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PHILOSOPHICAL
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Giving Some

ACCOUNT

OF THE

Present Undertakings, Studies and Labours

OF THE

INGENIOUS,

In Many

Considerable Parts of the WORLD.

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L O N D O N:

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TO
Sir Isaac Newton, Kt.
PRESIDENT,
AND TO THE
COUNCIL and FELLOWS
OF THE
Royal Society of London,
FOR
Improving Natural KNOWLEDGE,
This THIRTY FIRST Volume
OF
Philosophical Transactions
Is Humbly Dedicated
BY
Their most Obliged,
and most Obedient Servant,
James Furin, R. S. Secr.

TO

Sir James Watson, Kt.

PRESIDENT

AND TO THE

COUNCIL AND FELLOWS

OF THE

Royal Society of London

FOR

Improving Natural Knowledge

THE THIRTY FIRST VOLUME

OF

Philosophical Transactions

A Weekly Journal

BY

THE SOCIETY

AND BY OTHERS

James Smith, R. S. Secy.



Fig: 14.

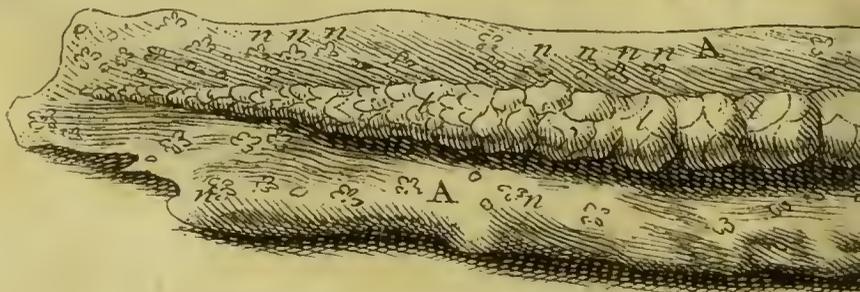
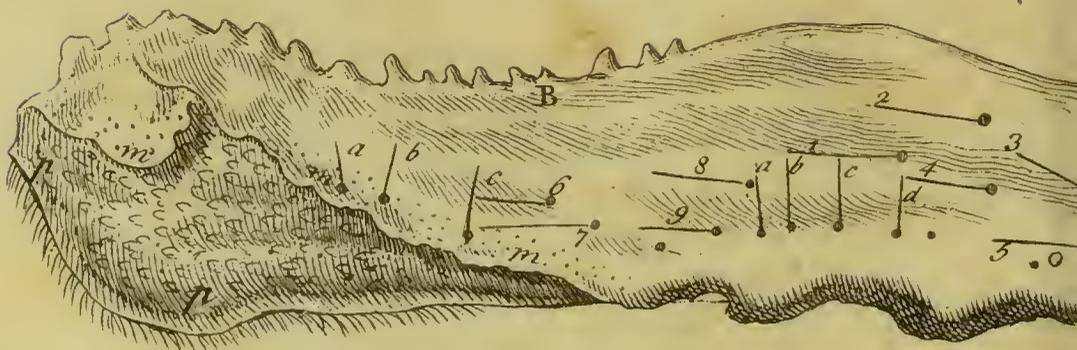
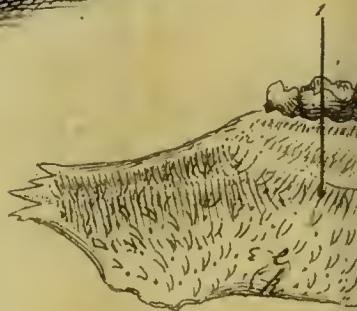
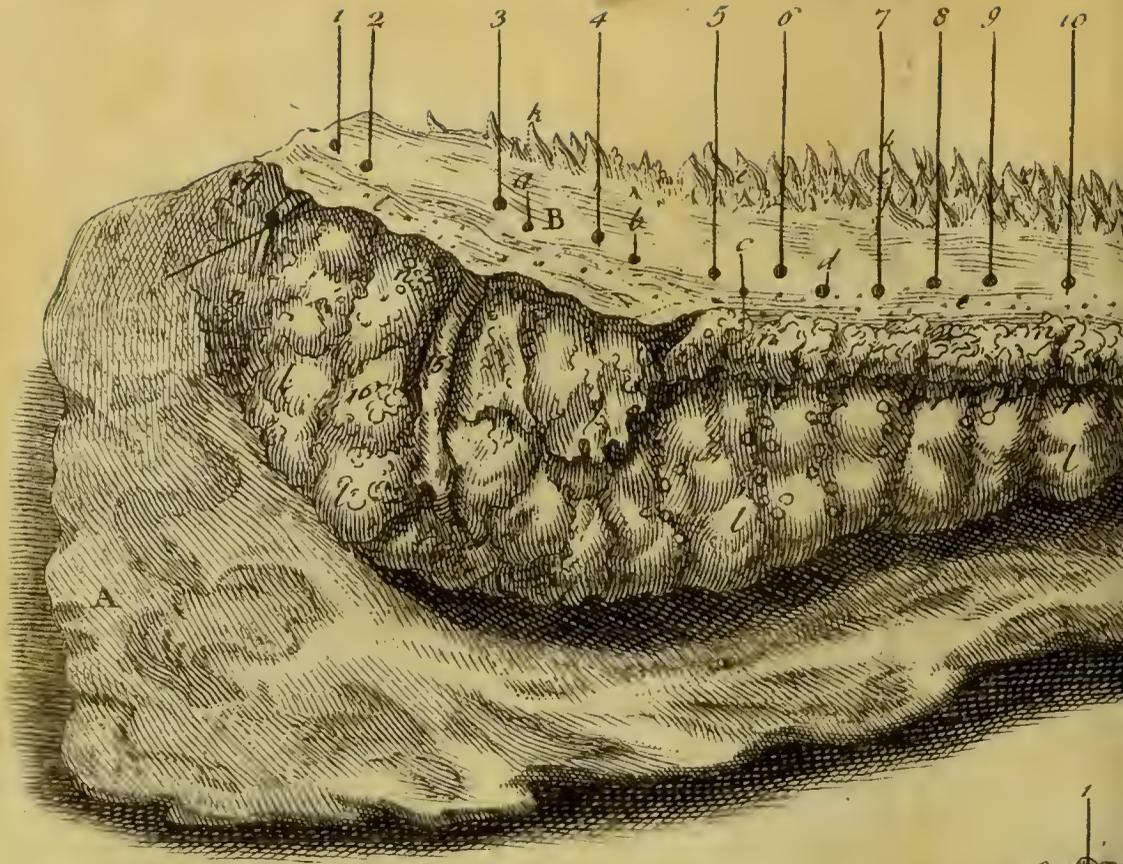


Fig: 17.

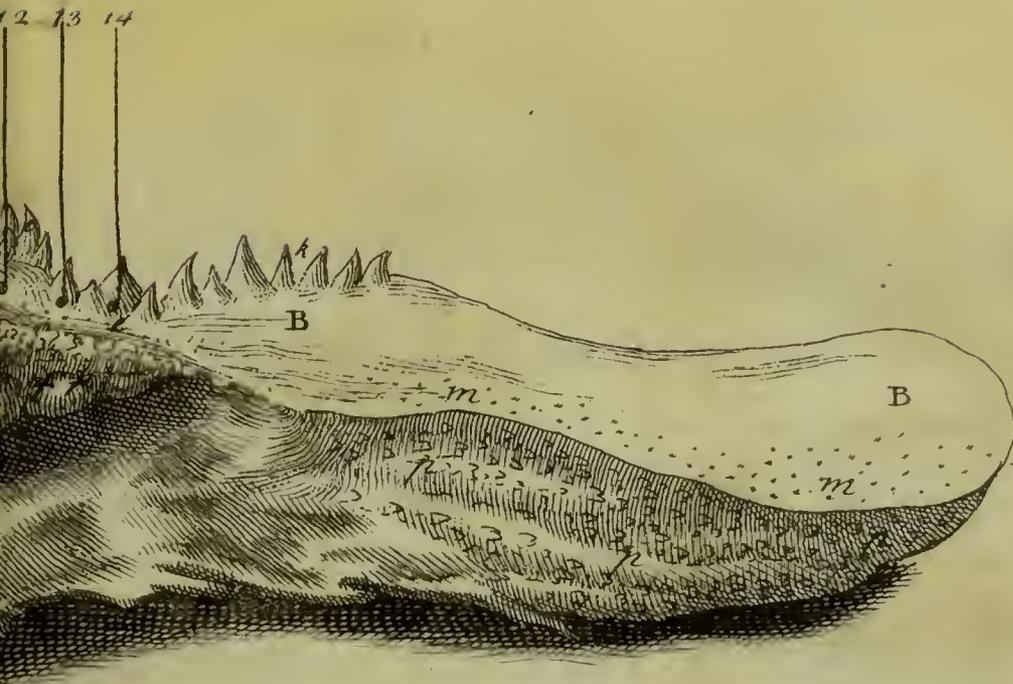


Fig: 15

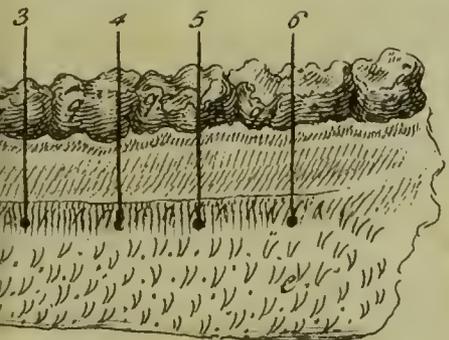
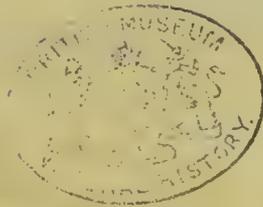


Fig: 16.



Fig: 18.



Beginning the Thirty First Volume.

PHILOSOPHICAL TRANSACTIONS.

 For the Months of *January, Feb. March and April.* 1720.

The CONTENTS.

- I. *Some Remarks on a late Essay of Mr. Cassini, wherein he proposes to find, by Observation, the Parallax and Magnitude of Sirius.* By Edmund Halley, L. L. D. Astron. Reg. Geom. Prof. Savil. & R. S. S.
- II. *An Account of the External Maxillar, and other Salivary Glands: Also of the Insertions of all the Lymphatics (as well above as below the Subclavians) into the Veins; which Glands and Insertions have not hitherto been mentioned, or not truly described by any Authors.* By Richard Hale, M. D. Reg. Soc. & Col. Med. Lond. Soc.
- III. *De Peste Constantinopoli grassante. Auctore nupero V. Cl. Emanuele Timone, M. D. Hoc scriptum ab Auctore Clarissimo, qui Constantinopoli per multos annos Medicinam fecerat, Excellentissimo Ablegato Britanico, Roberto Sutton, Eq. Aurato, traditum, ejusdem Equitis permissu, cum Societate Regia communicavit idem R. Hale, M. D.*
- IV. *An Account of a Luminous Appearance in the Air, seen at Dublin on January the 12th, 17¹⁹. By Philip Percival, Esq; Brother to the Right Honourable the Lord Percival, F. R. S. and communicated by his Lordship to the Royal Society.*

- V. *Of the Infinity of the Sphere of Fix'd Stars.* By Edmund Halley, L. L. D. R. S. S.
- VI. *Of the Number, Order, and Light of the Fix'd Stars.* By the same.
- VII. *An Account of the Method of making Sugar from the Juice of the Maple Tree, in New England.* By the Honourable Paul Dudley, Esq; F. R. S. Communicated by John Chamberlayne, Esq.
- VIII. *A Copy of an Affidavit made in Scotland, concerning a Boy's living a considerable time without Food.* Communicated by Patrick Blair, M. D. F. R. S.
- IX. *A Discourse concerning a Method of discovering the Virtues of Plants by their External Structure.* By the Same.
- X. *An Account of a Book, Entituled, Geometria Organica, sive Descriptio Linearum Curvarum Universalis.* Auctore Colino Mac Laurin, Matheseos in Collegio Novo Abredonensi Professore, & R. S. S.
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I. *Some Remarks on a late Essay of Mr. Cassini, wherein he proposes to find, by Observation, the Parallax and Magnitude of Sirius.* By Edmund Halley, LL.D. R. S. S.

IN the *Memoires* of the *Royal Academie* of *Paris*, for the Year 1717. but now very lately published, there is one very remarkable Essay, by Mr. *Cassini*, concerning the *Annual Parallax* of the *Fix'd Stars*, and particularly of *Sirius*; and in conclusion, he determines the Diameter of *Sirius* to be as much bigger than that of the *Sun*, as the *Sun's* is greater than that of the *Earth*, which he supposes to be 100 times: And the distance from the *Sun* to the *Earth* being certainly about 100 Diameters of the *Sun*, it will follow, that the Globe of *Sirius* must be a Sphere, whose Diameter must equal the distance between the *Earth* and *Sun*.

To prove this, he tells us that he made use of an excellent Telescope of 34 *French* Feet, or 36 *English*, leaving an Aperture of but an Inch and half, to take off the spurious Rays of the *Star*, which then appeared round, and sufficiently well defined; and comparing his Body to that of *Jupiter*, which he says, was then 50 Seconds Diameter, he found that the Diameter of *Jupiter* was ten times greater than that of the *Star*, which by consequence was seen under an Angle of about 5 Seconds; which is his first Position.

Then he tells us, that to make the Observations of the Parallax of this *Star* with all the exactness possible, he employed a Telescope of *three Foot*, in a Copper
A Tube,

Tube, having fixed in the common *Focus* of the two Glasses, four threads crossing one another in the Center, under Angles of 45 Degrees. This Tube he firmly fix'd to the Plain of a *Mural Arch*, which had been for above 30 Years immovably cemented to the Wall of the *Royal Observatory*, to which he chose to fix it, because of the great Solidity thereof, and its being therefore the less liable to shake; and that after having stood 30 Years, there was no fear of its settling any further in the space of one Year; besides, that it was easy to perceive if any such alteration should happen to it.

Having therefore fix'd his three Foot Tube as above, so that, about the beginning of *April*, 1714. *New Style*, (I suppose, because then *Sirius* was in *Square* to the *Sun*) the Star being exactly in the *Meridian*, past over the Center of the Tube, he observed that on the 20th of *April* the Star touched the Horizontal Thread with its under edge, being apparently all above it, in the inverting Tube, but really below. On the 15th of *May*, and 6th of *June*, it past again by the Center. On *June* the 27th it appeared a little under, and on *July* the 9th it was found to touch the under part of the Thread. On *October* the 5th it again past by the Center; but on *December* the 29th, it touched the upper part of the Thread. *January* the 18th, 1715. being the coldest Day of that Winter, it past exactly by the Center; and on the 27th of *March*, and the 1st of *April*, it almost touched the upper side of the Horizontal Thread, from which it seem'd a little separated. But on *June* the 7th, it past a little under the Center; and on *June* the 29th, the *Sun* being then in conjunction with *Sirius*, it past under the Thread, so as to touch it with its upper edge. Whence it appears, that in the space of the whole Year, there had been no other variation of the *Meridian* Altitude of *Sirius*, than the breadth of the Thread, which appear'd

appear'd equal to the Diameter of the Star, which he takes to be five, or at most six Seconds.

Supposing this to be so, he then shews that the whole Diameter of the annual Orb is to the distance of *Sirius*, as the Sine of 6" to the Sine of $39^{\circ} 33'$ the Latitude of the Star, whence the aforesaid immense magnitude of the Body thereof, is a necessary Consequence.

But before this obtain a full assent, it may not perhaps be amiss to enquire whether the suppos'd visible Diameter of *Sirius*, were not an Optick Fallacy, occasioned by the great contraction of the *Aperture* of the *Object Glass*: For we all know that the Diameters of *Aldebaran* and *Spica Virginis*, are so small, that when they happen to immerge on the dark Limb of the *Moon*, they are so far from loosing their Light gradually, as they must do were they of any sensible magnitude, that they vanish at once with their utmost Lustre; and emerge likewise in a Moment, not small at first, but at once appear with their full Light, even tho' the Emerision happen very near the *Cusp*; where, if they were four Seconds in Diameter, they would be many Seconds of Time in getting entirely separated from the Limb. But the contrary appears to all those, that have observed the Occultations of these bright Stars. And tho' *Sirius* be bigger than either of them, yet he is by far less than two of them; and consequently his Diameter to theirs is less than the Square Root of 2 to 1, or than 1.4 to 1.0; whence, in Mr. *Cassini's* excellent 36 Foot Glass, those Stars ought to be about four Seconds in Diameter; and they would undoubtedly appear so, if view'd after the same manner; whereas we are aliunde certain, that they are less than one single Second in Diameter. The great strength of their native Light, forming the resemblance of a Body, when it is nothing else but the spissitude of their Rays.

As

As to the other part of the Argument, that the alteration of the declination of *Sirius*, on the score of the access of the Earth in *December*, and its recess in *June*, amounts to 6 Seconds; I can only remark, that, besides that a *Radius* of 3 Feet, as it seems that made use of was no more, is somewhat too small for so extremely nice an Observation, 6" being subtended by the $\frac{1}{1000}$ part of an Inch, some of the Observations before recited do plainly shew, that the *Refraction* of the *Medium* did intermix with those Differences that might be occasioned by the *Parallax*.

