° 416, p. 440.

At the beginning of the eclipse, the moon was clouded.

At 7^h	18^m	Apparent time, 2 digits were eclipsed.
8	11	The total immersion.
9	51	The moon emerged.
10	50	End of the eclipse.

In this and all the other observations Mr. S. made of both solar and lunar eclipses, during several years he has been in Barbadoes, he found that they always happened 10 minutes sooner than his computation. Whence he concludes, that that island lies 2 deg. and a half more westerly than is generally supposed.

The Anatomical Preparation of Vegetables. By Albertus Seba, F.R.S. N° 416, p. 441.

Take leaves of trees or other vegetables, that are somewhat substantial and tough, and have woody fibres; as for instance, leaves of orange trees, of lemon, jessamine, laurel, rose-trees, of cherry, apricot, peach, plum, apple, and pear trees, of poplar, pine, oak, &c.

There are many sorts of leaves that have no such woody fibres or veins; as for instance, vine and lime-tree leaves.

Those leaves are to be gathered in June or July, when they are most perfect, not touched by worms and caterpillars. They are to be put into an earthen pot, or a wide glass vessel, with a good deal of rain-water, and afterwards left in the open air, uncovered, and exposed to the warmth of the sun. The water must always stand above the leaves, and if it evaporates so as to leave them dry, fresh water must be poured on. Some of the leaves begin to putrefy in a month, others hold out 2 months and longer. When the two external membranes begin to separate, and the green substance of the leaf to grow liquid, then it is time to perform the operation. The leaf is to be put into a white and flat earthen plate or dish, filled with clear water; then being gently squeezed with the finger, the membranes begin to open in the extremities, and the green substance comes out. Take the membranes on both sides dexterously off with the finger, which must be most carefully done in the middle of the leaf near the stalk: if there be once an opening, the rest follows easily. The skeleton that remains between, is afterwards washed in clear water, and kept in a book.

The method of preparing fruits, as apples, pears, plums, cherries, peaches, and the like, is as follows:

The finest and largest pears that are soft and not stony, are most proper for this purpose. First, they are to be nicely pared, without squeezing them; and care taken not to hurt the stalk or the crown. This done, put them into a pot of rain or fresh spring-water; cover it, and let them boil gently till they become thoroughly soft; then take them out, and put them into a vessel of cold water. The pear, &c. to be anatomised, is to be put into a dish filled with cold water; then hold it by the stalk with one hand, and with one finger and the thumb of the other hand, rub the pulp gently off, beginning near the stalk, and rubbing equally towards the crown; and you will easily see in the water how the pulp separates from the fibres, which being most tender towards the extremities, it is there the greatest care is to be taken. No instrument is of any use in this operation, except last of all a penknife to separate the pulp sticking to the core. In order to see how the operation advances, you may fling away the muddy water from time to time, and pour on clean. All being separated, the skeleton is to be preserved in spirits of wine rectified. The same is to be observed with regard to apples, plums, peaches, and the like.

Turnips and other roots, that have woody fibres or ribs, must be boiled without paring, till they grow soft, and the pulp comes off. Not only many sorts of roots, but also the barks of several trees may be thus reduced into skeletons, presenting rare and curious views of vegetables.

Some Effects of Thunder and Lightning in Carmarthenshire. By Mr. Evan Davies. N° 416, p. 444.

Dec. 6, 1729, in the afternoon, there happened terrible thunder and lightning, which alarmed the whole neighbourhood; and about 4 o'clock, as a woman was carrying a pail of water into the house, there broke such a violent clap of thunder, that she and 3 of her children were very surprisingly struck, and instantly deprived of their senses, so that they lay miserable monuments of the terrible shock; and it seems they lay weltering in their blood, before they recovered, and were able to creep to the bed, till the next neighbour happened to come in (the husband being then abroad at his day-labour) to assist them. The lightning it seems struck at the east end, near the foundation, into the hearth, and split in two a thick stone of about half a yard in breadth beyond the fire, and shattered one half into small splinters, which shot into their flesh, and did the most hurt. About 24 or more of those stones were, from time to time, taken out of their wounds. It appears, that afterwards it forced its way out through the wall on the south side, within the compass of the hearth, when it made a terrible breach from top to bottom, and removed the stones from the foundation, making a deep hole perpendicular in the earth, that one might thrust in a staff to the top. The partitions in the house were moved out of their place; and a chest full of corn forced down towards the door, some yards from the place where it stood. The bucket the woman had in her hand, and other wooden vessels in the house, were all or most of them shattered, dishes and spoons, &c. blown off, and after some days, found and gathered in the garden, on the north side of the house, split and broken; and many more disorders were committed.

The woman has quite lost her left eye. She was speechless for a week or 9 days, and could not swallow. She has lately had a few stones come out from the roof of the mouth, under the tongue, and other parts inwardly: the tip of her tongue was taken off, by which she is still lisping; 3 of the fore teeth of the under jaw broken, with the lower lip slit, but now pretty well healed; 2 of the right-hand fingers quite off, and the colour of that hand still like a flame of fire, as if there were yet remaining some igneous particles in it. She has a terrible gash on that shoulder between the joints, that an egg might be contained in it, besides 3 or more bruises on the arm down to the wrist, that she is not able to lift it up, without the help of the other hand; besides several other wounds and bruises, over great part of her body. A boy had his hair all singed, his face and breast all scorched with blisters like bladders running from the raw flesh, with several stones taken out from his body and legs; and two other small children suffered greatly; so that the wounds are reckoned to be 30 at least between the mother and children, and many splinters of bones taken out of them. Only one girl, about 10 years old, that stood at a distance next the door, escaped, having her clothes only singed, but no hurt done her. They smelled so strongly of the sulphur for some days, that one could hardly go near them. They are now, however, freed from any grievous pain to complain of; so that they go about.

An Account of the Operation of Bronchotome, as it was performed at St. Andrews. By Geo. Martyn, M. D. N° 416, p. 448.

A young lad being in a good state of health, was suddenly taken ill with a violent trouble in his throat; in which however, Dr. M. could see nothing amiss, the amygdalæ, and other parts in view, being in all appearance sound enough, only looking a little drier than ordinary; without any external tumour appearing about the larynx, and no considerable frequency or strength in his pulse. But he had great pain and a dyspnœa, with an impossibility of swallowing either liquids or solids; every thing returning forcibly by the mouth and nose, when he made an effort to get it down. From all which the Doctor reckoned it an angina of the worst kind, (sine apparente tumore. See Hippocr. Prognost. xxiii, 3, et Prænot. Coac. iii, 96) and the seat of the disease in the larynx, and the fibres common to it, and the top of the gullet.

Notwithstanding repeated bleedings, blistering between his shoulders, cupping, &c. the disease continued so obstinate, and the patient so like to be suffocated, that next day in the afternoon his friends, earnestly desired that the operation of piercing the windpipe might be performed; and the poor lad bade us try any experiment to preserve his life. We directly set about the operation; which was done with such success, that in less than 4 days, his breathing being perfectly easy, and his deglutition being almost so, we removed the cannula, and left the glottis to do its own office.

In the very cutting, before we got a free aperture into the trachea, and the pipe introduced, the patient felt some relief, which might be ascribed to the effusion of blood in the operation; a small quantity of which evacuated so near the part affected, could not but make a more considerable revulsion, than a much greater taken away at a great distance. Whence the judicious Fabricus ab Aquapendente, p. 480, with very good reason supposed that by the derivation here, the patient would be more apt to feel some relief than trouble. And since there continued a greater flux of blood to the wound while it was suppurating, the circulation in the muscles of the larynx might be with less force than ordinary, and so probably contributed to diminishing the strength of the voice, which for a good many days after the operation, was observed to be much weaker than it used to be.

In performing the operation on a living person, one cannot but remark at the very first, that the cannula should not be made near so short as is usually proposed in books and surgical lectures: for we found that on cutting the parts, especially the thyroid gland, they soon become so much tumefied, that it requires a pipe above an inch long, to penetrate sufficiently into the aspera arteria. Which is more than double of Garangeot's allowance of 6 lines; who is one of the most recent writers, and has communicated to us all the surgery the French are masters of. The leaden pipe we had prepared not answering the design, that which we made use of was too long and too small, being the common cannula for tapping in the dropsy, flattened a little at the end, and hindered by a very thick compress, perforated in the middle, from penetrating too deep into the trachea.

The mucous particles and streams arising from the lungs, made a constant weeping of a thin slavery liquor from the mouth of the pipe, part of which thickening, and stuffing its cavity, sometimes very much incommoded the patient's respiration by it, so as to render it necessary to have it taken out and cleaned. And hence, when some moderns very precisely tell us to put a thin slice of sponge, or a bit of muslin, &c. close over the orifice of the cannula, to prevent the ingress of dust, downs, or the like, into the lungs; it confirms us of the unusualness of the operation, and looks as if they had only contemplated the matter in abstracto, as the metaphysicians say, without considering they had not to do with a pure thin dry air, but with a heterogeneous fluid, that is moistened and thickened with viscid particles, which are apt to run together in stiff concretions. And therefore, it must be acknowledged that there would have been less hazard of stoppage, if our cannula had been shorter, and wider, especially at the mouth. I cannot but think it an ingenious proposal of one of our ministers here, to make the pipe double, or one within another; that the innermost might safely and easily be taken out and cleaned when necessary, without any molestation to the patient: for it is no small trouble to him to be obliged to have the bandage frequently removed, and the pipe fitted a-new to the orifice made in the trachea.

And indeed we found no inconvenience in our patient's breathing the air as it passed through the pipe, without any cleansing or intercepting medium, though the house was none of the cleanest, being that of an ordinary tradesman. But if by a larger tube, one, especially of more delicate and ticklish lungs, should be incommoded that way, the access of dust, &c. might conveniently enough be hindered by a piece of muslin, or thin hair-crape, tied slackly about the neck over the orifice of the cannula, so however as not to touch it, or to be wetted by the liquor coming from it.

The patient was soon perfectly recovered: he breaths, speaks, eats, drinks, and performs all the other offices of life, and goes about his calling as formerly. And now the Doctor notices the needless pains some writers take, about healing up the wound by bandaging, stitching, &c. For we found it easily to fill up of itself in a very few days, by only dressing it every other day or so with a soft tent, made less and less every dressing, and armed in the common way with liniment. arcæi. He believes indeed it would have taken a little more time to heal, if the patient had been older.

Various Celestial Observations made at Pekin, in the Years 1728 *and* 1729. *By Father Carbone.* N° 416, p. 455.

1. Several occultations of the fixed stars by the moon.

1728, Nov. 20,	5^{h}	0^{m}	42^{s}	morning the moon covered ν Leonis.
	6	21	55	the star emerged.
1729, March 11,	7	56	3	P. M. the moon covered η of Cancer.
April 2,	8	23	2	the moon in conjunction with the Pleiades.
11,	8	12	0	the moon covered ν of Leo.

2. Dec. 6, 1728, was a conjunction of the moon and Saturn, about 7 in the evening.

3. Several eclipses of Jupiter's satellites, of no use.

4. A total eclipse of the moon, Feb. 14, 1729.

At 2^{h}	38^{m}	30^{s}	morning, the eclipse began.
3	39	0	the total immersion.

At 5^h 17^m 10^s. . the first emersion.
6 17 40 . . the end of the eclipse.

A Description of the Cereus Peruvianus, which flowered at Nuremberg, in the Year* 1730. *By Dr. Chr. Ja. Trew.*† N° 416, p. 462.

This cereus, separated from another, of which it was a branch 7 years before, and exposed in open air all summer, grew without pushing forth branches. It is 6 feet 3 inches high, and 13 inches thick. It has 7 angles at its basis, 8 about the middle, and 9 near the top. Its upper part is of a sea-green, on account of the powder with which it is covered; its lower of a grass-green. The down of its prickles is between pale and white about the top, every where else it is brown. On Sept. 5, at the height of 6 feet 2 inches from the ground, it shot forth a certain round knot from its trunk, which without any help of art, so increased and extended almost horizontally, that on the 14th, it was 8 inches long, and plainly showed a flower, though as yet closed, embellished with a beautiful mixture of green, purple, and white. This same evening the flower began to open, and continued till midnight, when being entirely spread, it was 6 inches in diameter. It was of a pretty strong, but not very pleasant smell. After midnight it gradually contracted about half an inch, and remained thus till next day at noon. It then began to contract faster, to half the diameter the expanded flower was of; and the next morning it was quite closed and withered, but hung on the trunk till Sept. 30.

The beginning of the flower is a sort of tube 3 inches long, not quite an inch thick, between a yellow and a pale green. Its surface smooth, but channelled by certain small narrow furrows, between which, blunt protuberances were seen to run, in a parallel order, along the ridges. Where the tube expanded itself, it divided into more than 40 petaloid segments, ranked in 6 separate series, the 3 inferior and exterior of which here and there confounded their order, while the 3 superior and interior remained regular and unmixed. These series were distinguished by their size and colour. The pistillum of equal height with the surface of the flower, and hollow like a small tube, ran, at its upper end, into as many fine pale filaments, spread in the form of a crown, as there were segments in the inmost row, viz. 13.

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* Cactus Peruvianus Linn.

† This celebrated German botanist wrote the following works. (1) Libror. Botanicor. Catalogi tres 1752—1757. (2) Plantæ Selectæ, folio 1750, a splendid work with coloured figures. A continuation was published under the title of Plantæ Rariores. (3) Disquisitiones duæ de Cedro Libani 1757—1767, to which add several Dissertations inserted in the Nova Acta Nat. Curios. Dr. Trew cultivated in his garden at Nuremberg many curious plants from all parts of the world; and in this manner, as well as by his writings, he contributed largely to the advancement of botany; insomuch, that Linnæus judged him worthy of the honor of having a large Malabar tree named after him. He was born in 1695, and died in 1769.

The day before the flower dropped from the ovarium, the place where it was to separate was marked by a blackish circle, at which the tube separated spontaneously from the ovarium or matrix, that is, the rudiments of the fruit; the pistillum still firmly adhering to the ovarium. The flower, now fallen, being dissected longitudinally, the origin of the stamina lay open to the eye; and it very manifestly appeared that the petaloid segments of the flower, far from affording the least mark of a natural partition, stuck so very close to the tube, that not one of them would quit it without tearing it off by violence.

The fruit, though it came not to its full growth, plainly evinced, by inspection alone, that it is not prickly. On dissection it yielded a viscous juice, and within there was a certain cavity, the sides of which were every where, except at the bottom, thick set with an innumerable quantity of small villi, to each one of which hung an oblong, white, pellucid vesicle, which is the rudiment of the future seed.

END OF VOLUME THIRTY-SIXTH OF THE ORIGINAL.

A Catalogue of the fifty Plants from Chelsea-Garden, presented to the Royal Society, by the Company of Apothecaries, for the Year 1729; *pursuant to the Direction of Sir Hans Sloane, Bart. By Mr. Isaac Rand, Apothecary, F.R.S.* N° 417, p. 1. *Vol. XXXVII.*

*A Description of the Water Works at London Bridge. By Mr. Henry Beighton.** N° 417, p. 5.

The wheels are placed under the arches of London bridge; and moved by the common stream of the tide water of the river Thames.

AB, fig. 2, pl. 7, represents the axle-tree of the water wheel, 19 feet long, and 3 feet in diameter; C, D, E, F, are 4 sets of arms, 8 in each place, on which are fixed GGGG, 4 rings, or sets of felloes, 20 feet in diameter, and the floats HHH, 14 feet long, and 18 inches deep, being about 26 in number.

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* Of Mr. Beighton, though a respectable member of the Royal Society, we find no accounts relating to his life, &c. We only know, from some circumstances, that he was eminently skilled in machinery, and that he followed the profession of a civil engineer, being indeed the most eminent of his time He was the intimate friend of Dr. Desaguliers, who received from him many valuable communications in mechanics, engines, &c. some of which may be seen in various parts of the 2d vol. of the Doctor's Course of Experimental Philosophy, particularly at pp. 431, 437, 449, 461, 472, 497, 534, 539, &c.

It is probable that Mr. Beighton died in 1743 or 1744, as it appears he conducted the Ladies' Diary for the Stationers' Company, from 1714 to 1744 inclusively; discharging that trust with such satisfaction to the company, that they permitted his widow to enjoy it for many years afterward, by employing a deputy to compile that very useful annual little book. In this almanac, for the year 1721, Mr. Beighton inserted a curious table of calculations on the steam engine.

The wheel lies with its 2 gudgeons or centres, A, B, on 2 brasses in the pieces MN, which are 2 large levers, whose fulcrum, or prop is an arched piece of timber L, the levers being made circular on their lower sides to an arch of the radius MO, and kept in their places by 2 arching studs, fixed in the stock L, through 2 mortises in the lever MN.

By these levers the wheel is made to rise and fall with the tide, which is performed in the following manner. The levers MN are 16 feet long; from M, the fulcrum of the lever, to O, the gudgeon of the water wheel, 6 feet; and from O to the arch N, 10 feet. To the bottom of the arch N is fixed a strong triple chain P, made in the fashion of a watch-chain, only the links arched to a circle a foot in diameter, with notches or teeth, to take hold of the leaves of a pinion of cast iron Q, 10 inches in diameter, with 8 teeth in it moving on an axis. The other loose end of this chain has a large weight hanging to it, to help to counterpoise the wheel, and preserve the chain from sliding on the pinion. On the same axis is fixed a cog-wheel R, 6 feet in diameter, with 48 cogs: to this is applied a trundle or pinion S, of 6 rounds or teeth; and on the same axis is fixed T, a cog-wheel of 51 cogs, into which the trundle V, of 6 rounds, works; on whose axis is a winch, or windlass, W, by which one man, with the two windlasses, raises or lets down the wheel, as there is occasion.

And because the fulcra of these levers, MN, are in the axis of the trundle K, viz. at M or X, in what situation soever the wheel is raised or let down, the cog-wheel II, is always equidistant from M, and works or geers truly.

By means of this machine the strength of an ordinary man will raise about 50 ton weight.

II is a cog-wheel fixed near the end of the great axis, 8 feet in diameter, and 44 cogs working into a trundle K, 4½ feet in diameter, and 20 rounds, whose axis or spindle is of cast iron 4 inches in diameter, lying in brasses at each end, as at X.

ZZ is a quadruple crank of cast iron, the metal being 6 inches square, each of the necks being turned 1 foot from the centre, which is fixed in brasses at each end in two head stocks, fastened down by caps. One end of this crank at Y is placed close abutting to the end of the axle-tree X, where they are at those ends 6 inches in diameter, each having a slit in the ends, where an iron wedge is put, one half into the end X, the other half into Y, by means of which the axis X turns round the crank ZZ.

The 4 necks of the crank have each an iron spear, or rod, fixed at their upper ends to the respestive libra or lever, a 1, 2, 3, 4, within 3 feet of the end. These levers are 24 feet long, moving on centres in the frame bbbb; at the end of which, at c, 1, 2, 3, 4, are jointed 4 rods with their forcing plugs, working into d 1, 2, 3, 4, 4 cylinders of cast iron 4¾ feet long, 7 inches bore above, and 9 below, where the valves lie, fastened by screwed flanches, over the 4 holes of a hollow trunk of cast iron, having 4 valves in it just over eeee, at the joining on of the bottom of the barrels, or cylinders; and at one end a sucking pipe and grate f, going into the water, which supplies all the 4 cylinders alternately.

From the lower part of the cylinders d 1, d 2, d 3, d 4, come out necks turning upward archwise, as gggg, whose upper parts are cast with flanches to screw up to the trunk hhhh; which necks have bores 7 inches in diameter, and holes in the trunk above, communicating with them, at which joining are placed 4 valves. The trunk is cast with 4 bosses, or protuberances, standing out against the valves, to give room for their opening and shutting; and on the upper side are 4 holes stopped with plugs, to take out on occasion, to cleanse the valves. One end of this trunk is stopped by a plug i; to the other, iron pipes are joined, as i 2 by flanches, through which the water is forced up to any height, or place required.

Besides these 4 forcers, there are 4 more, placed at the other ends of the libræ, or levers (not shown in the fig. to avoid confusion, but to be seen on the left hand) the rods being fixed at a 1, 2, 3, 4, working in 4 such cylinders, with their parts dd, &c. ee, f, gg, and i, as before described, standing near kk.

At the other end of the wheel at B, is placed all the same sort of work, as is described at the end A, viz.

The cog-wheel I
The trundle K
The spindle X
The crank Y, Z
The sucking pipes f

The 4 levers ac, ac, &c.
8 forcing rods ad, ad, &c.
8 cylinders de, de, &c.
4 trunks, as ee, hh, &c.
2 forcing pipes, as i.

So that one single wheel works 16 pumps. All which work could not be drawn in one perspective view, without rendering it very much confused.

The following is a calculation of the quantity of water raised by the engine at London-bridge.

In the first arch next the city is one wheel, with double work of 16 forcers.

In the third arch	first wheel double work at one end, and single at the other	12	
	second wheel in the middle	8	
	third wheel	16	
	In all	52	forcers
One revolution of a wheel makes in every forcer ……		2⅕	strokes
So that one turn of the 4 wheels makes …………		114	strokes

When the river is at best, the wheels go 6 times round in a minute, and but $4\frac{1}{2}$ at middle water .... } 6

The number of strokes in a minute 684
The stroke $2\frac{1}{2}$ feet, in a 7 inch bore, raises........ 3 } ale gallons
They raise per minute 2052

That is, 123120 gallons = 1954 hogsheads per hour, and at the rate of 46896 hogsheads in a day, to the height of 120 feet.

This is the utmost quantity they can raise, supposing there were no imperfections or loss at all.

But it is certain from the following considerations, that no engine can raise so much, as will answer the quantity of water the cylinder contains, in the length of the forcer, or piston's motion: for, 1. The opening and shutting of the valves lose nearly so much of that column, as the height they rise and fall. 2. No leather is strong enough for the piston; but some water must continually slip or squeeze by, when it is raised to a considerable height; and when the column is short, it will not press the leather enough to the cylinder, or barrel; but especially at the beginning, or first moving of the piston, there is so little weight on it, that before the leather can expand, there is some loss. 3. And this loss is more or less, as the pistons are looser or tighter leathered. 4. When the leathers grow too soft, they are not capable of sustaining the column to be raised. 5. If they be leathered very tight, so as to lose no water, then a great part of the engine's force is destroyed by the friction.

By some experiments Mr. Beighton accurately made on engines, whose parts are large and very well executed, they will lose $\frac{1}{5}$ and sometimes $\frac{1}{4}$ of the calculated quantity. However the perfections or errors of engines are to be compared together by the calculated quantities or forces; for, as they differ in those, they will proportionably differ in their actual performances.

The power by which the wheels are moved, is as follows.

The weight of the column of water on a forcer 7 inches in diameter, and 120 feet high.

7 × 7 = 49 lb. the pounds aver. in a yard nearly
40 yards high

1960 lb. on one forcer
8 forcers always lifting

The whole weight on the engine at once.. } 15680 lb. = 140 ct. = 7 ton weight.

Then the crank pulls the libra 3 feet from the forcer, and 8,3 feet from the centre.

7 ton
× 11,3

8,3) 79,1 (9,5 ton on the crank. ton
wallower 2,2) 9,5 (4,3 on the trundle
the spur-wheel 4

The radius of the great wheel 10) 17,2 (1,72 ton
20

The force on the floats 18 ct. 40 lb. 34,40 ct.

But to allow for friction and velocity, may be reckoned 1½ ton.

The ladles or paddles 14 feet long, 18 inches deep.............. } = 22,4 square feet.
The fall of water is sometimes 2 feet

44,8
6 gallon in a cubit foot
268,8
10 lb. in a gallon
11)2688(24 hundred.

The velocity of the water 4 feet in $21'''$ of time.

$21'''$: 4 feet :: $60''$: 685 feet per minute.

The quantity expended on the wheel, according to the velocity of the stream 1433 hogsheads per second.

But at the velocity of the wheel 645 hogsheads per second.

The velocity of the wheel to the velocity of the water, as 1 to 22.

Mr. Beighton makes the following observations on these water-works.

Though they may be justly esteemed as good as any in Europe; yet there are some things, as he conceives, which might be altered very much for the better.

1. If instead of 16 forcers, they worked only 8, the stroke might be 5 feet in each forcer, which would draw a great deal more water with the same power on the wheel; for, then there would be but ½ the opening and shutting of valves; consequently, but half that loss; and a 5-foot stroke draws above double the quantity of 2 strokes of 2½ feet each, by near ⅓, as the velocity is double; which is the most valuable consideration in an engine, where the pipes will sustain such force.

2. The bores that carry off the water from the forcers are too small, there being nearly always 2 columns of 7 inches diameter, forcing into one pipe of the same diameter, and $7 \times 7 = 49 + 49 = 98$.

Therefore, those pipes of conveyance should be near 9 inches in diameter.

The timber-work is all admirably well executed; and the composition and contrivance, both for strength and usefulness, not exceeded by any he has seen.

The cranks of cast-iron are better than of wrought iron; by reason they are very stiff, and will not be strained, but sooner break; and then they are cheap, and new ones easily put in.

The wedge for putting on, or releasing the crank and forcers, is better than the sliding sockets, commonly made use of.

The forcing barrels, trunks, and all their apparatus, are very curiously contrived for putting together, mending, altering, or cleansing, and subject to as little friction as possible in that part.

The machine for raising and falling the wheels is very good, though but seldom used, as he is informed: for, they will go at almost any depth of water, and as the tide turns, the wheels go the same way with it.

These machines at London-bridge are far superior to those so much famed at Marley in France, as the latter are very ill contrived in the cranks, and some other parts.

Account of a Stone broken in the Bladder, and voided through the Urethra. By Lawrence Heister, M. D.* N° 417, p. 13. *An Abstract from the Latin.*

A man upwards of 60 years of age, of a robust constitution, and addicted to free living, had for some years past been troubled with frequent and violent

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* Lawrence Heister was a native of Frankfort on the Mayne, and professor of anatomy and surgery, first at Altdorf, and afterwards at Helmstadt. He studied under Ruysch, Bidloo, Albinus and Boerhaave, and was surgeon to the Dutch forces during the campaigns of 1707 and 1709, when the war in Flanders afforded him frequent opportunities of displaying his medical and chirurgical skill. Besides a variety of smaller tracts, he wrote a very useful manual of anatomy, entitled "Compendium Anatomicum," 1717, 4to. which has been frequently reprinted, and has moreover been translated into most of the European languages; Compendium Institutionum Medicinæ, 4to. 1736; Compendium Med. Practicæ, 2 vols. 8vo. 1743; Chirurgiæ Institutiones, 4to. 1748, exhibiting an excellent systematic view of surgery, drawn from the most approved authors, both ancient and modern, and accompanied with numerous original observations and improvements. This work procured him a great reputation; it has gone through many editions, and has been translated into English, German, French, &c. After his death a collection of his medical, surgical, and anatomical observations, was published in 2 vols. 4to. 1753—1770, in the German language. Heister, as his friend Haller has remarked, was a diligent and industrious teacher, as well as an able and successful practitioner. It was in anatomy and surgery that he chiefly excelled, the knowledge of which he greatly promoted by his lectures, as well as by his writings. He was a good operator, and suggested many improvements in the construction of surgical instruments.

nephritic attacks, and at times voided by the urethra, with excruciating pain, a great number of calculi, several of which were larger than a pea; but about 4 years since he felt all the symptoms of stone in the bladder. In 1728, after the use of various medicines (at the same time constantly taking for his common drink a kind of Brunswick beer, called duchstein, in high repute for the stone, and on that account exported abroad) he sometimes felt violent pains in making water, accompanied with strong contractions of the bladder, and a sensation as if one or more calculi were broken in it. Immediately after this he voided with his urine some pieces of a broken calculus, and this he continued doing for several days after; till at length they were all discharged. His pain then left him, as well as every other symptom of the stone. The patient declared that he had voided above 100 pieces of broken calculi, some of which were half as large as one's thumb, but many of them were smaller; their external surface was convex, the internal of most of them concave; some of them exhibited the nucleus of a stone. The number and appearance of the pieces of broken calculi prove that they came from the bladder, and that they had once formed whole stones there, which were afterwards broken and discharged; but whether by means of the medicines, or of the beer, or by the force of nature, Dr. H. could not determine. From the convexity of the segments of the pieces which came away, it may be inferred that very few of the stones were larger than a nutmeg, and that several of them were smaller.

Concerning the Frost in January 1730-1. *By the Rev. Wm. Derham, D. D. Canon of Windsor, and F. R. S.* N° 417, p. 16.

In the Phil. Trans. N° 324, Dr. Derham gave an account of some of the most remarkable frosts he could find any account of; and particularly of that great, and he had almost said universal one, in 1708, which the Royal Society had very good histories of from divers parts, and which he in that N° had taken from the original papers.

He there made it very probable, that the greatest descent of the spirits in the thermometer, was on Dec. 30, 1708, when the spirit in his tube was within $\frac{1}{10}$ of an inch as low as it is with artificial freezing with snow, or ice and salt; and in the frost in Jan. 1730-1 it was almost, if not altogether, as low.

The freezing point of his thermometer is 10 inches, which he calls 100 degrees, above the ball; and the most intense freezing, according to the methods mentioned in that Transaction, is just at, or very little within the ball. And on Jan. 30, about sun-rise, the thermometer was but an inch, or 10 degrees above the point of extreme freezing; and on Feb. 3, only at $\frac{1}{2}$ an inch, or 5 degrees. And considering that the thermometer he observed with in 1708 was less accurate, and differently graduated from that which he had at this time, he is apt to think, that the frost on Feb. 3, was altogether as intense, as that on Dec. 30, 1708: for, though a frigorific mixture sunk the spirits but one tenth lower in the old thermometer, and about 5 or 6 tenths in that he observed with at this time; yet he takes the difference to be little, or none at all, on account of the tenderness of the new above the old glass.

And this degree of cold he takes to be as excessive as in any of the years mentioned in the said Transaction; nay, any of the years when the Thames at London was frozen over; and he is sure, colder than in 1716, when that river was frozen over for several miles; and booths and streets were made on the ice, an ox roasted on it, &c. For, the lowest point of freezing in 1716, was on Jan. 7, when the spirits fell to 35 degrees only, in the thermometer he made use of at this time: but the true cause of the freezing of the Thames that year, was not barely the excess of the cold, but the long continuance of it; which was also the chief cause of those remarkable congelations of that river in 1683 and 1708, when Dr. Derham saw coaches driven over the ice, large fires made on it, &c.

Several Experiments concerning Electricity. By Mr. Stephen Gray.

N° 417, p. 81.

In February 1728-9, Mr. Gray repeated some of the experiments he had formerly made, on the first discovery of an electrical attraction in several bodies, not before known to have that property. He made several attempts on the metals, to see whether they might not be made to attract by the same method as other bodies were, viz. by heating, rubbing, and hammering; but without any success: he then resolved to procure a large flint-glass tube, to see if he could make any further discovery with it, having recollected a suspicion which he had some years before, namely, that as the tube, when rubbed in the dark, communicated a light to bodies, whether it might not at the same time communicate an electricity to them; though he never hitherto tried the experiment, not imagining the tube could have so considerable and surprising an influence, as to cause them to attract with so much force, or that the attraction would be carried to such vast distances, as shall be found in the sequel of this Transaction.

Before he proceeds to the experiments, he gives a description of the tube: it is 3 feet 5 inches in length, and near 1.2 inches in diameter: he gives the mean dimensions; the tube being larger at each end than in the middle; the bore was about an inch: to each end he fitted a cork, to keep out the dust, when the tube was not in use.

The first experiment he made was to see, whether he could find any difference in its attraction, when the tube was stopped at both ends with the corks, or when left open; but he could perceive no sensible difference. But on holding a down-feather opposite the upper end of the tube, he found that it would go to the cork, being attracted and repelled by it, as by the tube, when excited by rubbing. He then held the feather opposite the flat end of the cork, which several times together attracted and repelled; at which he was much surprised, and concluded that there was certainly an attractive virtue communicated to the cork by the excited tube.

He fixed an ivory ball of about 1.3 inches in diameter, with a hole through it, on a fir-rod about 4 inches long, thrusting the other end into the cork; and on rubbing the tube, found that the ball attracted and repelled the feather with more vigour than the cork had done; repeating its attractions and repulsions for several times together. He then fixed the ball on longer rods; first on one of 8 inches, and afterwards on one of 24 inches long, and found the effect the same. He then made use of iron, and next of brass-wire, to fix the ball on, inserting the other end of the wire in the cork, as before; and he found that the attraction was the same, as when the fir sticks were used; and that when the feather was held against any part of the wire, it was attracted by it; but though it was then nearer the tube, yet its attraction was not so strong as that of the ball. When the wire of 2 or 3 feet long was used, its vibrations caused by rubbing the tube, made it somewhat troublesome to be managed. This put Mr. Gray on thinking, whether if the ball were hung by a packthread, and suspended by a loop on the tube, the electricity would not be carried down the line to the ball; he found it to succeed accordingly: for, on suspending the ball on the tube by a packthread about 3 feet long; when the tube had been excited by rubbing, the ivory ball attracted and repelled the leaf-brass, over which it was held, as freely as it had done, when it was suspended on sticks or wire; as did also a ball of cork, and another of lead, that weighed 1 pound and $\frac{1}{4}$.

After he had found that the several bodies abovementioned had an electricity communicated to them; he then went on to try on what other bodies the tube would have the same effect, beginning with the metals, suspending them on the tube by the method abovementioned; first in small pieces, as with a guinea, a shilling, a halfpenny, a piece of block-tin, and a piece of lead; then with larger quantities of metal, suspending them on the tube with packthread. Here he made use of a fire-shovel, tongs, iron-poker, a copper tea-kettle (which succeeded the same, whether empty, or full of cold or hot water) and a silver pint pot; all which were strongly electrical, attracting the leaf-brass to the height of several inches. After he had found that the metals were thus electrical, he went on to make trials on other bodies; as, flint-stone, sand-stone, loadstone, bricks, tiles, chalk; and then on several vegetable substances, as well green as dry; and found that they had all of them an electric virtue communicated to them, either by being suspended on the tube by a line, or fixed on the end of it by the method abovementioned.

He next proceeded to try at what greater distances the electric virtue might be carried; and having by him part of a hollow walking cane, which seemed to be part of a fishing-rod, 2 feet 7 inches long; he cut the great end of it, to fit it into the bore of the tube, into which it went about 5 inches; then when the cane was put into the end of the tube, and this last excited, the cane drew the leaf-brass to the height of more than 2 inches, as did also the ivory ball, when fixed to the cork and stick at the end of the cane. A solid cane had the same effect, when inserted in the tube after the same manner as the hollow one had been. He then took the two upper joints of a large fishing-rod, the one of Spanish cane, the other partly wood, and the upper end whale-bone, which, together with the tube, made a length of more than 14 feet. On the lesser end of the whale-bone was fixed a ball of cork, of about 1 inch and ¼ in diameter; then the large end of the rod being inserted in the tube, the leaf-brass laid on the table, and the tube excited, the ball attracted the leaf-brass to the height of about 3 inches by estimation. With several pieces of Spanish cane and fir-sticks he afterwards made a rod, which together with the tube, was somewhat more than 18 feet long, which was the greatest length he could conveniently use in his chamber, and he found the attraction very nearly, if not altogether, as strong as when the ball was placed on shorter rods.

May 14, 1729, between 6 and 7 o'clock in the evening, having procured a rod of about 24 feet, that consisted of a fir-pole, of cane, and the top of reed, on the end of which the ball of cork was placed, and the large end of the rod put into the tube about 7 or 8 inches; then the leaf-brass being laid down, and the tube rubbed, the ball attracted and repelled the leaf-brass with vigour: so that it was not at all to be doubted, but with a longer pole the electricity would have been carried much farther.

May 16, he made a rod 32 feet long, including the tube; the larger part of it was a fir-staff about 6 feet and a half long, the rest was of cane, and reed for the top part. All things being prepared as before, the effect was the same as in the last experiment; only the pole bending so much, and vibrating by rubbing the tube, made it more troublesome to manage the experiment. This put him on making the following experiments.

May 19, about 6 in the morning, the ivory ball being suspended on the tube by a line of packthread 26 feet long, and then the tube being rubbed, attracted the leaf-brass to the height of near 2 inches. This was repeated with the cork-ball with the same success.

May 31, in the morning, a line of 34 feet in length was tied to a pole of 18 feet: so that the pole and line together were 52 feet. With the pole and tube Mr. Gray stood in the balcony, the assistant below in the court, where he held the board with the leaf-brass on it; then the tube being excited as usual, the electric virtue passed from the tube up the pole, and down the line to the ivory-ball, which attracted the leaf-brass; and as the ball passed over it in its vibrations, the leaf-brass would follow it, till it was carried off the board: but these experiments are difficult to make in the open air, the least wind carrying away the leaf-brass.

Sometime after he made several attempts to carry the electric virtue in a line horizontally; since he had not the opportunity here, of carrying it from greater heights perpendicularly, but without success, for want of then making use of proper materials, as will appear from what follows. The first method he tried, was by making a loop at each end of a line, and hanging it on a nail, driven into a beam, the other end hanging downwards; through the loop at this end, the line with the ivory-ball was put, the other end of this line was by a loop hung on the tube: so that the part of the line next the ball hung perpendicularly, and the rest of the line horizontal: then the leaf-brass being laid under the ball, and the tube rubbed, there was not the least sign of attraction perceived. Hence he concluded, that when the electric virtue came to the loop, that was suspended on the beam, it went up the same to the beam: so that none, or very little of it at least, came down to the ball; which was afterwards verified, as will appear by the experiments mentioned hereafter.

June 30, 1729, Mr. Gray went to Otterden-place, to give Mr. Wheler a specimen of his experiments. The first was from the window in the long gallery, that opened into the hall, the height being about 16 feet. The next experiment was from the battlements of the house, down into the fore-court, 29 feet. Then from the clock-turret to the ground, which was 34 feet; this being the greatest height they could come at: and notwithstanding the smallness of the cane, the leaf-brass was attracted and repelled beyond what Mr. Gray expected. As they had no greater heights here, Mr. Wheler was desirous to try, whether they could not carry the electric virtue horizontally: Mr. Gray then told him of the attempt he had made with that design, but without success; as also of the method and materials made use of, as mentioned above. Mr. Wheler then proposed a silk line to support the line, by which the electric virtue was to pass. This Mr. Gray told him might do better, on account of its smallness: so that there would be less virtue carried from the line of communication; with which, together with the apt method Mr. Wheler contrived, and with the great pains he took, and the assistance of his servants, they succeeded far beyond expectation.

The first experiment was made in the matted gallery July 2, 1729, about 10 o'clock in the morning. About 4 feet from the end of the gallery there was a cross line, fixed by its ends to each side of the gallery, by two nails; the middle part of the line was silk, the rest at each end packthread; then the line to which the ivory ball was hung, and by which the electric virtue was to be conveyed to it from the tube, being 80½ feet in length, was laid on the cross silk line; so that the ball hung about 9 feet below it. Then the other end of the line was by a loop suspended on the glass cane, and the leaf-brass held under the ball on a piece of white paper; when the tube being rubbed, the ball attracted the leaf-brass, and kept it suspended for some time.

This experiment succeeding, and the gallery not permitting to go any further in one length, Mr. Wheler thought of another expedient, by which they might increase the length of the line; which was by putting up another cross line near the other end of the gallery; and over the silk part of both the lines there was laid a line, long enough to be returned to the other end, where the ball hung; and though now both ends of the line were at the same end of the gallery, yet care was taken that the tube was far enough off from having any influence on the leaf-brass, except what passed by the line of communication: then the cane being rubbed, and the leaf-brass held under the ivory ball, the electric virtue passed by the line of communication to the other end of the gallery, and returned back again to the ivory ball, which attracted the leaf-brass, and kept it suspended as before. The whole length of the line was 147 feet.

They then thought of trying, whether the attraction would not be stronger, without doubling or returning the line, which they found means of doing in Mr. Wheler's barn, where they had a line of 124 feet long, 14 feet of which hung perpendicular from the silk line: and now the attraction was stronger than when the line was returned, as in the matted gallery.

July 3, between 10 and 11 o'clock in the morning, they went again into the barn, and repeated the last experiment, both with the tube and cane; but the attraction was not so strong as the preceding evening, nor was there so great a difference in the attraction, communicated by the solid cane and glass tube, as was expected, considering the difference of their lengths and diameters.

They then proceeded further, by adding so much more line, as would make a return to the other end of the barn, the whole length of the line being now 293 feet; and though the line was so much lengthened, they found no perceivable difference in the attraction, the ball attracting as strongly as before. This encouraged them to add another return; but on beginning to rub the tube, the silk lines broke, not being strong enough to bear the weight of the line, when shaken by the motion communicated to it by rubbing the tube. On this, instead of the silk, they put up small iron wire; but this was too weak to bear the weight of the line. They then took brass wire, of a somewhat larger size than the iron wire. This supported the line of communication. But though the tube was well rubbed, yet there was not the least motion or attraction, communicated by the ball, neither with the great tube which they used, when they found the small solid cane to be ineffectual. By which they were now convinced, that the success they had before, depended on the lines that supported the line of communication, being silk; and not on their being small, as before trial Mr. Gray imagined it might be; the same effect happening here, as when the line that is to convey the electric virtue is supported by packthread, viz. that when the effluvia comes to the wire, or packthread that supports the line, it passes by them to the timber, to which each end of them is fixed; and so goes no further forward in the line that is to convey it to the ivory ball.

Finding that the silk threads were too weak to bear many returns of line, Mr. Wheler thought of another way of managing them: so that fewer returns might be on each silk line; which was by placing two other cross lines some feet below the upper ones: so that every other turn of line was suspended by the lower cross line. By this means there was but half the weight of line on each silk of what there was, when only two cross lines were used. By this contrivance they could add a much greater length of line, without danger of breaking the silk. They then put up a line, that was 666 feet in length, by 8 returns: then the leaf-brass being held on a piece of white paper under the ivory ball; and the tube, with the other end of the line suspended on it, being rubbed for some time, the leaf-brass was attracted as manifestly as it had been with much shorter lines. They then repeated the experiment with the little short solid cane, and found there was somewhat of an attraction, but not near so great as with the large tube.

Though the going and returning of the electric effluvia was very surprising, yet they were willing to try, how far the attractive virtue might be carried in a continued right line; the method of doing which was as follows: that end of the line, where the attraction was to be made, was suspended on a silk line, fixed across the garret-window on the north side of the house, which was about 40 feet high: at about 100 feet from hence two rods or poles, of about 10 feet in length, and at 2 feet distance from each other, were driven into the ground, in such a manner as that they stood nearly perpendicular. These were in the large garden, beyond these in the large field, that is separated from the garden by a deep foss: about the same distance from the first were fixed another pair of poles; then 4 others at a like distance. On the ends of these poles were tied the cross lines of silk, to support the line of communication; which being laid on the silk lines, the ivory ball hanging in the garret window; and the other end of the line being hung by a loop on the tube, the leaf-brass was held under the ball; and after the tube had been rubbed for some time, they called to Mr. Gray to let him know, that there was an attraction of the leaf-brass: this was several times repeated with success. Then Mr. Wheler came into the field, and rubbed the tube himself, that Mr. Gray might see there was an attraction; which he did, though he perceived it not to be so strong, as when the attraction was conveyed by a longer line by returning it, as in the experiments abovementioned. The length of the line was 650 feet. This was several times repeated; but the experiment being made in the evening, at length the dew began to fall. They began about 7 o'clock, or some little time after; but before 8 o'clock the attraction ceased: but whether this was caused by the dew falling, or by Mr. Gray's being very hot, could not positively be said. This experiment was made July 14, 1729.

Some days after, this experiment was repeated from the turret closet window, when the line was 765 feet; and the attraction was no less perceivable than in the experiment abovementioned.

The following experiments, made at Mr. Wheler's, show that large surfaces may be impregnated with electric effluvia.

A large map of the world, containing 27 square feet, as also a table cloth, containing 59 square feet, being suspended on the tube by packthreads, became electrical: an umbrella suspended by a packthread, tied to its handle, became strongly electrical.

An experiment to see whether the electric virtue would be any way hindered by the magnetical effluvia of a loadstone. The loadstone had a small key suspended by one of its arming irons, and both of them were suspended on the tube by a packthread; then the tube being rubbed, both the key and stone attracted the leaf-brass; the attraction being the same as that of other bodies.

An experiment made to show that the electric virtue is carried several ways at the same time, and may be conveyed to considerable distances. There were made 3 stands, each composed of two upright pieces of fir, fixed perpendicular, near the ends of a long square board, near a foot and a half distant from each other. On the tops of these were tied threads of silk, to support the lines of communication with the tube and attracting bodies. One of these stands was placed in Mr. Wheler's great parlour, near the farther end; another in the little parlour; and a third in the hall, which was between the two parlours. As the other two were one of them to the right, and the other to the left hand, this last was placed near the hall-window forwards: the first two were about 50 feet; the other about 20 feet from the place where the tube was held. Then there were taken 3 small square pieces of wood, tied to 3 lines of packthread; these were about the lengths abovementioned: they were laid on the silk lines; and by loops at the other ends were suspended on the tube: then the leaf-brass being held under the pieces of wood, and the tube rubbed, they all attracted the leaf-brass at the same time: and some time after in Mr. Gray's absence, Mr. Wheler tried a red hot poker; and found that the attraction was the same, as when cold. He also suspended a live chick on the tube, by the legs, and found that the breast of the chick was strongly electrical.

At Mr. Godfrey's Mr. Gray made the following experiments, showing that the electric virtue may be carried from the tube, without touching the line of communication, by only being held near it. The first of these experiments was made August 5, 1729. He took a piece of hair cloth, such as linen-cloths are dried on, of about 11 feet in length; which, by a loop at the upper end was suspended on a nail, driven into one of the rafters in the garret, and had at its lower end a weight of 14 pounds, hung to it by an iron ring: then the leaf-brass was laid under the weight, and the tube rubbed, and being held near the line without touching it, the lead-weight attracted and repelled the leaf-brass for several times together, to the height of at least 3, if not 4 inches. If the tube were held 3 or 4 feet above the weight, there would be an attraction; but if it were held higher up, so as to be near the rafter, where the weight was suspended by the hair-line, there would be no attraction.

An experiment showing that the electric virtue may be carried several ways at the same time, by a line of communication, without touching the said line. —There were taken two hair-lines, between 4 and 5 feet long; to each of these was tied a square piece of cork, by packthread; the lines were suspended by loops at their upper ends on two nails; near the lower ends there was tied to the hair-lines a piece of packthread, by which there was a communication between the two hair-lines; then the leaf-brass being laid under the corks, and the tube rubbed, and held near one of the lines, both the corks attracted: but that which was farthest much stronger than that near which the tube was held. About the middle of the line of communication they both attracted with equal force.

Some time after, at Mr. Wheler's, they made the following experiment, to try whether the electric attraction be proportional to the quantity of matter in bodies.—There were made two cubes of oak, about 6 inches square, the one solid, the other hollow: these were suspended by 2 hair-lines, nearly after the same manner as in the experiment abovementioned: the distance of the cubes from each other was about 14 or 15 feet; the line of communication being tied to each hair-line, and the leaf-brass placed under the cubes, the tube was rubbed and held over the middle of the line, and as near as could be guessed, at equal distances from the cubes; when both of them attracted and repelled the leaf-brass at the same time, and to the same height: so that there seemed to be no more attraction in the solid, than in the hollow cube; yet Mr. Gray is apt to think, that the electric effluvia pass through all the interior parts of the solid cube, though no part but the surface attracts: for, from several experiments it appears, that if any other body touch that which attracts, its attraction ceases till the body be removed, and the other be again excited by the tube.

The sequel of the experiments made at Mr. Godfrey's.

Mr. Gray next went on with an experiment to see if the electric virtue might not be conveyed to a rod, without inserting it into the bore of the tube, or without touching the rod: which he found to succeed, by suspending the rod either by lines of silk, or by pieces of horse-hair fishing lines, placing a ball of cork on the lesser end of the rod.

August 13, 1729, he took a large pole, 27 feet long, 2½ inches diameter at the great end, and about half an inch at the less end: it was that sort of wood they call horse-beech, with the rind on. This was suspended by 2 hair-lines of about 4½ feet in length: the first line was about 2 feet from the great end of the pole; the other about 8 feet from the less end: so that the pole hung horizontal. At the small end of the pole was suspended a ball of cork, about an inch and a half in diameter, by a packthread about a foot long, and a small leaden ball on the cork, to keep the packthread extended. Then the leaf-brass being laid under the cork, the tube rubbed, and held near the great end of the pole, the cork-ball attracted the leaf-brass strongly to the height of an inch, if not more: then the leaf-brass being held under several parts of the pole, it was attracted, as Mr. Godfrey observed, but not near so strongly as by the cork.

About the beginning of September 1729, Mr. Gray made the following experiment; which shows that the electric effluvia will be carried in a circle, and communicated from one circle to another.—There was taken a hoop, of about 2 feet 2 inches diameter; this he suspended by a hair-line on a nail, driven into a beam: the line was about 4 feet long: then the leaf-brass being laid under the hoop, the tube was rubbed, and held within the hoop, near its upper side, without touching it, by several inches: then the lower part of the hoop attracted and repelled the leaf-brass strongly; but when held near the lower part, there was very little, if any attraction. If the tube were held near the outside of the hoop, it attracted; but strongest, when at the same time it was held near the knot of the hair-line, by which the hoop was suspended. To this hoop there was tied a less hoop of about a foot and a half in diameter: it was tied to it by packthread; so as to hang below it about 2 inches. They were suspended together by the hair-line; then the leaf-brass and tube being prepared, as beforementioned, the tube being held near the upper hoop, the lower part of the lower hoop attracted strongly; and when held near the upper part of the lower hoop, but very weakly: but when held near the lower part of the lower hoop, there was no attraction.

September 5, Mr. Gray made the following experiment; which shows that the electric effluvia have the same effect in a circle, when its position is horizontal. He took a large hoop, of somewhat more than 3 feet diameter, and about $2\frac{1}{2}$ inches in breadth: to this were tied, at nearly equal distances, 4 lines of twine (i. e. 3 threads of packthread twisted together) each about 2 feet 8 inches long. These were tied with their ends together to a hair-line of about 2 feet and a half long, by which the hoop was suspended on a nail, as in the other experiments; so that the hoop hung now in a horizontal position. Then the brass-leaf being laid under the edge of the hoop, at between 2 and 3 inches below it, the tube being rubbed, and held between the cords without touching them, the leaf-brass was attracted and repelled for several times together; but when held near the outside of the hoop, opposite to that part where the leaf-brass lay, the attraction was much stronger.

About the latter end of autumn 1729, he resumed his inquiry after other electric bodies, and found many more that have the same property, and may be excited to attract by the same method. As for instance, the dry withered leaves of reeds and flags, grass and corn, both leaves and straw; the leaves of trees, as those of the laurel, oak, walnut, chesnut, hazle nut, apple and pear-tree leaves: so that it may be concluded, that the leaves of all vegetables have this attractive virtue.

Mr. Gray made the following experiments at his chamber, March 23, 1730. —He dissolved soap in the Thames water; then he suspended a tobacco-pipe by a hair-line, so as that it hung nearly horizontal, with the mouth of the bowl downwards; then having dipped it in the soap-liquor, and blown a bubble, the leaf-brass being laid on a stand under it, and the tube rubbed, the brass was attracted by the bubble, when the tube was held near the hair-line. He then repeated the experiment with another bubble, holding the tube near the small end of the pipe; and the attraction was now much greater, the leaf-brass being attracted to the height of near 2 inches.

March 25, he repeated this experiment after a somewhat different manner: the pipe was now suspended by 2 lines of white sewing silk, of about 5½ feet long: these were hung on 2 nails, driven into the beam of his chamber, about a foot distant from each other, by loops at the other ends of the lines, by which the pipe was suspended: then the bubble being blown, by holding the tube to the small end of the pipe, the bubble attracted the leaf-brass to the height of near 4 inches. This experiment was made to see whether fluid bodies would not have an electricity communicated to them.

April 8, Mr. Gray made the following experiment on a boy between 8 and 9 years of age. His weight, with his clothes on, was 47 lb. 10 oz. He suspended him in a horizontal position, by 2 hair-lines, such as clothes are dried on: they were about 13 feet long, with loops at each end. There was driven into the beam of his chamber, a pair of hooks opposite to each other; and 2 feet from these another pair in the same manner.

On these hooks the lines were suspended by their loops, so as to be in the manner of two swings, the lower parts hanging within about 2 feet from the floor of the room: then the boy was laid on these lines with his face downwards; one of the lines being put under his breast; the other under his thighs. Then the leaf-brass was laid on a stand, which was a round board of a foot diameter, with white paper pasted on it, supported on a pedestal a foot high, which Mr. Gray had frequently used in his experiments. The tube being rubbed, and held near his feet, without touching them, the leaf-brass was very vigorously attracted by the boy's face; so as to rise to the height of 8, and sometimes 10 inches. Mr. Gray put a great many pieces of leaf-brass on the board together, and almost all of them rose up together at the same time. Then the boy was laid with his face upwards; and the hinder part of his head, which had short hair on, attracted, but not at quite so great a height as his face did. Then the leaf-brass was placed under the boy's feet, his shoes and stockings being on, and the tube held near his head; his feet attracted, but not at so great a height as his head; then leaf-brass was again laid under his head, and the tube over it; but there was then no attraction; nor was there any, when the leaf-brass was laid under his feet, and the tube held over them.

April 16. Mr. Gray repeated the experiment with the boy: but now the attraction was not quite so strong as at the first, the leaf-brass not rising higher than to about 6 inches. The boy's hands being extended nearly horizontal, Mr. Gray placed a small stand with leaf-brass under each hand, and the large stand, furnished as the others, under his face; when the excited tube being held near his feet, there was an attraction by his hands and face at the same time. Mr. Gray then gave him the top of a fishing rod to hold in his hand; there was a ball of cork stuck on its small end, under which the leaf-brass being laid, and the tube rubbed and held near his feet, the ball attracted the leaf-brass to the height of 2 inches; and very vigorously repelled and attracted it for several times together.

April 21, Mr. Gray repeated the experiment on the boy; and now he attracted much stronger than at the first: the leaf-brass rose to his face at the height of more than 12 inches. He then gave the boy to hold in each hand the tops of 2 fishing rods, with a ball of cork on each of their small ends; then a small stand being set under each ball, with the leaf-brass on it, the tube being rubbed and held near his feet, both the corks attracted and repelled together strongly. The length of each of the poles was about 7 feet. Then the boy was laid on his left side, and a fishing rod, of near 12 feet in length, given him to hold with both his hands; there was a small ball of cork at the end of the rod, that was an inch and three quarters in diameter: then every thing being prepared, the tube held near the boy's feet, the cork ball attracted and repelled the leaf-brass forcibly to the height of at least 2 inches.

N. B. That when Mr. Gray speaks of holding the tube near the boy's feet, he means over against the soles of his feet; and when near his head, he means the crown of his head: for, when the tube is held above, or over his legs, the attraction is not so strongly communicated to the other parts of his body.

By these experiments it is seen that animals receive a greater quantity of electric effluvia; and that they may be conveyed from them several ways at the same time to considerable distances, wherever they meet with a passage proper for their conveyance, and there exert their attracting power.

In these experiments, besides the large stand abovementioned, Mr. Gray used two small ones, the description of which is as follows: their tops were 3 inches diameter; they were supported by a column of about a foot in height; their bases of about 4½ inches; they were turned of lignum vitæ; their tops and bases made to screw on for conveniency of carriage; on the tops was pasted white paper. When the leaf-brass is laid on any of these stands, he finds it is attracted to a much greater height than when laid on a table; and at least 3 times higher than when laid on the floor of a room.

June 20, 1730, Mr. Gray made the following experiment, showing that the attraction and repulsion is as strong, if not stronger; and that the effluvia may be carried to considerable lengths, without touching the line by the tube.—There was taken a line of packthread 231 feet in length; it was supported on 2 cross lines of blue silk, whose distance was near 18 feet: about 4 feet below one of these lines was put up another silk line of the same colour; to this was tied one end of the packthread; at the other end the ivory ball hung; the line was returned over the cross lines 13 times: then the leaf-brass being laid under the ball, on one of the small stands, and the tube excited, the ball attracted and repelled to the height of one of its diameters, which was about an inch and a quarter.

Mr. Gray found by several trials, that rubbing the tube, and putting it up between the returns of the line in several places, before he went with the tube to the end of the line, much facilitates, and causes the attraction much sooner, than when one stands with the tube, and applies it to the end of the line only.

August 1, 1730, at Mr. Wheler's was made the following experiment; being an attempt to see how far the electric virtue might be carried forward in a line, without touching the same.—This experiment was made by carrying the line out of the great parlour room into the garden, and down the great field before it. The line was supported by 15 pair of poles; each pair had a line of blue silk, tied from one pole to the other, the length about 4 feet, equal to the distance of the 2 poles: about 10 feet from the window there was a silk line put up across the room, by which that part of the line hung that had the ivory ball on it. Below the cross line of the farthest pair of poles, was placed another cross line, 4 feet from the ground; to which was fastened the other end of the communicating line, as mentioned in the experiment above. Then the leaf-brass and tube being prepared as usual; the tube being held over the line at several distances, beginning towards that end where the ball hung, and so proceeding towards the farther end of the line, the leaf-brass was attracted pretty strongly at the stations, not exceeding 2 or 300 feet; but became still weaker towards the farther end of the line: yet even at the end of it, the leaf-brass would be lifted by the ball, when the tube touched the line, whose length was 886 feet.

Coloured bodies, as Mr. Gray discovered in 1729, attract more or less, according to what colours they are of, though the substance be the same, and of equal weight and thickness: only that the red, orange, or yellow, attract at least 3 or 4 times stronger than green, blue, or purple: but he lately found out a new and more accurate method of making these experiments.

The General Quadrature of Hyperbolic Curves, defined by Trinomial Equations. By Samuel Klingenstiern, Professor of Mathematics in the Academy at Upsal, and F. R. S. N° 417, p. 45.

A paper now not of any use.

A remarkable Plica Polonica. By Abraham Vater, M.D. with an Account of the Cause of it. By Dr. Sprengell. N° 417, p. 50. *Abridged from the Latin.*

A country woman of Poland, who was married at the age of 15, was in her 18th year seized with the endemic disorder of that country, which from the plaiting of the hair is called the plica polonica.* She was afflicted with it for 50 years together, and during most of that time, she was confined to her bed by pains and contractions of the limbs and joints, which at length ended in marasmus. Worn out with age, she died in her 78th year. Dr. Floerke, physician to Prince Radzivil, not only visited this woman while she was living, and caused a drawing to be taken of her as represented in fig. 3, pl. 7, but also cut off the plica after her death, and brought it to Wittemberg. It was 4 ells long, 1 palm broad, and 2 inches thick; but it would (Dr. F. says) have been much longer, had not a great part of it been rubbed and rotted off during the long time the patient was confined to her bed.

The plica has always been supposed to be an infectious disorder; but Dr. Sprengell, from the best information he could procure, thinks it is owing to nastiness, from neglecting to comb the hair and wash the head:† for if it were really an infectious disorder, persons of the higher ranks would be as liable to it as the common people; whereas it is confined to these last. This opinion is confirmed by the following article in the Acta Breslav. for August 1724.

" The great number of people in Poland who are troubled with the plica, first made me reflect, whether it were a real distemper or not; and I am now convinced that their filthy mode of living, and the opinion entertained by the generality of the people that these plaited locks of hair cannot be cut off without danger of their lives, have contributed more to this complaint than any real indisposition of the body; for it is only the middling or poorer sort of people who are troubled with it, and whom it is impossible to contemplate without horror; but no Germans, of whom great numbers live in Poland, ever have

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* Termed by nosological writers trichoma.

† Joined to bad diet. It is stated in another part of the Transactions that this woman's only sustenance was coarse bread, raw herbs and water.

such a complaint. Several of them who are married to Polish women, are scarcely able to persuade their wives not to train up their children to this nastiness. Not long since I saw a fellow at church, who had about 70 such locks, hanging down, twisted hard like so many penny cords: so that one might easily have taken his for a Medusa's head; and it is probable, that, in ancient times, some such locks as these might have given rise to the poetical fiction of snakes growing on the head, instead of hair: be that as it may, it is certain that it is a a most odious sight."*

An unusual Agitation in the Magnetic Needle, observed to last for some time, in a Voyage from Maryland. By Captain Walter Hoxton. N° 417, p. 53.

On the 2d of September, 1724, a little after noon, being in latitude 41° 10′ N. and about 28° E. difference of longitude from Cape Henry in Virginia, the weather fair, a moderate gale and smooth sea, Capt. Hoxton's mate, who was on deck, came and told him, that the compass traversed so much that he could not possibly steer by it. On which the captain went up, and after trying it in several parts of the ship, found what the mate said to be true. The captain then had all his compasses brought up, and placed in different parts of the ship, and in places most remote from iron; and to his great surprise found them all in the same condition: so that they could not steer by any of them.

He then new touched some of them with a loadstone; and lest that should affect them, sent it out to the end of the bowsprit; but he did not perceive that the new touching was of any service: for they all continued traversing very swiftly, for about an hour after the captain came upon deck; and then on a sudden every one of them stood as well as usual. During the whole time the ship had very little motion: and there was an azimuth compass, and 4 or 5 others on board.

Of an Aurora Borealis seen in New-England, Oct. 22, 1730. By Mr. Isaac Greenwood. N° 417, p. 55.

The aurora borealis has been very frequent in New England of late; but none either for brightness, variety, or duration, so considerable as what occurred the 22d of October. This meteor has been observed in New England, at different times, ever since its first plantation; but it seems at much longer intervals

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* For further information respecting this extraordinary disease, the medical reader is referred to Rzaczinsky's Nat. Hist. of Poland, to Stabel's Dissertation inserted in the 1st Vol. of Haller's Disp. de Morb. Hist. et Curat. and to the 4th Vol. of the Manchester Memoirs. See also Coxe's Travels in Poland.

than of late years, and never to so great a degree as the present instance: nor indeed is there any recorded in the Phil. Trans. that seems to equal it; excepting only that celebrated one of the 6th of March, 1716, observed by Dr. Halley, and in many respects that also must yield to this.

The aurora continued till day-light. The day preceding this meteor was very warm for the season, though early in the morning there was a very considerable hoar-frost. The morning following was remarkable for an abundant dew. The temper of the air much the same as the preceding day. About 8 o'clock the heavens fair and calm. Barom. 30. 1. Therm. $\frac{38}{100}$.

Mr. Greenwood compared his observations with what he could find relating to the aurora borealis in the Phil. Trans. &c. and thinks that few particulars are mentioned there, but what occurred in this wonderful instance; some rare ones confirmed, and a few altogether new. He relates the circumstances very minutely, and at considerable length; but after so many descriptions in former papers, it is not necessary to be more particular.

An Account of the same Aurora Borealis, as seen at Annapolis in Maryland. By Mr. Richard Lewis. Communicated in a Letter to Mr. Peter Collinson, F.R.S. N° 418, p. 69.

We were entertained with a phenomenon on the 22d of October last; which, as it was never observed before in this quarter, was very surprising to most people.

About 6 at night the north part of the hemisphere appeared of a faint red, the horizon was very dusky, and this redness was bounded above by a very dark cloud.

As the night advanced, this meteor reddened, till it was of as deep a colour as blood; and it spread itself to the north east. It continued all night, but about 2 in the morning, it sent forth two or three streams from its north part, of a whitish colour, which shot up to the zenith. These emanations looked much like the rays of the sun, when they pass through a dark cloud, when it is said to be drawing water. It was taken to be an aurora borealis, but it appeared much fainter than those seen in England.

The Sequel of a Table of Magnetic Variations, collected from several Observations taken from the Year 1721 to 1729, in nine Voyages to Hudson's Bay in North America. By Captain C. Middleton.* N° 418, p. 71.

The following table shows the variation of the needle according to the annexed latitudes and longitudes; these being counted from the meridian of London.

lat.	long.	var.
50°	2°E	12°
49½	0	12
50	2 W	13
50	4	13
50	6	13
51	8	14
51	10	14
52	12	15
53	12	15
54	12	15
55	12	16
56	12	16
57	12	17
58	12	17
59	12	18
50	14	14
51	14	14
52	14	15
53	14	15
54	14	16
55	14	16
56	14	17
57	14	17
58	14	18
59	14	18
50	16	15
51	16	15
52	16	16
53	16	16
54	16	17
55	16	18
56	16	18
57	16	19
58	16	19
50	18	17
51	18	17
52	18	17
53	18	17
54	18	18
55	18	18
56	18	18
57	18	19
58	18	19
59	18	19
50	20	18
51	20	18
52	20	18
53°	20°	19°
54	20	19
55	20	19
56	20	19
57	20	19
58	20	20
59	20	21
50	22	19
51	22	19
52	22	19
53	22	20
54	22	20
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56	22	20
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52	28	22
53	28	23
54	28	23
55	28	23
56	28	23
57	28	24
58	28	24
59	28	24
50°	30°	23°
51	30	23
52	30	23
53	30	24
54	30	24
55	30	24
56	30	24
57	30	25
58	30	25
59	30	25
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50	40	28
51	40	28
52	40	28
53	40	29
54	40	29
55	40	29
56	40	29
57	40	30
58°	40°	30°
59	40	30
51	42	29
52	42	29
53	42	30
54	42	30
55	42	30
56	42	30
57	42	31
58	42	31
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61	50	35
58	52	34
59	52	34
60	52	34
61	52	35
62	52	35
58	54	34
59	54	35
60°	54°	36°
61	54	36
62	54	36
58	56	36
59	56	36
60	56	36
61	56	37
62	56	37
58	58	36
59	58	37
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60	66	40
61	66	41
62	66	43
59	68	40
60	68	43
61	68	44
62	68	47
60	70	43
61	70	44
62	70	47
61	72	42
62	72	43
63	72	48
62°	74°	41°
63	74	48
62	76	41
63	76	47
64	76	49
62	78	40
63	78	42
64	78	49
63	80	40
64	80	49
60	82	38
61	82	39
62	82	40
63	82	42
64	82	44
50	84	19
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61	84	30
62	84	30
55	86	22
56	86	23
57	86	24
58	86	25
59	86	26
60	86	27
56	88	22
57	88	23
58	88	24
59	88	25
60	88	26
57	90	21
58	90	22
59	90	23
60	90	24

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* See p. 136, of this volume.

Observations on the Weather, in a Voyage to Hudson's Bay in North America, in the Year 1730. *By Mr. Christopher Middleton.* N° 418, p. 76.

This is merely a sea journal, of the winds, weather, and variations; with the height of the barometer and thermometer.

An extraordinary Instance of the almost instantaneous freezing of Water. By Mr. Triewald, Director of Mechanics to the King of Sweden, and F. R. S. S. of England and Sweden. N° 418, p. 79.

December 15, 1730, Mr. Triewald coming into the hall where his apparatus is placed, the weather being very cold, he feared that the glass for showing the experiment with the Cartesian devils, or those glass figures in water, which by the pressure of the air on the surface of the water, are made to change their places, and sink to the bottom of the glass, would be in danger, if the water should freeze in them; he took it down from the shelf, and found the water in a fluid state; but before he could empty the glass, as some friends that were present had not seen that experiment, he placed his hand on the bladder tied on the top of this cylindrical glass, which was of a pretty large size, 16 inches high, and 3½ inches diameter, containing 3 glass figures: in that very instant, and in the space of a second of time, he found all the water changed into ice; when in that time 2 of the figures had reached very near the bottom, but the third, as well as they, fixed in the middle of the glass, surrounded with ice, as transparent as the water itself before it congealed. This is the matter of fact; but the reason why the whole body of water, in such a short space of time, should turn into ice, is, in his opinion, not easily to be accounted for.

An Account of Tulips, and other Bulbous Plants, flowering much sooner, when their Bulbs are placed on Bottles filled with Water, than when planted in the Ground. By the same. N° 418, p. 80.

In September 1730, Mr. Triewald placed some bulbs of tulips, and other flowers, in water in glasses, at the same time putting into each glass two grains of saltpetre. These glasses he kept in his study, sometimes on a shelf, at other times before the window. In a fortnight's time they began to strike new roots; the latter end of November they put forth leaves; and in January they all flowered; as well as if they had been on a garden-bed; whereas in gardens we seldom see, in this country, tulips before the latter end of May, and this year not so soon, the ground being yet covered with abundance of ice and snow.

Though these experiments seem to be calculated only for amusement, yet they have furnished him with some lights, as to the rise of the sap in plants.

Experiments relating to the same Subject. By Philip Miller, F. R. S. Gardener to the Company of Apothecaries, at their Botanic Garden in Chelsea. N° 418, p. 81.

The glasses, marked N° 1, were roots of a hyacinth, commonly known by the name of pulchra. N° 2 were roots of the common oriental blue hyacinth. The flowers of these were not so large as they are commonly produced when planted in a bed of earth; but this was occasioned by the bulbs dividing into several off-sets, each of which are as so many different small roots, sending forth stems and leaves. N° 3, was a bulb of a tulip, which though placed on the glass of water at the same time as the hyacinths, yet was not likely to flower in a month. N° 4, was a root of narcissus. This was also as backward as the tulip, though put on the water at the same time as the hyacinths. These roots were placed on the glasses the beginning of November, at which time Mr. M. put them into a green-house, where the air was kept constantly in a temperate warmth. The glasses were filled with common Thames water, so near to the top, that when the bulbs were placed on the glasses, it might be about a quarter of an inch below the bottom of the bulbs. Into those glasses marked N° 5, he put a small quantity of common garden mould, to try whether that would forward their flowering, or increase their strength; but he found that all the roots which were placed on those glasses, into which the earth was put, were at least a fortnight later than the others, before their fibres were emitted, and their progress has been since much slower. Also the water, in those glasses were the earth was put, did not waste above half so fast as it did in those glasses where there was none, which might be occasioned by the terrestrial matter mixing with the water, and so rendering it thicker, and less capable of being attracted by the plants, or evaporating by the heat. And from those glasses, where the bulbs did not exactly cover their neck, the water evaporated much faster, than from those where the bulbs entirely covered the tops of the glasses, so as to leave no vacuities round them.

In about a month after the roots were put on the glasses of water, they began to put out their fibres into the water; but they did not begin to put forth their leaves, till their fibres were extended all over the glasses, and were almost full grown. When their leaves began to appear, the buds of the hyacinth-flowers were soon visible, and in about 3 weeks were fully blown. The tulips and narcissuses being much backwarder than the hyacinths, as they always are when planted in a garden; these should always be placed on the glasses of water 6 weeks or 2 months earlier in the season than the hyacinths, when they are designed to flower at the same time; and the præcoces or early blowing tulips should always be chosen for this purpose.

By this method a person who has not a garden, may have some of these flowers growing in his chambers, where, if they are not kept too close from the air, or in a place too warm, they will flower almost as well as in a bed of earth, provided the roots are good, and are every year renewed; especially the tulips, because they every year form new bulbs, the old ones being always exhausted in nourishing the leaves and flowers, a new bulb is annually produced by the side of the flower-stem. The hyacinths flowered 2 years successively on glasses of water; but their flowers were very weak the 2d year. So that it is much the better way to have fresh roots every year.