But the principal Objection against the Conclusion of this Argument, seems to be, that the Meridian altitude of *Sirius* at *Paris* being under 25 Degrees, the ordinary *Refraction* of the Star is 1' 55" or 115 Seconds; and the Barometer rising and falling above two Inches in Thirty, shews that the *density* of the *Air*, on that score, may be a 15th part more at one time than another. Whence the *Refractions* being always proportional to the *density* of the *Medium*, as we have all seen it often demonstrated by Mr. *Hawksbee*, both in *Vacuo*, and in a *doubly* and *trebly condensed Air*, it is plain that in that Altitude the *Refraction* of a Star may differ about 7 or 8 Seconds, or the 15th part of 115", which is more than the whole *Parallax* supposed to have been observed.

It were to be wish'd that Mr. *Cassini* would please to try this Matter by the *Lucida Lyra*, instead of *Sirius*, which, tho' somewhat less than him, is as near to the *Softital Colure*, and has much greater Latitude, being but 28 *grad.* from the *Pole* of the *Ecliptick*, whence its *Parallax* would be so much greater; and being at *Paris* within 10 *grad.* of the *Zenith*, the grand Objection of the difference of *Refraction*, would be almost wholly removed.



Glands) which constitute each distinct Lobe, and has the same structure as the Pancreatick Duct. Each Lobe is depressed on its sides, where it is joined to other Lobes; and between the Lobes many Buccal Glands are interspersed.

In Calves seldom more than six or seven Ducts admit any Probe; when the Animal grows older, the Ducts appear more plain and open.

In Sheep six excretory Ducts are always found in each external Maxillar Gland.

In Dogs and Cats, &c. these Ducts are fewer, in proportion to the smallness of the Glands. 'Tis observable, that these Ducts in Dogs open obliquely towards the Mouth, whereby the *Saliva* may be better mixt with the Food in Mastication; which might be swallowed unmixt from another structure of Ducts, in these Animals that swallow greedily.

Dr. *Wharton* * first mentions the external Maxillar Glands. What he says of them, is applicable only to their appearance in Men, in which Subjects they are of the Conglobate kind, and very small, unless in Scrophulous and Venereal Cases. 'Tis plain that he had not seen them in Brutes; for in his Figures (which were drawn from Brutes) no notice is taken of these Glands. He describes them as very small, and calls them Emunctories of the Nerves, which was the Notion (in his time) concerning the use of the Conglobate Glands; and the *Saliva* was said † *è Nervoso Genere profundi*.

* *Steno* justly blames *Blasius* for ascribing to the external Maxillars an Excretory Duct opening into the Mouth, like the common one from the Parotid Gland. Yet *Steno* (otherwise very accurate) does not truly describe these Glands, nor distinguish them from the Buccal, tho' they

* *Cap. 21.*

† *Cap. 21. pag. 134.*

* *Obs. Anat. p. 14.*

are as distinct from the Buccal, as the Sublinguals are from the internal Maxillars. *Steno* divides his Buccal into 3 Parts. The large Ducts in a Line rise from the external Maxillars; and how distinct these Glands are from the Buccal appears plainly in Fig. 17th, &c. *Steno's* 2d part of the Buccal, * *intra quæ, & in mediâ parte,* are mark'd *e e,* in Fig. 14. *quæ alias, &c.* higher are the same *e e,* among the *Papillæ.* The third Part *quæ à superiore descendunt,* are *a b c d.*

The external Maxillars differ from the Buccal, in bigness, figure, structure, particular number of Ducts, colour, &c. The Buccal, Labial, internal Maxillar, and sublingual Glands, are of a yellow Colour; besides the Buccal are separated from the external Maxillars by a strong Membrane. Indeed many of the Excretory Ducts of the Buccal Glands open near the Ducts of the Maxillars (from whence *Steno* confounded these Glands) but they do so likewise round his own Ducts from the Parotids; and some Ducts from like Glands open near the Sublinguals, as also about *Nuck's* Ducts, in which places the Buccal Ducts are most numerous.

In short, there is a very great Number of Excretory Ducts dispersed all over the Membrane, that invests the Mouth, *Fauces, &c.* which rise from Glands that lie under this internal Membrane. These Glands are more numerous in some Parts than others, and receive different Names, according to the Part they belong to; as Labial, Buccal, Palatine, &c. But these are small Glandules with one Excretory Duct, and tho' they separate Saliva like the large Conglomerate Glands, Parotids, Maxillars, &c. yet they differ from these in Constructure, one common Excretory Duct, &c. Whereas the external Maxillars differ from all the other Glands of the Mouth, *viz.*

* *Pag.* 18.

by many ways from the Buccal, besides their Colour ; in which particular, they are also distinguished from the internal Maxillar and Sublingual Glands ; they differ also from these as well as from the Parotids, in having a great number of common Excretory Ducts. This number of Excretory Ducts was not observed by *Steno*, nor did he know that these Ducts in the same Line, were the Excretory Ducts of large Conglomerate Glands (like the Parotids) distinct from the Buccal.

Bartholine * mentions the external Maxillar Glands, but does not describe them. *Nuck* † only gives them a Place in his Catalogue of Glands, but takes no farther Notice of them, tho' he writes a * Book chiefly about a new Salival Duct rising from a Gland, that is found in no Animal besides a Dog.

Mr. *Comper* had never seen these external Maxillar Glands, as appears by a Letter of his (now by me) written above Twenty Years ago, in answer to one I sent him upon the first discovery of these Glands. The external Maxillars in Men (of the Conglobate kind) are marked *g*, in the first Figure of his *Myotomia Reformata*.

The Ducts of the external Maxillar Glands are opposite to the Orifices of *Steno's* Ducts ; from which Glands and Ducts, as also from the Buccal, Labial, and Gingival Glands, the Saliva flows from all parts of the Mouth without the Teeth. From *Wharton's* and the Sublingual Ducts, from the Tonsils, *Fauces*, *Fretum Stenonis*, Gingival, Lingual and Palatine Glands, the Saliva is derived, from the upper and lower, former and hinder parts of the Mouth within the Teeth.

What has been said of these Salivary Glands, &c. will be best understood by the following Figures, which were

* *Pag.* 542.

*, *Sialog.* p. 15. 158.

† *Adenol.* p. 5. n. 11.

drawn for me in *October*, 1697. at *Trin. Coll. Oxon.* by Mr. *Burghers*, and have been lately compared with the Parts themselves in Cows, Calves, &c. These Figures are part of many more taken from Preparations at the same time, which were figured in the same Order as drawn. This is the Reason that the Cuts are marked in this manner, and it can't be of use to alter these Marks and Numbers.

The Insertions of all the Lymphatick Vessels into the Veins can be discovered but in few Subjects, and no Figure has yet been given of them.

These Figures shew the Course of the *Lympha* both below and above the Subclavians in Men, and Axillars in Dogs. The *Lympha* below the *Receptaculum Chyli* is conveyed from all the inferiour Parts by a great number of small Lymphatick Vessels, which uniting with others obliquely above the Valves, become bigger in proportion, till at length they constitute two large Trunks near the Emulgents, which are the *Pedunculi* or Beginnings of the *Receptaculum Chyli*. The *Lympha* from the Parts above the Subclavians, is derived in like manner from lesser Lymphaticks, to the common Ducts that are here delineated.

I know *Pecquet* has given a Cut of the Thoracick Duct in a Dog; which Duct is double from the Receptacle, and is inserted by four Branches into each Axillar. I believe with * *Bartholine*, (who has borrowed this Figure from *Pecquet*) that such an Insertion is a *Lusus Naturæ*. For tho' the Thoracick Duct may be double, and is sometimes divided into two Parts near the Subclavians, yet generally it is single, the *Lympha* from all parts on both sides the Body being carried by proper Lymphæducts into one common Thoracick Duct, that conveys this Liquor, together with the Chyle from the Lacteals, into the left

* *Barth. p. 616, 620.*

Subclavian Vein, by one, three, or more Branches. For there is as great a variety in the number of these Branches, as in the places of their Insertion

Mr. *Cowper* injected the Thoracick Duct in a Humane Subject, and has given a Figure of that Preparation in his Book of Anatomy. But this Figure is imperfect, and the Insertion of the Thoracick Duct so ill drawn, that little can be learnt by it. However, no Anatomist has given any Cut, that demonstrates the Insertions of the Lymphaticks from both Arms and both sides of the Head, &c above the Subclavian Veins, which appear so plain in these Figures, that no Description can make them plainer.

Explanation of the Figures.

Fig. 12. Demonstrates the Passages, or Vessels, by which the Chyle and *Lympha* pass into the Veins of a Dog.

12, 12 Those Lymphaticks that bring *Lympha* from the Thighs and lower Parts.

13, 13 Are lateral Lymphaticks arising from the Groin, Testicles, and neighbouring Parts.

14 The Receptacle of the Chyle.

15 An Indenture in the Receptacle, thro' which passes one Tendon of the Diaphragm.

16 Lymphaticks from a neighbouring Gland.

17 Some Lymphaticks from the Diaphragm.

18 An Artery that serves the Loins, and runs through a Division of the Receptacle.

19 The *Pancreas Asellii*.

20 The *Vasa Lactea 2di Generis*.

21 The beginning of the *Ductus Thoracicus*.

22 Some Divarications of the *Ductus*.

* Barth. p. 620.

23 The Continuation of the *Ductus*, and its progress.
 24 The *Aorta Descendens*. N B. 18, 24, by their Pulsation (and the Tendon at 15) much promote the Ascent of the Chyle and *Lympha*.

25 A common Divarication of the Duct.

26 A Lymphatick from some neighbouring Gland.

27 A double Lymphatick from the secondary Gland 42, in *Fig. 13*.

28 That part of the *Ductus Thoracicus* where both its Branches, and the Lymphaticks from the left side of the Head and left Fore-Leg meet.

29 The Lymphaticks from the left side of the Head and left Fore-Leg united; they lie on the inside of the Vein.

30 A Lymphatick with a Pin in it from a neighbouring Gland, perhaps the *Thymus*.

31 A Lymphatick from the Neck, &c. It is divided and enters the Jugular by two distinct Branches under the *Sacculus* 43.

32 The Lymphatick from the right side of the Head.

33 The Lymphatick from the right Fore-Leg.

34 The large *Sacculus*, or Receptacle of the *Lympha*, on the right side, that receives all the *Lympha* on that side, and conveys it into the Jugular.

35 The *Cava Descendens*.

36 The *Vena Mammaria*, which is sometimes single.

37 The *Vena Subclavia*.

38 The *Vena Vertebralis*.

39 The Axillars.

40 The Jugulars.

41 The right internal Jugular not injected.

42 A small secondary Lymphatick Gland on the back part of the top of the *Thorax*.

43 The *Sacculus*, that receives all the Chyle and *Lympha* from the whole Body (except 30, 31, 32, 33, 34) and .

and discharges it into the Vein: at least we know of no other Lymphaticks that any where else enter the Veins.

44 A Lymphatick, (or Membrane, for 'twas not injected) that joins 29 to the largest Branch of the *Ductus Thoracicus*.

Fig. 13. Is the upper part of *Fig. 12.* revers'd, the Duct, &c. being turned up, that the Insertion, both *Sacculi*, &c. may be better discovered.

This is to be explained by the preceding, and has only from 42. to 44 more Figures than the upper part of *Fig. 12.* has; all which are already taken notice of.

N. B. In this Subject the Chyle and *Lympha* are emptied into the Jugulars, and not into the Axillars; they are sometimes emptied partly into the Jugular, and partly into the Axillar, or Subclavian. In Men generally into the Subclavian.

Fig. 14. Represents part of the Left Cheek of an Ox, separated from the lower Jaw-bone, with the external Maxillar Glands, its Ducts, &c.

1, 2, 3, &c. to 14. Bristles inserted into the Ducts of the external Maxillary Gland *l l l*. These Ducts open sloping into the Mouth, for the better mixture of the *Saliva* with the Food.

15 The Duct 3 injected with Wax, to discover its division and bigness, in respect of the Orifice.

16 A Lobulus of the Maxillar Gland. Its excretory Duct is filled with Wax, and ends at 15.

17 The Duct 1 laid bare and open'd, to shew its large Cavity, &c.

A A, Part of the Muscles and Fat, &c. belonging to the lower Jaw.

B B, Part of the internal Membrane that invests the Mouth.

a b c d, Bristles in those Ducts of the Buccal Glands, *n n*, that I could pass any into.

e e e, Those

eee, Those Orifices of the Buccal Glandules, that were too little to admit Bristles.

kkk, The *Papilla* on the inside of the Mouth.

lll, The Lobes that constitute the external Maxillary Gland.

mmm, The Orifices of the Labial Glandules *pp*, that were too small for passing Bristles.

nnn, Buccal Glandules interspersed between the Lobules of the Maxillary Gland.

nnn near *rrr*, Part of the Buccal Glandules, where they appear thickest, and are rais'd to discover the Ducts *rrr*, running under them.

ppp, The Labial Glandules like the Buccal. Mr. *Cowper* in *Fig. 4.* letters them H, H.

rrr, The Ducts mark'd 6, to 14. as they appear under the Glandules *nn*.

N. B. The same Numbers and Letters express the same Things in the following Figures.

Fig. 15. Exhibits part of the left Jaw-bone and Cheek of a Sheep, where the Bristles 1, 2, 3, &c. shew the constant number of excretory Ducts from the external Maxillary Gland in these Animals.

Fig. 16. Shews part of the right Cheek of a large Dog, taken from the lower Jaw-bone.

f, The Orifice of *Steno's* Salival Duct.

g, The Orifice of *Nuck's* Duct, which rises as a *Papilla* on the Membrane B B.

h, *Nuck's* new Duct, not found in Men, Oxen, or Sheep, but in Dogs, their Orbit not being entirely bony.

i, *Nuck's* Gland.

ooo, The Orifices of some excretory Ducts, belonging to the external Maxillary Gland, that were too strait for the admission of Bristles.

q q, The Teeth. In this Subject they are the Teeth of the upper Jaw; near the second of which, the Orifice of *Nuck's* Duct appears.

Fig. 17. Demonstrates the back part (next the Cutis) of the external Maxillar Gland of the same Dog, as 'tis beset with the Buccal Glandules.

Fig. 18. Explains the external Maxillar Gland in the right Cheek of a Calf. In this Subject I could only probe two Ducts, 3, &c. would not admit Bristles.

III. *De Peste Constantinopoli grassante. Auctore nupero V. Cl. Emanuele Timone, M. D. Hoc Scriptum ab Auctore Clarissimo, qui Constantinopoli per multos annos Medicinam fecerat, Excellentissimo Ablegato Britannico, Roberto Sutton, Eq. Aurato, traditum, ejusdem Equitis permissu, cum Societate Regia communicavit R. Hale, M. D.*

Pestem Constantinopoli ex *Ægypto* communicatam tum historiis, tum quotidianâ observatione constat. Nidum tamen in hac urbe sibi fecit, & quamvis nunquam fere veteris Pestis desint seminia, novus etiam subinde fomes advehitur. Ab hyemali intenso frigore satis bene sopitur; emicant tamen hinc inde scintillæ aliquot hyeme & vere: æstate incrementum sumit; autumnali tempore summo flagrat incendio. Venti Aquilonares statis temporibus æstate flantes (Etesix scil. hujus loci) quamvis frigidiusculi sint, veneni tamen pestilentialis dilatationi haud obsistunt. Australes satis calidi, si constanter flent, æstivo tempore Pestem supprimunt. Quoad symptomata Pestis Constantinopolitana adamussim respondet pesti Noviomagensi

Noviomagensi annorum 1636 & 1637, ab *Isbr. de Diemerbroeck* descriptæ, Humanum genus invadit *cap. 4. Diemerbr.* Compertum tamen est unum vel alterum equum, canem, selemve peste bubonaria correptum, & mortuum. Vulgus (præcipue, cui augusta domi res est) tum Turcarum, tum etiam Christianorum, Hebræorumq; facile sibi persuasum habet divinitûs missam pestem; hinc non cavet à contagio: Politiores, & præcipuè Christiani sibi cavent.

Symptomata à * dicto auctore enumerantur ista, Febres, bubones, carbunculi, exanthemata, capitis dolor phrenitis, sopor, vigiliæ, anxietas, debilitas, seu magna virium prostratio, visus turbulentus, cordis palpitatio, linguæ siccitas, vomitus, singultus, lumbrici, fluxus alvi, hæmorrhagia narium, menstruorum profluvium, sanguinis mictus, sputum cruentum, dolores lateris, hepatis, renum, aliarumve partium.

His adjicio ego lassitudinem artuum ulcerosam, horripilationem, subsequente aliquando calore, sæpius non subsequente, nauseam sine vomitu, vertiginem, & motus vertiginosos, tremorem manuum statim à morbi initio. Nullum ex omnibus symptomatibus est inseparabile, imo nec tria illa pesti specialia, bubones dico, carbunculos, & exanthemata; febris sæpissime abest; ita ut regula ista generalis stabiliri possit: *Quando adsunt manifesta pestis signa, certo statuere possumus morbum esse pestem: sed quando absunt signa ista, non possumus certo statuere contrarium.* Multis enim peste correptis, leni præcedente rigore, quo nec catarrho se laborare quis suspicaretur, per plures dies ex istis signis non manifestatur nec minimum, postea autem cætervatim erumpunt. Nonnulli post susceptum contagium levissimo tantum languore affecti, mox creditè obambulant, & consueta munia sine incommodo exercent; ter-

* *Cap. 7. Diemerbr.*

tiâ autem, vel quartâ die statim corruunt, animamque exhalant in viâ : & ne quis suspicetur apoplexiam vel quid aliud, post mortem demum nigræ maculæ deprehenduntur : fidem etiam facit tum præcedens causa contagii manifestissima, tum propagatum postcâ contagium. Multi etiam sine ullâ manifestâ læsione erectè obambulantes sanantur, erumpentibus etiam bubonibus & carbunculis. Plerique tamen febricitant, & graviter ægrotant. Vomitus & alvi fluxus cum subitâ virium concidentia, & cum febre pestem indicant, & sine febre adhuc magis. Minime ex signis suspectis si dolor in emunctoriis communibus jungatur, pronuntiandum est esse pestem.