Of 2 Women being poisoned by the Simple Distilled Water of Laurel-Leaves; and of several Experiments on Dogs; by which it appears that this Laurel-Water is one of the most dangerous Poisons hitherto known. By T. Madden, M. D. of Dublin. N° 418, p. 84.

A very extraordinary accident, that lately happened at Dublin, has discovered a most dangerous poison, which was never before known to be so, though it has been in frequent use among us; viz. the simple water, distilled from the leaves of the lauro-cerasus.* The water is at first of a milky colour; but the oil which comes over the helm with it, being in a good measure separated from the phlegm, by passing it through a flannel-bag, it becomes as clear as common water.

It has the smell of the bitter almond, or peach-kernel, and has been for many years in frequent use among our housewives and cooks, to give that agreeable flavour to their creams and puddings. It has also been much in use among drinkers of drams; and the proportion they generally use it in, has been one part of laurel-water to 4 parts of brandy.

Nor has this practice, however frequent, ever been attended with any apparent ill consequences, till some time in Sept. 1728, when one Martha Boyse, a servant, who lived with a person that sold great quantities of this water, got a bottle of it from her mistress, and gave it to her mother Anne Boyse as a very rich cordial.

Anne Boyse made a present of it to Frances Eaton her sister, who was a

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* Prunus lauro-cerasus Linn. Cherry laurel.

shopkeeper in the town, and who, she thought, might oblige her customers with it. Accordingly in a few days she gave about 2 oz. of the water to one Mary Whaley, who had bought some goods of her.

Mary Whaley drank about 2 thirds of what was filled out, and went away. Frances Easton drank the rest. Mary Whaley went to another shop to buy something else, and in about $\frac{1}{4}$ of an hour after she had drank the water, she complained of a violent disorder in her stomach. She was carried home, and from that time she lost her speech, and died in about an hour, without vomiting, or purging, or any convulsion.

The shopkeeper, Frances Eaton, sent notice to her sister Ann Boyse of what had happened, who came immediately to her, and affirmed, that it was not possible the cordial (as she called it) could have occasioned the death of the woman; and to convince her of it, she filled out about 3 spoonfuls, and drank it. She continued talking with Frances Eaton about 2 minutes longer, and was so earnest to persuade her of the liquor being inoffensive, that she filled out 2 spoonfuls more, and drank it off likewise. She was hardly well seated in her chair, when she died, without the least groan or convulsion.

Frances Eaton, who, as was before observed, had drank somewhat above a spoonful, found no disorder in her stomach, or elsewhere; but to prevent any ill consequence, she took a vomit immediately, and has been well ever since.

Mary Whaley was buried without being examined by any one, except the coroner. The Dr. went to see Ann Boyse about 24 hours after her death, but could not prevail to have her opened. She was about 60 years of age; her countenance and skin appeared well coloured, and her features were hardly altered, so that she looked as one asleep. Her belly was not swelled, nor had she any other external mark of poison.

This accident brought into discourse another of the like nature, which happened about 4 years before in the town of Kilkenny. A young gentleman, son to Mr. —— Evans, an alderman of the town, mistook a bottle of this laurel-water for a bottle of ptisan. What quantity he drank is uncertain, but he died in a few minutes, complaining of a violent disorder in his stomach. This affair was not much regarded at that time, because he laboured under a distemper, to which, or to an improper use of remedies, his death was attributed by those about him.

The Dr. to satisfy himself further as to the effects of this poison, made some experiments, in conjunction with some of his friends, an account of which follows:

Exper. 1.—October 3, 1728, they gave a large setting-dog 3 oz. of laurel-water by the mouth. In 3 minutes he began to be strongly convulsed. His convulsions continued about 5 minutes; after which being untied, he then fell into a most violent difficulty of breathing, which lasted about 8 minutes, and abated gradually; on which he endeavoured to raise himself, but could not. The Dr. tied him down once again, and gave him 1½ oz. more; on which he sunk at once, and without any return of his convulsions, or difficulty of breathing, he expired in 2 minutes.

On opening the stomach, they found in it the whole quantity of water which he had taken; its surface was covered with froth, but it was not otherwise altered in its colour, consistence, or smell. The inside of the stomach was not at all inflamed, nor was there any visible alteration in the tunica villosa. The veins of the stomach, all the mesaraic veins, and likewise the vena cava, were much distended with blood; but the arteries, on the contrary, were remarkably empty. The liver and gall-bladder were not altered. The kidneys were unusually full of blood, and appeared of a bluish colour, almost as deep as that of the violet plum. On making an incision into one of the kidneys, the blood flowed in much greater plenty, and was more fluid than usual. In the heart there appeared nothing preternatural, nor in the brain.

Exper. 2.—October 24, Dr. M. gave 1½ oz. of the same water to a bitch of a smaller size. She was immediately let loose, and in 2 minutes she lost the use of her limbs. She attempted several times to raise herself, and walk; but she staggered and reeled about, and then fell down. She repeated this without ceasing about 5 or 6 minutes. At last she was violently convulsed, especially in the muscles that extend the head and spine. About the space of a minute she had that sort of convulsion called the opisthotonos, the back of her head being drawn almost to her tail.

After this she vomited plentifully, and her convulsions ceased. She then lay still for 7 or 8 minutes, labouring for breath, though not so violently as in the former case, and foaming at the mouth. Dr. M. gave her 1 oz. more of the water; on which her difficulty of breathing increased, and she died in 2 minutes.

On opening the abdomen, the thorax and the head, every thing was found in the same state as in the former instance.

Exper. 3.—October 25, Dr. M. gave 2 oz. of the water to a dog of the same size with the former; which produced the like appearances as in the foregoing case. This dog was half an hour a dying; for the dose was not repeated, because he did not vomit up what he had taken. On opening him, every thing was found in the same state as in the former instance.

Exper. 4.—October 26, Dr. M. gave 2½ drams of the water to a dog of a middle size; and immediately untied him. He then ran about the room very briskly for about a minute, and seemed to be no way affected with it; yet he soon lost the use of his limbs. He often attempted to raise himself, and walk, but still fell down again before he had moved 2 yards from the place.

After this he vomited plentifully, considering that he had fasted 24 hours; on which he was seized with a convulsion more violent than any of the former dogs, especially in the muscles that extend the head and spine. These convulsions continued about 8 or 10 minutes; on their ceasing, he lay still, breathing deeply, though regularly, and seemed to be asleep. In about 10 minutes he raised himself, took some food, and walked about tolerably well. Dr. M. left him, and returning after 3 hours, he found him perfectly recovered.

Exper. 5.—Oct. 28, Dr. M. injected 1 oz. of the water into the intestinum rectum of a strong spaniel dog, and let him loose. In 2 minutes he began to lose the use of his limbs, and to stagger as the others had done. He was convulsed more violently than any of the rest, and chiefly in the muscles of the neck and spine. The muscles of his eyes were strongly convulsed, which appearance was not observed in the other dogs. He foamed at the mouth, yelled frequently, and breathed with more difficulty than any of the rest. His convulsions continued 20 minutes; on their ceasing he lay quiet, as though he slept, only that his eyes were open. His limbs were now grown perfectly paralytic.

We raised him up several times, and offered to set him on his legs, but he did not attempt to use them. He continued in this way about 15 minutes longer, and then was seized with another violent convulsion, which in 5 minutes put an end to his life.

On opening the abdomen, the veins of the stomach and guts were found very much distended with blood, as in all the former instances. In the heart, lungs and brain, there was no visible alteration.

Exper. 6.—Oct. 30, Dr. M. injected 1½ oz. of the water, diluted with 3 oz. of common water warmed, into the anus of a small bitch. Before he could untie her she was seized with convulsions, and yelled much. She fell as soon as she was loosed, and never after endeavoured to rise. She had convulsions, and great difficulty of breathing about 2 minutes. She then lay still, with her limbs stiff and extended, about 3 minutes; during which time her lower jaw was convulsed, and pulled alternately to and from the upper jaw, with a very quick motion.

After this her limbs became paralytic, and she gasped for breath about 2 minutes longer. She was quite dead in 7 or 8 minutes after the injection of the clyster.

In the abdomen, thorax and brain every thing appeared as usual.

Exper. 7.—Nov. 2, Dr. M. injected ½ oz. of the water, diluted with 3 oz. of common water warmed, into the anus of a small bitch. In the space of 4 minutes she began to breathe with difficulty. He let her loose, but she was not able to stand, or walk without stumbling. The muscles that extend the head were convulsed, and her fore-legs were affected for 3 or 4 minutes with a tetanus, but had no convulsive motion. She vomited and purged plentifully. She did not yell, nor seem to suffer much pain, nor did she lose her senses all the time. In $\frac{1}{2}$ an hour she recovered.

Exper. 8.—The next day he injected a drachm of the water into the external jugular of the same bitch. She was seized with convulsions as violent as the former, before he could untie her. They lasted about 5 minutes; after which she recovered gradually, and continued well.

Exper. 9.—Nov. 20, he injected 4 oz. of the water by the anus, without any dilution, into a strong dog of a middling size. He was seized with convulsions and difficulty of breathing, in less than 2 minutes after the injection. He fell to the ground as soon as his convulsions began, and never once attempted to rise; nor were his convulsions in any sort so violent, neither did they continue so long as in the former instances. He bled at the nose about 4 spoonfuls. The blood was of a very bright florid colour. His convulsions lasted about 4 minutes; after which he became entirely paralytic, and died in 3 minutes more.

Dr. M. found the stomach, intestines, liver, &c. in the same state as those abovementioned. On cutting about an inch from the lower part of one of the lobes of the lungs, the blood flowed from it in great plenty, and appeared more florid and fluid than usual.

Exper. 10—Dec. 14, he gave 5 oz. of laurel-water by clyster to a dog, somewhat of the size and shape of the Italian greyhound. He seemed at first to be no way affected with it, but in about 5 minutes he began to droop, and lose the use of his limbs. He did not once yell, or struggle as the others had done, but sunk gradually, till he became at last entirely paralytic. He had no convulsion, except a kind of spasmus cynicus, a few minutes before he died, which happened in $\frac{1}{2}$ an hour after the injection of the clyster.

On opening the abdomen, Dr. M. found the veins much distended with blood, as were also the veins and sinuses of the brain.

Exper. 11.—Dec. 19, he gave 3 oz. of the water in the same manner to a cur of the lap-dog size. He died in 7 minutes, without any convulsion, except a tetanus in the muscles that extend the head.

The lauro cerasus being an ever-green, and abounding with a warm essential oil, Dr. M. imagined that other ever-greens might partake of the same poisonous quality. Accordingly he made trial of a water distilled in an alembic from the leaves of the yew-tree, so much noticed by the ancients, and whose very shade they supposed to be fatal to those who sat or slept under it.

Exper. 12.—He gave 3 oz. of this water by clyster to a very small cur dog, but he was not in the least affected with it

Exper. 13.—He also gave, by the mouth, 2 oz. of a water, distilled from the leaves of the bay-tree, to a young spaniel, without any effect.

Exper. 14.—He afterwards made an experiment with the distilled water of box-leaves, which had a very strong narcotic smell. He injected 5 oz. of this water, by the anus, into a small cur dog; but he was no way affected with it, though he kept him 12 hours after the operation.

Exper. 15.—Being desirous to know whether the virulency of laurel-water was owing to the fire in distillation, Dr. M. poured warm water on some laurel-leaves bruised, and made a strong infusion of them. He poured 1 oz. of it down a dog's throat, half of which was supposed to enter the stomach, and 5 minutes after another oz. was given in like manner. The dog seemed to be somewhat sick at his stomach, but was soon as lively as ever. A few minutes after this another oz. was given to him by the mouth, of which he supposes $\frac{1}{4}$ part to have been lost. He soon after stared, and trembled very much. In 5 minutes another oz. was exhibited; on which he trembled as before, but in a little time he appeared easy and lively.

Imagining that these small quantities lost their power, during the intervals of giving them, in 10 minutes after his taking the former dose, he poured down his throat $2\frac{1}{2}$ oz. at once. He immediately tumbled on his back convulsed, and tumbled over 3 or 4 times, but quickly returned to his feet. He staggered, his eyes stared, and he sat down like a dog that is tired. At length he shut his eyes, his neck became extended, and it was apprehended he was falling into convulsions; but instead of it, he vomited a vast quantity of indigested chyle, in which appeared a great portion of the infusion; after which he seemed to be perfectly recovered.

Exper. 16.—In about 25 minutes after this, Dr. M. gave the same dog by the mouth 2 oz. of the juice expressed from laurel-leaves, and in about 10 minutes more another oz. was given him in the same manner. In a few minutes he began to lose the use of his hinder legs, but he quickly recovered them. On his taking another oz. soon after the former, he fell into a great difficulty of breathing, and yelled much. After this he was seized with very strong convulsions, which affected his lower jaw and hinder legs most remarkably.

In about the space of 5 minutes these convulsions were succeeded by an entire resolution of all the limbs. He breathed with great difficulty, and very slowly. No appearance of expiration. Sometimes Dr. M. observed two attempts at inspiration without intermission, or closing of the mouth. At other times there was near the space of a minute between two inspirations. After this he was seized with a trembling in his limbs, and in about $\frac{3}{4}$ of an hour from his taking the last oz. he died without any struggling, with his tail extended.

There were several other experiments made of the same kind, by some gentlemen of the profession here, which corresponded exactly with the foregoing, excepting this one circumstance, that they were of opinion, that this poison occasioned an inflammation in the stomach and guts.

Towards clearing this dispute, Dr. M. who thought otherwise, put together the following hints, from which it appears that the fact is not as they imagined, and that though we find on an animal being killed by this poison, the veins greatly distended with blood, yet there is not any inflammation produced by it.

Perhaps nothing can illustrate this matter better, than the analogy which may be observed between the convulsions occasioned by the epilepsy, and those which are the effect of laurel-water. For instance, in the epilepsy, the body is universally convulsed, especially the muscles of the neck, the tongue, the lower jaw, and those of the arms.

The effect of these convulsions is this: the heart beats with unusual violence and frequency; the necessary consequence of which is, that the blood will be thrown in greater plenty from the arteries into the veins. But because the muscles compress the veins more than the arteries, their systole enabling them to overcome that pressure; therefore the blood, which is still pushed forward by the systole of the heart into the veins, will be retained there by the said pressure of the muscles, and will return in a very small quantity to the heart.

For instance, the abdominal muscles being convulsed, press the stomach and intestines on the vena cava ascendens, and likewise on the vena portæ; by which means the blood, returning from the lower extremities, is retained in those vessels. Accordingly we see the visible and immediate effects of this pressure are the forcing out the contents of the bladder and intestines, and very frequently the profluvium seminis.

In like manner, the pressure of the muscles of the neck, tongue, and lower jaw, on the jugular veins and their branches, will not suffer the blood to return to the heart by the vena cava descendens. To this we may add the pressure of the diaphragm and ribs on the lungs; by which means the trunks of the vena cava ascendens and descendens are compressed at their insertion into the heart. Hence follows that frightful blackness of the face during the paroxysm, and the prodigious swelling of the veins of the head, especially the temporal.

The necessary consequence of all this must be, that if the convulsion lasts long enough, the man must die, on account of the blood being thrown out of the arteries into the veins, and not returning to the heart. And doubtless if such a person was opened after death, we should find the vena cava, the vena portæ, the veins and sinuses of the brain, with all their smallest ramifications, very much distended with blood, and the arteries on the contrary almost empty.

But if the epileptic convulsion ceases before the circulation of the blood is entirely stopped, then all becomes calm again; the pressure is taken off the veins, the blood returns to its usual course, and in a few hours the sick person is perfectly recovered.

And yet all this violent convulsion of the body, this prodigious distension of the veins, and interception of the course of the blood, passes without any inflammation; as appears from the speedy recovery of the sick person: for if the convulsion had occasioned an inflammation, a fever must necessarily have ensued; which would discover itself by manifest tokens, and would require a much longer time for its abatement.

Let us now observe the analogy between these appearances, and those produced by laurel-water.

We find by experiment, that 1 oz. or even 2½ drs. of laurel-water, will occasion more violent convulsions than 3 oz. or even 5 of it. Exper. 4, 5 to 11. If therefore an inflammation was the necessary consequence of this water being taken into the stomach or guts, the more violent the convulsion is, the greater the inflammation ought to be. On the contrary, we find that the more violent the convulsion is, the greater is the probability that the creature will recover. Exper. 4 to 7. And when it so happens, the manner is exactly the same as in the recovery of an epileptic person. In a few minutes the creature becomes as brisk as if no such thing had happened.

Now if an inflammation was at all the necessary consequence of this poison, though the creature recovers, yet there must be some inflammation, greater or less, produced, which must occasion more violent and lasting symptoms. But since none such appear; since the recovery is so sudden and effectual; it is the strongest and plainest argument, that there is not any inflammation produced.

If the laurel-water be administered to the quantity of 1 oz. or more, the creature unavoidably dies in a few minutes; and on opening him, the appearances are these. Both the trunks of the vena cava, and all the ramifications of the meseraic veins are greatly distended with blood. These vessels are easily distinguished from the arteries, not only by the thinness of their coats, but also by the colour which the blood exhibits to the eye. Now the Doctor conceives that all inflammations have their beginning in the arteries, and that they are produced, because there is no free passage for the blood into the veins. But if once this passage become free (as in this case it surely is, for we find all the veins distended with blood beyond their natural dimensions) the inflammation is then at an end, the cause which produced it being taken off.

Also, the fact laid down, that the veins are preternaturally distended with blood, necessarily concludes, that the arteries are not distended with it, and consequently that there cannot be any inflammation; for if the quantity of blood be increased in the veins, it must be proportionably diminished in the arteries.

To what has been said, may be added the following observation; viz. that if there was any inflammation produced by this poison, it ought to appear most remarkably on the inside of the stomach and intestines, because of the immediate contact it has with those parts. All other poisons which occasion inflammations in the stomach and guts, first act on the blood-vessels, and corrode the parts inflamed. They occasion vomitings and fluxes of blood, which at length terminate in convulsions.

One may very easily be deceived upon opening the stomach of a dog, and may mistake the redness of the tunica villosa for an inflammation. The inner coat of a dog's stomach is naturally of a ruddy flesh-colour; and therefore, of all domestic animals, a dog has the quickest and strongest digestion. Accordingly we see that they swallow bones, and digest them perfectly well; and though they are but half chewed when taken into the stomach, yet they are at last reduced to as soft a consistence as any other part of their aliment. It is for this reason therefore, that the stomachs of dogs are more plentifully supplied with blood than those of other animals; by which means, not only the muscular force of the stomach, but its warmth also, which is the principal instrument of digestion, is very much increased.

Dr. John Rutty of Dublin, informed the Editor, that whereas there were several other experiments made at the same time by other gentlemen, as Dr. Madden mentions, which agreed with these. Some persons who were present at them proposed several things to be tried as antidotes to this poison; accordingly bole, vinegar, and milk were given to a dog which had swallowed some of the laurel-water: the bole and vinegar were not observed to do much good, but the dog which drank the milk recovered without any bad symptoms; but at that distance of time the Doctor could not recollect the proportions that were given: he thinks a pint of milk. The editor tried several experiments in Essex and in London, which correspond with, and confirm the above-related. Of these experiments, an account will be found in the present volume of this Trans.

An Account of Mr. James Christopher Le Blon's Principles of Printing, in Imitation of Painting, and of Weaving Tapestry, in the same manner as Brocades. By Cromwell Mortimer, M. D., S. R. Secret. N° 419, p. 101.

Mr. Le Blon, endeavouring to fix the true harmony of colouring in painting, found that all visible objects may be represented by the 3 primitive colours, red, yellow, and blue; for out of them, all others, even black itself, may be compounded. We are beholden to the great Sir Isaac Newton for the discovery of the difference of colours contained in the rays of the sun; and that the union of them all produces a white, which is light itself.

For distinction sake, Mr. Le Blon calls those colours which are comprehended in the rays of the sun, impalpable colours, and those used in painting, material colours. In the material colours, a mixture of all three produces a black or darkness, contrary to what is observed in the impalpable, which as above said produces white. Mr. Le Blon takes this phenomenon to be owing to the body or substance of which these three material colours consist, and to their particles being opaque, and not transparent; for they only reflect certain rays of light, that strike on their surfaces; and therefore, when small particles of different colours are placed close together, if they are so small that each of them cannot be seen separately by the eye, we do not discern the colour of each particular atom, but only the blended reflected rays, proceeding from the adjoining particles: thus yellow and red produce an orange; yellow and blue a green, &c. which seems to be confirmed by placing two pieces of silk near together; viz. yellow and blue; when by intermixing of their reflected rays, the yellow will appear of a light green, and the blue of a dark green; which deserves the farther consideration of the curious.

M. Le Blon has reduced the harmony of colouring in painting to certain infallible rules, built on this foundation: whereas, according to the common practice of painters, their colouring is the effect of mere chance or guess-work at first, but improved by experience; all painters usually declaring that there can be no certain rules given for mixing colours. Mr. Le Blon published some years ago an ingenious book on this subject, entitled, Coloritto; or, the harmony of colouring in painting.

By these rules he discovered the manner of printing any object in its natural colours, by the means of three plates, and the three primitive colours; an art attempted and sought after ever since the invention of printing, but in vain, and thought impossible, till he put it in practice about 15 years since.

The plates are engraved chiefly after the mezzotinto manner; only the darker shades, and sometimes the out-lines, where they are to appear very sharp, are done with a common graver. Each plate is not completely engraved, but only contrived to take such a portion of the colour as is necessary with the other two plates, to make the picture complete.

This art of printing consists in 6 articles; viz. 1. To produce any object with 3 colours, and 3 plates. 2. To make the drawings on each of the 3 plates, so that they may exactly tally. 3. To engrave the 3 plates, so that they cannot fail to agree. 4. To engrave the 3 plates in an uncommon way, so as that they may produce 3000 and more good prints. 5. To find the 3 true primitive material colours, and to prepare them, so as that they may be imprimable, durable, and beautiful. 6. To print the 3 plates, so as that they may agree perfectly in the impression.

The first of these is the most considerable, comprehending the theoretical part of the invention; and the other 5 are subservient to bring it into mechanical practice; and of such importance, that if any one of them be wanting, nothing can be executed with success or exactness. Sometimes more than the 3 plates may be employed; viz. when beauty, cheapness, and expedition require it.

The observation of the compounded colours reflected from two pieces of silk, of different colours, placed near together, first gave him the thought of what the effect of weaving threads of different colours would be, when all the threads were so fine as not to be distinguished from each other at a small distance.

By the same principles of producing any visible object with a small number of colours, he arrived at the skill of producing in the loom all that the art of painting requires. An art likewise often attempted, but as often abandoned, and declared impossible till now, as well as the other of printing in colours. And probably many improvements may from hence be made in several trades, especially in combing of wool, where the mixing of several colours may be of great use; but he has not yet had time to apply it to any thing else besides painting, printing, and weaving.

The colours used in weaving being only superficial, and so differing from both the impalpable and the material colours, and not being to be so closely joined or incorporated together as those, will not of themselves produce a white or black, but only a light cinnamon: therefore, in weaving he has been obliged to make use of white and black threads, besides red, yellow and blue; and hough he found he was able to imitate any picture with these 5 colours; yet for cheapness and expedition, and to add a brightness where it was required, he found it more convenient to make use of several intermediate degrees of colours.

There are two ways in use at Brussels, and at the goblins in Paris, for making tapestry after the common manner: one they call the flat way, and the other the upright. In the flat way they have the warp stretched in a frame lengthwise of the piece: it is made of white worsted, and the pattern lies close under it; so that the workman can see the figures through the warp: he is provided with bobbins of various colours of silk or worsted, as the piece requires: then he takes up with his fingers one thread after another, as they answer to any colour in the printing beneath; and with the other hand passes the bobbin with the same colour, and strikes the threads close with an ivory comb. Some of these frames are made like a loom, with a warp passed through the leishes, and tredles for the feet, with which they open the threads of the warp, to pass a common shuttle through them, when it is necessary to make a long throw, as is required in grounds, pillars, and tall uprights.

In the upright way, the warp runs from top to bottom of the piece; the pattern is placed upright, and close behind it, and the out-lines are drawn in charcoal upon the foreside of the warp. The workman is placed with his back to the light, by which means he can see the pattern better; then he takes up the threads one by one, and passes the bobbin, as in the other, and strikes it close with the comb: all which is near as tedious as needlework itself; which is the reason why fine tapestry comes to such high prices; and what can be had at a moderate price is always coarse, and of a low taste: for workmen who have any good notion of painting, and are capable of adjusting the colours, are not to be had, but for excessive wages; which much enhances the price likewise: but in Mr. Le Blon's new way of weaving tapestry in the loom with a draw-boy, tapestry may be performed almost as expeditious as fine brocades: for when the loom is once set and mounted, any common draft-weaver, though not acquainted with drawing nor painting, nay hardly knowing what figure he is about, exactly produces what the painter has represented in the original pattern: and thus a piece of tapestry may be woven in a month or two, which, in the common way of working, would take up several years: and what in the common way costs 1000 pounds, may, by this means, be afforded finer and better for 100 pounds. Therefore it is likely this woven tapestry may become a currant merchandise; and that many thousand industrious families may be well employed about it.

The main secret of this art consists in drawing the patterns, from which any common draft-weaver can mount the loom; and when that is done, the piece may be made of any size, by only widening the reeds and the warp; and a reverse may be made with the same ease; which is done by the boy's pulling the leishes up again in the same order in which he pulled them down before; by which contrivance the tapestry may be suited to any room, whether the light comes in on the right hand, or on the left.

The patterns are painted on paper, on which are printed squares from copper plates, and these subdivided by as many lines as answer to the threads of the warp, which run lengthwise of the piece: then they try how many threads of the shoot answer in breadth to every subdivision of the squares. Every thread of the warp goes through a small brass ring, called a male, or through a loop in the leish, and has a small long weight or lingoe hung below, to counterbalance the packthreads, which going from the top of the rings or loops, are passed over the pulleys in the table directly over the loom, and are continued nearly in a horizontal position on one side of the loom, to a convenient distance; where they are all spread on a cross-piece fastened to two staples: these are called the tail of the mounture; and from each of these packthreads, just by the side of the loom, are fastened other packthreads called simples, which descend to the ground; so that by pulling these simple chords, you raise any of the threads of the warp at pleasure; therefore they fasten a loop or potlart to as many of these simple chords as there are threads of the warp to be pulled up at every shoot, or every throw of the shuttle; by which means the shoot shows itself on the right side, where the warp is pulled up: and in ordering this, they are guided by the pattern, on which they count the distances of the subdivisions, which contain the same colours in the same line, and can be shot at once: then they fasten potlarts to the several simple chords, that draw up the rings, through which those threads of the warp run, which are to lie behind this colour; they tie all these loops together, and fasten a piece of worsted or silk to the knot, of the same colour that the workman is to throw; and the boy, when he pulls each loop, names the colour, that the weaver may take the proper shuttle; and so on for every colour to be thrown.

An Account of the Condition of the Town of Hastings, after it had been visited by the Small-pox. By Mr. T. Frewen. N° 419, p. 108.

The following is a true account of the present condition of the town of Hastings, after having been visited with the small-pox; which raged there about a year and half. An account of those inoculated should have been here inserted, if any thing remarkable had happened: they all had the distemper very favourably, and continue perfectly well.

The number of those that recovered of the small-pox, including 4 that were inoculated, 608; died of it 97; escaped it 206; died of other illnesses since the small-pox raged there 50; the whole number of inhabitants in that town are 1636; males 782, females 854.

N. B. There is at present no small-pox in that town.

A Catalogue of Eclipses of the four Satellites of Jupiter, for the Year 1732. *By James Hodgson,** *F. R. S. and Master of the Royal Mathematical School at Christ's Hospital, London.* N° 419, p. 109.

The great number of eclipses that happen in a year, amounting to 352, though the 4th satellite will pass wide of the shadow, after the middle of January next; the ease with which they are observed, especially since the great improvement made in the reflecting telescope by Mr. Hadley; the small skill required in making the observations, since the difference of times, when observed by the largest glasses, and the smallest through which they may be seen, amounts to scarcely one quarter of a minute; render these observations the most proper of any that the heavens afford us, at present, for determining the longitude of places; and it may be truly asserted, that there are very few places of note on the surface of the habitable globe, whose longitudes are already known, that have not either been absolutely determined, or at least, have been rectified and confirmed by them.

These predictions of eclipses for the year 1732 cannot be of any use now.

An Account of a Polypus, resembling a Branch of the Pulmonary Vein, coughed up by an Asthmatic Person. By Frank Nicholls, M. D. F. R. S. N° 419, p. 123.

Nicholas Tulpius, in observ. 7, book 2, gives the case of a man who, with a large effusion of blood, threw up, by coughing, two branches of the pulmo-

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* Mr. James Hodgson was a useful writer and teacher in the elementary and practical parts of science. He was the relation of Mr. Flamsteed, by whom he was educated, and by whom he was appointed one of his executors, for publishing his tables and observations; having indeed been very useful to him in his life-time, by making numerous calculations for him, particularly in computing the latitudes and longitudes of all the stars in the British catalogue, amounting to more than 3000. Besides numerous communications to the Philos. Trans. from vol. 24 to vol. 49, he published several other books: as, 1. A System of the Mathematics, in 2 vols. 4to. 1723. 2. The Theory of Jupiter's Satellites, 4to. 1750. 3. The Valuation of Annuities on Lives, in 4to. 4. An Introduction to Chronology. 5. The Doctrine of Fluxions, 4to. 1758.

I find no account of the life or death of Mr. Hodgson. But by the date of the first of the above books, it appears that in 1723 he was F. R. S. and mathematical master to Christ's Hospital; and that he died some time between the years 1755 and 1758; as the last of his papers in the Philos. Trans. relates to an event in the former of these limits; and in the title of his last work, on Fluxions, in 1758, he is spoken of as being then dead.

nary vein, 6 inches long, with their several ramifications, freed from the trachea and substance of the lungs, as if very accurately dissected. This case he observes is very extraordinary, and not to be paralleled in the writings of physical authors.

A small acquaintance with the structure of the lungs sufficiently evinces the impossibility of the fact, as there stated. Therefore, not doubting the veracity of the author, Dr. N. always believed him to be deceived by a polypus of the vein, which might be coughed up in the manner described.

But the following case will put this matter in another light. July 18, 1730, Dr. N. was consulted on behalf of a person, living in Essex, who was asthmatic, and coughed up phlegm resembling worms: to remedy which, he directed the use of a lac ammoniacum with squills; by means of which he expectorated more easily, but continued still to cough up the same substances.

July 11, 1731, on the road to London, the patient was seized with a shivering, and pleuritic pains; his tongue was white, pulse hard and quick, &c. By repeated bleeding his pains decreased, but the cough remained more violent than usual. On examining the expectorated phlegm, which was tinged with blood, it was fibrous, and when expanded in water, exactly resembling the vessels in the lungs. These substances are as tough as the coats of the veins, and like them hollow. The patient coughed up more or less of them every day, for 7 years; sometimes perfectly white, and sometimes tinged with blood; yet he has had no other complaint: has had a good appetite and colour, and a greater share of fat than any man would choose. The specimen represented fig. 1, pl. 12, was expectorated when the Doctor was present, July 16, 1731. It nearly resembles the first draft by Tulpius, and is no more than a viscid phlegm, secreted by the relaxed glands of the trachea, and afterwards concreted by the heat of the part.

An Experiment explaining a Mechanical Paradox, viz. that two Bodies of equal Weight suspended on a certain kind of Balance, do not lose their Equilibrium, by being removed, the one farther from, the other nearer to the Centre. By the Rev. T. J. Desaguliers, LL.D. et F.R.S. N° 419, p. 125.

PROP.—If the two weights P, W, in fig. 2, pl. 12, hang at the ends of the balance AB, whose centre of motion is C; those weights will act against each other, because their directions are contrary, with forces made up of the quantity of matter in each multiplied by its velocity; that is, by the velocity which the motion of the balance turning about C will give to the body suspended. Now the velocity of a heavy body is its perpendicular ascent or descent, as will appear by moving the balance into the position ab; which shows the velocity of P to be the perpendicular line ea, and the velocity of B will be the perpendicular line bg: for if the weights P and W are equal, and also the lines ea and bg, their momenta made up of ea multiplied into W, and bg multiplied into P, will be equal, as will appear, by their destroying each other in making an equilibrium. But if the body W was removed to M, and suspended at the point D, then its velocity being only fd, it would be over-balanced by the body P; because fd multiplied into M, would produce a less momentum than P multiplied into bg.

As the arcs Aa, Bb, and Dd, described by the ends of the balance, or points of suspension, are proportionable to their sines ea, gb, and df, as also the radii or distances CA, CB, and CD; in the case of this common sort of balance, the arcs described by the weights, or their points of suspension, or the distances from the centre, may be taken for the velocities of the weights hanging at A, B, or D; and therefore the acting force of the weights will be reciprocally as their distances from the centre.

SCHOLIUM.—The distances from the centre are taken here for the velocities of the bodies, only because they are proportional to the lines ea, bg, and fd, which are the true velocities. For there are a great many cases in which the velocities are neither proportionable to the distances from the centre of motion of a machine, nor to the arcs described by the weights or their points of suspension. Therefore it is not a general rule, that weights act in proportion to their distances from the centre of motion; but a corollary of the general rule, that weights act in proportion to their true velocities, which is only true in some cases. Therefore we must not take this case as a principle, which most workmen do, and all those people who attempt to find the perpetual motion, as is most amply shown in the Philos. Trans. N° 369.

But to make this evident even in the balance, we need only take notice of the following experiment, fig. 3; where ACBEKD is a balance, in the form of a parallelogram, passing through a slit in the upright piece NO, standing on the pedestal M, so as to be moveable on the centre pins C and K. To the upright pieces AD and BE of this balance are fixed, at right angles, the horizontal pieces FG and HI. That the equal weights P, W, must keep each other in equilibrio, is evident; but it does not at first appear so plainly, that if W be removed to V, being suspended at 6, yet it shall still keep P in equilibrio; though the experiment shows it. Nay, if W be successively moved to any of the points 1, 2, 3, E, 4, 5, or 6, the equilibrium will be continued; or if, W hanging at any of those points, P be successively moved to D, or any of the points of suspension on the cross piece FG, P will at any of those places make an equilibrium with

W. Now when the weights are at P and V, if the least weight that is capable to overcome the friction at the points of suspension, C and K, be added to V, as U, the weight V will overpower, and that as much at V as if it was at W.

From what has been said above, the reason of this experiment will be very plain. As the lines AC and KD, CB and KE, always continue of the same length in any position of the machine, the pieces AD and BE will always continue parallel to each other, and perpendicular to the horizon; however, the whole machine turns on the points C and K, as appears by bringing the balance to any other position, as abed; and therefore as the weights applied to any part of the pieces FG and HI, can only bring down the pieces AD and BE perpendicularly, in the same manner as if they were applied to the hooks D and E, or to X and Y, the centres of gravity of AD and BE; the force of the weights, if their quantity of matter is equal, will be equal; because their velocities will be their perpendicular ascent or descent, which will always be as the equal lines 4 l and 4 L, whatever part of the pieces FG and HI the weights are applied to. But if to the weight at V be added the little weight U, those two weights will overpower, because in this case the momentum is made up of the sum of V and U multiplied by the common velocity 4 L.

Hence it follows, that it is not the distance C 6 multiplied into the weight V, which makes its momentum, but its perpendicular velocity L 4 multiplied into its mass. Q. E. D.

This is still further evident, by taking out the pin at K: for then the weight P will over-balance the other weight at V, because then their perpendicular ascent or descent will not be equal.

An Account of a very profuse Vomiting of Blood, cured by the coldest Drinks in the Winter-time. Communicated to the Royal Society by Peter Antony Michellotti, M. D. and F. R. S. N° 419, p. 129. *An Abstract from the Latin.*

In this communication an account is given of a young nobleman affected with a scirrhosity of the spleen, who was cured of a profuse vomiting of blood, with which he was seized in the winter of 1728,* by drinking water excessively cold, and taking milk, barley water, almond milk, chicken and rice broth, and other alimentary liquors, frozen by setting the vessels containing them in a mixture of pounded ice and nitre. At the same time a sponge wet with cold vinegar

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* He had vomited blood 4 years before, and from early youth he had been subject to bleedings from the nose every spring and autumn. Dr. M. mentions that in this attack the patient brought up by vomiting more than 12 lb. of blood in the space of 2 hours. (Sanguinem ad libras duodecim et amplius duarum circiter horarum spatio ex ore processisse.)

was applied to the epigastric region, and the room in which the patient lay was kept as cool as possible, and he had very few bed-clothes upon him. Some nitrous and opiate medicines were given. Previous to the use of the iced liquors, about 8 oz. of blood were drawn by leeches from the hæmorrhoidal veins. On the 3d or 4th day a clyster, composed of milk, butter, yolk of egg, and brown sugar, was administered, which brought away a great quantity of blood as black as soot, and in a powdery form. To prevent a return of the hemorrhage, the patient was advised to have 7 or 8 oz. of blood taken away every 3d or 4th month, either from the arm or from the hæmorrhoidal veins.

By this method the patient was kept free from a relapse until the year 1730, when the hemorrhage again returned. Dr. M. directed about 10 oz. of blood to be taken away from the arm, and prescribed 15 or 18 drops of the laudanum Helmontii, &c.; but the vomiting of blood was not restrained by these remedies more than a couple of hours. He therefore directed about 4 oz. of blood to be drawn from the hæmorrhoidal veins, with the use of water as cold as possible, and of those iced alimentary liquors which had been given so successfully in the former attack, and which he had also prescribed with similar good effect in a case of uterine hemorrhage. The patient vomited up some blood, but not in any considerable quantity, 3 days afterwards; on which account Dr. M. prescribed some opiate medicines, by which it was stopped. A milk and egg clyster, as beforementioned, was injected every 2d or 3d day, &c. and acidulated and mucilaginous liquors were given, together with opiates occasionally; and by these means the patient got well in about 5 weeks.

Dr. M. subjoins some reflections on this case, wherein he shows in what respect his method of treating a vomiting of blood differs from that of Aretæus, who, though he recommends what he supposes to be cooling, coagulating, and constringing medicines, makes no mention of ice or snow, or of frozen liquors. Tho. Bartholine, in his book de Nivis Usu Medico, quotes Galen, Avicenna, Rhazes, &c. as recommending liquids cooled by snow and cold topical applications in hot disorders of the stomach; yet none of those authors, Dr. M. says, ever prescribed iced liquors in a vomiting of blood. But Hippocrates, Aphorism. 23, sect. v. mentions cold water as a proper topical application in all kinds of hemorrhage.

Dr. M. adds that he once suppressed a uterine hemorrhage, against which the usual remedies, and even water exceedingly cold, had been given in vain, by applying ice to the thighs; by which means the crural, and consequently the iliac arteries being constricted, a less quantity of blood was transmitted to the uterus.

But though the aforesaid method be suited to hemorrhages, and especially to hemorrhages from the stomach, depending on too much heat and impetus of the blood, and occurring in young subjects; yet Dr. M. is far from recommending the use of iced liquors in cases of flooding after parturition, or in hemorrhages which occur in persons who, having weak stomachs and bowels, are subject to flatulency and indigestion.

The Description of a new Instrument for taking Angles. By John Hadley, Esq. Vice Pr. R. S. Communicated to the Society on May 13, 1731. N° 420, p. 147.

This instrument is designed to be of use, where the motion of the objects, or any circumstance occasioning an unsteadiness in the common instruments, renders the observations difficult or uncertain.

Its contrivance is founded on this obvious principle in catoptrics; that if the rays of light diverging from, or converging to any point, be reflected by a plane polished surface, they will, after the reflection, diverge from, or converge to another point on the opposite side of that surface, at the same distance from it as the first; and that a line perpendicular to the surface passing through one of those points, will pass through both. Hence it follows, that if the rays of light, emitted from any point of an object, be successively reflected from two such polished surfaces, that then a third plane, perpendicular to them both, passing through the emitting point, will also pass through each of its two successive images made by the reflections; all three points will be at equal distances from the common intersection of the three planes; and if two lines be drawn through that common intersection, one from the original point in the object, the other from that image of it which is made by the second reflection; they will comprehend an angle double to that of the inclination of the two polished surfaces.

Let RFH and RGI, fig. 1, pl. 13, represent the sections of the plane of the figure by the polished surfaces of the two specula BC and DE, erected perpendicularly on it, meeting in R, which will be the point where their common section, perpendicular also to the same plane, passes it, and HRI is the angle of their inclination. Let AF be a ray of light from any point of an object A, falling on the point F, of the first speculum BC, and thence reflected into the line FG; and at the point G of the second speculum DE, reflected again into the line GK; produce GF and KG backwards to M and N, the two successive representations of the point A; and draw RA, RM, and RN.

Since the point A is in the plane of the scheme, the point M will be so also by the known laws of catoptrics. The line FM is equal to FA, and the angle

MFA double the angle HFA or MFH; consequently RM is equal to RA, and the angle MRA double the angle HRA or MRH. In like manner, the point N is also in the plane of the scheme, the line RN equal to RM, and the angle MRN double the angle MRI or IRN; subtract the angle MRA from the angle MRN, and the angle ARN remains equal to double the difference of the angles MRI and MRH, or double the angle HRI, by which the surface of the speculum DE is reclined from that of BC, and the lines RA, RM, and RN are equal.

Corol. 1.—The image N will continue in the same point, though the two specula be turned together circularly on the axis R; so long as the point A remains elevated on the surface of BC, provided they retain the same inclination.

Corol. 2.—If the eye be placed at L, the point where the line AF continued cuts the line GK; then the points A and N will appear to it at the angular distance ALN, which will be equal to ARN; for the angle ALN is the difference of the angles FGN and GFL; and FGN is double FGI; and GFL double GFR; and consequently their difference double FRG or HRI; therefore L is in the circumference of a circle passing through A, N, and R.

Corol. 3.—If the distance AR be infinite, those points A and N will appear at the same angular distance, in whatever points of the scheme the eye and specula are placed: provided the inclination of their surfaces remain unaltered, and their common section parallel to itself.

Corol. 4.—All the parts of any objects will appear to an eye viewing them by the two successive reflections, as before described, in the same situation as if they had been turned together circularly round the axis R, keeping their respective distances from one another, and the axis, with the direction HI, i. e. the same way the second speculum DE reclines from the first BC.

Corol. 5.—If the specula be supposed to be at the centre of an infinite sphere: objects in the circumference of a great circle, to which their common section is perpendicular, will appear removed by the two reflections, through an arch of that circle, equal to twice the inclination of the specula, as is before said. But objects at a distance from that circle will appear removed through the similar arch of a parallel; therefore the change of their apparent place will be measured by an arch of a great circle, whose chord is to the chord of the arch equal to double the inclination of the specula, as the sines complements of their respective distances from that circle, are to the radius; and if those distances are very small, the difference between the apparent translation of any one of these objects, and the translation of those which are in the circumference of the great circle aforesaid, will be to an arch equal to the versed sine of the distance of this object from that circle, nearly as double the sine of the angle of inclination of the specula, is to the sine complement of the same.

The instrument, fig. 2, consists of an octant ABC, having on its limb BC an arch of 45 degrees, divided into 90 parts or half degrees; each of which answers to a whole degree in the observation. It has an index ML moveable round the centre, to mark the divisions: and on this, near the centre, is fixed a plane speculum EF perpendicular to the plane of the instrument, and making such an angle with a line drawn along the middle of the index, as will be most convenient for the particular uses the instrument is designed for; for an instrument made according to this figure the angle LMF may be of about 65 degrees, IKGH is another smaller plane speculum, fixed on such part of the octant as will likewise be determined by its particular use, and having its surface in such direction, that when the index is brought to mark the beginning of the divisions, i. e. 0°, it may be exactly parallel to that of the other; this speculum being turned towards the observer, and the other from him. PR is a telescope fixed on one side of the octant, having its axis parallel to that side, and passing near the middle of one of the edges IK or IH of the speculum IKGH; so that half its object-glass may receive the rays reflected from that speculum, and the other half remain clear to receive them from a distant object. The two specula must also be disposed in such manner, that a ray of light coming from a point near the middle of the first speculum, may fall on the middle of the second in an angle of 70°, and be thence reflected into a line parallel to the axis of the telescope, and that a clear passage be left for the rays coming from the object to the speculum EF by the side HG. ST is a dark glass fixed in a frame, which turns on the pin V, by which means it may be placed before the speculum EF, when the light of one of the objects is too strong: of these there may be several.

In the distinct base of the telescope, represented by the circle abcdef, fig. 3, are placed three hairs, two of which, ac and bd, are at equal distances from, and parallel to the line gh, which passes through the axis, and is parallel to the plane of the octant; the third fe is perpendicular to gh through the axis.

The instrument, as thus described, will serve to take any angle not greater than 90°; but if it be designed for angles from 90° to 180°, the polished surface of the speculum EF, fig. 2, must be turned towards the observer; the second IKGH must be brought forward to the position NO, so as to receive on its middle the rays of light from the middle of the first in an angle of about 25°, their surfaces being perpendicular to each other when the index is brought to the end of the divided arch next C; and this second must stand 5 or 6 inches wide of the first, that the head of the observer may not intercept the rays in their passage towards it, when the angle to be observed is near 180°. The smaller speculum is fixed perpendicularly on a round brass plate, toothed on the edge; and may be adjusted by an endless screw.

In order to make an observation, the axis of the telescope is to be directed towards one of the objects, the plane of the instrument passing as near as may be through the other, which must lie to such hand of the observer, as the particular form of the instrument may require; viz. the same way that the speculum EF does from IKGH, if it be composed according to this figure and description. The observer's eye being applied to the telescope, so as to keep sight of the first object; the index must be moved backward and forward till the second object is likewise brought to appear through the telescope, about the same distance from the hair cf, fig. 3, as the first: if then the objects appear wide of one another, as at i and k, the instrument must be turned a little on the axis of the telescope, till they come even, or very nearly so, and the index must be removed till they unite in one, or appear close to one another in a line parallel to cf, both of them being kept as near the line gh as they can. If the instrument be then turned a little on any axis perpendicular to its plane, the two images will move along a line parallel to gh, but keep the same position in respect of one another; so that in whatever part of that line they be observed, the accuracy of the observation will be no otherwise affected than by the indistinctness of the objects. If the two objects be not in the plane of the instrument, but equally elevated on, or depressed below it, they will appear together at a distance from the line gh, when the index marks an angle something greater than their nearest distance in a great circle: and the error of the observation will increase nearly in proportion to the square of their distance from that line; but may be corrected by help of the 5th corollary. Suppose the hairs ae and bd, each at a distance from the line, gh, equal to $\frac{100}{4146}$ of the focal length of the object-glass, so as to comprehend between them the image of an object, whose breadth to the naked eye, is a little more than $2\frac{3}{4}$°: and let the images of the objects appear united at either of those hairs: then as the sine complement of half the degrees and minutes marked by the index, is to the doubled sine of the same; so is one minute to the error; which is always to be subtracted from the observation. Other hairs may also be placed in the area abcdef, parallel to gh, and at distances from it proportional to the square roots of the numbers 1, 2, 3, 4, &c. and then the errors to be subtracted from the same observation, made at each of those hairs respectively, will be in proportion to the numbers 1, 2, 3, 4, &c. This correction will always be exact enough if the observer take care (especially when the angle comes near 180°) to keep the plane of the instrument from varying too much from the great circle passing through the objects.

In regard to the workmanship, if an exactness be required in the observations, the arch ought to be divided with the greatest care; because all errors committed in the division are doubled by the reflections. The index must have a steady motion on the centre, so that its axis remain always perpendicular to the plane of the octant; for if that alter, it will be liable to vary the inclination of the speculum it carries to the other: the motion must likewise be easy, lest the index be subject to bend edgeways: for the same reason it should be as broad at that end next the centre, as conveniently can be. The specula should have their surfaces of a true flat; because a curvature in either of them, besides rendering the object indistinct, will vary its position, when seen by reflection from different parts of them: they must also be of a sufficient length and breadth for the telescope to take in a convenient angle, without losing the use of any part of the aperture of its object-glass, and that in all the different positions of the index. They may be either of metal, or glass plates foiled, having their two surfaces as nearly parallel as they can; yet a small deviation may be allowed; provided either their thickest or thinnest edges, and consequently the common section of their surfaces, be parallel to the plane of the octant: for in that case, though there are several representations of the object, they will be always very near one another, in a line parallel to cf; and any of them may be used, except when the angle to be observed is very small. The chief inconvenience will be, that a small star will be more difficultly discerned, the light being divided among the several images. The telescope may be contrived to alter its situation, so as to receive the reflected rays on a greater or less part of its object-glass, if the objects differ in brightness. The second speculum may have a part unfoiled, that if either of them be sufficiently luminous, the less bright may be seen through it by the whole aperture. If the sun be one of the objects, or the moon be compared with a smaller fixed star; their reflected images must be still more weakened by the interposition of one or more of the dark glasses ST. An exact position of the telescope is not necessary; and the instrument may be used without one, the disposition of the specula, with regard to the sector and index, being such, as may allow the eye to be brought as near the second speculum as may be, and make the instrument the most commodious for the observer.

It will be easy to judge, that scarcely any greater degree of steadiness is requisite in the pedestal, or machine which carries this instrument, than what is sufficient for the telescope used with it: for though the vibrating motion of the instrument may occasion the images of the objects also to vibrate across one another; their apparent relative motion will be very nearly in lines parallel to cf; and it will not be difficult to distinguish whether they coincide in crossing one another, or pass at a distance: and if the objects are near one another, and the telescope magnify but about 4 or 5 times, it may be held in the hand without any standing support. In this manner the altitude of the sun, moon, or some of the brighter stars, from the visible horizon, may be taken at sea, when it is not too rough.

Fig. 4 shows an instrument designed for this purpose; differing from the foregoing description chiefly in the placing the specula and telescope, with regard to the sector and index; it has also a third speculum NO, disposed according to the directions when the angle is greater than 90°, whose use is to observe the sun's altitude by means of the opposite part of the horizon. In placing these two smaller specula, it will be further necessary to take care that the speculum IKGH do not stand so as to intercept any of the rays coming from the greater one fixed on the index to the third NO, nor either of them hinder the index from coming home to the end of the divided arch. WQ is a director for the sight; which is necessary when the telescope is not used. This consists of a long narrow piece, which slides on another fixed on the back of the octant, and carries at each end a sight erected perpendicularly on it: it may be removed at pleasure, and exchanged for the telescope, which slides on in the same manner, both serving indifferently with either of the two smaller specula. The eye is to be placed close behind the sight at W; and the thread stretched across the opening of the other sight at Q, perpendicular to the instrument, is to assist the observer in holding it in a vertical position, who is to keep this thread as near as he can parallel to the horizon, and the object near the upright one. How far an instrument of this kind may be of use at sea, to take the distance of the moon's limb from the sun or a star, in order to find the ship's longitude, when the theory of that planet is perfected, is left to trials to determine.

An Account of the Stylus of the Ancients, and their different Sorts of Paper. By the Hon. Sir John Clerk, F. R. S. Extracted by Roger Gale, Esq. V. P. R. S. N° 420, p. 157.

Sir John Clerk takes occasion from some antique brass implements, found near the wall of Antoninus Pius, now named Graham's Dyke, in Scotland, to give this curious dissertation on the stylus, an instrument used by the ancients for writing, with the figures of some of them annexed in a copper plate; two of which are represented in the shape and form of the Roman fibula; but the author is of opinion they were designed for a different purpose, for which he produces very cogent reasons.

He observes, that before the use of pens, the ancients performed their writing with an instrument called by them a stylus or graphium. The matter of it was gold, silver, brass, iron, or bone; the shape various, but alike in being pointed and sharp at one end, and flat and broad at the other: the first for writing, or rather cutting their letters, the latter for defacing or rubbing out whatever wanted correction; for all which, as well as for every thing else asserted by him, he produces sufficient proofs from proper authors.

He observes, that the styli made of iron, were sometimes used as daggers, and quotes two passages out of Suetonius to prove it; one where Julius Cæsar is said to have wounded Cassius in the arm, graphio; the other, where he tells us it was customary with Caligula to get his enemies murdered, graphiis, when they came into the Senate-house, and confirms these two passages by a third, taken from Seneca's first book De Clementia. He supposes the stylus made of bone was for the use of women and children, as less dangerous than those of metal; by a quotation from Prudentius, it appears that Cassianus the Martyr was killed by his scholars with iron styli.

He agrees with Petavius, or his editors, that the implements which gave birth to this dissertation, were styli, and not fibulæ ad connectendas vestes, as Monfaucon and other antiquaries have imagined; and thinks an objection, that the tongues of the styli must have been much longer than the tongues of their supposed fibulæ, to be of little weight; since there must have been some of them longer, and some shorter, according to the different fancies of the writers. Military men might sometimes write with the point of their daggers, and from this practice the words stylus and pugio came to be confounded; but men of business and private persons cannot be supposed to have made use of daggers for writing. He observes also, which is no small argument for his side of the question, that if Monfaucon had consulted the numerous draughts he has published of the habits belonging to the old Greeks and Romans, he would not have found one of these implements, either as a fastening or an ornament on them.

He proceeds next to a description of these styli found in Scotland, and shows how they were accommodated to the business he supposes them designed for. But as the copper plate prefixed to his dissertation will give us a much clearer notion of that, the reader is referred to it; only observing that the 5th figure in it is entirely different from the others, that he is in some doubt about it, and owns it might have served the aruspices, in examining the bowels of animals, and have been one of those instruments called exstispicia. However, he thinks that if he had pronounced it to have been a stylus, he should not have been much out of the way, since the ancients had their thecæ graphiariæ, which name will agree very well with this brass case, and the instrument found within it. From the stylus used to form letters comes that figurative expression that a person writes such or such a sort of a style, to denote his manner, as a lofty style, or a low style; which way of speaking our own and other modern nations have introduced into their language.*

As to the several sorts of charta used for writing, he observes the most ancient were made of barks of trees, or skins, or were such as are called pugillares. The oldest were of the inner bark of trees, called liber in Latin, whence a book had the name of liber; but very little of this sort is now in being, except the Egyptian papyrus may be accounted one species of it.

The papyrus was called Βύβλος or Βὶβλος by the Greeks, and thence their books Βίβλοι or Βίβλια. This sort of charta was made of a plant† having many pelliceous tunicles, as Pliny informs us, which were separated from one another by a needle, and then glued again together, to give them a strength and firmness sufficient to retain what might be written on them. Alexandria was the place most eminent for this manufacture. There are some fragments of this sort still extant in Libraries, particularly the famous manuscript of St. Mark's gospel at Venice.

The chartæ membranaceæ are made of the skins of animals, dressed either like our glove-leather, or modern parchment. The first sort was commonly used by the Jews for writing the law of Moses on it, and from the rolling up of these skins comes the word volumen. But the skins which Varro and Pliny say were first made by Eumenes king of Pergamus, were in more common use: however, Eumenes, who is related by these authors to have made them in opposition to Ptolemy king of Egypt, who had forbidden the exportation of the papyrus from his dominions, does not seem to be the inventor of the chartæ membranaceæ; since Herodotus, who lived long before his time, informs us, that the Ionians and other nations used to write on goat and sheep-skins. Josephus also tells us, that the Jews sent their laws written on skins in letters of gold to Ptolemy; by which it seems as if the writing on skins was no new thing at that time among the Jews.

The use of the pugillares was also very ancient, being mentioned by Homer,

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* And it might be further observed, that our phrase of " changing or altering the style," might arise from the writer's inverting the stylus, to smooth out or obliterate some part with its flat end, in order to introduce some alteration in the writing on the tables of wax or lead, &c. Also, from the circumstance of the styli being sometimes used as daggers, might come the idea and expression of " writing in blood." Edit.

† There is a learned dissertation on this plant by Montfaucon, in the 6th vol. of the Mem. de l'Acad. des Inscriptions, and a good figure of it in Bruce's Travels. It is the Cyperus papyrus Linn.

and among the Latins by Plautus. They were made of all sorts of wood, ivory, and skins, covered over with wax. They were likewise of several colours, as red, yellow, green, saffron, white, and others. Being waxed over, any thing was easily written on them by the point of the stylus, and as easily rubbed out, and altered by the flat part of it. Sometimes these pugillares were made of gold, silver, brass, or lead, and then there was a necessity of an iron stylus to write or cut the letters on them; which explains that passage in the 19th chapter of Job, Quis mihi det ut exarentur in libro, stylo ferreo et plumbi lamina, vel certe sculpantur in silice. They consisted sometimes of 2, 3, 5 or more pages, and thence were called duplices, triplices, quintuplices, and multiplices; and by the Greeks, Διπτυχα, Τρίπτυχα, &c.

The diptychs and triptychs that were covered with wax, served only for common occurrences; the other sorts received every thing else that was written on chartæ or membranæ, and were sometimes called by the Greeks palimpsestæ, from the rubbing out of the letters on them.

The chartæ linteæ, and bombycinæ, which were made of linen or cotton, were of much later date; and from these we learned to make the paper now in use of linen rags, an invention probably of about 600 years standing.

Writing was practised on all these chartæ with a reed, and afterwards with a pen, except on the pugillares. These reeds grew on the banks of the Nile; the Greeks also used reeds imported from Persia for the same purpose. Calami argentei are also mentioned for writing.

Their letters were formed with liquors of various colours, but chiefly black, thence called atramentum, and in Greek μέλαν or μελάνιον. It was sometimes made of the blood of the cuttle fish, sometimes of soot. Apelles composed a black of burnt ivory, which was called elephantinum. They had ink also from India of an approved composition, as Pliny says.

The titles of their chapters and sections were written in red, or purple: hence the titles of the Roman laws are called rubricæ. Their purpura was an exceedingly bright red, or crimson, much in vogue with the Byzantine writers, and called Κιννάϐαρις; which was a liquor made of the murex boiled, and its shell very finely powdered; or as Pliny relates, of the blood of that fish. Almost all the ancient emperors wore this colour; their names were painted in it on their banners; and they frequently wrote with it, and wore it. This colour was often the distinction of a Roman magistrate, and to put on the purple, was the same thing as to assume the government. This colour was so admired by the poets, that they called every thing which was very bright and fine, purple; as Horace compliments the swan, which is never of any colour but white, with purpureis ales cloribus. We find even snow honoured with the same epithet; whence some have imagined that purpureus signified white.

The children of the emperors, and such as had a prospect of rising to the throne, and their guardians, sometimes wrote with green; gold also was employed for the like purpose. Those who wish to see more on this subject, may have recourse to Mabillon De Re Diplomatica, and Monfaucon in his Palæographia Græca.

Experiments concerning the Poisonous Quality of the Simple Water distilled from the Lauro-cerasus, or common Laurel. By Cromwell Mortimer, M. D., R. S. Secret. N° 420, p. 163.

Dr. Mortimer took a peck of laurel leaves, and put them into an alembic with 3 gallons of water, which he distilled in the common way, as penny-royal, mint-water, or any other simple waters. The fire at first being too hot, there came over an oilyness with the water, (1) which made it appear milky, till about half a pint had run: this tasted and smelt very strong like apricot kernels, as did the next running, (2) which was clearer. He kept the first quart of it by itself: then he drew off another quart, (3) which was not near so strong in taste or smell, but rather resembled black-cherry water: the remainder was almost insipid. The leaves after the distillation looked brownish, were brittle, and tasted bitter, without the roughness, or apricot kernel flavour, which they have while fresh.

In the afternoon of the same day he took a mungrel puppy, weighing 2½ lb. about 16 days old; it had sucked its dam in the forenoon, but had now fasted 6 hours. He took 1 oz. of the 3d water, and gave some of it to the puppy, gradually by tea-spoonfuls, that it might the better swallow it. When it had taken half the quantity, he let it go; it walked about pretty strongly for 5 minutes, when it began to foam at the mouth, and soon after vomited up some curdled milk, and then discharged the fæces; after which the sickness seemed to go off. He then gave it 3 tea-spoonfuls more; in 10 minutes it began to stagger, and draw its hind parts after it; it sat on its breech, whined, and made several efforts to vomit, but brought nothing up; and then again would walk about, and sit down and whine, and again seem to recover, for about 15 minutes longer. Then thinking the second water would dispatch it sooner out of its misery, it seeming to be very uneasy, he took 1½ oz. of the second running; and gave it first 3 tea-spoonfuls, and set it down, when in 2 minutes time it became strongly convulsed, put out the tongue, and made strong efforts to vomit, but to no effect; it could not stand on its legs, but lay with its hind legs stretched out. Five minutes after he gave 3 tea spoonfuls more, when it was stronger convulsed, rolled over and over several times, drew its head back to its rump, then lay on its side and panted much: about 8 minutes after, he gave it two tea-spoonfuls more, and it had fresh and strong convulsions, but kept lying on its side, and thus stretching out its 4 legs one after another, drawing in its flanks very quick; in 15 minutes more it died; being in all about an hour from the first dose.

An hour after it was dead, the Doctor opened it, and found all the contents of the abdomen well; the stomach was distended with wind, but empty of milk, though full of froth, and a clear mucus of a much thicker consistence than the liquor gastricus naturally is: they had no smell at all; and the inside of the stomach was not at all inflamed.

On opening the thorax, he found the lungs a little redder than natural, with some vessels on their outer membrane very turgid: on cutting them out a good deal of clear red blood ran from them. The veins and both ventricles of the heart were turgid, and full of coagulated blood, of a dark-brown colour, which tinged his fingers of a dirty yellow, as if some gall had been mixed with it. There was no blood in the arteries; the foramen ovale was open.

On opening the head, the dura mater appeared livid, as if bruised; its vessels and the sinus falciformis were turgid, and full of the same blood, as the heart and vessels near it. The cortical substance of the brain looked of an unusual livid colour.

Next day about 5 in the afternoon he took a large mastiff dog, weighing 75lb. They tied him to a post as he stood on his legs, one holding him strongly by the tail, he being very fierce and unmanageable. They injected per anum 3 oz. of the second running; in 5 minutes he trembled and staggered much; would let them handle him; he drew his hind legs after him; tumbled on his head; panted and slabbered; but gradually recovered so as to stand up, though reeling and often sinking with his hind legs. Fifteen minutes after, they injected 1 oz. more; he immediately staggered and sunk behind; soon after he made water plentifully. They then led him to another kennel, where he soon discharged the fæces plentifully, but of a hard consistence: the fæces seemed moistened with the last injected oz. which Dr. M. imagined came away by this stool; he therefore immediately injected another oz.; on which he seemed more uneasy than before, tumbling on one side, and in about 10 minutes after, he fell fast asleep, breathing with difficulty; ½ an hour after, they roused him, found him slabbering, drowsy, sinking behind, and giddy: about 1¼ hour after the first injection, they found him as before; but provoking him with a stick, he bit at it, and though naturally fierce, he was very quiet when not struck: in a few minutes he reeled and fell a snoring again: about 9 at night he seemed very well, only drowsy. They left him all that night without water and victuals, but through hunger he ate some of the straw he lay on, as they found afterwards on opening his stomach. Next morning they gave him water and bones; he drank greedily, and ate the bones, bread, and whatever was given him, seeming perfectly recovered and well all day and the next night, only very thirsty, and a little drowsy, but perfectly gentle.

About 9 next morning, they fastened him to a post, and put a rope into his mouth, by which his nose was tied fast to a rail, great care being taken that there should be no rope about his neck so tight, as to hinder his swallowing or his breathing: Dr. M. then have him 3 oz. of the second running, at 3 times, with a horn, such as they drench horses with: he swallowed it with great difficulty, and guggled some up again: to prevent which, he thrust the horn a good way down his throat. They then untied him from the post, to see how he could walk, but he instantly reeled, fell down, rolled over and over, discharged much urine, and some hard fæces, had no motion to vomit, but dribbled much, panted, and showed great difficulty of breathing, snuffing up the air with his nostrils, holding his nose up, as he sat on his breech; for he could not then stand on his hind legs: he often shook his head, as if stung by some fly: he gradually recovered, and in about 20 minutes time could walk about very steadily on all his legs, though he still appeared weakest behind. Therefore imagining he might linger a long time, or perhaps recover entirely, they made him fast again, and gave him 3 oz. more, near half of which he spilt; and indeed out of the 6 oz. not above 3 or 4 entered his stomach: he gave one terrible loud howl, and sunk down at once, before they could untie him from the post, to see whether he could walk or not. He never offered to rise again, but lay on one side, panted, hung out his tongue, and slabbered much, stretched all four legs out 3 or 4 times, and was quite dead and motionless in about 5 minutes. There were no convulsions in the muscles of the neck and back, nor was his head and tail drawn nearer together, as in the puppy.

About ½ an hour after he was opened, being still warm. The bladder was contracted and empty; the rectum slightly inflamed, the small guts not distended with wind, but contracted, and almost close; the bile was evacuated in great quantity into the duodenum, and was very thick, appearing like congealed honey; the gall-bladder was almost empty; but what remained in it was as thick as the other; to the inside of the gall-bladder there adhered several excrescencies in form and size of lentils, like drops of softish yellow wax: the liver was exceedingly inflamed, and almost livid: the stomach was contracted near the pylorus, and again about 3 inches above it; some pieces of bone were in it, a good deal of straw, and about 2 oz. of fluid, which smelt strong of the laurel water; but no mucus as in the puppy: some of the villi seemed slightly inflamed, the blood vessels being very turgid: there was a great deal of mucus in the œsophagus, which did not seem inflamed. The lungs appeared exceedingly contracted, and very red and inflamed. In this dog it was very evident, that the pericardium did not adhere to to diaphragm, as in erect animals, here being a distance of above 2 inches, which was filled up by an appendix to the right lung; on removing which, the vena cava ran from the diaphragm about 3 inches to the pericardium quite free, not adhering to the back by any fibres or membranes, and was entirely enveloped by this appendix of the lung. The vena cava and all the veins were vastly distended, and the blood in them coagulated, though the body was yet hot: there was little or no blood in the aorta: only on pressing it, a small quantity of a transparent fluid, like serum, flowed out of it. The blood was strongly coagulated in the right auricle and ventricle of the heart, being of a very dark colour, and filled them quite; but the left auricle and ventricle contained only a small clot of congealed blood, which looked more red and florid: they kept some clots of the blood out of the vein, and also out of the left ventricle, 24 hours, but neither of them liquified or ran into serum. The head was cut off, but not opened till 24 hours after; a great deal of blood drained from it; and on opening it, the vessels did not then appear distended, but the dura mater looked livid: there was no blood at all in the sinus falciformis; the brain looked very well; the vessels of the plexus choroides in each ventricle were not distended, but livid; nor were they burst, there being no extravasation in the ventricles, only a very small quantity of lymph; which was the case likewise of the pericardium, which had not above a tea-spoonful of water in it.

In both these instances, this poison seems to act by coagulating the blood; so that it cannot pass the lungs or brain: and probably the puppy lived longer than the great dog, because in the puppy the foramen ovale was open, by which the thickened blood could pass, and perform a few circulations more than it could have done, had it had the lungs to pass through; and that in the puppy the brain was the part the most affected, as was evident from the convulsions it had: whereas the dog was little convulsed, but seemed to die of a difficulty of breathing; and the greatest accumulation was found at the right ventricle of the heart.

Dr. M. procured a middling-sized spaniel, and poured some laurel water down his throat: he struggled pretty much at first, and whined; but when about $1\frac{1}{2}$ oz. was down, he ceased struggling: that he might not be too long in dying, as much more was given him; he spilt about one third of the whole quantity: he was then laid down on the ground, but never offered to get up, only stretching out his legs, expired presently. Being opened, about 2 oz. of the laurel water was found in his stomach, and some frothy mucus; the veins in general were very turgid, but the blood was still fluid; and they could discern no alteration in any of the viscera.

Dr. M. gave 4 oz. of laurel water to Dr. Porter. He forced down a pretty large dog 3 oz. with no great difficulty. The creature instantly returned about 2 oz. by vomit, clear and unaltered; in a few minutes he grew very convulsed; soon after became motionless, and to all appearance was dying. Within 10 minutes he vomited a second time, and threw up a small quantity of a viscid, green, and very frothy matter: from which moment he began to recover, and within half an hour was perfectly well. He was kept in the yard all night, and the next morning not the least disorder was perceived in him.

About half an hour after 6 in the evening Dr. M. gave about $\frac{1}{2}$ oz. of the laurel water to a middle-sized spaniel, weighing near 16lb. which he swallowed with great reluctance, spilling near as much more, which he endeavoured to pour down his throat. Some of the company desired he might be set down, to see what effect so small a quantity would produce: he remained about a minute and a half on his legs; he then began to reel, and in about 3 minutes more fell into most violent convulsions, and his neck and tail were strongly drawn towards each other; he neither vomited nor purged, but they expected he would expire every minute, the convulsions being so exceedingly strong, when some of the company called for some milk, in order to try whether it would prove an antidote to so desperate a poison. They poured a little milk into his throat, which at first he could not swallow, but guggled it up again as if almost strangled with it. After several trials he began to swallow some, about a spoonful at a time, and seemed a little relieved, his convulsions leaving him, only fetching his breath very hard; but he lay still and snorted, as if in a profound sleep; and the milk frothed out of his nose: on rousing him, he opened his eyes, and swallowed the milk better, which seemed to revive him much; so that the company imagining he would entirely recover, went away. Dr. M. staid some time longer, till at last he began to lap the milk himself when held up to it: he vomited up a good deal of milk, which relieved him more; and then he lapped again, but could not stand on his legs. He left him in this condition about 7 o'clock, thinking he would have recovered, and left orders that he should have a pan of milk, and another of water, about a pint of each, set by him, and that he should be kept shut up all night. About 11 o'clock he was seen alive and walking about; but next morning he was found dead, after having drank up all the milk and water, and having vomited and purged a good deal.

Thus it appears that this simple water distilled from a vegetable, is equally mortal with the bite of the rattle-snake, and more quick in its operation than any mineral poison; and though it may not immediately bring on death, when taken in small quantities, or mixed with other liquors, it being common among our good housewives to put laurel leaves into cream to give it the rattafia or apricot-kernel flavour; and some compounders of cordial waters, to use the berries of this plant in brandy instead of black-cherries, to mix some of the distilled poisonous water with brandy to make rattafia, or to dilute it with common water, till it resembles black-cherry water in taste; though one single draught at a time may not prove immediately mortal, yet the habitual use of these liquors must be exceedingly prejudicial and unwholesome, and in weakly persons must hasten death.

Dr. M. was informed that a gentleman and his wife, who used for several years to drink daily a dram or two of brandy in which laurel berries had been infused, both died paralytic, having lost their speech some time before.

Continuation of an Account of Mr. Mark Catesby's Essay towards a Natural History of Carolina and the Bahama Islands. By Dr. Mortimer, Sec. R. S. N° 420, p. 174.

This is an account of several kinds of birds, &c.

A Solar Eclipse observed at Pekin, July 15, 1730, *N. S. By the Fathers Koegler and Pereyra.* N° 420, p. 179.

At 0^h 2^m, P. M. at the centre, or 6 digits eclipsed.
0 51 the greatest obscuration 9 dig. 54′.
1 2 the emersion.
1 39 emersion of the centre.
2 $27\frac{1}{6}$ the end of the eclipse.

Some Immersions and Emersions of Jupiter's Satellites observed at Pekin. By the same. N° 420, p. 182.

Omitted as unimportant.

An extraordinary Impostumation of the Liver. By Thomas Short, M. D. N° 420, p. 184.

Dr. Short had a patient, who died lately of an impostumation of the liver. He opened him, and out of the lowest and thinnest lobes he took 6 quarts of purulent, thick, most intolerably fetid, reddish-brown matter, very acrid; for no sooner was it exposed a little to the open air, than it fermented exceedingly. The patient had drained off the thinner part, the last week of his life, by violent vomiting and purging, to 30 or 40 stools a day, and as many vomitings. It was thrown into the duodenum by the ductus choledochus communis, and there pumped up and thrown out, both by its sharpness and stimulation. All the upper part of the liver, to about an inch below the gall-bladder, was sound. The tumour had so compressed the right kidney, that it was emaciated away to less than the glandula renalis.

A Proposal of a Method for finding the Longitude at Sea within a Degree, or 20 Leagues. By Dr. Halley, Astr. Reg. Vice Pres. R. S. N° 421, p. 185.

Upwards of 20 years before, Dr. Halley added an appendix to the 2d edition of Mr. Street's Caroline tables, containing a set of observations he had made in 1683 and 1684, for ascertaining the moon's motion; giving a specimen of what he thought at that time might be the only practicable method of attaining the longitude at sea. What he published then, was to this effect.

" The advantages of the art of finding the longitude at sea, are too evident to need any arguments to prove them: and having by my own experience found the impracticability of all other methods proposed for that purpose, but that derived from a perfect knowledge of the moon's motion; I was ambitious, if possible, to overcome the difficulties that attend the discovery of it.

" And first, I found it only needed a little practice to be able to manage a 5 or 6-foot telescope, capable of showing the appulses or occultations of the fixed stars by the moon, on ship-board in moderate weather; especially in the first and last quarters of the moon's age, when her weaker light does not so much efface that of the stars. Whereas the eclipses of Jupiter's satellites, how proper soever for geographical purposes, were absolutely unfit at sea, as requiring telescopes of a greater length than can well be directed in the rolling motion of a ship in the ocean.

" Now the motion of the moon being so swift, as to afford us scarcely ever less than 2 minutes for each degree of longitude, and sometimes 2 and $\frac{1}{2}$; it is evident, that could we perfectly predict the true time of the appulse or occultation of a fixed star, in any known meridian, we might, by comparing it with the time observed on board a ship at sea, conclude safely how much the ship is to the eastward or westward of the meridian of our calculus.

" But after much examination, and carefully collating the Caroline tables of Mr. T. Street, though generally better than those that went before him, as also those of Tycho, Kepler, Bulliald, and our Horrox, with many accurate observations of the moon, carefully made on land; it does not appear that any of these tables represent the motions with the certainty required; and though many times the agreement seem surprising, when the errors of the several equations compensate one another; yet in those parts of the orbit, where they all fall the same way, the fault is intolerable, and the result many times not to be depended on, to more than 100 leagues; that is, it is entirely insufficient.

"Yet still this is the fault of the artist, not of the art: for, observing the periods of the lunar inequalities, which is performed in 18 years and 11 days, or 223 lunations; it is found that the returns of the eclipses, and other phænomena of the moon's motion, are very regularly performed: so that whatever error is found in a former period, the same is again repeated in a second, under the like circumstances of the same distance of the moon from the sun and apogæum.

"Thus, from the observation made of the eclipse of the sun, which was seen June 22, 1666, in the morning, at London and Dantzic, I was enabled to predict, with great certainty that other, which I observed July 2, 1684, by allowing the same error I found in the calculus of the former: and the like will do with equal certainty, in the cases extra syzygias, when the mean and synodical anomalies are nearly the same, about the same time of the year.

"Being thus assured, from the certainty of these revolutions, that all the intermediate errors of our tables were not uncertain wanderings, but regular faults of the theories; I next thought how I might best be informed of the quantity and places of these defects; that being apprised how much, and which way my numbers erred, I might apply the difference; so as at all times to represent the true motion of the moon: nor was there any other way, but from the heavens themselves to derive this correction, by a sedulous and continued series of observations, to be collated with the calculus, and the errors noted in an abacus: from whence, at all times, under the like situation of the sun and moon, I might take out the correction to be allowed.

"And having by me the sextant I made to observe the southern stars at St. Helena, in 1677, I fixed it for this purpose; resolving to have continued to observe, till I had filled my abacus, so as that it might have the effect of exact lunar tables, capable of serving at sea, for finding the longitude with the desired certainty.

"With this design, I applied the leisure I had in 1683, to observe diligently, as often as the heavens would permit, the true place of the moon, especially as to longitude; and in the space of about 16 months I had gotten near 200 several days observations, most of which I collated with the Horroxian theory, whose calculus is something more compendious than that of Mr. Street, and having placed the errors in an abacus, I perceived how regular the irregularities were; and that where the moon had been exactly observed formerly, at the distance of one or more periods of 223 months, I could even predict the errors of the tables, with a certainty not much inferior to that of the observations themselves. But this design of mine was soon interrupted by domestic occasions; and since then, my frequent avocations have not permitted me to resume these thoughts.

" In the mean time I have taken care to present my observations, such as they are, to the public, in order to preserve them; assuring that as on the one hand they were made with a very sufficient instrument, with all the care and diligence requisite; so in the remote voyages I have since taken to ascertain the magnetic variations, they have been of signal use to me, in determining the longitude of my ship, as often as I could get sight of a near transit of the moon by a known fixed star: and thereby I have frequently corrected my journal from those errors, which are unavoidable in long sea-reckonings.

" If therefore you happen at sea to observe nicely the time of an occultation, or close application of a star to the moon; and can find a correspondent observation, about the same mean anomaly and distance of the moon from the sun, either among these of mine, or in any other collection of observations, accurately made, especially near the same time of the year; and above all, after the aforesaid period of 18 years and 11 days, you may, without sensible error, from thence pronounce in what meridian your ship is; taking care in so operose a calculation to commit no mistake; and notwithstanding the direction the moon gives you, not confiding so much in it, as to omit any of the usual precautions to preserve a ship when she approaches the land.

" I had intended to insist more largely on this method of obtaining the moon's place, and consequently the longitude at sea; but that I find it requires a just treatise too long to be here subjoined; and more especially, that the great Sir Isaac Newton, to whom no mathematical difficulty is insuperable, has given us a true and physical theory of the moon's motions; by which the defects of all former tables are so far amended. that it is hoped the error may scarcely ever exceed 3 minutes of motion, or so little in longitude, that perhaps it may be thought a sufficient exactness for all the uses of navigation. If therefore what is here offered find a kind acceptance from those that it chiefly concerns, I shall be encouraged to proceed on a work I have long meditated, to improve the abovementioned period, as to the abbreviating the computation of eclipses; and in general, to facilitate the too laborious calculation of the moon's place extra syzygias."

Not long after, her late Majesty Queen Anne was pleased to bestow on the public an edition of the much greater and most valuable part of Mr. Flamsteed's observations; by help of which the great Sir Isaac Newton had formed his curious theory of the moon, a first sketch of which was inserted by Dr. David Gregory, in his Astronomiæ Physicæ et Geometricæ elementa, published at Oxford in 1702; and again in the second edition of Sir Isaac Newton's Principia, which came out in 1713, we have the same revised and amended by himself, to that degree of exactness, that the faults of the computus, formed from it, rarely exceed a quarter part of what is found in the best lunar tables extant before that time.

Being thus provided with proper materials, viz. a large set of observations, and a theory of the motions so very near the truth, Dr. Halley resumed his former design of filling up his abacus or synopsis of the defects of this lunar theory, and made tables to expedite the calculus according to it, and compared its numbers with several of the most certain of Mr. Flamsteed's places observed. By this it was evident that Sir Isaac had spared no part of that sagacity and industry so peculiar to himself, in settling the epochas and other elements of the lunar astronomy, the result many times, for whole months together, rarely differing 2 minutes of motion from the observations themselves; nor is it unlikely but good part of that difference may have been the fault of the observer: and where the errors were found greater, it was in those parts of the lunar orbit, where Mr. Flamsteed had very rarely given himself the trouble of observing; viz. in the 3d and 4th quarters of the moon's age, where sometimes these differences would amount to at least 5 minutes.

Mr. Flamsteed was long enough possessed of the Royal Observatory to have had a continued series of observations for more than 2 periods of 18 years; by which he had it in his power to have done all that could have been expected from observation, towards discovering the law of the lunar motion. But he contented himself with spare observations, leaving wide gaps between; so as to omit frequently whole months together; and in one case, the whole year 1716. So that notwithstanding what he has left us must be acknowledged more than equal to all that was done before him, both as to the number and accuracy of his accounts; yet for want of an uninterrupted succession of them, they are not capable of discovering, in the several situations of the lunar orbit, what corrections are necessary to be allowed, to supply the deficiency of our computus.

On Mr. Flamsteed's decease, about the beginning of the year 1720, his late Majesty King George I, was graciously pleased to bestow on Dr. Halley the post of his astronomical observer, expressly commanding him to apply himself with the utmost care and diligence to the rectifying the tables of the motions of the heavens, and the places of the fixed stars, in order to find out the so much desired longitude at sea, for perfecting the art of navigation. These are the words of his commission; and here the Dr. might have thought himself in a condition to put in execution his long projected design of completing his abacus, or table of the defects of the lunar numbers: but on taking possession, he found the observatory wholly unprovided with instruments; and indeed, of every thing else that was moveable; which postponed his endeavours, till such time as he could furnish himself with an apparatus capable of the exactness requisite: and this was the more grievous to him, on account of his advanced age, being then in his 64th year, which put him past all hopes of ever living to see a complete period of 18 years observation.

But hitherto, he owns, he has had sufficient health and vigour to execute his office in all its parts with his own hands and eyes, without any assistance or interruption, during one whole period of the moon's apogæum; which period is performed in somewhat less than 9 years. In this time he has been able to observe the right ascension of the moon at her transit over the meridian, near 1500 times, and with an exactness, he is bold to say, preferable to any thing done before, a number not less than those of Tycho Brahe, Hevelius and Flamsteed, taken in one sum, there being near 4 of his lunar observations for each degree of the zodiac, as also for each degree of the argumentum annuum, or distance of the sun from the moon's apogæum: and that these might be duly applied to rectify the defects of our computations, he has himself compared with the aforementioned tables, made according to Sir Isaac Newton's principles, not only his own observations, but also upwards of 800 of Mr. Flamsteed's.

This comparison of his own observations, and from the time he esteems them complete, with the computus by the said tables, being now continued for above 9 years, he designs speedily to communicate to the public, together with the tables themselves, which have been printed, and should have been published long since, had not his post at Greenwich given him an opportunity to examine, with proper nicety, in what parts of the lunar orbit, and how much, the numbers erred. So useful an addition as this, it is hoped, may fully answer the long delayed expectation some persons may have had of seeing the tables sooner: by means of which those that are qualified may, if they please, examine by their own observation the truth of what is here asserted.

Comparing likewise several of Mr. Flamsteed's most accurate observations made 18 or 36 years before, that is one or two periods before Dr. Halley's, with those of his own which tallied with them, he had the satisfaction to find that what he had proposed in 1710 was fully verified; and that the errors of the calculus in 1690 and 1708, for instance, differed insensibly from what he found in the like situation of the sun and apogæum in 1726. The great agreement of the theory with the heavens compensating the differences, that might otherwise arise from the incommensurability and eccentricity of the motions of the sun, moon and apogæum.

Encouraged by this event, the Dr. next examined what differences might arise from the period of 9 years wanting 9 days; in which time there are performed very nearly 111 lunations, or returns of the moon to the sun; but the return of the sun to the apogæum in that time differing above 4 times as much from an exact revolution, as in the period of 18 years, he could not expect the like agreement in that. However, having now entered on the 10th year, he compared what he had observed in 1721, 1722, with his late observations of 1730, 1731; and he rarely found a difference of more than one single minute of motion, part of which may probably arise from the small uncertainty that always attends astronomical observations, but most commonly this difference was wholly insensible; so that by the help of what he observed in 1722, he presumes he is able to compute the true place of the moon with certainty, within the compass of 2 minutes of her motion during this present year 1731; and so for the future. This is the exactness requisite to determine the longitude at sea to 20 leagues under the equator, and to less than 15 leagues in the British channel.

It remains therefore to consider after what manner observations of the moon may be made at sea with the same degree of exactness: but since the worthy vice-president Mr. Hadley, to whom we are highly obliged for his having perfected and brought into common use the reflecting telescope, has been pleased to communicate his most ingenious invention of an instrument for taking the angles with great certainty by reflection, (vide Phil. Trans. N° 420) it is more than probable that the same may be applied to taking angles at sea with the desired accuracy.

An Account of the Contrayerva. By Dr. William Houston. N° 421, p. 195.

Contrayerva is a Spanish word, signifying as much as herba contra (venena) or an herb against poisons. The name of contrayerva, seems to have been given by the Spaniards to as many plants of this sort as have come under their knowledge. Dr. Houston, however, does not pretend to give a history of all those roots, but only offers a short account of that plant, whose root is called contrayerva here in England, and well known to all that deal in medicines.

The root itself being so commonly known, the Dr. confines himself to the description of the plant that produces it, which he had not hitherto met with to his satisfaction in any author.

F. Plumier, in his book entitled Nova Plantarum Americanarum Genera, describes a genus he calls dorstenia, of which the Dr. found two species in the West Indies; the roots of which are gathered and exported indifferently; as being very much alike, both in appearance and virtues. One of these he thinks may be called Dorstenia dentariæ radice, sphondylii folio, placenta ovali; the other, Dorstenia dentariæ radice, folio minus laciniato, placenta quadrangulari et undulata.

The first kind, fig. 4, pl. 12, seems to be the tuzpatli of Hernandez, p. 147. Its roots, which are perennial, put forth in the month of May, or as soon as it happens to rain, each 6 or 8 leaves 4 or 5 inches long, and as many broad, cut into several segments almost as deep as the middle rib, somewhat after the manner of the sphondylium. They stand on footstalks 5 or 6 inches long; and from the middle of them come forth other footstalks somewhat longer, sustaining each a strong sort of body, flat, and situated vertically, or with one edge uppermost, which the Dr. has called placenta. In this species it is of an oval figure, with its longer axis parallel to the footstalk. One side of it is smooth and green, like the outside of the calyx in other plants; but from the other arise a great many small yellow apices; and after they are gone, several small roundish seeds begin to appear, which when ripe are somewhat like those of gromwell or lithospermon. It grows in the kingdom of New Spain, near old Vera Cruz, on the high ground, by the side of the river.

The second kind, fig. 5, has much the same number of leaves as the former, but of a different figure: for, some of them are entire, and shaped like those of a violet; others angular like ivy; and some almost as much divided as the leaves of the common maple. They are thin, and of a dark green colour, and smooth, or have only a few, scarcely perceptible, hairs on the back. The pedicles that sustain the flowers arise immediately from the root, as in the other species, and attain to the same height of 6 or 8 inches. But the placenta which sustains the flowers is in this kind quadrangular, waved about the edges, and broader transversely than vertically. Yet the flowers and seeds themselves are perfectly the same as in the other. The second kind grows plentifully on the high rocky grounds about Campechy, where the Dr. gathered it in perfection in the beginning of Nov. 1730.

The Dr. cannot guess why F. Plumier has called this a monopetalous plant: for, that which the latter calls the petalum, and the former the placenta, is of a green colour; and, which is of more consequence, sustains the seeds when ripe, and never envelopes the organs of generation when young; so that the Doctor thinks it can by no means be called a petalum, nor even properly a calyx: and therefore he has given it the name of placenta, whose office it certainly performs.

The Doctor has not been able to observe exactly the structure of the organs of generation, because of their excessive smallness; but they appear to the naked eye as represented in the figures, and in Plum. N. G. tab. 8. The Dorstenia sphondylii folio, dentariæ radice of Plumier differs from both of the Doctor's; for, in the former's drawings, done by order of the late king of France, of which the Doctor had seen a copy in the collection of the late Dr. Sherard, the leaves are represented serrated, the placenta quadrangular, and the roots consisting of several knobs tied together lengthwise. From which last particular, the Doctor is persuaded that the root of that species is the drakena radix, mentioned by Clusius in his Exotics, p. 83.

Concerning Diamonds found in Brazil. By Dr. De Castro Sarmento.
N° 421, p. 199.

Dr. De Castro had the following account of diamonds from a gentleman, who for these 15 years last past had lived and dug gold in the mines of Brazil, and who brought from thence several diamonds of considerable value.

Near the Prince's town, capital of the county Do Serro do Frio, belonging to the government of the gold mines, there is a place called by the natives Cay the Merin, where they used to dig gold for many years, as also from a small river, called Do Milho Verde. The miners, that dug gold in those places, turned up the grounds and sands of the banks of the said river, in order to extract the gold from them, and by so doing found several diamonds, which then they did not prize as such; for, some of the miners kept several stones for their figure and curiosity, which, though so valuable, by length of time they neglected and lost, and did so till the year 1728, when one of the miners coming to work there, and being better acquainted, deemed them to be diamonds, and made experiments on them; and finding them really such, began to seek for them in the same ground and sand, where the former miners had ignorantly left them; and the rest of the people followed his example.

Having thoroughly examined those places, they began to search for them in the river itself, and they actually found diamonds there, but with more difficulty and trouble. In the former places they found them together among the earth and sand, as they lay; but in the river, as the sand is more dispersed, they lie farther asunder.

Experience and common reason teach the people there, that these diamonds came from another place by the current of the waters, and are not the natural product of the situation where they now are found. So they are using all possible diligence to find out the place where they grow. They have not hitherto discovered it; but their hopes are much encouraged by having near the said situation several mountains, where there are abundance of fine solid rock crystals.

The diamonds that have been found, are commonly from 1 grain to 6 carats; some larger, and among these, one of 45 carats. Their colour, solidity, and their other properties, are the same with those of the oriental diamonds; only it was observed, that those that lay more superficially, and exposed to the air and sun, were more scurfy; and consequently lost more by polishing than the others.

Meteorological Observations, made for six Years at Padua. By Sig. Poleni. N° 421, p. 201. *Translated from the Latin.*

In the first place it is to be observed, that S. Poleni, in denoting the times, has, after the manner of astronomers, computed the beginning of each day from noon; and that he has made his observations a little after noon, unless otherwise prevented.

He made use of the old stile in designing the times, and of the English foot and its parts, in measuring. And, if in the progress of the observations, any of them be accommodated to the new stile, and French measures, he mentions that alteration.

In measuring the snow, he caused it to be melted, and then he measured it in the same manner as rain-water. The tube of his barometer is pretty large, and the diameter of the cistern or vessel, containing the stagnant mercury, is almost 20 times the diameter of the tube; therefore, in the ascent and descent of the mercury in the tube, the height of that in the vessel may be with safety considered as invariable.

His thermometer is one of those of M. Amonton's invention, with a recurve tube terminating in a phial, or ball, whose lower part is filled with quicksilver, and upper part with air; and by the greater or less dilatation of the air, according to the different degrees of heat, the mercury rises more or less in the tube; but because the extremity of the tube is open, the true height of the thermometer must be compounded, of the observed height of the mercury in the tube of the thermometer, and of the height of the mercury in the barometer, collected together into one sum; and that height be set down in the Ephemerides. On immersing the ball of his thermometer into ice, the mercury falls $47\frac{3}{10}$ inches; and into boiling water, it rises $63\frac{1}{10}$ inches 10 dec.

After premising these things, the following table exhibits the quantities of rain-water and of melted snow collected together, as the sums corresponding to each year, viz.

In the years	1725,	1726,	1727,	1728,	1729,	1730.
The inches	29.99,	25.33,	46.41,	52.83,	35.42,	34.30.

When the same months of these six years were collected together into one sum; it was found that the least quantity of water fell in the month of February; as not exceeding 7.74 inches; and that the greatest quantity fell in the month of October, which was 30.57 inches.

Besides, S. Poleni collected apart the numbers of the quantities of water, that fell in each season of the year; reckoning the seasons for each year, in such manner as to refer the beginning of winter to the 10th of December of the preceding year; and thus beginning the rest of the seasons at the 10th of March, June, and September, respectively. The sums found are exhibited n the following table.

	Winter. inches.	Spring. inches.	Summer. inches.	Autumn. inches.
1725	0.91	8.17	7.58	13.33
1726	2.81	9. 1	7.35	5.0
1727	8.28	5.92	11.87	15.50
1728	11.42	10.75	12.08	20.56
1729	7.47	9.43	6.31	13.62
1730	8.69	8.82	12.82	6.56
Sum	39.58	52.10	58.01	74.57

From which table it is evident that the respective quantities of water, in summer and autumn for every year, was greater than that in winter and spring.

If the respective quantities for each season be collected into one sum, and these sums be compared together, it will easily appear, that the increments proceed in the same order as the seasons do, beginning from winter; that is, that the least quantity of water is had in winter, a greater in spring, a still greater in summer, and the greatest of all in autumn.

If the height of the barometer, not of each year, but of all the 6 years, be collected into one sum only, the mean height of the barometer, corresponding to each day of all the said years, will be found to be 29.7 inches.

And if the heights of the thermometer, not of each year, but of all the 6 years, be collected into one sum only, the mean height of the thermometer, corresponding to each day of all the said years, will be found to be 50.16 inches.

Therefore, by bare inspection into the table, it is easy to understand, that the mean heights both of the barometer and thermometer, for each day of each year, differ but a very few parts from the mean heights of the days, that arise from these six taken collectedly.

In the next place, S. Poleni proceeds to his observations on the magnetic declinations; and these he discusses briefly. It is now well known, that at different hours of the same day, some small changes happen in the declination of the magnetic needle; so that the same constant declination is not to be observed for one entire day; but it varies sometimes a few minutes of a degree. It is besides well known, that different needles, especially those touched by different magnets, do not entirely exhibit the same declination, but sometimes vary some few, (and but very few, when the needles are made by good workmen,) minutes of a degree. Excepting therefore the very small variations, that easily arise from these causes, S. Poleni, for these whole 6 years, observed the declination of the magnet, 13° towards the west. The compass he makes use of, and on which he greatly depends, was made by Bernard Facinus, an artist well skilled in these matters, and very diligent. The needle is 6 inches long, and weighs 32 grains.

*An Account of the Coccus Polonicus.** *By M. Breynius.* N° 421, p. 216.

M. Breynius, after having briefly accounted for the two kinds of the cocci tinctorii now in use, viz. that of Pliny gathered from the ilex, and the American coccus or cochineal, proceeds to give the natural history of the coccus polonicus, which he calls radicum; because it is chiefly found adhering to the roots of the polygonum cocciferum, Kosmaczeh Polonis C. B. Kosmaczeh Pilosella Herbario Polon. This he takes to be the polygonum germanicum, incanum, flore majore perenni Raii: of which he has given a print, with the cocci, as they stick to the roots.

The coccus, he says, is found sometimes single, sometimes more, even to 40 adhering to one plant, of different sizes, from a poppy seed to that of a white pepper-corn. It is roundish, smooth, and of a purple violet colour, and in a thin cuticle incloses a blood-red succus: one half or more of it is covered with a rough, dark, brown crust, by which it adheres to the roots. The countrymen gather it about Midsummer, and dry it with a slow fire in earthen platters.

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* The insect here described is the coccus polonicus of Linnæus.

In open glasses he exposed to the sun several of these cocci, and found that by the 24th of July, every one, according to its size, had excluded a small worm with 6 feet. That part which seemed to be the head, had two short carneous antennæ; for, he could not perceive with glasses any thing either like mouth or eyes. On the back lengthwise there were two sulci, more or less visible, according to the different motions of the animalculum. Its feet seemed armed with claws, and the first pair stronger and darker than the rest. The whole animalculum was of an obscure purple colour, and had several bristles of a brown grey.

These, after 10 or 14 days, lay in a state of rest, and soon became covered with an exceedingly white fine lanuginous substance; in which condition they continued 5 or 8 days longer, and then laid their eggs, 50, 100, or more apiece; which to the naked eye appeared but like so many red oblongish points; but with glasses looked like ant's eggs, almost transparent with diluted blood-red contents.

These eggs, being again exposed in the sun about Bartholomew-tide, were hatched a month after, when some animalcula were excluded, which in the microscope appeared to be hexapods of a purplish hue, with two antennæ at their head, and two greyish bristles at their tails, scarcely visible except on black paper.

He supposes these last excluded animalcula, after some wanderings, at last fix themselves to the roots, and some of the lowest contiguous branches of the polygonum, where being deprived of local motion and sense, by some way or other, they imbibe that succus from the plant; and at last become the cocci so called, full of that blood-red succus so useful in dying.

A Botanical Invitation to forward a History of the Plants of Swisserland. By Dr. John Jacob Scheuchzer, M. D. F. R. S. sent to Sir Hans Sloane, Bart. Pr. R. S. to be communicated to the Royal Society. N° 421, p. 219.

Dr. S.'s work will be in the form of a dictionary, that it may serve at the same time for an index. He therefore was disposing in an alphabetical order, the various kinds and characters, subjoining to each of them the proper species hitherto observed in Swisserland, either by himself or by others, with the synonymous names used by the different authors, which are again to be inserted in their proper places, according to the order of the alphabet. He was adding, and remarking, under each plant, whatever seemed to be wanting in their description by other authors, or anywise necessary for the fuller knowledge of them, or applicable to medicinal or other uses. Besides other prints, there will appear in the work itself those of Fuchsius in folio, he having purchased the original plates. He would add all the Alpine plants, had he some patrons at hand who would bear the expence of engraving the plates.*

Of a large Umbilical Rupture. By Mr. Ranby. N° 421, p. 221.

About 6 years before, a man gave his wife a kick on the belly, and from that time she complained of a pain, and a swelling about the navel, which in time increased to about the size of a man's head, seldom giving her any uneasiness but by its weight; and that chiefly when her bandage was off, which she generally wore, except when her diet, or any other accident, brought on a diarrhœa, which was always attended with colic pains, particularly in the rupture; to ease which, she had been advised to iron it with a hot iron, and she had thereby burnt it so often, that there remained on the skin several large cicatrices. Three days before her death she was taken with the diarrhœa, attended with a slight fever.

On opening the bag, the caul first presented itself to view, the greatest part of which adhered to the peritoneum. Removing this, the small guts, to the length of 2½ ells, were contained in this bag, with all the colon, except so much of it as is below the left kidney; and the beginning of the colon, with the cæcum, was attached to the mesentery, in such manner, as to be but 2 inches distant from the pylorus; which, with about one-third of the stomach, was by this means drawn into the bag. The beginning of the duodenum just entered the bag, and then returned out again; which, with but a small portion of the jejunum, was the chief that remained in the abdomen.

A Catalogue of the fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1730; *pursuant to the Direction of Sir Hans Sloane, Bart. By Isaac Rand, F. R. S.* N° 422, p. 223.

Experiments concerning the Electricity of Water. By Mr. Stephen Gray. N° 422, p. 227.

In the former account of experiments in N° 417, Mr. Gray described the manner of communicating an attraction to a bubble of soaped water. But he has now found, that even a body of water receives an attractive virtue, and also a repelling one, by applying the excited tube near it, after the same manner as solid bodies do. To perform this experiment, he caused a wooden dish to be turned, with a screw hole at the bottom, but not so far as to come through the

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* A history of the plants of Switzerland was given about 10 years afterwards by the celebrated Haller, entitled Enumeratio Method. Stirp. Helvet. 2 Vols. folio with plates 1742; to which were annexed some supplements in 1761. The corrections and additions contained in these supplements were inserted in their proper places in a new edition of this great botanical work, published in 3 vols. folio 1768, under the title of Historia Stirpium Helvetiæ.

wood. This was screwed on to the upper end of one of the stands, formerly mentioned, the other top being taken off; the dish was about 4 inches diameter, and 1 inch deep. Then the stand was set on a cake of rosin, or a plate of glass, or the brims of a drinking glass, or of a cylindric one, such as are used for water glasses. The glass must be first warmed; then the dish being filled with water, the tube rubbed, and moved both under the dish and over the water 3 or 4 times, without touching them. After it has been excited, not only the dish, but the water also, becomes electric; and if a small piece of thread, or a narrow slip of thin paper, or a piece of sheet brass, commonly called tinsel, be held over the water in a horizontal position, within about an inch or sometimes more, any of the said bodies will be attracted to the surface of the water, and be repelled, but not so often as by solids. If a pendulous thread be held at some distance from the outside of the dish, it will be attracted and repelled by it many times together, with a very quick motion, but not at so great a distance as when the dish is empty.

An Experiment, showing that Water is attracted by the Tube, and that the Attraction is attended with several remarkable and surprising Phænomena.—This experiment being to be made with small quantities of water, Mr. Gray at first made use of some of the brass concave little dishes in which he formerly ground microscopes; but has since caused a more convenient apparatus to be made, which consists of a small pedestal of about 4½ inches long, the base of ivory about 2 inches diameter. On the upper end, as in the larger stand, there is a screw, upon which is screwed one of the little ivory dishes; of which he has several sizes, from three quarters to one-tenth of an inch diameter. When any one of these little vessels is filled with water, so that it may stand above the brims of the cup, and has acquired a spherical surface, as it will do in the smallest cups, let it be set on the table with the little stand to which it had before been screwed, or, which is better, on the larger stand mentioned above, the great dish being taken off, and the small plain top screwed on; being thus prepared, let the tube be excited, and held over the water at the distance of about an inch or more. If it be a large tube, there will first arise a little mountain of water from the top of the drop, of a conical form, from the vertex of which there proceeds a light, very visible when the experiment is performed in a dark room, and a snapping noise, almost like that when the fingers are held near the tube, but not quite so loud, and of a more flat sound. On this immediately the mountain falls into the rest of the water, and puts it into a tremulous and waving motion. Having since repeated this experiment in the daytime, where the sun shined, he perceived that there were small particles of water thrown out of the top of the mount, and that sometimes there would arise a very fine stream of water from the vertex of the cone, in the manner of a fountain, from which there issued a fine steam, or vapour, whose particles were so small as not to be seen; yet it is certain that it must be so, since the under side of the tube was wet; and though there does not always arise that cylinder of water, yet there is always a steam of invisible particles thrown on the tube, and sometimes to that degree as to be visible on it. When some of the larger cups are used, they are to be filled as high as may be, without running over; the surface will be flat about the middle part; but when the tube is held over it, the middle part will be depressed into a concave, and the parts towards the edge be raised; and when the tube is held over against the side of the water, the little conical protuberance of water issues out with its axis horizontally, and after the crackling noise, returns to the rest of the water; and sometimes there will be thrown out of it small particles of the same, as from the smaller portions of water above-mentioned.

The last experiment was repeated with hot water, when the water was attracted much stronger, and at a much greater distance: the steam arising from the vertex was in this case visible, and the tube was sprinkled with large drops of water. Mr. G. tried the experiment in the same manner on quicksilver, which was likewise raised up; but by reason of its great weight, not so high as the water; the snapping noise was louder, and lasted much longer than in the water.

The Method of making the best Mortar at Madras in the East Indies. By Isaac Pyke, Esq. Governor of St. Helena. N° 422, p. 231.

Take 15 bushels of fresh pit-sand, well sifted: add to it 15 bushels of stone-lime: let it be moistened or slacked with water in the common manner, and so laid 2 or 3 days together. Then dissolve 20 lb. of jaggery, which is coarse sugar, or thick molasses, in water, and sprinkling this liquor over the mortar, beat it up together till all is well mixed and incorporated, and then let it lie by in a heap. Then boil a peck of gram, (which is a sort of grain like a tare, or between that and a pea,) to a jelly, and strain it off through a coarse canvas, preserving the liquor that comes from it.

Take also a peck of myrabolans, and boil them likewise to a jelly, preserving that water also as the other; and if you have a vessel large enough, you may put these three waters together, that is, the jaggery water, the gram water, and the myrabolan. The Indians usually put a small quantity of fine lime in it, to keep their labourers from drinking it.

The mortar beaten up, and when too dry, sprinkled with this liquor, proves extraordinary good for laying brick or stone, keeping some of the liquor always at hand for the workman to wet his bricks with; and if this liquor prove too thick, dilute it with fresh water.

Observe also, that the mortar here is not only to be well beaten and mixed together, but also laid very well, and every brick, or piece of brick, flushed in with the mortar, and every cranny filled up, yet not in thick joints, like the common English mortar; and also over every course of bricks, some to be throwed on very thin; and where the work has stood, though but for a breakfast or a dining-time, before you begin again, wet it well with this liquor with a ladle, and then lay on your fresh mortar; for this mortar, notwithstanding its being thus wetted, dries much sooner than one not used to it would conceive, but especially in hot weather.

For some very strong work, the same mortar above is improved as follows: Take coarse tow, and twist it loosely into bands as thick as a man's finger, (in England ox-hair is used instead of this tow) then cut it into pieces of about an inch long, and untwist it so as to lie loose; then strew it lightly over the other mortar, which is at the same time to be kept turning over, so that this stuff be beaten into it, keeping labourers continually beating in a trough, and mixing it till it be well incorporated with all the parts of the mortar. And, as it will be subject to dry very fast, it must be frequently softened with some of the aforesaid liquor of jaggery, gram, and myrabolans, and some fresh water; and when it is so moistened and beaten, it will mix well; and with this they build, though it be not usual to build common house-walls thus, when the work is intended to be very strong, as for instance, Madras church steeple, which was building when Mr. Pyke was last there; and also for some ornaments, as columns, good arched work, or imagery set up in gardens, it is thus made.

Though for common buildings about Madras, where the rainy season holds not above 3 months in the year, and sometimes less, they usually lay all the common brick-work in a loamy clay, and plaster it over on both sides with this mortar, which is yet further to be improved.

Having the mortar thus prepared, as above, separate some of it, and to every half bushel, take the white of 5 or 6 eggs, and 4 oz. of ghee, or ordinary unsalted butter, and a pint of butter-milk, beaten all well together: mix a little of the mortar with this, till all the ghee, whites of eggs, and butter-milk be soaked up; then soften the rest well with plain fresh water, and so mix all together, and let it be ground, a trowel full at a time, on a stone with a stone-roller, in the same manner that chocolate is usually made, or ground in England; and let it stand by in a trough for use. When you use it, in case it be too dry, moisten it with some water, or the before mentioned liquor. This is the second coat of plastering.

When the first coat of plastering is laid on, let it be well rubbed on with a hardening trowel, or with a smooth brick, and strewed with a gritty sand, moistened, as occasion requires, with water, or the before-mentioned liquor, and then well hardened on again; which, when half dry, take the last mentioned composition for the fine plastering; and when it is almost dry, lay on the whitening varnish; but if the work should be quite dry, then the chinam liquor must be washed over the work with a brush.

The best sort of whitening varnish is thus made. Take one gallon of toddy, a pint of butter-milk, and as much fine chinam, or lime, as shall be proper to colour it; add to it some of the chinam liquor, washing it gently over with it; and when it is quite dried in, do the same again. A plaster thus made is more durable than some soft stone, and holds the weather better in India, than any of the bricks they make there.

In some of the fine chinam that is to endure the weather, and where it is likely to be subject to much rain, they put * gingerly oil instead of ghee; and also in some they boil the bark of the mango-tree, and other barks of astringent natures, and aloes, which grow here in great plenty by the sea-shore; but to all of the fine chinam, for outside plastering, they put butter-milk, which is here called toyre. And for inside work, they use glue made very thin and weak, instead of size, for white-washing; and sometimes they add a little gum to it.

As several ingredients here mentioned, are not to be had in England, it may not be amiss to substitute something more plentiful here, which may be of the same nature. As to all the astringent barks, oaken-bark may be as good as any. Instead of aloes, either turpentine, or the bark and branches of the sloe-tree. Though turpentine be not so strong, yet, if used in greater quantity, may serve to the same purpose. But there is a sort of aloes hepatica, often very cheap. Instead of myrabolans, some juice of aloes [sloes;] also instead of jaggery, coarse sugar, or molasses, will do; instead of toddy, which is a sort of palm-wine, the liquor from the birch-tree comes near to it.

Note, That in China, and some other parts, they temper their mortar with blood of any sorts of cattle; but the ingredients before mentioned are said to be as binding, and do full as well, and do not make the mortar of so dark a colour. The plastering above described, is thought in India vastly to exceed any sort of stucco-work, or plaster of Paris; and Mr. Pyke has seen a room done with this sort of terrass mortar, that has fully come up to the best sort of wainscot-work, in smoothness and in beauty.

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* Oleum Sesami.

A Letter from Dr. Huxham to Jas. Jurin, M. D., F. R. S. and Fellow of the Coll. of Physicians, concerning a remarkable Disease of the Colon. N° 422, p. 236. *From the Latin.*

A gentleman of rank and fortune in Devonshire, aged 40, of a bilious and scorbutic habit of body, was long afflicted with colicky pains, especially in the lower part of the abdomen, accompanied with flatulency. About 2 years before his death, he was, in addition to these symptoms, troubled with bilious, purulent, fetid stools, which were occasionally streaked with blood, and were so frequent, that he often had as many as 20 motions, attended with tenesmus, in the space of 3 or 4 hours; and at length there came away portions of fungous flesh, livid and very foul, some of which were as large as a nutmeg. But though he was generally troubled with a looseness, yet he was sometimes so costive, as to require clysters and cathartics. Sometimes he had a voracious appetite, at other times none at all. His urine was scanty, and always bilious. His countenance was lurid, often tinged with yellow. Before he died he had œdematous swellings of the feet, and was delirious. He had taken a great variety of medicines in vain; nothing but laudanum afforded him even temporary relief.

On opening the body, the omentum was found very much wasted, and in a putrid state; the liver much enlarged, and abounding in hard white tubercles; the gall-bladder half full of dark coloured bile; the whole of the duodenum with the adjoining portion of the colon, tinged of the same colour; the pancreas in a scirrhous state; the middle part of the ileum, to the extent of about 5 fingers, inflamed, and almost livid. The kidneys were in a sufficiently sound state, nor were the mesenteric glands so scirrhous as might have been expected. But what was most remarkable, that part of the colon which has been improperly termed cæcum (a name which is more applicable to its appendix) was not attached, as it commonly is, to the right kidney by means of the vermiform appendix; but, having descended into the pelvis about 3 fingers below the valvula Tulpii, had formed a strong adhesion to the upper part of the rectum. Moreover, it adhered slightly to the peritonæal covering of the urinary bladder, and thence turning upwards, it formed a very acute angle with the rectum; then ascending under the concave surface of the liver, it stretched beneath the fundus ventriculi, and descending in the usual manner terminated in the rectum. Both intestines were in a gangrenous state. The rectum being laid open, its internal surface was found entirely sphacelated, and as black as ink, and there were adhering to it 6 or 7 fungous, black caruncles, the smallest of which were of the size of a filbert. An ulcer large enough to admit one's finger, was also discovered penetrating from the rectum into the colon at the place where these intestines adhered; and these intestines were so putrid that they would scarcely bear to be handled. Although the patient had been troubled with a looseness before his death, yet the greater portion of the colon was stuffed up with indurated fæces; the liquid parts of the fæcal matter having passed directly into the rectum through the ulcerated orifice, while the more solid parts were retained in the colon. The urinary bladder was extremely flaccid, and its internal surface was lined with a reddish mucus.

Two Problems concerning the Figures assumed by Revolving Fluids; with Conjectures concerning Stars which sometimes appear and disappear; and on Saturn's Ring. By Peter Lewis de Maupertuis, F. R. S. &c.* N° 423, p. 242. *Translated from the Latin.*

PROB. 1.—To find the figure of a fluid spheroid revolving about an axis; supposing the parts of the fluid attracted to the centre according to any power of the distance from the centre.

Let PQ, fig. 6, pl. 12, be the axis of rotation, and PAQB a section of the

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* Peter Lewis Moreau de Maupertuis, a celebrated member of the French Academy, and of the Academies of Sciences of Paris, Berlin, and London, &c. was born Sept. 1698, at St. Malo. He served in the army at first as a musqueteer, and rose to the rank of a colonel; but afterwards quitted the service, and devoted himself entirely to the mathematical sciences. In 1736 he was placed at the head of the academicians sent to the north, to measure a part of the meridian there, for determining the figure of the earth. After his return he was invited to Berlin by the King of Prussia, and appointed president of the academy there. Maupertuis followed that prince to the field, exposed his life bravely, was taken prisoner, and carried to Vienna, where he was loaded with marks of favour by the emperor and empress. One of the anecdotes mentioned on this occasion is pleasant enough: M. lamenting that the hussars, who took him prisoner, among other things, had plundered him of a curious watch made by Graham of England; the emperor having another made by the same artist, but enriched with diamonds, presented him with it, saying, " it was only a little trick the hussars put upon you; they have brought your watch to me, and I now return it to you."

Maupertuis soon returned to Berlin; where amidst all the honours and pleasures the Prussian monarch lavished on him, he could not conquer the unhappy discontentedness and irritability of his temper; his disputes with Koenig and Voltaire embittered his life. Koenig was expelled from the academy, and Voltaire from Berlin. But M. was still unhappy there, and returned into France in 1756, under pretence of recovering his health. And in 1758 going to visit his friends, the Bernoullis at Basil, he died with them in the summer of 1759.

Maupertuis's writings discover genius, fire, and imagination; but not always a deep knowledge of mathematics, nor much solidity or judgment.

The above paper is the only one of his in the Philos. Trans. but his memoirs in those of the Academy of Sciences, and elsewhere, are very numerous. His works have been collected, and published in 4 vols. 8vo. 1768; but the above paper in the Philos. Trans. is not among them.

spheroid through the axis. Now since the parts of the fluid are at rest among themselves, every column CD will have the same weight towards C; considering therefore one column CD, which makes with CP a given angle, whose sine is $= h$, to the radius 1, and which is composed of an infinite number of small cylinders; I find the weight of any small cylinder towards C.

Since the absolute gravity at A is given and $= p$, to have the gravity at G, it will be $p : p^1 :: \mathrm{CA}^n : \mathrm{CG}^n$; hence is the gravity at G, or $p^1 = \frac{p.\mathrm{CG}^n}{\mathrm{CA}^n}$.

But since, because of the rotary motion, any part of the fluid is repelled by the centrifugal force in the direction GH; and since, in motions arising from contemporaneous rotations, the centrifugal forces are as the radii of the circles described; if the centrifugal force at A be given and $= f$, to have the centrifugal force at G, it will be $f : f^1 :: \mathrm{CA} : \mathrm{LG} =$ (because $\mathrm{LG} : \mathrm{CG} :: h : 1$) $h.\mathrm{CG}$; hence the centrifugal force in G, or $f^1 = \frac{fh.\mathrm{CG}}{\mathrm{CA}}$: but this force, since it acts in direction GH, is decompounded into the two forces GK, KH, of which GK is the only part acting in the direction CG. Therefore this force GK will be had by saying as $\mathrm{GH} : \mathrm{GK}$ or as $1 : h :: \frac{fh.\mathrm{CG}}{\mathrm{CA}} : f^1 = \frac{fhh.\mathrm{CG}}{\mathrm{CA}} =$ the force of the small cylinder Gg towards D. Therefore the force of that cylinder Gg towards C, will be only $\frac{p.\mathrm{CG}^n}{\mathrm{CA}^n} - \frac{fhh.\mathrm{CG}}{\mathrm{CA}}$; and the weight of that cylinder towards C, will be $\left(\frac{p.\mathrm{CG}^n}{\mathrm{CA}^n} - \frac{fhh.\mathrm{CG}}{\mathrm{CA}}\right) \mathrm{G}g$. Now since G$g$ is the element of CG, therefore the integral will give $\frac{p.\mathrm{CG}^{n+1}}{n+1.\mathrm{CA}^n} - \frac{fhh.\mathrm{CG}^2}{2\mathrm{CA}}$ for the weight of the column CG; and $\frac{p.\mathrm{CD}^{n+1}}{n+1.\mathrm{CA}^n} - \frac{fhh.\mathrm{CD}}{\mathrm{CA}}$ for the weight of the whole column CD, which ought to make a constant weight A.

If therefore there be called $\mathrm{CA} = a$, $\mathrm{CD} = r$, it will be $\frac{pr^{n+1}}{n+1.a^n} - \frac{fhhrr}{2a} = \mathrm{A}$. And since this equation, whatever be the value of h, will always obtain, if h be assumed as indeterminate, the preceding equation will give the relation between any radius CD and the sine of the angle it makes with the axis PQ.

Now the constant quantity A is to be determined. That the preceding equation be adapted to the section of that spheroid whose semiaxis is $\mathrm{CA} = a$, when DCP is a right angle, or when $h = 1$, then will $r = a$; hence it will be $\frac{pa^{n+1}}{n+1.a^n} - \frac{faa}{2a} = \mathrm{A}$, or $\mathrm{A} = \frac{2p - nf - f}{2.n+1}a$.

And thus the equation corrected will be $\frac{pr^{n+1}}{n+1.a^n} - \frac{fhhrr}{2a} = \frac{2p - nf - f}{2.n+1}a$, or $2pr^{n+1} - (n+1) fh^2r^2a^{n-1} = (2p - nf - f) a^{n+1}$.

This equation determines the sections of every spheroid, whatever be the power of the distance by which the attraction acts; excepting only the hypothesis in which the attraction is in the simple ratio of the distance from the centre inversely.

In that case the general quantity $\left(\frac{p.\text{CG}^n}{\text{CA}^n} - \frac{fh^2.\text{CG}}{\text{CA}}\right)$ Gg will be $\left(\frac{p.\text{CG}}{\text{CA}} - \frac{fh^2.\text{CG}}{\text{CA}}\right)$ Gg, whose fluent can only be had by logarithms, and gives $p.\text{CA}$ log. CG $- \frac{fh^2.\text{CG}^2}{2\text{CA}} = \text{A}$; or for the whole weight of the column palog. $r - \frac{fh^2r^2}{2a} = \text{A}$.

To correct this equation, when $h = 1$, then must $r = a$, and then palog. $a - \frac{1}{2}fa = \text{A}$; hence the equation corrected is pa log. $r - \frac{fh^2r^2}{2a} = pa$log. $a - \frac{fa}{2}$, or $2\,pa$ log. $\frac{r}{a} = \frac{fh^2r^2}{a} - fa$; or taking the natural numbers of these logarithms, and putting $c =$ the number whose logarithm is 1, then is had $r = ac^{\left(\frac{fhhrr}{2paa} - \frac{f}{2p}\right)}$.

Hence it appears that the meridians of the spheroids will always be algebraical curves, except only in this last hypothesis.

If the equation of all these curves be desired in the usual manner, by perpendicular co-ordinates, it may be easily done, thus: taking CE $= x$, and DE $= y$, there will be $r^2 = x^2 + y^2$, and $hr = y$. Then, exterminating h and r from the general equation, there will result

$$2p(x^2 + y^2)^{\frac{1}{2}n + \frac{1}{2}} - (n + 1)fa^{n-1}y^2 = (2p - nf - f)a^{n+1}.$$

And when $n = -1$, then $x^2 + y^2 = a^2c^{\left(\frac{fyy}{paa} - \frac{f}{p}\right)}$.

But our former method, of defining the curves by radii and angles, is as well, and perhaps more commodious than that which defines them by co-ordinates.

Though h be treated as a variable quantity, yet it varies not beyond certain limits, which are 0 and 1; so that our radial equation will only define the part of this curve whose amplitude is a right angle; but when those curves consist of four similar and equal arcs, the whole curves of all the meridians will be given by our equations.

It will now be easy to determine the ratio between the two arcs of the section in any hypothesis.

Since the general equation is $2pr^{n+1} - (n + 1)fh^2r^2a^{n-1} = (2p - nf - f)a^{n+1}$; to find r when $h = 0$, it will be $2pr^{n+1} = (2p - nf - f)a^{n+1}$; from whence results CA : CP :: $(2p)^{\frac{1}{n+1}} : (2p - nf - f)^{\frac{1}{n+1}}$.

And in the case of the gravity being in the simple reciprocal ratio of the distance, it will be log. $\frac{r}{a} = -\frac{f}{2p}$, or the log. of CA − log. of CP $= \frac{f}{2p}$.

It appears that, n being an affirmative number, either integer or fraction, that is in every hypothesis of gravity being directly proportional to any power of the distance, the equatorial diameter will always be greater than the axis of rotation. But if n be any negative number, that is, if gravity be inversely proportional to any power of the distance, then will $\text{CA} : \text{CP} :: (2p)^{\frac{1}{1-n}} : (2p - nf - f)^{\frac{1}{1-n}}$. Now if n be less than 1, make $k = 1 - n$, and there will be $\text{CA} : \text{CP} :: (2p)^{\frac{1}{k}} : (2p - kf)^{\frac{1}{k}}$; but if n be greater than 1, make $n - 1 = k$, and then $\text{CA} : \text{CP} :: (2p)^{\frac{1}{-k}} : (2p + k)^{\frac{1}{-k}}$, or $\text{CA} : \text{CP} :: (2p + kf)^{\frac{1}{k}} : (2p)^{\frac{1}{k}}$. Further, when $n = -1$, we found that log. of CA − log. of CP $= \frac{f}{2p}$. So that it appears there is no hypothesis in which the equatorial diameter is not greater than the diameter of the meridian.

It is sufficiently apparent that the figure of the spheroid depends on the ratio of the centrifugal force to gravity. Now what that ratio can be may always be seen in any hypothesis, and that hence the figure of the spheroid will result.

If gravity be supposed uniform, it will be $n = 0$, and then $\text{CA} : \text{CP} :: 2p : 2p - f$. Therefore in the earth, where the centrifugal force at the equator is equal to the 289th part of gravity, if there be sought the ratio of the equatorial diameter to the axis in the hypothesis of a uniform gravity, putting 289 for p, and 1 for f, then $\text{CA} : \text{CP} :: 578 : 577$.

The centrifugal force may be equal to gravity, which would happen if the diurnal rotation were 17 times quicker; and then $\text{CA} : \text{CP} :: 2 : 1$. But should the rotation become greater and greater, the parts would be successively dissipated, till at length the earth would be reduced to a mere atom. Hence it appears that in this hypothesis of a uniform gravity, the earth cannot be more depressed at the poles than when the equatorial diameter is double the axis of rotation. In this case the earth will consist of two paraboloids, like as found by Huygens, in his tract on the Cause of Gravity, for that particular hypothesis which he only examined.

If gravity be made proportional to the distance from the centre; then will $n = 1$, and $\text{CA} : \text{CP} :: \sqrt{p} : \sqrt{(p - f)}$. If therefore the centrifugal force be equal to the force of gravity, the equatorial diameter would be infinitely greater than the axis of rotation: that is, the spheroid would become only a circular plane. And since, in this hypothesis, the centrifugal force can have to gravity all ratios, from nothing up to the ratio of equality, it appears that the equatorial diameter can have to the axis of rotation all ratios; and that the spheroid, which in this hypothesis is always an ellipsoid, can be all ellipsoids, from a perfect sphere down to the circle. But in this hypothesis also the centrifugal force cannot increase further.

If gravity be taken reciprocally proportional to the square of the distance, then will $n = -2$, and CA : CP :: $2p + f : 2p$. From which it appears that, in this hypothesis, the centrifugal force may continually increase, or, which comes to the same thing, that the rotation can be continually quicker, without the parts of the spheroid being dissipated.

SCHOLIUM.—But, of all these hypotheses, there cannot be usurped any one as really given in nature; as I do not suppose that the interior parts of bodies gravitate towards any one centre, according to any proportion of the distances from that centre. The attraction of the parts depends on the form of the body, and interchangably the form depends on the attraction. Therefore all these determinations are more mathematical than physical. Hence it is that Newton, in the determination of the axis and equatorial diameter of the earth, found the ratio different from that of Huygens and ours, namely that of 229 to 230. That great man neglected the solution merely geometrical from hypothesis, that he might give it more agreeable to nature.

PROBLEM II.—Supposing that the matter flowing about an axis taken without the fluid, be attracted towards a centre posited in that axis, by a force proportional to any power of the distance from the centre; while at the same time, because of the mutual attraction of the parts of the fluid, there is made another attraction towards another centre taken within the fluid, which in any section made through the exterior centre perpendicularly to the fluid of rotation, is proportional to any power of the distance from the interior centre: to find the figure the fluid will take.

Let ADP*ad*QA, fig. 7, be a section of the fluid gyrating about the axis Λλ, drawn perpendicularly through the plane of rotation passing through the centre γ. Let γ be the centre of the centripetal forces taken without the fluid; and C the centre taken in the section towards which the parts of the fluid are attracted.

That the parts of the fluid may remain in equilibrio, the weight of any column CD by gravitating towards γ, and towards C, and by a centrifugal force, should remain every where the same.

Let then the gravity at A towards γ be given, and $= \pi$, the gravity at A towards C be given, and $= p$, and the centrifugal force at A also given, and $= f$. Put AC $= a$, C$\gamma = b$, $cg = r$, and the sine of the angle DCP $= h$, to the radius $= 1$; then will GL $= hr$, and from γ demitting the perpendicular γR on the radius CD produced, it will be CR $= bh$, and γG $=$ (by Eucl. ii, 12) $\surd(bb + 2bhr + rr)$.

Now since the gravity at A towards γ is $= \pi$; by saying $\pi : \pi' :: (a + b)^m : (bb + 2bhr + rr)^{\frac{1}{2}m}$, the gravity at G or $\pi' = \frac{\pi (bb + 2bhr + rr)^{\frac{1}{2}m}}{(a + b)^m}$.

And to derive that towards C, say $\pi' : \pi'' ::$ Gγ : GR, or $\frac{\pi (bb + 2bhr + rr)^{\frac{1}{2}m}}{(a + b)^m} : \pi'' :: (bb + 2bhr + rr)^{\frac{1}{2}} : bh + r$; hence is had the force from the attraction towards γ, derived towards C, or $\pi'' = \frac{\pi (bh + r) . (bb + 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(a + b)^m}$.

There will further be had, since the gravity at A towards C is $= p$, the gravity of G towards C $= \frac{pr^n}{a^n}$; therefore the whole gravity towards C, arising from both the gravities towards γ and C, will be $\frac{\pi (bh + r) . (bb + 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(a + b)^m} + \frac{pr^n}{a^n}$.

Now since the centrifugal force at A is $= f$; by saying $f : f' :: a + b : b + hr$, there will be the centrifugal force at G, or $f' = \frac{f(b + hr)}{a + b}$; and to find the part of that force which draws towards D, make $f' : f'' ::$ GH : GK, or $\frac{f(b + hr)}{a + b} : f'' :: 1 : h$; hence there is had the force opposite to the gravity towards C, or $f'' = \frac{fh (b + hr)}{a + b}$.

Therefore the force towards C, resulting from all these forces, will be $\frac{\pi (be + r) . (bb + 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(a + b)^m} + \frac{pr^n}{a^n} - \frac{fh (b + hr)}{a + b}$.

Conceiving then, as in the first problem, the column CD, composed of an infinity of small cylinders $\dot{r}$, there will be F $\left(\frac{\pi (bh + r) . (bb + 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(a + b)^m} + \frac{pr^n}{a^n} - \frac{fh (b + hr)}{a + b}\right) \dot{r}$, which must be equal to some constant weight. Therefore

$$\frac{\pi (bb + 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(m + 1) . (a + b)^m} + \frac{pr^{n + 1}}{(n + 1) . a^n} - \frac{fbhr}{a + b} - \frac{fhhrr}{2 (a + b)} = \text{A}.$$

To correct this equation, when $h = 1$, it must be $r = a$; then there will result $\frac{\pi (a + b)}{m + 1} + \frac{pa}{m + 1} - \frac{fab}{a + b} - \frac{faa}{2 (a + b)} = \text{A}$. And the equation corrected will be $\frac{\pi (bb + 2bhr + rr)^{\frac{1}{2}m + \frac{1}{2}}}{(m + 1) . (a + b)^m} + \frac{pr^{n + 1}}{(n + 1) a^n} - \frac{fbhr}{a+b} - \frac{fhhrr}{2(a+b)} = \frac{\pi(a+b)}{m+1} + \frac{pa}{n+1} - \frac{fab}{a+b} - \frac{faa}{2(a+b)}$.

Or, by writing C for $a + b$, and q for $(m + 1) \times (n + 1)$; then $2 (n + 1) \pi a^n (bb + 2bhr + rr)^{\frac{1}{2}m + \frac{1}{2}} + 2 (m + 1) pc^m r^{n + 1} - 2 qfa^n bc^{m - 1} hr - qfa^n c^{m - 1} hhrr = 2 (n + 1) \pi a^n c^{m + 1} + 2 (m + 1) pa^{n + 1} c^m - 2qfa^{n + 1} bc^{m - 1} - qfa^{n + 2} c^{m - 1}$.

It appears in every hypothesis, that the section of the fluid is an algebraic curve, except only that of the attraction towards γ or towards C being in the simple inverse ratio of the distance; for if it be only $m = -1$, the fluent for the section will be, $\frac{\pi(a+b)}{2} \log.(bb + 2bhr + rr) + \frac{pr^{n+1}}{(n+1)a^n} - \frac{fbhr}{a+b} - \frac{fhhrr}{2(a+b)} =$ $= \frac{\pi(a+b)}{2} \log.(a+b)^2 + \frac{pa}{n+1} - \frac{fab}{a+b} - \frac{faa}{2(a+b)}$; or $\frac{\pi c}{2} \log. \frac{bb + 2bhr + rr}{cc} = -\frac{pr^{n+1}}{(n+1)a^n} + \frac{fbhr}{c} + \frac{fhhrr}{2c} + \frac{pa}{n+1} - \frac{fab}{c} - \frac{faa}{2c}$.

And if only $n = -1$, then it will be $\frac{\pi(bb + 2bhr + rr)^{\frac{1}{2}m + \frac{1}{2}}}{(m+1).(a+b)^m} + pa \log. r - \frac{fbhr}{a+b} - \frac{fhhrr}{2(a+b)} = \frac{\pi(a+b)}{m+1} + pa \log. a - \frac{fab}{a+b}$ $- \frac{faa}{2(a+b)}$; or $pa \log. \frac{r}{a} = -\frac{\pi(bb + 2bhr + rr)^{\frac{1}{2}m + \frac{1}{2}}}{(m+1)c^m} + \frac{fbhr}{c} + \frac{fhhrr}{2c} + \frac{\pi c}{m+1} -$ $\frac{fab}{c} - \frac{faa}{2c}$.

But if both $m = -1$ and $n = -1$ together; then it will be $\frac{\pi(a+b)}{2} \log.(bb + 2bhr + rr) + pa \log. r - \frac{fbhr}{a+b} - \frac{fhhrr}{2(a+b)} =$ $= \frac{\pi(a+b)}{2} \log.(a+b)^2 + pa \log. a - \frac{fab}{a+b} - \frac{faa}{2(a+b)}$; or $\frac{\pi c}{2} \log. \frac{bb + 2bhr + rr}{cc} + pa \log. \frac{r}{a} = \frac{fbhr}{c} + \frac{fhhrr}{2c} - \frac{fab}{c} - \frac{faa}{2c}$.

If there be desired the equation for the section of the fluid by perpendicular co-ordinates, making CE $= x$, and DE $= y$, there will result these two equations, $rr = xx + yy$, and $hr = y$, by means of which r and h will be exterminated from the equations above found; and there will result for the general case, $2(n+1)\pi a^n (bb + 2by + yy + xx)^{\frac{1}{2}m + \frac{1}{2}} + 2(m+1) pc^m (xx + yy)^{\frac{1}{2}m + \frac{1}{2}} -$ $2qfa^n bc^{m-1} y - qfa^n c^{m-1} yy = 2(n+1) \pi a^n c^{m+1} + 2(m+1) pa^{n+1} c^m -$ $2qfa^{n+1} bc^{m-1} - qfa^{n+2} c^{m-1}$.

And in like manner, in the cases of $m = -1$, $n = -1$, will be found the equations for the perpendicular co-ordinates.

As we have found the curve PAQ, so also, mutatis mutandis, may the curve PaQ be found. For then, if the gravity at a towards γ be given and $= \pi$, and the gravity at a towards C $= p$, and the centrifugal force at $a = f$; also C$a = a$, C$\gamma = b$, C$g = r$, $gl = hr$, and $\gamma g = \sqrt{(bb - 2bhr + rr)}$, there will be found the gravity at g towards C, arising from the attraction towards γ, $\pi'' = \frac{\pi(bh - r).(bb - 2bhr + rr)^{\frac{1}{2}m - \frac{1}{2}}}{(b-a)^m}$.

There will also be had for the gravity at g towards C, $p' = \frac{pr^n}{a^n}$.

Also the part of the centrifugal force towards C, $f'' = \frac{fh(b - hr)}{b - a}$.

But these latter forces are opposite to the former. Therefore

$$F\left(\frac{\pi(-bh+r).(bb-2bhr+rr)^{\frac{1}{2}m-\frac{1}{2}}}{(b-a)^m}+\frac{pr^n}{a^n}+\frac{fh(b-hr)}{b-a}\right)\dot{r}=A.$$

Hence is deduced $\frac{\pi(bb-2bhr+rr)^{\frac{1}{2}m-\frac{1}{2}}}{(m+1).(b-a)^m}+\frac{pr^{n+1}}{(m+1)a^n}+\frac{fbhr}{b-a}-\frac{fhhrr}{2(b-a)}=$
$=\frac{\pi(b-a)}{m+1}+\frac{pa}{n+1}+\frac{fab}{b-a}-\frac{faa}{2(b-a)}$.

And in the cases when $m=-1$, $n=-1$, there will be found, as above, the equations of the sections, only changing the signs where they ought to be changed.

And by these radial equations may be found the equations to the co-ordinates, as was done for the curve PAQ.

And since the weight of the column, both in the superior and in the inferior curve, ought to be the same, there will be had an equation between the weight A in the superior curve, and the weight A in the inferior; from which there will be determined *ca*, to the CA before determined; and thus the whole section of the fluid will be determined.

Whatever the hypothesis of gravity be, the radius CD can be always obtained of a given length, for any given angle DCP, and thus the figure of the fluid be made either thicker or thinner, and that indeed in infinite ways; by putting determinate values in the equation for h and r. Thus, the points P and Q can be made to coincide, by writing o for h and r; and then the section of the fluid will consist of two oval figures joined at C. For there are an infinity of ratios among π, p, and f, which will agree to that determination.

For example, if the last be required, viz. that P and Q may coincide in C, there will result

$2(n+1)\pi b^{m+1}=2(n+1)\pi c^{m+1}+2(m+1)pac^m-2qfabc^{m-1}-qfaac^{m-1}$.
Hence will arise infinite ratios among π, p, and f.

If we suppose the gravity both towards γ and towards C, to be in the simple ratio of the distance from the centre; the section of the fluid will be a conic section. And if it be then required that the points P, Q, C, may coincide, the figure will consist of two ellipses joined together at C.

Now if the distance Cγ vanish, or the two centres coincide; then will $b=o$, and $e=a$; and the fluid will form a spheroid.

Further, if there be put $m=n$, and $\pi=o$, the general equation for the section of the fluid will become $2pr^{n+1}-(n+1)fa^{n-1}hhrr=(2p-nf-f)a^{n+1}$. Or in the case of $n=-1$, it is $2pa\log.\frac{r}{a}=\frac{fhhrr}{a}-fa$, as we found in the first problem, which is only a special case of this.

SCHOLIUM.—This consideration of the figures which fluids may take, according to the different ratio of gravity to the centrifugal force, suggested to M. Maupertuis, that probably the planets have such forms; since for this there is only necessary a swifter motion round the axis, or a less density of matter: for, though few planets, that we know of, come sufficiently near a spheroidal figure, why may we not admit of other forms, either about other suns, or even our own? these lentiform planets would never be seen by us, either by reason of their distance, or because they would be in the plane of the ecliptic, or in a plane somewhat inclined to it, to which plane their axis of revolution would be perpendicular, or nearly so: for, in this situation they could not be seen from the earth.

And why might not such a variety of forms obtain among the fixed stars? especially, since it is very probable, that they revolve round their axis, like our sun. There are probably lentiform fixed stars in the heavens; and probably they are surrounded with very excentric planets, or comets, which, since they are not fixed in the plane of the equator, when they approach the perihelion, disturb the direction of the star's axis; and then the star, which by reason of its situation now disappears, appeared; or that which appeared before, now disappears. And so a reason might be assigned, why some stars seem to appear and disappear alternately.

But if in any system a comet with a tail move near some powerful planet, what will be the consequence? why, the matter emitted from the body of the comet, will be attracted round the planet; and by the comet's sending out new matter, or a sufficient quantity being already emitted, there will arise a continual flux of matter round the planet; and though the column, emitted from the comet, may at first be either of a cylindrical, conical, or any other form, yet its centrifugal force, with the gravities arising both from the planet and from the effluent matter, will always render it broader and thinner; and this incurvated column will approach to some of the forms determined in prob. 2. And thus a reason might be assigned for Saturn's ring, the most surprising phenomenon in nature.

And while the tail of the comet would furnish the planet with such a ring, the comet itself might probably be attracted, if at a due distance, and become a new satellite to the planet; and thus probably several comets have furnished out both Saturn's satellites and his ring; for it is not likely that Saturn's ring is owing to the effluvia of one comet, since it projects a shadow on his disk; whereas the matter of the tails of comets is so rare, that the stars may be seen to shine through it. Saturn's ring therefore seems to consist of the tails of several comets, whose matter is become more dense on account of his attraction.

It is evident that a planet may acquire satellites, and yet not a ring; for, all comets have not a tail; and if a comet without a tail be attracted, it will furnish the planet a satellite without a ring.

The great Sir Isaac Newton has concluded that the vapours of comets are dispersed among the planets; nay he reckoned this communication necessary, in order to repair the loss of liquid matter. And Dr. Halley and Mr. Whiston are of opinion that both comets and their tails, cause considerable changes in the planets, as a variation in their poles, or deluges, and conflagrations; but comets may possibly produce more benign effects, and even sometimes supply the planets with useful and surprising things.

On the Arcuccio used by Nurses in Italy. By Oliver St. John, Esq. F. R. S. N° 422, p. 256.

When we consider how many children are charged overlaid in the bills of mortality, it is remarkable that the arcuccios universally used in Italy are not used in England. The following is a design of one, drawn in perspective, with the dimensions, which are larger than usual. In fig. 8, pl. 12, a represents the place where the child lies; b, the head-board; c, the hollows for the nurse's breasts; d, a bar of wood to lean on when she suckles the child; e, a small iron arch to support the said bar. The length is 3 feet 2½ inches.

Every nurse in Florence is obliged to lay the child in it, under pain of excommunication. The arcuccio, with the child in it, may be safely laid entirely under the bed-clothes in the winter, without danger of smothering.

Of an extraordinary large Horn of the Stag Kind, taken out of the Sea on the Coast of Lancashire. By Mr. Hopkins. N° 422, p. 257.

Fig. 9, pl. 12, represents part of the horn; the dimensions of which are exactly set down, as Mr. Hopkins took them, by laying a string along the surface.

ae the length, 30 inches; bb the circumference above the third branch, 7 inches; c the circumference above the second branch, 8 inches; dd the circumference between the brow and second antler, 11 inches; ee the circumference at the root, 10 inches; de the circumference of the brow-antler, 6¾ inches; ef the length of the antler, 16¾ inches.

This horn was drawn out of Raven's Barrow Hole, adjoining to Holker Old Park, by the net of a fisherman, on the 20th of June, 1727. The tide flows constantly where it was found, and the land is very high near it.

Three extraordinary Cases communicated by Claudius Amyand, Esq. F. R. S. viz. 1. *A Child born with the Bowels hanging out of the Belly.* 2. *A Suppression of Urine in a Woman.* 3. *A Stricture in the middle of the Stomach, dividing it into two Bags.* N° 422, p. 258.

1. Dec. 18, 1730, a child was born with the greatest part of the bowels hanging out of the belly, by an aperture about half an inch in diameter on the right side of the navel string. The birth was natural and easy.

Mr. A. found the aperture lined with a skin, and a ligament that opposed the reduction; the parts livid, and tending to mortification; yet the child lived near 3 days. On opening, he found the prolapsus to consist of all the small guts, except the duodenum, and of all the large ones, except a small portion of the rectum: the gall-bladder was about 2 inches long, one half of which stood out of the abdomen, and a small portion of the stomach: all these were so coalesced and confounded together, that it was impossible to separate them; though on blowing, the intestinal tube seemed to have its usual length. The liver was much thicker and larger than usual, and convex in that part of it, which is naturally concave: and the uterus and bladder pressed on the left side, by the weight of the bowels pressing on the right.

The mother could assign no cause for this preternatural formation. The child came at full term; but its inquietudes for some months before the birth, made the mother apprehend it was not well.

2. Mr. A. was called to a woman who had a suppression of urine, occasioned by the menses collected in the vagina, pressing on the urethra. She had been delivered 8 months before of 2 children; after which the carunculæ myrtiformes had joined together so closely, that there was no room for any evacuation of the menses. He made a cross aperture, by which near 3 quarts of the menses collected were discharged; the suppression of urine was immediately removed, and the patient cured.

3. On opening the body of a young country girl, dead of a consumption, Mr. A. found her lungs suppurated in many places, and a stricture in the middle of the stomach, dividing this viscus into 2 bags. This stricture appeared to have been of some standing, and likely to have occasioned some difficulty in digestion; but on inquiry, her mistress and fellow servants said, that her appetite and digestions were natural, and that she had continued in a good plight, till on coming to London she contracted a cough, that brought on the consumption.

An Abstract of Meteorological Diaries communicated to the Royal Society, with Remarks on them by W. Derham, D. D. Canon of Windsor, and F. R. S. N° 423, p. 261.

Most of these Diaries are accounts of the course and strength of the winds, the heights of the barometer, and the weather, as to rain, &c.; now of no use.

Dr. Derham, in his observations, notices that he found a great conformity in the above articles at different places, at considerable distances.

Dr. D. remarks that in January 1716, the river Thames was frozen for several miles, and particularly so intensely at London, that whole streets of booths were erected on the ice, oxen roasted, coaches driven, and many diversions exercised above bridge. And so strong was the ice below bridge, as to allow people to walk and skait at their pleasure on it.

Also that one day in 1715, the wind was so violent, that the Thames was emptied from London-bridge as far as ———, so that only a small rivulet of water, no larger than a brook of 10 or 12 feet over, remained; insomuch, that people walked on the bottom, and found treasure there.

On Feb. 12, 1715-16, Mr. Robie notes an earthquake to have been at Salem village; and on Oct. 21 following the day was so dark, that people were forced to light candles to eat their dinners by. Which could not be from an eclipse, the solar eclipse being the 4th of that month.

On Feb. 13, 1716-17, he observed an immersion of the first satellite of Jupiter, at $10^h\ 48'\ 17''$; and on Feb. 8, he observed an emersion at $8^h\ 7'\ 30''$; according to which, the difference of longitude between Harvard-College and Upminster, is $4^h\ 45'$, and Mr. Robie says, that it is $4^h\ 44'$ from London, by the latest and best observations.

Sept. 23, 1717, Mr. Robie, at Harvard-College, New England, observed the solar eclipse.

	h	m
The beginning at	12	23
The middle about	1	47
The end at	3	$5\frac{1}{6}$ P. M.

About 9 digits were eclipsed.

Observations of the Eclipse of the Moon on June 28, 1721.—About 2 in the morning Mr. Robie viewed the moon with his 8-foot telescope, and she was untouched.

Time Correct.			
At 2^h	10^m	0^s	a thin penumbra.
2	12	0	shadow is plainly entered.
2	47	10	moon eclipsed about 6 digits.
3	18	30	moon wholly covered.