Pustula rubicunda ad semi-ciceris magnitudinem in extremo purulenta maligna est : in lividum enim carbunculum brevi excrescit. Carbunculi in omnibus corporis partibus indifferenter erumpunt, non exceptis labiis, lingua, bulbis oculorum, glande penis, &c. Bubones tantum in emunctoriis. Glandulæ parvæ duræ circa collum malignæ. Exanthemata semper sunt lethalia. Bubones si cito ad suppurationem tendant, spem promittunt. Non malum est, si etiam non bene maturi ferro aperiantur. Multi per bubonis resolutionem curati, annis sequentibus, si peste infecta loca petant, eo in loco dolorem obscurum sentiunt, in quo bubonem habuerunt. Pestis in aliquibus individuis per plures dies latitare potest, & postea in actum erumpere. A peste convalescere incipiens, si ante quadragesimum gravem aliquem in diætâ errorem fecerit, novo erumpente bubone moritur. Pro monstro habetur, si quis à peste perfecte sanatus eodem anno peste corripiatur. Si dispositionem corporum ad octo gradus placeat revocare ; intacta peste corpora dispositionem habent ut septem, & rudem merita ut unum. Infectas ædes per menses aliquot impune incolens tandem peste correptus fuit. * Senes difficillimè peste corripiuntur, facillimè

* Cap. 4. Diemerb.

pueri. Exteri multo magis periclitantur, quam Cives. Armenii omnium nationum minimè ad pestem sunt dispositi. Observo illos paucissimis uti carnibus; cœpis, porris, alliis, vinoque maximè utuntur. Caro porcina in peste præsentaneum venenum est. Nil æque ad pestem disponit ac animi pathemata, ac præcipue intensus mœror, & terror inopinatus. Lue venereâ infecti indifferenter se habent ad contagium: bubones tamen illis suppurantes in fistulas ut plurimum degenerant. Nitidæ ædes haud æque facile inficiuntur ac sordidæ. Cachectici, ictero laborantes, & variis aliis chronicis morbis conflictati homines dantur, & tamen pestis spicula vel eludunt, vel superant; roseâ è contra facie præditi, & robustissimi moriuntur multi.

Suffumigia ex baccis Juniperi, pice, sulphure utilia sunt, si diu noctuque continuentur, ita ut ab inferiore parte totam penetrent domum. Odoramenta ex aceto haud inutilia censentur. Oleum succini naribus illitum optimum est. Vinum moderate sumptum, hilaritas animi, & bona victûs ratio haud parvi momenti sunt in præservatione. Phlebotomia damnatur, & præcipue sera; sanguis è venâ sectâ fluens, nullo modo multoties sisti potest; ex scarificationibus etiam pro cucurbitularum applicatione factis ad plures horas, hoc est ad mortem usque, sanguinem vi magnâ effluxisse observatum est. Vomitoria levia non profunt, fortia valde nocent: purgantia statim enecant. Sola spes in sudore, & alexipharmacis. Communissimè utuntur in hac urbe Theriaca Veneta. Lapis Bezoar, quamvis genuinus, nullius fere est momenti. Judæi acidis utuntur. Armenii & Græci vino, & spiritu vini. A carnibus, & carniûm jusculis relligiosè abstinent Christiani ad multos dies. Turcæ etiam hanc diætam non spernunt. Multi, queis parvus tuberculus nullo livore insignis erupit, à carnibus abstinentes,

per

per hebdomadam sani obambularunt, postea carnibus usi brevissimè mortui sunt. Opium crudum, & opiata omnia bona sunt pro scopo præservativo, sæpe etiam procurativo. Summum remedium Turcis magnatibus habetur oleum naphthæ, seu Petroleum albicans, ad duas usque drachmas epotum in vehiculo ad libitum; convenit hoc cum Camphorâ à nostris tantopere celebratâ. Quidam multos sanâsse dicitur phlebotomiâ (primâ tamen morbi die) ad lipothymiam usq; celebratâ, propinato postea haustu aceti acerrimi, in quo sanguinis draconis & boli armenæ à drachmam unam dissolvebat. Phlebotomiæ negotium in medium examinandum relinquo; ast certe præstantissimum remedium est. Frictio rubificans tum in pectore, tum in dorso, in locis utrinque cordi respondentibus, cum allio contuso fortissimè per aliquot horas celebrata, proficua est. Radix hellebori, sectione factâ, carni ad emunctoria transversim infixâ utilis est. Pulli gallinacei, vel columbini, seu vivi, seu per medium secti, bubonibus & carbunculis, & etiam regioni cordis appliciti, & sæpissime iterati (sed ultra semihoram non retinendi) utilissimi comperti sunt. Oleum Succini, & extractum Juniperi in praxi sæliciter successerunt. Methodus prædicti *Diemerbroeck* per omnia utilis est huic pesti; certè bene de Peste scripsit auctor hic, excepto quod pestis causam nimis alte deducat. *Barbette* etiam haud est contemnendus.

Anno 1712. Pests *Byzantii* in fine *Maii* Mensis incrementum sumpsit. In fine *Julii* erat in summo furore. In determinato loco observatorem habui, qui mortuorum cadavera ad nonaginta & supra uno die numeravit. Etesis flabant vehementissime; cessantibus Etesis, Auster flavit satis intense. Primâ post Austri flatum hebdomadâ, cadavera non numeravit in die nisi circiter quadraginta; secundâ hebdomadâ triginta circumcirca;

cumcirca ; tertiâ minus viginti : qui numerus in bonâ etiam urbis salubritate tali anni tempore singulis diebus non deest. Sic autumnali tempore pestis sopita est anno illo : sed ut plurimum tali tempore sævire solet, & mediâ æstate, vel in fine æstatis incrementum sumere. Pestem vere incipientem, autumno sopitam alias etiam observatum est. Notandum est post solstitium æstivum pestem in Ægypto, quamvis ferocissimam, infalibiliter desinere : causa altioris indaginis est. Climatis Ægyptiaci characterismus in hoc peculiari pestis phænomeno aliquatenus ad Smyrnam usq; insulamque Chiam, ipsaq; Hellesponti angustias sese extendit. Observandum ulterius Etesias, ventos scil. Aquilonares, cursum solis sequentes, aerem vehere, non excrementitiâ solum humiditate, sed nitrosis particulis valde saturatum & imbutum. Ansa mihi fuit hujus rei cognoscendæ vestis quædam, quâ nos utimur ex pilis caprarum textâ. Vestis hæc Hygroscopii quoddam genus videtur ; nam quâ parte sudore manuum, vel colli, fuerit antea madefacta, quamvis sefecit, & post multos etiam menses in usum denuo accipiatur, horâ tamen dimidiâ, vel unâ post ortum Solis, quando nimirum Etesix spirare incipiunt, talem madorem denuo contrahit, ut maculâ nigrâ intensissimâ inficiatur. Vestis post duas, vel tres horas denuo exsiccatur, & macula evanescit. Hic duo sunt notanda : primum est ; si simplici aquâ vestis illa fuerit sæpius imbuta, & postea sefecit, haud succedit hoc phænomenon madoris & maculæ, prout quando sudore imbuta est : secundum consideratione dignum est hoc ; si forte fortunâ tempus sit actualiter pluvium haud æque facile madere inficiq; maculâ vestem illam, dummodo actualiter pluvix non exponatur ; ex quibus colligo, præter aqueas particulas, nitrosis salibus imbutum aërem ex Ponto Euxino huc advehi primo Etesiarum flatu. Particulæ istæ nitrosæ à congeneribus

à congeneribus salinis particulis à sudore vesti illi communicatis fixantur & retinentur, & combinatione factâ maculam efficiunt. Salibus igitur istis imbutus aer fermenti pestilentialis (quod indolis salino-corrosivæ esse probabile est) dilatationi fomitem præbere potest. *Ægyptium* autem aerem, mediante vento austrino huc pervenientem, summe rarefactum & talium expertem salium, Pestis dilatationi obsistere posse verisimile est: Adde, quod calidus Auster corpora ad transpirationem magis disponat.

Si quis quærat interim, utrum Pestis sanguinis coagulationem vel nimiam dissolutionem inducat? dico neutrum primario, & immediate facere, sed utrumq; posse accidere tum processu temporis, tum ratione massæ sanguinæ, in variis individuis, quoad principia activa, variantis. Primario autem & immediate Pestis spiritus adoritur: liquidis subsequenter, & solidis varia inducit mala, secundum varium tum liquidorum, tum solidorum schematismum. * Hinc varia symptomata in peste, & sibi è diametro opposita, quæ unico remedio vinci non possunt, sed prudentis Medici requirunt Judicium, ut, quod opportunum est, in actum deducatur. Generaliter tamen observandum, ut citissime generosissima remedia, & in magna dosi propinentur: ut in principio oleoso-balsamica, & spiritibus amica volatilia usurpentur; ut omnes evacuationes præter sudorem vitentur; artificiales igitur non tentandæ, naturales sistendæ suis quæque appropriatis; (sic alvi fluxus salutariter sedatus fuit clystere astringente, cui Theriaca erat immixta,) si spiritus sint effrænes & peccent in motu aucto, quod à Vigiliis potissimum cognoscitur, ut sistantur per Opiata. Si contra necrosi, seu languore afficiantur, quod ab effectibus potissimum soporis cognoscitur, ut suscitentur per volatilia, & Cam-

* Diemerb. cap. 7.

phorata: ut sanguinis consistentia integra servetur in processu, mali, quod fit acidis & terreis adstringentibus, ut Venenum ad peripheriam alliciatur, &c.

Anno 1714.

Emanuel Timonius,
Constantinopolitanus.

IV. *An Account of a Luminous Appearance in the Air at Dublin, on January the 12th, 17¹⁹₂₀. By Philip Percival, Esq; Brother to the Right Honourable the Lord Percival, and communicated by his Lordship to the Royal Society.*

I Here send you a Sketch of an odd appearance in the Sky; it began about 10 a Clock on *Tuesday* last, but nothing very remarkable till about half an Hour after Eleven, when I was call'd out to see it, by the Servants, who had been looking at it about half a quarter of an Hour, and told me it looked just like Fire. But it appeared first to me in long streams of light, of a round Body, as at A, and very bright, tho' some were coloured, as at A a. They came before the Wind, which was then West, as near as I could guess, there not being a Cloud in the Sky, and the brightest Moon I have known. We had Rain about Five, but at 6 a Clock the Night was clear. The Streams of Light A A, moved very slow, (there being but little Wind) but as they moved they joined, and, swelling out in the middle, formed themselves into the Figure b b B, continuing to advance slowly in that shape for about a Minute, when the two Ends b b, approaching near each other, as described by the

D

prick'd

prick'd Lines, the advanced part B, suddenly, and with great swiftneſs, ran back, and joining it ſelf with the Ends *b b*, formed it ſelf into the Figure C, quivering in the upper part, and darting down perpendicularly in ſharp Points, as at D D D; and its Colour from a bright Light changed into the colours of a Rain-bow, but much fainter. It continued this way about a Minute, and then the ſharp points D D D, gathering themſelves up into C, it changed again into a ſquare Sheet of Light, as at E, and ſwell'd out at F, as before at B; and advancing leiſurely, repeated the ſame Scene as before, till it ſeem'd at a great diſtance to diſperſe it ſelf into ſmall thin light Clouds; tho' 'tis probable that to thoſe who ſaw it in a like Situation, as it travell'd, it might make the ſame appearance as it did to me. I was very particular in obſerving it, and the next Morning drew it, and I think very exactly. I ſhould have continued longer to look at it, (which I did for above a quarter of an Hour) but that it was exceſſive cold; the beginning of it was very like the *Aurora Borealis*, which has been very frequent this Winter here.

V. *Of the Infinity of the Sphere of Fix'd Stars.*
By Edmund Halley, L. L. D. R. S. S.

THE System of the World, as it is now underſtood, is taken to occupy the whole *Abyſs* of *Space*, and to be as ſuch actually infinite; and the appearance of the Sphere of Fixt Stars, ſtill diſcovering ſmaller and ſmaller ones, as you apply better Telescopes, ſeems to confirm this Doctrine. And indeed, were the whole System

stem finite; it, though never so extended, would still occupy no part of the *infinitum* of Space, which necessarily and evidently exists; whence the whole would be surrounded on all sides with an infinite *inane*, and the superficial Stars would gravitate towards those near the center, and with an accelerated motion run into them, and in process of time coalesce and unite with them into one. And, supposing Time enough, this would be a necessary consequence. But if the whole be Infinite, all the parts of it would be nearly *in equilibrio*, and consequently each fixt Star, being drawn by contrary Powers, would keep its place; or move, till such time, as, from such an *equilibrium*, it found its resting place; on which account, some, perhaps, may think the Infinity of the Sphere of Fixt Stars no very precarious Postulate.

But to this I find two Objections, which are rather of a Metaphysical than Physical Nature; and first, this supposes, as its consequent, that the number of Fixt Stars is not only indefinite, but actually more than any finite Number; which seems absurd *in terminis*, all Number being composed of Units, and no two Points or Centers being at a distance more than finite. But to this it may be answer'd, that by the same Argument we may conclude against the possibility of eternal Duration, because no number of Days, or Years, or Ages, can compleat it. Another Argument I have heard urged, that if the number of Fixt Stars were more than finite, the whole superficies of their apparent Sphere would be luminous, for that those shining Bodies would be more in number than there are Seconds of a Degree in the *area* of the whole Spherical Surface, which I think cannot be denied. But if we suppose all the Fixt Stars to be as far from one another, as the nearest of them is from the Sun; that is, if we may suppose the Sun to be one of them, at a greater distance their Disks and Light will be diminish'd

in the proportion of Squares, and the Space to contain them will be increased in the same proportion; so that in each Spherical Surface the number of Stars it might contain, will be as the Biquadrate of their distances. Put then the distances immensely great, as we are well assured they cannot but be, and from thence by an obvious *calculus*, it will be found, that as the Light of the Fix'd Stars diminishes, the intervals between them decrease in a less proportion, the one being as the Distances, and the other as the Squares thereof, reciprocally. Add to this, that the more remote Stars, and those far short of the remotest, vanish even in the nicest Telescopes, by reason of their extream minuteness; so that, tho' it were true, that some such Stars are in such a place, yet their Beams, aided by any help yet known, are not sufficient to move our Sense; after the same manner as a small Telescopical fixt Star is by no means perceivable to the naked Eye.

VI: *Of the Number, Order, and Light of the Fix'd Stars. By the same.*

AT the last meeting of the Society, I adventured to propose some Arguments, that seemed to me to evince the Infinity of the Sphere of Fixt Stars, as occupying the whole Abyss of Space, or the *τὸ πᾶν*, which at present is generally understood to be necessarily Infinite; and thence I laid before you what may seem a very *Metaphysical Paradox*, viz. That the number of Fixt Stars must then be more than any finite Number, and some of them more than at a finite distance from others. This seems to involve a Contradiction, but it is not the only one that occurs to those who have undertaken freely to consider

sider the nature of Infinite, which perhaps the very narrow limits of humane Capacity cannot attain to.

Since then, I have attentively examined what might be the consequence of an Hypothesis, that the Sun being one of the Fixt Stars, all the rest were as far distant from one another, as they are from us; and by a due calculation I find, that there cannot, upon that Supposition, be more than 13 Points in the Surface of a Sphere, as far distant from the Center of it, as they are from one another; and I believe it would be hard to find how to place thirteen Globes of equal magnitude, so as to touch one in the Center: for the twelve Angles of the *Icosaedron* are from one another very little more distant than from its center; that is, the side of the Triangular Base of that Solid, is very little more than the Semidiameter of the circumscribed Sphere, it being to it nearly as 21 to 20; so that it is plain that somewhat more than twelve equal Spheres may be posited about a middle one; but the Spherical Angles or Inclinations of the planes of these Figures being incommensurable with the 360 degrees of the Circle, there will be several interstices left, between some of the Twelve, but not such as to receive in any part the thirteenth Sphere.

Hence it is no very improbable Conjecture, that the number of the Fixt Stars of the first magnitude is so small, because this superior appearance of Light arises from their nearness; those that are less shewing themselves so small by reason of their greater distance. Now there are in all but sixteen Fixt Stars, in the whole number of them, that can indisputably be accounted of the first magnitude; whereof four are *extra Zodiacum*; viz. *Capella*, *Arcturus*, *Lucida Lyra*, and *Lucida Aquile*, to the North; four in the way of the Moon and Planets, to wit, *Palilicium*, *Cor Leonis*, *Spica*, and *Cor Scorpii*; and five to the Southward, that are seen in England, viz. The

Foot and *Right Shoulder* of *Orion*, *Sirius*, *Procyon*, and *Formalhaut*; and there are three more that never rise in our *Horizon*, viz. *Canopus*, *Achernâr*, and the *Foot* of the *Centaur*. But that they exceed the number *Thirteen*, may easily be accounted for from the different magnitudes that may be in the Stars themselves; and perhaps some of them may be much nearer to one another, than they are to us; this excess of Number being found singly in the Signs of *Gemini* and *Cancer*. And indeed within 45 degrees of Longitude, or one 8th of the whole, there are no less than *five* of these *sixteen* to be seen. If therefore the Number of them be supposed *Thirteen*, omitting Niceties in a Matter of such Irregularity, at twice the distance from the Sun there may be placed four times as many, or 52; which, with the same allowance, would nearly represent the number of the Stars we find to be of the 2d magnitude: so 9×13 , or 117, for those at three times the distance: and at ten times the distance 100×13 or 1300 Stars; which distance may perhaps diminish the light of any of the Stars of the first magnitude to that of the sixth, it being but the hundredth part of what, at their present distance, they appear with. But if, since we have room enough for it, we should suppose the Sphere continued to 10 times the last, or 100 times the first distance, the number of Stars would be 130,000, and they would appear but with the 10,000th part of the Light of a first magnitude Star, as we now see it. This is so small a pulse of Light, that it may well be questioned, whether the Eye, assisted with any artificial help, can be made sensible thereof. But 100 times the distance of a Star we see, is still Finite: from whence I leave those that please to consider it attentively, to draw the Conclusion.