There remained a light on the western side of the moon for some time.

The Observations Mr. Robie made on the Solar Eclipse, Nov. 27, 1722, were as follow :

$7^h\ 27^m\ 0^s$	He saw the sun rise eclipsed about 4 digits on his upper part. Then we could observe no more till
8 30 0	The sun began to appear, and about 6 digits were eclipsed.
9 25 45	End of the eclipse.

Description of a new Quadrant for taking Altitudes without a Horizon, either at Sea or Land. Invented by Mr. John Elton. N° 423, p. 273.

This instrument, fig. 1, pl. 14, contains 4 principal parts; viz. a frame, an index, a label, and a shield; and these consist of several parts.

The frame, ABCDEF, has two parts; one a graduated arch DE of 30°; each degree being subdivided into 6 equal parts; the other a chord BC of an arch of 60°, divided into 2 equal parts (at the extremities and in the middle of which are holes or stops for the label) together making 90° or a quadrant.

The index GH turns on the centre of the frame the whole compass of the arch, and has 3 parts; viz. a nonius plate, n, and eye-vane, v, and a tubet. The nonius plate moves with the index, and subdivides each of the small divisions of the arch into 10 equal parts or minutes. The eye-vane is to look through in forward observations. The tube is to show when the index is horizontal.

The label IK moves on the centre of the frame the whole compass of the chord of the arch of 60°, having 3 fixed stations on it, at 30°, 60°, and 90°, and contains two principal parts; viz. a lens l, and a lantern whose stool is o. The lens is to form the sun's image on the shield. The lantern is necessary in nocturnal observations.

The shield, or ray-plate dfg, is fixed in the centre of the frame, and has three parts ; viz. an azimuth tube, z, a horizontal tube, h, and an axis, x, or in backward observations a ray-plate. The hole in the shield is to receive the sun's image. The azimuth tube is to direct the plane of the instrument perpendicular. The horizontal tube is to show when the label is level. The axis is to cut the object in forward observations.

A Rule for either backward or forward Observations.—If the altitude do not exceed 30°, the label must be placed at the station on the radius or longest limb of the quadrant ; if the altitude be between 30° and 60°, at the middle station; and if the altitude exceed 60°, at the uppermost station.

To take the Sun's Altitude by a backward Observation.—This is done without using the sight-vane or horizontal tube on the shield. Hold the quadrant with both hands in such a manner as is aptest for keeping it steady, the back of the arch being turned toward the sun. When the bubble of the azimuth tube is brought under the hole in the shield, cause the sun's image to fall on the hole in the shield, so that it may rest in the centre of the sun's image; the instant the azimuth tube and sun's image are thus regulated, see if the bubble in the horizontal tube on the index (which till then is disregarded) leaves the open end of the tube, or stops any where clear of the ends of the tube: if these happen at the same juncture, the altitude is then truly taken; but if the bubble had remained in the inclosed end of the tube, when the azimuth bubble and sun's image were regulated, the index must have been slid up; and if tarried in the open end, moved down, till the horizontal bubble on the index quit the open end of the tube, or stop between the ends, as was before observed; and then is the quadrant set. In continuing the observation for a meridian altitude, the quadrant being set, as the sun rises, the horizontal bubble on the index, will not quit the open end of the tube, or stop between the ends, but hang there, or leave it after the azimuth bubble and sun's image have been regulated; which will require the index to be continually moved down, in order to keep the quadrant set. When the sun is up, or on the meridian, the quadrant will remain set for some time; and on the sun's falling, the horizontal bubble will have a reverse tendency, inclining or running wholly to the inclosed end of the tube.

To take the Altitude of the Sun or Stars by a forward Observation.—In this method, the lens and tube on the index are disregarded. Hold the quadrant vertical; and looking through the eye-vane, direct the axis or upper edge of the shield to the sun or star; if the axis cut the sun or star at the same instant that the bubble in the horizontal tube on the shield quit the open end, the altitude is then truly taken, and the quadrant set. But if it should leave the open end of the tube before the axis or upper edge of the shield cut the sun or star, then the eye-vane (or which is the same, the index) must be slid down; and if it remain at the open end, or quit it when the axis is above the sun or star, moved up, till the quadrant is set. In continuing the observation for a meridian altitude, as the sun or star rises, the bubble in the horizontal tube will always quit the open end of the tube before the axis cut the object; so that to keep the quadrant set, the eye-vane must on every such alteration be constantly moved down; while the sun or star is on the meridian, the quadrant will remain set; and when the sun or star falls, the bubble will act contrary to what it did in the rising, resting wholly in the open end of the tube.

To take the Sun's Altitude with the Horizon.—Turn the back of the arch towards the sun, and cause the sun's image to fall on the hole in the shield, at the same time looking through the eye-vane, cut the horizon with the axis.

N. B. In taking the altitude of the stars, a small light must be fixed in the lantern; the less the better. It will be best in forward observations of the sun, to take the altitude of the upper limb, allowing for the semidiameter; and when the sun is very clear, take his altitude by a backward observation, the forward method being chiefly intended for nocturnal observations, and when the sun is too much obscured to give any shade or image.

A remarkable Case of a Gentlewoman who died of a Hydrops Ovarii, after having been tapped 57 *Times. By Mr. John Belchier, Surgeon.* N° 423, p. 279.

In 1725, the wife of one Mr. Newberry complained of a pain in her left-side, near her groin, internally, which sensibly increased; and perceiving a swelling in that part, she at first thought herself with child; but having other symptoms not very common with women in such a case, she sent for a physician, who immediately discovered it to be hydropical; and after following his prescriptions for some time, and finding little or no benefit, she sent for another, and so for a 3d and 4th; and after between 2 or 3 years fruitless trial of proper medicines prescribed by the physicians, growing very big and uneasy with her burden, she was advised to be tapped, to which she accordingly submitted: and on May 6, 1728, sent for Mr. Cheselden, who took from her between 4 and 5 gallons of water. But in a week or 10 days after the operation, she perceived herself to fill again, in which state she continued to the 1st of July following, when Mr. Cheselden tapped her again, and took from her about the same quantity of water as before. And in this manner she continued to fill and be tapped every 3d or 4th week;* from May 6, 1728, to the 3d of March, 1731-2, when she died, in the 33d year of her age.

During the last 37 times of her tapping, Mr. B. constantly attended her with Mr. Cheselden, when she always, till the last two times, appeared very brisk and lively the whole time of the water's running from her, and was not in the least sick or faint after the discharge, as is usual; and though she was a very thin emaciated woman, she would frequently walk 3 or 4 miles the day before the operation, and most commonly went abroad the 3d day after it.

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* Parallel to this is the case related by Dr. Mead of the relict of Sir Gregory Page, Bart. She died in the 56th year of her age; and in 67 months was tapped 66 times. In these different operations, there were taken from her 240 gallons of water. These particulars are commemorated upon her monument in Bunhill Fields.

The quantity of water taken from her each time of tapping, was between 4 and 5 gallons; and during the whole 57 times tapping, there never was above 1 or 2 quarts at most, different in the quantity, till the last two times, at each of which the quantity did not exceed 2 gallons: but in the intervals of these last two operations, she was frequently troubled with retchings to vomit, which burst open the orifice twice where she was tapped, and at each time discharged about 6 quarts.

The quantity of water which was taken from her each time was always measured, and on computation the whole amounts to near 250 gallons. The water that was taken from her the last two times of tapping was much more viscid than the former.

At times she frequently complained of a violent pain on her right side, and a heavy aching pain in the pelvis. She had likewise a prolapsus uteri; and some time before her death she could not expel her fæces but with great difficulty and pain, and at the same time laboured under an incontinency of urine.

March 6, 1731-2, Mr. B. opened her in the presence of her physician, when he found the whole viscera, from the diaphragm to the ossa pubis, covered with a thick gelatinous substance, which seemed to be membranous, which at its first appearance he took for the omentum in a putrefied state; but after a further examination, he found it to be only the more viscid parts of the extravasated fluid, which could not be discharged by the operation: after removing this, he found several portions of a hard scirrhous substance arising from the fundus of the stomach, one large portion of which was inserted into that part of the colon near the right kidney, and in appearance resembled the pancreatic gland. Another portion, which was cylindrical, and about $\frac{2}{3}$ of an inch in diameter, passed straight over the intestines, adhering strongly to that part of the colon which lies under the stomach, and was inserted into the rectum, in the pelvis. Another portion of this substance passed directly over the intestines to the pelvis; but about the middle of the abdomen it sent out 2 smaller portions, the one inserted into the mesentery, the other reflecting back, was inserted into the colon on the left side, near the stomach. As soon as he cut into one of these portions, he discovered it to be a part of the omentum twisted up, and contained in a very thick capsular membrane.

The diaphragm was forced up so far by the contents of the abdomen, that the cavity of the thorax was decreased to near $\frac{1}{3}$.

The liver was much larger than in a natural state, and of one entire substance, and not divided into lobes, the whole convex surface adhering firmly to the diaphragm.

The stomach was very small, as to its cavity, but its coats were increased to

6 times their natural thickness (as were likewise all the coats of the intestines and mesentery) and very much inflamed.

Two-thirds of the stomach adhered to that part of the diaphragm which did not cover the liver, and the other part adhered to the concave surface of the liver; as did likewise the duodenum, whose cavity was very large. Below the duodenum, the colon adhered to the lower part of the concave surface of the liver; so that the whole liver was contained in a kind of bursa, composed of the diaphragm, stomach, duodenum, and colon.

The cæcum, colon and rectum were much larger than in a natural state, and adhered so very strongly to the parts over which they passed, that it was with much difficulty they could be separated.

The spleen was not $\frac{1}{4}$ of its natural size, and $\frac{1}{2}$ of its external surface was entirely cartilaginous.

The pancreas was smaller than usual, as were likewise the kidneys, ureters and bladder; and in the pelvis of each kidney there were small sabulous concretions.

The left ovarium was distended to so large a size, as to fill the whole cavity of the pelvis up to the os pubis; its surface was cartilaginous, like that of the spleen, and in it were contained a great number of hydatides of different sizes; whereas the right ovary was not in the least diseased.

The difficulty and pain complained of in the expulsion of the fæces, naturally arose from the pressures on the diseased ovarium; at the same time that its increased bulk, by compressing the intestinum rectum impeded the egress of the fæces, and brought on the inflammation of the intestines.

The prolapsus uteri, and the incapacity of the bladder's retaining a proper quantity of urine, were likewise occasioned by the pressures of this diseased ovarium on those parts.

But what seems most material in this case, is the viscid matter found in the cavity of the abdomen; which, as the waters were originally incysted in the ovary, was probably extravasated from the cystus into the abdomen, in the last two operations; by which, as well the quantity drawn off as the customary relief, were very much diminished; instead of which, the stimulus, from such a fluid, might reasonably bring on the vomiting observed from that time.

Quer. Therefore, if such a vomiting ensuing the operation is not a fatal symptom?

Quer. If any method can be found to prevent such extravasations?

The relations of this gentlewoman are of opinion, that her disease was occasioned by pulling off her cloaths, when she was very hot, to go into a bathing-tub of water to cool her; when finding the water excessive cold, she put only her legs in, the other part of her body being out of the water, and naked at the same time; which happened a few weeks before she perceived the swelling and pain in her pelvis; and probably this might be the cause. As the constriction of the lower parts by the cold water might, in a great measure, impede the fluids circulating through the lower parts, and the blood being at the same time rarefied and expanded by the heat, might therefore burst through the more tender lymphatics, and produce the extravasation.

Further Experiments concerning Electricity. By Mr. Stephen Gray. N° 423, p. 285.

In N° 422 Mr. Gray gave an account of experiments, showing that water will be attracted by electric bodies, and have an electric virtue communicated to it, so as to attract solid ones; and since then he has been on another inquiry, viz. Whether there might not be a way found to make this property of electrical attraction more permanent in bodies? How far he has succeeded in this attempt, will appear by the following experiments made on several bodies; and as they were all prepared after the same manner, excepting N° 18 and 19, which will be described afterwards, a general description of the method of preparing and preserving them in a state of attraction, may suffice.

The bodies on which the experiments were made, were, rosin both black and white, stone-pitch, shell or gum-lac, bees-wax, and sulphur. He procured 3 iron ladles of several sizes, in which he melted these substances, in due quantities. When any of these bodies were melted, they were taken off the fire, and set by in the ladle to cool and harden; then it was returned to the fire, where it remained until it was melted about the bottom and sides of the ladle, so as to be moveable; so that by inverting the ladle, it might be taken out; having the form of nearly the section of a sphere, the convex surface, as also the plane one, being naturally (as it were) polished, excepting the sulphur, which cools without retaining its polish, except when cast in glass vessels.

When any of the substances were taken out of the ladle, and their convex surface hardened, they would not at first attract, until the heat was abated, or until they came to a certain temper, and then there was a small attraction; which warmth he estimated to be nearly that of a hen's egg when just laid: the attraction increasing so, as when cold, to attract at least 10 times further than at first.

The manner of preserving them in a state of attraction, was by wrapping them up in any thing that would keep them from the external air; as at first for the smaller bodies he used white paper, but for the larger ones white flannel; but afterwards found that black worsted stockings would do as well. Being thus cloathed, they were put into a large fir box, there to remain till he had occasion to use them.

The cylinder of sulphur, N° 18, was made by melting the sulphur, and pouring it into a cylindric glass vessel, which had first been heated, to prevent its cracking. When the sulphur was hardened, it was somewhat less than the glass; so that by inverting the glass, it came out easily, and had a polished surface, almost as smooth as the glass in which it was cast. The large cone of sulphur, N° 19, was made after the same manner; viz. by being cast in a large drinking-glass.

Of the following catalogue, the first column contains the number, which in a small piece of paper is fixed on each of the several bodies; the name of which is given in the 2d column, whether they are single or compound substances. The 3d column shows their weight when melted, in avoirdupois ounces and drachms. In the 4th column are the days of the month when the body was melted and received its form, and consequently when it first began to attract.

For 30 days he continued to observe every one of these bodies, and found that at the end of that time they attracted as vigorously as at the first or second day, and as they do still. By subtracting the times mentioned in the catalogue, from any time after, it will be shown how long any of the bodies have continued their attractive virtue; by which it will appear, that some of them have not lost their attraction for more than 4 months: so that we have some reason to believe, that we have now discovered that there is a perpetual attractive power in all electric bodies, without exciting by either rubbing, beating, &c. or any other attrition. But this will further appear by the following account of the last two bodies mentioned in the catalogue. The cone of sulphur, N° 19, that was cast in a large drinking-glass, in about 2 hours after it was taken out of the glass, attracted, and the glass attracted too, but at a small distance. Next day the sulphur was taken out of the glass, and then it attracted strongly, but there was now no perceivable attraction of the glass. Then the cone of sulphur was set with its base on the lid of the fir box, where the other electric bodies lay, and the glass whelmed over it. He examined it every day after, and still found it to attract; but finding the place not so convenient, having occasion to look into the box often, he removed it to the table between the two windows of his chamber, where it still continued, and whenever the glass is taken off, it attracts at near as great a distance as the sulphur that is clothed and shut up in the box. And though at first there was no attraction, when the glass was taken off, yet he now finds, that in fair weather the glass also attracts, but not at so great a distance as the sulphur, which never fails to attract, let the wind or weather be ever so variable, as all the other bodies do;

only in wet weather, the attractions are not made at so great a distance, as in fair weather.

Number 20 is a cake of sulphur that was melted; and as the other bodies have taken the form of a convex section of a sphere, this, when cold, was laid with its flat side downwards, on the same table with the cone of sulphur: they were both placed so near the wall, as to prevent the sun shining on them. This was on the 18th of April; and though it had no manner of cloathing or covering, has attracted ever since. And in this, as in the other bodies, the attraction will be according to the weather; but when it attracts the strongest, it is not more than the 10th part of what the cone of sulphur, that is covered, attracts.

The manner of observing these attractions, is best performed by holding the attracting body in one hand, and a fine white thread tied to the end of a stick, in the other; by this means, far less degrees of attraction will be perceived, than by making use of leaf-brass. When the thread is held at the utmost distance, it may be attracted; the motion of it is at first very slow, but still accelerating as it approaches nearer to the attracting body.

With a small hand air-pump he has made experiments on several bodies, and finds that they will attract in vacuo, and that at very nearly the same distance as in pleno, provided that the experiment be made in the same receiver filled with air; as will appear by the following experiments.

There was taken a hollow glass sphere, of rather more than 2½ inches diameter, being first excited. It was suspended by a loop of silk that went through a small cork, with which the hole in the glass ball, by which it was blown, was stopped, and by the loop suspended a small hook that was screwed on to the brass wire that came through the collar of leather in the brass-plate that covered the top of the open receiver; as in the experiment of letting fall the guinea and feather in vacuo. Then the ball was drawn up to the top of the receiver, and the top of the small stand, covered with paper, was laid on the wet leather on the plate of the pump, and leaf-brass laid on the same. Then the air was exhausted, when the glass ball was let down to about an inch, or somewhat more, towards the pieces of leaf-brass: many of them were attracted by it. Then the air was let into the receiver, and the leaf-brass laid on the stand, the ball being as before suspended, was let down to about the same distance from the leaf-brass as before, and there seemed to be very little difference in the attraction.

He has made the same experiments with sulphur, shell-lac, rosin, and white bees-wax. These would be attracted to the height of an inch and a half by estimation; and when the experiment was made with the receiver full of air, there was very little, if any difference, in the height of the attraction, when there was the same time spent before the attraction was begun in pleno, as there was required to exhaust the receiver.

A Catalogue of the several Electric Bodies mentioned above.

N°	Names of the Bodies.	Weight. oz.	dr.	Month.	Days.
1	Fine black rosin	2	0	January	31
2	Stone pitch and black rosin	2	2	January	31
3	Fine rosin and bees-wax	2	1	February	1
4	Stone pitch	1	7	February	1
5	Stone sulphur	3	6	February	4
6	Shell-lac	10	0	February	10
7	Fine black rosin	10	4	February	10
8	Bees-wax and rosin	9	0	February	12
9	Rosin 4, and gum-lac 1 part	10	0	February	12
10	Sulphur	18	0	February	15
11	Stone pitch	10	12	February	16
12	Black rosin	23	0	February	23
13	White rosin	7	12	February	25
14	Gum-lac	11	14	February	26
15	Gum-lac and black rosin ana	9	12	February	26
16	Gum-lac 4 parts, rosin 1 pt.	17	8	February	28
17	Shell-lac, fine black rosin ana	28	4	March	2
18	A cylinder of stone sulphur	19	4	March	20
19	A large cone of stone sulphur	30	0	March	29
20	A cake of sulphur	11	4	April	29

An Experiment to show that the Friction of the several Parts in a Compound Engine may be reduced to Calculation; by drawing Consequences from some of the Experiments on Simple Machines in various Circumstances. By J. T. Desaguliers. N° 423, p. 292.

The machine consists of three pulleys, 2 upper and one lower, or a tackle of 3, whose diameters are exactly as follows, 2 inches, $1\frac{1}{2}$ inch, $1\frac{1}{4}$ inch; and all the centre pins of $\frac{1}{4}$ inch diameter: the rope being of $\frac{1}{10}$ inch in diameter.

The weight is 18 pounds avoirdupois, and consequently the power to keep it in equilibrio must be = 6lb., and a very little more must make the power raise the weight, if there was no friction; but here no less than 20 ounces are required, though the machine is as nicely made as it can possibly be.

Dr. D. has shown by experiment, that when the weight is unknown, $\frac{2}{3}$ of the power is the friction of a cylinder whose surface moves as fast as the power, and whose gudgeons are equal in diameter to the cylinder. Now as the diameter of the first pulley is 8 times that of its pin, its friction must be 8 ounces.

The second pulley, whose surface moves as slow again as the power, and whose pin is 6 times less in diameter, must of consequence have its friction of only $5\frac{1}{3}$ ounces; because $64 \div 12 = 5\frac{1}{3}$.

The third pulley moving with $\frac{1}{3}$ of the velocity of the power, on a pin of $\frac{1}{4}$ of its diameter, has for its friction $4\frac{1}{3}$ oz. because $64 \div 15 = 4\frac{1}{3}$.

Now the sum of all these frictions being 17.6 oz. which is the 5.4 part of the power 6lb.; this addition so increases the friction, as to require a superaddition of the 5.4 part of that first addition, and so on, in this series, $17.62 + 3.2 + 0.59$ &c. $= 21.41$ oz.

Then the sum of the frictions on account of bending the ropes, deduced from the experiment that a rope of $\frac{1}{10}$ inch in diameter, stretched by 6lb. requires 4.5 oz. to bend it round a cylinder of 1 inch, amounts to $1.8 + 1.15 + 1.124 = 4.424$ oz.; which with the other friction, amounts to 25.834 oz. But as he had formerly shown in the Trans. that when a rope drawn by unequal weight runs over a pulley, the pressure on the pin is diminished; that diminished pressure, found by calculation to be near 6 oz. being taken from the above sum, the friction remaining will be 19.834 oz.; and the experiment is just 20 oz.

Nothing was here allowed for the weight added to bend the ropes, which would still bring the experiment nearer the theory.

A Way of Communicating the Magnetical Virtue to Iron and Steel, without the help of a Loadstone. By M. Arnold Marcel. N° 423, p. 294.

In 1722, M. Marcel observed, that a long heavy bar of iron being set upright, and some filings of iron, or a bit of iron-wire laid on its upper end, those filings or bit of wire would stick to another piece of bright pointed iron, and suffer itself to be lifted up from the standing bar, even to the height of 5 inches.

In 1726, making several more observations about the magnetical force, which he found in large pieces of iron, he made use of a large iron vice, about 90lb. weight, in which he fixed a small anvil of about 12lb. On the bright surface of this anvil he laid the steel, to which he would communicate the virtue, in a position north and south, which happened to be in a diagonal of the square surface of the anvil. He then took a piece of iron, an inch square, and 33 inches long, of about 8lb. weight, having at one end the figure represented fig. 2, pl. 14, brightly polished at A, and taper at the other end. Then with one hand he held the piece of steel fast down on the anvil, and with the other he held the iron bar perpendicular with it, with its point a on the steel, and pressing hard, he rubbed the steel with the iron bar towards himself, from north to south, several strokes, always carrying the bar far enough round about to begin again at the north, to prevent the drawing back of the magnetical force: having thus given 10 or 12 strokes, he turned the steel upside down, having it in the same position as to north and south; and after rubbing and turning it, till he rubbed it about 400 times, it received by degrees more and more strength, and at last had as much as if it had been touched by a strong loadstone. The place where he began to rub, was always that which pointed to the north, when the needle was hung, the end where he had ended the stroke turning to the south. Sometimes it has happened, that in a few strokes he gave the steel its virtue; nay even in the very first stroke one may give a great deal to a small needle.

In this way M. Marcel communicated the magnetical virtue to needles of sea-compasses, made of one piece of steel, as fig. 3, so strongly, that one of the poles would take up $\frac{3}{4}$, and the other a whole ounce of iron, though these needles were anointed with linseed oil, which made a hard coat, to keep them from rusting; yet they retained the virtue: but in strengthening this sort of needles, he rubbed by turns first to the right, and then to the left side.

The same way he brought the virtue into the point of a knife; so that it would sustain 1 ounce and $\frac{3}{4}$.

He brought the said virtue into 4 small pieces of steel, each 1 inch long, and $\frac{1}{12}$ inch broad, as thin as the spring of a watch. He joined these 4 pieces together, as into an artificial loadstone, weighing 18 grains troy; and then it drew up and sustained an iron nail, which weighed 144 grains troy. This artificial loadstone was for 6 years tumbled about, and lay among iron and steel, and in any position; and yet it rather acquired more, than lost any of its virtue.

The magnetical virtue being thus communicated to iron or steel, he further observed, that that end where the stroke was begun, would draw to the north, and where the stroke ended to the south, in whatever situation the steel had been laid on the anvil to give it the virtue. He took a piece of steel, and rubbed it from one end to the middle; and then from the other end to the middle, and found it had 2 north poles, one at each end, and the middle a south pole.

Further, beginning to rub from the middle towards each end, of another piece of steel, he found it to have a south pole at each end, and a north pole in the middle.

He put a pretty heavy compass-needle, after having given it its virtue, into the fire, and made it red hot three times, one after another, letting it grow cold every time: it lost some virtue every heat, but at the 3d heat it had a great deal still left; and making it for the 4th time white hot, it lost it all.

When he covered the anvil with a piece of woollen cloth, and the end of the iron bar with a piece of shamoy leather, it gave no virtue to the steel: then covering only the bar, and leaving the anvil uncovered, it communicated no virtue that way neither: but covering the anvil, and leaving the bar uncovered, it communicated the full virtue.

He tried whether his vice had any fixed pole by standing long in one position, but he found it had none.

He tried to do this with an anvil of about 30lb. weight, fixed in wood; but could not come up to the other proofs.

He believes if one took an iron bar of 3 inches square, and 10 or more feet long, or several of them on each other, and a suitable piece or bar of iron to rub with, and giving the underpart of the standing bar the figure aforesaid, represented by B, fig. 6, it might be brought to a vast strength. N. B. The steel for the needles is always of a spring temper.

M. Marcel made 2 pieces of iron, at one end $\frac{3}{4}$ of an inch, and so taper to $\frac{1}{4}$ of an inch square each, and fixed those 2 pieces of iron to a piece of wood in the shape of an armed loadstone, at about 8 inches one from the other, applying to the under part of these irons, or legs, a piece of iron with a hook to it, as to an armed loadstone: he hung this armed piece of wood with each leg over an iron bar, at a distance that something might hang between them; then he placed the piece of iron with the hook to it to the 2 feet; and he found it to draw very strongly; but his trial was only with small tools. He supposes if one did this in a large proportion, it would have a considerable effect.

Having ground some loadstones with emmery, he saved the grindings, and mixing them with water, so that they might easily be moved, he put them into a bottle to sink, placing a loadstone on each side; one with its north, and the other with its south pole towards the bottle; and he found, after the matter was settled and dried, that it formed itself into a sort of loadstone, which had a moderate strength, and 2 regular poles.

Fig. 2, pl. 14, represents the end of the iron bar, with which the virtue is rubbed into steel or iron.

Fig. 3, the needle of a sea-compass.

Fig. 4, the figure of the point on one side.

Fig. 5, the figure on the point of the other side.

A, fig. 11, represents the needle of a compass; BB the end or edge of the bar, with which the needle is rubbed, beginning at CC, and proceeding to DD.

An uncommon Case of a Distempered Skin. By John Machin, Sec. R. S. N° 424, p. 299.

A country labourer near Euston-hall in Suffolk, had a boy, about 14 years of age, having a cuticular distemper, of a different kind from any hitherto mentioned in the histories of diseases.

His skin, if it might be so called, seemed rather like a dusky coloured thick case, exactly fitting every part of his body, made of a rugged bark, or hide, with bristles in some places, covering the whole, excepting the face, the palms of the hands, and the soles of the feet, caused an appearance as if those parts alone were naked, and the rest cloathed. It did not bleed when cut or scarified, being callous and insensible. It was said he sheds it once every year, about autumn, at which time it usually grows to the thickness of $\frac{3}{4}$ of an inch, and then is thrust off by the new skin coming up beneath.

It was not easy to think of any sort of skin, or natural integument, that exactly resembled it. Some compared it to the bark of a tree; others thought it looked like seal-skin; others like the hide of the elephant, or the skin about the legs of the rhinoceros; and some took it to be like a large wart, or a number of warts uniting and overspreading the whole body. The bristly parts, which were chiefly about the belly and flanks, looked and rustled like the bristles or quills of a hedge-hog, shorn off within an inch of the skin.

His face was well featured, and of a good complexion, if not rather too ruddy; and the palms of his hands were not harder, or in worse condition, than is usual for workmen or labourers. His size was proper for his age; his body and limbs straight, and well shapen.

This rugged covering gave him no pain or uneasiness, except that sometimes after hard work, it was apt to start and crack, and cause a bleeding. His natural excretions were said to be in the ordinary course and manner, without any thing remarkable attending them.

The father knew of no accident to account for this distempered habit. But said that his skin was clear at his birth as in other children, and so continued for about 7 or 8 weeks; after which, it began to turn yellow, as if he had the jaundice; from which it gradually changed black, and in a little time afterwards thickened, and grew into its present state. That he has been in health from his birth, and has no sickness at the season when he sheds it. He further said, that his mother had received no fright to his knowledge, whilst she was with child; and has borne him many other children; none of which have ever had this, or any other unusual distemper or deformity.

Fig. 7, pl. 14, represents the back of the boy's hand.

Fig. 8, a portion of this extraordinary epidermis, which was probably a prolongation of the nervous papillæ grown to about the size of common twine packthread; and these, standing as close together as the bristles in a brush, seemed like them, to be all shorn off even, and of the same length, viz. about half an inch above the skin.

Fig. 9, shows some of these bristles, or stumps, magnified; where it is visible that some of them are flat at top, others concave; some pointed like a cone, and others very irregular.

Conjectures on the Nature of Intermitting and Reciprocating Springs. By Mr. Joseph Atwell, F. R. S. N° 424, p. 301.

The following conjectures on the subject of intermitting and reciprocating springs, were suggested to Mr. Atwell by the phænomena of a particular fountain he had seen the winter before.

The spring is situated at one end of the town of Brixam, near Torbay in Devonshire, and is known by the name of Laywell. It is a long mile distant from the sea, on the north and north-east side of a ridge of hills, lying between it and the sea, and making a turn or angle near this spring. It is situated in the side of those hills, near the bottom, and seems to have its course from the south-west towards the north-east. There is a constantly running stream which discharges itself near one corner into a basin, about 8 feet in length, and 4½ in breadth; the outlet of which is at the farthest end from the entrance of the stream, about 3 feet wide, and of a sufficient height. On the outside of the basin are 3 other springs, which always run, but with streams subject to a like regular increase and decrease with the former. They seem indeed only branches of the former, or rather channels discharging some parts of the constantly running water, which could not empty itself all into the basin; and therefore when, by means of the season, or weather, springs are large and high, on the flux or increase of this fountain, several other little springs are said to break forth, both in the bottom of the basin, and without it, which disappear again on the ebb or decrease of the fountain. All the constantly running streams put together, at the time that he saw them, were more than sufficient to drive an overshut mill; and the stream running into the basin, might be about half of the whole.

Mr. Atwell made a journey purposely to see it, in company with a friend. When they came to the fountain, they were informed that the spring had flowed and ebbed about 20 times that morning; but had ceased doing so, about half an hour before they came. He observed the stream running into the basin for more than an hour, without perceiving the least variation in it, or the least alteration in the height of the surface of the water in the basin. Thus disappointed, they went to take some refreshment at an inn; after which they intended to come back and spend the rest of the time by the fountain, before returning home. They were told in the town, that many had been disappointed in this manner; and the common people superstitiously imputed it to some influence which the presence of some people had over the fountain; for which reason they advised, that in case it did not flow and ebb when they were both present, one of them should absent himself, to try whether it would do so in the presence of the other.

On their return to it, a man, who was still at work near it, said that it began to flow and ebb about half an hour after they went away, and had done so 10 or 12 times. In less than a minute, they saw the stream coming into the basin, and likewise the others on the outside of the basin, begin to increase and to flow with great violence; on which the surface of the water in the basin rose an inch and a quarter perpendicularly, in near the space of 2 minutes: immediately after which, the stream began to abate again to its ordinary course; and in near 2 minutes time the surface was sunk down to its usual height, where it remained near 2 minutes more. It then began to flow again as before; and in the space of 26 minutes flowed and ebbed 5 times: so that, an increase, decrease and pause, taken together, were made in about 5 minutes, or a little more.

It could be observed by the mark on the stones, that the surface of the water in the basin had risen before they came at least three quarters of an inch perpendicularly higher, than when they saw it; and they could perceive some very little abatement each turn, both in the height, and in the time of the rising of the surface, and consequently in the time of its sinking; but the time of the pause, or standing of the surface at its usual height, or equable running of the stream, was lengthened; yet so, as to leave some abatement in the time of the rising, sinking, and pause taken together.*

Mr. Atwell comes now to his hypothesis, for explaining the phænomena observed; and he imagines them to be occasioned by two streams or springs, one

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* Another account of this spring, by Dr. Oliver, a little varied in the circumstances, may be seen in N° 204.

of which passing through two caverns or natural reservoirs with syphons, meets with the other stream in a third reservoir, without a syphon; where being joined, they come out of the earth together.

The petitio principii, or supposition of reservoirs and syphons in the bowels of the earth, has been made by others: Pêre Regnault, in his Phil. Conversations, Vol. ii. Conv. 6, p. 125, &c. Eng. edit. has mentioned it in general; and Dr. Desaguliers, in Phil. Trans. N° 384, has attempted to apply it to two cases in particular; as Dechales, Tract. xvii. de Fontibus Naturalibus, &c. prop. xv, had done in two other cases before him. It is indeed unnatural or hard to be granted. Whoever has seen the Peak of Derbyshire, the hilly parts of Wales, or other countries, must be satisfied that they abound with caverns of many sorts. Some of them are dry, others serve only for passages, or channels to streams, which run through them; and a third sort collect and hold water, till they are full. They must also have observed, that there are sometimes narrow passages, running between the rocks which compose the sides, and going from one cavern to another. Such a passage, of whatever shape or dimensions, how crooked and winding soever in its course, if it be but tight, and runs from the lower part of the cavern, first upwards to a less height than that of the cavern, and then downwards below the mouth of the said passage, will be a natural syphon.

A natural reservoir then, ABCD, fig. 10, pl. 14, with such a natural syphon, MNP, may be supposed. Let a feeding stream enter it, near the top at O. The cavern must contain all the water which comes in at O, till it is filled to the top of the syphon at N. Then the syphon beginning to play, and being supposed always to discharge more water than comes in by the feeding-stream at O, will empty the cavern, till the water is sunk in it below the mouth of the syphon at M; when it must stop, till the cavern is filled, and the syphon runs again as before. If the water discharged by such a syphon, MP, be brought out of the earth by a channel PQ, the water will flow out of the earth, and stop alternately, making an intermitting fountain at Q.

By this plain and easy contrivance, several of the flowing and ebbing springs, observed by the naturalists, may probably be explained; and even a much greater variety of them than is hitherto known. For if the feeding-stream at O should arise only from the rains in winter, or from the melting of the snow in summer, the intermitting fountain would become a temporary spring, as Dr. Plot calls such springs which are confined to a season. Or if the feeding-stream at O should be constant, but yet liable with other springs to an increase and decrease, arising from the seasons, weather, or other causes, the construction of the syphon would make a great alteration. For when the syphon is so made, that its discharge (which is continually decreasing, as the surface of the water subsides in the cavern) shall at any time be equal to the feeding-stream entering at O, in such a case, the syphon must continually run, and yet not empty the cavern, till the feeding-stream at O is sufficiently diminished. But, when the diameter of the syphon at N, according to the height of the cavern, is so great, and the feeding-stream at O so small, that the syphon can carry off, in the manner of a waste-pipe, all the water which comes in, and yet not run with a full stream; the syphon must then continue to run without emptying the cavern, till the feeding stream at O is sufficiently enlarged. So that by these different constructions of the syphon, there may be some fountains which shall flow constantly in the winter, or a wet season, and intermit in the summer, or a dry season; and on the contrary, others which shall flow continually in the summer, or a dry season, and intermit in the winter, or a wet season. There is a third variety, which may arise from the make of the syphon, and will occasion such irregularities as admit of no certain explanation. This happens when the discharge of the syphon at the very last, is just equal to the feeding-stream, and the cavity of the syphon at N is large; for in this case, the air-bubbles, made by the fall of the feeding-stream from O to the bottom of the cavern, will sometimes accidentally get into the mouth of the syphon at M, and lodging at N, will so choke it, as to render its running and stopping, as well as the quantity of its discharge, entirely uncertain; so that these sort of fountains will admit of no further consideration.

But before leaving the consideration of fountains explicable by one reservoir and syphon, it may not be amiss to observe, that those which intermit regularly will have their flux always longer, and their pause or intermission shorter, in winter and in wet weather, than in summer or in a dry season; which is a consequence of this hypothesis, by which it may be examined, whether it be applicable to any particular intermitting fountain or not.

If the single reservoir and syphon have another outlet at R, fig. 11, situated between the bottom CD of the cavern, and the top of the syphon N, we shall have another kind of fountains. For if the feeding-stream at O, be capable of being discharged by the outlet at R, a fountain derived from R will continually run, while the feeding-stream can be discharged that way, and will increase and decrease with any little alteration happening to the feeding-stream at O, provided the said stream does not grow too large for the outlet at R. But in that case the cavern must be filled up to N, and the syphon may begin to play; which, together with the outlet at R, may discharge so much, as to make the surface of the water in the cavern sink below R, and consequently the fountain proceeding from R must stop. If the discharge of the syphon be so great as to empty the cavern, then the fountain derived from R will, after some time, begin to run again, and increase till the water rises in the cavern to N; after which it will decrease, and at length stop. But if the discharge of the syphon only keeps the surface of the water below R, without emptying the cavern, then the fountain derived from R shall be dried up, so long as the stream at O continues increased; and shall run again when the said feeding-stream is lessened. Thus we may have a spring which shall run all summer, and be dry all winter; such a spring will increase just before it begins to fail, i. e. while the water in the cavern is rising to N, will be dried up sooner in a wet summer, and break out later in a wet winter, contrary to the nature of other springs.

If the syphon MNP, fig. 12, of the reservoir ABCD, having no outlet at R, should discharge itself into a second reservoir EFGH of a smaller capacity, but furnished with a syphon STV, which discharges the water more plentifully than it comes in; a fountain derived from this second syphon STV would flow and intermit, while the first syphon MNP continued running; i. e. till the great reservoir ABCD should be emptied. After which it would entirely stop, till the said reservoir ABCD was filled again by the feeding-stream at O, and then it would flow and intermit as before.

Such a sort of compound fountain would be liable to all the variations of the former fountains derived from a single reservoir, if we take the fits of flowing and intermitting of this, for the flux of the former, and the long stop in this, while the great reservoir is filling, for the pause or intermission of the former. Besides, as the flux in the former fountains may be changed, and be made longer or shorter, so in this, the number of intermissions during one fit of flowing and intermitting, may not always be the same, because of the different capacities of the two reservoirs, and a difference or change occasioned in the feeding-stream at O. For if, while the great reservoir ABCD is emptying, the little reservoir EFGH should empty itself 9 times, for instance, and be half full again, the fountain derived from its syphon STV must have 9 intermissions in one fit, and 10 in another, alternately, while the feeding-stream at O remains the same. But the feeding-stream at O being lessened or enlarged, without making the syphon MNP run continually, the number of intermissions in each fit will be diminished or augmented accordingly. But it is peculiar to this last sort of fountains, that in each fit of flowing and intermitting, the first flux will be larger and longer than the second, and the second than the third; but the first intermission will be shorter than the second, and the second than the third; because the syphon MNP running faster at first than at last, the reservoir FFGH must be a shorter time in being filled, and a longer time in being emptied, the first time than the second, the second than the third, and so on. As to the whole time of the first flux and intermission, in comparison of the whole time of the second flux and intermission, it is a particular, requiring so many things to be taken into consideration, for determining it in each case, that he contents himself with showing that it may be longer, by an experiment that will presently be made. Another variety in this sort of fountains might be made by a second feeding-stream z, coming into the second reservoir EFGH; but the bare mentioning of that will at present be sufficient.

If in the contrivance of a single reservoir and syphon, the stream derived from the syphon should fall into another reservoir IKKL, fig. 13, having no syphon, but only a common outlet x, and should in this reservoir meet and join with another stream constantly running, a fountain derived from the said outlet x would be a reciprocating spring; by which name he calls those springs which flow constantly, but with a stream subject to increase and decrease, to distinguish them from intermitting springs, which flow and stop alternately. And if the outlet x be too small to carry off all the water brought into the reservoir IKKL, by the syphon, over and above what is brought in by the constantly running stream w; then the surface of the water in the said reservoir IKKL must continually rise, till the velocity of the stream, going out at x, is sufficiently increased, to carry off the water coming in; on which, the discharge of the syphon being continually lessened, the said surface will again subside, and the velocity of the stream at x will diminish; so that both the increase and decrease in this reciprocating fountain will be gradual. Besides, if the reservoir IKKL, or the channel derived from it, should have any leaks, crevices, or other outlets, the water will issue through them on the rising of the surface in the said reservoir, and occasion springs, which will cease again when the surface subsides.

Let us now suppose such a reservoir IKKL, fig. 14, with a constantly running stream w, and an outlet x, to receive the water of a syphon STV, coming through two reservoirs ABCD and EFGH, as before described. A fountain derived from x in this case, would be an intermitting reciprocating spring, whose stream would reciprocate; but its reciprocations would sometimes stop, and have fits of intermission.

Such, in all probability, is the fountain called Laywell, before described, whose phænomena gave occasion to these thoughts, and seem capable of being accounted for by such a contrivance. And for the better discovery of the nature of this fountain, whether it be owing to such a piece of natural machinery, or otherwise, it would be proper to observe the length of time of each increase, decrease, and pause, in every reciprocation, with the number of reciprocations in every reciprocating fit, and also the length of the intermissions of the said fits. These observations should be continued for some time, both in a settled season, when the feeding-stream at O cannot change, and in variety of seasons, when the said stream may be altered.

Mr. Atwell concludes, by presenting to view an artificial fountain of this kind. fig. 14, which being very easily made, may be buried in the bottom or slope of a terras, where a constant stream of water can be brought, and will furnish us with a new sort of water-works in gardens. The two reservoirs ABCD, EFGH, with their syphons MNP, STV, and the third reservoir IKKL, with its outlet X, are included in a box YYYY. Into this box at λ enters a funnel ΓλΓ divided within the box into two pipes, viz. λO, which serves for a feeding-stream to the great reservoir, and λW, which serves for a constant stream to the third reservoir. A stream of water being let into the funnel ΓλΓ, will discharge itself like such an intermitting reciprocating fountain at X, where there is a basin YZZZ without the box to receive it; with an outlet z, and a diagonal gage ZY, to mark the rise and fall of the water in the basin.

Eclipses of Jupiter's Satellites observed at Pekin in 1730, 1731; *with some other Astronomical Observations made there. By F. I. Koegler and Pereira.* N° 424, p. 316.

A Catalogue of Eclipses of Jupiter's Satellites for the Year 1733. *By James Hodgson, F. R. S. Mathematical Master at Christ's Hospital.* N° 424, p. 321.

Precalculated, and proposed for observation in that year.

Experiments to prove the Existence of a Fluid in the Nerves. By Alexander Stuart, M. D. F. R. S. &c. N° 424, p. 327.

Exper. 1.—Dr. Stuart suspended a frog by the fore legs in a frame, leaving the inferior parts loose; then the head being cut off with a pair of scissars, he made a slight push perpendicularly downwards, on the uppermost extremity of the medulla spinalis, in the upper vertebra, with the button-end of the probe, filed flat and smooth for that purpose; by which all the inferior parts were instantaneously brought into the fullest and strongest contraction. This he repeated several times, on the same frog, with equal success; intermitting a few seconds of time between the pushes, which, if repeated too quick, made the contractions much slighter.

Exper. 2.—With the same flat button-end of the probe, he pushed slightly towards the brain in the head, on that end of the medulla oblongata appearing in the occipital hole of the skull; on which the eyes were convulsed. This also he repeated several times, on the same head, with the same effect.

Exper. 3.—He tied a piece of fine twine, or thread, parallel to the crural artery, vein, and nerve of a dog; and made a ligature on them, and on the parallel twine, above and below, at the distance of about 4 inches; then he cut beyond the ligatures above and below, so as to take out the vessels and nerve, with the parallel twine, in one bundle; and laying them on a board, both the artery and vein contracted immediately, and were shortened to almost one half of the natural length which they had in the body, viz. to $2\frac{1}{2}$ inches; whereas the nerve remained uncontracted, at its natural length, and commensurated to the parallel twine of 4 inches, as before it was cut out of the body.

By which it appears that the proportion of the blood-vessels in their completest contraction, to the same in a state of extension, and to the nerves at their constant and natural length, is nearly as 5 to 8; or, which is the same thing, any given section of a blood-vessel, cut out and left to itself, is capable of contracting, so as to lose $\frac{3}{8}$ parts of its length.

But though this experiment may suffice for estimating the elasticity of the blood-vessels in general; yet it is not to be doubted, but the degree of their strength and elasticity may differ a little more or less in animals of different species, and individuals of the same species, nay even in the same individual at different stages of life; but these differences are not material to the present purpose, which is only to show that the nerves are not elastic, and that the blood-vessels are so to a very considerable degree.

Inferences from these Experiments.—The first two experiments show, that the brain and nerves contribute to muscular motion, and that in a very high degree.

The third experiment makes it as plain, that what they contribute in muscular motion, cannot arise from, or be owing to elasticity, which they have not.

What remains therefore but to conclude, that the action of the nerves in muscular motion, is owing to the fluid they contain, by whatever name we may choose to call it.

To fortify this conclusion, let us consider, that we can have no other evidence of the existence of that invisible fluid the air, and of its several qualities of elasticity and gravity, but what arises from experiments and observations of its effects; which are sufficiently satisfactory, and convince us of its existence, though the minute particles of its composition fall under none of our senses. Therefore, in the same manner, seeing these experiments put the elasticity and elastic vibrations of the nerves quite out of the question, we may as fairly conclude, that there is a fluid in the nerves, though invisible, as that there is such a fluid called the air, though it cannot be seen.

And though we may call this nervous fluid by any name, to which a proper, determined, and fixed idea is annexed, yet the word spirits, was an unhappy choice, as it includes an idea either of something like to the spirits of fermented liquors, or some of the saline volatile spirits, as that of hartshorn, &c. or a flying vapour or exhalation; all which being loose and indetermined, have served only to mislead the inquisitive, and amuse the ignorant.

But the source from which this fluid arises, viz. the circulating blood; the vessels through which it is secerned; and the nerves in which it moves and is contained; the soft and almost insipid taste, and no smell, observable in the brain and nerves, suggest no idea of such spirits; and the simple qualities of a pure and perfectly defecated elementary water, will better suit all that our senses can discover of it, and are indeed sufficient to solve all the phænomena of the animal economy, as far as they depend on the nerves.

Observations of Latitude and Variation, taken on board the Hartford, in her Passage from Java Head to St. Helena, Anno Dom. 1731-2. *Communicated by Edmund Halley, LL.D. Regius Astronomer at Greenwich.* N° 424, p. 331.

On Wednesday, Feb. 2, we took our departure from Java Head, allowing it to lie in the lat. of 6° 45′ south.

Feb.	7....	By a good amplitude made	3°	28′.	variat. N wly.
		Latitude by account	9	59..	south.
		Meridian distance from Java Head	0	43	west.
		Longitude from ditto..................	0	45	
....	13....	By a good azimuth made	4	45..	variat. N wly.
		Latitude by a good observation..........	13	43..	south.
		Meridian distance from Java Head	3	31	west.
		Longitude from ditto..................	3	36	
....	15....	By a good amplitude..................	4	52..	variat. N wly.
		Latitude per observation	15	18..	south.
		Meridian distance from Java Head	6	1	west.
		Longitude from ditto	6	9	
....	21....	By a good azimuth and amplitude........	4	51..	variat. N wly.
		Latitude per observation................	18	12..	south.
		Meridian distance from Java Head	17	28	west.
		Longitude from ditto	18	0	
...	25....	By good amplitude	6	8..	variat. N wly.
		Latitude per observation	19	59..	south.
		Meridian distance from Java Head........	21	17	west.
		Longitude from ditto..................	22	1	
....	29....	By a good azimuth....................	10	3..	variat. N wly.
		Latitude per observation	21	0..	south.
		Meridian distance from Java Head........	30	28	west.
		Longitude from ditto	32	12	

March	5.	By a good amplitude made	15°	15′	variat. N wly.
		Latitude per observation	23	16.	south.
		Meridian distance from Java Head	37	18	west.
		Longitude from ditto	38	58	
....	8.	By a good amplitude made	18	2.	variat. N wly.
		Latitude per observation	25	11.	south.
		Meridian distance from Java Head	40	30	west.
		Longitude from ditto	42	33	
....	10.	By an azimuth and amplitude made	19	0.	variat. N wly.
		Latitude per observation	26	18.	south.
		Meridian distance	42	42	west.
		Longitude	44	15	
....	13.	By a very good amplitude	21	45.	variat. N wly.
		Latitude per observation	27	23.	south.
		Meridian distance	44	14	west.
		Longitude from Java	46	34	
....	17.	By a good azimuth made	24	23.	variat. N wly.
		Latitude by account	30	25.	south.
		Meridian distance from Java Head	51	29	west.
		Longitude ditto	54	52	
....	19.	By a good azimuth had	24	50.	variat. N wly.
		Latitude per observation	30	27.	south.
		Meridian distance	56	40	west.
		Longitude	59	21	
....	22.	By a good azimuth had	24	15.	variat. N wly.
		Latitude per account	31	23.	south.
		Meridian distance from Java Head	61	37	west.
		Longitude from ditto	66	3	
....	24.	By a good amplitude had	23	51.	variat. N wly.
		Latitude per observation	32	47.	south.
		Meridian distance	63	0	west.
		Longitude	67	44	
April	1.	By a good amplitude made	20	16.	variat. N wly.
		Latitude by observation	34	58.	south
		Meridian distance from Java Head	73	36	west.
		Longitude from ditto	79	44	
....	4.	By a good azimuth and amplitude	20	7.	variat. N wly.
		Latitude per observation	35	33.	south.
		Meridian distance from Java Head	74	42	west.
		Longitude from ditto	81	24	
....	6.	By a good amplitude made	19	7.	variat. N wly.
		Latitude by observation	35	41.	south.
		Meridian distance from Java Head	77	2	west.
		Longitude from ditto	87	12	
....	7.	By a very good amplitude made	17	30.	variat. N wly.
		Latitude per observation	36	25.	south.
		Meridian distance from Java Head	77	56	west.
		Longitude from ditto	87	38	

April	10.	By a good azimuth and amplitude made	16°	9′.	variat. N wly.
		Latitude per observation	38	18..	south.
		Meridian distance from Java Head	77	24	west.
		Longitude from ditto	87	26	
....	13.	By a very good azimuth and amplitude.	15	40..	variat. N wly.
		Latitude per observation	37	58..	south.
		Meridian distance from Java Head	77	21	west.
		Longitude from ditto	85	15	
.. .	14.	By a very good azimuth and amplitude.	15	45..	variat. N wly.
		Latitude per observation	37	4..	south.
		Meridian distance from Java Head	76	54	west.
		Longitude from ditto	84	42	

N. B. This day he judged Cape Bonne Esperance to bear N. by W. from him, distance 2° 34′.

....	16.	By a very good azimuth made	16	14..	variat. N wly.
		Latitude per observation	36	15..	south.
		Meridian distance from Java Head	77	59	west.
		Ditto from Cape Bonne Esperance	0	30	
		Longitude from Java Head	85	14	
....	18.	By a very good amplitude made.	15	45..	variat. N wly.
		Latitude per observation.	35	33..	south.
		Meridian distance from Java Head	79	5	west.
		Ditto from Cape Bonne Esperance	1	36	
		Longitude from Java Head	86	10	
....	21.	By a very good azimuth made	14	40..	variat. N wly.
		Latitude per observation	32	23..	south.
		Meridian distance from Java Head	81	9	west.
		Ditto from Cape Bonne Esperance	3	40	
		Longitude from Java Head	87	9	
....	24.	By a good amplitude made	12	39..	variat. N wly.
		Latitude by observation	27	1..	south.
		Meridian distance from Java Head	84	52	west.
		Ditto from Cape Bonne Esperance	7	23	
		Longitude from Java Head.	89	18	
....	29.	By a good azimuth made	11	20..	variation.
		Latitude per observation	21	45..	south.
		Meridian distance from Java Head	89	8	west.
		Ditto from Cape Bonne Esperance	11	41	
		Longitude from Java Head.	92	20	
May	5.	Latitude per observation	16	0..	south.
		Meridian distance from Java Head.	97	43	west.
		Ditto from Cape Bonne Esperance	20	16	
		Longitude from Java Head.	99	53	
		By an amplitude the night before came in..	8	0..	N wly.

At noon Barn Point bore W. by N. half N. distance 4 miles.

An extraordinary Eruption of Mount Vesuvius in March 1730, *extracted from the Meteorological Diary of that Year at Naples. Communicated by Nichol. Cyrillus, M. D. R. S. S.* N° 424, p. 336.

The thermometer used in this Diary, was made by Mr. Hauksbee, in which the freezing point is marked at 65° under the point extreme hot; but the doctor observes, that at Naples water will freeze when this thermometer stands at 55° only: which, according to him, seems to argue, that there is something else besides an intense degree of cold required for freezing water; that the air of Naples abounds in it, more than the air of London; and that this may probably be of a saline nature; because when we turn water into ice by the help of snow, it is necessary to mix salt with it.

March,	Therm.		Winds.		
8,	40	0	s.	3	Cloudy weather; strong south wind. Vesuvius sent forth a great smoke and stream of fire, with hollow rumbling.
9,	38	0	w.	1	The weather cloudy. The following night Vesuvius thundered as it were twice. In the day the windows trembled a little.
10, 11, 12,	39	0	s.	1	Cloudy; rain now and then: The clouds hide the smoke and fire.
13,	41	1	N.W.	1	Weather rather clear. The smoke lessened.
14,	47	0	N.	2	A little rain in the night; in the morning snow in the mountains. In the forenoon the snow increased again. In the evening after 8 o'clock the fire arose to a vast height, and threw huge stones to almost half the perpendicular height of the mountain. Pumice stones red hot of 2 or more oz. weight, were driven several miles like a shower of hail, and frightened away the birds. In about an hour's time the height of the flame was somewhat lessened; and through the middle of the thick smoke flashes of lightning were often seen.
15,	50	0	N. E.	1	Clear weather. Thick smoke scattered the ashes many miles over the sea.
16,	48	0	s.	1	Clear in the morning; about noon cloudy, small rain and cold. By change of the winds the smoke and ashes were carried towards the N. Clouds hide the mountain.
17,	40	1	s.	1	A few thin clouds. The smoke turned with the wind.

March,	Therm.	Winds.		
18,	40	s s. w.	1	Clear. The city was sprinkled over with small ashes, like kitchen ashes, which were attracted by the loadstone.
19,	42 0	w.	1	A few thin clouds.
20,	37 0		0	Almost clear. Vesuvius became entirely quiet.

A short Account, by Mr. John Eames, F.R.S. of a Book intitled, Tuhhfat ilkibar, printed at Constantinople, Anno Dom. 1728. N° 424, p. 338.

The advantage of printing above writing, has at last prevailed with the Grand Signior to permit a printing-press to be set up at Constantinople. It was obtained on a memorial presented to him by the Grand Vizier, with the consent of the Mufti.

The privilege is granted to Zaid, the son of Mehemet Effendi, late Ambassador in France, and Ibrahim Mutafarrica, the author of a small Tract in this book. The licence extends to the printing all sorts of books written in the Oriental languages, except such as treat of the Mahometan religion.

This book seems to be one of the first that ever was printed there. For though Giauhauri's Arabic Dictionary, translated into Turkish, was what the Turks designed, for particular reasons, to begin with: yet the manuscript, from which they printed off the first sheets, was found to be so incorrect, that the Grand Signior ordered the printing of it to be stopped, till a more correct copy could be procured. During this stop, the present book was printed, containing about 150 leaves.

The language of it is not Arabic, as was supposed, but Turkish, though it has several words and some sentences that are so. The title, or what may serve as such, is Tuhhfat ilkibar, or, A Present to the Great; containing an account of several engagements at sea. The author is Hagi Califa, stiled Chelebi Almurhhum, i. e. a gentleman who has obtained mercy, or deceased.

It consists of two Tracts, a large, and a very small one. The latter is done by the publisher, Ibrahim Mutafarrica. Mutafarrica is a title of honour, signifying a horse-soldier, obliged to go to the wars when the Grand Signior goes in person, but not else.

It is a short account of geographical measures of distances, &c. particularly of the circumference of the earth.

The principal Treatise is partly historical, and partly geographical; the latter treats of the nature of the terrestrial globe, the use of maps, and the situation of places, particularly of Venice, Corfu, Albania, &c. and such as border on the Turkish dominions.

The historical part, is an account of several naval expeditions and battles, between the Turks and Christians, especially during the holy war, in the Mediterranean sea, with their conquest of the islands and places of note on the sea-coasts. It is an abridgment of the history of their admirals, from the taking of Constantinople, to the year 1653; gives a description of the Grand Signior's Arsenal at the Porte, with the charges of maintaining it; and concludes with some directions to the Turkish officers.

Besides a general map of the world, there are three others; one of the Mediterranean, or White Sea, as the Turks call it; another of the Archipelago, and the third of the Venetian Gulph: they have each scales of Turkish, French and Italian miles annexed; but, what is rather surprising, these maps have the degrees of latitude, but not of longitude, marked on the sides.

The last plate has two mariner's compasses, the one containing the Turkish names of the several points; the other, besides the Turkish, has the Arabic names, which are taken from certain stars supposed to rise and set on or near those points.

The book ends with words to this sense: this Treatise was done by the persons employed in the business of printing, at the printing-house erected in the noble month of Dulkaadah, in the year 1141, (i. e. 1728) in the good city of Constantinople. May God, &c.

It has prefixed the Imprimatur, or Commendations of a Turkish divine, and three Effendis; and at the end, an Index of the Errata, with their Emendations in three pages. The whole is done on shining or gummed paper, stamped with three crescents in pale, and an Imperial crown, proper to the Turks.

Observations made on Board the Chatham-Yacht, Aug. 30 *and* 31, *and Sept.* 1, 1732, *in pursuance of an Order of the Lords of the Admiralty, for the Trial of an Instrument for taking Angles, described in the Philos. Trans.* N° 420. *By John Hadley, Esq. V. P. R. S.* N° 425, p. 341.

In N° 420, Mr. Hadley communicated to the Royal Society the description of a new instrument for taking angles, and produced a specimen of an instrument made accordingly. Several of the gentlemen to whom it was shown, as well then as at other times, entertained a favourable opinion of the probability of its usefulness, particularly Dr. Halley and Mr. Bradley, not only expressed their desire that trial should be made of it at sea, but promised the favour of their company and assistance on that occasion.

The instrument produced at the Society was made of wood, and was intended chiefly for taking altitudes of the sun, moon and stars, from the visible horizon, either forwards or backwards. Mr. Hadley therefore procured another to be made of brass by Mr. Sisson, for taking the distance of any kind of objects. It is supported by a single stem screwed to it on the under side, the lower end of which may rest on the ground, to ease the observer of the weight of the instrument. This stem is also made to lengthen or shorten, by which means the instrument is brought to the proper height for any observer's eye, either standing or sitting. Instead of a ball and socket, it has two circular arches fixed on its back, by which it is readily set to any position which the situation of the objects may require.

The commissioners of the Admiralty having been pleased to order the Chatham-Yacht for the trial of the instrument, and to give directions to Mr. James Young, master attendant at Chatham, a gentleman well skilled in navigation, to be present at the trial, Mr. H's two brothers and himself went on board accordingly Aug. 30; being favoured with the company also of Sir Rob. Pye, Bart. and Rob. Ord, Esq. Members of this Society. They met Mr. Young at Sheerness the next day, who accompanied them down about 3 leagues below the Nore, near the Spile-Sand, and was on board on Friday, Sept. 1, when they lay by there, and the several altitudes of the sun were taken as it approached the meridian from about 10 o'clock till noon.

The Observations were as follow:—Aug. 30, near midnight, Mr. Bradley observed the distance of Lucida Lyræ from Cor Aquilæ by the brass instrument off Gravesend in still water .... 34° 13′ 30″

The same repeated was 34 13 15

The error of the instrument in that place is 23″ to be subtracted.

The distance of those stars, according to Mr. Flamsteed, is 34 11 50

Which by the refraction is reduced to 34 11 10

Aug. 31, about $10^h\ 30^m$, Mr. Bradley observed the distance of Capella from the North Pointer in the Great Bear's back, by the same instrument, while we lay at anchor in the mouth of the Medway near Sheerness, the wind blowing hard at north east .... 49° 14′ 0″+

Or 49 15 0

Mr. Bradley and myself making a small difference in numbering the angle marked by the index.

The error of the division of the instrument there is 30″ to be added.

The distance of those stars, according to Mr. Flamsteed, is .. 49° 16′ 0″

By the refraction reduced to 49 14 20

Clouds coming up prevented the repeating this observation, nor had we any opportunity of making any others of this kind.

The following altitudes of the sun were observed by Mr. Bradley, lying at anchor in the mouth of the Medway. Aug. 31, afternoon, the wind at north-east, a fresh gale, by the wooden instrument forwards. The watch, by the mean of the observations, appeared to be about 8^{m} 45^{s} too slow; the visible horizon being supposed 3′ 30″ depressed below the true, by the height of the observer's eye above the surface of the water, amounting to about 8 or 9 feet.

Time by Watch.	True Time.	True Alt. of Sun's upper Limb from the visible Horizon.	Refraction, add.	App Alt. of Sun's upper Limb from the visible Horizon.	Alt. of the Sun's upper Limb observed.	Errors of Division of the Inst. Substr.	Observed Alt. of the Sun's upper Limb corrected.	Errors of Observations.
5h 11m 50s	5h 20m 35s	9° 50′ 31″	4′ 54″	9° 55′ 25″	9° 57′ 0″	2′ 15″	9° 54′ 45″	− 0′ 40″
16 30	25 15	7 0	5 17	12 17	13 30	2 15	11 15	− 1 2
18 20	27 5	8 49 52	5 27	8 55 19	8 57 30	3 0	8 54 30	− 0 49
21 20	30 5	21 51	5 44	27 35	30 0	3 0	27 0	− 0 35
28 5	36 50	7 18 44	6 28	7 25 12	7 27 30	2 0	7 25 30	+ 0 18
30 35	39 20	6 55 22	6 46	2 8	5 0	2 15	2 45	+ 0 37
32 25	41 10	38 13	7 3	6 45 16	6 48 0−	2 15	6 45 45−	+ 0 29−
36 30	45 15	0 0	7 40	7 40	10 0	2 15	7 45	+ 0 5
38 37	47 22	5 40 11	8 3	5 48 14	5 51 0	2 15	5 48 45	+ 0 31
40 35	49 20	21 50	8 24	30 14	34 0−	2 15	31 45−	+ 1 31−
42 34	51 19	3 14	8 50	12 4	15 0	3 0	12 0	− 0 4
43 50	52 55	4 51 24	9 7	0 31	3 30	3 0	0 30	− 0 1

The following altitudes of the sun were observed Sept. 1, before noon, under sail from Sheerness towards the Spile-Sand, with the tide of ebb, the wind blowing hard at north east, by the wooden instrument forward. The second speculum being removed by some accident from its due position, so as to increase the angles observed about 1° 3½′, as appeared by the first observations of the afternoon of the same day, made with the same instrument, in the same manner, while they continued lying-by near the Spile; which 1° 3½′ are added to the errors of the divisions of the instrument in the 7th column. While these observations were making. the yacht steered at first chiefly east, sometimes south east, afterwards stood to the north-east, towards the Swin. The time of the watch was regulated by some of the later observations made when they were most eastward, and this was probably the cause why the first altitudes which were taken, while they were more westerly, fall so much short of the computations, the difference decreasing gradually as they advanced towards the east.

Altitudes observed by Mr. BRADLEY.

Time by Watch.	True Time.	True Alt. of the Sun's lower Limb from the visible Horizon.	Refraction, add.	App^t Alt. of the Sun's lower Limb from the visible Horizon.	Altitude of the Sun's lower Limb observed.	Errors of the Instrument substract.	Observed Altitude of the Sun's lower Limb corrected.	Errors of Observations.
7h 9m 15s	7h 18m 15	15° 5′ 39″	3′ 15″	15° 8′ 54″	16° 9′ 0″	1° 5 45″	15° 3′ 15″	−5′ 39″
11 44	20 44	28 13	3 11	31 24	33 0−	1 5 45	27 15−	−4 9+
13 38	22 38	45 23	3 8	48 31	49 0	1 5 45	43 15	−5 16
14 53	23 53	56 43	3 5	59 48	17 2 0	1 5 45	56 15	−3 33
16 33	25 33	16 11 47	3 2	16 14 49	18 0−	1 5 45	16 12 15−	−2 34+
18 4	27 4	25 31	2 59	28 30	32 0	1 5 45	26 15	−2 15
23 54	32 54	17 18 0	2 50	17 20 50	18 25 0	1 6 30	17 19 30	−1 20
25 38	34 38	33 31	2 47	36 18	43 0+	1 6 0	37 0+	+0 42+
28 25	37 25	58 23	2 43	18 1 6	19 7 0−	1 6 0	18 1 0−	−0 6+
30 44	39 44	18 19 4	2 40	21 44	28 0	1 5 0	23 0	+1 16
34 21	43 21	51 10	2 35	53 45	20 0 0	1 4 30	55 30	+1 45
36 24	45 24	19 9 18	2 33	19 11 51	16 0	1 4 30	19 11 30	−0 21
38 44	47 44	29 54	2 30	32 24	38 0−	1 4 30	33 30−	+1 6−unc.
40 30	49 30	45 27	2 28	47 55	52 0+	1 4 30	47 30+	−0 25−
42 0	51 0	58 41	2 26	20 1 7	21 4 0+	1 4 30	59 30+	−1 37−
45 34	54 34	20 29 51	2 22	32 13	35 0	1 4 30	20 30 30	−1 43
The same continued by Mr. JOHN HADLEY.								
7h 52m 18s	8h 1m 18s	21° 28′ 21″	2′ 15″	21° 30′ 36″	22° 36′ 0″−	1° 4′ 30″	21° 31 30 −	+0′ 54″−
54 18	3 18	45 43	2 13	47 56	52 0	1 4 30	47 30	−0 26
55 40	4 40	57 25	2 12	59 37	23 4 0	1 4 30	59 30	−0 7
58 22	7 22	22 20 34	2 9	22 22 43	30 0	1 5 15	22 24 45	+2 2unc.
8 2 51	11 51	58 46	2 6	23 0 52	24 4 0	1 5 15	58 45	−2 7
9 19	18 19	23 53 26	2 0	55 26	25 0 0+	1 5 30	23 54 30+	−0 56−
13 10	22 10	24 25 35	1 57	24 27 32	32 0	1 5 30	24 26 30	−1 2
14 45	23 45	38 43	1 56	40 39	45 0+	1 5 30	39 30+	−1 9−
16 55	25 55	56 24	1 55	58 19	26 3 0	1 5 30	57 30	−0 49
19 5	28 5	25 14 34	1 53	25 16 27	22 0+	1 5 30	25 16 30+	+0 3+
22 57	31 57	46 14	1 51	48 5	52 0−	1 5 30	46 30−	−1 35+
25 5	34 5	26 3 35	1 49	26 5 24	27 10 0−	1 5 30	26 4 30−	−0 54+
26 43	35 43	16 50	1 48	18 8	22 0+	1 5 30	16 30+	−1 38−
28 20	37 20	29 54	1 47	31 41	35 0	1 5 30	29 30	−2 11

The following altitudes of the sun were observed lying-by near the Spile, Sept. 1, before noon, with the wooden instrument backward, the wind continuing to blow hard, as before, at north-east. The instrument when used for the back observation was so adjusted, as to allow for a dip of the visible horizon of 2½′; consequently that dip being supposed, as before, 3½′, there remains only 1′ to be accounted for, in computing the height of the sun, which is accordingly subtracted in the the third column from the altitudes found by computation. The watch now appeared to be 9m 30s too slow.

Altitudes observed by Mr. JOHN HADLEY.

Time by Watch.	True Time.	True Alt. of the Sun's upper Limb.	Refraction, add.	App^t Alt. of the Sun's upper Limb.	Alt. of the Sun's upper Limb observed.	Errors in the Division. Subtr.	Observed Altitude of the Sun's upper Limb corrected.	Errors of Observation.
9^h 52^m 55^s	10^h 2^m 25	36° 52′ 54	1′ 11″	36° 54 5″	36° 46′ 0″	1′ 0″	36° 45′ 0″	−9′ 5″
10 2 7	11 37	37 43 37	1 9	37 44 46	37 44 0	1 0	37 43 0	−1 46
6 0	15 30	38 4 0	1 8	38 5 8	38 4 0	1 0	38 3 0	−2 8
8 53	18 23	19 0	1 8	20 8	22 0+	1 0	21 0+	+0 52+
12 25	21 55	36 25	1 7	37 32	41 0	1 30	39 30	+1 58
16 30	26 0	56 9	1 6	57 15	39 0 0	1 30	58 30	+1 15
18 50	28 20	39 7 7	1 6	39 8 13	6 0	0 30	39 5 30	−2 43
20 40	30 10	15 26	1 6	16 32	14 0	0 30	13 30	−3 2
The same continued by Mr. BRADLEY.								
10 30 18	10 39 48	39 57 28	1 4	39 58 32	40 0 0	2 0	39 58 0	−0 32
33 23	42 53	40 10 2	1 4	40 11 6	12 0	2 0	40 10 0	−1 6
35 53	45 23	19 51	1 4	20 55	16 0−	2 0	14 0−	−6 55+
37 48	47 18	27 13	1 3	28 16	32 0−	2 30	29 30−	+1 14−
39 22	48 52	33 3	1 3	34 6	36 0	2 30	33 30	−0 36
Continued by Mr. HENRY HADLEY.								
11 8 5	11 17 35	41 59 36	1 0	42 0 36	42 15 0+	2 30	42 12 30+	+11 54+
16 20	25 50	42 16 36	1 0	17 36	20 0	2 30	17 30	— 0 6
22 0	31 30	25 48	0 59	26 47	31 0	2 45	28 15	+ 1 28
24 20	33 50	29 35	0 59	30 34	34 0	2 45	31 15	+ 0 41
28 0	37 30	34 27	0 59	35 26	39 0	2 45	36 15	+ 0 49
33 45	43 15	40 32	0 58	41 30	45 0−	3 0	42 0−	+ 0 30−
37 45	47 15	43 43	0 58	44 41	48 0+	3 0	45 0+	+ 0 19+
40 30	50 0	45 25	0 58	46 23	49 0	3 0	46 0	— 0 23
43 0	52 30	46 34	0 58	47 32	51 0	3 0	48 0	+ 0 28
47 0	56 30	47 42	0 58	48 40	52 0	3 0	49 0	+ 0 20
again.					52 0	3 0	49 0	+
again.	12 0 0	48 2	0 58	49 0	52 0	3 0	49 0	0 0

Between each of the last five of these observations, the Index was removed so as to make them entirely independent of each other; and from their near agreement among themselves, and with good part of the preceding, Mr. Hadley concluded the true height of the sun's centre above the real horizon at noon, was exactly enough 42° 33′, his semidiameter being 16′; from which, and the sun's declination 4° 1′, the latitude of the place will be 51° 28′, which is accordingly used in all the computations.

Altitudes of the sun observed Sept. 1, 1732, afternoon, near the Buoy of the Spile, and under sail westward, by the wooden instrument forwards, the second speculum remaining displaced as in the morning.

Altitudes observed by Mr. BRADLEY.

Time by Watch.	True Time.	True Alt. of the Sun's lower Limb from the visible Horizon.	Refraction, add.	Appt Alt. of the Sun's lower Limb from the visible Horizon.	Altitude of the Sun's lower Limb observed.	Errors of the Instrument subtract.	Observed Altitude of the Sun's lower Limb corrected.	Errors of Observation.
12^{h} 7^{m} 30^{s}	12^{h} 17^{m} 0^{s}	42° 12′ 13″	1′ 0″	42° 13′ 13″	43° 20′ 0″	1° 6′ 0″	42° 14′ 0′	+0′ 47″
8 30	18 0	11 9	1 0	12 9	19 0	1 6 0	13 0	+0 51
12 0	21 30	7 18	1 0	8 18	13 0	1 6 0	7 0	−1 18
19 50	29 20	41 56 0	1 0	41 57 0	1 0	1 6 0	41 55 0	−2 0
The same continued by Mr. HENRY HADLEY.								
1 0 0	1 9 30	40 9 21	1 3	40 10 24	41 13 0	1 6 0	40 7 0	−3 24 unc.
1 35	11 5	3 20	1 4	4 24	10 0	1 6 0	4 0	−0 24
3 2	12 32	39 57 59	1 4	39 59 3	4 0	1 6 0	39 58 0	−1 3
4 21	13 51	52 57	1 4	54 1	2 0	1 6 0	56 0	+1 59
6 14	15 41	45 31	1 4	46 35	40 52 0	1 6 0	46 0	−0 35
7 30	17 0	40 28	1 4	41 32	49 0	1 6 0	43 0	+1 28
8 43	18 13	35 37	1 5	36 42	40 0	1 6 0	34 0	−2 42 unc.
10 0	19 30	30 28	1 5	31 33	38 0	1 6 0	32 0	+0 27
11 29	20 59	24 22	1 6	25 28	34 0	1 6 0	28 0	+2 32
14 23	23 53	11 46	1 6	12 52	18 0	1 6 0	12 0	−0 52

The 1st and 6th columns, of the preceding Tables of Observations, are copied from the minutes as they were set down at the time. The divisions of the wooden instrument being not exact, Mr. H. found it necessary to make a Table to correct them by, which was done partly by measuring with compasses, and partly by examining them against those of another instrument. The corrections are every where to be subtracted from the angles observed, and the errors of 1° 3½′, occasioned by the misplacing the 2d speculum, in all the forward Observations of Sept. 1, being of the same kind, are joined with them, in the 7th column of the Tables of those Observations. The last column contains the differences between the observed Altitudes, corrected by the aforesaid Table, and the Altitudes as they ought to have appeared by the computations. Among them there are two or three which so much exceed any of the rest, that for that reason they seem to be rather owing to mistakes, in counting the minutes on the instrument, or the time by the watch, than to the Errors of the Observations.

The greatest part of the altitudes were taken by a horizon not clear of land, and by that means not always so readily distinguishable. The observers were all persons quite unaccustomed to the motion of a ship at sea, which in this case was generally very great and quick, the vessel we were in being only of about 60 tons burden, the smallness of which made it also more liable to be lifted up and let down again by the waves: and if the difference of height occasioned by that means was about 4 or 5 feet, as we judged it to be, it must necessarily sink and raise the visible horizon by turns near one minute. The computations of the sun's altitudes are all made for the latitude of 51° 28′; whereas a good part of them were taken under sail, and on different tacks, the vessel sometimes standing north-east or north, and at other times south-east, for near a quarter of an hour at a time.

Several of these circumstances may probably have contributed to increase the inconsistency of the observations; but as no particular notice was taken of them at the time, they are here barely mentioned.

Postscript.—The principle on which the contrivance of this instrument depends, was laid down in the beforementioned Philos. Trans. N° 420, in one proposition, and several corollaries, the 5th of which contains the grounds of an approximation for correcting some small errors, which will arise if the plane of the instrument be suffered to vary too much from the great circle passing through the two objects, when the observation is taken. There appears reason to think, that there will be very little occasion in practice for that correction; but it was necessary to mention it, in order to explain the nature of the instrument; and as the manner of deducing that corollary from the proposition may not appear obvious to every reader, Mr. Hadley has here annexed the demonstration of it.

Let OBC, fig. 1, pl. 15, represent an infinite sphere, at whose centre R are placed the two specula inclined to each other in any given angle; and let their common section coincide with the diameter ORC. Let BAN be the circumference of a great circle, to the plane of which the common section of the specula ORC is perpendicular, and BR its radius: let ban be the circumference of a circle parallel to BAN, and at the distance from it Bb: draw bD the sine, and br the sine complement of the arch Bb: BD is the versed sine of the same. Let A be a point of an object placed in the circumference of the great circle BAN, and N the point in which its image is formed by the two successive reflections, as before described; and let a be a point of another object placed any where in the circumference of the parallel ban, and n its image; and let ahn be an arch of a great circle passing through the points a and n. The point a is at the same distance from the great circle BAN, as the point b, i. e. at the distance Bb. Draw AR, AN, RN, ar, an, rn, aR and nR.

By corol. 4, the figures ARN and arn are similar; consequently the line AN is to the line an, as AR or BR, is to ar or br, i. e. as the radius is to the sine complement of the distance Bb. But AN is the chord of the arch AHN of the great circle BAN, equal to the translation of the point A, or double the inclination of the specula; and an is the chord of the arch ahn, of a great circle measuring the angle ARn, by which the point a appears removed by the two reflections, to an eye placed in the centre R. Therefore the translation, or apparent change of place, of the point a, is measured by an arch of a great circle, whose chord is to the chord of the arch AHN (equal to double the inclination of the specula) as the sine complement of its distance from the great circle BAN, is to the radius.

From any point C of the circumference OBC, draw the chords CM and Cm, to the same side of the point C, and equal to the chords AN and an respectively; draw the radius RM; and from R and m, draw RQ and mP, both perpendicular to CM, and cutting it in Q and P. RQ is the sine complement, and CM double the sine of half the angle MRC, or ARN, or of the angle of inclination of the specula. The little arch Mm will represent the difference of the apparent translations of the objects in A and a; and if it be very small, may be considered as a straight line; and the small mixed triangle MmP as a rectilinear one, which will be similar to RMP, because RM is perpendicular to Mm, and RQ to CM, and the angles at Q and P right angles. The line CP may be taken as equal to Cm, and MP as the difference of the lines CM and Cm. Therefore the small arch Mm, is to the line MP, nearly as RM to RQ: but CM (i. e. AN) was to Cm (i. e. an), as BR to br, and the difference MP, of CM and Cm, to the difference BD, of BR and br, as CM to BR. Therefore Mm, the difference of the apparent translations, is to BD, the versed sine of the distance Bb, or to an arch equal to it, in the compound ratio of RM the radius, to RQ the sine complement of the angle of inclination of the specula, and CM double the sine of the same, to BR the radius, i. e. as CM to RQ.

The observation may be corrected by one easy operation in trigonometry, as will appear from the first part of this corollary, viz. by taking the half of the angle observed, and then finding another angle, whose sine is to the sine of that half, as the sine complement of the distance Bb, is to the radius: this angle doubled, will be the true distance of the objects. But as this operation, though easy, will require the use of figures, Mr. H. rather chooses the method of approximation, because by that the observer, retaining in his memory the proportions of the sines of a few particular arches to the radius, may easily estimate the correction without figures, when the angle is not great, and by a line of artificial numbers and sines, may always determine it with greater exactness than will ever be necessary.

When the angle observed is very near 180 degrees, the correction may be omitted; for then it will be easy to keep the plane of the instrument so near that of the beforementioned great circle, as not to want any, if the situation of that circle be known: if it be not, the observer, when he sees the two objects together, may turn the instrument on the axis of the telescope, till he finds that position of it by which he obtains the least angle; and this, if the specula are set truly perpendicular to the plane of the instrument, will always happen when the objects appear to coincide in the line gh, as expressed in fig. 3, pl. 13.

In N° 420, a rule is given for finding to which hand of the observer the object seen by reflection ought to lie, but is restrained to the particular form of the instrument there described. The general rule is, that when the index is brought to the beginning of the scale (i. e. to 0° when the instrument is designed for angles under 90°, or to 90° when it is designed for angles from 90° to 180°) if then a line be conceived to be drawn on it, parallel to the axis of the telescope, or line of direction of the sight, so as to point towards the object seen directly; which ever way this line is carried by the motion of the index along the arch from 0° towards 90° in the first case, or from 90° towards 180° in the second, the same way the object seen by reflection ought to lie from that which is seen directly.

Ephemerides Meteorologicæ, Barometricæ, Thermometricæ, Epidemicæ, Magneticæ, Ultrajectinæ, conscriptæ à Petro Van Muschenbroek, L. A. M. Med. et Phil. D. Phil. et Mathes. Profess. in Acad. Ultraj. Anno. 1729. N° 425, p. 357.

May be consulted in this author's works.

A Discourse concerning the difficulty of curing Fluxes, written occasionally on reading Dr. de Jussieu's Memoir in the History, &c. of the Royal Academy of Sciences in Paris, for the Year 1729. *By William Cockburn, M.D., F.R S. and of the Coll. of Physicians, London.* N° 425, p. 385.

There is nothing in this paper sufficiently interesting for republication.

An Account of a Comet seen Feb. 29, 1731-2. *By Mr. John Dove.* N° 425, p. 393.

Feb. 29, at about half an hour past 10 at night, being in lat. 34° 28′ south, and long. 12° 35′ west from Cape Bonne Esperance, the moon shining very bright, being near the full, they saw something very bright rise about west, which Mr. Dove judged to be a comet: it set about east, passing from west to east in about 5 minutes, between the moon and the zenith, and to the southward of Spica Virginis. It carried a stream of light after it about 40° long, and 1° or 1½° broad; the brightness of the moon outshone the comet as it came near her.

Two Experiments on the Friction of Pulleys. By the Rev. J. T. Desaguliers, LL. D. F. R. S. N° 425, p. 394.

The first experiment was made with a tackle of 5 brass sheevers in iron frames or blocks; that is, 3 sheevers in the upper block, and 2 in the lower. The sheevers were 5 inches diameter, the pins half an inch, and the rope three quarters. Having made an equilibrium, by hanging one hundred and a quarter at the lower block, and a quarter of a hundred at the running rope; he added 17½ pounds before the power could go down and raise the weight.

Exper. 2.—Two hundred and a half being balanced by half a hundred, the addition of 28lb. made the power raise the weight.

In the experiment 17½ pounds exceeds by 4½ pounds the sum of the frictions deduced from the theory. But in the second experiment 28 pounds exceeds the sum of the friction but 1 pound.

The reason of this appeared to be, that the rope at first was too large for the cheeks that held the sheevers; but in the second experiment, where the rope was more stretched, it was somewhat diminished in diameter, and so brought off from rubbing so hard against the cheeks.

From knowing the quantity of friction à priori, in such large tackles, we may know what to expect in practice: for if one man, who for a small time can exert the force of 100 pounds, thinks that he may draw up a stone, or a roll of sheet-lead, or any other such weight, to the top of a house, with a tackle of 5, because this would seem feasible from mechanical principles, will find himself mistaken, on account of the friction, which will not be surmounted without an additional force of 50 pounds.

Further Experiments concerning Electricity. By Mr. Stephen Gray, F. R. S. N° 426, p. 397.

About the latter end of August, 1732, being at Mr. Wheler's, after having repeated the experiment of making sulphur attract leaf-brass in vacuo, they suspended from the top of a receiver, which was first exhausted of air, a white thread, that hung down to about the middle of it. Then the receiver being well rubbed. the thread was attracted by it vigorously. When it was at rest, and hung perpendicular, the tube was rubbed, and being held near the receiver, the thread was attracted towards that side of it; when the tube was removed slowly, the thread returned to the centre of the receiver; but when moved swiftly, the thread was attracted by the opposite side of the receiver. When the hand was held near the receiver, and moved hastily from it, the thread was attracted by the opposite side, as before. This seemed at first difficult to account for; but on further consideration, they concluded it proceeded from the motion of the air made by the tube, and in the other case by that of the hand, which took off the attraction from that side, and not on the other side; so that by this means the balance of the attraction was taken off.

They made another experiment, by suspending a thread on the top of a small receiver, and whelming a large one over it; then by first rubbing this, and holding the rubbed tube near it, the thread in the middle receiver was attracted to that side of it where the tube was held.

An Experiment, showing that Attraction is communicated through opacous as well as transparent Bodies, not in vacuo.—There was taken a large hand bell, the clapper being first taken out, and a cork suspended by a thread from the top of the bell, the cork being smeared over with honey: then the bell was set on a piece of coach-glass, which had been well rubbed, on which the leaf-brass was laid; then the tube being rubbed, and held near the handle of the bell, and afterwards near the top and side of the same, the bell being taken off, there were several pieces of the leaf-brass sticking to the honeyed cork that had been attracted by it: it appeared also that some other pieces had been attracted by the bell, being removed from the places they were left in, when covered by it.

Some time after Mr. Wheler mentioned an experiment he had made in vacuo. He took a small receiver, and in it suspended a thread, and over this 4 other receivers, all exhausted, and the thread was attracted through all the 5 receivers; and he thought the attraction was rather stronger than before, when a single receiver only was made use of; but instead of wet leather, he made use of a cement Mr. G. had recommended to him, viz. bees-wax and turpentine, which was what Mr. Boyle used in his experiments with the air-pump, and that, as he had told him, the attractions would probably be much stronger; the steams of the wet leather taking off some of the attracting force.

Mr. Gray proceeds to give some account of the experiments made at Mr. Godfrey's: the first of which was giving an attraction by the tube to a boy suspended on hair-lines; when also, by the intervention of a line of communication, the attractive virtue passes to another boy that stands at several feet distant from him. The next is an experiment of the attractive power communicated to the boy standing on rosin.

June 16, 1731, in the morning, Mr. Gray made the following experiment on the boy, causing him to become attractive by suspending him on hair-lines. There was taken two pieces of white rosin, made into round flat cakes of rather more than 8 inches diameter, and 2 inches thick. These were laid down on the floor of his chamber, so near together, that the boy might stand with one foot on one, and the other on the other cake of rosin: then the leaf-brass being laid under his hands, the tube rubbed, and held near his legs, caused both his hands to attract and repel the leaf-brass to the height of several inches: or when there was laid leaf-brass under one hand, and the tube held near the other hand, there was an attraction communicated to the farther one; and when the tube was applied either to his hands or feet, there was an attraction given to his clothes; so that a piece of white thread being held by one end, the other end would be attracted at near the distance of a foot; so that the attraction is altogether as strong, if not stronger, than when the boy was suspended on hair-lines.

Now as to the first experiment at Mr. Godfrey's: one of the boys being suspended on the hair-lines, and the other standing on the two cakes of rosin, the boys holding hands with each other, under the boy's hand that stood on the rosin was laid the leaf-brass; then the tube being rubbed, and held near the boy's feet that hung on the hair-lines, the hand of the boy that stood on the rosin attracted strongly. Then there was taken a 4-foot rule, and given to the boys to hold by each end, and the same virtue of attraction was given to the other boy as before. After this, a line of packthread was given them to take hold of by the ends, and then an attraction was communicated from one end to the other, with as much vigour as by any of the other methods beforementioned. This experiment was made Sept. 13, 1732.

Sept. 14, Mr. Gray first made the following experiment. There was taken a rod, which was composed partly of wood and partly cane: it was 24 feet in length, and in form not unlike two fishing rods supposed joined together at their larger ends. This rod was suspended horizontally by two threads of silk: over this, at about 2 feet from the end, was suspended a small hazel wand, about 5 feet long, at right angles to it, but not touching the rod: then going to the other end of the rod, the tube being excited and held near it, repeating the same 3 or 4 times as usual, and going to the hazel wand with a small white thread, he found that it was attracted when held near any part of it. The next day he repeated the experiment, and found that by suspending the wand at several heights, he could perceive there was an attraction, when it was at the height of more than 12 inches.

Sept. 29, Mr. Gray repeated the experiment on two boys; first setting one of them on cakes of rosin, and suspending the other on the hair-lines; and the effect was the same as above related. He then caused both the boys to stand on cakes of rosin, giving them to hold a part of a Spanish cane fishing-rod, of 8 feet long, one boy holding one, and the other boy the other end of the rod; then the leaf-brass being laid on the stand, and one of the boys holding his hand over it; Mr. Gray went to the other boy, and the excited tube being held near the palm of his hand, the first boy's hand attracted and repelled the leaf-brass strongly. A piece of packthread was then given them to hold by each end, about the same length with the rod, viz. 8 feet long. Under each of their hands was laid leaf-brass; then going to the middle of the line, holding the tube near it, the farther hand of both the boys attracted the leaf-brass with so much vigour, that it is not to be doubted that had the line been much longer, they would have attracted at a far greater distance. He then caused the boys to stand on the cakes of rosin, so as to let the flaps of their coats touch, and then by holding the tube to one of their hands, the other hand attracted, but not with more force than when they were distant the length of the line. They then stood so much farther as not to let their coats touch by about an inch, and then exciting one of them to attract, the other received not the least degree of attraction. He then bid one boy put his finger upon the other boy's wrist, and then he immediately became electrical.

Oct. 4, Mr. Gray made the following experiment: a fishing rod of about 10 feet 8 inches long, being horizontal, and over it, towards the less end, a small rod, being the top end of another fishing-rod, at the less end, which was whalebone, there was put on a ball of cork 2 inches diameter, the small rod touching the great one; then the tube being excited, and held near the large end of the great rod, applying it as usual; on going to the cork with a pendulous thread, he found it attracted it at the distance of at least 2 inches. Then the rod was moved higher, so as not to touch the end of the long rod, by estimation about an inch, and after several trials, there was a visible attraction, when the little rod that carried the ball was above the great one 34 inches.

Oct. 5, he took a line of packthread 17 feet 4 inches long, with silk lines tied to the ends of the packthread, one of them about 4, the other 2 feet long, near two of the opposite corners of his chamber, where in each of them was driven a hook at about $3\frac{1}{2}$ feet high, to which the ends of the silk were fastened, drawn so tight as to bear the packthread nearly horizontal: then the small par of the fishing-rod was suspended over the packthread at about 4 feet from the end; then the tube being applied to the other end of the packthread, the cork ball at the end of the little rod was attractive, and at several removes, to the height of 47 inches, there was a visible attraction of the pendulous thread.

Oct. 6, instead of the small rod, he took a packthread about 4 feet long; and having tied silk threads to each end, by which the thread was suspended over the longer line horizontally, and at right angles nearly to the said line, which was by tying the ends to perpendicular lines of packthread fastened to hooks at each end, and had sliding knots on them, so that the cross line might be moved higher or lower occasionally: on one end of this line he put a ball of cork; and found that when the first line had been excited, the virtue was carried up to the second line, and caused the cork ball to attract. He then took off the cork ball, and put one of ivory in its place; and this attracted after the same manner. Afterwards he hung two ivory balls, one at each end of the line; and found there was a sensible attraction when the line that supported them was raised 38 inches above the line of communication.

Oct. 30, he repeated this experiment: and now, when the line that supported the ivory balls was elevated about an inch above the communicating line, either ball attracted the thread at the distance of more than its semi-diameter; and at the height of 10 inches, at least half the same distance.

By these experiments we find, that the electric virtue may not only be carried from the tube by a rod or line to distant bodies, but that the same rod or line will communicate that virtue to another rod or line at a distance from it, and by that other rod or line the attractive force may be carried to other distant bodies.

A small hoop, of about 20 inches diameter, and an inch and a half in breadth, suspended by two threads of silk, so that it hung perpendicular, and in a plane at right angles to the horizontal line of communication, which passed through, or at least very near the centre of the hoop, he went to the end of the said line, and applying the excited tube near it, an attractive influence was communicated to the hoop in all parts of it. Then by a screw-hole, made in the side of the hoop for that purpose, he screwed it on the top of a pedestal about 2½ feet in height, setting it on a cake of rosin, so that the beforementioned line might pass through the centre of the hoop; and he found that whether the hoop was placed so as its plane was at right angles, or in any other angle with the line of communication, the hoop attracted after the same manner as it had done when suspended on the silk lines.

Some time after he made the following experiment. Into the nose of a glass funnel he put the large end of the top of a small fishing-rod, and on the less end a ball of cork; then the funnel was set on the floor of the room, so as that the rod was at some inches distance from the line of communication; then the tube being excited, and applied near the end of the line, the electric virtue was conveyed by it to the cork ball, and it attracted strongly when the ball was not less than 2 feet distance from the line.

Dec. 11, being a hard frost, and a fair day, he repeated the experiment, making use of a large hoop about 40 inches diameter, and setting it perpendicular on a hollow cylinder of glass, 6 inches long, and 5½ inches diameter, so placing the hoop that the line of communication might pass through, or at least very near the centre of it; then applying the tube to the end of the line, an attraction was communicated to all parts of the hoop, attracting a pendulous white thread at the distance of about half an inch. He then set the hoop so as the inner surface of it might touch the line; then communicating an attraction by the excited tube to the packthread, the attractive virtue was carried by it to the hoop, causing it to attract with that force, as with the remotest part of the hoop to attract the thread at a distance of about 4 inches.

Some time after he made the following experiment. The large hoop being set upon the glass cylinder, and the packthread passing through, or near its centre, the tube being applied near the hoop, gave it a strong attraction, so that it would attract a thread at the distance of 7 or 8 inches, and at the same time an attraction was communicated to the packthread. He then suspended an ivory ball, of 2 inches diameter, at the other end of the packthread, and applying the tube to the hoop, an attractive virtue was carried to the ball, so that it would attract the pendulous thread at the distance of near an inch. He then placed the ball in or near the centre of the hoop; and now it was so far from being attracted, that it was repelled by the ball; but it was attracted by the packthread passing to it in the arch of a circle, whose centre seemed to be that of the ball.

Ephemerides Meteorologicæ, Barometricæ, Thermometricæ, Epidemicæ, Magneticæ, Ultrajectinæ, conscriptæ à Petro Van Muschenbroek, &c. Ultraj. Annis 1730 *et* 1731. N° 426, p. 408.

Omitted for the reason before given.

Of a very extraordinary Fossil Skull of an Ox, with the Cores of the Horns. By Mr. Jac. Theod. Klein, Secret. Dan. et F. R. S. N° 426, p. 427.

Near the city of Dirschaw was dug up part of the skull of an ox, with the cores of the horns, which in all probability must have been prodigious.

The root of the horns was 1 foot 6 inches in circumference. And the cores 11 inches in a straight line.

Mr. Klein does not determine to what kind of bulls this fossil is to be ascribed. He only conjectures it may belong to the taurolephantes, mentioned in the Dissertation of a Pair of very extraordinary large Horns, published in vol. 34 of Phil. Trans. N° 397. But as to the Zubrones which Gesner on the Urus, p. 144, mentions from Munster, there is no sufficient proof that the animal in question was of that kind.

A further Account of a Remarkable Plica Polonica; also a prodigious Swelling of the Eye. By the same. N° 426, p. 428.

This surprising plica polonica, (see Philos. Trans. N° 417) was sent to Dresden, where Mr. K. saw it. It is remarkable, that the woman affected with this plica, who lived in the district of Novogrod, during the 52 years that she laboured under it, never changed her resting place but twice a year, viz. in spring and winter. On the approach of winter she could endure cold so very well, that she shunned all sort of heat, even that of a lighted candle. She never used any strong liquor, but lived on very bad bread, raw herbs, and water, to 70 years of age. In the spring she used to be carried to some place where the heat could not easily penetrate.

The other case is of a prodigious swelling in the eye of a man. It was occasioned by hail; and it daily increases and grows hard. This circumstance is very singular, that the optic nerve and the tunicles have stretched so much, that the eye quitted its socket, and fell down to the beard. The unhappy man could move this eye, which weeps, but cannot see with it. The tumour is not painful, but it is very troublesome to him about his nose.

An Abstract, by James Douglas, M. D. Med. Regin. et F. R. S. of a Book, entitled, A short Account of Mortifications, and of the surprising Effect of the Bark, in putting a stop to their Progress, &c. By John Douglas, F. R. S. 8vo. Lond. 1732. N° 426, p. 429.

This account of mortifications is divided into three parts; in the first of which the author treats of mortifications in general; where he has collected the opinions of the most experienced physicians and surgeons, who all affirm that a mortification from an internal cause is always incurable; and when it proceeds from an external one, it can never be cured but by amputation, or separating the part affected from the sound.

In the 2d part he gives a very remarkable observation of his own, which proves to a demonstration, that a gangrene, even from an ill habit of body, may be cured, contrary to the hitherto received opinion.

In the 3d part, he makes some remarks on the present case, and adds some parallel observations from Mr. Rushworth, a surgeon in Northampton, who had the good fortune of making the first discovery of the great and surprising effects of the Peruvian bark in checking the progress of mortifications, which, he says, has been likewise confirmed by the repeated observations of that excellent surgeon Mr. Serjeant Amyand, who had often used the same medicine, in the same case, and with the same success as Mr. Rushworth.

Our author says further in this place, that it is only by taking off the fever that the bark produces all these good effects. But to return to the observation itself, which he has given with a great deal of judgment and accuracy. He says, that April 22, 1732, he was sent for about 15 miles out of town, to visit a gentleman near 50 years of age.

On examination he found the back of his right foot mortified, near the middle toes, about the breadth of a shilling, his pulse quick, and his tongue dry. There being no sign of any external hurt, bruise, or wound, his physician, apothecary, and himself, were all of opinion, that it must proceed from some internal cause residing in the mass of blood.

The necessary dressings being prepared, he scarified the mortified part, and cut to the very bones without being felt by the patient, having afterward carried his incisions through the skin as high up as the knee, before ever he began to complain of the least pain. His limb was at the same time stuped with a proper warm fomentation, and the wounds dressed up, as usual, with pledgets dipped in hot oil of turpentine, and over all a poultice, or cataplasm, was laid on of theriac. Londin. oatmeal and stale beer. The physician prescribed what alexipharmics he judged most proper on the occasion.

April 23, Serjeant Dickins and Mr. Cheselden being called in, they directed the same external applications to be continued as before.

April 24, the mortification did not seem to spread.

April 25, his fever was high, his tongue very dry, and the mortification began to spread a little. He then scarified again and deeper.

April 26, the mortification seemed to be at a stand.

April 27, the mortification spread across the toes towards the ball of the foot, which he scarified deeper, and dressed as before. The fever grew higher.

April 28, he was forced to use the actual cautery, the mortification getting ground in spite of all he had done.

April 29, he found no benefit from the cautery, though applied wherever the part was corrupted.

April 30, the two surgeons that had been consulted before, the physician, the apothecary, and the author, were all of opinion, that even the taking off of the limb could not save him, but that in all probability he must die in 24 hours, his symptoms being worse than ever; that is, his fever was very high, his tongue dry enough to grate a nutmeg, his visage wild, he had a great drought, was very restless, the mortification spread as far as the tendo Achillis, and the patient complained of a pain and hardness in the side of his belly.

In this deplorable condition the Jesuit's bark was proposed by Serjeant Dickins, and agreed to by the other surgeons present, and $\frac{1}{2}$ dr. was ordered to be given that evening, and repeated every 4 hours.

May 1, this morning he found a very surprising alteration for the better, with regard to the fever, and the other symptoms complained of the day before; the patient had a good night's rest, and the mortification had made no further progress.

May 2, there was a small discharge from the sore.

May 3, he found two large abscesses on each side of the ancle. The violence of the fever being taken off by the use of the bark, nature was enabled to form these abscesses, and from that he concluded, that the progress of the mortification was effectually stopped. He observed, that on giving the bark but once in 6 hours, a small return of the fever, with a worse digestion, which obliged him to give it every 4 hours as before, and continued in that dose for 28 days in all; and then every 6 hours for 5 or 6 days longer, though the fever had quite left him all that time.

The whole quantity of the bark given to this gentleman amounted to 10 oz.

May 5, his pulse was regular, and the digestion plentiful and laudable. The muscles and tendons on the sole of the foot being all mortified, before the bark was given, separated in process of time, and fell off very kindly, leaving the bones of the toes, metatarsus and tarsus bare and carious, which he afterwards cut off one after another, as he found occasion, and could be done with safety. About the middle of November following, the ends of the tibia and fibula were almost covered with a firm cicatrix; the patient at this time was well in all other respects, and was able to walk about by the help of a wooden leg, and from that time has continued in perfect health.

On the Use of the Peruvian Bark in Mortifications. By John Shipton, Surgeon in London. N° 426, p. 434. *An Abstract from the Latin.*

In the first part of this paper Mr. Shipton gives an account of the successful exhibition of the Peruvian bark in a case of mortification of the foot, from an internal cause, by Mr. Rushworth, surgeon at Northampton, in the month of October, 1731. Mr. Rushworth sent an account of the success which had attended this mode of treatment, to the corporation of surgeons in London; and in 1732, Mr. R. was informed by Mr. Amyand that, after his example, he had employed the Peruvian bark in 7 cases of mortification, with similar good effect. Mr. Rushworth was also informed by Mr. Douglas, in a letter dated July 5, 1732, that he, in consultation with Mr. Dickins and Mr. Cheselden, had succeeded with the same remedy in a mortification of the foot from an internal cause, occurring in a man 50 years of age. These facts were published by Mr. Rushworth in a small tract expressly written on this subject.

After this notice of Mr. Rushworth's publication, Mr. Shipton proceeds to give an account of 2 cases of mortification which he attended. In one of these from an internal cause, the Peruvian drug was of no avail; but in the other, in which the mortification was the consequence of a lacerated wound, occasioned by a fowling piece going off while the person was drawing out the charge, it effected a cure. In these cases Mr. S. gave the bark in powder, viz. 2 scr. every 4 hours at first, and afterwards at longer intervals. He remarks, that where the powder in such doses should be nauseated by the patient, the resin or extract administered in half the quantity might prove equally efficacious. He further adds, that from what he had experienced respecting the medicinal powers of the bark in cases of mortification, he was led to expect that it would prove extremely beneficial in certain ill-conditioned ulcers termed nomæ and phagedænæ, and perhaps also in malignant kinds of herpes.*

Some Corrections and Amendments to the Natural History of the Insect called Coccus Radicum. By J. P. Breyne, M. D. F. R. S. N° 426, p. 444.

In Dr. Breyne's Natural History of the Coccus Radicum, when after many repeated observations and experiments, he had given an account of the generation and metamorphosis of that insect, which uses to stick to the extremities of the roots like a spherical grain, and is commonly called coccus polonicus, he conjectured, that those small flies which are often found among the coccus, did not belong to the coccus, but owed their rise to small worms of their own kind, and were accidentally found among the coccus; and as he could not find any difference of sex among the worms of the coccus, and following chiefly the opinion of Signor Cestoni concerning the coccus of the ilex, he ventured to

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* Mr. Rushworth's pamphlet above alluded to was published in 1731, under the title of a Proposal for the Improvement of Surgery; from which it appears that the merit of first applying the Peruvian bark to the cure of mortifications is due to a British practitioner. Many years afterwards it was shown by Mr. Pott, that the cinchona is rendered much more efficacious in such cases by combining it with opium.

assert, that our coccus also is an insect of the hermaphrodite kind, which brings forth eggs of itself, and from itself, and propagates its species without being impregnated by a male. But the summer following he began to be sensible that this opinion was erroneous, and about the end of it was quite convinced of being in the wrong.

Having repeated his observations with the greatest exactness, and examined them in the strictest manner, at last he found that the metamorphosis, or evolution, through which our coccus passes, is as follows:

A. of the male.	B. of the female.
I. The egg.	I. The egg.
The eggs are laid about the end of July, or the beginning of August.	
II. A worm with 6 feet, no wings.	II. A worm with 6 feet, no wings.
The worms come out of the eggs about the middle of August, till the beginning of September.	
III. The less spherical grain; that is, the coccus, strictly so called, of the size of a grain of poppy-seed, or millet at the most, gathered from the 9th of June till the summer solstice, with other larger cocci.	III. The larger spherical grain; or the coccus of the size of a vetch, or as large as that of white pepper, which is gathered from the middle of June till about the middle of July.
IV. The less worm with 6 feet, no wings. It comes out of the abovementioned coccus, from the summer solstice till the middle of July.	IV. The larger worm with 6 feet, no wings. That is to say, the female coming out in the beginning of July, but chiefly about the middle of the said month; which being impregnated by the fly the male N° VI, brings forth the egg N° I.
V. The nymph which appears about the beginning of July and the following days.	
VI. The fly, the male coming out from the middle of July till the 24th of the same month, which impregnates the worm, the female, marked N° IV.	

This insect, under whatever shape it appears, viz. either of a grain, a male worm, a nymph, a fly, a female worm, or a worm coming out of an egg, always when pressed and crushed, affords a matter of a purple colour, which however is observed to run most copious in the cocci and the worms, especially the female ones.

A Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands, by Mark Catesby, F. R. S. with some Extracts out of the fifth Set. By Dr. Mortimer, R. S. Secret. N° 426, p. 447.

A general account of this splendid work has been inserted at p. 433 of the present vol. of these Abridgments.

END OF VOLUME THIRTY-SEVENTH OF THE ORIGINAL.

A Catalogue of the fifty Plants from Chelsea-Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1731; *pursuant to the Direction of Sir Hans Sloane, Bart. P. R. S. By Isaac Rand, F. R. S.* N° 427, p. 1. *Vol. XXXVIII.*

This is the 10th catalogue of this kind that has been presented, making 500 plants.

Explanation of an Essay on the Use of the Bile in the Animal Economy, by Alexander Stuart, M. D. F. R. S. N° 427, p. 5.

In a former short essay on the use of the bile in the animal economy, (Phil. Trans. N° 414) some points, which required further illustration, having been only hinted at, it is considered of use, to set these points in a clearer light, by solving such difficulties, and answering such remarks, as occurred in conversation and correspondence on that subject.

The first remark is, that no notice is taken of the effect of the gall spilt on the external coat of the intestines from the wound in the gall-bladder, whose stimulus on the outside is supposed sufficient to have produced, and to have solved all the phænomena, or symptoms observed and related in the case; so that all the symptoms which the Doctor attributed to a want of the stimulus of the gall on the inside of the intestines, might have been more properly ascribed to the same stimulus, acting on the outside of the uppermost guts, situated nearest to the gall-bladder, whose complete contraction by the force of that stimulus, expelling the air out of their cavity, and forcing it into the inferior guts, as in windy colics, would have distended them to the pitch mentioned in that essay.

Dr. Stuart acknowledges that there is some appearance of reason for this remark, and the objection which it implies; but the whole strength of the argument lies in a supposition that a stimulus on the outside of the intestines is capable of exciting a contraction, supplying the want of that stimulus on the inside, and also of causing a preternatural distension of the whole canal. The contrary of all which the Doctor endeavours to prove.

In order to this it is premised, 1. That the whole action of the nerves, whether in sensation or in muscular motion, is exerted at their extremities only. 2. That the sides of the nerves every where along their whole tracts, are entirely insensible, and serve neither for sensation nor motion.

The apparatus of nature towards both these actions makes this plain. Towards sensation we see, that the medullary substance of the nerves at their extremities is divested of its coverings, which are processes of the dura and pia mater, and ends bare in the form of small soft papillæ, from their figure called by anatomists pyramidales, on the surface of the cutis, covered over with the cuticula, where they act their part in sensation, or feeling, tasting, and smelling. The soft denudated branches of the optic nerve which compose the retina, and what for the same reason is called the portio mollis of the auditory nerve, the immediate instruments of seeing and hearing prove the same.

Again, it is the extremities of the nerves that enter with their coverings into the muscle, and into each fibre of the muscle to which they belong; where they deposit their contents, or act their part in muscular motion.

But the sides of the nerves along their whole tracts are insensible, or void of feeling; because their medullary substance, and its contents, which are the only immediate instruments of sensation in them, are here covered with the pia and dura mater, the last of which is the strongest, densest, and most impenetrable membrane of the whole body, capable of defending and conveying the tender medullary substance of the nerves and its contents, safe, unhurt, and undissipated to the several organs of sensation and motion, at their extremities, the seats of their action.

A further confirmation of this from experience, is the insensibility of the side of a large visible branch of a nerve, which sometimes happens to lie bare and exposed in a wound or ulcer, where it will bear the touch of the probe without feeling, and occasions no more pain than in wounds and ulcers of the same kind, where the nerves are not exposed, unless the investing membranes, the dura and pia mater, be by any accident wounded, lacerated, or corroded; in which case, the medullary substance being laid bare, exquisite pain is felt, and very severe symptoms ensue, which are hardly to be overcome, or never so easily as by cutting the nerve quite through, so as that the extremity may retire within the flesh, and the medullary substance be protected by it. By which it appears, that the sides of the nerves are insensible or void of feeling, and that the extremity of the medullary substance, either by nature, or by some accident, laid bare, is the only immediate instrument of sensation.

This being premised, the structure of the intestines, the parts in question in the case before us, comes to be considered.

The intestines are made up of 4 tunics, or coats. The first, or external coat, is a common membranous covering, borrowed of the peritoneum. The second is composed of their annular, contractile, muscular fibres, the immediate instruments of their peristaltic motion. The third is the nervous coat, a reticular plexus of nerves, intermixed with blood-vessels and glands, placed immediately under the muscular, and over the villous coat. The fourth is the villous or innermost coat, on the concave side, rightly called villous, as it appears viewed through a microscope; though from its appearance to the naked eye, it be erroneously called the mucous coat. This is generally allowed to consist of the capillary extremities, or rather roots of the lacteals, and the excretory ducts of the glands, which together form these villi that are seen in it. Among these, suitable to analogy in all other parts of the body, the papillæ pyramidales, or extremities of the nerves, are lodged under the cuticula of the nervous coat, for the uses of sensation, so necessary for the purposes of nature, in this very sensible part, the inside of the guts, which is known to be so quickly and necessarily affected by the qualities of their contents.

The proper nerves of the first or outer coat, are those of the peritoneum, of which it is a part, arising from the medulla spinalis of the loins and os sacrum. Whereas the nerves proper to the guts, are of the par vagum, and mensenteric plexus; therefore, as there is no communication of nerves between this external coat or covering, and the proper substance of the intestines themselves, a stimulus acting on this external coat only, would not affect the guts, so as to excite in them any considerable degree, either of sensation or motion.

Again, the proper nerves of the intestines, whose origin, disposition, and situation have been already described, terminate either in the muscular contractile fibres of the coat immediately above them, or carry their extremities to the inside, where they terminate under the cuticula, for the use of sensation; so that a stimulus on the outside of the intestines, besides the difficulty of passing through the two external coats, before it could reach the proper nerves of the guts, would at last only irritate their sides, where they are insensible, because covered with the dura mater: and if it might be supposed, that such a stimulus as is in question, viz. the gall, could have penetrated through these coats into the cavity, where the sensible extremities of the proper nerves of the guts lie exposed to it, yet such a filtration through all these coats, as it could not be performed soon, nor in great quantity, so it would enter at last, divested in a great measure of its grosser, saline, oleaginous, and pungent parts, by the filtration, and so lose the power of a stimulus on the inside; as the situation of the parts, and disposition of the nerves above described, made it an ineffectual one on the outside, as much as if it had been carried quite out of the body.

To conclude, if the gall spilt on the outside of the guts, had been capable of exciting a contraction in any part of them, as soon as it came to cover the whole surface, it must have had the same effect equally every where, and the whole canal should have been found contracted to its smallest diameter: whereas it was found every where distended to a great pitch.

It is therefore plain, that a stimulus on the outside of the intestines, has not the effect of such a stimulus on the inside. It can neither excite them to a contraction; nor promote their peristaltic motion; nor supply the defect or want of such a stimulus on the inside; much less occasion such a universal distention, or account for the symptoms arising from it, which is what the Doctor undertook to prove.

The 2d difficulty is, how a fresh recruit of chyle should be a cause of sleep.

The experiments mentioned in Transact. N° 424, the Doctor considers may serve to justify what he here assumes, concerning the nature and existence of the nervous fluid, or animal spirits, in the solution of this second difficulty. The argument which has been offered, runs thus: it is well known that people after eating plentifully, are often inclined to sleep, long before the chyle can be supposed to be got into the blood; therefore a fresh recruit of chyle cannot be the cause of sleep; but there must be some other cause, at least at that time. Which cause is assigned by supposing, that after a plentiful meal, the distended stomach will load and oppress the descending aorta, so as to hinder the blood in its descent, and so force a greater quantity than usual into the aorta ascendens, which, by its distended branches in the brain, will obstruct the secretion of the animal spirits through the glands of the cortical substance into the origin of the nerves, and thus produce sleep.

This being generally esteemed a mechanical account of the cause of sleep after meals, deserves the greater attention. In answer to which, if such was the true cause of sleep after meals, it ought to have the same effect on the cerebellum, from whence most of the nerves, that serve in the natural and vital functions, arise; and so would hinder these functions, viz. digestion, the peristaltic motion, respiration, and the circulation of the blood, all which, on the contrary, are observed to be more regular and stronger in sleep, than when we are awake; at least in a healthy and temperate person, who has used moderate exercise.

Again, gluttony, drunkenness, and flatuses, which overload the stomach, and therefore, according to this hypothesis, ought to produce the quietest and most serene repose in sleep, do, on the contrary, bring inquietude, or broken and interrupted rest; and when to the greatest excess, a lethargic sleep, which is a disease for the time, and sometimes terminates in death.

The incubus also, which is justly supposed to arise from an inflation or distention of the stomach, in a supine posture in bed, oppressing the aorta descendens, ought to produce quiet rest; whereas nothing disturbs more, as it first brings the person out of quiet sleep into a sort of waking dream, with a sense of oppression, and at last awakes him quite, in a kind of terror, with palpitation of the heart.

And indeed as nothing contributes more to sound and quiet rest, than an easy digestion and respiration, a sedate, equal, and regular circulation of the blood; that is, an uninterrupted function of all the natural and vital parts: the reverse of these, and particularly an interrupted or difficult circulation, if to any considerable pitch, must produce the contrary effects, viz. restlessness or inquietude of some kind or degree; as in fever and other distempers attended with such irregularities of the animal economy.

The difficulty which is suggested about the chyle's not getting soon enough into the blood, by the way of the lacteals, to produce this effect in such as sleep immediately after a plentiful meal, vanishes when we consider, that this very rarely happens, at least never attends temperate people, in perfect health, and in a temperate climate; but such as are gross feeders, drunkards, corpulent, short-necked, by constitution or make liable to apoplexy or palsy, or have formerly suffered by such distempers, or live in a hot country.

In gross feeders, drunkards, and such as are corpulent, from these causes the lacteals are never quite empty; in such, the food of the present meal, by exciting the peristaltic motion, will, in a few minutes, press forward the chyle of the preceding meal into the blood. In full vessels or tubes, the reception and discharge will be instantaneous, or nearly such; because supposing the apertures to be free or unobstructed, as much precisely will issue at one extremity of a full vessel or tube, as is forced into it at the opposite extremity; and that instantaneously, because of the contiguity of the globules, or particles of the fluid it contains.

In short-necked people, the passage between the heart and the brain being proportionally short, the force or momentum of the circulation in the brain, is by so much the greater; but a strong and swift circulation is an enemy to all secretions, as is evident in fevers, and mechanically demonstrable; for all the secretions being by lateral branches going off at or nearly right angles, which is very remarkable in the brain, a swift circulation or motion along, or parallel to the axis, carries along with it what should be laterally secerned. Hence a paucity of animal spirits in short-necked people, who by this make are liable to apoplexies, palsies, comas, lethargies, listlessness, inactivity, and drowsiness, especially after meals, when the fresh chyle has got admission, to absorb a part of the already few remaining spirits, which must be recruited in sleep.

Again, in hot climates, a continual waste or dissipation of the spirits by heat, makes the inhabitants generally lazy and inactive: in such the recent chyle, the grossest circulating fluid of the whole body, will quickly absorb the few remaining spirits, and dispose them to sleep after every meal: except when the cool of the evening checks perspiration, and the evaporation of those spirits which were recruited by sleep in the day-time, and therefore remain plentiful enough to support their activity after supper, when the business of the meaner, and diversions of the richer sort, begin; which, in colder climates, is the case after breakfast and dinner.

For a further confirmation of this, brandy, and the spirits of fermented liquors, are known to produce a drowsy stupidity in such as drink them to any pitch, and an habitual dulness in habitual drinkers of them; and, when drunk to excess, throw the drunken into a kind of lethargic sleep for some time. Yet the quantity taken down, sufficient to produce these effects, is never so much as to load or distend the stomach, so as to oppress the aorta descendens, or to hinder the circulation downwards; and therefore cannot be supposed to produce sleep or sleepiness in that manner, but in a different way, which shall be described in the sequel of this discourse.

Thus this position, concerning what has been generally esteemed a mechanical cause of sleep after meals, being sufficiently refuted, it remains to endeavour to establish such a general cause of sleep, as may be conformable to what is advanced in the essay under consideration.

And here it will hardly be denied, that the cause of sleep in general, is a want of a sufficient quantity of animal spirits for the use and exercise of the animal functions: therefore whatever prevents their recruit, hinders or impedes their secretion, and absorbs or fetters them when produced; and whatever exhausts or evaporates them, by occasioning a paucity of spirits, will, in a healthy person, produce a listlessness, laziness, a tendency to sleep, or sleep itself, in proportion to that paucity of the remaining spirits.

If we enumerate all the known remote causes of sleep or sleepiness, we shall find that in some one or other of the ways abovementioned, they all tend to produce this immediate or proximate cause, viz. an impairment of the nervous fluid, or animal spirits, and so bring on these several dispositions to sleep, or sleep itself.

All the remote causes of sleep, or sleepiness, may be fully comprehended in the 4 following particulars, and considered in the following order. 1. Exercise. 2. A too plentiful meal. 3. Drunkenness, or a too great quantity of fermented liquors, or of their distilled spirits. 4. The whole tribe of narcotics, or soporifics, of which opium, and its several preparations are the chief.

1. Exercise appears to waste all the fluids, and particularly the animal spirits, the active instruments of all motion; so that the remains are not sufficient for the exigencies of the natural and vital functions; and also to supply the demands of voluntary motion, and to assist in sensation, and the operations of the mind. And here it is proper to show how this waste necessarily brings on sleep in a healthy person; and how the natural and vital motions and functions, of digestion, respiration, and circulation, notwithstanding this waste, do necessarily go on in sleep, leading the remains of the spirits to their assistance, and making the deficiency fall to the share of the animal or voluntary motions and organs of sensation.

In order to show this, let us observe what is very obvious, that when any muscle is brought into action against our will, by a superior force, as when a stronger man bends or extends my arm contrary to my will or inclination, the benders or extensors of my arm swell and contract in the same manner, and the afflux of the blood and spirits to the contracting muscles, is the same, as when I do it voluntarily: therefore by any external or adventitious force, the blood and spirits will be derived on the part thus forced into action. But all the natural and vital parts have such an external or adventitious force continually acting upon them. In the primæ viæ the weight and other qualities of our food and drink, mixed with air and bile, excite the peristaltic motion, as necessarily as the weight of a clock, or spring of a watch wound up, keeps the wheels and pendulum, &c. in motion. The chyle forced from thence, together with the blood returning into the heart, as necessarily set its elastic springs at work, and the same blood and chyle forced into the arteries by it, make their diastole and following systole unavoidable. The air by its elasticity, and the whole weight of the atmosphere, forces itself into the elastic pipes and vesicles of the lungs, and dilates them; which by their elasticity and mechanism, assisted by various muscles, and the ribs and cartilages of the thorax, as necessarily repel it in expiration.

It is therefore evident, that all these natural and vital parts are acted on, and set at work by an external adventitious and irresistible force, continually exciting them whether we will or not, whether awake or asleep; therefore the blood and remaining spirits after labour, will be mechanically and necessarily led to all these parts that are thus forced into action at all times, but especially most regularly and copiously in sleep, when all external objects cease to solicit our senses, and the will no longer determines the spirits into the muscles of voluntary motion; which two kinds of actions, as well as the operations and passions of our mind, do, in the day-time, make strong derivations of the spirits from the natural and vital functions; which, for that reason, are never so perfect as in sound and undisturbed sleep.

Those who are acquainted with the doctrine of derivations and revulsions, founded on innumerable observations in the animal economy and practice of physic, know, that a flux of any of the animal fluids, arising from nature, or from a disease, or provoked by art, to any one or more parts of the body, or to any organ of secretion or excretion, will cause a sensible proportional diminution of the afflux to, and of the secretion and excretion, by the other parts and organs.

Therefore, as soon as a deficiency of animal spirits happens by labour, or from any other cause whatever, that defect will be first felt in the organs of sensation, the muscles of voluntary motion, and the operations of the mind; because these are not acted on by such powerful and irresistible agents, as the organs of the natural and vital functions are, in perfect health; for the mind, being sensible of the defect of spirits for its actions and operations, chuses to forbear; we retire from external objects, and then the whole of the remaining spirits are led to the natural and vital organs, by the mechanism above described; and the organs of sensation and voluntary motion must be entirely deserted by them, for that time: which is the state of sleep, and which will continue till a greater quantity of spirits be recruited, than is consumed in the natural and vital functions; at which time the redundancy or overplus begins again to be secerned into the other deserted nerves, viz. into those of sensation and voluntary motion; which, flowing now copiously into the relaxed muscles, excites stretching, yawning, &c. and at last rouses out of sleep.

2. A too plentiful meal is known to cause a heaviness, inactivity, listlessness, an aversion to motion or action, a drowsiness, sleepiness, and in some sleep itself, soon after eating.

It has been proved above, that this cannot proceed from a distention of the stomach; it has also been endeavoured to prove, that in such, the lacteals are never empty, and that the chyle of the preceding meal is forced through them into the blood by the succeeding, almost instantaneously, or as soon as the peristaltic motion is excited or increased by the food taken down, which must be during the time of such a meal, or very soon after, according to the degree of fulness of the lacteals before that meal. What change then can we imagine to have happened to the body, in this time of a meal, so remarkable, and so likely to affect the economy, as that of the admission of a fluid into the blood, much grosser and less fluid than itself? Such a mixture must render the whole mass grosser, or of a thicker consistence, than before, as it quickly mixes with the finer, and absorbs its most fluid parts; but it will hardly be denied, that if there is such a fluid as animal spirits, they must be the finest and most depurated fluid of the blood: these therefore will be absorbed, and mixed with this grosser crude fluid the chyle, and therefore will be diminished by it; and being thus entangled, will be more difficultly secreted, and in less quantity: hence that paucity of spirits, which will dispose to sleep in the manner above described, in speaking of a paucity of spirits after labour or exercise.

3. How far strong fermented vegetable juices or liquors, and their distilled spirits, drunk to any pitch of excess, bring on sleep, or some degrees of it, has already been said.

The distilled spirits of fermented liquors, are known to lessen all the secretions and excretions, and therefore are of use in diarrhœas, in excessive and colliquative sweatings; and I have known French brandy, taken incautiously, to have put a stop to a sweat procured by sudorifics. In habitual drinkers of them, they gradually lessen the secretion of the bile, and insensible perspiration, and thereby bring them at last into the jaundice and dropsy.

Spirituous liquors, and particularly French brandy in the most remarkable manner, being mixed with the blood, as it flows from a vein into a porringer, unites the serous with the globular red part of the blood, to such a degree, as that no serum separates from it in many hours, and in some not at all; an experiment which may be easily made; which shows in what manner it prevents the secretions in the body, these being all of the serous kind: hence that great impurity of the blood arising from a restraint of the secretions in such people; and also that paucity of spirits, the general cause of sleep and dulness, very different from the alacrity and vivacity of the temperate, and even of water-drinkers.

That therefore which fetters or binds up all the serosities, or most fluid parts of the blood, and proves a strong copula between them and its red globules, may be reasonably supposed to fetter or tye up the finest fluid of all, viz. the animal spirits, with the rest, and in the same manner to hinder their secretion, and thereby produce sleep, or some such degree of it as abovementioned.

4. As to opium, and all the class of soporifics, if we compare their visible effects with what has been said above of brandy, or spirits of fermented liquors, we shall find them much the same. Opium is known to lessen or suppress all the secretions and excretions, and is therefore of such remarkable use in fluxes,

rheums, catarrhs, &c. It has indeed been conceived to be a sudorific, but that only in composition with aromatics, as in Venice or London treacle; or with saline bodies, as the sapo tartareus in the Pil. Matthæi or Starkij; and that too assisted by plentiful dilution with warm sack-whey, or such like liquors, and the addition of volatile spirits of hart's-horn, &c. which are known to thin the blood, as evinced by Mr. Leuwenhoeck's microscopical observations, and the mixing of these volatile saline spirits with blood, as it runs out of the vein into a porringer. Which shows, that these volatile salts are good correctors of opium, as they break down and colliquate the blood, and therefore tend to promote the serous secretions, which opium by itself, and all distilled spirits of fermented liquors, retain, or restrain for some time, incorporating the serosities with the red globules of the blood, as before observed.

In hot countries, where large doses of opium are taken, the effects are nearly the same with what we observe in drinkers of distilled spirits of fermented liquors, viz. a small dose exhilarates, a greater brings on some degree of drunkenness, or temporary madness; this increased will lay to sleep, and a very great dose will kill.

In this comparison therefore, may we not justly conclude a parity in the causes, from the similitude of the effects; though all the secondary qualities of such causes, which offer themselves outwardly to our senses, be apparently very different: thus, gunpowder is as much a latent fire as brandy, and will exert itself in that shape to a far greater degree than it, in equal circumstances, that is, by the least contact of fire; therefore, though brandy and opium show no outward resemblance to our senses, in smell, taste, colour, consistence, and such like secondary qualities, no more than brandy and gunpowder; yet if in proper and equal circumstances, that is, in contact and mixture with the blood, they produce the same, or nearly the same effects, we may justly conclude, that there is a latent similitude of primary qualities in their natures, which they evince in proper and equal circumstances, in producing the same or parallel effects.

But it has been shown above how, and in what manner, brandy fetters and entangles the animal spirits, and other fluids of the blood, uniting them too intimately with the grosser parts, and thereby obstructing their due secretion for some time; whence a paucity of spirits, which discovers itself by an inequality and irregularity of their distribution in drunkenness; a still greater defect in dulness and drowsiness; yet more in sleep, and a total suppression of their secretion, as well to the natural and vital as to the animal organs, which is death, the effect of the greatest doses either of such distilled spirits or of opium.

From what has been said on this subject, it seems as plain as the nature of such a physical demonstration will admit of; 1. That the universal cause of sleep, is a paucity of animal spirit. 2. That this defect will arise from whatever exhausts, wastes or evaporates them when produced; as labour or exercise; or from whatever absorbs them, as a great quantity of crude chyle, recently and suddenly admitted into the blood, in the time of, or soon after, a plentiful meal; or whatever can fetter or reunite them with the grosser parts of the blood, as much as brandy or spirituous fermented liquors and opiates. All these, either by evaporating and wasting them, or by hindering their production or secretion, bring on that paucity of spirits spoken of, and sleep or some degree of sleepiness, as a necessary consequence.

Yet it will be still true on the same foot of reasoning, that where the blood is extremely depurated, and the secretions and excretions from it already perfectly performed, as in long fasting, the whole mass of blood is become only fit for the secretion of spirits; has no crudity or impurity in it, to absorb or fetter the spirits already produced; and no crude chyle admitted to answer that end: in such a case opiates can have no effect, the spirits cannot be absorbed, fettered or restrained, where the qualities of the mass of blood do not concur to that effect.

Another concurring cause of the inefficacy of opiates in the case of fasting, is, that all the natural parts, as those of the primæ viæ, which serve for digestion, are at rest, for want of the weight and stimulus of food, and also of the gall in the case referred to, to keep up their peristaltic motion; therefore, few or none of the spirits being spent on those parts, there is a greater supply sent to the animal organs of sensation and voluntary motion; and indeed in such a case even the vital parts for respiration and circulation act but very sluggishly, for want of a recruit of blood and fluids proper to excite their functions: hence also the supply of spirits to the organs of sensation and voluntary motion, is by so much the greater; and the possibility of restraining their secretion, for the reasons above assigned, impracticable by any power of opium, without the accession of a fresh recruit of chyle.

Hence also those who have any considerable defect in the natural and vital functions, or in either of them, by obstructions of the viscera, are generally bad sleepers, or watchful; and in such opiates have but little effect to procure rest; with this great disadvantage, that by impeding the secretions, they increase the obstructions; though in many cases, where the viscera are sound, they must be acknowledged to be excellent medicines.

What has been said will also sufficiently account for the anodyne power of opium; for if it impedes the secretion of the animal spirits, the immediate active instruments of all sensation, it must certainly obtund or abolish for that time the disagreeable sensation of pain.

The third difficulty is, how pus should be the product of chyle, and not of the blood or serum. As to which, Dr. S. thinks it would not be difficult to prove that all the gross secretions are from the chyle; these being only the depurations of it in sanguification, or in order to bring that crude and gross fluid the chyle into pure and defecated blood, from which no secretion can afterwards be made, but of that purest fluid, which it secretes into the nerves for the use of the whole economy.

If this be true, then pus in a wound, ulcer, or impostume, being a very gross feculent humour, is likelier to issue from the chyle, than from the purer and more defecated part of the mass.

A Catalogue of Eclipses of Jupiter's Satellites for the Year 1734. *By James Hodgson, F. R. S. Master of the Royal Mathematical School at Christ's Hospital, London.* N° 427, p. 26.

Calculated for the use of persons intending to observe them that year.

*Of the Flying Squirrel, or Mus Ponticus or Scythicus of Gesner; and of the Vespertilio admirabilis Bontii. By M. Klein.** N° 427, p. 32. *Abridged from the Latin.*

There is a peculiar kind of flying lizard, under the name of lacertus volans or dracunculus alatus, very common in Java. Belonius represents it as a biped; but this is deservedly contradicted by Piso and others; and indeed the dracunculi preserved in several museums, abundantly confirm their being quadrupeds. Quadrupeds are properly called flying, which really fly, that is, roam about freely in the air; but those are improperly said to fly which live generally in trees, as the common squirrels, and other animals of that kind, martens, &c.

Among these, the principal is the flying squirrel, so called, as it is provided with a kind of sail, or peculiar flying instrument. M. Klein finds one of them in Levinus Vincentius's Catal. and Descript. Animal. 1726, under the name of sciurus virginiensis volans, without any further description of it. He finds

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* Of the animals mentioned in this paper by Klein, the first is the *sciurus volans* of Linnæus; the *European flying squirrel* of Pennant, &c. It is a native of the northern parts of Europe and Asia. The other animal, the vespertilio admirabilis of Bontius, is the *lemur volans* of Linnæus; the *flying macauco* of Pennant, &c. and the *galeopithecus* of Pallas. It is a native of Ceylon, Java, and other East-Indian islands.

another in Grew's museum of the Royal Society, under the name of flying squirrel, which Dr. Grew takes to be the animal which Scaliger, in Exercit. 217, § 9, describes under the name of felis volans. Lawson, in his history of Carolina, exhibits a third. And Gesner de quadruped. p. 743, a fourth, which he calls mus ponticus or scythicus, or sciurus volans and alatus.

Fig. 2, 3, 4, pl. 15, represent the one M. Klein had. It is less than the common squirrel, but larger than the field mouse; its skin is very soft, elegantly adorned with grey and dark grey pile; it has large, prominent, black, and very beautiful eyes; small ears and very sharp teeth; most of them are mischievous; but M. Klein's was not much so; it would not catch at the finger though put to its mouth: but it could not be trusted if provoked. When it does not leap, its tail lies close to its back; but when it does, it hangs it down, moving from side to side. It eats bread, baked without salt; but the fresh tops of birch are its favourite food; it cares not for nuts or almonds; it makes its bed in an elegant manner of the moss of the birch, and with surprising facility drawing it with its feet, lies buried in it, and does not stir from thence in the day-time unless disturbed, or pressed with thirst. The skin of its flying instrument may be expanded from its sides like a sail for nearly the breadth of a palm; it adheres to the bending of the hinder feet, but is connected to a bony articulation with the fore feet; at the extremity of this articulation the skin is downy. When it sits quiet, or moves with its natural pace, this articulation, which is parallel with its feet, cannot be distinguished; but as soon as it leaps, it is moved, and forms a right angle with the fore foot; whence the skin is expanded, though likewise a strong panniculus carnosus, that passes under the whole skin, much assists its leaping. From this M. Klein gathers, that this little animal does not properly fly, but that it can leap to places at some distance, with greater ease than other animals of the same kind, and by means of its sails continue longer in the air. With this flying squirrel compare the vespertilio admirabilis Bontii in Hist. Nat. et Med. Ind. Orient. cap. 16, Apud Pison. p. 68. Piso himself would doubt, whether it is to be classed among the family of bats.

On considering the size and form of the vespertilio admirabilis M. Klein could not satisfy himself of the truth of what Bontius asserts, namely, that they fly in flocks, like wild geese; but he rather thinks, that such animals come nearer to the nature of flying squirrels, and that they use their sails in the same manner; notwithstanding what Bontius asserts, that about the evening they are observed pendulous in the air, or from trees; but that rather it may hence be proved, that these vespertiliones, as well as the flying squirrels, sleep in the day-time, and about the evening quit their retreats, leap from one tree to another; and therefore, that when they leap they are observed to be pendulous in the air; but when they have done leaping, they are found to hang from trees. Besides, these vespertiliones admirabiles may be called feles volantes, with equal propriety, as Gesner called the sciuri here spoken of, volantes.

A Description of a Barometer, of which the Scale of Variation may be increased at Pleasure. By the Rev. John Rowning.[*] N° 427, p. 39.

ABCD, fig. 5, pl. 15, represents a cylindrical vessel, filled with a fluid to the height W, in which is immerged the barometer SV, consisting of the following parts: the principal of which is the glass tube TP, represented separately at *tp*, whose upper end T is hermetically sealed: this end does not appear to the eye, being received by the lower end of a tin pipe GH, which in its other end G receives a cylindrical rod or tube ST, either hollow or solid, and made of any materials whatever, fixing it to the tube TP. The rod ST may be taken off, to put in its stead a larger or smaller, as occasion requires. S is a star at the top of the rod ST, which serves as an index, pointing to the graduated scale LA, fixed to the cover of the vessel ABCD. MN is a large cylindrical tube of tin, represented separately at *mn*, which receives in its cavity the smaller part of the tube TP, and is well cemented to it at both ends, that none of the fluid can get in.

The tube TP, with this apparatus, being filled with mercury, and plunged into the basin V, which hangs by two or more wires on the lower end of the tube MN, must be so poised as to float in the liquor contained in the vessel ABCD, and then it will rise when the atmosphere becomes lighter, and è contrà.

Let the specific gravity of quicksilver be to that of water, or to the liquor the barometer floats in, as s to 1: and if it be proposed that the variations of this compound barometer shall be to the contemporary variations of the common barometer, in the given ratio of n to 1; this effect will be obtained by making the diameter of the rod ST, to the diameter of the cavity of the tube HI, as $\sqrt{\frac{n+s}{ns}}$ to 1; which may be thus demonstrated.

Let us suppose that the variation of the height of the quicksilver in the common barometer, called v, is such, that a cubic inch of quicksilver shall rise into the vacuum XT; in order to which, a cubic inch of quicksilver must rise

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[*] Mr. Rowning was an ingenious mechanist, mathematician, and philosopher. He published a very neat "Compendious System of Natural Philosophy," in 2 volumes, 8vo. 1744; also Description of a Machine for finding the Roots of Equations, in 4to. 1771. Mr. Rowning was rector of Anderby in Lincolnshire; and he died in 1771, at 72 years of age.

from the vessel V, that is, the surface P must subside so far, that a cubic inch of water, if that be the fluid made use of, shall enter the vessel V; by which means the barometer, with the parts annexed, will be heavier by a cubic inch of the fluid.

Now this additional weight of a cubic inch of fluid, will make the whole barometer subside, according to the laws of hydrostatics, till a cubic inch of the rod HS, immediately above the surface at W, shall come under it; but the length of such a magnitude of HS will exceed the length of an equal magnitude of quicksilver in the larger tube X, as much as the square of the diameter at X, exceeds the square of the diameter at H, the lengths of equal cylinders being reciprocal to their bases. That is, the perpendicular descent of the compound barometer will be to v, the perpendicular ascent of the mercury in the common barometer, as d to 1, supposing this the ratio of the bases; and consequently will be equal to dv.

But by this descent, the distance PW, between the surface of the stagnant quicksilver and the top of the fluid, will be augmented by a column, whose height is dv, the descent of the compound barometer; consequently the weight of the whole column of the fluid, pressing on the lower surface of the quicksilver, to which the height X is partly owing, will be increased by a column of that length; and this increase would produce a second ascent of the mercury at X, equal to itself, namely dv, were the fluid as heavy as quicksilver; but since it is supposed to be lighter in the ratio of s to 1, the ascent of the mercury on this account will be only $\frac{dv}{s}$.

But now, as in the former case, when the ascent of the mercury was v, the descent of the compound barometer was shown to be dv, so here the ascent of the mercury being $\frac{dv}{s}$, the descent of the compound barometer will be $\frac{d^2v}{s}$, the next descent $\frac{d^3v}{s^2}$, the next $\frac{d^4v}{s^3}$, and so on to infinity. Therefore the whole descent of the compound barometer, is to the ascent of the mercury in the common barometer, that is, n is to 1, as $d + \frac{d^2}{s} + \frac{d^3}{s^2} + \frac{d^4}{s^3} +$, &c. to 1, or as $\frac{ds}{s-d}$ to 1; because the terms of the series being in geometrical progression, the sum of them all is $\frac{ds}{s-d}$. Hence we have $n = \frac{ds}{s-d}$, and $ns = ds + dn$; that is, $1 : d :: n + s : ns :: \frac{n+s}{ns} : 1$ and $1 : \sqrt{d}$, that is, the diameter of ST to the diameter of HI, as $\sqrt{\frac{n+s}{ns}}$ to 1. Q. E. D.

Example 1.—Putting $s = 14$, and $n = 1$, the variations in each barometer will be equal, by taking the diameter of ST to the diameter of HI, as $\sqrt{\frac{15}{14}} : 1$, that is, as 30 to 29 nearly.

Example 2.—If n be put infinite, the diameter of ST will be to the diameter of HI, as $\sqrt{\frac{1}{s}}$ to 1, or 1 to $\sqrt{14}$; that is, as 1 to $3\frac{3}{4}$ nearly.

The bottom of the vessel V, and the ends of the tubes, ought to be made rather round than flat, for their more easy motion up and down in the fluid. It will be convenient to have a small basin fixed on the star, to contain shot, for the more easy poising the barometer in the fluid.

An Account of a Book, intitled, Christiani Ludov. Gersten Tentamina Systematis novi ad mutationes Barometri ex natura elateris aërei demonstrandas, cui adjecta sub finem, Dissertatio Roris decidui errorem antiquum et vulgarem per observationes et experimenta nova excutiens. Francofurti 1733, *in 8vo.* N° 427, p. 43.

This essay consists of 3 chapters; the first is wholly mathematical, containing a new theory concerning the propagation of tremulous vibrations along a series of contiguous elastic bodies. The second applies this theory to the solution of the chief appearances of the baroscope; and the last explains the several states or constitutions of the air and weather connected with them.

The particles of air, says Mr. Gersten, however unknown in other respects, are very well known to be capable of receiving and propagating tremulous vibrations; from hence it follows, as also from some principles of Sir Isaac Newton, that the air may be dilated by repeated tremulous vibrations; and these vibrations may be generated or produced by a confused motion of the particles of the air, or by the agitation of a wind.

The author, in prop. 7 and 8, undertakes to demonstrate, that the dilatation produced by the motion of a wind, is less when the ambient air has a motion the same way, than if the wind moved with the same sensible velocity against the quiescent atmosphere; but that this dilatation would be greater, if the atmosphere had a flux or current in a direction contrary to that of the wind.

He demonstrates, that a perpetual easterly wind will reign in all places within the tropics, arising from the diurnal heat; and that this wind will diffuse itself to the other regions without the tropics, and have a direction declining from the east towards the north or south, according to the situation of the region on the terrestrial globe; that its motion will be more remiss, the nearer the places are to either pole, and that the angle of declination from the east will be greater for the same reason.

The preliminary propositions being settled, he proceeds to account for the rising and falling of the mercury in the barometer thus. The air of the atmosphere in our regions has a natural motion or current, whose direction is situated between the east and north points of the compass. If therefore a special wind should spring up and blow in a contrary direction, it will produce tremulous vibrations, and consequently dilatations of the air; then equal bulks of the dilated air dilated, will have a less quantity of matter than before; therefore the gravity of the air will be lessened, and by consequence the quicksilver in the weather-glass will fall. And this decrease of gravity in the air, and of the height of the mercury in the baroscope, will be proportional to the intensity of the force of the wind, and the degree of opposition of its direction to that of the flux of the atmosphere, conjunctly.

This, says Mr. Gersten, is the reason why the mercury falls when southerly or westerly winds blow, and why the quicksilver sinks so very low when these winds blow stormy. On the contrary, since the effect ceases when the cause is removed, the height of the mercury will be greater, the fewer special winds there are blowing in a contrary direction. So that the gentle winds that blow from the points of the compass which lie between the north and the east are, he believes, nothing but the natural and universal motion, current, or flux of the atmosphere, impeded by or meeting with very few special fluxes.

The last chapter is employed in accounting for the various changes of the weather connected with, or consequent on the rise and fall of the mercury in the weather glass. The ingenious author, beginning with the origin and manner of forming vapours, undertakes to settle and confirm, on solid principles, that which the learned and sagacious Dr. Halley had long ago communicated to the learned world, on this argument.

The Design of the Dissertation annexed, is to inquire into the Nature of Dew, explain its Origin and Kinds.—All dews, according to our author's philosophy, owe their origin either to vegetables or terrestrial ascending exhalations. Such as derive their origin from vegetables, he takes to be only exudations of their leaves, &c. congealed by the air.

In treating of that kind of dew which is a secretion or exudation of a juice in vegetables, he observes, that some plants furnish the spectator with a very entertaining sight, the little drops of dew being disposed after a very regular, not fortuitous manner, on the surfaces or edges of their leaves. To determine whether this beautiful disposition of the dewy particles is owing to a descent from the chilled air over the plant, or a secretion made from the juices of the plant itself, he covered several with glasses, or earthen vessels, having their mouths downwards; and yet the next day plenty of this kind of dew appeared in its usual regular form.

As to the next species, or common dew, he produces so many, and so differently made experiments, against the common opinion of its descent, that if they be all true, it seems difficult to support it against them. Thus, for two months together, viz. June and July in 1728, every night, several smooth plates of brass were laid on the bare ground; and during these experiments, he never observed the least impressions or traces of dew on the upper surfaces; whereas the lower were always covered with it. He also suspended these plates by threads, in a horizontal position, and found the dew spread almost equally over both surfaces, at the height of 3, 4, or 5 feet; at the distance of 1 foot and $\frac{1}{2}$, the lower was more bedewed; but at the heights of 1, 2, or 3 inches, the lower was overspread with dew, while the upper had none.

He however mentions some experiments which he made. and at first view seemed to contradict his theory: for instance, when he used convex bodies, whether round or cylindrical, he found the upper surface covered with dew, and that, whether they were laid on the ground, or suspended at any height from it.

This observation is general, and extends to bodies of this kind, that are only contiguous, as heaps of straw, hay or wool. It is to observations of this kind, the common opinion of dews falling, owes its birth and main support.