VII. *An Account of the Method of making Sugar from the Juice of the Maple Tree in New England. By Paul Dudley, Esq; F. R. S. Communicated by John Chamberlayne, Esq.*

MAPLE Sugar is made of the Juice of Upland Maple, or Maple Trees that grow upon the Highlands. You box the Tree, as we call it, *i. e.* make a hole with an Axe, or Chizzel, into the Side of the Tree, within a Foot of the Ground; the Box you make may hold about a Pint, and therefore it must shelve inwards, or towards the bottom of the Tree; you must also bark the Tree above the Box, to steer or direct the Juice to the Box.

You must also Tap the Tree with a small Gimblet below your Box, so as to draw the Liquor off. When you have pierced or tapp'd your Tree, or Box, you put in a Reed, or Pipe, or a bit of Cedar scored with a Channel, and put a Bowl, Tray, or small Cask at the Foot of the Tree, to receive your Liquor, and so tend the Vessels as they are full.

After you have got your Liquor, you boil it in a Pot, Kettle, or Copper. Ten Gallons will make somewhat better than a pound of Sugar.

It becomes Sugar by the thin part evaporating in the boiling, for you must boil it till it is as thick as Treacle. Ten Gallons must boil till it comes to a pint and half.

A Kettle of twenty Gallons will be near 16 Hours in boiling, before you can reduce it to three Pints; a good Fire may do it sooner.

When:

When you take it off, you must keep almost continually stirring it, in order to make it Sugar: otherwise it will candy as hard as a Rock.

Some put in a little Beef Sewet, as big as a Walnut, when they take it off the Fire, to make it turn the better to Sugar, and to prevent its candying, but it will do without. A good large Tree will yield twenty Gallons. The Season of the Year is from the beginning of *February* to the beginning of *April*.

Mr. Dudley in a following Letter adds this Note.

I have nothing to add to my Chapter of Maple Sugar, but that our Physicians look upon it not only to be as good for common use as the *West India* Sugar, but to exceed all other for its Medicinal Virtue.

VIII. *Copy of an Affidavit made in Scotland, concerning a Boy's living a considerable time without Food. Communicated by Patrick Blair, M. D. F. R. S.*

Court of the Barony of Erroll, holden at Erroll, upon the 26th day of December, 1719 Year. By Mr. Charles Brown, Bailly to the Right Honourable David Earle of Northesk, Lord Roeshill, &c. Gilbert Anthone, Clerk, Charles Gill, Officer.

Court lawfully fenced and affirmed.

THE whilk Day Compeared before the said Bailly sitting in Judgment, *James Jackson* sewar in *Carse Crange*, and *Eliz. Bell* his Spouse, who being purged of partial

partial Council, solemnly Sworne and Interrogat, Depons, That *Gilbert Jackson*, their sixth Son, being about Fifteen Years of Age, fell sick, and complained of pains over all his Body, upon the 3d day of *February*, 1716: when King *George* his Army was marching by their Dwelling House from *Perth* to *Dundee*; and toward the end of the said Month, was seized with a violent Fever, in which he continued for three Weeks, and then recover'd.

2. That he fell in a Fever again the beginning of *April* thereafter, wherein he also continued for the space of three Weeks; and during that Fever, he had a shaking in his Body, as if he had been paralytick.

3. Upon the 10th day of *June* following, he fell in a great Fever again, when he became Dumb, lost his Stomach intirely, and the use of his Limbs, and continued without eating or drinking any kind of thing, tho' all means were used to make him do both; but recovered of his Fever upon the 17th day of *May*, 1717. but continued still Dumb, without eating or drinking, or having the use of his Limbs, till the 10th day of *June* the said Year, when he was again seized with an extraordinary Fever, and the next day recovered his Speech; but continued in the Fever, without eating or drinking any thing at all, or having the use of any of his Limbs, till the 11th day of *November* following; when he recovered his Health pretty well, and the strength of one of his Legs. And thus he continued without eating or drinking, only washed his Mouth sometimes with Water; and always, when he saw the rest of the Family going to take any kind of Food, the sight of it being altogether uneasie to him, he retired.

Upon the 10th day of *June*, 1718 year, he fell in a Fever again, which continued till the beginning of *September* thereafter, when he recovered of the said Fever,

tho' he never could be induced to take any kind of Meat or Drink; and thus he continued in pretty good Health, and fresh coloured, till the 9th day of *June*, 1719 when he was seized again with a severe Fever; and upon the 10th, at Night, his Father pressed him extreamly to take a little Milk boiled with Oat-meal, which at length he agreed to; and he took a Spoonful of it, which stuck so long in his Throat, that his Parents thought he had been choaked; and ever since he has taken a little Food, but so very little, that a Halfpenny Loaf serves him eight Days. That all the time he fasted, he never had any Evacuation either by Stool or Urine; and it was fourteen or fifteen days after he began to eat, that he got any benefit that way; and that he is now in pretty good Health, but still wants the use of one of his Limbs. And this is the Truth, as we shall answer to God.

James Jackson,
Elizabeth Bell,
Charles Browne, Bailly.
Gilbert Anthone, Clerk.

IX. *A Discourse concerning a Method of discovering the Virtues of Plants by their external Structure. By the same.*

HAVING hitherto delivered my Thoughts concerning the structure of the Flowers, Fructification, various Methods, Generation and Nourishment of Plants, I now come to add somewhat concerning their Virtues. I cannot enough admire the Judiciousness and Sagacity of

of the Ancients, who, without any of those means made use of by the Moderns, have handed down to us such an account of the Virtues of those Plants, which by the unanimous Consent of all Physicians and Pharmacians, are more particularly dedicated for use in Physick, that all the laborious Endeavours of their inquisitive Successors, have never been able to outdo them. It must have been a long Tract of Experience, which enabled *Dioscorides* and *Theophrastus* to collect and receive from their wise Ancestors, such a lasting Catalogue of the Virtues of Plants, as scarce any thing has been added to even to this day. The Royal Academy at *Paris*, has been at great pains to find out the Virtues of Plants by the Chymical Analysis, and several other Experiments, of which we have the Abstracts in *Tournefort's Histoire des Plantes aux environs de Paris*, and *Tauvry* his *Traité des Medicaments*: But these laborious Endeavours only serve to confirm what the Ancients advanced, without any new Discovery. For *Tournefort*, after having made the Experiments with the *Tournesol* and blue Paper, and given an exact Account of the several active Chymical Principles, which are observ'd in such and such a Plant, usually concludes, *ainsi il n'est pas surprenant s'il a de telles vertues. Therefore, says he, 'tis not surprising if it is endow'd with such Virtues*; which is nothing but giving a Reason why the Ancients believed they were good for such a Distemper.

The Means used by our Forefathers to discover the Virtues of Plants, and their Use in the several Diseases, as they were the most simple, so they are most assisting at this very time. It seems they have narrowly considered their *Facies externa*, and thus concluded; If such a Plant partake of such Virtues, such another so very like to it, must be endow'd with the same, *v. g.*

Apium and *Feniculum* have the same manner of flowering; both produce their Seed after the same manner; their Roots are both alike, being long, white, streight, carnous, &c. Therefore since a long Tract of Experience, handed down by Tradition, shews that such a Plant has such Virtues, such another like to it must have the same. Thus we find *Apium*, *Feniculum*, *Petroselinum*, a'l join'd together, and prescrib'd as the opening Roots in the Dispensatory.

This induc'd that expert Botanist, and diligent Enquirer into the Knowledge of the *Materia Medica*, the Celebrated Dr. *Herman*, to lay down these general Maxims, *Quaecunque flore & semine conveniunt easdem possident virtutes*: And *Omnia semina striata sunt carminativa*. The late ingenious and accurate Natural Historian, sometime a noted Member of this Society, Mr. *James Petiver*, a few Years ago oblig'd us with a Discourse upon this Subject, printed in the *Philosophical Transactions*, in which he observes, That the *Plantæ Umbelliferae*, *Galeatae*, *Verticillatae*, *Tetrapetale*, *Siliquosae* and *Siliculosae*, for the generality, have a tendency to the same Virtue and Use. This was the occasion of some Intercourse betwixt him and me, in two or three Letters, printed in my *Miscellaneous Observations*, in which he observes, that the *Plantæ Flore stamineo*, which he calls Blink Flowers; such as Hops, Nettles, Docks, Sorrels, Betes, Blites, Spinage, *Oraches*, *Bonus Henricus*, or *English Mercury*, and *Kali minus album*, are all good Sallads; raw, or boil'd. Also the *Leguminosae*, or Pea-kind; such as Pease, Beans, *Phaseoli*, are good nutritive Food for Men; and the Tares, Trefoils, *Medica*, *Loti*, and Saintfoins, are good *Pabulum* or Fodder for Beasts. To these he adds the *Fruментaceæ* or *Cereales*; as the Wheat, Rye, and Oats, in *Europe*, and the Maiz, Millet, Panick, and *Sorgum*, in the *Indies*, make good Bread; and that

from

from Barly and Rice we have good fermented and spirituous Liquors. To these he adds, that the *Iris*, or Flag-kind, in Foreign Parts, afford us prevalent Drugs, of no mean Virtue and Use; such as Ginger, Galingal, *Turmerick*, *Zedoary*, *Casumuniar*, and *Cardamoms*. The *Laurus*, or Bay-kind, has some noble Attendants of the same Tribe with it self; such as *Cinnamon*, *Cassia Lignea*, *Malabathrum*, *Folium Indicum*, and the *Camphire Tree*.

In answer to his, I added, that all the *Pappescentes* & *Lactescentes*, such as the *Sonchus*, *Dens Leonis*, *Hieracium*, *Lactuca*, *Cichoreum*, *Endivia*, *Tragopogon*, and *Scorzonera*, have the same Virtues, and serve for the same Uses both in the Kitchin and Shops. All the *Asperifoliae*, such as *Borago*, and *Buglossum*, are those which are called Coolers in a more or less intense degree; for some are Astringent, as *Consolida*, others Narcotick, as *Cynoglossum*. All the *Galeata* and *Labiatae*, for the most part consist of subtile Particles, and are therefore Cephalicks; as *Lavendula*, *Rosmarinus*, *Majorana*, &c. *Mentha*, *Pulegium*, *Melissa*, Hystericks. Attenuaters and Inciders, as *Salvia*, *Horminum*, &c. A fourth Sort somewhat Astringent, as *Bugula*, *Lanium*, &c. So that by having an *Idea* of the Virtues of a *Majorana*, *Mentha*, *Salvia*, *Lanium*, we come to know the Virtues of all their Congeners. All the *Papavers* are Narcotick. The *Esula* and *Tithymali* are Cathartick; tho' both these are Lactescent, yet they differ from those which are Pappescent also. All the *Malva's* are chiefly Emollient; the Pentaphyllous kind Astringent; as are also the Plantains. The Corymbiferous kind, are either Stomachicks, Hystericks, or Vermifuges. The Gentian Bitters, Stomachicks, Hystericks, and Febrifuges. The *Pomiferæ Scandentes*, as Cucumbers, Melons, &c. are Coolers; but some are Cathartick, as *Cucumis sylvestris*, and *Colocynthis*. The *Convolvuli*, as *Mechoacanna*, &c. are Purgative; to which *Jalappa*, by
Flower.

Flower and Fruit, is near of kin. *Digitalis* and *Gratiola*, are Emetick and Purgative. The squamous and bulbous Roots are Emollient, and more or less Acrid. Thus *Allium*, *Cepa*, *Porrum*, unboil'd, are Hot, Diuretick and Lihontriptick. All the *Seda* are coolers.

Thus at the first view, without knowing the Charactericks so nicely as Botanists do, but only exactly observing the *Facies externa* of the Plant, when the Virtue of one Species is known, the Virtues of all the Congeners may be guess'd at, if not fully determin'd.

The next simple Method of the Ancients, to discover the Virtues of Plants, seems to have been the Taste and Smell. Thus *Apium* and *Petroselinum* have a Taste resembling to each other, therefore they are to be prescribed together. The Seeds of *Faniculum* and *Anisum* have much of the same taste and smell, and therefore both of them must be Carminative, or Expellers of Wind, &c. They had likewise recourse to the Temperament and Qualities, such as Hot and Dry, Cold and Moist, in the 1st, 2d, 3d, and 4th Degrees. But since the Taste is not always the same in one Person, and that different Persons have different Sensations; that, as being too much subjected to the different Tempers and Imaginations of People, is deservedly exploded.

I have lately compos'd a Compendious Scheme of all the Plants us'd in Physick; in which, that I might render it less liable to Objection, and not seem to introduce any innovation in the distribution, I have not so strictly observ'd the making their Characterislick Notes and Virtues agree, as the distributing them according to their Operations.

The first Distribution, is, by joining together all those which are prescribed under one Title in the Shops; such as the Opening Roots, Emollient and Capillary Herbs, Cordial Flowers, hot and cold, greater and lesser

fer Seeds. In this I have not kept to the Dispensatory Catalogue, but have added several Congeners, that I might give a Specimen of what is proposed concerning the Virtues and Characters. Thus I have added *Cuminum* and *Meum* to *Feniculum*; *Laurus Alexandrina*, and *Hippoglossum* to *Ruscus*; *Alcea* to *Malva* and *Althea*; *Bonus Henricus*, *Atriplex*, &c. to *Beta*, under the Title of *Oleraceous Emollients*; *Lingua Cervina*, *Polypodium*, &c. to the Capillary Herbs; and so on in the Cordial Flowers, and hot and cold Seeds.

I have, 2dly, distributed the Plants into such as are Altering and Evacuating. The Altering are divided into those that consist of Gross, and such as are said to consist of Tenuous and Subtile Particles. Those consisting of Gross Particles, are Astringent. Such as prevent Abortion and Ruptures, Stoppers of the *Fluxus mensstruus immodicus*, *Fluxus Albus*, *Diarrhæa*, *Dysentery*; good in Burnings, Bruises, Cancers, spitting of Blood. Gross Medicines are Narcoticks, Vulnerary, good for Scrophulous Tumors, Squinancy, Refrigerators.

Plants consisting of subtile Particles, are Aperient; such are all Ophthalmicks, Arthriticks, Nephriticks, Lithontripticks, Diureticks, Hydropicks. They are also Pectoral, Anti-Apoplectick, Paralytick, Hysterick, Hypochondriack, Provokers of Birth, Febrifuges, Scorbuticks, Stomachicks, Vermifuges.

The Evacuating Medicines are Emetick, or such as work upward; or Laxative and Purgative, such as work downwards. The Nutritive Medicines are the *Plantæ Cereales* and *Leguminosæ*.

It is here to be noted, that I have not inserted any Plant in this Table, but such as are indigenals in *Britain*, or such as are Cultivated in *British Gardens*; and to render it still the more useful, I have added such particular Parts as are used in the Shops; viz. The
 Root,

Root, Herbs, Leaves, Tops, Flowers, Fruit, Nuts, Bark and Wood.

Having thus reduc'd within a small compass the most considerable Virtues of Plants, both General and Specifick, and shewn the most easy, simple, and natural Method of discovering them, I would not be so far misunderstood, as if I were averse from using other Experiments in finding them out. On the contrary, I could heartily recommend another Method, hitherto much neglected, and which I am convinc'd would be of great Use, if accurately gone about; and that is, their Infusion in different Liquors, in order to find out the proper *Menstruum* for extracting their more useful Parts.

Every Physician is sensible that there are several Simples, and these Specifick too, which adhibited in Substance, are of great efficacy; whereas, if their Contexture is dissolved, their Parts can never be so reunited as to produce the same effect. Thus *Cortex Peruvianus* is never so effectual, as when given in Powder. That there are others which will communicate their useful Particles when infus'd, to one Liquor and not to another; and that the same Substance will impregnate two Liquors diversely, according to the different *Menstruums*. That expert Chymist, Mr. *Lemery*, advises to infuse *Opium* in Water and Spirit of Wine, separately; and after to mix both Infusions together, in order to make the *Laudanum* or Extract; wisely considering, that the Water will be impregnated by the more soluble saline Particles, whereas the Spirit will only imbibe the more resinous; for Water is the proper *Menstruum* for a saline Substance, which will not dissolve in Spirit of Wine; this rather hardening and preserving it from being dissolv'd, either by Air or Water. Thus the most convenient way to preserve the volatile Salt of
Animals,

Animals, is to keep it among Brandy; and every one knows, that Water immediately dissolves Sugar, which Brandy will not do. Therefore *Senna* will impart its purgative Quality to Water or Ale, having its saline Particles more disengag'd; but the purgative Virtue of *Jallap* consisting in its resine, requires Wine or Brandy for the *Menstruum* or Dissolvent.

Therefore, in my humble Opinion, a most proper Means to find out the Virtues of Plants, is to have recourse to the proper *Menstruums*. A Simple may be infus'd in Rain Water, Snow Water, or pure Fountain Water; if its Texture is loose, and it abound with saline Particles, those pure Elements will be impregnated by it; but if the Texture be more compact, firm and solid, if its Particles are more fix'd, Mineral Waters; or by the addition of a proportional quantity of the fix'd Salt of a Plant, a proper *Menstruum* may be prepared. And next to the adhibiting of the Bitters in substance, such as Wormwood, Gentian, and Camomile Flowers, this is the most convenient way of administering them; not but their Tincture extracted by Brandy or Wine may do very well; But since they abound very much with a fix'd Salt, a great deal of their Virtue may be communicated to a less spirituous Liquor, when a more spirituous will not extract it. The proper means to know which *Menstruum* will best extract the more useful parts of any Simple, or rather suspend its more solid Particles, is to use the Hydrostatical Balance; when having weigh'd the *Menstruum* before infusion, and after the *Materies* has been intus'd for some time, it will soon be observ'd by the Augmentation of the Weight, how far the *Menstruum* is impregnated, and which is the most proper Dissolvent. The properest Method of adhibiting the fix'd Simples, if not in Substance, is by Decoction, Infusion, or Tincture. (N. B.

It is call'd Infusion, when the *Menstruum* is either Water, Ale, or Wine; but a Tincture, when Brandy is employ'd;) and the best way to obtain the useful Particles of volatile, tenuious, or subtile Substances, is by Distillation. These may indeed be proper Ingredients for an Infusion or Tincture. But there are a great many fix'd Substances as improper for Distillation, as the Volatile are improper for Extracts. Thus I have thought fit to shew the means of finding out the Virtues of Plants without dissolving their Texture: But if any has a mind rather to do it by the Chymical Analysis, this is not to dissuade them.

X. *An Account of a Book, Entituled, Geometria Organica, sive Descriptio Linearum Curvarum Universalis. Auctore Colino Mac Laurin, Matheseos in Collegio Novo Abredonensi Professore, & R. S. S.*

THE Design of this Treatise, is to examine the various Methods propos'd by Mathematicians, for describing Geometric Curves; and at the same time to demonstrate a new one, infinitely more General than any hitherto published; built on those Theorems propos'd by our Illustrious President, at the end of his Enumeration of the Lines of the Third Order.

The great Improvements that have been made by most of the other Modern Geometricians, have related chiefly to the Lines of the Infinite Order; they have been so fond of applying their new Methods to Mechanic and Exponential Curves, (which undoubtedly ought to
give

give place to those that are more strictly Geometrical) that they have neglected to cultivate Geometry after the most regular manner. The Writers on these Subjects commonly rise at once, from considering the Lines of the Second Order, or Conic Sections, to those of the Infinite Order, overlooking all the intermediate Ranks. And hence it was, that all the Orders of Geometric Curves lay unregarded, without the known Limits of Geometry, besides the first two, and a few of the superior Curves that had been considered with some particular Views, till that great Author, by enumerating the Lines of the Third Order, enlarged the Bounds of Geometry, and enriched it with almost Seventy new Curves. Their Properties which he has given, and the manner of describing those of them that have a *Punctum Duplex*, have almost brought them on a Level with the Lines of the Second Order; which alone had long usurped the Place in Geometry.

After this great Example, 'tis attempted in this Treatise, to give an universal Description of all Geometric Lines of the Third, or any Order whatsoever. But because the higher Kinds cannot be described but by means of the inferior Sorts, some of these must be postulated to describe those: And because straight Lines are the simplest and most easily described, and are always the same, that is, of one Sort, therefore it was thought proper to investigate of what use they alone might be, for describing Lines of all the higher Orders, in the First Part of this Treatise; an Abstract of which has been published in the Transactions for *January* and *February* last. I shall only add, that besides the Method of describing the Curves, the Manner of determining their Asymptotes and Species is also demonstrated; and the more simple Curves of every Order,

are particularly considered as Examples of the Method. In the First Section the Lines of the Second Order are considered; in the Second, those of the Third Order, that have a *Punctum duplex*; in the 3d Section, the Lines of the Fourth Order, and those of the Third Order that have no *Punctum duplex*. In the last Section there are many various Methods of describing the Lines of any Order.

In the Second Part, the Curves of the inferior Orders are made use of for describing those of the higher kinds. In the First Section, the Theorems published by Sir *Isaac Newton* at the end of the Enumeration of the Lines of the Third Order are demonstrated. In the Second Section, Curves are substituted in the room of straight Lines, in all the Propositions of the First Part. From one of these Propositions, Lines of the 1024th Order may be described by making Angles move on seven Conick Sections; and by three Conick Sections more, Lines may be described above the 11,000th Order. Lastly, these Theorems are applied to shew how the more Complex of the Infinite Order, may be described from the more Simple.

In the Third Section, some other Methods of describing Curves are considered, that are not so general as the preceding, but give sometimes more simple Methods of describing some few Lines of the Superior Orders. Particularly the *Epicycloids* described by the Motion of any Curve, whether Geometric or not, upon another equal to it are easily constructed, and several Infinite Series of them rectified or measured by Arches of more simple Curves. In this Section, several other Descriptions of Curves are treated of, that have been proposed by others. In the last Section, to shew the use of Curves in Natural Philosophy, two of the most eminent Problems in Mathematical Philosophy are solved

ved. In the first, the Centripetal Force, by which a Body describes any Curve, is investigated after an easy manner; and a simple Construction of all those Curves that a body would describe, if projected with the velocity it might acquire by falling from an infinite Height, in any Hypothesis of Gravity, is demonstrated. In the Second, 'tis found, that if any body describe a Curve in a resisting Medium, the Resistance is always as the Moment or Fluxion of a Quantity, that expresses the *ratio* of the Centripetal Force, to that Force by which it would describe the Curve *in Vacuo*, multiplied by the Fluxion of the Curve. 'Tis also demonstrated, that if a body describe any Curve in a resisting *Medium*, which *in Vacuo* could have been described by a Centripetal Force, proportional to any power of the Distance, the Density of that *Medium* will be reciprocally as the Part of the Tangent intercepted between the Point of Contact, and a Line perpendicular to the *Radius* at the Center of the Forces. This Theorem is applied to several Curves; and then the 10th *Prop.* of the Second Book of the Principles, and all its Examples, are demonstrated from it. These Propositions are treated of here, not only because they shew the use of Curves in Philosophy, but because more simple *Ideas* of the Descriptions of some Curves may be drawn from them, than from any other Method; and because this is the Method, by which Nature herself describes Curve Lines.

The whole is concluded by an attempt to draw a Line of any given Order, through any given Number of Points, that is sufficient to determine the Curve. Thus if a Curve of the Order $2m$ is to be described through as many Points, as determine a Line of the Order m , and three more Points, each of which are Nodes, formed by the concurrence of as many Arches of the Curve, as there are Unites in m , then the
Curve

Curve is determined, and a Method how to describe it is demonstrated. This, and some other Theorems relating to the Number of Points that determine Curves, and the manner of describing them thro' these Points, conclude this Part.

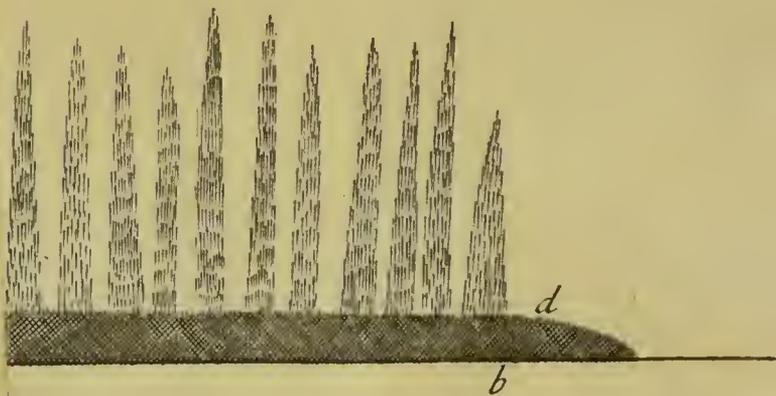
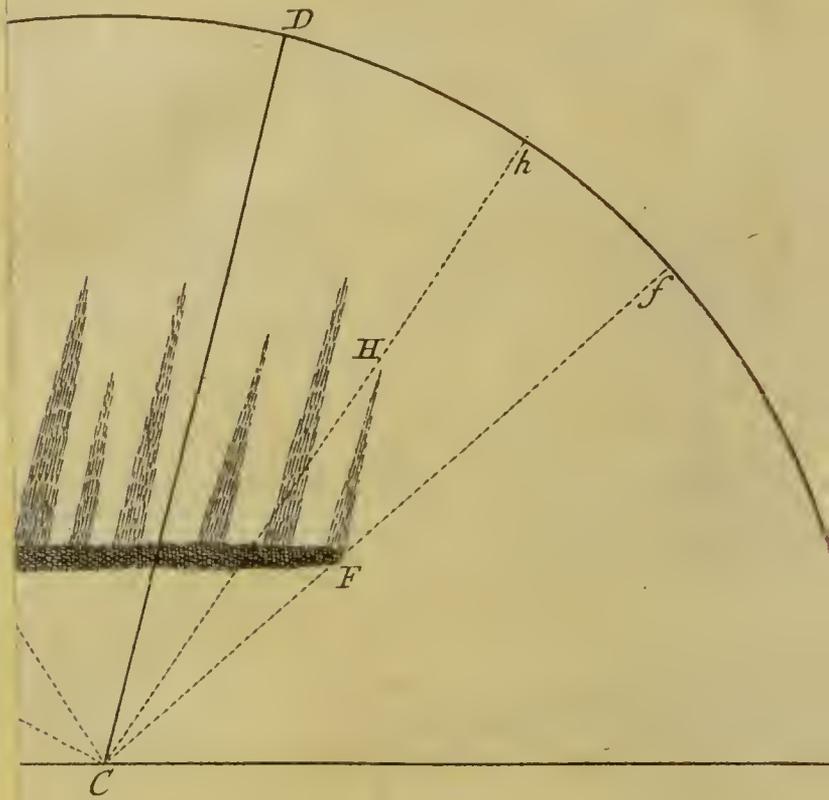
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at the West End of St. Paul's Church Yard.*

E R R A T U M.

P. 14. l. 5. For *Calf* read *Cat*.

L O N D O N : Printed for *W. and J. Innys*, Printers
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PHILOSOPHICAL TRANSACTIONS.

For the Months of *May, June, July and August, 1720.*

The CONTENTS.

- I. **P**ART of a Letter from Mr. Deverel Surgeon at Bristol: Concerning a Case in Chirurgery, which is commonly mistaken for a Fracture of the Patella.
- II. A Dissertation on the antiquity of the Venereal Disease. By William Becket, Surgeon, F. R. S. communicated by Dr. Wagstaffe.
- III. An Account of the great Meteor, which was on the 6th of March, 171 $\frac{1}{2}$. in a Letter from the late Reverend Mr. Roger Cotes Plum. Prof. at Cambridge; to Dr. Danye Rector of Spofforth, in Yorkshire.
- IV. A Discourse on the Operation of Medicines. In a Letter from Dr. Quincy, to the late Learned Mr. Sam. Moreland, F. R. S.
- V. An account of two extraordinary Cases in Chirurgery. Communicated by Dr. Steigertahl, F. R. S.
- VI. An account of an Experiment made before the Royal Society, to prove the Existence of an interspersed Vacuum. By John Theoph. Desaguliers. LL. D. & R. S. S.
- VII. An Account of a Book, entituled, *Conghietture del Dottor Pietre Ant. Michelotti, Filosofo, e Medico d' Arco, sopra la Natura, Cagione e Rimedi dell' infermita regnanti ne' Animali Bovini, &c. Nell' Autunno del' Anno cadente, 1711. In Venezia, 1712.*

- I. *Part of a Letter from Mr. Deverel Surgeon at Bristol ; concerning a Case in Chirurgery, which is commonly mistaken for a Fracture of the Patella.*

RICHARD BURT, a Gardiner, living with Mrs. Lloyd of Stoke near Bristol, was thrown from his Horse, and in the fall receiv'd such a hurt in one of his Knees as made him incapable of remounting: He felt somewhat crack in that Knee (as he express'd it) before it touch'd the ground. Upon examining the Part I found (as I then thought) the ends of the broken Bone drawn above four fingers distance from each other: But upon a stricter examination of the parts, I found the *Patella* (which was drawn upwards by the extensors of the Leg) retain'd its natural Figure, and that the hardness which was felt below was the end of the torn Ligament that ties it to the *Tibia*. The ends of the Ligament were brought as near as possible, and kept so about three Weeks without any very remarkable accident intervening. He then began to walk, which was a little too soon, causing thereby some pains, and loosening the *Cicatrix*, which made it the longer before it was perfectly firm; however he walks without any perceptible lameness: I have met with two others in the same case, the one a Person of distinction, who has not met with the good Fortune of walking so well as she used, tho' not for want of all the care and circumspection imaginable; for it is hardly to be expected that one in ten, to whom this Accident happens, should ever go right, it being next to an

an impossibility that the ends of the torn Ligament should be so exactly plac'd, and retain'd as not to lie one over the other.

Pareè in the 22d Chapter of his 15th Book affirms, that he never saw one of those who have had this Bone broken, but that halted during the rest of their lives : I am very apt to believe what this excellent Author affirms about the halting, and am sorry I must dissent from his notions of the cause, and the cause of the Lameness. *Hildanus* in his *Observationes Chirurgicæ. Cent. 5th. Observ. 88. P. m. 485*, has given us a History of a transverse Fracture of this Bone, which, after all the Symptoms were remov'd, was cur'd. *Sed claudicatio ac summa imbecillitas totius cruris secuta est, ita ut non nisi maxima cum difficultate ambulare possit.* He afterwards mentioning the cause of the Lameness makes a Quæry, if what *Pareè* (to whom this Observation is dedicated) says in his *Exc. lib. de ossibus, lib. 4. cap. 2.* can be the cause of the Lameness; his words are, *Etsi vero Opinionem hancce rationi consentaneam esse in dubium revocare minimè conabor, unus tamen restat scrupulus, videlicet, an, fractâ patellâ, extuberantia calli talis esse possit, ut cavitatem hancce (quæ inter femur & os tibiæ magna est) adeo adimpleat, ut motum actionemque genu impedire possit? Videmus enim ut plurimum in reliquis ossium fracturis, (nisi contritio ossis & periostei fuerit maxima) naturam tam decenter & eleganter connectere ossa, ut rarò relinquatur vestigium fracturæ. Periostium enim, quam diu integrum est, materiam calli retinet, quo minus in extuberantiam excrescere possit, nisi, &c. Deinde in hoc nostro Ægro callum non fuisse causam claudicationis ex sequentibus apparebit: quapropter distinctione hic opus est.* After this he goes on to show us how many ways this Bone may be fractur'd, but doth not observe that he ever saw it any other way broken, (unless in a Gun-

shot.

shot-wound) than transversely; if he had, I am persuaded, that this curious Observator would never have omitted it in his Observations.

Ruyſch in his *Centuria observationum Anatomico-Chirurgicarum* 40. observatio 3, writes as follows. *Patellam propter duritiem suam haud frangi posse nonnulli perverse asserunt. Nos vero eandem non solum à casu graviore in genu frangi experti sumus, verum etiam sine casu; cujus rei Historiam recensere animus est. Ante tres præterpropter annos, visitavi cum Magistro Petro Adriani filio, virum satis robustum, qui a ponte descendens in terram ferè ceciderat; uno pede lubrico, resistens tamen quantum potuit, in Terram non fuerat prolapsus; sed ab illa resistantia vehementissima transversim fracta ejus patella, & adeo quidem evidenter, ut inter utrumque locari potuerit manus, una enim supra altera infra genu sentiebatur.*

This Observation is so like in all its circumstances to that which I related above, especially in that the Fracture was made without a fall, that I am very apt to believe it is the same; and do imagine he might be deceived by mistaking the hard end of the Ligament for the broken Bone.

And when these sort of Accidents, which are now called Fractures of the *Patella*, come to be nicely examined; they will perhaps be found to be generally of the same Nature.