Since hoar-frost is only common dew congealed, he made some of the same kind of experiments on that, with brass plates laid on the ground as before. These likewise he found covered with this kind of frost below, but free on the upper superficies, agreeably to his hypothesis.

The author closes the dissertation with a curious inquiry into the nature and origin of honey-dew. This he takes to be nothing but the excrements of some insects which are to be met with, adhering to the lower superficies of the leaves of plants; and appeals to the evidence of sense for a demonstration.

Experiments shown before the Royal Society with the Spiritus Vini Æthereus, and the Phosphorus Urinæ. By Dr. Frobenius, F. R. S. N° 428, p. 55.

Nov. 18, 1731, Dr. Frobenius took a solution of phosphorus in the ethereal spirit of wine, which he called liquor luminosus, and poured it into a tub of warm water; on which it gave a blue flame and smoke, attended with so small a degree of heat, as not to burn the hand, if put into it.—He poured some of his ethereal spirit of wine on a tub of cold water, and set it on fire with the point of his sword; which being first heated a little, he touched with it a piece of phosphorus lodged before-hand on the side of the tub. After the deflagration the water was cold.

He then showed a very extraordinary process with phosphorus glacialis urinæ, or stick phosphorus, of Mr. Ambrose Godfrey Hanckewitz.

He had a very pompous machine, which he calls machina Frobeniana, pro resolutione combustibilium; inventa anno 1730. It is really an improvement of the common bell, under which the oleum sulphuris per campanam is commonly prepared. This machine consisted of a concave plate of glass, AB, fig. 13, pl. 16, with a hole in the middle C, which communicated by a glass pipe CD, with a glass receiver EEF, below the plate AB. On the plate stood a massy golden tripus, sustaining a basin, about 4 inches diameter GH, having within it another smaller one IK, of the same metal, about 2½ inches diameter; this was heated a little. He then took small pieces of phosphorus out of a basin of water, which he soaked up with brown paper, that the phosphorus might be quite dry, which he put into a spoon, and flung it into the smaller golden basin IK; where it immediately took fire. Then he lowered down a large glass bell LMO, of about 18 inches diameter, and forming 3 quarters of a sphere; the rim LM being exactly ground to fit close on the plate of the glass AB. This glass bell was suspended by a wooden circle PQ PQ, to which were fastened 4 cords, that united into one knot at R, and from thence went a rope over a pulley S, in the crown of the machine, which coming down by the side of one of the pillars, served to raise up or let down the bell.

At the first firing of the phosphorus, the whole bell appeared luminous, and full of flame for a few minutes: when the deflagration of the first spoonful was over, he flung in another, and so on, till 2 ounces of phosphorus were consumed; from which were sublimed a large quantity of flowers into the bell, and some fell down on the concave glass AB. The bell at first felt cold, and never became more than moderately warm. As the flowers began to cover the inside of the bell to some considerable thickness, the flame was not seen through so brightly as before, but the whole appeared of a light azure, or sky-colour, which the doctor likened to the formation of the firmament: the flowers sublimed he likened to snow. The bell then being drawn up again, and the golden basins taken out, there remained in the smaller basin an almost fixed red earth, or caput mortuum. On the admission of the cold air, the snow flowers began soon to melt as per deliquium: which he compared to the formation of dew and rain; and as it dripped from the inside of the bell on the concave plate AB, it ran through the hole in the middle at C, by the tube CD, into the receiver EEF; where it was collected in form of a clear transparent liquor, somewhat clammy like gum-water, which he called water.

Some of the flowers mixed with any combustible matter, as common olive oil, &c. and put into a golden basin set over a lamp, fired immediately, and flamed like phosphorus, being in reality phosphorus regenerated, and burnt away to a substance like tar.

Some of the clammy water was put into a golden basin set on a lamp, and by augmenting the fire gradually, in about a quarter of an hour, when all the air bubbles were exhaled, the liquor became hard like gum, which had been dissolved in water, and was nearly dry, and perfectly transparent: this he called vitrum molle.

Next day he made some more of this vitrum molle, which he put into a crucible heated red hot, and then set it in a wind-furnace, and gave it the greatest heat for a quarter of an hour; when the matter in the crucible appeared fluid, like melted glass. He then poured it out into an iron pan; the matter remained red hot some time; when it was perfectly cold, it was hard, transparent, and brittle like common glass; but it soon began to relent, and in 24 hours was almost all turned to water again.

The Dr. said, if this vitrum molle be again entirely resolved in the air, which will take up near 14 days time, by distilling off the water, and letting the remainder melt per deliquium again, till all the saltish matter be resolved into water, there remains an insipid whitish earth, which fluxed in a glass-furnace, gives a true fixed glass.

Some Experiments on the Phosphorus of Urine, which may serve to explain those shown to the Royal Society by Dr. Frobenius; with Observations tending to explain the Nature of that wonderful Chemical Production. By Mr. Ambrose Godfrey Hanckewitz, Chemist, F. R. S.* N° 428, p. 58.

Mr. H. repeated the experiment of the deflagration of phosphorus under a bell, which had been first shown to the R. S. by Dr. Frobenius; but he found that a much more simple apparatus was sufficient, than the pompous machine the Dr. used. He took a strong wide-mouthed glass jar, which serves as a stand for the concave glass dish to rest on. In the middle of the glass dish is a hole, communicating with a pipe, which goes down into the abovementioned jar. Instead of the golden basins, a China cup a little warmed, serves perfectly as well for burning off the phosphorus: the last and main thing is a large glass bell, which fits nearly close on the glass dish. This bell may be easily lifted off and on with the hands by an assistant, without any frame or ropes to suspend it.

He took 1 oz. of phosphorus, which he deflagrated in the same manner as in Dr. Frobenius's experiment, and obtained of the white sublimed flowers 10

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* Mr. A. Godfrey Hanckewitz was once assistant to Mr. Boyle, and one of the best chemical operators of his time. The above, with some experiments on ambergris, and an account of the West Ashton well water, appear to be all the observations which this chemist communicated to the public

drs. that is 2 drs. more than the weight of the phosphorus before deflagration: they were so very light, that they just filled a half pint pot.*

The 10 drs. of flowers being set in a cool moist place, exposed to the air, resolved into a liquamen, weighing 4 oz. 2 drs. which liquamen much resembles Ol. Sulph. per campanam; but contains an acid salt, more fixed in the fire than any other salt we know of in nature, and having many other properties peculiar to itself, which other acid salts have not.

The phosphorus receives this fixed acid from the urine only; for the salt of urine is so fixed, that on a live charcoal with a blow pipe, it plays and rolls about like silver on the cupel. And whereas all other liquid acids evaporate with ease, this, on the contrary, is so fixed, as to require a greater heat for its evaporation than that which keeps lead in fusion; and the phlogistic part, notwithstanding its lightness, is so intimately and firmly connected with the rest of its principles, as to sustain a degree of heat equal to that of red hot iron; during which heat the salt sparkles and emits flames very bright for a good while; and this sparkling being over, it remains red hot in fusion, and perfectly transparent; and by greater heat may be vitrified, as will be shown hereafter.

He put the abovementioned liquamen into a glass retort, which he set in a balneum marinæ, and distilled it to a strong inspissation. It yielded only an insipid phlegm; except that towards the last it came over a little impregnated with the acid, but not sharper on the tongue than if it had been a mixture of half an oz. of vinegar with 4 oz. of water.

Then removing the retort with the inspissated liquor into a sand-furnace, he increased the heat gradually, so as to make the sand and retort thoroughly red hot, till at last the bottom of the retort was ready to melt; he then left it till next day, when being perfectly cold, he broke the retort, and found a most admirable white salt at the bottom, which was so united with the glass as not to be separated from it; and some was spread all over the retort quite up to the neck, and, as near as he could guess by view, it seemed to be as much in quantity, could he have taken it out to weigh it, as the original phosphorus from whence it was produced: its taste was very sharp and saline; but notwithstanding its great fixity, in having endured a melting heat for several hours, it relented again in a moist air, and in a few days was entirely resolved into a liquamen.

The phosphorus, after its deflagration, leaves an almost fixed red earth, or caput mortuum, behind it, as mentioned in Dr. Frobenius's experiment. Though it might be imagined, that all the inflammable parts of the phosphorus had been burnt off in the first deflagration, which seemed very violent, yet this

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* The white flowers here mentioned were phosphoric acid.

red earth retains so much of an unctuous phlogistic, that being placed over a red hot fire, it swells up, and keeps in fusion a great while, emitting flames and flashes of light, as long as it is kept on the fire; but when cold again, if exposed to a moist air, it relents and resolves as the flowers do: for the acid salt of the urine adheres so strongly to it, that though it undergoes severa strong ignitions, it will relent again as often, when set in the air.

He took some of the white salt that stuck to the retort, and in order to try the utmost degree of its fixity, he put some of it into a crucible, and gave it a vitrifying heat, in which it remained some hours, but was not yet run to glass, appearing only like a fixed white earth, as hard as stone, and shining as if just ready to vitrify; yet it was so far fixed, as not to relent any more in the air; had no saline taste, nor was dissolvable in water. He therefore took another portion of the same salt of phosphorus, which he kept a longer time in the vitrifying heat, and he found it at last run into perfect glass.

Thus we see what a wonderful subject phosphorus is! and how surprising is it that such an inflammable body, consisting of the unctuous and acid parts of the urine, should thus become glass!

The conclusion which he makes from this remarkable experiment is, that here is a perfect transmutation of bodies; the phosphorus being transmuted into a fine transparent glass, of a bluish green colour, coming nearer to the hardness of a diamond than any other glass, and in the same quantity as the phosphorus at first used, which, without any addition, produces this glass, ounce for ounce.*

He further adds, that the crude phosphorus, without any deflagration, but only cut very small, or scraped fine with a knife, and laid on a glass dish in moist air, will in about a week resolve into a liquamen near 8 times its original weight: which liquamen is the same in all respects as that which comes from the sublimed flowers by deflagration, and may be vitrified likewise. In scraping the phosphorus, great care is to be taken not to do it too hastily, lest by heating it, you set it on fire.†

Reflections on these Experiments.—Respecting the nature of phosphorus, it is Mr. H.'s opinion that it does not exist in animals by itself; but when formed out of urine, by means of putrefaction and fire, its principal contexture is found to consist of a subtle acid, concentrated by the salt of urine, and of a fat depurated oil.

Phosphorus affords us so many wonderful phænomena, that to explain them all would take up a large treatise; a perfect phosphorologia, being what would exceed the limits of this short account.

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* It is now known that this so called glass of phosphorus, is phosphoric acid deprived of water.

† The liquamen here mentioned is in the modern chemical nomenclature, termed phosphorous acid.

The phlogistic part is so slightly connected with the other principles, that the least motion, friction, or warmth, sets it on fire.

The fixed part seems to consist chiefly in the acid salt of the urine; which is at first so intimately concentrated with the phlogistic part, as in deflagration to be hurried up or sublimed along with it; yet being by this operation freed from it, it becomes fixed, and can by no degree of heat be again sublimed.

Phosphorus may be called a urinous sapo, or soap, as it consists of the saline and oleaginous parts of the urine: but phosphorus is not to be got in so great plenty out of urine alone, as when the fæces alvinæ are elixirated along with it, and then brought to a magma fit for distillation: nor is there so great a quantity of phosphorus in the urine of other animals, as of men; nor is it to be got from any natural productions, or any parts of animals or vegetables in their crude state, before they have undergone concoction in the stomach of an animal. How far therefore the liquor gastricus, the bile, and succus pancreaticus may contribute to the formation of it, is a disquisition he does not here enter on, but leaves it to the inquiry of philosophers.

As to the parts of which phosphorus consists, it may be considered as the soot of a deflagrated oil; and so may every combustible substance be considered as a kind of phosphorus, as consisting of inflammable materials.

Phosphorus is more immediately compounded of a salt tending to the nature of sal ammoniac, of a urinous salt, of an acid, and an oily phlogiston with a subtle earth; by means of these salts existing in the urine, the fæces alvinæ are the better elixirated, and those particles extracted which contribute to form the phosphorus. With these salts are very intimately combined, in the phosphorus, oleaginous or fat particles, which are the proper materials of that subtle phlogiston, the true domuncula ignis, and indeed the main constituents of the whole compound.*

As for the preparation of this wonderful production, it is done by distilling the saponaceus magma in a close vessel, with a reverberatory fire, much stronger than that used for the distillation of aquafortis, or the other mineral acid spirits; the rest of the proper encheiresis belongs only to the operator to manage secundum artem. When this operation succeeds rightly, there comes forth, 1st, A thick unctuous oil. 2dly, A more subtle oil, resembling the oleum philosophorum, which is olive oil distilled from brick-dust. 3dly, The fixed acid inclosed in a very subtle acid. Near the end of the distillation, comes over that depurated oil, which constitutes the inflammable part of the phosphorus,

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* Mr. H. represents phosphorus to be a very mixed compound indeed; whereas in the present day this inflammable body is considered as a simple substance, the basis of the phosphoric acid.

which is not raised up till the last, and that by the continuance of a very strong reverberatory fire.

But an operator that is not well exercised in the degrees of fire, and does not know how and when to take away these oils apart, will have nothing but a volatile salt, and fetid oil, and get at last only a little unctuous opaque phosphorus; such as the famous Kunckel, Dr. Krafft, and Brandt did, as they acknowledge in their writings; but not our hard transparent glacial phosphorus.* Since Kunckel therefore, and his followers, were never able to make the true solid glacial phosphorus, it was absurd for him to write, that he could make it even out of crude indigested things, in their natural state: for either this famous man spoke too much at large, and had never tried the experiments, or else he must design to impose upon the world: for Mr. H. says he can boldly contradict him in this point, from the several experiments he has made, but never found any true phosphorus, except in such things as had undergone digestion in animals. And he knows himself to have been for these 40 or 50 years, that is, ever since he left the laboratory of his master, the Hon. Mr. Boyle, the only person in Europe able to make and produce in any quantity the true solid phosphorus.

Mr. H. did not content himself with working on the urinous sapo of man only, but examined likewise the excrements of other animals; as for example, of horses, cows, sheep, &c. and got phosphorus, but not in so great quantities as from man; probably because they feed on nothing but vegetables. He then examined the dens of lions, tigers, and bears, making experiments on their excrements, and likewise on those of cats and dogs, which being carnivorous animals, he obtained more phosphorus thence than from the other creatures: his curiosity led him likewise to the rats-nests, and mouse-holes, and he had phosphorus thence. He then addressed himself to the feathered tribe, visiting the hens' roosts, and pigeon-houses, and got some small matters thence also: he emptied the guts of fish in order to get their excrements, and had a little phosphorus from these, but none from the fishes by themselves.

He was next induced, by Kunckel's assertion, to try what he could obtain out of crude vegetables, viz. corn and other fruit: he thought that putrefaction would bring them the nearest to an ammoniac and urinous state, because of the heat produced in them by it; but his labour was all in vain. After these experiments, he took in hand fossils and minerals: he began with the common fossil coal, thinking that the phlogiston in this bituminous substance might

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* On this subject the reader is referred to p. 489, Vol. II. also to a note at p. 478, Vol. III. of these Abridgments.

have been to his purpose; but he found nothing like phosphorus, there coming over only a bituminous oil; and at last, by increasing the fire to the highest degree, there sublimed some white talcky flowers, which were neither sulphureous, nor acid, nor alcalic, but insipid like talc; so he gave up all further experiments on other minerals.

He has often wished for a sufficient quantity of the flies which shine in the dark, of which there are great numbers in Italy, especially in Tuscany; or of our common glow-worms, which seem to have phosphorus lodged in their bodies.

This phosphorus is a subject that occupies much the thoughts and fancies of some alchymists, who work on microcosmical substances; and out of it they promise themselves golden mountains. Of this number was the famous Dr. Dickinson, physician to king Charles II: he toiled and laboured many years in experiments on the stercus humanum; and has several times with the greatest pleasure showed me metallic reguluses, he had extracted from it. This is what Mr. H. had often done himself, and no wonder (he observes) for we take in daily with our food, and sometimes in medicines, both mineral and metallic substances, besides what metallic vessels, kettles, pots and dishes furnish: we see a solution of the metal on a knife after cutting any acid fruit, by the black spots on it, and the metallic taste it communicates to the thing it cuts.

Dr. Lister has shown, that stones out of the human bladder being calcined, iron may be extracted from them by a loadstone. And the great Boerhaave has made it evident, by various experiments, that there is scarcely any terrestrial substance, either in men, brutes, or plants, which after union does not exhibit some metallic particles. Dr. Becher says, that out of brick-earth mixed with any fat or oil, and calcined in the fire, he has produced iron: for it is only the iron that causes the redness of the bricks, and can be extracted from them again. Moreover, metals are dissolved by the salts and moisture in the earth, and so mix with the nutritious juices of vegetables; hence it may in some respect be said, that we eat metals with the greatest part of our food.

Having given the foregoing short account of the production of phosphorus, Mr. H. subjoins, that there is produced out of the residuum, after the phosphorus is made, a particular salt, which he names sal phosphori, or salt of phosphorus. This salt is fixed in some degrees of fire; yet it may be sublimed in a close vessel, which other fixed salts cannot be, except they still contain somewhat volatile in them; but this salt has no such thing in it, neither is it anywise alkaline. How to produce this salt, remains as much a secret as the phosphorus itself; for he that cannot produce this salt, will never be able to make phosphorus.

There is scarcely any substance, out of which a chemical operator cannot produce water and earth, salts, or an acid spirit, and a urinous unctuosity, in more or less quantity, according to the nature of the body; and where there is one of these, there is fire to be demonstrated, but not without each other's help. The encheiresis of this would be too long for this place, he therefore omits it here.

From this preparation of phosphorus, we may reflect on the fuligo, or soot, of all combustible substances; for it is the phlogiston only that burns and produces flame; it exists in sulphureous bodies, and unctuous earths, in pitch, rosin, wax, and oils; and in the fat of animals: but the finest exists in ardent spirits, which when brought to that surprising subtilty, as that liquor described by Dr. Frobenius in Trans. N° 413, do truly deserve the name of ether.

From what has been said, we see, 1. That the saponaceous magma of urine has great affinity with common sulphur; being a sulphureous body, composed of an acid and depurated oil, joined with a small proportion of earth.* 2. This phosphoreal magma comes very near to Homberg's pyrophorus,† which wants only the salt of urine in it, instead of which, alum is used to fix the sulphur. 3. We may observe hence, that urinous particles exist in greater abundance in animals: but that phlogiston abounds most in vegetables, from which is prepared that fine ethereal spirit shown by Dr. Frobenius. 4. We produce the phlogiston out of fat substances; and from the phlogiston, fuligo, or soot; and from the fuligo a urinous salt. 5. From the corrosive oil of sulphur, we have a pure subtle oil, which is intimately combined with it, and is the actual fire of the phosphorus, that by barely rubbing, or the least degree of heat, is kindled into flame. 6. He who knows perfectly the method of making phosphorus, can choose whether he will sublime his magma of urine into phosphorus, or into sulphur; for the difference consists only in the encheiresis.‡

Observations of the Appearances among the fixed Stars, called Nebulous Stars. By W. Derham, D. D. Canon of Windsor, F. R. S. N° 428, p. 70.

Dr. Derham having, in autumn 1732, made some good observations, with his 8-foot reflecting telescope, of the appearances in the heavens, called Nebulous Stars, communicated them to the Royal Society, to incite others to

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* See note p. 599 of this vol.

† Homberg's pyrophorus (which must not be confounded with Homberg's phosphorus improperly so called) would appear to be a compound of sulphuret of potass and charcoal, the alum being decomposed in the process for preparing it. His pyrophorus therefore is a very different substance from the phosphorus described in this paper.

‡ This is very erroneous, phosphorus and sulphur being totally distinct substances, by no encheiresis can the one be converted into the other.

make further observations of them; because he thinks there is much more in them worthy of the inquiry of the curious, than has hitherto been thought; and because he fears he will not be able to pursue his observations much further, as his reflector loses its excellence and power, by beginning to tarnish.

These appearances in the heavens, have had the name of nebulous stars: but he says they are neither stars, nor such bodies as emit, or reflect light, as the sun, moon, and stars do; nor are they congeries, or clusters of stars, as the milky way: but whitish areæ, like a collection of misty vapours: whence they have their name.

There are many of them dispersed about, in diverse parts of the heavens. This catalogue of them, here trancribed from Hevelius's Prodromus Astronomiæ, may be useful to such as wish to inquire into them.

A Catalogue of the Nebulosæ.

Their Places.	Their R. Asc. Anno 1660.			Their Declin. Anno. 1660.			
In Andromeda's Girdle	6°	4′	45″	39°	27′	57″	N.
Forehead of Capricorn	300	2	53	20	1	53	S.
Another preceding its eye	301	59	55	19	11	30	S.
Another following it	302	35	9	19	36	0	S.
One above those, adjoining to the eye of Capri.	302	25	31	18	48	58	S.
Preceding above the Swan's Tail, and last in its N. foot	304	54	8	47	54	20	N.
One following a star above the Swan's Tail, out of the constellation	312	10	5	53	5	20	N.
On the outside of Hercules's left foot	264	52	46	48	9	10	N.
In the left leg of Hercules	265	38	37	38	5	50	N.
On the top of Hercules's Head	252	24	3	13	18	37	N.
At the Ear of Pegasus	332	38	45	3	3	12	N.
In the western border of Sobieski's Shield	272	32	34	14	23	35	S.
Under the beam of the Scales of Libra	219	26	15	9	16	27	S.
Above the back of Ursa Major	183	32	41	60	20	33	N.
In the third joint of Scorpio's Tail	12	43	00	19	1	0	
	♐ long.					s. lat.	
Between Scorpio's Tail, and the Bow of Sagitta.	24	32	00	11	25	0	
	♐ long.					s. lat.	

Besides these, Dr. Halley, in Phil. Trans. N° 347, mentions one in Orion's Sword; another in Sagittary; a third in the Centaur, never seen in England;

a fourth preceding the right foot of Antinous; a fifth in Hercules; and that in Andromeda's Girdle.

Five of these six Dr. D. has carefully viewed with his excellent 8-foot reflecting telescope, and finds them to be phænomena much alike; all except that preceding the right foot of Antinous, which is not a nebulose, but a cluster of stars, somewhat like that in the milky-way.

Between the other four, he finds no material difference; only some are rounder, some of a more oval form, without any fixed stars in them to cause their light; only that in Orion has some stars in it, visible only with the telescope, but by no means sufficient to cause the light of the nebulose there. But by these stars it was, that he first perceived the distance of the nebulosæ to be greater than that of the fixed stars, and put him on inquiring into the rest of them. Every one of which he could very visibly and plainly discern, to be at immense distances beyond the fixed stars near them, whether visible to the naked eye, or telescopic only; yea, they seemed to be as far beyond the fixed stars, as any of those stars are from the earth.

And now he concludes them certainly not to be lucid bodies, that send their light to us, as the sun and moon. Neither are they the combined light of clusters of stars, like that of the milky-way; but he takes them to be vast areæ, or regions of light, infallibly beyond the fixed stars, and devoid of them. He says regions, meaning spaces of a vast extent, large enough to appear of such a size as they do to us, at so great a distance as they are from us. And since those spaces are devoid of stars, and even that in Orion itself has its stars bearing a very small proportion to its nebulose, and they are visibly not the cause of it, he leaves it to others to judge, whether these nebulosæ are particular spaces of light; or rather, whether they may not, in all probability, be chasms, or openings into an immense region of light, beyond the fixed stars.

Some Magnetical Observations, made in May, June and July, 1732, *in the Atlantic or Western Ocean; as also the Description of a Water-Spout. By Mr. Joseph Harris.* N° 428, p. 75.

The knowledge of the magnetical variation is of such consequence to the mariner, that without it he cannot know his course; and were its theory once established, it might be of great use for estimating the longitude in several parts of the world, as has been often and very justly observed by others. But till this be determined, we must rely on observations.

Sometime before, Mr. Harris took notice of the imperfections of the common azimuth compass, and how ill it is adapted for the purpose intended. He also gave the description of a new instrument, proposed to remedy the principal objections to the former; and further experience has sufficiently confirmed him in what he advanced. But he would be glad to have it determined by those who have convenient opportunities of making experiments of this kind, what would be the properest diameter and weight for a needle and card, and what ought to be their proportional weights to each other when taken separately: regard being had that the friction be no more than what is necessary to prevent the card from being too much affected by the motion of the ship. Some observations incline him to think, that a sea-card should not exceed 6 inches diameter; and that most of those generally used, are too heavy for nice experiments, though they may be well enough adapted for common purposes.

In March and April, 1732, the variation at Black-River in Jamaica, was very accurately observed to be from 6° to 6° 5′ easterly. Off the Havanna about 4½ deg. easterly. The rest of the observations he made, are exhibited in the following table.

Lat. N.	Long. from Lond. w.	Variation.	Latitude.	Long. from Lond. w.	Variation.
27° 0′	80° 0′	4° E.	35° 55′	65° 30′	5° W.
28 45	80 0	3½	38 6	60 30	6½
31 0	77 45	1¾	39 10	57 30	8⅓
32 15	72 30	00	39 40	56 30	8¾
32 40	72 0	1 W.	43 0	45 0	9½
32 45	71 30	1½	43 5	44 35	9½
32 52	70 40	2¼	44 40	35 15	11½
34 30	67 25	4⅓	47 20	20 20	11

The instrument used was so easily managed, that unless the sea was pretty rough, an observation might be depended on to about a quarter of a degree, had the card performed to the same exactness. But by comparing several observations made under the like circumstances, as to the weather, it seems as if the virtue of the needle was not always of equal strength. Sometimes several observations would agree exceedingly well; at other times the card would stand indifferently any where within a degree or more of its meridian; and this was observed in several cards. Another circumstance was surprising: the card would sometimes differ about 2° from itself between the morning and evening of the same day; and this difference would continue as it were regularly for several days, then vanish for a week or more, and afterwards would return and continue as before.

The greatness of this difference, and the near agreement between the observations made in the same forenoon, or afternoon, among themselves, leave no room to suspect that it proceeded altogether from an error in observing. Whatever be the cause of this, the error was always the same way; that is, the westerly variation in the morning, less than in the afternoon. He carefully examined if this could be any ways owing to the instrument, or to any iron near the place where it was usually set for observation; but he was fully convinced it could proceed from neither.

It now appears that the numbers in the foregoing table cannot be strictly accurate; but the error can scarcely any where exceed half a degree; for in most cases several observations were made pretty near together, of which a medium was taken, making allowances according to the circumstances attending each.

To this Mr. Harris adds the description of a water-spout, which they saw about sun-set, May 21st, 1732, in lat. 32° 30′ N. and long. 9° easterly from the meridian of Cape Florida.

When first seen, the spout was whole and entire, and much of the shape and proportion of a speaking-trumpet, the small end being downwards, and reaching to the sea, and the large end terminating in a black thick cloud. The spout itself was also very black, and the more so the higher up. It seemed to be exactly perpendicular to the horizon, and its sides perfectly smooth, without the least ruggedness. Where it fell, the spray of the sea rose to a considerable height, which made somewhat of the appearance of a great smoke.

From the first time they saw it, it continued whole about a minute, and till it was quite dissipated about 3 minutes. It began to waste from below, and so gradually up, while the upper part remained entire, without any visible alteration, till at last it ended in the black cloud above. On which there seemed to fall a very heavy rain in that neighbourhood. As it wasted, the bottom of the remaining part was irregular, somewhat like the trunk of a tree broke asunder: there was but little wind, and the sky elsewhere was pretty serene. The spout seemed to be above 2 leagues off, and the angle under which the small end appeared, must be at least 20 min. According to which estimation, the thickness of it must be upwards of 60 yards, and its height or length about three quarters of a mile.

An Account of an Earthquake in Apulia and most other parts of the Kingdom of Naples, in the Year 1731. By Dr. Cyrillus of Naples. N° 428, p. 79. *Translated from the Latin.*

Dr. Cyrillus made the following short abstract from the observations sent him by Dr. Rosetti from Apulia, and by other persons at Geovenazzo and Foggia.

March 9, 1730-1, o. s. at 4 in the afternoon, there was an earthquake almost all over the kingdom of Naples, but it was felt most in Apulia. While it lasted, all those appearances mentioned by the ancients, were observed here also: as first a tremor; then a pulse (σφυτμος) according to Aristotle, or a succussation, as Possidonius from Seneca calls it; and last of all an inclination, or a nutation of the earth, like that of a ship, as it were. These various motions succeeded each other alternately for 3 minutes and a few seconds. At that time the air was overcharged with dense, low, and immovable clouds, which were afterwards dissipated by a gentle northerly wind. Next day the sun shone more languid, as if he had been covered with very thin clouds, though there were then none in the heavens. This phenomenon was also observed in the following stronger shocks. The fishermen near the shore observed the sea swell suddenly, and they weathered out a storm from Siponte and Barletta, that is, nearly, from the north, without any wind, but not without apprehensions of being ship-wrecked.

March 10, at 8 in the forenoon, there happened a new, but a shorter and weaker earthquake, in the same province; but not so weak but that it was felt at Naples. This was preceded by a kind of accension, or short coruscation, about Mount Garganus, observed by the inhabitants of Terra di Bari, and which insensibly vanished into smoke or darkness. In the parts about Foggia a strong N. E. wind generally preceded this second earthquake, as also the others that happened afterwards in April, October, and November; though sometimes the air was quite calm. The number of houses that fell, and of men buried in their ruins, was considerable; not less than 600. The town of Foggia seemed to be the centre of these shocks: for, there the shocks and downfall of the houses were most considerable; and from thence they diffused themselves to more remote places, the impetus gradually remitting; so that it may be said that the propagation of this earthquake was successively diminished (unless the different solidity and interruption of the interjacent earth caused any alteration) in the duplicate ratio of the distances, according to the common laws of nature in many other things; which was carefully observed in the oscillations of pendulums placed at different distances from Foggia: for pendulums of a palm in length at Ascoli di Satriano, and at Giovenazzo, applied to a graduated semicircle, and moving in the concussions of the earth, erred more or fewer degrees from the centre of oscillation, according as they were more or less distant from Foggia: for, the number of these degrees (greater in the nigher Ascoli, and less in the remoter Giovenazzo) answered nearly to the duplicate ratio of the distance of these places from the centre of the earthquake: and hence it likewise happened, that when there was but a very slight trembling at Foggia, the pendulum moved slowly at Ascoli, but stood still at Giovenazzo.

In almost all the shocks this year, it was constantly observed, that a crashing in the air and a horrid noise preceded them. Pliny, lib. 2, p. 80, also observes, that sometimes terrible sounds, bellowings, and shouts like human, ushered in earthquakes. This crashing in the air was diffused in a contrary direction: for, whereas the parts of the earth were shook by a motion from the centre to the circumference; so on the contrary, the motion of the air plainly converged from the circumference to the centre; which phenomenon may have yielded no small matter of speculation to naturalists. The Doctor would observe that this is different from what Aristotle thought was the case with meteors, namely that an external wind must contribute to an earthquake, as according to him the coast of Achaia was shook by the conflict of a north and south wind.

Lastly, it is worth observing in this earthquake, that near a country farm of Carthusians, called Tré Santi, whose house had by the earthquake been levelled with the ground since the 1st of March, in that spot where the channel of the Fontana del Pesce is most depressed, there broke out in a plentiful stream a new spring of muddy hot water. This indeed, is no new thing, nor was it unknown to the ancients: since we find from their accounts, that waters burst out when the body of the earth opens, in the same manner as water enters through the seams of a ship; nay, they give an account not only of small streams, but deluges of water that drowned whole cities; which may seem more probable to those who hold with Thales, according to Seneca, that the earth, supported by the waters, sometimes floats like a ship: but these things will seem absurd to such as know the true structure of the terraqueous globe. The water that burst out in Apulia began to dry up gradually, and in a month's time it quite disappeared; but the dry sand, even for some time, retained a sulphureous smell. Thus Pliny, lib. 31, 4, affirms, that earthquakes pour out and drink up waters: so that it is not surprising, that we have accounts of lakes, fountains, or rivers breaking out, where there were none before, and of others being dried up. It was universally reported, that shallow wells, at the time of the first earthquake, threw out their waters from their wide mouths: yet it is not at all credible, that from the greatest shock water should burst out, but that probably new water springing up in the bottom of these wells, as in other places, and filling their cavities, it was thrown out.

The water which had burst out near Tré Santi, when examined, exhibited the following phænomena.

1. Bulk for bulk by the areometer it weighed 82 grains more than rain water;

and only 15 grains more than the water of a brackish fountain in that place. 2. A pound of the same water, distilled to driness, left behind in the bottom of the vessel, half a drachm of a substance inclining to the nature of crocus martis, sprinkled over with a scruple of a white and insipid earth: the loadstone attracted some reddish particles from this dust after drying it. In the distillation a sulphureous smell was pretty sensible. And hence, after the experiments of the celebrated M. Lemery, we have a new accession of arguments, that subterraneous fires and volcanos may be easily accended by the commixture of sulphur and iron; and consequently, that earthquakes may be produced by the successive kindling of latent fires. 3. Two drachms of the galls, called di levante, and with which ink is made, reduced to a very fine powder, and infused for 4 hours in 2lb. of that water, tinged it of a light azure colour, with a subsequent precipitation of the powder.

A Lunar Eclipse observed at Rome, Dec. 1, 1732. *By S. S. Revillus, Botrario, and Manfredi.* N° 428, p. 85.

True time, afternoon.

At 8h	45m	28s	The penumbra was sensible.
8	51	19	The beginning of the true eclipse.
9	48	24	The total immersion.
11	31	13	Beginning of the emersion.
12	26	55	End of the eclipse.

An Eclipse of the Moon observed in Fleet-street, London, Nov. 20, 1732, *at Night. By Mr. Geo. Graham, F. R. S.* N° 428, p. 88.

The beginning at	8h	1m	30s	apparent time.
Immersion	8	59	30	
Emersion	10	38	0	
End	11	37	0	

Observed with a small telescope about 18 inches long, which magnifies about 13 times.

N. B. Mr. Hodgson at Christ's-Hospital, with a 4-foot telescope, observed the beginning at $8^h\ 1\frac{1}{2}^m$, and the end $11^h\ 36\frac{1}{2}^m$.

The Bills of Mortality for the Town of Dresden, for a whole Century, viz. from the Year 1617 *to* 1717, *containing the Numbers of Marriages, Births, Burials, and Communicants. By Sir Conrad Sprengell, M. D., F. R. S.* N° 428, p. 89.

Sum total from 1617 to 1717 inclusive, viz.

Married 24294 couples. Christened 83412. Buried 98611. Communicants 4654064, among whom 1686 received holy orders.

The Bills of Mortality for the Imperial City of Augsburg, from the Year 1501 *to* 1720 *inclusive, containing the Number of Births, Marriages, and Burials. By the same.* N° 428, p. 94.

Total of all in the 220 years, are, born 347481, died 394837, couples married 99142.

Remarks on the foregoing Bills of Mortality for the Cities of Dresden and Augsburg. By Mr. William Maitland, F. R. S.* N° 428, p. 98.

By the first septenary of the centenary of the bills of mortality for the city of Dresden, from year 1616 to anno 1624, it appears, that there died in that electoral capital 3136 persons; and in the last septenary of the said centenary, from anno 1709 to 1717, 8836.

And by the first septenary of the same centenary of the bills of mortality of the imperial city of Augsburg, from the year 1616 to anno 1624, it appears that there died in that city 11371; and in the last septenary, from 1709 to anno 1717, only 6297. By which appears the great vicissitudes of sublunary affairs, in the vast disparity between the aforesaid cities; for as the former has increased near two thirds in the number of its inhabitants, so the latter decreased near one half in the same space of time.

An Account of Symptoms arising from eating the Seeds of Henbane,† with their Cure, &c. and some occasional Remarks. By Sir Hans Sloane, Bart. P.S.R. N° 429, p. 99.

In 1729, a person came to consult Sir Hans Sloane on an accident that befel 4 of his children, aged from 4½ to 13½ years, by eating some seeds they had gathered in the fields, near Pancras church, which they mistook for filberts. By one of the capsules Sir Hans instantly knew it to be that of the hyoscyamus

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* A native of Scotland, the same who wrote a History of London in 2 folio vols. He also wrote a History of Edinburgh, and a compilation on the Antiquities of Scotland. He died in 1757, aged 64.

† Hyoscyamus niger. Linn.

niger, vel vulgaris, C. B. or the common henbane, which bears some gross resemblance to the husk of a filbert; and the seeds are like those of the poppy. The symptoms that appeared in all the 4 were, great thirst, swimmings of the head, dimness of sight, ravings, and profound sleep; which last, in one of them, continued 2 days and nights.

Sir H. ordered them all to be bled, blistered in several places, and afterwards purged with a medicine composed of elect. lenitiv. ol. amygd. dulc flor. sulph. et syr. flor. persicor. which operated both by vomit and stool: and by this method they perfectly recovered.

The delirium occasioned by these seeds, differs from the common, and in some measure agrees with that produced by the datura, a species of stramonium; and by the bangue of East India, a sort of hemp.

On this occasion, Sir Hans gives an instance of the great virtues of henbane-seeds in the tooth-ache. A person of quality tormented with this racking pain, had an empyric recommended to him; his anguish obliging him to submit to any method of procuring ease. The quack conveyed the smoke of burning henbane-seeds, by means of a funnel, into the hollow tooth, and thus removed the pain: but at the same time there dropped some maggots from the tooth, as he pretended, into a pail of water placed beneath for that purpose; which was very surprising to the beholders. Sir Hans procured one of the maggots, and sent it wrapped up in silk to Mr. Leuwenhoeck, at Delft in Holland, where it arrived safe and alive. On examination, he found it to be entirely like those bred in ordinary rotten cheese: therefore, he got some of these latter, and carefully fed them, and that which was sent him, on the same cheese, and they were all, according to the usual modes of nature turned into small scarabæi; so that there appeared not the least difference between them, either when maggots or scarabæi, both being returned back from Holland.

On the whole, though the smoke of the henbane-seeds cured the tooth-ache, it is highly probable the maggots had been conveyed thither, and let drop into the water by some slight of hand; seeing, by means of some such dexterity, empyrics daily acquire reputation from a medicine, which from the prescription of an honest physician would be little noticed.

Abstract of a Journal of Meteorological Observations, made at Petersburgh, from Nov. 24, 1724, *to June* 23, 1725. *By the Rev. Mr. Tho. Consett; with Meteorological Observations at Lunden in Sweden, in* 1724; *with Remarks on them. By Wm. Derham, F. R. S.* N° 429, p. 101.

This journal contains observations, three times in the day, of the barometer, the winds and their strength, the weather, and (after April 15) of the thermometer.

An Account of the Damp Air in a Coal-Pit, sunk within 20 *Yards of the Sea. By Sir James Lowther, Bart.* N° 429, p. 109.

Sir James Lowther having occasion to sink a pit very near the full sea-mark, for draining one of his principal collieries near Whitehaven, in the county of Cumberland, which it was known would be near 80 fathom in depth to the best seam of coals, which is 3 yards thick; the work was carried on day and night very successfully, through several beds of hard stone, coal, and other minerals, till the pit was sunk down 42 fathom from the surface, where they came to a bed of black stone, about 6 inches thick, very full of joints, or open cliffs, which divided the stones into pieces of 6 inches square, the sides being all spangled with sulphur, and in colour like gold. Under this black-stone lies a bed of coal 2 feet thick: when the workmen first pricked the black-stone bed, which was on the rise side of the pit, it afforded very little water, contrary to what was expected; but instead of it, a vast quantity of damp corrupted air, which bubbled through a quantity of water, then spread over that part of the pit, and made a great hissing noise; at which the workmen being somewhat surprised, held a candle towards it, and it immediately took fire on the surface of the water, and burned very fiercely; the flame being abont half a yard in diameter, and near 2 yards high, which frightened the workmen, so that they took the rope, and went up the pit, having first extinguished the flame, by beating it out with their hats.* The steward of the works being informed of it, went down the pit with one of the men, and holding a candle to the same place, it immediately took fire again, as before, and burnt about the same size; the flame being blue at the bottom, and more white towards the top. They suffered it to burn near half an hour, and no water being drawn in that time, it rose and covered the bottom of the pit near a yard deep, but that very little abated the violence or bulk of the flame, it still continuing to burn on the surface of the water. They then extinguished the flame as before, and opened the black-stone bed near two feet broad, that a greater quantity of air might issue forth, and then fired it again; it burned a full yard in diameter, and about 3 yards high, which soon heated the pit to so great a degree, that the men were in danger of being stifled, and so were as expeditious as possible in extinguishing the flame, which was then too strong to be beaten out with their hats; but with the assistance of a spout of water, of 4 inches diameter, let down from a cistern above, they happily got it extinguished without further harm. After this no candles were suffered to come near it, till the pit was sunk down quite

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* This so called "damp air" was hydrogen gas.

through the bed of black-stone, and the two feet coal underneath it, and all that part of the pit, for 4 or 5 feet high, was framed quite round, and very close jointed, so as to repel the damp air, which nevertheless, it was apprehended, would break out in some other adjoining part, unless it was carried quite off as soon as produced out of the cliffs of the stone; for which end, a small hollow was left behind the framing, to collect all the damp air into one side of the pit, where a tube, of about 2 inches square, was closely fixed, one end of it into the hollow behind the framing, and the other carried up into the open air, 4 yards above the top of the pit; and through this tube the said damp air has ever since discharged itself, without being sensibly diminished in its strength, or lessened in its quantity, since it was first opened, which was 2 years and 9 months before. It is just the same in summer as in winter, and will fill a large bladder in a few seconds, by placing a funnel at the top of the tube, with the small end of it put into the neck of the bladder, and kept close with one's hand.

The air thus inclosed in the bladder, and tied close, may be carried away, and kept some days, and being afterwards pressed gently through a small pipe into the flame of a candle, will take fire, and burn at the end of the pipe as long as the bladder is gently pressed to feed the flame; and when taken from the candle, after it is so lighted, it will continue burning till there is no more air left in the bladder to supply the flame.

The air when it comes out at the top of the tube, is as cold as frosty air. It is to be observed that this sort of vapour, or damp air, will not take fire except by flame; sparks do not affect it, and for that reason it is frequent to use flint and steel in places affected with this sort of damp, which will give a glimmering light, that is a great help to the workmen in difficult cases.

After the damp air was carried up in a tube, in the manner above described, the pit was no more annoyed with it, but was sunk down very successfully through the several beds of stone and coal, without any other accident, or interruption, till it came to the main seam of coals, which is 3 yards thick, and 79 fathom deep from the surface; and the said pit being oval, viz. 10 feet one way, and 8 the other, it serves both for draining the water by a fire-engine, and also for raising the coals.

An Observation of a Solar Eclipse, May 2, 1733, in the Afternoon. By Mr. George Graham, F.R.S. in Fleet-street, London. N° 429, p. 113.

Apparent time.

At 5h	44m	45s	It began.
6	25	30	The cusps were vertical.

6^h 37^m 30^s The eclipse was greatest, the lucid part of the sun's diameter measuring 426 parts, whereof the sun's diameter measured 2311. So that the eclipse was $9\frac{4}{5}$ digits.
6 46 0 The cusps were horizontal.
7 28 23 The eclipse ended.

The same Eclipse observed at Norton-Court, by Mr. Stephen Gray; and at Otterden-Place, by Granville Wheler, Esq. both in Kent. N° 429, p. 114.

At Norton-Court, near Feversham in Kent.
Apparent time.
At 5^h 49^m 15^s The beginning.
6 40 0 Greatest obscuration, $9\frac{7}{8}'$.
7 32 30 The end.

Mr. Wheler, at Otterden-Place, near Lenham in Kent, observed the beginning at 5^h 49^m 0^s, and the end at 7^h 31^m 49^s.

An Observation of the same Eclipse. By Mr. J. Milner, at Yeovil in Somersetshire. N° 429, p. 116.

The beginning at	5^h	34^m.
The end at	7	$14\frac{1}{2}$.

Some Eclipses of Jupiter's Satellites observed at Bologna. By Sig. Manfredi. N° 429, p. 117.

An Account of a remarkable Generation of Insects; also of an Earthquake; and of an Explosion in the Air. By Mr. Rd. Lewis, of Annapolis in Maryland. N° 429, p. 119.

About the latter end of June 1732, Mr. Lewis procured some leaves of the fly-tree. so called from the vast swarms of flies observed to issue from it, on which were fixed tough little bags, as large as the husk of a filbert, of a dusky green colour. On cutting them open, a fly like a gnat, comes out; and he could discover no more, till looking with a glass, he could discern something moving among the bluish pulp, and after a while observed that it contained many red grubs, very small, without wings: he bound up the nidus, and next morning the grubs had gotten bluish wings, and their body was of a grayish colour; there was a great number of them, but they soon flew away. Both the bark and leaf of the tree resembles a male mulberry. Among all the excrescences

Mr. Lewis had seen on leaves, he observed none like these. When the leaf is small, they are scarcely discernible: they grow with the leaf, which is not discoloured or crumpled by them. Redi, in his curious treatise on the Generation of Insects, gives no account of any such nests.

On Tuesday the 5th of Sept. 1732, about 11 in the morning, an earthquake was felt in divers places in Maryland. One Mr. Chew had his house shook by it for some time, and the pendulum of his clock stopped. During its continuance, a rumbling noise was heard in the air, and many people who did not feel the shaking, as well as those who did, complained of a dizziness in their heads, and sickness at their stomach. At the same time it was felt in Pennsylvania, and New England.

Mr. Lewis had the following account from Capt. Smith, of a surprising phenomenon that happened in 1725, something of the nature of the earthquake, but with some remarkable difference. Oct. 22, 1725, about 2 in the afternoon, the sky being very serene and clear, Capt. Smith heard, as he then thought, the noise of a gun, of a minion size, about 12 miles eastward from him, which noise was repeated at least 20 times, but at unequal intervals; and was soon after followed by a very loud explosion, as if a ship had been blown up. On inquiry, he was told by several persons, who lived about 12 miles distant from his house, that they were greatly amazed with the appearance of an extraordinary brightness in the zenith, resembling flame, which continued for about 5 minutes; after which the imaginary guns were fired 20 or 30 times, which so disturbed the atmosphere, that the birds lost the use of their wings, and fell to the ground in great disorder. This noise was heard about 50 miles each way, from the bright appearance aforesaid.—Thus far the Captain.

Mr. Lewis heard the noise, as most people did, but saw not the brightness at Patapsko, being about 60 miles from the Captain's house. He was told that the shock, occasioned by the noise, threw down pewter that was set to dry against the side of a house.*

Concerning some Children inoculated with the Small-pox, at Haverfordwest in Pembrokeshire. N° 429, p. 121.

The method of inoculating for the small-pox was first introduced at Haverfordwest, about the year 1722, by the ingenious and learned Dr. Perrott Williams, who then lived in this town, and had practised physic for several years in this country with eminent success; but is since removed to London. He had then his own children inoculated among some of the first on whom the

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* This phenomenon has much the appearance of a large fire-ball or meteor, and its explosions.

experiment was made; and an account of this was afterwards published in the Philos. Trans.

Some little time before Christmas 1732, the small-pox appeared in this town, chiefly of the confluent kind; some had it with purple spots, and other violent symptoms, of which several died. Towards the spring, the measles became more epidemical, and also more fatal, than the small-pox. Some of the subjects that had been visited but a little time before with the small-pox, and on their recovery had their bodies purged, yet died of the violent cough which attended and succeeded the measles, which afterwards seized them. The measles continued to rage till almost all the subjects in this place were visited with them, the small-pox continuing also during the whole time, yet making but a slow progress, and to this time (August) it has not left us.

About the end of February last, (1732) Mr. Francis Meyler inoculated his own son, near 3 years of age, from a child of about the same age, who had the distinct sort, but the pustules small. He made a slight incision on both legs which took only in one; after 4 days a pustule appeared on the part wounded, but did not much inflame it, nor make much progress. On the 7th day the child grew feverish, and on the 8th, or towards the 9th day, instead of the intended small-pox, the measles appeared all over his body, attended with a cough; at which time the feverish disorder abated, till the 11th or 12th day; he then became feverish again, and towards the 14th day the small-pox appeared, a small distinct sort, and few in number. After the eruption was full, he became hearty; and so continued, not being visited with a second fever. After this Mr. Meyler inoculated two other children from his own son, by applying the matter, after a slight incision, to both the legs of each of them, but it did not succeed. About the same time he inoculated two other children, a little way out of town, from a neighbour's child, but neither of them were infected. Its not succeeding he knows not what to impute to: whether to the slightness of the incision, or to the want of a sufficiency of matter to infect with, or to the want of a disposition in the subjects to be infected.

About the latter end of March last, (1732) Mr. Richard Wright inoculated a daughter of Tho. Kymer, Esq. of this town, between 3 and 4 years of age, from another child of about the same age, who had the distinct kind. The matter was applied to one of her arms, the incision being made pretty deep. The inflammation began about the 4th or 5th day, and afterwards appeared considerably great. She proceeded till the 7th day in a very hearty and brisk state, at which time she became heavy, sick, and very feverish. Then an eruption of the small-pox was expected; but her fever increased, and the next day there were eruptions seen all over her body, which proved to be the regular measles. She was treated accordingly, and got well; excepting a pretty severe cough she had, which continued through the whole course of the following small-pox. About the 12th day she sickened again, and about the 14th the small-pox appeared, the distinct kind, and very favourable; they came out, filled, and dried away very kindly, and were attended with very little of a second fever. She went through the distemper with a great deal of cheerfulness; she was purged afterwards, and seemed very well; but in a little time after, a boil came on the lower part of the shoulder-blade of the same arm that was inoculated, which was brought to suppurate, and was healed in the common manner.

From this subject Mr. Wright inoculated two daughters and a son of Nicholas Roch, Esq. at his seat, about 5 miles from this town. These 3 children were from 3 to 8 years of age. The incision was made in one arm of each child; it produced the same effect on every one of them as it did on Miss Kymer, viz. the measles on the 7th or 8th day, and the small-pox of the distinct sort on the 14th. They went all three very well through every stage of the distemper; the secondary fever was but slight. One of these had them somewhat thick, and the other two had a good many of them; but they all thoroughly recovered, and have all since continued in a good state of health.

Observations of the Variations of the Needle and Weather, made in a Voyage to Hudson's Bay, in the Year 1731. *By Capt. Christopher Middleton.* N° 429, p. 127.

There is nothing remarkable in this journal, or different from many other sea journals, of the daily latitudes and longitudes, heights of barometer and thermometer, the winds and weather, with the regular variation of the compass. This gradually and regularly increased on the outward voyage, beginning the account with the latitude 59° 17′, and longitude 5° 52′, where the variation was found 17° 30′; from thence increasing with the longitude, till the variation was the greatest, viz. 43°, in long. 72° and lat. 63°. After this, the variation gradually decreased as the latitude decreased, though the longitude increased, till it came down to 24°, which was in lat. 57°, and long. 85½°. After this, the variation gradually increased again, till it became 42°, viz. in lat. 63°, and long. about 70°. Lastly, it gradually decreased again, on the returning voyage, till it was 14°, viz. in lat. 49½°, and long. 5°.

Capt. Middleton remarks that, when they come in or near ice, they are obliged to keep one of the compasses continually moving, there being either some magnetic particles in the air, or some other quality that hinders them from traversing, which makes the course very difficult to direct; this happens generally in entering Hudson's straits and bay, but never so without being near or among ice. He has inquired of the commanders, and others that use Greenland and Davis's straits, and finds great complaints from them of their compasses not traversing. He has tried the needle of the azimuth compass without the chart, and finds it to traverse much better; so that he designed next voyage to have isinglass charts, as being lighter.

An Observation of a Total Eclipse of the Sun, at Gottenburg in Sweden, in Lat. 57° 40′ 54″, May 2, 1733, O. S. By M. Birger Vassen, Mathematical Lecturer there. N° 429, p. 134.

The beginning of the eclipse, which could not be observed for clouds, seems to have before 6^h 26^m afternoon.

At 6^h 49^m 52^s. . About 6 digits were eclipsed.
7 14 6 . . The planet Jupiter appeared.
7 14 46 . . The entire disk of the sun begins to be covered.
7 15 50 . . A very great darkness; and many stars appeared.
7 16 54 . . The sun began to emerge very bright.
7 41 38 . . Six digits had emerged.
8 5 50 . . The end of the eclipse.

Proposals for the Improvement of the History of Russia, by publishing, from time to time, separate Pieces to serve for a Collection of all sorts of Memoirs relating to the Transactions and State of that Nation. Printed at St. Petersburg, for the Imperial Academy of Sciences. By Ger. Fred. Muller, Prof. Hist. Petropol. and F. R. S. Translated from the German by M. Zolman. N° 429, p. 136.

An Account of an Experiment contrived by G. J. Gravesande, Prof. Math. at Leyden, F. R S. relating to the Force of moving Bodies, shown to the Royal Society. By J. T. Desaguliers, LL.D. and F. R. S. N° 429, p. 143.

Dr. D. having last year shown several persons in Holland the experiment contrived by Mr. Geo. Graham, to explain the doctrine relating to the momentum of bodies, viz. that the momentum or quantity of motion in bodies, is always as the mass multiplied into the velocity; which experiment is made with a flat, pendulous body, that receives the addition of a weight equal to itself at the lower part of its vibration, and by the reception of that equal quantity of matter always loses half its velocity. Dr. Muschenbroek, professor of mathematics and astronomy at Utrecht, communicated to him the following experiment, made in opposition to that above, by Mr. Professor Gravesande. In this last, a spring equally bent every way, pushes forward unequal quantities of matter successively, and in every experiment the product of the mass of the body by the square of the velocity is the same; and therefore, as the quantity of motion must always be the same from the same cause, viz. the same tension of the spring, it follows, by every experiment, that it is as the mass multiplied into the square of the velocity.

Exper. 1.—The pendulous cylinder is shot by the spring, from 0° to 7°, measured on a tangent line.

Exper. 2.—The cylinder with a leaden weight in it, that makes its weight double, is shot forward to $4\frac{9}{10}^\circ$.

Exper. 3.—The cylinder with a weight in it that made its weight triple, was shot forward to 4°, and a little farther.

Exper. 4.—The cylinder with a triple weight of lead, so as to quadruple the whole weight, was shot forward to $3\frac{1}{4}^\circ$.

These 4 experiments at first seem agreeable to the new hypothesis; for according to the old, the cylinder in the 2d experiment ought to have gone but to $3\frac{1}{2}^\circ$, in the 3d experiment but to $3\frac{1}{7}^\circ$; and in the last but to 2°.

But if we take in the consideration of time, all will be reduced to the old principle. As for example, let us compare the first and last experiments. In the first, the spring during a certain time acts on the cylinder, which is driven forward with the velocity 8. When the quadrupled weight is driven forward with the velocity 4 instead of 2, it is because the same spring acts twice as long on the cylinder before it ceases to impel it; and certainly the same cause acting twice as long must produce a double effect.

On Quicksilver. By Herman Boerhaave, Phil. et M. D. Professor of Physic at Leyden, &c.* N° 430, p. 145. *An Abstract from the Latin.*

In this paper Professor Boerhaave gives an account of a numerous set of experiments made upon quicksilver, with a degree of labour and perseverance rarely equalled.† From these experiments he showed, that pure quicksilver is convertible into a black powder, possessing considerable acrimony, by long continued agitation or concussion in a glass-bottle; that from this powder liquid quicksilver is again obtainable by distillation in a strong heat; that quicksilver is convertible into a similar black powder by exposure for some months to a

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* Of this celebrated professor an account has been given at p. 556, vol. v, of these Abridgments.

† In these experiments Dr. B. distilled a given quantity of quicksilver above 500 times.

degree of heat equal to 180 of Fahrenheit; that it is convertible into a red acrid substance by exposure to a higher degree of heat, from which, as in the former instance, liquid quicksilver may again be obtained by simple distillation; and that, contrary to what the alchemists have pretended, by no operation of fire is quicksilver convertible into gold or silver.*

A Spirit Level to be fixed to a Quadrant for taking a Meridional Altitude at Sea, when the Horizon is not visible. By John Hadley, Esq. V. Pr. R. S. N° 430, p. 167.

The necessity of seeing the horizon, in order to find the latitude of a ship at sea, has always been so great an inconvenience, that any method for determining it without the help of the horizon, will be of considerable use, though it should be liable to an error of a few minutes; and as it is generally agreed by seamen, that they are much oftener sensible of this inconvenience in calm weather than in rough; it is hoped that the following manner of constructing and using a spirit level, may in that case be capable of so much exactness, at least, as may render it acceptable to the public.

This level, fig. 6, pl. 15, is composed of a glass tube AB, bent into an arch of a circle, and containing such number of degrees as will be most suitable to the degree of exactness with which the observation can be made. Its bore must not be wider than the 10th of an inch in diameter, that the liquor in it may the better keep together, and its two ends stand perpendicular to the tube in all positions: nor should it be much less, lest the hanging of the spirit to the sides hinder it from settling so truly by its weight to the lowest part of the tube. This tube is cemented into another brass one CDEF, of the same curvature, the outer half of which is taken off, to show the glass, leaving only a small part in the middle DF entire, in which a small stop-cock G is placed. The glass tube is divided in two in the middle, to make room for this stop-cock, the key of which must be pierced through with a hole of only about $\frac{1}{100}$ part of an inch, for the passage of the liquor. The outer ends of the glass tube must have a communication with each other round about by means of two small pipes I and K, and the tube H, the manner of which is sufficiently shown by the figure.

Each half of the glass tube AB must have a scale of degrees, answering the curvature of the tube, subdivided at pleasure. They may be numbered either as the upper or under scale in the figure; and observe that in the under scale

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* A continuation of these experiments is inserted in the next, (i. e. the 39th,) vol of the Phil Trans.

2 degrees are numbered as one; the reason of which is, that the motion of the spirit in the tube increasing the number on one hand, and at the same time as much diminishing that on the other, their difference is thus altered, so as to answer to double that motion. The divisions of the scales are cut on the edge of the brass half tube, or trough, which is made thick for the greater strength.

In one of the small pipes I or K, just against the return of it, which enters the end of the first mentioned glass tube at A or B, is a small hole, by which to introduce into it so much spirit of wine as may fill it from the middle of the scale on one hand, to the middle of that on the other; this hole may be afterwards stopped by a screw-pin.

The inner ends of the two halves of the glass tube AB, should be fixed into the entire part of the brass tube DF, with a cement made with old hard bees-wax, or some other materials not dissolvable by spirit of wine; as should also the ends of the small pipes I and K into this and the tube H; those halves, as to the remaining part of their lengths, may be fastened down with any strong cement.

This level may be set on to one of the limbs of the quadrant, fitted up for this purpose, in the manner expressed in the figure. It has an index moveable on the centre, and a spring at the other end to keep it steady, when it is directed to any of the divisions on the arch; which needs no other division than into whole degrees. The index may be furnished either with plain sights, or may carry a short telescope, with a vane in its focus, to receive the image of the sun, when it is bright enough; but if the sun be hazy, or the moon, or a star be observed, a sliding shutter may be drawn out to transmit the rays of light to the eye-glass. The vane has also a thread fixed on it, perpendicular to the plane of the quadrant. The whole instrument, for the easier managing it, may be supported by a staff, resting with one end on the floor.

The manner of using it is thus: holding the quadrant in a vertical position, with that limb to which the level is fixed parallel to the horizon, raise the index to some division of the arch, as near as you can to the true height of the object; which is supposed to be near the meridian, and consequently to alter its altitude but slowly: then turning the key of the stop-cock, so as to let the spirit of wine pass through the small hole in it, keep the image of the object as close to the thread on the vane as you can, endeavouring that its unavoidable vibrations above and below the thread, may be equal, both in respect to their length, and the swiftness of their motions, &c. Continue this till the spirit seems quite settled to some part of the scale, and something longer. This it will do slowly, but without any sensible vibrations; for the stop-cock allowing it no passage but through the small hole in its key, will give such a check to its motions, as not only to stop those vibrations, but also to hinder its being thrown backwards and forwards in the tube by any shocks of the instrument; and yet, as far as has been observed, will not prevent its settling, with sufficient truth, though slowly, to the lowest part of the tube. About half a minute of time or more may be necessary for this, according as the aforesaid small hole is greater or less, in proportion to the bore of the tube. When you judge the spirit quite settled, turn the stop-cock again: it is of no importance that the image of the object be exactly on the thread at the instant that this is done. Observe against what degree, and part of a degree, each end of the spirit in the tube stands. If the scale be numbered like the upper one in the figure, and the quantity of spirit be exact, both ends will agree, and the degree and parts marked must be added to, or subtracted from, the altitude shown by the index, according to the directions. If the ends do not exactly agree, take the mean between them. If you use the under scale, subtract the less number from the greater, and add or subtract the excess, the number resulting will show the mean elevation of the index during the latter part of the observation, and will differ from the true altitude of the object about half as much as the vibrations of its image above and below the aforementioned thread on the vane fail of compensating one another during that time. If either end of the spirit leave the scale, the index must be removed 3 or 4 degrees, and the observation repeated.

Instead of the curve tubes A and B, two straight ones might be used, set together so as to make a very obtuse angle in the middle; but then it will be convenient to have the quantity of spirit more exactly fitted to the scale, because the allowing for the difference will be something more troublesome.

If the observer have an assistant to attend to the level, while he himself observes the object, the whole apparatus of the brass tube and stop-cock may be omitted, substituting in its stead only a plug with a small hole in it, which may be wrapped round with a very thin slice of cork, and so thrust down into the middle of the glass tube. The cutting the glass tube in half in the middle may likewise be avoided, if, instead of the stop-cock at G, there be one fixed in one or both of the pipes I and K, to open and stop the passage of the air, having a larger hole in their keys, there being also a plug, with a small hole, thrust down into the middle of the tube, as before.

The bore of the small pipes I and K, and the tube H, must not be so narrow as to make it difficult to reduce the spirit into its place, if by any accident either end of it should get into them.

Mr. Hadley has been informed, that an object may be kept in view without much difficulty, even in pretty rough weather, through a telescope magnifying about 10 times. Now as such telescopes seldom comprehend an area of much more than 1° in diameter, or at most 1° 20′, it follows, that the axis of the telescope is always kept within 40′ at most of the object, and that is the greatest vibration of the image above and below the thread on the vane. If this be allowed, it seems reasonable to expect that the medium of the vibrations one way, should not exceed the medium of those the other, more than by about $\frac{1}{5}$th or $\frac{1}{6}$th part of the greatest vibration; i. e. about 7 or 8 min. the half of which will be the error of the observation. In still weather it will probably be much less, if the instrument be in the hands of a person moderately skilful in observing.

The Dissection of a Female Beaver, and an Account of Castor found in her. By C. Mortimer, M. D. R. S. S. N° 430, p. 172.

In the Acta Erudit. for Aug. 1684, p. 360, et seq. Dr. M. remarks that there is an account of the dissection of a male and female beaver by E. G. H. who mistakes, in opening the male, the receptacles of the castor for the uterus, and the 2 glands below them for dugs; and as they found a penis and testicles in the same animal, it was concluded to be an hermaphrodite: but on dissecting the female, they found a uterus, with 2 horns like that of bitches, besides the receptacles of the castor, which he should have thought sufficient to have set the author to rights, as to the former beaver being an hermaphrodite.

Johannes Francus, a German physician, has published a Treatise, called Castorologia explicans Castoris animalis naturam et usum medico-chemicum, August. Vindel. 1685, 8vo. being a Commentary on a Treatise formerly written by one Johan. Marius, a physician at Ulm. In this, Sect. 7, Marius describes the receptacles of the castor, as being bags near as large as a goose-egg; and that they have been erroneously called the testicles, being in females as well as males, but that they have no communication with the pudenda. His commentator Francus recites the opinions of some modern writers, who are still in the old error as ancient as Ælian, who says, that the beaver bites out his own testicles, when pursued by the hunters, as if he were conscious those were the parts his persecutors want, and seek his life for. He cites Adam Zwiker, as having this notion, and likewise Joh. Harder and Joh. Schappler; nay, some have thought so absurdly, as to imagine that the beaver had 4 testicles: and he says, that Gulielmus Rondeletius was the first person who dissected a beaver with accuracy sufficient to refute the old error; showing that the castor was not the testicles, but peculiar bags lying in the groin.

Marius, Sect. 9, says, that beavers are found in the Ilera, and the Danube, particularly in a small river near Leipheim, called the Biber. The Commentator says this river has its name from the vast numbers which were formerly found thereabouts, biber being German for a beaver, but that now they are all destroyed, and none to be found in the Danube, except in Austria; that there are a few in some rivers in Switzerland, in Poland, in Muscovy, in the Wolga, in the West-Indies, especially in Canada. The greatest quantity of castor, which is brought to England, comes from Maryland, New-England, and Hudson's-Bay.

In Sect. 11, Marius speaks of a peculiar virtue in the fur of the beaver, which he had from a Jew, who informed him, that by wearing on one's head a cap made of the fur of the beaver, and by anointing the head once a month with oil of castor, and taking 2 or 3 oz. of castor in a year, one's memory will be so strengthened, as to be able to remember every thing one reads. Though this seems to be only a superstitious fancy, yet the Dr. mentions it, because probably such a notion might have at first brought the use of the flock of this animal into request for making hats.

In the Memoirs of the Academy of Sciences at Paris, for the year 1704, p. 48, is an extract of a letter from M. Sarrasin, the king's physician in Canada, concerning the dissection of the beaver. He says, the largest are 3 or 4 feet long, and about a foot or 15 inches broad in the chest, and in the hanches; that they commonly weigh about 50lb.; that they usually live to the age of 20 years; but Francus, Sect. 8, says, they live 30 or 40 years, and that he heard of a tame one being kept 78 years: perhaps the European may generally be longer lived than the American. Dr. Sarrasin says further, that a great way north these animals are very black, though there are some white ones to be seen; those in Canada are commonly brown; but their colour grows lighter, as they are found in more temperate countries; for they are yellow, and even almost of a straw-colour in the country of the Ilinois and Chaovanons. The author then gives a very particular account of the several parts, external and internal, of this animal: he takes especial notice of the stomach, which, he says, is above a foot long, and about 4 inches broad in the part next the spleen; that at about $\frac{2}{3}$ of its length, it is contracted to half its former capacity for an inch in length; that then it widens again to 3 inches towards the pylorus, which is raised very high, is round, and drawn towards the spleen by a membrane which adheres to the œsophagus by its other end. Though this dilatation seems to make a second stomach, it only serves to retain the aliment a longer time, especially the more solid, as wood, which only undergoes a slight extraction, passing through with very little alteration, whereas herbs, fruits and roots are perfectly dissolved. The membranes of the stomach are very thin, so that this second part will scarcely bear being distended with wind.

In a full grown beaver the cæcum, which is in form of a sickle, is 18 inches long on the hollow side, and 30 inches on the round side, and 4 inches broad at the larger end, and will contain between 5 and 6 pints of water. In describing the receptacles of the castor, he says, that the uppermost bags contain a soft resinous matter, but that the lower ones are filled with an oily matter; the greatest bags weigh but 2 oz.

Dr. Sarrasin says, that he was never able to discover what use this castor was of to the beavers themselves, being well assured that they do not themselves swallow it to excite their own appetite. It is likewise false, that the hunters use it as a bait to draw the beavers into their toils, though they do use it to entice those animals which infest the beavers, as martins, foxes, bears, &c.

As to their manner of living; they choose a low level ground, watered with a small rivulet, that it may be easily overflowed, which they do by making dams across it: they make these dams by thrusting down stakes of 5 or 6 feet long, and as thick as one's arm, pretty deep into the ground; these they will wattle across with tender pliable boughs, and fill up the spaces with clay, making a slope on the side against which the water presses, but leaving the other perpendicular. They make their houses after the same manner; the walls are upright, 2 feet thick, and at top in form of a doom: they are usually oval, 5 or 6 feet long on the inside, and near as broad, being sufficient to lodge 8 or 10 beavers, and 2 or 3 stories high, which they inhabit as the water rises or falls.

Sometimes they build several houses near together, which communicate with one another. He says there are some beavers called terriers, which burrow in the earth: they begin their hole at such a depth under water as they know that the water will not freeze so deep; this they carry on for 5 or 6 feet, and but just large enough for them to creep through; then they make a bathing-place 3 or 4 feet every way; from whence they continue the burrow, always ascending by stories, that they may lodge dry as the waters rise: some of these burrows have been found to be 100 feet long. They cover the places where they lie with weeds; and in winter they make chips of wood, which serve them for matelases: they live on herbs, fruits, and roots in summer; but against winter they lay up a provision of wood, a stack of 25 or 30 feet square, and 8 or 10 high, is the usual quantity for 8 or 10 beavers: they only eat those pieces which are soaked in the water. Marius says, they only live on such vegetable food; but his commentator Francus says, Sect. 4, that they prey upon fish, cray-fish, and frogs likewise, as otters do: and that they make burrows in the banks of the rivers, opening under the water.

In the Memoires pour servir à l'Histoire Naturelle des Animaux, composed by order of Louis 14, printed at Paris, 1671, in folio, at p. 64, is an anatomical description of a beaver, with a plate, in which are represented some of the most remarkable parts; as the brain, the fore-foot, the intestinum cæcum, and the parts of generation of a male beaver, with the receptacles of the castor, delineated in their natural situation. Our author says, that the real testicles resemble those of dogs; that they lie close to the os pubis, on the outward part of the sides, and that they are not at all discernible through the skin. The penis had a sharp-pointed bone, in its extremity, like that of a dog; but instead of lying with its point towards the navel of the creature, it lay with it towards the tail, and was so deep buried in the fissure, which serves in common for the anus, for the penis, and the excretory ducts of the castor, that they could not distinguish what sex the beaver was of, till the skin was taken off.

Our author says, that in opening the intestines, they found in them 8 large worms, resembling common earth-worms, 3 of which were 7 or 8 inches long, the rest only 4. In the heart were the plain traces of the foramen ovale. A little below the coronary-vein, he mentions a valve, which he says is called valvula nobilis, and closes the whole vena cava, but opens so that the blood can flow readily from the liver towards the heart, and not from the heart back again towards the liver.

This author says, that the brain was but $1\frac{2}{3}$ inch long, and $1\frac{1}{2}$ broad, which was very small in proportion to the size of the creature; and still more so in proportion to the sagacity with which it is said he is endowed.

These are the most remarkable particulars met with in reading over the above-mentioned books. Dr. M. then adds such as they have passed over, or what especially regard the sex of this female beaver.

This creature was kept at Sir Hans Sloane's, in his garden, for about 3 months. She was but about half grown, not being above 22 inches long from the nose to the root of the tail; the tail 8 inches long. She was very thick, paunch-bellied; the shape of the head, and indeed of the whole animal, except the tail, and hind feet, very much resembled a great over-grown water-rat.