II. *A Letter concerning the Antiquity of the Venereal Disease. By Mr. William Becket, Surgeon, F. R. S. to William Wagstaffe, M. D. S. R. & Coll. Med. Lond. Soc. and by him communicated to the Royal Society.*

S I R,

BEFORE I engage in the principal Design of this Letter; which will be to prove, that the Venereal Disease, when it came to be confirmed, was frequently known among us some hundreds of Years before the Siege of *Naples*: I shall endeavour to refute the Opinion of those Persons, who believe it to have had its rise there, if any such shall remain, who have read over my former Letter*. True indeed it is, that there have not been wanting several modern Authors, who have asserted it; but I determine to make it appear to be an Error as inconsiderately, and hastily received, as started by some Chimerical Author; who, because several Writers about that time, observing the Disease to begin in the *Pudenda*, separated it from another, with which it was before confounded, must likewise take upon him to assert its being a new Distemper, and to assign a certain Time and Place for its Rise. Now one might with all the Reason in the World expect, that, if the Disease had its Original there, it must have been so certainly and infallibly known, that there could have been no doubtful or uncertain Opinions about it, but

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that

* *Philosophical Transactions*, Num. 357.

that the Physicians, who resided in or near the Place, and those more especially, who interested themselves so far as to write of it, must have all of them, to a Man, agreed upon the Certainty of a thing, the Knowledge of the Truth of which was so easily attainable. But on the contrary, *Nicholas Leonicensus*, who was the first *Italian* Physician, that wrote of this Disease, and who lived at the very time, when *Naples* was besieged, is so far from acknowledging it to have had its rise there, from the *French* Soldiers Conversation with the *Italian* Women, and so little did he know of its true Cause, that he does not allow it to be the Consequent of impure Embraces. About this time it was likewise, that Pope *Alexander* the VIth engaged *Gaspar Torella* to write of this Distemper. This Pope was in League with *Alphonsus* King of *Naples*, against *Charles VIII.* King of *France*, to prevent his passing thro' *Italy*, when he went to besiege *Naples*; yet this Author is so far from allowing it to have had its Original there, that he tells us, the Astrologers were of opinion, that it proceeded from I know not what particular Constellations. Nor does *Sebastianns Aquilanus*, who lived at that time, allow it to be any other than an ancient Disease; or *Antonius Scanarolius*, who wrote in 1498, which was but four or five Years after the beforemention'd Siege. Nor do several other Authors, then living, say one Word about this *Neapolitan* Story. But it seems *Ulricus de Hutten*, a *German* Kt. who was no Physician, positively affirms this Disease to have had its rise there; but how he should come to know this, who lived at such a distance from the Place, and they, who were Physicians residing as it were upon the spot, be ignorant of it, will be as much credited, as his following inconsistent Relation, which will sufficiently prove, how

how little care he took to be apprised of the Truth of what he wrote. This very Author tells us, this Disease was unknown till the Year 1493, or thereabouts; that he himself had it, when he was a Child, and so consequently that it was hereditary, or from the Nurse. He wrote his Book of this Distemper at *Mentz*, where it was printed by *John Scheffer* in 4to, in the Year 1519. Now if we allow him to be but 27 Years of Age, when he wrote, (for he cannot be suppos'd to be less, who before this took upon him to cure his Father of the Venereal Disease, without the Assistance of any Physician or Surgeon,) he must have had the Distemper upon him, according to his own Account, before ever it was in being. Thus we may see, how Persons may be impos'd upon by a hasty and inconsistent Writer, no way qualified for such an Undertaking, and greedily receive in Fashions instead of Truths, if they will not be at the Pains of consulting the Original Writings of our Predecessors, the only sure Method of overthrowing such Chimerical and imaginary Notions.

But to come to what is principally designed in this Letter; If I have, *Sir*, in my former sufficiently proved, that the first Degree of the Venereal Disease was very common among us some hundreds of Years, before it is commonly said to have been known in *Europe*; there will be no Reason for any body to conceive we were at that time in any measure Strangers to it, when it came to be confirmed; more especially, when we consider the Methods of Treatment in those Times, which consisting principally in topical Applications, many of their Patients could not possibly escape having it confirmed on them. Now when it was in this confirmed State, the Writers of those early times looked upon it as an entirely new Disease, and not a

Consequent of any Evil before contracted, because they were not apprised, that the first Symptoms being removed, and the Disease to Appearance cured, it should afterwards discover it self in such a manner, as should not seem to have the least Analogy with the Symptoms, that first attack'd a part, which had been for a considerable time free from any Misfortune: But because the Symptoms are the only true Characteristics, whereby we are infallibly able to know one Disease from another, it may be expected, that I produce sufficient Authorities, to demonstrate they were all of them known and described by ancient Physical and Chirurgical Writers, just as they appear to be in the Venereal Disease at this Day, if I would prove that Disease to be of a much more ancient Date, than is generally thought; and if I do this, I cannot but think it will be satisfactory, since we can have no other way of coming to a Knowledge of any one Distemper, than by its Symptoms. The Method of laying down the exact Succession of them, will be impossible to be reduced to any certain and infallible Rule, there being so great a Variety of Causes, that obstruct such a Regularity; for which Reason, I shall take notice of them in such Order as they most generally appear, which was upon no account to be expected from our ancient Writers, for as much as they mention every particular Symptom by it self, not knowing but that they were independent of each other, and that each of them was a distinct Disease. However, the proving these Symptoms were in being in these early times, will be as strong an Argument to prove the Antiquity of this Distemper, as if they had been register'd in the most exact Order of Succession, because we shall, upon the strictest Examination,

tion, find they are peculiar to the Venereal Malady only. I have, I hope, sufficiently made it appear in my former Letter, that the first degree of this Disease was anciently known among us by the name of the Brenning, or Burning; and that it was the same Thing with what we now call a Clap. The Symptoms, which are usually its concomitants, are the Phymosis, and Paraphymosis, both which are accurately described, and proper Remedies for them set down by the before mentioned *John Arden* Esq; in another Manuscript of his, curiously written upon Vellum, and beautifully illuminated. The imprudent Method of Cure of this first degree of the Venereal Malady, is sometimes attended with a Caruncle in the Urethra, which was a Disease very common among us anciently: For not to mention other early writers, our before mentioned Author gives us the Case of a certain Rector, that had such a Substance, like a Wart, growing in the Penis, which in another Place he says frequently happens, and of another, which had such an Excrecence as big as a small Strawberry, which (says he) proceeded from the corrupted Matter, which remained in the Urethra.

And indeed there is not any Symptom of the Venereal Disease, that I find so often mentioned as this of the Caruncle, insomuch that it seems to have been more common in those early times, than at this Day. But this must be certainly owing to the smooth and oily Remedies they were continually injecting, which, by their relaxing and softning the Fibres of the part; must necessarily dispose the contexture of small Blood Vessels, lodged at the bottom of the little Ulcerations, to fill with nutritious juices, and to extend themselves so, as

to form such fungous Excrescences; and so solicitous were they for removing these inconveniencies, that they made use of several ways by Corrosives and other Methods, to accomplish this end; and a very early Writer among us, has given us a very methodical and curious Tract on this Subject, wherein he recommends the removing them by the medicated Candle, which we use at this Day, and lays down divers other Instructions, in Relation to it, which makes it probably the best Discourse on this Subject, that was ever yet written. The same Author takes notice of those contumacious Ulcers, which happen upon the *Glans* and the neighbouring parts, which we now call Shankers; and the great Trouble, our ancient Authors found in attempting their Cure, sufficiently discover them to have had their Original from a Venereal Infection. These several Symptoms of the Venereal Malady our early Writers are very full in their Accounts of, and others, when the Disease was in a more confirmed State, to which they appropriated particular Names, perhaps more significant and expressive than those imposed by modern Authors. Thus for instance, the Bubo's in the Groins they called Dorsers, which I have given a Reason for before; and the Venereal Nodes on the Shin Bones they termed the Boonhaw, which gives us a perfect Idea, not only of the part affected, but after what manner it was Diseased; for the old English Word, *Hawe*, signified a swelling of any Part. Thus for instance, a little Swelling upon the *Cornea*, was anciently called the Hawe in the Eye, and the Swelling that frequently happens on the Finger, on one side the Nail, was called the white Hawe, and afterwards Whitflaw. The process our last mentioned Author recommends, for the Cure of the Boon or Bone Hawe, is by making use of a Plaister, which had

had a Hole cut in the midst, to circumscribe it; and applying a Caustick of unslacked Lime, and black Soap incorporated together, which, with Plaister and Bandage, was to be secured on the part four Hours, and longer, if that was not found sufficient; after this he proceeds to the separating the Slough, &c. This Practice of his seems to have been found out by accident. For he tells us, when he was a young Practitioner, he having applyed both the Natural and Artificial Arsenick to the Leg of a Man, that was his Patient, it so mortified the Flesh, as surprized him; but by proper digestives, the Eschar coming off, and leaving the Bone bare, he scraped it with an Instrument for several Days, and drest it with Incarnatives, designing to have ingendred Flesh on it; but this proving unsuccessful, he continued to scrape it, till he observed it move under the Instrument, after which having separated it, he found the Sore covered with new Flesh, and that the Bone was four Inches in length, two in breadth, and very thick, upon the removal of which the Patient was soon cured. Thus its probable this Observation of this great Man led our Predecessors to practice the very same Method; and we do at this Day in our Hospitals treat the Venereal Nodes on the Shins exactly as is here described, where we observe the same appearances, he so long before took notice of; and 'tis not in the least to be doubted, but the Boon Haw and our Venereal Nodes are the same Disease. By the appearance of some of the last of the above mentioned Symptoms, we infallibly judge the Patient has had the Infection upon him a considerable time, and that the Disease is making its gradual advances, to the corrupting and destroying the whole frame of the Body. That this was the Conclusion of the miseries of those Persons,

that

that gave themselves up to the deceitful delights and entertainments of lewd Women, in those early times as well as now, I cannot better prove than by those remarkable instances you quoted from a MS. in *Lincoln Colledge, in Oxon*, which you kindly communicated to me, after you had mentioned them in one of your Learned Lectures in our Theatre. They are as follow, *Novi enim ego Magister Thomas Gascoigne, licet indignus, sacrae Theologiae Doctor, qui hac scripsi & collegi, diversos viros, qui mortui fuerunt ex putrefactione membrorum suorum genitalium & corporis sui; quae corruptio & putrefactio, ut ipsi dixerunt, causata fuit per exercitium copulae carnalis cum mulieribus. Magnus enim dux in Angli, scil. J. de Gaunt, mortuus est ex tali putrefactione membrorum genitalium, & corporis sui, causata per frequentationem mulierum. Magnus enim fornicator fuit, ut in toto Regno Angliae divulgabatur, & ante mortem suam jacens sic infirmus in lecto, eandem putrefactionem Regi Angliae Ricardo secundo ostendit, cum idem Rex eundem Ducem in sua infirmitate visitavit; & dixit mihi qui ista novit unus fidelis sacrae Theologiae Baccalaureus. Willus etiam longe vir maturae aetatis & de civitat. Londontii, mortuus est ex tali putrefactione membrorum suorum genitalium & corporis sui, causata per copulam carnalem cum Mulieribus, ut ipsemet pluries confessus est ante mortem suam, quum manu sua propria eleemosynas distribuit ut ego novi anno Dni. 1430.* Now what those instances mentioned from *Arden*, or these from *Gascoigne*, who was then Chancellor of *Oxford*, cou'd possibly be but Venereal Cases, I would be obliged to any body to inform me. Certain it is, no Disease was ever known to be gotten by the carnal Conversation of Women, which first attacked the *Genitals*, causing a Corruption and Putrefaction of them, and afterward of the whole Frame of the Body, but that which is Venereal. For nothing is more commonly

commonly known at this Day, than that after the venereal ingagement with an impure Woman, the Penis is the part where the Scene is first laid for the succeeding Tragical appearances; and there, and in the Neighbouring Parts, do the Symptoms of the Disease as its retainers, always first assemble, till the malignant Poyson taints the Blood and other Juices; which being convey'd over the whole Frame of the human Fabrick, if not check'd, soon brings about its total Corruption.

What I have further to add in relation to this, is, because we do not find the Disease mentioned by *Gascoigne*, was distinguish'd by any particular Name, and that great Numbers must unavoidably die of the Venereal Malady at that time, from the imperfect Knowledge of those who had the treatment of the first Degrees of it, it must necessarily follow, that when the whole Frame of the Body had received a taint from the Venereal Poyson, so as to occasion its breaking out in Scabs and Ulcers, almost all over its Surface it must generally be called by the Name of some particular Disease, whose appearances had somewhat of an Affinity to it. Now if we examine the Nature of all the Diseases that attack the Human Body, we shall not find the Venereal Malady, when it arrives at this State, to bear a greater Similitude to any than the Leprosy, as it is described by the Ancients: Nay so great was the Analogy betwixt these Diseases supposed to be, that *Sebastianus Aquilanus* has endeavoured to prove from *Galen*, *Avicen*, *Pliny*, &c. That the *Pox* is only one Species of the Leprosy; and *Jacobus Cataneus*, a Writer almost as early as the rise of the Name of the *Pox*; tells us 'tis not only possible there may be a transition, from one of these Diseases into the other; but that he saw two Persons

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in whom the *Pox* was changed into the Leprosy : That is, from having great Pockes or Pustules on the Surface of their Bodies, from whence the *Pox* is denominated, to have become Ulcerous or Scabby. This particular State of the Disease anciently put the Surgeons to a great deal of trouble: For they finding that these Ulcers were of a very contumacious and rebellious Nature, they were obliged to make use of great numbers of remedies, in order to conquer the evil Disposition of them. But they observed that all of them were useless, unless Mercury was joined with them. Now the dressing each particular Ulcer being so very tedious, they ordered the Patients to daub the Ointments over the Parts which were ulcerated; which done they were wrapt in Linnen Cloaths till the next dressing: But after a few Days they were extremely surprized, to find their Mouths began to be sore, and that they spit very profusely; but they tell us to their astonishment, that in a little time the Sores became healed, and the Patients cured. And by this accident it was the Method of Salivating by Uñction was first discover'd, which is in so much use among us at this Day. From these and some other instances I have given of the Industry and Application of our Predecessors, and with what Sagacity they applyed every accidental hint, to the relieving their distressed fellow Creatures from the Misfortunes they laboured under; we ought to be led to the highest Esteem and Veneration of them, (and so much the more most certainly) forasmuch as they were principally our own Country Men, who I can prove not only from several Persons, coming from Foreign Parts to be cured of their Diseases here in *England*, but for other Reasons that they excelled most of their Contemporaries in the Divine Art of Healing. Now although

though those Foreign Authorities, I before mentioned, might be looked upon as sufficient to convince any one, how our Ancestors blended these two Diseases together: Yet shall I pursue my designed Method, and prove from our own Writers, long before those, that altho' the *Pox* was not only among us, but in distant Nations anciently confounded with the Leprosy; yet so exact were our Writers in their Observations of the Infectious Nature of one Species of that Disease, and describing the Symptoms, as was sufficient to lead any Person to the distinguishing between them, so as to separate one Disease from the other. I shall therefore first enquire into the manner how the Leprosy was sometimes said to be gotten in those early times, and then examine the Symptoms of the Disease, that attacked the Patient. *John Gadisden* a very learned and famous *English* Physician, who flourished about the Year 1340, in an excellent Work of his, he entitles *Rosa Anglica*, speaking *de infectione ex coitu Leprosi, vel Leprosæ*, says as follows, *Primo notandum quod ille qui timet de excoriatione & arsure Virgæ post coitum statim lavet Virgam cum aqua mixta aceto, vel cum urina propria & nihil mali habebit*; and in another place speaking, *de Ulcere Virgæ*, he says, *sed si quis vult membrum ab omni corruptione servare, cum a Muliere recedit, quam forte habet suspectam de immunditie, lavet illud cum aqua frigida mixta cum aceto, vel urina propria intra vel extra præputium*, He likewise speaking still of the Leprosy recommends a Decoction of Plantain and Roses in Wine, to be made use of by the Woman, immediately after the Venereal Encounter; upon which he tells us she will be secure. From hence it is evident some of their Leprous Women (as they call'd them) were capable of communicating an infectious Malady to those that had carnal conversation with

them which proves, the Pudenda of the Women must be diseased, for as much as we are absolutely assured Infections of that Nature only happen when a sound Part comes to an immediate Contact with a diseased one; for the Symptoms always first display themselves in those Parts through which the Virulency is first conveyed. Now in a true Leprosy we never meet with the mention of any disorder in those Parts, which, if there be not, must absolutely secure the Person from having that Disease communicated to him by coition with Leprous Women; but it proves there was a Disease among them, which was not the Leprosy, although it went by that Name; and that this could be no other than Venereal because it was infectious: for there is no other Disease that is capable of being communicated this way but the Venereal Disease, seeing the Pudenda are only in that Distemper so diseased as to become capable of communicating their Contagion. I find the learned *Gilbertus Anglicus* who flourished about the Year 1360, reasoning concerning the manner how it is possible a Man shou'd be infected by a Leprous Woman; where if we allow him to call the Malignant Matter, which, is lodged in the Vagina, [*the Womans seed*] we shall find he accurately describes the very first Venereal Infection, by part of the virulent Matters being received into the Urethra; from whence by the Communication of the Veins and Arteries, it is conveyed into the whole Body, after which (says he) ensues its total Corruption. Let us now examine the Symptoms of one sort of their Leprosy, for it must be necessarily divided into different Species, when another Distemper was blended with it, in which we observe such a diversity of appearances; and this I shall the rather do in this Place, because it will furnish us with the

next Succession of Symptoms after those already mentioned, as the Venereal Ozænas, the Ulcers of the Throat, the Hoarsness, the proof of its being communicable from the Nurse to the Child, by Hereditary succession, &c. All which we find to be true in the Venereal Disease at this Day. Our Country-Man *Bartholomew Glanville*, who flourished about the Year 1360, in his Book *de Proprietatibus Rerum*, translated by *John Trevisa* Vicar of *Barkley* in 1398 tells us *some Leprous Persons have redde Pymples and Whelkes in the Face, out of whome oftenne runne Blood and Matter: In such the Noses swell and ben grete, the vertue of smellynge faylyth, and the Brethe stynkyth ryght fowle. In another place the same Author speaks of unclene spoyd glemys and qyttery, the Nofethrilles ben stopyl, the wafen of the Voys is rough, and the voyce is horse and the Heere falls. Among the Causes of this loit of Leprosy, he reckons lying in the Sheets after them, easing Nature after them; and others which the first Writers on the Pox looked upon to be capable of communicating that Contagion: also, says he, it comyth of fleshy lyking by a Woman, after that a Leprous Man hath laye by her; also it comyth of Fader and Moder; and so thys Contagyon passyth into the Chylde, as it ware by Lawe of Herytage. And also when a Chylde is fedde wyth corrupte Mylke of a Leprouse Nouryce. He adds by what ever Cause it comes, you are not to hope for Cure if it be confyrmyd; but it may be somewhat hidde and lett that it dystroye so soone. Thus we see how our Author under the Name of one Species of the Leprosy, gives a Summary of the Symptoms of the Pox, and the severall ways whereby it is at this time communicated. Now when these two Diseases were anciently blended together and passed under the Name*