Her food was bread and water; some willow-boughs were given her, of which she eat but little; but when she was loose in the garden, she seemed to like the vines much, having gnawn several of them as high as she could reach quite down to the roots: she gnawed the jessamy likewise, but least of all some holly trees. In Carolina it is said they particularly like the sassafras, and will cut down trees of between 2 and 3 feet diameter. She was turned into a fountain with some live flounders, but never offered to strike at them, as an otter would have done. When she eat, she always sat on her hind legs, and held the bread in her paws like a squirrel. When she slept, she commonly lay on her belly, with her tail under her. In swimming, she held her fore-feet close up under her throat, and the claws closed, as when one brings the ends of one's thumb and of all the fingers close together, never moving her fore-feet till she came to the side, and endeavoured to get out. She swam with her hind-feet only, which had five toes, and were webbed like those of a goose; the tail, which was scaly, and in form of the blade of an oar, served as a rudder, with which she steered herself, especially when she swam under water, which she would do for 2 or 3 minutes, and then come up to breathe, sometimes raising her nostrils only above water: she swam much swifter than any water-fowl, moving under water as swift as a carp. The hind legs being much longer than the fore, made her walk but slowly, or rather waddle like a duck when on dry land; and if driven along fast, she could not run, but went by jumps, flapping her tail against the ground. Her excrements were always black, and very fetid; her urine turbid and whitish, and very strong scented. She made no noise, except a little kind of grunting, when driven fast and angred. She seemed very brisk, and thrived well with the abovementioned food, being turned into the fountain to bathe 3 or 4 times a week. But the author of the Memoires de l'Histoire des Animaux, above-cited, says, that the male beaver they dissected, had lived several years at Versailles without being permitted to go into the water. Our beaver had one day convulsion fits, very like the epilepsy in men, from which she recovered soon, and was very well after them, till at last she was killed by a dog; when she was so torn, that we could see nothing particular in the heart, or in the lungs. In the abdomen the liver and kidneys were quite torn in pieces. There were several holes bit through the stomach, out of one of which crawled a worm about 6 or 7 inches long, like a common earth-worm, being probably of the same sort as those mentioned before by the author of the Memoires, &c. The bowels in general seemed very much to resemble those of dogs, except the intestinum cæcum, which was of that prodigious size as abovementioned. This creature being a female, they found the ovaria and the uterus divided into 2 horns, in the same situation as in bitches: the bladder was contracted about the size of a walnut, very much wrinkled on the outside; it lay exactly over the body of the uterus; the meatus urinarius ran upon the vagina above 2 inches in length. Just below the os pubis, on each side of the vagina, and above the meatus urinarius, supposing the animal to lie on her back, as when they opened her, they found a pair of bags in form of pears, about 1 inch and ¾ long, and 1 inch broad, diverging at their bottoms, or broad ends, but joined almost close together at their necks, or narrow ends, which were canals communicating with the adjoining glands. The membranes which formed these bags were very tough, full of wrinkles and furrows, and of a livid dirty colour; they were hollow, and able to contain about 1 oz. of water. On opening one of them, they found a small quantity of a dark brown liquor like tar, of the consistence of a thick syrup, which smelt exactly like castor, and had a sort of pungency, like spirit of hartshorn, which the dried castor does not retain. It is very probable that the youth of this animal was the reason why these bags were not full; and that the castor itself was not of that soft resinous consistence as mentioned by Dr. Sarrasin, loc. citat. These must be the bags mistaken in the Act. Eruditor. for the uterus. About 1 inch lower were situated a pair of glandular bodies, one on each side the vagina, about 1½ inch in length, and ½ inch in breadth: they were of an oblong irregular shape, of a pale flesh-colour, like the pancreas, or other glands, and having several protuberances outwardly. These glands seem to communicate with the above-described bags, the canals coming down from them being implanted into the glands, and both the bag and gland on each side has but one orifice, which is black, beset with long black hairs, and opens into the lower part of the rima, or great fissure, into which likewise open the vagina and the anus. From the structure of these glands, and their connection with the bags, he concludes that the castor is secreted in these glands, where it is fluid like oil, light-coloured, and hardly having any smell; that it runs down into the bags, which serve as receptacles to collect a large quantity together for the use of the beaver, and that in these receptacles it loses its thinner parts, becomes more inspissated, of a higher colour, and of a stronger scent, much in the same manner as the gall in the gall-bladder, which there becomes so different from what it was in the liver.

It is certain that ducks, geese, and all sorts of water-fowl, have a gland in their rump, from which they express with their bill an oily matter, and with it anoint or dress their feathers, to prevent their being soaked by the water in which they swim; and the glands of that large sort of duck commonly called the muscovy-duck, or more properly the musk-duck, afford such an oil, as sweet-scented as civet: he therefore thinks it probable, as the beaver is an animal which frequents the water as much as those water-fowls, that the castor is a substance provided by nature for him to grease and anoint his fur with, to prevent the water from soaking quite to his skin: and as the castor is impregnated with penetrating pungent particles, it may likewise contribute to keep off the cold and chill, which the water might otherwise strike to his body, by remaining a long time in it.

As none of the authors Dr. M. has met with have given any delineation of the parts of generation, or of the receptacles of the castor in a female beaver, he has drawn them after nature, as they are represented in fig. 7, pl. 15; where A shows the 2 ureters; BB the ovaria; C the uterus lying under the bladder; D the bladder, contracted and empty of urine; E the meatus urinarius, above 2 inches long; FF the receptacles, containing the castor; GG the 2 glandules, which open by one common orifice, with the receptacles, at HH the orifices of the castor-ducts; I the vagina cut off; K the anus; L part of the tail.

A Natural History of the Air and Earth, for the Year 1732. *By Dr. Cyrillus, Profes. Med. in the University of Naples, and F. R. S.* N° 430, p. 184.

A kind of meteorological journal, of no use now.

An Account of a Book entitled, Jo. Ph. Breynij, M. D. &c. Dissertatio Physica de Polythalamiis, nova Testaceorum classe, &c. Gedani, 1732, *4to. or a Physical Dissertation of a new Class of Shells, which he stiles Polythalamiums, &c. with* 14 *Copper Plates. By Richard Middleton Massey, M. D. F. R. S.* N° 430, p. 191.

In the first chapter the author discourses of shells in general, and premises a method of placing them in different classes, which he reduces to 8, viz. tubulus, cochlidium, polythalamium, lepas, concha, conchoides, balanus, and echinus.

Chap. 2, treats of polythalamiums, which he defines a tubulous shell divided into several cavities, conical, straight, or regularly spiral, with a pipe, or canal, passing through each cavity. This again he subdivides into 4, viz. 1. orthoceras, 2. lituus, 3. ammonia, and 4. nautilus.

Chap. 3, treats of the nautilus and nautilites, which last he takes to be a stone formed under ground in the cavities of the nautilus.

Chap. 4, is of the ammonia and ammonites.

Chap. 5, is of the lituus (which he names from some resemblance it has to the lituus, or crosier, which the antient Roman Augurs used in their ceremonies) and the lituites, or stone formed in its cavities under ground. The shell is yet unknown; but of the stone he has given a curious draught, as it appears in a marble which was brought from Oelandt, an island of Sweden.

Chap. 6, is of the orthoceras and orthoceratites, or stony concretion in its cavity. Of these last stones, he produces 9 different species, which he distinguishes chiefly by the pipe, or canal, which runs through them.

In his note concerning the belemnites prusscici, of which he describes two species, he remarks that the stony cone, or nucleus of it, is never found articulate, as in those that come from Sweden, and some other countries.

At the latter end of his book he proposes a methodical distribution of the Echini and Echinites, or stones that are generated under ground in the cavities of the Echini.

The whole method he proposes for ranging shells in general, may be seen in the following Table.

Tabula Methodica Testaceorum.

TESTA	Tubulosa	Monothalamia	*Tubulus,*	Dentalia, entalia, solen-univalvis, — unde belemnites pruss.
			Cochlidium,	Nautilus tenuis, auris marina, nerita, cochlea, buccinum, murex, cassis, cylindrum, voluta, porcellana.
		Polythalamia	*Polythalamium,*	Nautilus, ammonia, lituus, orthocerata.
	Vasculosa	Simplex	*Lepas,*	Patella.
			Concha,	Chama, mytulus, tellen, pinna, ostrea, pecten, anomia-
		Composita	*Conchoides,*	Pholas, concha anatifera.
			Balanus,	Balanus.
			Echinus,	Echinometra, echinoconus, echinocorys, echinanthos, echinospatagus, echinobrissus, echinodiscus.

An Account of a Book entitled, Osteographia, or, the Anatomy of the Bones. By Wm. Cheselden, F.R.S. By John Belchier, F.R.S. N° 430, p. 194.

Of this work mention has been before made in the biographical account of the author, at p. 672, vol. 5, of these Abridgments. In delineating the bones, Mr. Cheselden made use of a Camera Obscura, somewhat different in its construction from that in common use, and of which he gives a description. By this he informs us, he was not only enabled to give the true proportions and outlines of the bones, but by the help thereof to do more in 1 day than could possibly be done without it in several days.

A Catalogue of the Fifty Plants, from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1732 ; *pursuant to the Direction of Sir Hans Sloane, Bart. Med. Reg. Præs. Col. Reg. Med. et Soc. Reg. By Isaac Rand, Apothecary, F. R. S.* N° 431, p. 199.

This is the 11th present of this sort, completing 550 plants.

On Camphor extracted from Thyme. By Caspar Neuman, M. D. Professor of Chemistry at Berlin, and F.R.S. An Abstract from the Latin. N° 431, p. 202.

This paper contains a reply to Mr. Brown's objections* to the propriety of calling the product obtained by Mr. N. in the distillation of thyme, the camphor of thyme. Mr. Brown rather considers it to be a coagulated or condensed oil, differing from the true oriental camphor in several of its chemical properties. Dr. Neuman in his reply observes, that as it coincides in its leading properties with camphor, and has a nearer affinity to that than to any other known substance, he thought himself justified in calling it a species of camphor. He further remarks that this product is not of a greasy or butyraceous quality, but that it appears under a dry crystallized form, and cannot therefore with any degree of propriety be termed an oil. The oils of aniseed, of olives, &c. howsoever congealed by the winter cold, are nevertheless found to have the feel and consistence of butter or fat when rubbed between the fingers, and never become dry and hard like vitriolated tartar or sugar-candy. Dr. N. employs the word camphor as a generic term ; and conceives it to be no objection to considering this product of thyme as a species of camphor, because with some chemical agents the results obtained from it and he oriental camphor are somewhat different. Gold and silver (he observes) are both metals, although they are not both acted upon in the same manner by the same acid menstruums.

The Settling of a new Genus of Plants, called after the Malayans, Mangostans. By Laurentius Garcin, M. D. and F. R. S. Translated from the French by Mr. Zollman, F. R. S. N° 431, p. 232.

The mangostans is a kind of pomiferous tree,† growing in the Molucca Islands, the fruit of which is one of the best in the world for eating.

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* Vol. vii. 103 of these Abridgments. Mr. Neuman's former observations on this subject are inserted in Vol. vii. p. 94 of these Abridgments.

† The tree here described is the *garcinia mangostana* of Linnæus, the genus being so named in honour of Dr. Garcin, who travelled into the East Indies.

Its Character.—This genus has its flower complete, tetrapetalous, regular, hermaphrodite, containing the ovary. Its calix is monopetalous, divided into 4 lobes, roundish on the edges, and hollowed in the shape of a spoon. The ovary is nearly cylindrical, with a tube on it, cut out in the shape of a rose, which covers it like a little cap. The stamina which surround it, are spherical at the top, and their number is four times that of the petala. When these are gone off, the pistil changes into a round fruit, adorned with its calix, and its tube, cut into the shape of a star with rays squared at the corners. Its cortex, which is thick and brittle, encloses a cavity, filled with as many pulpous and juicy segments, as there are rays in the tube. These segments are white, in the shape of a half-moon, sticking together, and containing each but one grain of seed; which latter is oblong, something flattened, resembling an almond, wrapped up in a tunica, which is covered with a hairy coat of fibres or vessels, which together with the pulp make up the parenchyma of a segment of the fruit. The leaves of the tree are entire, smooth like those of the laurel, and grow opposite to each other on the branches. The stem of the tree grows up straight to the top of its tuft, and its branches and twigs come out opposite to each other like the leaves.

Dr. G. mentions only one species of this genus, which admits indeed of some variation, but without any other mark than what appears in the fruit.

Mangostans garciæ, Clus. Bont. arbor peregrina aurantio simili fructu. Clus. exot. 12. Laurifolia javanensis C. B. Pin. 461.

Its Description.—The mangostans is a tree of a very moderate size, rising to about 18 feet high. Its stem runs up straight to the top of its tuft, like the fir. This tuft is regular, in form of an oblong cone, composed of many branches and twigs, spreading out equally on all sides, without leaving any hollow.

The stem grows at bottom to the thickness of a man's thigh, or about 8 or 10 inches in diameter; it afterwards diminishes in thickness by degrees up to the tuft. Its wood is white, as long as the tree is growing, but brownish when the tree is cut down and dry. Its bark is a little tender, and separates easily from the wood; it is of a dark grey colour, and slit, or full of cracks up the stem, but on the twigs it is more even, and greener, resembling that of evonymus, or spindle-tree.

The branches grow out of them by stories, and opposite to each other; those stories cross each other obliquely, and not at right angles. The thickness of those branches is always proportionable to that of the stem at the place where they come out of it: this proportion is about 1 to 4, or 1 to 5. The length of the inferior branches of the tuft is 5 or 6 feet, the others shorten as they come near the top. The distances of the stories of the branches are a little unequal; but where they are widest, they do not exceed the length of the greatest leaves, that is, 8 or 9 inches.

The twigs grow on the branches in the same order as those do on the stem, that is, opposite to each other. The longest are commonly of the length from one's hand to the elbow. The greater twigs grow out to a certain distance from the stem; and the others, which garnish the rest of the branches, always grow less and less towards their extremity.

The branches and twigs never divide themselves.

The leaves are large, entire, beautiful, smooth, of a shining green on the upper side, and of an olive colour on the back, pointed at their extremities. The rib, which divides its extent into two equal parts, is straight, and equally prominent on both sides. From the sides of this rib there issue forth fibres pretty small, and almost by pairs, which extend themselves in parallels, and bend a little archwise quite to the edge of the leaf, where they unite themselves into a thread, which forms there a kind of margin. The meshes, or filaments of the net, are not very perceptible. The size of these leaves varies; the largest are 8 or 9 inches long, but commonly 7. The breadth of each leaf is near equal to half its length, which proportion is always the same in every leaf. Their pedicles are thick, short, and wrinkled, flat on the inside, and raised in the shape of an ass's back on the outside, most frequently half an inch long. They come out near and on the extremities of the twigs, opposite to each other, like the branches themselves. There appear seldom above two pairs of leaves on each twig; and those that shoot out last, always make up the extremity of that twig.

The flower is 2 inches in diameter, pretty much like a single rose. It is composed of 4 petala, almost round, or a little pointed, about an inch broad, very thick, firm, fleshy, brittle, and somewhat hollowed into the shape of a spoon. Their greatest thickness is near their basis, of above a line, which decreases gradually towards the extremity. They entirely resemble the petal of a rose; except that instead of being indented like a heart, they end gradually in roundish points. Their colour is also like that of a rose, except that it is deeper and less lively. The basis, which is the thickest and firmest part of it, is the whitest, and the most brittle.

The pistil, or ovary, is a round or almost cylindrical body, 5 lines thick, raised to the height of 4. The upper part of this pistil, viz. its tube, is cut in the shape of a small rose, covering the ovary like a cap. The diameter of this cap is of an equal breadth with the ovary, which it covers entirely, sticking very close to it. The colour of the ovary is a pale or whitish green, and that of the tube a sullied or dirty white.

The stamina rise from the base of the pistil: they are whitish, round at the tops, and raised to the circumference of the tube, applying themselves to the ovary. They are 16 in number; 4 for each petal.

The calix is of one piece, expanded and cut into 4 lobes, down to its basis. These lobes are thick, round, skinny, hollowed in the manner of a spoon, resembling also petala of roses not fully blown. They seem to cross each other like the petala. The two upper lobes are something larger than the lower ones; they are greenish on the outside, and of a fine deep red within, which makes them more agreeable to the eye than the petala; the red of the upper ones is more lively than that of the lower ones. All these lobes in short are hollower than the petala; they do not cover those latter farther than half way their height. This calix encloses all the parts of the flower. It is supported by a pedicle of 7 or 8 lines long, its thickness being commonly of one third of its length. This pedicle is green, and constantly comes out of the end of a twig, above the last pair of leaves.

The fruit is round, of the size of a middling orange: it varies however very much, from one inch and a half to two inches and a half in diameter. The top of it is covered with a sort of cap embossed, cut out in the shape of a rose, or a star with rays squared off, of a finger's breadth, or sometimes of an inch in diameter. The rays of this little rose are most frequently 6 or 7 in number, but seldom of 5 or 8. These rays, by being thus squared, form together a kind of polygon: this is the part which had served for the tube to the ovary.

The body of this fruit is a capsula of one cavity, composed of a thick shell, brittle, a little like that of a pomegranate, but softer, thicker, and fuller of juice. It is commonly 3 lines in thickness: its outer colour is of a dark-brown purple, mixed with a little grey and dark green; the inner colour, viz. on the inside of the case, is of a rose colour. Its juice is purple. Finally, this skin is of a styptic, or astringent taste, like that of the pomegranate; and it does not stick to the parts of the fruit it contains. The inner part of this fruit is a furrowed globe divided into segments, much like those in an orange, but unequal in size, which do not adhere to each other. The number of these segments is always equal to that of the rays of the tube which covers the fruit. The fewer there are of these segments, the larger they are. There are often in the same fruit segments as large again as any of those that are on their side.

These segments are white, a little transparent, fleshy, membranous, fibrous, full of juice like cherries or raspberries, of a taste of strawberries and grapes together. Each of the largest segments encloses a grain of seed, of the figure and size of an almond stripped of its shell, having a protuberance on one side, which is nothing else but its navel. This grain is covered with two small skins, the outermost of which serves for a basis to the filaments and membranes ot which the pulp is composed. The substance of these grains comes very near to that of chestnuts, as to their consistency, colour, and astringent quality. The calix always remains adhering to the fruit, to which it serves for an ornament, and when half dried up, it is of the colour of the pomegranate shell on the outside. It covers about a 6th part of the circumference of the fruit.

Remarks.—Garcias, Clusius, and Bontius, are the first authors who have mentioned the mangostans; but they have left us only indifferent descriptions, and those so short, that it is not possible to form from them a sufficient idea for discovering its characters. The first of those authors was ill informed, when he was told the fruit was yellow. Clusius has spoken of it under two different names, without apprehending that it was one and the same plant. The figure which he has given of the fruit, and which he calls arbor peregrina aurantio simili fructu, though ill done, yet represents it enough to know it again. If in that figure the fruit appears little in regard to the twig which supports it, this can be for no other reason, but because he received from the Indies some of that fruit which had been gathered before its state of perfection, from which he drew his figure. And hence it is, that the fruit being shrunk up and imperfect, he found nothing in it but a few shrivelled grains, not much larger than those of a fig.

It is surprising however, that the most delicious fruit of all the Indies, and which yields to none of the best in Europe, is that which of all has been hitherto least known. But as I have often eaten of it, and found it as excellent as it is reputed in the countries where it is cultivated, I resolved to examine its genus, to settle its characters, and to give a description of it, which might make it better known for the future to botanists, and other curious persons.

This tree originally grows in the Molucca islands; but for some years past it has been transplanted into the Isle of Java, and some few at Malacca, in which places it thrives very well. Its tuft is so fine, so regular, so equal, and the appearance of its leaves so beautiful, that it is at present considered at Batavia as the most proper for adorning a garden, and affording an agreeable shade; yet there have been but few Europeans in the Indies who have used it for this purpose, because they were unacquainted with it. They employed other trees, which did not near come up to it for usefulness and beauty.

Travellers, who mention its fruit, always speak of it with great encomiums. Linschooten is the only one who, after having given a description of several Indian fruits in his own way, thought it needless to describe the mangostans, as well as some others, because, says he, they are little valued. Probably he never saw it, but on inquiry took upon credit what some person or other told him, who knew nothing of it besides the name, and confounded it with others which are little esteemed.

There are few grains to be met with in this fruit that are good for planting, for most of them are but abortive.

Sometimes this fruit is found spoiled within, which may be known by yellow spots appearing on some of the segments. Some people scruple then to eat them; but others make no difficulty about it. It is certain however, that they are not so good, especially if the spots are considerable. I observed that this corruption proceeded from the juice in the capsula, which being spoiled by the sting of some insect, and thereby becoming yellow, and spreading over the segments of the fruit, infected them with that colour, and so changed them. This wound is so small, and so hard to be discovered, that one often is left in a doubt whether there be any at all.

One may eat a great deal of this fruit without any inconvenience; and it is the only one which sick people may be allowed to eat without any scruple. It is very wholesome, refreshing, and more cordial than the strawberry.

Its shell has the same virtue as that of the pomegranate; at Batavia they make an infusion and a tincture of it, against loosenesses, and chiefly against dysenteries. The wood is good for nothing but firing.

In the Memoires de Mathematique et de Physique de l'Academie Royale des Sciences de Paris, of the year 1692, there is a short description of the mangostans by Father Beze, which is pretty good; but as he took the calix for the flower, it is plain he observed it not till after the petala were fallen off. His description is too short and defective, for determining from thence alone the true characters of this genus.

Explanation of the Figures.—Fig. 1, pl. 16, the flower, as it appears in the inside and outside: a the four petala of the flower; b the four lobes of the calix; c the tube; d the pedicle.

Fig. 2, the calix, as it appears in the inside with the pistil and the stamina: e the end of the pedicle of the flower, which supports the calix.

Fig. 3, a petal, as it appears on the back, separated from the flower: f its basis, which is the thickest, the firmest and the most brittle part; g four stamina belonging to the petal, arising from the basis of it, and of the pistil.

Fig. 4, the entire fruit, seen from the side of the calix or the pedicle: h the calix; i the pedicle; k a part of its tube.

Fig. 5, the same, seen from the side of the tube, which is cut out in the shape of a small rose: l the tube, which always sticks fast to the fruit; m the pedicle, and part of the calix.

Fig. 6, the fruit cut into two halfs, containing 6 segments: n the segments good to eat, of unequal sizes; o the calix; p the pedicle.

Fig. 7, a separate segment of the fruit, in the shape of a half-moon, containing a grain.

Fig. 8, a grain or seed separated from the segment, its coat covered with filaments, which formed the parenchyma of the segment.

Fig. 9, a leaf of the tree which bears the mangostans, with its fellow cut off near the bottom, supported by a piece of its twig.

An Account by Mr. John Eames, F. R. S. of a Book entitled, Traite Physique et Historique De L' Aurore Boreale, Par Mr. De Mairan. Suite des Memoires de l'Academie Royale des Sciences, Année* 1731: *or, a Philosophical and Historical Treatise concerning the Aurora Borealis. By Mr. De Mairan, being a Supplement to the Memoires of the Academy of Sciences for the Year* 1731. N° 431, p. 243.

The frequent appearances of the northern lights in several parts of Europe and America, and the surprising beautiful phænomena that have been observed in some of them, such as the rainbow-colours, canopy, &c. have very justly engaged the philosophers of the present age in a search after their causes; and several hypotheses have been invented and proposed by the learned, to explain them. Most of them suppose these phosphorus like appearances to proceed from certain effluvia, either perspired out of the earth, or at least passing through it. But our ingenious author has thought of a cause very distant, as well as very different from all these, viz. the atmosphere of the sun, which at some times shows itself under the appearance of a light, which he calls the zodiacal light, but at other times produces an aurora borealis. The zodiacal light is the purer unmixed atmosphere of the sun; but an aurora borealis is the effect of the solar atmosphere, consequent on its making a descent into, and blending itself with the atmosphere of the earth, at certain times and seasons of the year.

The work consists of 5 sections; the first gives a short history of the zodiacal light. In the 2d he treats at large of the atmosphere of the earth; its altitude, and the height of the aurora borealis in it, and the exclusion this circumstance gives to some of the causes, which have been already assigned, of this phenomenon. In the 3d he proposes the cause, and accounts for the formation of this appearance in general, and then descends to a detail of the several particulars, adding the solution of each. The next section is employed

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* M. de Mairan was author of various philosophical treatises, viz. Dissert. de la Glace; Dissert. sur les Phosphores; Traitè de l'Aurore boreale, &c. He succeeded Fontenelle in the office of secretary to the Parisian Acad. of Sciences, and wrote a vol. of Eloges of deceased members. A number of communications of his are inserted in the Memoirs of the aforesaid Academy. He died in 1771, aged 93.

in relating the historical proofs of his hypothesis concerning the northern lights, taken from the records we have of several appearances of those lights, to be met with in ancient authors, compared with those of the zodiacal light, their supposed cause, and the situation of the earth in her annual orbit at those times. The last section consists of 28 curious questions, concerning several other phænomena of nature, which the ingenious theorist believes to have a dependance on his new hypothesis, and explicable by it.

The cause of an aurora borealis, in general, the author takes to be a light called the zodiacal light, which is in reality nothing else but the atmosphere of the sun, spread on each side of him along the zodiac, in the form of a pyramid. This sometimes is extended to such a length as to reach beyond the annual orbit of our earth, and in these circumstances sometimes to blend itself with our atmosphere, and being of an heterogeneous nature, produces the several appearances which are observed in, and usually compose the northern lights. This he undertakes to explain. and prove more largely, in the sequel of the work.

*On Electricity. By Mons. Du Fay,** *F. R. S.* N° 431, p. 258.

The writings of Mr. Gray and Mr. Hauksbee, it seems first put M. Du Fay on the subject of electricity, and furnished him with the hints that led to the following discoveries.

1. He found, that all bodies, (metallic, soft, or fluid ones excepted) may be made electric, by first heating them more or less, and then rubbing them on any sort of cloth. So that all kinds of stones, as well precious as common, all sorts of wood, and in general every thing that he tried became electric, by heating and rubbing, except such bodies as grow soft by heat, as the gums, which dissolve in water, glue, and such other substances. It is also to be remarked, that the hardest stones and marbles require more chafing or heating than others, and that the same rule obtains with regard to the woods; so that box, lignum vitæ, &c. must be chafed almost to the degree of burning; whereas fir, lime-tree, and cork, require but a moderate heat.

2. Having read, in one of Mr. Gray's Letters, Phil. Trans. N° 422, that water may be made electrical, by holding the excited glass tube near it, M. Du Fay found on trial, that the same happened to all bodies without exception, whether solid or fluid; and that for that purpose it was sufficient to set them on a glass-stand slightly warmed, or only dried; and then by bringing the tube

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* M. du Fay was a French officer; but after some years service, he quitted the army and devoted himself wholly to scientific pursuits. He was an active member of the Parisian Acad. of Sciences, and had the superintendance of the Royal Botanic Garden at Paris. He died of the small-pox in the 43d or 44th year of his age. His life is in the collection of Eloges by Fontenelle.

near them, they immediately became electrical. He made this experiment with ice, with a lighted wood-coal, and with every thing that came into his mind: and he constantly remarked, that such bodies as of themselves were least electrical, had the greatest degree of electricity communicated to them at the approach of the glass tube.

3. Mr. Gray says, Phil. Trans. N° 417, that bodies attract more or less according to their colours. This led M. Du Fay to make several very singular experiments. He took 9 silk ribbons of equal size, one white, one black, and the other 7 of the 7 primitive colours, and having hung them all in order on the same line, and then bringing the tube near them, the black one was first attracted, the white one next, and the others in order successively to the red one, which was attracted least, and the last of all. He afterwards cut out 9 square pieces of gause, of the same colours with the ribbons, and having put them one after another on a hoop of wood, with leaf-gold under them, the leaf-gold was attracted through all the coloured pieces of gause, but not through the white or black. This inclined him at first to think, that the colours contributed much to electricity. But 3 experiments convinced him of the contrary: the first, that by warming the pieces of gause, neither the black nor white pieces obstructed the action of the electrical tube more than those of the other colours. In like manner, the ribbons being warmed, the black and white are not more strongly attracted than the rest. The second is, the gauses and ribbons being wetted, the ribbons are all attracted equally, and all the pieces of gause equally intercept the action of electric bodies. The third is, that the colours of a prism being thrown on a piece of white gause, there appear no differences of attraction. Whence it follows, that this difference proceeds not from the colour, as a colour, but from the substances that are employed in the dying. For when he coloured ribbons, by rubbing them with charcoal, carmine, and such other substances, the differences no longer proved the same.

4. Having communicated the electricity of the tube by means of a packthread, after Mr. Gray's manner, he observed, that the experiment succeeded the better for wetting the line; and that it may be supported on glass-tubes instead of silk-lines. And he made this experiment at 1256 feet distance, in a garden, though the wind was high, and though the line made 8 returns, and passed through two different walks. By means of two silk loops he adjusted two lines in such a manner, that their ends were but a foot distance from each other, and he remarked that the electric virtue was still communicated. He has since that seen, in the Philos. Trans. N° 426, that Mr. Gray had the same thought, and that he had done the same with rods. This experiment put him on placing several different bodies between the two lines, to examine which diminished or intercepted the electricity, and which gave no obstruction to it.

5. He suspended a child on silk lines, and made all the surprising experiments described by Mr. Gray, Philos. Trans. N° 417. But having tried the experiment on his own body in the same manner, he observed several things very remarkable. First, when he takes the paste-board or stand, on which the leaf-gold is laid, into his hand, neither his other hand nor his face has any attraction. But if another person, who is in the chamber, comes near him, he will attract it with his face, his hand, or even with a stick. 2. While he is suspended on the lines, if the electric tube be put near one of his hands, or his legs, and then if another person approach, and pass his hand within an inch or thereabouts of his face, legs, hand, or clothes, there immediately issues from his body one or more pricking shoots, with a crackling noise, that causes to that person, as well as to M. Du Fay a little pain, resembling that from the sudden prick of a pin, or the burning from a spark of fire, which is as sensibly felt through the clothes, as on the bare hand or face. And in the dark these snappings are, as may be easily imagined, so many sparks of fire. These snappings, or sparks, are not excited, if a bit of wood, cloth, or any other substance than a living body, be passed over the person suspended on the lines, unless it be a piece of metal, which produces very nearly the same effect. Any other living animal does the same, if put on the lines, and that first the tube, and then the hand be applied near it: but it is otherwise, if the experiment be made with the carcase of an animal; for then one perceives only, if it be in the dark, a still uniform light, without snappings or sparks.

6. On making the experiment related by Otho de Guerik, in his collection of experiments de Spatio Vacuo, which consists in making a ball of sulphur rendered electrical, to repel a down-feather, M. Du Fay perceived that the same effects were produced not only by the tube, but by all electric bodies whatever; and he discovered a very simple principle, which accounts for a great part of the irregularities, and if he may use the term, of the caprices that seem to accompany most of the experiments on electricity. This principle is, that electric bodies attract all those that are not so, and repel them as soon as they are become electric, by the vicinity or contact of the electric body. Thus leaf-gold is first attracted by the tube; and acquires an electricity by approaching it; and of consequence is immediately repelled by it. Nor is it re-attracted, while it retains its electric quality. But if, while it is thus sustained in the air, it chance to light on some other body, it presently loses its electricity; and consequently is re-attracted by the tube, which, after having given it a new electricity, repels it a second time; which continues as long as the tube keeps its electricity. On applying this principle to the various experiments of electricity, one is surprised at the number of obscure and puzzling facts it clears up. For Mr. Hauksbee's famous experiment of the glass globe, in which silk threads

are put, is a necessary consequence of it. When these threads are ranged in form of rays by the electricity of the sides of the globe, if the finger be put near the outside of the globe, the silk threads within fly from it, as is well known; which happens only because the finger, or any other body applied near the glass globe, is thereby rendered electrical, and consequently repels the silk threads, which are endowed with the like quality. With a little reflection one may in the same manner account for most of the other phænomena, and which seem inexplicable, without attending to this principle.

7. Chance threw in M. Du Fay's way another principle, more universal and remarkable than the preceding one, and which casts a new light on the subject of electricity. This principle is, that there are two distinct electricities, very different from each other; one of which he calls vitreous electricity, and the other resinous electricity. The first is that of glass, rock-crystal, precious stones, hair of animals, wool, and many other bodies: the second is that of amber, copal, gum-lac, silk, thread, paper, and a vast number of other substances. The characteristic of these two electricities is, that a body of the vitreous electricity, for example, repels all such as are of the same electricity; and on the contrary, attracts all those of the resinous electricity; so that the tube, made electrical, will repel glass, crystal, hair of animals, &c. when rendered electric, and will attract silk, thread, paper, &c. though rendered electrical likewise. Amber, on the contrary, will attract electric glass, and other substances of the same glass, and will repel gum-lac, copal, silk, thread, &c. Two silk ribbons rendered electrical, will repel each other; two woollen threads will do the like; but a woollen thread and a silk thread will mutually attract each other. This principle very naturally explains, why the ends of threads, of silk, or wool, recede from each other in form of a pencil or broom, when they have acquired an electric quality. From this principle we may with the same ease deduce the explanation of a great number of other phænomena. And it is probable that this truth will lead us to the further discovery of many other things.

In order to know immediately, to which of the two classes of electricity any body belongs, we need only render electrical a silk thread, which is known to be of the resinous electricity, and see whether that body, rendered electrical, attracts or repels it. If it attracts, it is certainly of that kind of electricity called vitreous; if on the contrary it repels, it is of the same kind of electricity with the silk, that is, of the resinous. M. Du Fay also observed that communicated electricity retains the same properties: for if a ball of ivory, or wood, be set on a glass stand, and this ball be rendered electric by the tube, it will repel all such substances as the tube repels; but if it be rendered electric by applying a cylinder of gum-lac near it, it will produce quite contrary effects, viz. precisely the same as gum-lac would produce. In order to succeed in these experiments, it is requisite that the two bodies, which are put near each other to find out the nature of their electricity, be rendered as electrical as possible; for if one of them was not at all, or but weakly electrical, it would be attracted by the other, though it be of that sort, that should naturally be repelled by it. But the experiment will always succeed perfectly well, if both the bodies be sufficiently electrical.

Experiments and Observations on Bulbous Roots, Plants, and Seeds growing in Water. By Mr. William Curteis. N° 432, p. 267.

The art of raising hyacinths and many other plants in water is now so well understood, that the present paper is of no other consequence than merely as a kind of memorandum, marking the period at which this practice became common in England.

A Catalogue of the Eclipses of Jupiter's Satellites, expected to happen in the Year 1735. *By James Hodgson, F. R. S.* N° 432, p. 279.

*The Case of a Man who was poisoned by eating Monkshood or Napellus.** *By Mr. Vincent Bacon, Surgeon, F. R. S.* N° 432, p. 287.

About 10 at night Mr. Bacon was called to one John Crumpler, a silk-weaver, in Spital-fields. When he came into the room, the man was lying on the bed, his head supported by a by-stander, his eyes and teeth fixed, his nose pinched in, his hands, feet, and forehead cold; and all covered with a cold sweat; no pulse to be perceived, and his breath so short as scarcely to be distinguished. Inquiring into the case, Mr. B. was told, that he had been very well all day, and about 8 had eaten a very hearty supper of pork, and a sallad dressed with oil and vinegar; that immediately after he began to find an indisposition; that the sallad consisted of common sallad herbs, bought at a stall in the market, except some celery picked out of their own garden. Suspecting that he had been eating some poisonous herb, Mr. B. asked if he found in the beginning of the disorder any inclination to vomit? they said no, but that when he found his illness come upon him with great violence, he believed himself to be poisoned, and forthwith drank a large quantity of oil, not less than a pint in all, and after that he loaded his stomach with carduus tea till he vomited, and though he threw up the greatest part of his supper, yet the

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* Aconitum napellus. Linn.

symptoms still increased; and before Mr. B. could get to him, things were come to the extremity above described. Having nothing at hand but a tea spoonful or two of spirit of hartshorn, he forced open his teeth with the handle of a spoon, and as his head was reclined, poured the spirit into his mouth, which a little roused him, and first set him a coughing, and next a vomiting. Mr. B. took the advantage of the little sense that was returned, and continued plying him with carduus-tea, till he had vomited several times more, but could not hinder his swooning often between the times of retching, though he gave him after each 40 or 50 drops of sal volatile et tinctur. croc. aa. p. æ. in a glass of wine. The patient at length began to find a working downwards, as he afterwards expressed himself, which was followed by a stool; after which he vomited two or three times more, and then said his head was so heavy, and his strength and spirits so exhausted, though his stomach and bowels were much easier, that he must needs lie down: his pulse was then a little returned, though very much interrupted and irregular, sometimes beating two or three strokes very quick together, and then making a stop of as long or a longer time than the preceding strokes altogether took up. Having observed that what he had last vomited was little more than the pure carduus-tea, Mr. B. then gave him a draught made of aq. epidem. ther. androm. conf. alkerm. &c. and gave orders to make him some sack-whey to drink between whiles, sometimes alone, and in case of great faintness, with some of the above-named drops. It being near 1 o'clock, Mr. B. left him, and calling to see him next morning, found him much better. He had lain awake, though still, an hour or two after he left him, but being very cold and chilly, had a great deal of covering laid on him, and then found a kindly warmth come over his limbs, which was succeeded by a moderate sweat, and then a quiet sleep of 4 or 5 hours, from which he waked very much refreshed. Mr. B. could see none of the sallad.

The alterations the patient found in himself after eating it, and how they came on, were thus: the first symptom was a sensation of a tingling heat, which not only affected his tongue, but his jaws, so that the teeth seemed loose; and his cheeks were so much irritated, that the people about him, nay even his looking-glass, could scarcely persuade him but that his face was swelled to twice its proper size. This tingling sensation spread itself farther and farther, till it had seized his whole body, especially the extremities; he had an unsteadiness in the joints, especially of the knees and ancles; with twitchings on the tendons, so that he could scarcely walk across the room; and he thought that in all his limbs he felt a sensible stop or interruption in the circulation of his blood; and that from the wrists to the fingers ends, and from the ancles to the toes, there was no circulation at all; but he had no sickness or disposition to vomit till he took the oil, &c. Afterwards his head grew giddy, and his eyes misty and wandering; next a kind of humming or hissing noise seemed continually to sound in his ears, which was followed by the syncopes abovementioned.

There supped with him two women the same night, one of them happened to have a dislike to celery, and therefore laid aside all that she took for such; the other having before been out of order, and was not then perfectly recovered, eat but sparingly, but took this supposed celery along with the other herbs, and felt and complained of all the same symptoms, but in a less degree than the man had done. She would not be prevailed on to vomit, but only took the cordial draught above described. The man got quite well, but the woman is still out of order.

They say there was not put into the whole sallad more that what grows on one of the roots.

Auroræ Boreales observed at Wittemberg in 1732. *By J. F. Weidler, LL. D. and F. R. S.* N° 432, p. 291. *Abridged from the Latin.*

Feb. 18, 1732, O. S. about 9 in the evening, the sky serene, there appeared an aurora borealis. A black arch, whose middle was 20 degrees high, was seen in the north, where a little before, that same evening, the sky was observed serene. The part of the heavens over the black arch was white, and from it at times, shot forth the usual radiations of the lumen boreale, or the luminous pyramids, as also very thin white vapours, like small clouds, were carried with a swift motion towards the vertex.

At 10 o'clock the motion of the luminous matter seemed to cease for some time; yet presently from that white part of the heavens white undulating vapours issued; but the representation of a canopy near the vertex was not seen.

The shining pyramids rose on both sides near the north point; but the fluctuating vapours were more frequent towards the west; the air was all the time still and calm.

Oct. 12, 1732, O. S. immediately after 6 in the evening, there again appeared an aurora borealis, namely a dark arch expanded between N. N. W. and N. E. Above the arch there was a remarkable bright space of the heavens, about 10 degrees broad, but not exactly expressing the figure of an arch. The broader portion declined about 10 degrees from the north to the west; and from thence, as from the fountain of the luminous matter, at 30 minutes after 6, many white pyramids issued which almost reached the zenith; some of them were red, and vanished soon: one in particular, extended between the Crown and

Hercules, continued for a longer time up to the very zenith. In a quarter of an hour this sportive scene was ended. Yet all that night a thin light possessed the northern part of the horizon.

By these and other observations M. Weidler had taken of this northern light, he was more inclined to Dr. Halley's surmise, that its seat is about the magnetic pole, or at least that its motion is in some measure governed and determined from thence.

As to the effect of the aurora borealis, it does not hitherto sufficiently appear, only M. Weidler observed that generally one or more very clear days immediately succeed it. The Swedes and Norwegians, to whom this phenomenon frequently appears, are said to have learned by long experience, that the northern light, when it shines more frequently about the beginning of autumn, portends milder weather and a plentiful harvest. To this hypothesis agree the experiments taken at Witemberg in autumn 1731; for, on the 4th, 7th, 8th, 10th, and 23d of October 1731, N. S. a very frequent and bright lumen boreale was observed, which was succeeded by such seasonable weather, that corn and fruit were very plentiful in 1732.

Of the destroying the Caterpillars and Locusts that infested the neighbouring Parts of Wittemberg. By the same. N° 432, p. 294. *From the Latin.*

Among the particular observations of the year 1732, the following may be worth mentioning, viz. the destroying the caterpillars and locusts, which for several years before had in a grievous manner devoured up the fruits of the earth in the northern parts of the circle of Saxony, the Marche of Brandenburgh, in Lusatia, &c. In the spring of 1732 both these sorts of insects were produced in incredible numbers. The caterpillars in several places soon destroyed all the leaves both of barren and fruit trees; and the locusts likewise again threatened the greatest destruction to the fruits of the earth as in the preceding year: the country people therefore began to dig several pits, and gather the locusts that had not strength enough to fly, into them, and so cover them with earth and kill them.

But this contrivance would have been of little avail, had not these insects been weakened and destroyed by some inclemencies of weather; in such manner that they all soon perished the beginning of the summer, before they could propagate; for, after the kindly heat of the sun, about the beginning of April, 1732, O. S. had invited them from their nests sooner than ordinary, this heat was succeeded by a sudden severe cold for some nights, and by cold and plentiful showers of rain in April and May; and afterwards by constant and plentiful rains about the latter end of May, and for the greatest part of June and July. On these accounts it was, that these noxious animals did not arrive to their usual size and strength; so that they were yet small, about the beginning of June, having not reached that maturity to which they usually arrive about this time of the year. The locusts in particular, impatient of wet, were in the beginning of July found dead all over the fields; and many of them that had retired into the longer stalks of herbs and flowers, and had stuck close to them by their mouths, hung dead from them.

As to the shape of these locusts, they were different from the green ones, commonly observed every year, in the fields and meadows, and which are few in number. The colour of the head and back was black, and in some grey, with yellow specks interspersed; their belly was yellowish; the muscles of the hinder feet red; and when they were on wing, they looked of a purple colour. The bodies of most of them were not above 1½ inch in length; though in August 1731 M. Weidler observed some shrivelled up, to be upwards of 2 inches. In the same month the male and female copulate; each dam contains upwards of 30 eggs, which they lay in holes made in the earth; and at the close of September they die upon them. Four years before, when they first came to these parts from Poland, through Lusatia and the Marche, they flew high in the air in a body, in the middle of summer, above the tops of the houses and turrets; so that at a distance they had the appearance of a cloud. On whatever place they alighted, they quite covered it, and spread far and wide. They seemed to be fond of the more tender tops of the ears of corn; to gain which the better, they cut down the whole unripe ear, especially in the night time. In one night the ears of whole fields were cut down in such a manner, that in some villages the farmers had not even the seed they sowed.

An Extract from the History of the Inoculation of the Small-pox, written by E. Timoni, M. D. Communicated to the Royal Society by Sam. Horneman, M. D.* N° 432, p. 296. *From the Latin.*

Dr. T. states, that at the beginning of the practice of inoculation of the small-pox, at Constantinople, there was a person who used to make an incision through the skin, and then introduce into the wound the scab of a dried pustule, tying a bandage over it. But he observes that this mode of operating was objectionable, not only on account of the pain attending it, but also because it sometimes excited the small-pox in its worst form, while at other times it failed to communicate the disease, though even then it produced very bad sores in

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* See his Account of Inoculation, vol. 6, p. 88, of these Abridgments.

the places where the incisions had been made. Moreover in some instances this mode of inoculation terminated fatally.

Experiments on the Indian Magnetic Sand. By Dr. Muschenbroek.

N° 432, p. 297.

The Indian sand, which is brought to Holland, is said to be chiefly gathered on the sea-shore in Persia; after which it is boiled in water, to free it of its saltness; after which it becomes a black powder, consisting of grains of different sizes; some of which have a very rough surface, and others one part of their surface something rough, and the other very shining. Their figure is very irregular, like grains of common sand; only this Indian sand is smaller. These little lumps have neither taste nor smell, and are friable, so as to be easily reduced to a very subtile powder. It has some parts, which are strongly attracted by the loadstone; and others so very inactive, as scarcely to seem to be magnetical at all; the blackest are the strongest, but the inactive ones are more shining, and inclining to the colour of lead; these are in the greatest quantity: and from them the others are got out by a loadstone. Montenus has several ways examined such a kind of sand, which is brought from Virginia, and described it in Phil. Trans. N° 197. Dr. Muschenbroek examined the Indian sand another way; of which he gave an account in his Physical Dissertations, p. 127. But a great deal still remained to be considered; and as there is much more of this substance of the inactive than of the active or magnetic sort, it was proper to try, whether a magnetic virtue might not be excited or increased in all of it; and after a few trials he found the thing succeed. He suspected that there might perhaps be too great a quantity of sulphur adhering to the sand, to suffer it to be turned into any metalline regulus by a long continuance in the fire; he therefore toasted it in an open crucible for 2 hours, with half the quantity of pot-ash; he afterwards washed away the salt with water, and the sand remained much blacker than before, of which he found more than a quarter endued with a greater magnetic force. Muschenbroek does not scruple to attribute this virtue to the salt; because, though the action of the fire alone does increase the force of the sand, yet it does not give it near so much attractive force.

And because common black soap is made of oil boiled with a lixivium of potash, he had a mind to try whether soap might not do more than salt alone in exciting the virtue in the sand; so he mixed the sand with an equal quantity of soap, which he first exposed to a gentle fire in an open crucible, to dry up the soap, which swells very much: then the fire was heightened for $\frac{3}{4}$ of an hour, the oily substance entirely consumed, and the matter in the crucible strongly fired; afterwards boiling it in water, and washing it well, he obtained a black sand, which was all endued with a vigorous attracting force. Pleased with this success, he had a mind to try whether he could excite a greater force in it; he therefore again roasted it with black soap as before, and even a third time: but no addition was thus made to its virtue. He found that keeping it too long in the fire is as prejudicial as keeping it too short a time; between half an hour and an hour seemed to him the most proper space of time.

He afterwards added to the black soap half the quantity of salt of tartar, mixing it with an equal quantity of sand; which, when exposed in a crucible to a reverberatory fire for $\frac{3}{4}$ of an hour, he washed in water; and then so considerable was the virtue of the sand, that if it did not exceed the former, it was at least equal to it.

And because he had observed the oiliness of the soap to conduce much to excite the virtue in the sand, he mixed beef-tallow with an equal quantity of sand, and having closed the crucible very well, he exposed the whole mass to a reverberatory fire for 2 hours; by which the sand became much blacker, and received a great deal of attractive virtue; but that sand, which was burned 2 hours with an equal quantity of pitch, became more active, as also very black, subtile, and very little shining; but when it was exposed a longer time in the same crucible, he observed it to be weaker; as also when it was in the crucible with the pitch but $\frac{1}{8}$ of an hour, it scarcely acquired any virtue; so that there must be a determined action of fire to raise the virtue in the sand. Yet he could not excite a greater virtue in the sand than by the following means, viz. mixing the sand in the crucible with equal parts of rosin, pitch, frankincense, and rape-oil, and exposing it to a reverberatory fire for an hour, having first closed up the crucible well. Between the black coals of the oily matter, there adheres a very black sand, which leaps up swiftly to the loadstone, as soon as it is brought near it. He then considered whether the sand did not acquire the greatest force, as it came nearer to the nature of steel, by burning it with the abovementioned bodies; and in order to try this, he put it among such bodies as turn iron into steel, according to the operations described by M. Reaumur, in that excellent book, The Art of turning Iron into Steel. He therefore took 3 parts of sand, 2 parts of chimney soot, and of sea salt, powdered charcoal, and ashes, one part each; having accurately mixed all these bodies together, they were exposed for 6 hours in a close crucible to a strong fire; the whole mass was then boiled and washed in water; then dried, and so received a great deal of attracting force; but it was not near so active as that prepared with soap, or in the manner last described.

Dr. Muschenbroek conjectured at first, that this sand is an imperfect magnet, or subtile powder of it, which, when it is increased to a larger lump, forms the common loadstones; but when he found by experience that common loadstones exposed to the fire, according to some of the methods abovementioned, rather lost of their force than gained, he altered his opinion, and owns that he has not hitherto penetrated into the knowledge of this matter.

Whatever it be, it is certain that there are several kinds of this sand, brought from different countries; for it is brought from Persia: and some from Virginia; there is another sort in Italy, common enough at Leghorn: and this last is naturally very attractive; there are two sorts found in the Eber, a river of Hassia; of which one resembles the Italian, and the other consists of large grains, almost as large as hemp-seed, but scarcely having any virtue. Muschenbroek had, besides, a very vigorous sort, which he is told was got near old Ragusa in Dalmatia. The several kinds of this sand are unknown, which time and the diligent observations of philosophers must hereafter show.

Some Observations made in London, by Mr. George Graham; and at Black-river in Jamaica, by Mr. Colin Campbell, about the going of a Clock; in order to determine the Difference between the Lengths of Isochronal Pendulums in those Places; and communicated by Mr. Bradley. N° 432, p. 302.

Though it be upwards of 60 years since M. Richer first discovered, that pendulums of the same length do not perform their vibrations in equal times in different latitudes; and though several experiments, since made in different parts of the earth, concur to prove, that pendulums swinging seconds are in general shorter, as we approach the equator; yet what the real difference is between their lengths in different latitudes, does not seem to have been determined with sufficient exactness, by the observations that have hitherto been communicated to the public; as may be gathered from the 20th proposition of the 3d book of Sir Isaac Newton's Principia, where they are compared as well with each other, as with the theory of that illustrious author. It is therefore to be wished that more of this kind of experiments could be made with greater accuracy in proper places, by such persons as have sufficient skill and opportunities to do it; that we might be enabled to judge with more certainty concerning the true figure of the earth, and the nature of its constituent parts.

As an inducement to such as may have it in their power to put the like again into practice, here follows an account of a very curious experiment of this sort, made in Jamaica by Mr. Campbell: his clock, which was made by the ingenious Mr. Graham, was so carefully contrived, that its pendulum might at pleasure be reduced to the same length, whenever there should be occasion to remove the clock from one place and set it up in another.

This clock, being chiefly designed for astronomical observations, had no striking part, and its pendulum was adjusted to such a length, that in London it vibrated seconds of sidereal, and not solar, time. When it was finished, Mr. Graham fixed it up in a room, situated backward from the street, and on the northside of his house, to prevent its being disturbed by coaches, or other carriages that passed through the streets, and that it might be as little affected by the sun as possible. Having set it a-going, he compared it with the transits of the star Lucida Aquilæ over the meridian, which passed, by the clock,

1731, August 20. . at. . 8^h 59^m 15^s
22. . at. . 8 59 18
23. . at. . 8 59 20½
25. . at. . 8 59 22
28. . at. . 8 59 25½
29. . at. . 8 59 26
30. at. . 8 59 27

Hence it appears that the clock gained 12 seconds in 10 apparent revolutions of the star.

In order to estimate how much the pendulum may be lengthened by greater degrees of heat, or how much slower the clock would go on that account, when removed into a warmer climate, a thermometer was fixed by the side of it; and between the hours of 10 and 11 o'clock in the morning, and at night, notice was taken at what height the spirits stood, and the mean height for each day was as follows:

days	therm.	days	therm.
1731, Aug. 21. .	32½ div.	1731, Aug. 26. .	27¼ div.
22. .	30¾	27. .	27½
23. .	28¾	28. .	27½
24. .	27¾	29. .	27½
25. .	28¼	30. .	27¾

Hence the mean height for all these days was about 28½ divisions.

The clock-weight, that keeps the pendulum in motion, is 12 lb. 10½ oz. and is to be wound up once a month. The weight of the pendulum itself is 17 lb.; and, during the time that the clock was compared with the transits of the star, it vibrated each way from the perpendicular 1° 45′. The magnitude of the vibrations was estimated by means of a brass arch, which was fixed just under the lower end of the rod of the pendulum, and divided into degrees, &c.

August 31, Mr. Graham took off the weight belonging to the clock, and hung on another of 6 lb. 3 oz. and with this weight the pendulum vibrated only 1° 15′ on each side; and the clock went 1½ second slower in 24 hours, than when its own weight of 12 lb. 10½ oz. was hung on.

This experiment shows, that a small difference in the arcs, described by the pendulum, or a small alteration in the weight that keeps it in motion, will cause no great difference in the duration of the vibrations, and therefore a little alteration in the tenacity of the oil on the pivots, or in the foulness of the clock, will not cause it to accelerate or retard its motion sensibly; from whence we may conclude, that whatever difference there shall appear to be, between the going of the clock at London and in Jamaica, it must be entirely owing to the lengthening of the pendulum by heat, and the diminution of the force of gravity on it.

Mr. Graham sent very full directions to Mr. Campbell, describing in what manner the clock was to be fixed up, and how the pendulum might be reduced exactly to the same state as it was when in England; but no intimation was given concerning the going of the clock, that the experiment might be made with all possible care, and without any bias or prejudice in favour of any hypothesis, or former observations.

In July 1732 was received an account of the success of the experiment, by the hands of Mr. Joseph Harris, who was present at the making of it in Jamaica, and who brought over with him the original journal of the observations of the transits of two stars, viz. Sirius and β Canis majoris, over the meridian, compared with the clock, after it was fixed up in Jamaica, as Mr. Graham had directed; with the height of the spirits of the aforesaid thermometer, on the several days of observation.

The chief of those observations are contained in the following table; the first column shows the day of the month; the second the name of the star, and the time by the clock of its observed transit over the meridian; the third contains the hour of the day, when the thermometer was observed, with the height of the spirits at those hours; the morning hours being denoted by the letter A, and those of the afternoon by the letter P.

1732	Canis majoris	Time of transit			Hour of day		Thermometer
Jan. 23,	β	11^{h}	59^{m}	50^{s}	$10\frac{1}{2}^{h}$	A	$14\frac{3}{4}$
	α	12	22	14	$9\frac{1}{2}$	P	11
24,		cloudy			$11\frac{1}{2}$	A	$15\frac{1}{4}$
25,	β	11	55	40	$8\frac{1}{2}$	A	$17\frac{1}{2}$
	α	12	18	4	$9\frac{1}{4}$	P	$11\frac{1}{4}$
26,	β	11	53	35	8	A	20
	α	12	16	0	2	P	$8\frac{1}{2}$
					9	P	10
27,	β	11	51	31	7	A	$17\frac{1}{2}$
	α	12	13	55	2	P	$8\frac{1}{2}$
					$9\frac{3}{4}$	P	$12\frac{1}{2}$
28,	β	11	49	26	7	A	$20\frac{1}{2}$
	α	12	11	51	2	P	11
					10	P	12
Jan. 29,	β	11^{h}	47^{m}	22^{s}	$6\frac{3}{4}^{h}$	A	19
	α	12	9	46	3	P	9
					9	P	$11\frac{1}{4}$
					7	A	$20\frac{1}{2}$
30,		cloudy			4	P	7
					11	P	13
31,	β	11	43	12	7	A	20
	α	12	5	37	9	P	$8\frac{1}{2}$
Feb. 1,	β	11	41	$8\frac{1}{2}$	10	A	$18\frac{3}{4}$
	α	12	3	33	11	F	16
2,	β	11	39	0	$9\frac{1}{2}$	A	$17\frac{1}{2}$
	α	12	1	$23\frac{1}{2}$	2	P	9
					5	P	6
					9	P	$8\frac{1}{2}$

1732	Canis majoris	Time of transit			Hour of day		Thermometer
Feb. 3,	β	11	36^m	53^s ..	$8\frac{1}{2}^h$	A	19
					1	P	$9\frac{1}{2}$
					9	P	9
4,	β	11	34	46 ..	$6\frac{1}{4}$	A	18
	α	11	57	11 ..	12		$9\frac{1}{2}$
					9	P	8
5,	β	11	32	40 ..	$7\frac{1}{2}$	A	$19\frac{1}{4}$
	α	11	55	5 ..	$3\frac{1}{2}$	P	6
					$8\frac{1}{2}$	P	8
6.	β	11	30	35 ..	7	A	$18\frac{1}{2}$
	α	cloudy		..	4	P	$7\frac{1}{2}$
					$8\frac{1}{2}$	P	8
7,	β	11	28	31 ..	7	A	$20\frac{1}{2}$
	α	11	50	55 ..	12		12
					$8\frac{1}{2}$	P	$8\frac{1}{2}$
8,	β	cloudy		..	$6\frac{1}{2}$	A	$21\frac{1}{2}$
	α	11	48	50 ..	$8\frac{1}{2}$	P	$8\frac{1}{4}$
9,	β	11	24	20 ..	$9\frac{1}{2}$	A	14
	α	11	46	44 ..	$8\frac{1}{2}$	P	8
10,	β	11	22	$12\frac{1}{2}$ ..	$7\frac{1}{4}$	A	16
	α	11	44	37 ..	$11\frac{1}{2}$	A	10
					$3\frac{3}{4}$	P	$3\frac{3}{4}$
					$8\frac{1}{4}$	P	6
Feb. 11,	β	11	20^m	6^s ..	$7\frac{1}{2}^h$	A	16
	α	11	42	30 ..	12		$9\frac{1}{2}$
					$8\frac{1}{2}$	P	$5\frac{3}{4}$
12,	β	11	18	0 ..	10	A	$17\frac{1}{2}$
	α	11	40	24 ..	12		13
					8	P	$5\frac{3}{4}$
13,		clouds		..	9	A	17
					8	P	6
14,	β	cloudy		..	$7\frac{1}{2}$	A	16
	α	11	36	15 ..	12		11
					8	P	10
15,		clouds		..	9	A	18
					12		$13\frac{3}{4}$
					$8\frac{1}{2}$	P	$7\frac{1}{2}$
16,	β	cloudy		..	8	A	14
	α	11	32	4 ..	8	P	7
17,	β	11	7	34 ..	12		12
	α	11	29	59 ..	8	P	$6\frac{1}{4}$
18,	β	11	5	29	..12		$12\frac{1}{4}$
	α	11	27	53			

The pendulum, during this interval, vibrated about 1° 52′ each way from the perpendicular.

The transits of the stars over the meridian were observed with a telescope, fixed at right angles to an horizontal axis, whose ends lay exactly east and west; by the turning of which axis, the line of collimation of the telescope was constantly directed in the plane of the meridian. This instrument was daily adjusted to a mark, fixed in the meridian; and in the journal, between the 2d and 3d of February, the following remark was made.

N. B. This day was hotter than usual, as appears by the thermometer; and the transit instrument had lost the level a little; but after we had adjusted it, it pointed exactly to our meridian mark; and therefore we are at a loss for the cause of this difference in the clock.

From the foregoing table it appears, that the clock lost $54^m\ 21^s$ in 26 revolutions of the stars; that is, about $2^m\ 5\frac{1}{2}^s$ in one revolution; the difference from this medium somewhat varying on account of a greater or less degree of heat on different days.

The mean of all the observed heights of the thermometer, from Jan. 26 to Feb. 18, was about $12\frac{1}{4}$ divisions; therefore the difference between the mean heights of the thermometer at Jamaica and London, during the intervals of the respective observations, was $15\frac{1}{4}$ divisions; the spirit standing so much higher in Jamaica, because of the greater heat in that island.

That we might be able to judge, how much the different degrees of heat, corresponding to any number of divisions on this thermometer, would cause the clock to go slower, by lengthening its pendulum, Mr. Graham took notice of the lowest point to which the spirits sunk at London in the winter 1731, and the greatest height to which they rose in the following summer; and comparing the motion of the spirits in this thermometer, with the alterations in another made with quicksilver, which he had for some years made use of; he concluded that at London, the spirits in this thermometer would stand, one year with another, about 60 divisions higher in summer than in winter.

By several years experience he likewise found, that his clocks, of the same sort with Mr. Campbell's, when exposed as usual to the different degrees of heat and cold of our climate, do not vary in their motion above 25 or 30 seconds in a day.

From these observations and experiments, we may therefore reasonably conclude, that sufficient allowance will be made for the lengthening of the pendulum by heat, if we suppose the clock on that account to go one second in a day slower, when the spirits of this thermometer stand 2 divisions higher, and in the same proportion for other heights.

Admitting then that the mean height of the thermometer, while the clock was compared with the stars at Jamaica, exceeded that at London between 15 and 20 divisions; if we allow 8 or 9 seconds on that account, the remaining difference must be entirely owing to the difference of the force of gravity in the two places.

On comparing the observations, it appears, that in one apparent revolution of the stars, the clock went $2^m\ 6\frac{1}{2}^s$ slower in Jamaica than at London; deducting therefore $8\frac{1}{2}^s$, on account of the greater heat in Jamaica, there remains a difference of $1^m\ 58^s$, which must necessarily arise from the diminution of gravity, in the place nearest the equator.

Mr. Bradley has allowed the clock to have lost somewhat more, on account of the difference of heat, than the mean heights of the thermometer may seem to require, on a supposition that the total heat of the days, compared with the cold of the nights, bears a greater proportion in Jamaica, than in London; but if that supposition be not admitted, then the clock in Jamaica must have gone rather more than $1'\ 58''$ in a day slower than in England.

Mr. Campbell's observations were made at Black-river, in 18° N. lat. Now if we suppose, with Sir Isaac Newton, that the difference in the going of the clock, is owing to the greater elevation of the parts of the earth towards the equator; it will follow from these observations, and what is delivered by him in prop. 20 of the third book of his Principia, that the equatorial diameter is to the polar as 190 to 189; the difference between them being $41\frac{1}{2}$ miles; which is somewhat greater than what Sir Isaac Newton had computed from his theory, on the supposition of a uniform density in all the parts of the earth.

Without entering into the dispute about the figure of the earth, Mr. Bradley at present supposes, with Sir Isaac Newton, that the increase of gravity, as we recede from the equator, is nearly as the square of the sine of the latitude, and that the difference in the length of pendulums, is proportional to the augmentation or diminution of gravity. On these suppositions Mr. Bradley collects, from the abovementioned observations, that if the length of a simple pendulum, that swings seconds at London, be 39.126 English inches, the length of one at the equator would be 39.00, and at the poles 39.206. And, abstracting from the alteration on account of different degrees of heat, a pendulum clock, that would go true time under the equator, will gain 3^{m} $48\frac{1}{2}^{s}$ in a day at the poles; but the number of seconds which it would gain in any other latitude, would be to 3^{m} $48\frac{1}{4}^{s}$, nearly as the square of the sine of that latitude, to the square of the radius; whence it follows, that the number of seconds a clock will lose in a day, on its removal to a place nearer the equator, will be to 3^{m} $48\frac{1}{4}^{s}$, nearly as the difference between the squares of the sines of the latitudes of the two places, to the square of the radius. Thus the difference of the squares of the sines of $51\frac{1}{2}^{0}$ and 18^{0}, the latitudes of London and Black-river, being to the square of the radius, as 118 to $228\frac{1}{4}$, the clock will go 1^{m} 58^{s} in a day slower at Black-river than at London; as was found by observation.

It may be hoped that Mr. Campbell's success in this experiment, and the little trouble there is in making it, will induce those gentlemen who may hereafter carry pendulum clocks into distant countries, to attempt a repetition of it after his manner; that is, by keeping or restoring the pendulums of their clocks to the same length in the different places, and carefully comparing them with the heavens; and at the same time taking notice of the different degrees of heat, by means of a thermometer. From a variety of such experiments we should be enabled to determine how far Sir Isaac Newton's theory is conformable to truth, with much greater certainty than from those trials which are made by actually measuring the lengths of simple pendulums; because a difference of the 100th part of an inch, in the length of a pendulum, corresponds to 11 seconds in a day, and it being easy to observe how much a clock gains or loses in a day, even to a single second. It is certain, that by means of a clock, compared in the manner abovementioned, we may distinguish a difference in the lengths of isochronal pendulums, of 1000th part of an inch, or less; whereas it will be scarcely possible to measure their true lengths, without being liable to a greater error than that. Besides, by taking notice how much a clock gains or loses, on the falling or rising of a thermometer, we can better allow for the different degrees of heat in this, than in the other method of making the experiment, by actual measurement; since it may not be easy to determine how much the measure itself, which we make use of, will be lengthened by different degrees of heat.

For these reasons, Mr. Bradley reckons Mr. Campbell's experiment, to be the most accurate of all that have hitherto been made, and the properest to determine the difference of the gravity of bodies in different latitudes; and therefore he subjoins a table he computed from it, that contains the difference of the length of a simple pendulum, swinging seconds at the equator, and at every 5th degree of latitude, with the number of seconds that a clock would gain in a day, in those several latitudes, supposing it went true when under the equator; by means of which, any one may readily compare other like observations with his; and thus discover whether the alteration of gravity in all places be uniform, and agreeable to the rule laid down by Sir Isaac Newton, or not.

The lat. of the place.	The difference of the length of the pendulum in parts of an English inch.	Seconds gained by a clock in 1 day.	The lat. of the place.	The difference of the length of the pendulum in parts of an English inch.	Seconds gained by a clock in 1 day.
deg.	inch.	seconds.	deg.	inch.	seconds.
5	0.0016	1.7	50	0.1212	134.0
10	0.0062	6.9	55	0.1386	153.2
15	0.0138	15.3	60	0.1549	171.2
20	0.0246	26.7	65	0.1696	187.5
25	0.0369	40.8	70	0.1824	201.6
30	0.0516	57.1	75	0.1927	213.0
35	0.0679	75.1	80	0.2003	221.4
40	0.0853	94.3	85	0.2050	226.5
45	0.1033	114.1	90	0.2065	228.3

Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands. By Mark Catesby, F.R.S. With some Extracts out of the sixth Set, by Dr. Mortimer, R. S. Secr. N° 432, p. 315.

Conjectures on the charming or fascinating Power attributed to the Rattle-snake, grounded on credible Accounts, Experiments, and Observations. By Sir Hans Sloane, Bart. P. R. S.* N° 433, p. 321.

As to rattle-snakes, all accounts agree that by keeping their eyes fixed on any small animal, as a squirrel, bird, or such like, though sitting on the branch of a tree of a considerable height, it shall, by such stedfast or earnest looking, be made to fall dead into their mouths.

Sir Hans Sloane had a rattle-snake given him. It had lived 3 months before

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* On this subject see some ingenious observations by Professor Barton, of Philadelphia, referred to at p. 642, vol. 6, of these Abridgments.

without any sustenance, and had in that time parted with its outer coat or exuviæ, which was found among the gravel. Captain Hall, a very understanding and observant person, who had lived many years in Virginia, ventured to take the snake out of the box, though the poison from its bite is almost instant death; for, he gave an instance of a person bitten, who was found dead at the return of a messenger going to the next house to fetch a remedy, though he was not gone above half an hour. Nay, so certain are the mortal effects of this poison, that sometimes the waiting till an iron can be heated, in order to burn the wound, is said to have proved fatal. This gentleman therefore thought the securest way was immediately to cut out the part where the wound was made; for he had seen several, who carried these hollow scars about them, as marks of the narrow escape they had had, and never felt any inconvenience afterwards.

An experiment was tried before several physicians, in the garden belonging to their college in London. The Captain, by keeping the head fast with a forked stick, and making a noose, which he put about the tail of the snake, tied it fast to the end of another stick, wherewith he took him out of the box, and laid him on the grass-plat. Then a dog being made to tread upon him, he bit the dog, which howled very bitterly, and went away some few yards distant from the snake; but in about one minute of time he grew paralytic in the hinder legs, after the manner of dogs who have the aorta descendens tied. He died in less than 3 minutes time, as is related by Mr. Ranby, in an account of this experiment in Philos. Trans. N° 401, and by Captain Hall, N° 399.

In Sir Hans's opinion, the whole mystery of enchanting or charming any creature is chiefly this, viz. that when such animals as are their proper prey, as small quadrupeds or birds, &c. are surprised by them, they bite them, and the poison allows them time to run a small way, as our dog did, or perhaps a bird to fly up into the next tree, where the snakes watch them with great earnestness, till they fall down, or are perfectly dead, when having licked them over with their spittle, they swallow them down; as the following accounts relate.

" Some people in England, says Colonel Beverley, in his History of Virginia, are startled at the very name of the rattle-snake, and fancy every corner of that province so much pestered with them, that a man goes in constant danger of his life, that walks abroad in the woods. But this is a gross mistake; for this snake is very rarely seen, and when that happens, it never does the least mischief, unless you offer to disturb it, and so provoke it to bite in its own defence. There are several other snakes, which are seen more frequently, and have very little or no hurt in them; as black-snakes, water-snakes, and corn-snakes. The black viper-snake, and the copper-bellied-snake, are said to be as venomous as the rattle-snake; but they are as seldom seen. These three poisonous snakes bring forth their young alive; whereas the other three sorts lay eggs, which are hatched afterwards; and that is the distinction they make, esteeming only those to be venomous which are viviparous. They have likewise the horn-snake, so called from a sharp horn in its tail, with which it assaults any thing that offends it, with such force, that it is said it will strike its tail into the but-end of a musket, from whence it is not able to disengage itself."

"All sorts of snakes will charm both birds and squirrels, and the Indians pretend to charm them; several persons have seen squirrels run down a tree directly into a snake's mouth. They have likewise seen birds fluttering up and down, and chattering at these snakes, till at last they have dropped down just before them."

"In the end of May, 1715, stopping at an orchard, by the road-side, to get some cherries, being three of us in company, we were entertained with the whole process of a charm between a rattle-snake and a hare, more than half grown. One of the company in his search for the best cherries, saw the hare sitting, and though he went close by her, she did not move, till he gave her a lash with his whip. This made her run about 10 feet, and there sit down again. The gentleman not finding the cherries ripe, immediately returned the same way, and near the place where he struck the hare he spied a rattle-snake. Still not suspecting the charm, he went back about 20 yards to a hedge, to get a stick to kill the snake, and at his return found the snake removed and coiled in the same place from whence he had moved the hare, This put him on looking for the hare again, and soon spied her about 10 feet from the snake, in the same place to which she had started when he whipped her. She was now lying down, but would sometimes raise herself on her fore feet, struggling as it were for life, or to get away, but could never raise her hinder parts from the ground; and then would fall flat on her side again, panting vehemently. In this condition the hare and snake were when he called me, and though we all three came up within 15 feet of the snake to have a full view of the whole, he took no notice of us, nor even gave a glance towards us. There we stood at least half an hour, the snake not moving, but the hare often struggling and falling on its side again, till at last the hare lay still as dead for some time; then the snake moved out of his coil, and slid gently and smoothly on towards the hare, his colours at that instant being ten times more glorious and shining than at other times. As the snake moved along, the hare happened to make another struggle, on which the snake made a stop, lying at his length, till the hare had lain quiet again for a short space; and then he advanced again, till he came up to the hinder parts of the hare, which all this time had been towards the snake. There he made a survey all over the hare, raising part of his body above it, then turned off, and went to the head and nose of the hare, after that to the ears, took the ears in his mouth one after the other, working each apart in his mouth, as a man does a wafer to moisten it; then returned to the nose again, and took the face into his mouth, straining and gathering his lips sometimes by one side of his mouth, sometimes by the other. At the shoulders he was a long time puzzled, often hauling and stretching the hare out at length, and straining forward first one side of his mouth, then the other, till at last he got the whole body into his throat. We then went to him, and taking the twist-band off from my hat, I made a noose, and put it about his neck. This made him at length very furious; but we having secured him, put him into one end of a wallet, and carried him on horseback 5 miles, to the house where we lodged that night. The next morning we killed him, and took the hare out of his belly. The head of the hare was begun to be digested, and the hair to fall off, having lain about 18 hours in the snake's belly."

"Again, in my youth I was bear-hunting in the woods above the inhabitants; and having straggled from my companions, I was entertained at my return with an account of a pleasant rencounter between a dog and a rattle-snake, about a squirrel. The snake had got the head and shoulders of the squirrel into his mouth, which being rather too large for his throat, it took him up sometime to moisten the fur of the squirrel with his spittle, to make it slip down. The dog took this advantage, seized the hinder parts of the squirrel, and tugged with all his might. The snake on the other side would not let go his hold for a long time; till at last, fearing he might be bruised by the dog's running away with him, he gave up his prey to the dog. The dog eat the squirrel, and felt no harm."

"Another curiosity concerning this viper, which I never met with in print, I will also relate from my own observation. My waiting-boy being sent abroad on an errand, brought home a rattle-snake in a noose. I cut off the head of this snake, leaving about an inch of the neck with it: this I laid on the head of a tobacco hogshead. Now these snakes have but two teeth, by which they convey their poison; and they are placed in the upper jaw, pretty forward in the mouth, one on each side. These teeth are hollow and crooked like a cock's spur: they are also loose or springing in the mouth, and not fastened in the jaw-bone, as all the other teeth are. The hollow has a vent also through by a small hole a little below the point of the tooth. These two teeth are kept lying down along the jaw, or shut like a spring-knife, and do not shrink up as the talons of a cat or panther: they have also over them a loose thin film or skin of a flesh-colour, which rises over them when they are raised; which I take to be only at the will of the snake to do injury. This skin does not break by the rising of the tooth only, but keeps whole till the bite is given, and then is pierced by the tooth, by which the poison is let out. The head being laid on the hogshead, I took two little twigs or splinters of sticks; and having turned the head on its crown, opened the mouth, and lifted up the fang or springing-tooth on one side several times; in doing which I at last broke the skin. The head gave a sudden champ with its mouth, breaking from my sticks; in which I observed that the poison ran down in a lump like oil, round the root of the tooth. I then turned the other side of the head, and resolved to be more careful to keep the mouth open on the like occasion, and observe more narrowly the consequence. For it is to be observed, that though the heads of snakes, terrapins (a sort of tortoise) and such like vermin, be cut off, yet the body will not die in a long time after. After opening the mouth on the other side, and lifting up that fang also several times, he endeavoured to give another bite or champ. But I kept his mouth open, and the tooth pierced the film, which emitted a stream like one full of blood, in blood-letting, and cast some drops on the sleeve of the carpenter's shirt, who had no waistcoat on. And though nothing could then be seen of it on the shirt, yet in washing there appeared five green specks, which every washing appeared plainer and plainer, and lasted as long as the shirt, which the carpenter told me was about three years after. The head we threw afterwards down on the ground, and a sow came and eat it before us, and received no harm."

" I will likewise give you a story of the violent effects of this sort of poison, because I depend on the truth of it, having it from an acquaintance of good credit, one Colonel James Taylor of Metapony. He being with others in the woods a surveying, they found a rattle-snake, and cut off his head, with about 3 inches of the body. Then with a green stick, which he had in his hand, about a foot and a half long, the bark being newly peeled off, urged and provoked the head, till it bit the stick in fury several times. On this the colonel observed small green streaks to rise up along the stick towards his hand. He threw the stick on the ground, and in a quarter of an hour, the stick of itself split into several pieces, and fell asunder from end to end."

Father Labat likewise tells us (in his Nouveau Voyage aux Isles de l' Amerique, Tom. iv.) that serpents, when they bite their prey, retire, to avoid being hurt by them; and when dead, cover them with their spittle, extend their feet along their sides and tails, if quadrupeds, and then swallow them.

Concerning the Squilla Aquæ Dulcis. By Dr. Richardson, F. R. S.* N° 433, p. 331.

Dr. Richardson has observed what he does not remember to be noticed by any naturalist, viz. the great destruction made among the small fry of fish by the squilla aquæ dulcis, which abound in most standing waters. In a small breeding pond near his house, where he had formerly plenty of small carp and tench every year, and of late scarcely any young breed to be met with, his gardener observed one of the squillæ, with a carp in its mouth almost as large as itself; and has since observed these insects hunting among the weeds, and vigorously pursuing the small fry. The Dr. ordered the gardener to catch some of these insects, and bring them home alive, with some of the smallest fish he could meet with. He put them together in a large basin of water. The insects were so rapacious, that they fell upon the fish immediately, and destroyed several in his sight; and before morning had devoured all that were in the basin.

A Solar Eclipse observed at Wittemberg, May 2, 1733. O. S. By J. F. Weidler, LL. D. Math. Profess. and F. R. S. N° 433, p. 332.

At 6h	36m	5s	p. m. the beginning of the eclipse.
7	2	50	6 digits were eclipsed.
7	29	20	11 digits were eclipsed.
7	46	5	the sun set, cloudy.

An Abstract of the Meteorological Diaries, communicated to the Royal Society, with Remarks on them. Part 3d. By W. Derham, D. D. F. R. S. N° 433.

PART III.† Containing Meteorological Observations made at Berlin, and in Sweden at Lunden, Bettna, Upsal, Bygdea, Pithea, in 1726.

These Meteorological Observations were made in the year 1726, some twice, some thrice, every day; viz. at Berlin, by the society there, and communicated by Joh. Theod. Jablonski; and in Sweden, at Lunden, by Conrad Quensel, mathematic professor in the Caroline academy; and at Bettna in Sudermanland, by Andr. Gering, pastor and provost of the place; and at Upsal, by Eric Burman, Astron. Professor in the Gustavian Academy; and at Bygdea, in

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* The insects here mentioned are the larvæ of *dytisci,* which are common in stagnant waters, and are very destructive to fish and many other animals in a young state.

† See Part 2d in N° 429.

Westro-Bothnia, by the late Jacobus Burman, pastor of the place; and lastly, at Pithea, in the same province, by Olave Burman, and Israel Stecksen, students.

The most useful of the observations are represented in a table; showing the highest, lowest, and mean heights of the barometer and thermometer in every month, at the several places.

Dr. Derham remarks on the barometrical ranges at the several places, in every month of the year 1726, that there is a great agreement between the ascents and descents of the mercury, sometimes at the very same time, and generally near it. If the mercury was remarkably high or low, it was so in all, or most of the places: if stationary for 3 or 4, or more days, it was the same in all. Only the alteration would begin, or end, somewhat sooner or later perhaps, in one place than another; and when any deviation was from this rule, it was commonly most remarkable in the Pithea observations.

The Thermometrical observations he can give no account of, as he understands not the thermometers used, nor the freezing, temperate, or other points. Only the Upsal thermometer, which was made by Mr. Hauksbee, must serve for all, in which the point of extreme heat is marked 5° above 0; and so is graduated downwards to 45°, which is the point of temperate; and 65°, which is the point of freezing. The mean of all the degrees of every month, at Upsal, M. Burman has noted according to Dr. Jurin's directions, in the Philos. Trans. N° 379; which is, by adding the whole month's degrees, and dividing by the number of days. A number of observations are made of the weather in the several places, and the different months, as to the rain, snow, frost, &c. which are all omitted, as quite uninteresting.

On Ambergris. By Caspar Neuman, M. D. Professor of Chemistry at Berlin, and F.R.S. Part 1. *An Abstract from the Latin.* N° 433, p. 344.

This is the first of 3 elaborate dissertations by Dr. N. on ambergris; an abstract of all which we shall lay before the reader in this place, rather than resume the subject in different parts of the volume, as, owing to the great length of the author's papers, is done in the original Transactions.

In this first part Dr. N. gives an account of the various and opposite opinions which naturalists have entertained, respecting the origin and nature of ambergris; which some have referred to the vegetable, some to the animal, and others to the mineral kingdom. After refuting the opinion respecting its vegetable origin, he proceeds to mention the fact of its having been found in the stomach and intestines of whales, whence it is said to be discharged with their excrement, or to be the excrement itself (Kæmpfer Amœnitat. Exot. Fascicul. III. 635) an opinion which he in like manner rejects. This is the substance of this first part of Dr. N.'s dissertation.

In the 2d part he examines the accounts given by Dr. Boylston (Phil. Trans. N° 385,*) and by Mr. Paul Dudley (ibid. N° 387†), who relate that ambergris is found in the spermaceti whale, in a peculiar cyst or bag, corresponding to the musk-bag in the musk-deer, &c. thus making it to be a peculiar secretion. But from considering the situation and connexion of this bag, and the description of the supposed balls of ambergris found in it, Dr. N. is induced to believe, that the abovementioned cyst or bag, was the urinary bladder of the whale; that the supposed balls of ambergris, were nothing but calculous concretions; and that the yellow liquor in which those concretions floated, was the whale's urine; an opinion which had been previously entertained by the Rev. Mr. Price of Boston.—If ambergris were a peculiar secretion, resembling musk, castor, civet, &c. it should be found in every spermaceti whale; whereas, according to Dr. Boylston's account, it is found scarcely in 1 whale out of 100 male, or bull-whales, and never in the female or cow whales; not to mention that some of the specimens of ambergris are too large ever to have been contained in the aforesaid cysts.‡ Again, if this were a secreted substance resembling castor and musk, how, he asks, could the beaks of birds, fish-bones and other foreign bodies be found in it? But what after all is with him the strongest proof that ambergris is not of animal origin is, that pure ambergris yields by distillation, products very different from those obtained by the same process from animal substances.

Being himself convinced, for the reasons given in the two preceding papers, that ambergris is neither of vegetable nor of animal origin, Dr. N. in the next place endeavours to prove that it belongs to the mineral kingdom; being (as he is of opinion) a species of bitumen issuing from the bottom of the sea, at first soft and viscid, and afterwards hardened by the action of the salt-water.

Having thus explained (as he thinks) the origin of ambergris, he proceeds, in the 3d part of his communications on this subject, to give an account of its physical and chemical properties, showing at the same time in what manner the genuine may be distinguished from the fictitious or adulterated ambergris. He states that by distillation he obtained from it a water, oil and salt, possessing the same properties with the water, oil and salt obtained by similar treatment from amber (succinum) itself. He says that it is soluble in highly rectified

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* Vol. VII. p. 57, of these Abridgments.

† Vol. VII. p 78, of these Abridgments.

‡ Dr. N. mentions instances of masses of ambergris having been found which weighed many cwt.

sp. of wine, provided that the ambergris be broken into small pieces, and that the alkohol be subjected to a degree of heat sufficient to make it boil. In this manner he found that ℈ij of pure ambergris might be dissolved in ℥j of highly rectified sp. of wine. By distillation he obtained from ʒi of ambergris ℈iss of oil, 5 grs. of water and 2 grs. of salt. The powdery residuum amounted to about 1 gr. In this operation there was a loss of 2 grs.

Dr. N. subjoins a remark respecting the white suetty matter commonly deposited by the spirituous solution, or tincture of ambergris, and which Lemery supposed to be wax. Dr. N. says, that this deposition, or precipitation, only happens when a part of the alkohol evaporates, in consequence of the bottle, in which the solution or tincture is kept, being only ½ or 3 parts full, or being provided with a stopple that does not closely fit; and that the precipitate itself is pure ambergris.*

On Ambergris. By Caspar Neuman, M. D. Professor of Chemistry, at Berlin, and F.R.S. Part II. N° 434, p. 371.

The substance of this paper has been incorporated with the abstract above given.

An Account of a New Engine for raising Water. By Walter Churchman, the Inventor of it. N° 434, p. 402.

In this engine, the horses draw horizontally in a straight line, and at right angles, by which they exert their utmost force. By these advantages a far greater power is gained from their strength, than by their going round in a circle; for by the twist and acuteness of the angles, they draw in towards the centre, which wastes their power, and also shortens their levers: besides, their muscles and tendons, from their hinder legs all along their sides to their necks, are unequally strained, as the duty is harder on one side, even though their walk be large. So that each of those inconveniencies must be attended with pain to the animals when at work, and a great loss of their strength.

A crank does not rise quite ⅓ of its circle; neither do the regulators or rods rise or fall perpendicularly, but obliquely, by which an oval figure is made by the piston's motion in every cylinder, which occasions great friction and a loss of water, and every arm of it is continually varying in its power while working, as its lever is distant from the perpendicular line, and two of the arms (supposing it a quadruple one) as they cross the perpendicular, are always drawing to and from their own centre; by which the power is not only lost, but the time also; and further, by the shortness of the strokes, all the adjacent water is

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* According to Mr. B Lagrange, who has recently analyzed ambergris (Ann. de Chimie, Vol. 47), the abovementioned precipitate consists of what is termed by the French chemists adipocire.

often contrarily moved, and by the frequent opening and shutting of the valves, there is also a great waste of the water, besides the many heavy bearings, frictions, surges, and repairs belonging to it; all which inconveniencies and impediments being thoroughly considered, there must certainly be required a much greater power to work the same, than by this new method. For here a stroke of 24 feet will rise, and by enlarging or diminishing the fixed wallower, a stroke of any height is obtained, even to the extent of the atmosphere's pressure. By this great advantage, the water rises freer, and with greater velocity; and as the lifters or forcers rise and fall exactly perpendicular, and with an equal continued strain: and as the bearings also are fewer and lighter, consequently the friction in all these will be a great deal less than with the crank, &c. And lastly, $\frac{7}{8}$ of that water which is always lost by the slow opening and shutting of the valves will be saved.

From the above considerations, and by the many experiments he has made on this occasion, in order to know the real difference between these different ways of working, Mr. C. finds, that near twice the quantity of water will be raised to the same height, in the same time, with the same power, by his method, more than with the best crank-work that has ever been yet erected.

In fig. 1, pl. 17, aaaa represent the great frame, the ends of which under the pine-apples are to be contracted to the place of the little frame; so that the cross piece at III may support the 3 bearings, now shown in the little one, for a better view only; bb the little frame on which are the cap-brasses, which receive the turned gudgeons T in the 3 horizontal shafts; cc the strong supporters by the loose wallowers; dd the loose wallowers, whose turned rounds geer truly with the cogs in the great wheel; eee the regulator, fig. 2, which has a circular, direct and retrograde motion; ff the strong shoulder or stud fixed to the shaft close by the wallower, which stops this loose wallower, when the end of the regulator comes against it, confining it for 2 revolutions; after which it quits this stud, and does the same on the opposite side of the wheel; and so on alternately, to reverse the motion of the stems in the different cylinders; gg the wheels, with their cogs, which alternately work the fixed wallower lying between them; h the fixed wallower, supposed to be 4 feet in diameter, on a very short shaft, whose rounds must be of cast soft iron, and truly turned, to elevate and depress the racks to the height of 24 feet by its 2 revolutions; iiii are the 4 lifters or forcers, behind each of which must be a small leverage back wheel, truly fitted to direct the same to rise and fall easily and exactly perpendicular, to avoid friction and loss of water in the cylinders; kk the large vertical wheel, a small segment of which comes through the floor in the dome for the 4 horses to stand and draw on; lm, fig. 3, the arms and the main shaft of the same; the arms lie horizontal, and the oval part is perpendicular; n the turned T gudgeon, with its collar and shoulder, both of which must clasp the rim of the under leverage wheel, to keep all firm and steady when in working; o the leverage wheel, about 4 feet in diameter, with a brass or iron rim supposed to be truly turned, and to have a strong short iron spindle through its centre, and at each end a turned steel collar and shoulder, bearing on 2 cast cap-brasses exactly level, and sunk into a strong arched piece of timber well braced and supported for this purpose. And here it is to be noted, that in large engines and machines, where the motion is regular, every heavy bearing should have one of these wheels: for, they save power by greatly abating friction. On the principle of these leverage-wheels, Captain Rowe has published what he calls his friction-wheels, though subsequent to Mr. Churchman's specification; pp two small side leverage wheels, exactly fitted to the turned part of the great gudgeon, between the collar and shoulder; they are to be placed and keyed in such manner, that their friction from the gudgeon may be alike when at work; qq the steps which the horses feet press, about 8 or 9 inches broad, 2 inches thick behind, and declining to an edge, being designed to make level ground, and good footing for their hinder legs when they draw; rr 4 horses only in view to avoid confusion, all drawing horizontally in a straight line, and at right angles, by which these useful animals will soon be taught a new and pleasant way of working to themselves, a more advantageous one to their masters, and of greater utility to the public; s the fastening places behind the horses, supposed to be strong arms below in the supporter, and a cross bar above, at both which may be placed small sheeves or rollers, the upper part of them to be level with each horse's breast when drawing, and the rope or strap to come over the same, in order to keep a weight of 300lb. more or less, suspended, an inch or two from a plank. By this method, we may be exactly informed of the strength of each horse, how long it continues, and when to relieve him; as also when justly to correct the slothful one, whose weight resting on the plank will always discover his laziness; t the fastening places before, which are designed to direct their heads; u the dome, merely for ornament, instead of which, erect a workloft, over that a horizontal windmill: on the lower end of its upright shaft fix a spur wheel, to work with the cogs of the great wheel, to assist the horses, or when there is a sufficient force of wind to do their whole duty: w the coupling staples with the brasses; x the strong catch which confines the great wheel to the frame; y the screw or key-band, to confine all close and tight; z the cylinders which are screwed together at their ends out of sight; and all the same sort of work chiefly for uniformity in the draught.

An Abstract of the Meteorological Diaries, communicated to the Royal Society, with Remarks on them. By W. Derham, D. D., F. R. S. Part 4, *containing Meteorological Observations made at Naples, Bengal, Christiana, in* 1727. N° 434, p. 405.

An Abridgment of the Meteorological Observations were made in the year 1727, at Naples, by Dr. Nic. Cyrillus, prim. med. profes. and at Bengal, by the Rev. Mr. Bellamy, chaplain to the English factory; and at Christiana in Norway, by ———, communicated by Mr. Pr. Kink. Extracted, for the use of the Royal Society, by W. Derham, F. R. S.

The 3d part of these observations were noticed in N° 433; and these in the 4th part are much of the same nature.

By the barometrical observations it appears, that the ascent and descent of the quicksilver is not so great at Naples, as in the more northerly climes: for it was but twice in the whole year, about 30 inches; and but thrice as low as 29.12 inches. And so in Phil. Trans. N° 321, it was observed, that at Zurich the range is only about an inch; but at Upminster the range is about 2½ inches: and by the account of the Petersburg observations in 1724, it appears that the mercurial range there is 3.31 inches, in the Phil. Trans. N° 424. As for Norway, the observations are too few, and all made only in the summer months, so that no good judgment could be made: and Bengal had no barometer.

At Naples, the rain of the whole year was 43⅔ inches. And to show how much wetter this year was than the others, the following quantities are stated, viz. of the year 1724, 34½ inches; of 1725, 34¾ inches; of 1726, 23⅔ inches.

Of the Dead Bodies of a Man and Woman, preserved 49 *Years in the Moors in Derbyshire. By Dr. Charles Balguy of Peterborough.* N° 434, p. 413.

These two persons were lost in a great snow on the moors, in the parish of Hope, near the woodlands in Derbyshire, Jan. 14, 1674; and not being found till the 3d of May following, the snow lasting probably the greatest part of that time, they then smelt so strong, that the Coroner ordered them to be buried on the spot. They lay in the peat-moss 28 years 9 months, before they were looked at again, when some countrymen, having observed the extraordinary quality of this soil in preserving dead bodies from corrupting, were curious enough to open the ground, to see if these persons had been so preserved, and they found them no way altered, the colour of their skin being fair and natural, their flesh soft as that of persons newly dead. They were afterwards exposed for a sight 20 years, though they were much changed in that time, by being so often uncovered; and in 1716, their condition was as follows: viz. The man perfect, his beard strong, and about a $\frac{1}{4}$ of an inch long, the hair of his head short, his skin hard and of a tanned-leather colour, pretty much the same as the liquor and earth they lay in. The woman, by some rude people had been taken out of the ground, to which one may well impute her greater decay; one leg was off, the flesh decayed, the bone sound, the flesh of one hand decayed, the bone sound; on her face, the upper lip and the tip of her nose decayed, but no where else. Her hair was long and springy, like that of a living person. They were afterwards buried in Hope church, where viewing them some time after, it was found they were entirely consumed.

They had lain about a yard deep in the soil or moist moss, but without any water in the place. When their stockings were drawn off, the man's legs, which had never been uncovered before, were quite fair; the flesh, when pressed with the finger, pitted a little, and the joints played freely, and without the least stiffness: the other parts were much decayed: what was left of their clothes (for people had cut away the greatest part as a curiosity) was firm and good; the woman had on a piece of new serge, which seemed never the worse.

An Account, by Dr. Richard Middleton Massey, of a Book, entitled Locupletissimi Rerum Naturalium Thesauri accurata Descriptio, &c. Vol. I. Amstel. 1734, *in Fol. An exact Description of the principal Curiosities of Nature, in the large Museum of Albertus Seba, F. R. S. Vol. I. Amsterdam,* 1734. N° 434, p. 415.

This magnificent work is to consist of 4 large folio volumes. The ingenious, curious, and diligent collector, takes in all parts of natural history, and gives descriptions and figures of things scarcely ever seen or heard of before in Europe, which he has collected from all parts of the world, at great charge and industry.

The first volume contains 111 plates, besides the author's portrait, and the decorations curiously engraven. He begins with the anatomy and skeletons of several fruits, leaves, and roots: the method of performing which, is printed in the Phil. Trans. N° 416. He then gives a description of several curious exotic plants, with a particular account of the zagoe amboynensium, morus papyrifera, &c. After these follow a great variety of different sorts of animals from all parts of the world. A description of the pipal, a sort of toad, whose young are produced on the back of the female. An account of the transformation of frogs from fishes, and back again from frogs to fishes. Several kinds of scarce lizards, iguanas, chamæleons, &c. A dragon or basilisk from America, with about 50 several sorts of serpents.

On Ambergris. By Casper Neuman, M. D. Professor of Chemistry at Berlin, and F. R. S. Part III. N° 435, p. 417.

The substance of this 3d part is incorporated with the first part at p. 661 of this Vol. of these Abridgments.

The Editor's Account, with Observations, of Experiments on Ambergris, made by Mr. John Browne, F.R.S. and by Mr. Ambrose Godfrey Hanckewitz, F.R.S. To which are subjoined Dr. Neuman's Vindicatory Remarks. N° 435, p. 437. *From the Latin.*

At the request of the Society Messrs. Browne and Godfrey Hanckewitz repeated Dr. N.'s experiment on the distillation of ambergris.

Mr. Browne took ℥iss of ambergris, and reduced it to a powder with some terra cimolia alba, such as he always used in the distillation of sal succini. This he put into a retort, and subjected to various degrees of heat. He obtained first, a phlegm, as clear as the purest water; next a pale brown coloured spirit; then an oil of a deeper brown colour; and lastly when the heat was very strong, a thick black balsam. Although this oil and balsam had the smell of amber (succinum), yet Mr. B. could not extract from them any volatile acid salt, like that which amber (succinum) yields; nor did the spirit of ambergris produce an effervescence with alkalies, as the acid spirit of amber does. It is this volatile acid* which Mr. B. conceives to be the true criterion or characteristic of amber (succinum). A hard jet black residuum was left, different from that which remains after the distillation of ambergris.

Mr. Godfrey distilled ambergris with twice its weight of the finest white sand, and obtained a limpid oil and a bituminous residuum. The oil rectified per se, gave a phlegm which had a pleasant subacid taste, like weak vinegar; this was followed by a limpid balsamic oil, resembling petroleum. He afterwards distilled ʒss of ambergris per se, and obtained the same results with a moderate heat; when the ambergris was distilled to perfect dryness, he urged the fire, and there remained at the last 3 grs. of a white saline earth, which effervesced with acids, and deliquesced on exposure to the air. As he could not obtain from the black residuum either any volatile alkali or phosphorus, he infers that ambergris is not an animal substance, and particularly that it is not an animal excrement; for phosphorus (as he had shown in the Phil. Trans. N° 428) may be extracted from the excrements of all animals. He concludes that ambergris is a bitumen very nearly allied to amber (succinum), but not a true amber, since it does not yield a volatile acid salt, as amber does. Mr. G. repeated the distillation of ambergris with equal parts of pounded glass; and the result was the same, except that the phlegm had the taste rather of a neutral salt than of an acid.

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* Described by subsequent chemists under the name of the succinic acid.

On these experiments, the editor of the Transactions remarks, that all the differences attending them may be reconciled by reflecting, that ambergris is a substance which contains various foreign admixtures. Hence different specimens of ambergris contain different quantities of the acid salt. Thus in one of Mr. Godfrey's experiments, the phlegm had a subacid taste, clearly indicating the presence of that salt, and in another experiment the phlegm tasted like a neutral salt; whilst the specimen examined by Dr. Neuman yielded a much larger quantity of this salt. He further remarks that the more the salt is enveloped in the oil, the more difficult its separation. Besides, Dr. N. in one of his letters to Dr. Sloane, declares that he never meant it to be understood that he supposed ambergris to be the same as amber (succinum), but merely that it was a bitumen very much allied to amber. With regard to the acid volatile salt, of which he obtained a grain or two in his experiment, he could not (he says) be deceived as to the nature of it; for it was soluble in water like any other salt; it reddened syrup of violets like other acids; and it rose in distillation, a proof that it was volatile.

An Account of Mr. T. Godfrey's Improvement of Davis's Quadrant, transferred to the Mariner's-Bow. Communicated by Mr. J. Logan. N° 435, p. 441.

Thomas Godfrey, having under the greatest disadvantages, made himself master of the principles of astronomy and optics, as well as other parts of mathematical science, applied his thoughts to consider the instruments used in navigation. He saw that on the knowledge of the latitude and longitude of the place a ship is in, the lives of thousands of useful subjects, as well as valuable cargoes, continually depend; that for finding the first of these, certain and easy methods are furnished by nature, if observations be duly made: but Davis's quadrant, the instrument generally used by British navigators, he perceived was attended with this inconveniency, that the observer must bring the shade or spot of light from the sun, and the rays from the horizon, to coincide exactly on the fiducial edge of the horizontal vane: that though this can be done in moderate weather and seas, with a clear sky, and when the sun is not too high, without any great difficulty; yet in other cases it requires more accuracy than can in some junctures possibly be applied, and more time than can be allowed for it. In European latitudes, or to those nearer the northern tropic, when the sun is in the southern signs, and near the meridian, he rises and falls but slowly: yet in voyages to the East and West Indies, of which many are made, he is at noon often, and for many days together, in or near the zenith, and when approaching to, or leaving it, he rises and falls, when he has declination, faster than even at the horizon; for it is well known to persons acquainted with the sphere, that when his diurnal course takes the zenith, he there rises and falls a whole degree or 60 minutes, in the space of 4 minutes of time; so that the observer has but one minute, to come within 15 minutes of the truth in his latitude: while in a middle altitude, as 45 deg. he is at noon above 5¼ minutes in rising or falling one single minute of space, the odds between which is more than 80 to 1. And yet perhaps no parts of the world require more exactness in taking the latitude, than is necessary in voyages to the West Indies: for it is owing to the difficulty of it, that vessels have so frequently missed the island of Barbadoes, and when got to the leeward of it, have been obliged to run down a thousand miles farther to Jamaica, from whence they can scarcely work up again in the space of many weeks, against the constant trade winds, and therefore generally decline to try for, or attempt it.

But further, as the latitude cannot be found by any other method, that our mariners are generally acquainted with, than by the sun or a star on the meridian: in a cloudy sky, when the sun can but now and then be seen, and only between the openings of the clouds for very short intervals, which those who use the sea know frequently happens: as also in high tempestuous seas, when though the sun should appear, the observer can scarcely by any means hold his feet; it would certainly be of great advantage to have an instrument by which an observation could also be, as it were, snatched or taken in much less time, than is generally required in the use of the common quadrant.

Tho. Godfrey therefore considering this, applied himself to find out some contrivance, by which the necessity of bringing the rays from the sun, and those from the horizon to coincide, which is the most difficult part of the work, on one particular point or line from the centre, might be removed. In order to which, he considered, that by the 21. 3d Elem. of Eucl. all angles at the periphery of a circle, subtended by the same segment within it, are equal, on whatever part of the circumference the angular point falls; and therefore, if instead of a quadrant, a semicircle were graduated into 90 degrees only, accounting every two degrees but one; this would effectually answer: for then, if an arch of the same circle were placed at the end of the diameter of the instrument, every part of that opposite arch would equally serve for taking the coincidence of the rays abovementioned. But such an instrument would manifestly be attended with great inconveniencies; for it would in great altitudes be much more unmanageable, and the vanes could not be framed to stand, as they always ought, perpendicular to the rays. He therefore further resolved to try, whether a curve could not be found, to be placed at the centre of a quadrant, which would, at least for a length sufficient to catch the coincidence of the rays, with ease fully answer the intention.

A curve which in all the parts of it would in geometrical strictness effect this, cannot be in nature, any more than that one and the same point can be found for a centre to different circles, which are not concentric. It is certain that every arch on the limb may have a circle that will pass through the centre, and be a locus or geometrical place for the angle made by that arch to fall on: but then every arch has a different one from all others; as in fig. 10, pl. 16. Let ABC be the quadrant, and AB, EF, GH be taken as arches of it: circles drawn through each two of these respectively, and through the centre C as a third point, will manifestly be such loci or places: for every pair of these points stands in a segment of their own circle, as well as on a segment of the quadrant; and therefore by the cited 21, 3d elem. the angles standing on these first segments will every where be equal at the periphery of their respective circles, and their radius will always be equal to half the secant of half the arch on the quadrant. For in the circle CEDF, for instance, the angle CED is right, because in a semicircle, CE is the radius of the quadrant, ED the tangent of the angle DCE = $\frac{1}{2}$ the arch EF, and CD is the secant of the same = the diameter of the circle CEDF, and therefore its radius is half that secant.

Now from the figure it is plain, that in very small arches the radius of their circular locus will be half the radius of the quadrant, that is, putting this radius = 10, the other will be 5. And the radius for the arch of 90, the highest to be used on the quadrant, will be the square root of half the square of the radius = sine of 45 degrees = 7.071, and the arches at the centre drawn by these two radii are the extremes, the medium of which is 6.0355. And if a circular arch be drawn with this radius $\frac{1}{20}$ part of the length of it, that is, in an instrument of 20 inches radius, the length of one inch on each side of the centre affording 2 inches in the whole, to catch the coincidence of the rays on, which must be owned is abundantly sufficient, the error at the greatest variation of the arches, and at the extremity of these 2 inches, will not much exceed one minute.

But in fixing the curvature or radius of this central arch, something further than a medium between the extremes in the radius, is to be considered: for in small arches, the variation is very small, but in greater it equally increases, as in the figure, where it appears, the difference between the angles ABC and ADC, fig. 11, is much greater than the difference between EBC and EDC, though both are subtended by the same line BD: for their differences are the angles BAD and BED. Therefore this inequality was likewise to be considered; and compounding both together, Tho. Godfrey pitched on the ratio of 7 to 11, for the radius of the curve to the radius of the instrument, which is 6.3636 to 10. But on further consideration, he now concludes on $6\frac{4}{10}$; and a curve of this radius of an inch on each side of the centre, to an instrument of 20 inches radius, or of $\frac{1}{20}$ of the radius, whatever it be, will in no case, as he has himself carefully computed it, produce an error of above 57 seconds; and it is well known that navigators in their voyages entirely slight a difference of one minute in latitude.

This radius is the true one for the circular locus to an arch of 77° 15′, and the variation from it is nearly as great at 90 degrees as at any arch below it, the greatest below being at about 44 degrees, which is owing to the differences expressed by the last figure above, and not to those of the curvatures or circular loci. Yet this variation of 57 seconds arises only when the spot or coincidence falls at the extremity of the horizontal sight or vane, or a whole inch, in an instrument of 20 inches radius, from the centre, and then only in the altitudes or arches of about 44 or 90 degrees. And in these, at the distance of half an inch from the centre, the variation is but $\frac{1}{4}$ so much, viz. about 14″; and at $\frac{1}{4}$ of an inch, not 4″; at the centre it is precisely true. Therefore as an observation may be taken with it in one fourth of the time, that Davis's quadrant, on which 3 things must be brought to meet, in a general way requires: I say, considering this, and the vast importance of such dispatch, in the case of great altitudes, or of tempestuous seas, or beclouded skies, it is presumed the instrument thus made, will be judged preferable to all others of the kind yet known. Some masters of vessels, who sail from hence to the West Indies, have them made as well as they can be done here; and have found so great an advantage in the facility and in the ready use of them, in those southerly latitudes, that they reject all others. And it can scarcely be doubted, but when the instrument becomes more generally known, it may on the Royal Society's approbation, if it appear worthy of it, more universally obtain in practice.

It is now 4 years since Tho. Godfrey hit on this improvement; for his account of it, laid before the Society last winter, in which he mentions two years, was written in 1732. And in the same year, 1730, after he was satisfied in this, he applied himself to think of the other, viz. the reflecting instrument by speculums, for a help in the case of longitude, though it is also useful in taking altitudes, and one of these, as has been abundantly proved by the maker, and those who had it with them, was taken to sea, and there used in observing the latitude, the winter of that year, and brought back again hither before the end of February, 1730-1. It was unhappy indeed, that as he had no acquaintance nor knowledge of persons there, that no account of it was transmitted sooner. But it was owing to an accident which gave Mr. Logan some uneasiness, viz. his attempting to publish some account of it in print here, that he did it at that time, viz. in May 1732, when he transmitted it to Dr. Halley; to whom he made not the least doubt but the invention would appear entirely new.

The bow had best be an arch of about 100 degrees, well graduated, and numbered both ways; the radius of 20 or 24 inches; the curve at the centre to be $\frac{1}{20}$ of the radius on each side, that is, $\frac{1}{10}$ of it in the whole; the radius of that curve $\frac{64}{100}$ parts of the radius of the instrument; that the glass for the solar vane should not be less, but rather larger, than a silver shilling, with its vertex most exactly set. And that the utmost care be taken to place the middle of the curve at the centre exactly perpendicular to the line or radius of 45 degrees. As the observer must also take care that the two vanes on the limb be kept nearly equidistant from that degree; and it may be best to give the horizontal vane only one aperture.

Note, That the radius of the quadrant being divided into 20 equal parts, the centre ×, in fig. 12, of the curvature of the horizon-vane AB, must be $12\frac{[illegible]}{10}$ of those parts from the centre c of the quadrant. The breadth, AB or gh, of that vane, should be $\frac{1}{10}$ of the whole radius, that is, $\frac{1}{20}$ on each side of the centre c.

The Description and Use of an Instrument for taking the Latitude of a Place at any Time of the Day. By Mr. Rich. Graham, F. R. S. N° 435, p. 450.

The necessity of finding the latitude a ship is in, is too well known to be insisted on: frequent opportunities of observing the latitude must consequently be of very great advantage to navigation. The method usually practised, is by taking the sun or stars meridian altitude or zenith distance: in this case, if the sun does not shine but for some small time only, before noon and after, though it be clear all the rest of the day, it is of no use for this purpose. In 1728, Mr. Fatio proposed a method for finding the latitude, from two or more observations of the sun or stars, at any time, the distance of the said observations, in time, being given by a watch; but as his method requires a vast number of computations, and a good skill in spherical trigonometry, it has very seldom been made use of, and never but by good mathematicians. The instrument here described will answer the same end, and has these advantages: 1st. It may be easily understood by seamen. 2dly, It immediately shows the latitude of the place. 3dly, It gives the time of day at sea, when no other instrument can. 4thly, It may be made as large, and consequently as accurate, as is desired.

A Description of the Instrument is as follows.—ABC, fig. 4, pl. 17, represents part of the hemisphere of a large globe (half the globe, and the part below the tropic are cut off, that it may take up the less room.) AC, half the equator, divided into 12 hours above, and 180 degrees below, and subdivided into minutes, as is likewise the lower tropic DD; EE a moveable graduated meridian, turning on the axis FF; G an index to fix it to any hour, by the means of the screw H. II a circular beam-compass, the centre I to be fixed on the meridian to any degree and minute of declination, by the method commonly called Nonius's divisions: k the point for drawing arches, which is likewise fixed to any degree and minute by the same method. As the meridian is at some distance from the globe, L is a piece of brass to fix on the meridian, marked with Nonius's divisions, with a point reaching down to the intersection of the arches, by which means the distance of the said intersection from the equator, or its latitude, is found. The degrees and minutes may likewise be shown by diagonal lines.

The Use of the Instrument is as follows.—PROP. 1. From two observations of the height of the sun, and the distance of the said observations in time, being given by a watch, as also the declination of the sun; to find the latitude of the place, and the hour of the day.

1. When the ship is at rest, that is, at anchor, or in a calm, so as to have little or no progressive motion.

Case 1.—Suppose the sun in the equator on the day of observation: fix the centre of the beam-compass at 0 degree, or at the equator, and move the point k to the zenith distance, the complement of the altitude taken by the usual instruments, and from any hour, as from c, describe an arch of a circle with the said point, as bc, Ex. 1. Suppose 8 hours after, by the watch, you have another observation; move the meridian 8 hours farther, to d, and fix it there; and with the zenith distance then observed describe another arch, as ef; the point where it cuts the former, is the place of observation, and its distance taken on the meridian from the equator, shows its latitude; and the minutes reckoned on the equator, from the meridian to c and d, the times of observation, show what those hours were.

Case 2.—When the sun has declination: fix the centre of the beam-compass on the meridian, to the proper degree of declination for the day of observation, and proceed as before.

Case 3.—When the observations are at a greater distance than 12 hours, but in the same day: make use of the complement to 24 hours of the distance in time, and take the declination on the contrary, or lower side, of the equator; and instead of the zenith distances, take the nadir distances or altitudes increased by 90 degrees.

Thus you will find the latitude, and the time of each observation from midnight. In this case the beam-compass must extend to more than 90 deg.

Case 4.—When the observations are more than a day asunder; as for instance a day and 2 hours, or 26 hours: place the centre of the beam-compass 2 hours farther than it was the day before; but in different declinations, according to the table of declination for the several days.

Case 5.—When the observations are made by a star: the centre of the beam-compass must be set to the declination of the star; then proceed as before. To find the hour in this case, the right ascension must be likewise given.

Scholium.—The same method may be useful at land, when no meridian observation offers.

II. *When the Ship is in Motion.*—*Case* 1. Suppose the sun in the equator: the distance between the two observations 8 hours, as before, and the arch aaa, Ex. 2, described by the zenith distance of the first observation from the centre c, and the angle cab, 40°, is the angle between the ship's way and the azimuth of the sun continued, given by the azimuth compass; and that during the 8 hours the ship has made one degree, or 60 minutes, from a to b, or from the sun; then, as radius is to the cosine of cab, 40°, so is ab, 60′ to ac 46′; add 46′ to the zenith distance ca; and with k, the point of the beam-compass set at that distance, describe the arch cbe; then with the zenith distance of the last observation, whose centre is d, draw the arch ff; the point where it cuts the arch cbe, is the place where the ship was last; and its distance, taken on the meridian from the equator, shows its latitude; the minutes reckoned on the equator from the meridian to d, the time of the last observation, show the hour, or its distance from 12 o'clock.

Case 2. If the ship had sailed from α to β, or towards the sun: the cosine of the angle $\beta\alpha\gamma$, or of the angle between the ship's way and the sun, must be subtracted from the zenith distance of the first observation.

N. B. Only the two arches cbe, ff, are to be drawn on the globe, the rest being added here, to show the reason of the construction.

Case 3. To find the latitude of the first place, from the equator, with a pair of compasses, take the distance sailed 60′; and with one foot in the intersection of the arches be, ff, the place found before, put the other in the arch aaa, the zenith distance of the first observation, and in this instance, on the left hand of the azimuth of the sun, this is the place sought; and its distance taken on the meridian from the equator, shows the latitude; and the minutes, reckoned on the equator, from the meridian to c, the time of the first observation, show the hour. The interval, in time or degree, between the two places, shown by the index G, is the difference of longitude. Those observations are best, whose arches cross each other almost at right angles.

PROP. II.—*The zenith distances of two stars observed at the same time, their declination, and right ascension being known; to find the latitude of the place of observation.*—Fix the centre of the beam-compass to the declination of either of the stars, and with the zenith distance of that star describe an arch; move the meridian as many hours farther as is the difference of right ascension of the other star; and fix the centre of the beam-compass to its declination, and with its zenith distance cross the first arch; the intersection shows the latitude of the place of observation, and also the distance of the right ascension of the zenith from that of either of the stars; by which means the hour may be known.

When a celestial globe is used, then place the centre of the beam-compass over the several stars. The latitude and hour being given, the variation of the compass is easily known.

An Abstract of the Meteorological Diaries communicated to the Royal Society, with Remarks upon them. By W. Derham, D. D. F. R. S.* N° 435, p. 458.

Part 5, containing meteorological observations made at Hall in Saxony, 1729; at Goslar, Wittemberg, Naples, Southwick; in Sweden, viz. Lunden, Swenæker, Risinge, Bettna, Upsal, Hudiskswald, Hernœsand, Bygdea, in 1728.

Here are contained tables of the greatest, the least, and the medium heights, both of the barometer and thermometer, for several places. Also the rain at Southwick and at Naples.

It appears from the highest and lowest stations in 6 years, that the greatest range of the barometer at Padua is 1.84 inches; but at Naples, it is only 94 centesimals of an inch.

That the magnetical declination at Padua is 13° west, and has decreased in 6 years $\frac{10}{60}$; that every day there is a small alteration in the declination, so that it does not continue the same a whole day together; that the declination of all needles, especially if touched by different magnets, is different a few sexagesms.

A gentleman going from Dr. Derham's house, saw towards the east, about 30° high, a ball of fire, about 4 inches diameter, blazing and standing still at first, and presently after, it ran towards the north, and in about 5 minutes he heard an explosion like thunder. Its blaze emitted a light equal to that of the moon at full. At the same time, the newspapers say, a light in the sky, like a comet, was seen at Watford in Hertfordshire, with sparks of fire issuing from its tail; that then it broke out with a prodigious lustre, like the sun, which lasted not long, and was followed with a terrible clap of thunder, the stars twinkling all the while, and not a cloud to be seen. Which clap was probably the same as that above heard, and which was about 5 minutes in its passage hither.

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* See part 4 in N° 434.

END OF VOLUME THIRTY-EIGHTH OF THE ORIGINAL.

END OF VOLUME SEVENTH.

Erratum. In page 276, *for* fig. 1, pl. 6, *read* fig. 10, pl 5.

C. and R. Baldwin, Printers,
New Bridge-street, London.

Fig. 1.

Fig. 2.

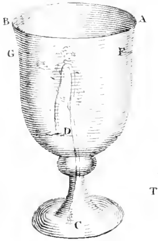


Fig. 4.

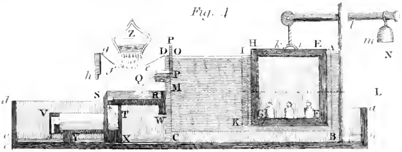


Fig. 5.

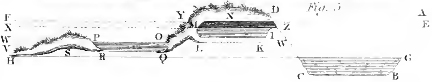
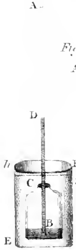


Fig. 3.

Fig. 7.



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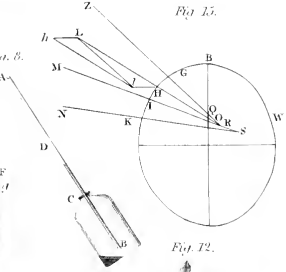


Fig. 9.

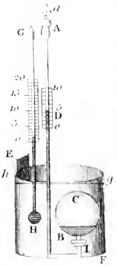


Fig. 10.

Fig 11.

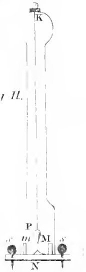
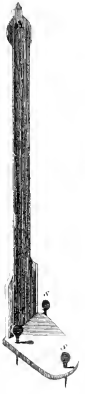


Fig. 8.

Fig. 6.

Fig. 12.



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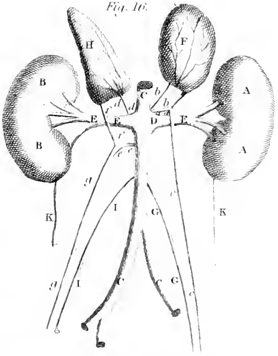

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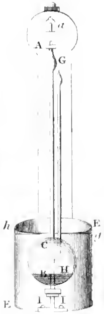
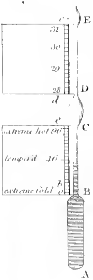


Fig. 14.



Mutlow sc. Russel Co.

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Fig 1

Fig 2

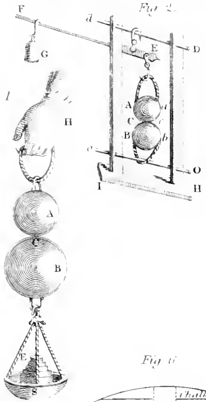


Fig 5

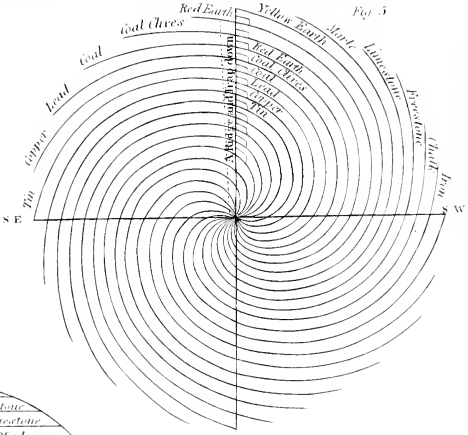
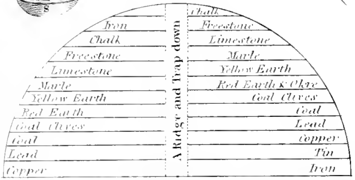
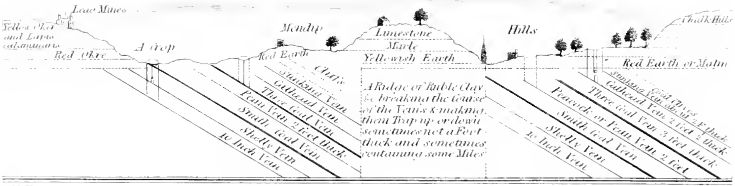


Fig 6



A Section of a Coal Country in Somersetshire about 20 Miles from South East to North West

Fig 3.



N E

S W

Fig. 4

A Section of the same Country from North East to S W on the Level of the Coal and at right Angles with the former

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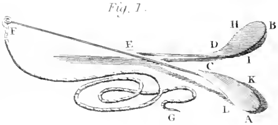
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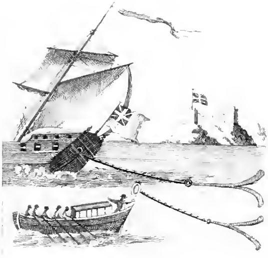
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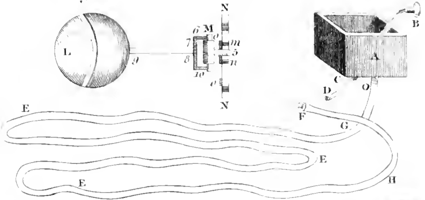
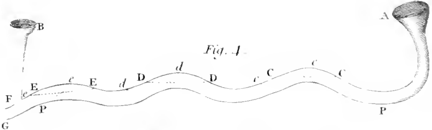


Fig. 8.

Fig. 3.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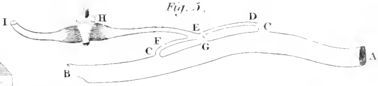
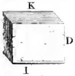


Fig. 5.



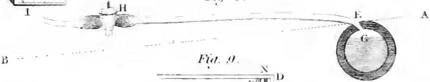
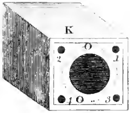


Fig. 6.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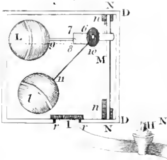
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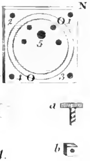
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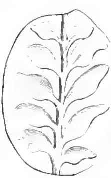
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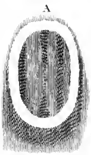
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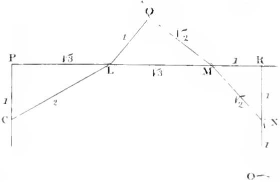
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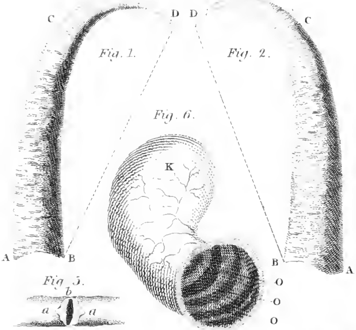
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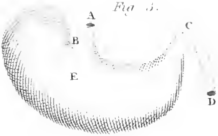
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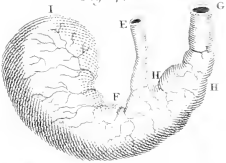
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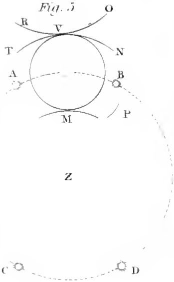
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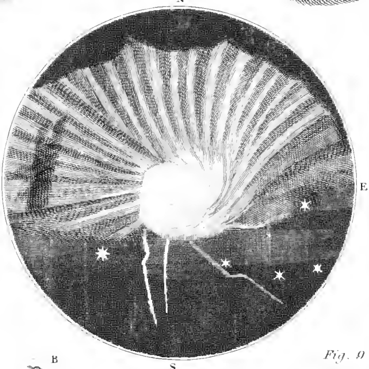

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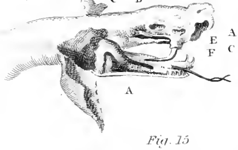

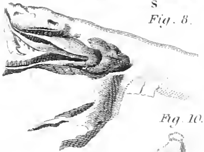
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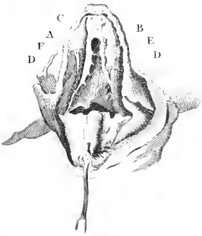

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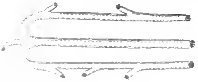

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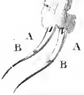

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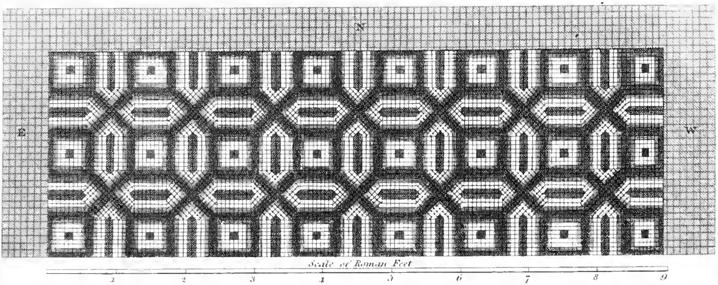

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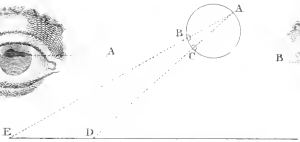

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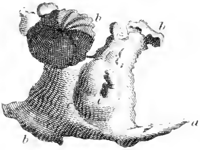

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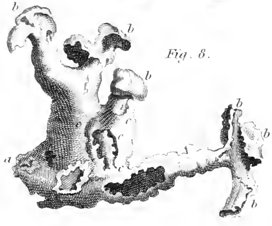

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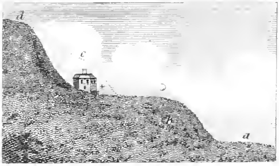

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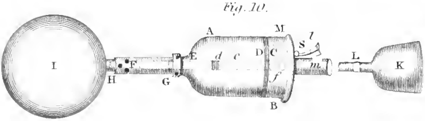

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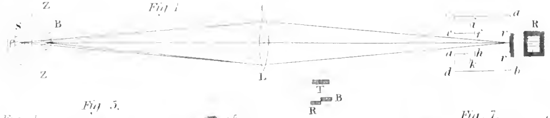

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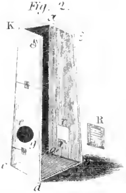

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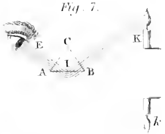

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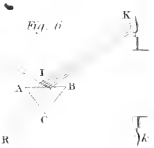

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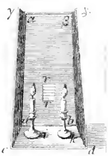

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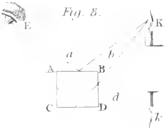

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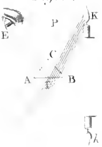

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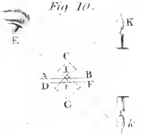

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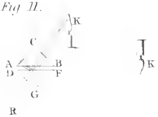

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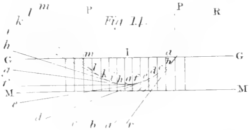

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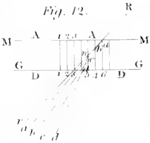

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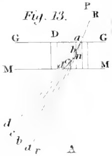

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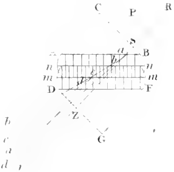

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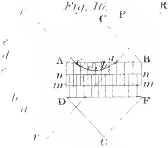

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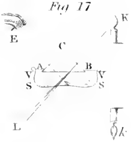

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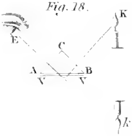

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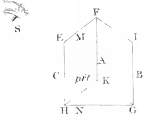

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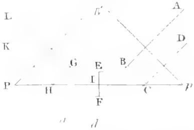

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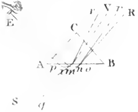

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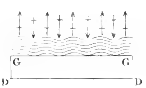

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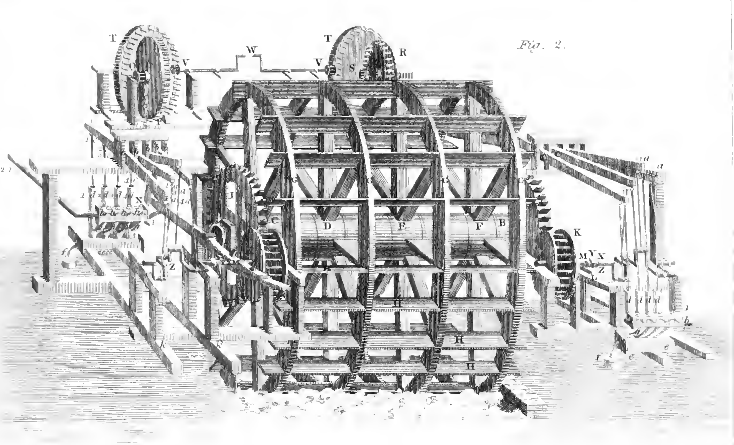

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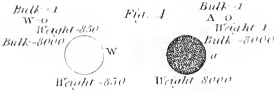

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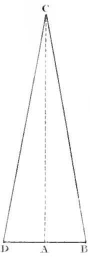

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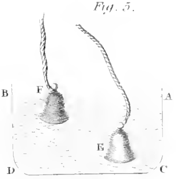

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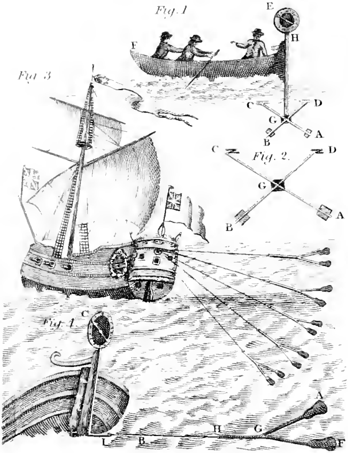
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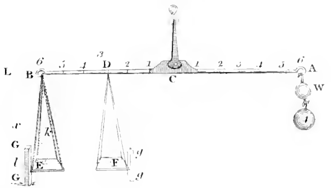

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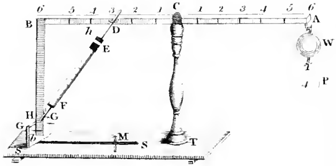

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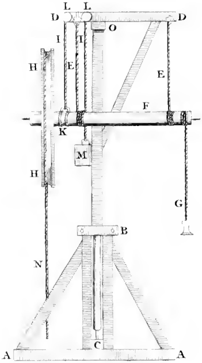

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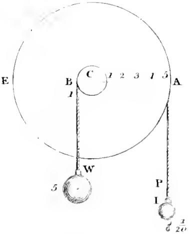

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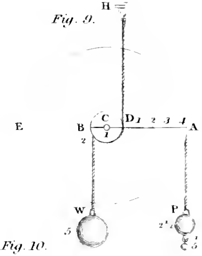

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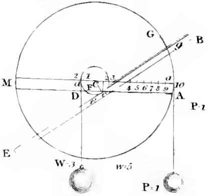

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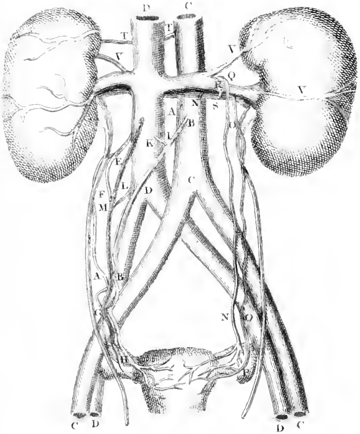

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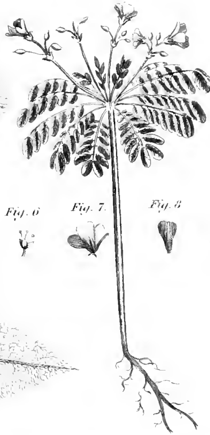

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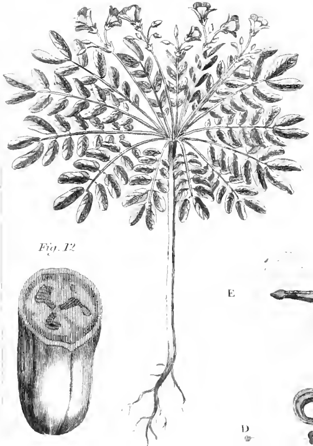

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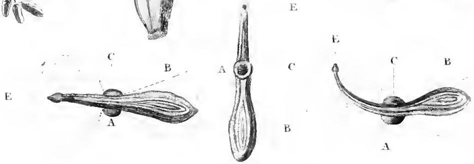

Fig. 13.

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Fig. 10.

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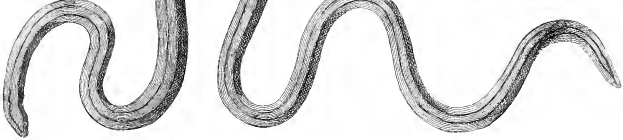

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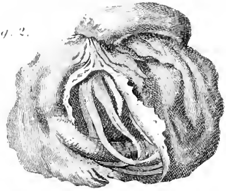

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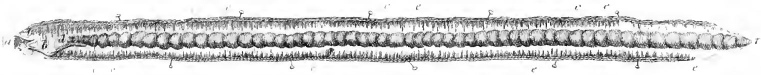

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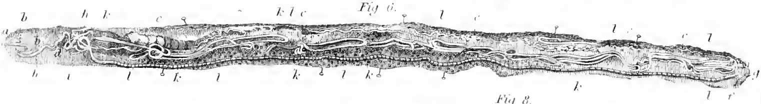

Fig. 7.

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Fig. 2.

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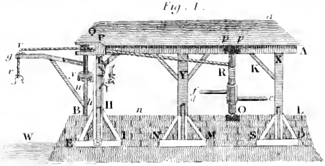

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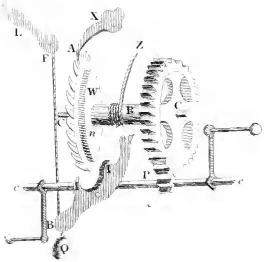

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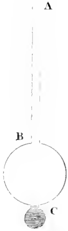

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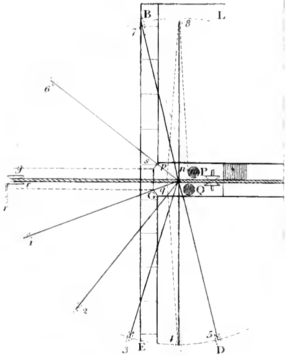

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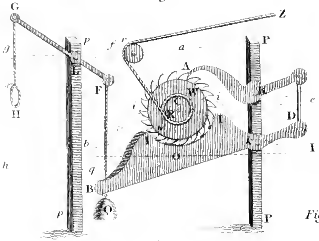

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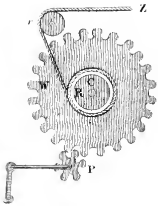

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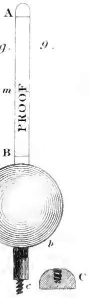

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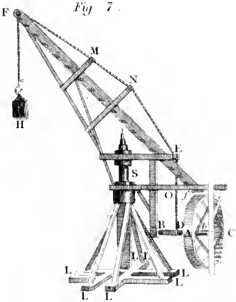

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Fig. 2

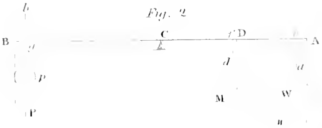


Fig. 1

Fig. 4

Fig. 3

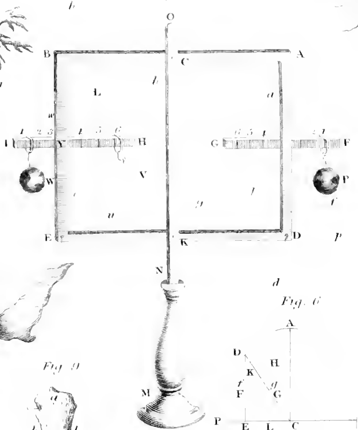
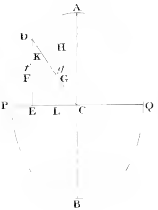


Fig. 5

Fig. 6



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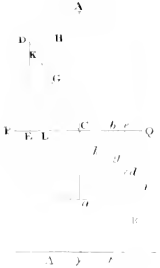
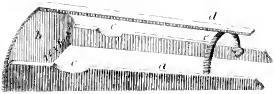


Fig. 9

Fig. 8



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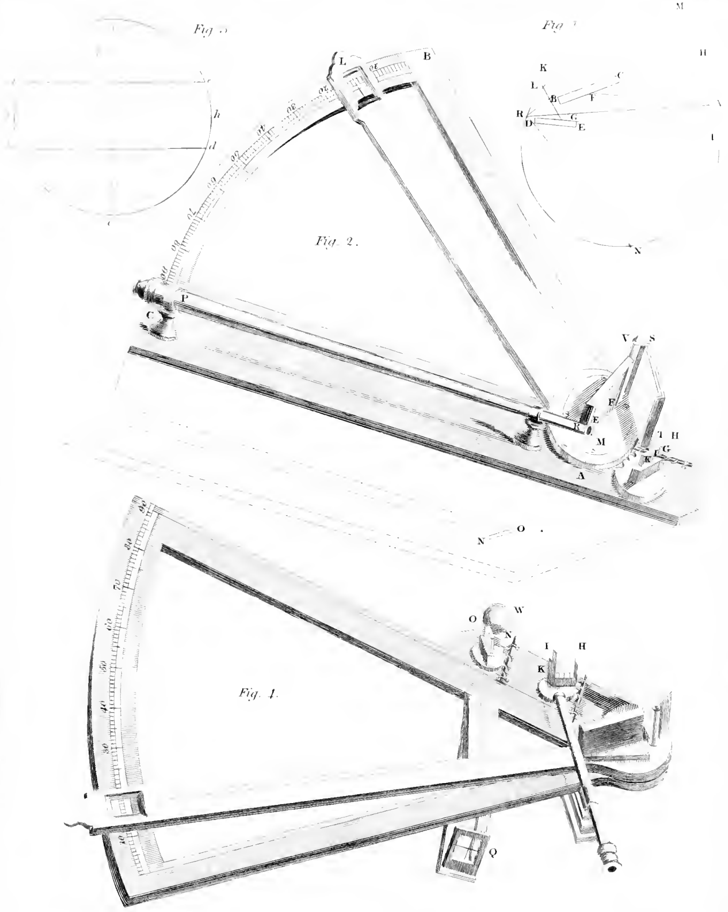

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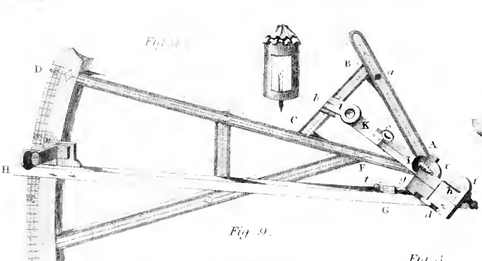


Fig 2.

Fig. 1

Fig. 5.

Fig 6.

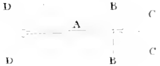
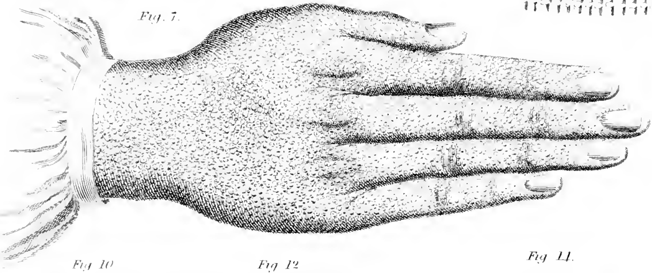


Fig 9.

Fig 3.

Fig 8.

Fig. 7.



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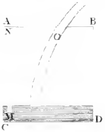

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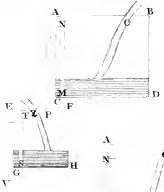

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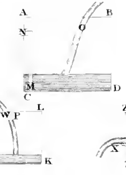

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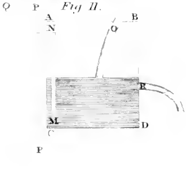

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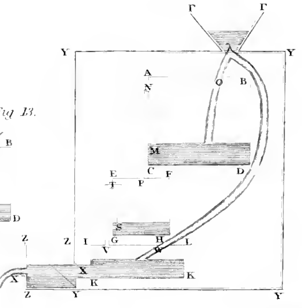

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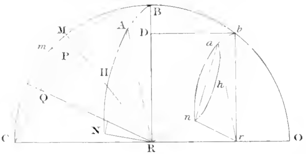
Fig 1



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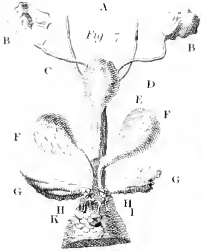


Fig 2

Fig . 5 .

Fig 3

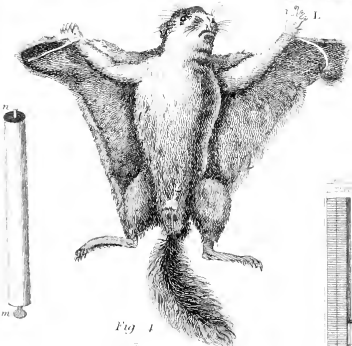
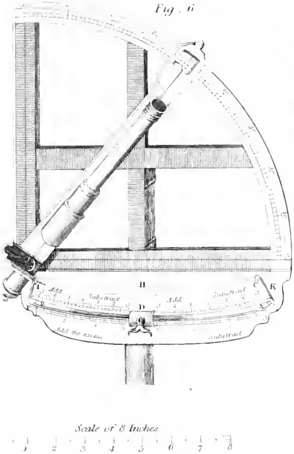


Fig 4

Fig . 6



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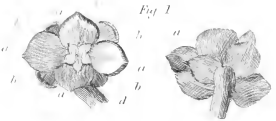

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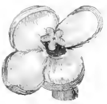

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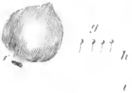

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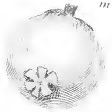

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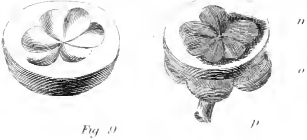

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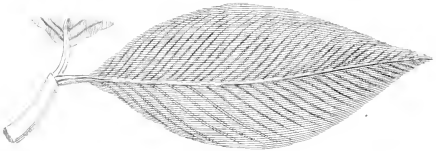

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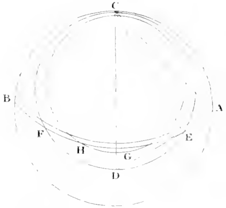

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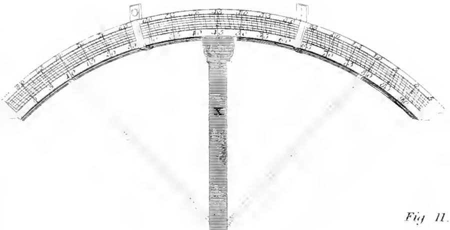

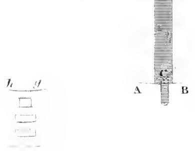

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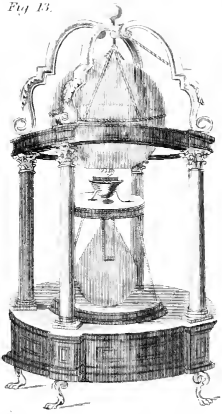

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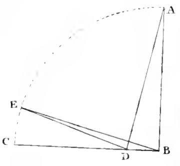

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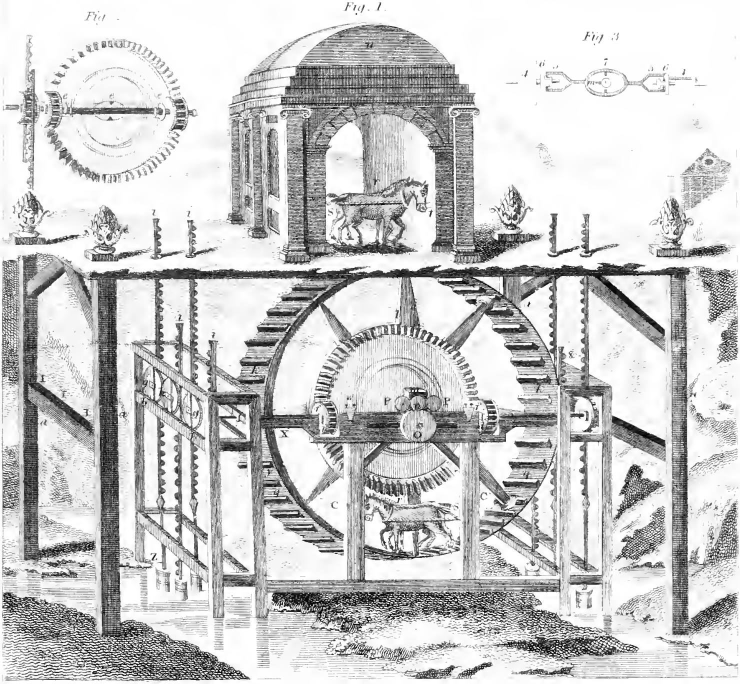

Fig. .

Fig. 1.

Fig. 3

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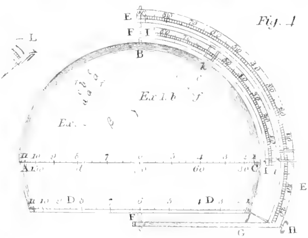

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