of

of the Leprosy only, it must be the real Cause why that Disease seemed to be so rife formerly; for two Distempers passing under one Name must necessarily make it more taken notice of and much more frequent; not but that much the greater Number of those who were formerly said to be Leprous were really Venereal, seems to be very evident; for since that Disease has been separated from the Leprosy, it has drawn off such vast Numbers, that the Leprosy is become as it were a perfect Stranger to us. Those that are acquainted with our English History well know the great Provision that was anciently made throughout all *England* for Leprous Persons, insomuch that there was scarce a considerable Town among us but had a Lazar-House for such diseased. In a Register which belonged to one of these Houses, I find there were in *Hen.* the VIIIth's time six of them near *London*, (*viz.*) at *Knight's Bridge*, *Hammersmith*, *Highgate*, *Kingsland*, the *Lock*, and at *Mile-end*, but about 40 Years before I find but four mentioned; and in 1452 in the Will of *Ralph Holland*, Merchant Taylor, registred in the Prerogative Office, mention is made but of three, which, with his Legacies to them, are as follow. *Item lego Leprosis de Lokes, extra Barram Sti Georgii* 20s. *Item Lego Leprosis de Hackenay* (which is that at *Kingsland*) 20s. *Item lego Leprosis Sti Egidii extra Barram de Holborn* 40s. from which its worth while to note, that the *Lock* beyond *St. Georges Church*, and that at *Kingsland*, are at this time applyed to no other use than for the entertainment and Cure of such as have the Venereal Malady. Some of our learned Antiquaries have been much concerned to know the Cause why the Leprosy shou'd be so common in those early times, and so little known among us now: But I believe the Reason will be impossible

possible to be assigned, unless we allow according to the Proofs which I have already brought, that the Venereal Disease was so blended with it, as to make up the Number of the diseased. It seems to have been the same thing with them in *France* as with us: For the Author of the History of that *Kingdom*, which was lately published here in two Volumes in Octavo tells us that the House of the Fathers of the Mission of *St. Lazarus*, was formerly an *Hospital* for Leprous People, but that Disease being ceased in this last Age (since the *Pox* has been separated from it) these Lazar Houses have been converted to other Uses; and it may not be perhaps foreign to my purpose to take notice that the Writ *de Leproso amovendo* contained in the Register of Writs was, (according to *Coke* upon *Littleton*;) to prevent Leprous Persons associating themselves with their Neighbours, who appear to be so by their Voice and their Sores; and the Putrefaction of their Flesh; and by the Smell of them. Well then, let us examine what Method was to be taken to prevent this noysom and filthy Distemper, the Leprosy; why truly that which would infallibly prevent their getting the *Pox* after the usual Method, and that was Castration. It is certain that *Eunuchs* are rarely or never troubled with the Leprosy, according to *Monsieur le Prestre*, a Councillor in the Parliament of *Paris*, who has these Words (a) *Antipathia vero Elephantiasis veneno resistit: Hinc Eunuchi & quicumque sunt mollis, frigida & effeminate naturæ nunquam aut raro Leprosæ corripuntur, & quidem quibus imminet Leprosæ periculum de consilio medicorum sibi virilia amputare permittitur.* And *Mezeray* says, he has read in the Life of *Philip the August*, that some Men had such Apprehensions,

(a) Centur. I Cap. 6. de separatione ex causa Luis Veneræ.

prehensions of the Leprosy, (that shameful and nasty Distemper) that to preserve themselves from it, they made themselves *Eunuchs*. Now it is highly probable that those Persons that submitted to such a painful Operation, having before observed, that those that gave themselves up to a free and unrestrained use of Women, fell at length under such unhappy circumstances; and so found the only measures to preserve themselves from it was to be disabled for such engagements, which sufficiently proves this Species of the Leprosy was Infectious; and for the reasons before assigned could be no other than Venereal; for how the true Leprosy should be prevented by such means will be, I believe, impossible for any Person to determine. There yet remains one very considerable Symptom of the Venereal Malady for me to take notice of, because it is looked upon to be the most remarkable in that Disease, which, is the falling of the Nose; but since it has been already proved, that this Disease when it had arrived to such a pitch as to discover it self by those direful Symptoms as are the immediate forerunners of this, was by the Ancients confounded with the Leprosy, and called by that Name, it must be among the Symptoms of that Disease we are the most likely to meet with it, if any such thing as the falling of the Nose was known among them. Now the most likely Method of coming to a certain Knowledge of the Infallible Symptoms of the Leprosy of the Ancients in its more confirmed State, is to consult the Examinations those unhappy Persons were obliged to undergo, before they were debarred the Conversation of Human Society, and committed to close confinement: But this being a thing some Ages since laid aside, no Author that I know of having the particular History of it, and somewhat of it

being absolutely necessary in this design, I shall do it as briefly as I can from what Remains I have met with in Records, and other scattered Papers. First then, after the Persons appointed to examine the diseased, had comforted them, by telling them this Distemper might prove a Spiritual Advantage; and if they were found to be Leprous, it was to be looked upon as their Purgatory in this World; and although they were denied the World, they were chosen of God: the Person was then to swear to Answer truly to all such Questions as they should be asked; but the Examiners were very cautious in their enquiries, lest a Person that was not really Leprous should be committed, which they looked upon to be an almost unpardonable Crime: They considered the Signs as Univocal, which properly belonged to that Disease, or Equivocal, which might belong to another, and did not upon the appearance of one or two Signs, determine the Person to be a *Lazar*; and this I find to be the Case of the Wife of *John Nightingale Esq*; of *Burntwood* in *Essex*, who in the Reign of *Edw. the IVth An. 1468* being reported to be a *Lazare*, and that she did converse and communicate with Persons in publick and private Places, and not (according to custom) retire her self, but refused so to do, was accordingly examined by *William Hattecliff*, *Roger Marcall*, and *Dominicus de Serego* the Kings Physicians; but they upon strict enquiry adjudged her not to be Leprous, by reason the Appearances of the Disease were not sufficient: Some of the Questions put to the Leprous Persons (as they called them) which will more fully confirm what I have before advanced, I shall now give as I transcribed them from an Ancient Book of Surgery *if there were any of his lygnage that he knew to be Lazares and especially their Faders*

and Moders; for by any other of their Kynred they ought not to be Lazares, then ought ye to enquire yf he hath had the Company of any lepreys Woman, and yf any Lazare had medled with her afore him; and lately because of the infect matter and contagious filth, that she had received of hym. Also his nostrills be wyde outward, narrow within and gnawn. Also yf his lips and Gummes are foul stynking and coroded, Also yf his voice be horse, and as he speaketh in the nose. Now the Sigas which are here mentioned, were looked upon to be Univocal: And these were they that made the Examiners principally determine the Persons to be Leprous; but what Determinations any one would immediately give from such Symptoms now, no Person is surely ignorant of. But even these certain appearances would not always satisfy some Persons, if we may believe *Felix Platenus* in his Medicinal and Chirurgical Observations, *Lib. 3.* who tells us some did not look upon them to be so. till they had an horrible aspect, were hoarse, and Noses fell. Likewise in the *Examen Leprosorum* printed in the *De Chirurgia Scriptorum Optimi*, the Author speaking of the Signs of the Leprosy relating to the Nose begins thus, *Si nares exterius secundum exteriorem partem ingrossentur, & interius constringantur, & coarctentur. Secundo si appareat cartilaginis in medio corrosio, et casus ejus significat Leporam incurabilem.* And the before mentioned *John Gadsden* in his Chapter *de Lepra* says as follows, *Signa confirmationis etiam incurabiliter sunt corrosio cartilaginis quæ est inter foramina & casus ejusdem.* Thus Sir have I proved we had a Distemper among us some hundreds of Years before the Venereal Disease is said to have been known in Europe, which was called the Burning; that this Burning was Infectious, and that it was the first Degree of the Venereal Disease; that this being
common

common at that time, from their Method of Treatment; the *Pox* must be unavoidable: That it had exactly the same appearances it has now, although they were generally called by different Names, that the Ancients confounded it with the Leprosy; that the vast Numbers of Leprous Persons among us, before the Venereal Disease was separated from it; and the small Number we observe at this Time, is a flagrant Proof of the former; that in describing the Symptoms of the Leprosy, they give us those of the Venereal Malady; and, by mentioning how it is communicated, they describe the ways by which the *Pox* is gotten at this Day; that such Remedies were by them recommended to prevent the first attack of the Leprosy, as are at this Time in Use to prevent the first Symptoms of the *Pox*; and that the falling of the Nose, which has been look'd upon to be the most remarkable Symptom of the Venereal Disease, was commonly observed in what they called the Leprosy in former Ages.

I am, Sir,

Yours &c.

William Beckett.

III. Part

III. *A Description of the great Meteor which was on the 6th of March, 171 $\frac{5}{6}$. sent in a Letter from the late Reverend Mr. Roger Cotes, Plumian Professor at Cambridge, to Robert Danye, D. D. Rector of Spofferth in Yorkshire.*

THE appearance of the Meteor was very nearly the same with us here. at *Cambridge* as with you, excepting that the triangular Streams of Light were not so permanent as you seem to describe them, and the Point to which they all converg'd was distant from the Zenith about 20 Degrees, its Azimuth lying between the South and the East at about 10 Degrees from the South, towards which Point of the Compass the Wind tended. The position of this point of Convergence may be more accurately determin'd, if there be occasion: For at a quarter after Seven, when the appearance to us was in its greatest perfection, it lay nearly in the middle between the two bright Stars, in the heads of *Castor* and *Pollux*. I am told that some Streams were seen to shoot forth immediately after Sun set, and that they did not perfectly cease till about 3 or 4 in the morning.

It was after Seven before I had notice of this uncommon sight. At first I saw only two or three of the triangular

triangular Streams towards the North and North-West: These were not of long Duration, but were succeeded by others which appear'd and vanished again by turns, arising from, and ascending up to Places in the Heavens, of very different Altitudes above the *Horizon*. From the time I began to view them, they continued to ascend more and more copiously, being propagated still further and further from the North towards the West and East, and directed always to the Heads of *Gemini*, till at length when they seem'd almost to meet at the Point of Convergence, they began to ascend up towards it from the Southern Parts also and all around it; insomuch that at a Quarter after Seven, we had a perfect Canopy of Rays over us: The bottom of this Canopy did no where reach down to the *Horizon*; for near the North, where it descended the most, its Altitude was about 10 or 15 Degrees; and near the South where it descended the least, its altitude was about 40 Degrees. It remain'd in this State about 2 Minutes, during which time, we saw several Colours, some fainter and more permanent, others brighter, but quickly vanishing. Thus in the West I observ'd the Rays to be ting'd for some considerable time with an obscure and heavy Red; and in one of the brightest Streams at another time, there suddenly broke out a very vivid Red which was instantly and gradually succeeded by the other Prismatic Colours, all vanishing in about a Second of Time. These Colours affected the Sense so strongly; that I thought them to be more intense than those of the brightest Rainbow I had ever seen. A small time before the Appearance lost it's perfection; we were surpriz'd to observe a Shaking and Trembling of the Streams chiefly in their upper Parts, during which,

which their Convergence was confounded, and the whole Heaven seem'd to be in a Convulsion. At the same time I cou'd perceive Waves of light towards the North, which moved upwards, and in their motion cross'd the streams, lying parallel to the Horizon. These Waves were different from those broad ones, which you mention, and which I also took notice of: Their breadth seem'd to be about a Degree, their length about 90 Degrees; and I can compare them to nothing better than to those slender Waves upon the surface of stagnant Water, which are made by casting in a small Stone.

About seven or eight Years ago, I happen'd to see a Meteor which it will be of use to describe to you. Along the Horizon in the North, there lay a white and luminous, and seemingly dense matter in the form of a Cloud represented by a b c d; the length of it, a b, was about 10 or 15 Degrees. From this there arose, directly upwards, pointed streams of the like luminous and white matter, which yet did not appear in any part of it to be so dense as the former; and grew gradually more and more rare in its upper Parts so as to vanish almost insensibly at the Points. There was some little difference in the height of these streams, but they generally ascended up to about 4 degrees above the Horizon. They were very numerous and contiguous to each other, and seem'd to be compos'd of very slender parallel filaments or Rays. This was the common appearance, and the only remarkable Thing which I farther observed was, that sometimes a Fire or Flame would break out in the Cloud, a b c d, and move along it in a direction parallel to the Horizon: And during this motion, a pointed Stream directly over the Fire seem'd to run along with it, and to pass by the
other

other more fix'd Streams to which it always kept it self parallel.

I am persuaded that the late Appearance was of the same Kind with this, which, I have now been describing. For let A B, represent the plane of the Horizon, C the place of the Spectator. E F, a fund of Vapours or Exhalations at a considerable height above us, diffus'd every way into a large and spacious Plane, parallel to the Horizon. This Fund of mixt matter by Fermentation will emit Streams from it self, such as EG, F H, &c. which, if the Wind be perfectly still, will ascend perpendicularly upwards; if it be boisterous and irregular, they will be blended and confounded together; but if it be very gentle and uniform, as it was at the time of our Appearance, they will be inclined towards the point of the Horizon, which is opposite to that from which the Wind blows. Now if A D B represent the concave of the Heavens and a Line, C D, be drawn parallel to the Columns E G, F G, &c. 'tis certain by the Rules of Perspective, that these Columns will appear upon that concave to converge all around towards the Point D: Thus the Column, E G, will seem to arise from the Point e, to ascend up to g, and to take up the Space e g; and in like manner the Arch f h will be the Projection of the Column F H. From hence it is evident that the reason why the triangular Streams ascended at first only from the Northern Parts of the Heavens was this: The Fund of Matter, E F, was not yet arriv'd by its motion to the Line C D. After it had pass'd that Line, it is plain they must appear to ascend from all Quarters. A great number of Columns being therefore dispos'd to emit Light, at the same time, caus'd that perfect Canopy, which, I described above. The reason why that Canopy descended lower in the North, than in the South, was this: The shining Columns

lums which had not yet pass'd the Line C D, were more numerous and more remote from it than those which had pass'd it; for if the Point E, be farther distant from C D than the Point F, the Arch A e, must needs be less than the Arch B f. An irregular gust of Wind blowing upon and shaking the Columns, was (I suppose) the Cause of that trembling, which appear'd in the triangular Streams, and the Cause also which destroy'd that fine appearance of the Canopy. The slender circular waves seen at the same time might also be explain'd from the same Cause. I need not detain you any longer by endeavouring to make out some other particulars of this unusual Appearance: I fear I have been already too tedious. However I will not omit to mention a very easy Contrivance by which the Thing may be tolerably well represented to view. Take a Hoop and round about it fasten several streight Sticks parallel to each other, but all inclin'd to the plane of the Hoop, hold this plane parallel to the Horizon, and in that posture move it with Sticks over a Candle, the shadows of the Sticks upon the Ceiling of your Room, will converge to a point not directly over the Candle, (as they would have done, had the Sticks been perpendicular to the plane of the Hoop) but to the Point in which a Line drawn from the Candle parallel to the Sticks, shall intersect the plane of the Ceiling.

IV. *A Letter of Dr. John Quincy, to the late learned Mr. Sam. Moreland, F.R.S. concerning the Operation of Medicines.*

WHAT I had an opportunity some time since of saying in Conversation concerning the Operation of Medicines, and particularly of Purges, I here beg leave to send you for a Re-Examination, because I shall be glad to have my thoughts confirm'd by your Approbation, or be advertis'd of any mistakes which self Partiality may lead me into, before they are too far drawn into Consequences.

With regard to this affair I suppose,

1. That all those parts of an animal Body, which are vascular, or through which any Fluid passeth, from the Intestines to the minutest Fibre, are the seat of Medicine's Operation.

2. That this whole course of Circulation, or Animal Motion is naturally distinguished into three different Stages, by the different capacities of the Vessels and motions of their Contents, each having its proper Out-let, and that these are the seat of the three Concoctions, so often mentioned by Physical Writers; the first in the Stomach and Bowels, having the Anus for its Emunctory; the second, all within the Blood's motion, so far as it retains its Colour, having the Kidneys; and the third, all beyond that Circuit, having the Skin for an excretory Organ.

3. That every Medicine which causeth Evacuation is a Purge.

4. That every Purge operates as a Dissolvent, by fusing the Juices, and increasing the quantity fit for Ex-

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pulsion;

pulsion; or as a Stimulus, by accelerating their Motions, so as to bring the matter fit for Expulsion, of-
tener to the secretory Outlet; or both.

These Assumptions, which, I expect, will not be disputed, are premised only for the better proof of this grand Proposition.

That a change in the Bulks, Figures, and Motions of the component particles of a purging Medicine, will change the seat of its Operation, and fit it for exertion in larger or lesser Vessels as those mechanical Affections are intended, or remitted.

For Illustration hereof, it may be convenient to attend the management of common Practice, in making a Purge operate more, or less, than it otherwise would do.

Substances which are gross and heavy, as those consisting chiefly of Saline and earthly Particles, such as Tartar, Manna, and the like, if they are reduced smaller by Triture, or repeated Solutions, operate more gently, but if acuated by Acids, or any management which exposeth their Angles more sensibly to the Membranes, they are rougher, and take Place sooner.

Resinous Medicines, as Scammony, Gamboge, Jallap, and most of vegetable Production are more violent, and operate sooner, when they are more tenacious, and adhesive, as in their Extracts, but gentler, when divided by hard brittle Substances, such as salt of Tartar, Sugar, &c.

Medicines which have in their Composition, Sulphur and Salt, are more or less rough, and speedy in their Operation, in proportion to their greater or lesser participation of the Saline Ingredient, and the asperity of its Angles; of this kind are most Minerals, and their Preparations, and it may be sufficient to instance in the management of Antimony, and
Mercury;

Mercury; The first of these is by chymical Analysis known to be a composition of a subtil Sulphur and Salt, and the more the Saline Part is let loose by Preparation, and opening the Sulphur, as it is commonly term'd, with the more vehemence will this operate, and the sooner, whereas in its lesser Preparations when the Salts are closely wrap'd up in their native Sulphur they will hardly work at all till they are got into the farthest stages of Circulation. Mercury *per se* is little known as a Medicine, and its first Preparation which makes it into Sublimate, so loads it with Saline Spicula, that it amounts even to a Poyson; but the more those Spicula are broke by Triture, Sublimation, &c. the milder doth it operate, and if to the comminution of its Points be added a Sulphur subtil enough to join it, it may be reduced to so mild a Medicine, as not to be felt, but in the last stage of Operation.

This short View may be sufficient to shew, that it is Excess of Asperity, and Motion in a Medicine, that will not suffer it to pass the Stomach, without irritating it into such Convulsions, as will throw it up again by Vomit; that a farther Comminution, and smoothing its Figure, will admit it into the Bowels, and make it operate by Stool; that a yet farther remission of its Properties will carry it into the Blood, and allow it there to promote evacuation by Urine; and that a yet farther Comminution will pass it into the minutest Canals, where by the same Properties, only in a lower Degree, it will increase Perspiration, or cause Sweat: So that the subtiler Medicines operate in the Capillaries, and smallest Fibres by the same Mechanism, that more gross do in the common stream of the Blood, when they go off by Urine, or

the grossest of all do in the greater Passages, when they promote Stool.

Hence the Skill of Preparing, and Administring of Medicine consists in proportioning its manifest, and known Properties to the Capacity, and Circumstances of the Part it is to operate in, and to intend, or remit its mechanical Affections, as it is sooner, or later to take place in the greater or lesser Vessels.

Of the first Class there are few to be reduced small enough to go beyond the greater Passages, and none of them are worth the Pains they require, to fit them farther than for Diureticks: besides, their natural fitness to attract, and join with the serous Part of the Blood, whenever they get into that Stage of Motion, runs them off by the Kidneys, before they can undergo Comminution enough to get farther; but if by frequent Repetitions of such Medicines, and uncommon Laxity of the Passages, any thing is passed into the Habit, their grossness fouls the delicate Strainers, which are left for their Expulsion; and they lodge upon the Glands, and Capillaries in such manner, as induceth Intermittents, which is observable in many Persons, after the use of Cream of Tartar, the common Cathartick Salts, and the purging Waters, especially at the latter end of the Summer, when the heat of the preceding Season hath debilitated the Solids, and left them under too great Relaxation.

Among the resinous Purges, there are many very powerful ones, but where their Operation is desired in the Viscera, Blood and remoter Parts, they must be extreamly divided, and this we find Spirituous Menstruums will do, by taking up the most subtil Parts only, and carrying them into very small Passages, where their Operation is chiefly by Fusion, because the softness of such Substances cannot enable

ble them in hardly any Degree to act as Stimuli; farther at least than ordinary Detergents. And thus we find Aloes, the chief of this Tribe, to go farther into the Habit, and be longer e'er it operates, when managed in a spirituous Menstruum, as in the *Tinctura Sacra*, the *Rad. Turpethi*; And *Colocynth* likewise, with all of the vegetable Kind, that will yield to a spirituous Liquor, are to be carried by that means into the farthest scenes of Animal Action, and there prove efficacious Medicines in Cases, that with other management, they would never be able to reach: And on this Foot it undoubtedly must have been, that we frequently meet with, in practical Writers, many of this sort mention'd, as Alterants; the *Colocynth* particularly by *Helmont*, for all Medicines which operated in the farthest Passages they commonly included under that general Appellation.

But the most efficacious Purges, and those which require the most Skill are from the mineral Kingdom; these abound in solidity beyond any other Materials, and therefore wheresoever they are brought into Action, excel in quantity of Impulse: Many of these therefore want not only the utmost Communion to carry them into the farther Scenes of Operation, but also some restraint to their Asperities, and Motions, to fit them for many Intentions. Thus Sublimate is not only to be much sweeten'd, that is, smooth'd in its Points, to make it a safe Purge in the larger Vessels; but if it is intended to go farther than the Blood, and those Glands, which in that Circuit, they are most apt to be lodged upon, when it Salivates, it must be rendred not only very fine, but covered with such Substances, as weaken its Points, and make it strain into the last Subdivisions of the Constitution. To this purpose, the common
 Practice:

Practice wisely contrives in Distempers which lie farthest off, according to the Course of Circulation, to wrap up the Basis of this Medicine, in Sulphurs, and such like Substances, as follow it into its last Division, without giving it any Asperities to make it act as a Stimulus. Thus for all cutaneous Foulnesses, and habitual Taints, the Cinnabar, the Æthiops, and all of that Sortment are in readiness; and that ordinary Sulphurs will cover and deaden the efficacies of Mercurial Preparations, so that they shall not operate, but in such Parts only and in certain Circumstances, is demonstrable in ordinary Salivations, which are to be lower'd at pleasure by Sulphureous Medicines.

Medicines from such Minerals where a Salt, and Sulphur are united by Nature, as they are in some Mercurials by Art, as in Antimony, the native Cinnabar, Steel, &c. are manageable only upon the same Principles, and the more they are designed to be carried into the Habit. the more are they to be restrained by their natural, or adventitious Sulphurs: Steel when opened by, and joined with, the points of acid Liquors, operates sooner, and will sometimes prove even Emetick, but when it is covered with an additional Sulphur, it will go farther, and answer intentions much more remote, as is manifest in the common Preparations of Steel, with Tartar, or Vinegar, and with Sulphur.

This way of thinking on these Occasions, seems to me also the more just, from considering the Texture of those Substances, which by a natural Preparation are fitted for Operation in the minutest part of an Animal Body, such as those of the Aromatick Kind, all which more or less, according to their greater, or lesser Degree of Subtilty, and Smoothness promote a Diaphoresis: They consist of exquisitely fine Salts,

covered

covered with a most Subtil Sulphur, as is demonstrable by Chymical Analysis; and the common Sal Volatile Oleosum is an admirable Contrivance upon the same Foundation, where a very Volatile Animal Salt is covered with a most exalted vegetable Oil, whereby it is suited to pass into the minutest Fibres, and make as it were, a Part of the Animal Spirits themselves.

And here it may not be amiss to observe, that all Animal Salts are very Volatile. or easily render'd so, but when bare, and naked, just as the Fire draws them out, with a mixture also of its own Particles in their Composition, they are too pungent to be felt without painful Sensations, but when soften'd with a fine Portion of an opposite Texture, which is smooth and yielding, they become most efficacious, and safe Sudorificks.

On these Considerations it likewise ceaseth to be a wonder why the subtil Salts of Cantharides are more sensibly injurious to the Bladder, than any other Parts, and why Camphire prevents those injuries; for the exquisite smallness of those Spicula makes them imperceptible, but in the most minute Canals, into which the Fibres composing the Membranes of the Bladder are known to be divided; and Camphire blunts their irritations, because its exquisite Subtilty enables it to follow them into those Meanders, and sheath their Asperities.

To this purpose it is very remarkable, what many (as I have been informed) now commonly practice in guarding even Mercurials against their stimulating Properties, and sending them into the finest Passages to operate by Fusion, and the bare Force of Impulse: for not only Calomel and the Mercurius Dulcis may be restrained from manifest Operation in the wider Passages.

Passages, and the Glands about the Mouth, but even the mineral Turbith, which of it self in a small Dose, will operate powerfully by Vomit and Stool, when mixed with Camphire, will not be so much felt in those respects, but go into the farthest Circuit of Motion, and promote the cutaneous Discharge in a more efficacious manner, than any Medicine of less specifick Gravity. But in this management the Camphire is to be mixed but very little before taking, otherwise it hath not this effect, which appears to be from its great Volatility, which makes it in a great measure exhale, while it stands mixed in a Medicine.

I am sensible of many good purposes in Practice, that this Theory is applicable to, but because the Limits I have set my self herein will not suffer me to enlarge, I shall content my self with this only Instance of Camphire, which is enough to suggest to those who turn their thoughts this way, in what Cases, that, and such Substances of like Subtilty, and Texture may be used with success: For the Seat and Causes of many Chronick Distempers lie most remote in the Course of Circulation, and the reason why they elude the ordinary means of Cure, it is to be feared, is owing to the want of sufficient Attention to that particular management of efficacious Remedies, which is necessary to carry their Operations so far; An ordinary Judgement knows how to intend, or remit the Efficacies of Medicines by acuating their Points, and Quantity of Impulse; or softening, and weakening them with Broths, and the gross-express'd Oils of Almonds, or Linseeds in the first and larger Passages; but an active Medicine, or a distemper'd irritating Salt in the minutest Capillaries and Fibres is not to be managed by such coarse Instruments.

If, upon the best examination, these thoughts be found to hold good, which I am much persuaded they in great measure will; I promise my self an opportunity of drawing some farther practical Remarks, and shall venture to communicate them to you, with the same freedom, I now do.

V. *An Account of two extraordinary cases in Surgery: Communicated by Dr. Steigerthall, F. R. S.*

Ebstorff, *May the 15th, 1720.*

Present *Dr. Nottellman, Mr. Henzel an Officer of the Custom, and Mr. Niemeyer, Auditor.*

John Henry Oizmann, aged 31 Years and born at Barum, was fifteen Years of Age, when the following Misfortune befel him.

He felt a Spasmus or Cramp in his left Hip, and the inferiour Part of his Leg; as this pain seized him pretty often, he consulted Mr. Raek a Surgeon at Ulzen, who applied several Plaisters to the place where the pain was, but without any relief to the Patient. After all those fruitless efforts, the Surgeon, to see whether Oizmann had still a feeling in his Leg (which to outward appearance was become very brown) made about 37 Incisions over the whole Leg of which the Patient was not at all sensible, except at such times when the Instrument happened to grate upon the Bone, the Periosteum being as yet sound and not infected by the Disease of the Flesh. The Leg however did daily grow blacker, and the pain continued both in the Periosteum and in all the Bodies

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of the superior and inferior Part of the Leg. At last a black Circle was seen round about the Muscles of the Hip as an indication of an approaching Putrefaction. This Circle appeared so visibly, as if it had been cut off with a Knife from the other Part. It has ever since spread it self, and come to such a head, that without any other help and cure the Flesh began gradually to rot away from the Bones; and at last quite fall away from the superior Part of the Leg which has preserved it's soundness. After this nothing was seen but the bare Tendons or Sinews hanging down like so many Strings or Cords. There remained also one piece of the inferior Muscles of the Hip fastned to the superior Part. At last the Tendons being grown dry, consumed away, and after all, the Leg it self, I mean the *Os Femoris*, did wholly drop off in such a manner, that there remained about four Inches betwixt the Bones and the Flesh loosely hanging down from them. The Flesh is at last grown up to the Bone, and without the help of any man has fastned it self to them. And in this sound Part the Patient feels a great Pain, when ever the Weather proves tempestuous. It is remarkable that at the same time he perceives also a swelling in *Tarso pedis Dextri*, the Matter whereof discharged it self through the Toes, and is of so corrosive a Nature, that it had consumed all the Toes but the little one. The Surgeon has at last healed up this Wound; but after all there is as yet but little feeling or warmth in the Foot.

This Man after this great accident is married now about seven Years to a Woman, whose bodily Constitution is almost as remarkable. She is now in the 41st Year of her Age. In her younger Years she had the misfortune to be goared by a wild Boar under

der the short Ribs of the left side. Of this Wound she then received, she has still a Fistula to this day. what Food she eats dischargeth it self half concocted through this opening, and she is obliged to clean this Wound often for that purpose, however she has notwithstanding this, her daily Evacuation *per Anum*. It is remarkable, that the Victuals thus discharged, by the Wound aforesaid, are still distinctly known what kind they have been of.

All this is attested by *Oizmann* mentioned above.
In fidem Subscripsit,

F. Niemeyer.

VI. *An Account of an Experiment made on Thursday the last Day of June, 1720. before the R. Society, to shew by a new proof, that Bodies of the same Bulk do not contain equal quantities of Matter, and therefore that there is an interspers'd Vacuum.* By J. T. Desaguliers,

I Took 3 Pound of *Mercury*, which by measure fill'd three times a small glass Jar exactly full, and pour'd it into a thin Florence Flask: then having pour'd the same quantity of Water (that is, three of the same Jars full) into another such Flask, I set both the Flasks in a Pail, and pour'd boiling Water about them, keeping the Flask that had the Water down by force, that it might be as low in the hot Water as the *Mercury*. After the Fluids in the Flasks had receiv'd a sufficient degree of heat from the Water, which was round the Flasks, for the space of five Minutes, I took

the Flasks out of the hot Water, and putting that which held the Water into a Cylindrick Vessel that had three Pints of cold Water in it, I did at the same time plunge the Flask with *Mercury* into another Cylindrick Vessel containing also three Pints of cold Water, and observ'd which of the cold Waters was most heated in the following manner.

A little Thermometer being held in the first Vessel of cold Water so as to have its Ball cover'd with the Water, upon the putting in the Flask of warm Water, the Spirit rose 2 degrees; then putting the Thermometer into the Water where the Flask that had the *Mercury* was, the Spirit rose three degrees higher. The Thermometer being again put into the first Vessel fell 4 degrees, and afterwards again into the last it rose almost 3 degrees.

This shews that more heat is communicated by warm *Mercury*, than by an equal Bulk of Water equally warm'd; and therefore that there is more matter in the *Mercury*; but how much more matter there is in the *Mercury* is not determin'd by this Experiment alone.

N. B. The warm *Mercury* and the warm Water were not pour'd into the cold; but only communicated their heat thro' the Flasks.

An Account of a Book, intituled, *Conghietture del Dottor Pietre Anton. Michelotti, Filosofo, e Medico d' Arco, sopra la Natura, Cagione e Rimedi dell' infermità regnanti ne' Animalì Bovini di molte Città, &c. Nell' Autunno del' Anno cadente, 1711. In Venezia, 1712.*

THE Learned Author of this Discourse happening to spend some time in the Country about the Month of *October*, in the Year 1711, took that opportunity of making a particular Enquiry into the circumstances of the Mortality, that then reign'd among the Black Cattle in the *Venetian Territories*. The result of which he gives in this relation, having been an Eye-Witness to the greatest part of the Facts herein contain'd, and having receiv'd the rest upon the place from Persons of Integrity and Credit.

Almost all the sick Cattle refused every sort of Food and Drink, they hung their Heads, had shiverings in their Skin, and in their Limbs, they breath'd with difficulty, and their Expiration in particular was attended with a sort of rattling noise, they were so feeble, that they could scarcely go or stand upon their Legs. Some few of them eat a little, and drank very much; others had Fluxes of Excrements variously colour'd of a very offensive smell, and frequently tinged with Blood: Many of them had their Heads, and their Bellies swell'd in such a manner, that, upon clapping them with the Hand on their Paunches

ches, or along the Vertebræ of the Loins, they sounded like a dry Bladder when full blown. In some the Urine was very turbid, in others of a bright flame Colour. In comparing the Pulses of the sound Cattle with those of the diseas'd, he found the latter to be quicker and weaker. There was but little heat perceivable by the touch in any of them, their Tongues were soft and moist, but their Breath was exceedingly offensive. Besides these particulars he was inform'd by those, who attended the sick Cattle, and by other Persons worthy of Credit, that in some of these Beasts they had observed crude Tumors in several parts of the Body, as likewise watery Pustules and disorderly motions of the Head, with dry, black, and fissur'd Tongues; that in others of them they met with Tumours, that came to Maturation, putrid Matter issuing from the Mouth and Nostrils, Worms in the Fæces, and in the Eyes, bloody sweats, and the falling off of the Hair.

In comparing the Flesh of the Cattle dead of this Distemper, with that of others kill'd for the Market, he found the Muscles in the former lying immediately under the Skin to be something livid. Having opened the three Cavities of the Body, he applied himself with the utmost diligence to examine the Brain with it's Membranes; the Trachæa, Oesophagus, Lungs, Heart with its Auricles, the Vena Cava, Aorta, and Diaphragm; the Liver, Spleen and other parts of the lower Venter. In all which there was no discernable difference, either as to figure, size, contents, situation, or connexion, with the neighbouring Parts, from what was observ'd in sound Cattle kill'd by the Butcher, except the particulars hereafter mention'd. The Blood found in the Ventricles of the Heart, in the Pulmonary Vessels, in the Aorta and
Cava,

Cava, though still warm, was considerably blackish, and near a Coagulation. In opening the upper and middle Cavity, the scent was offensive, but tolerable enough, whereas the Stink, that proceeded from the lower Belly, was not to be endur'd without prejudice. In some few Carcasses the Viscera differ'd from their natural State, with regard to their size, their consistence, their contents, colour and smell. In many of them the Paunch was found very much contracted and dry'd, with a hard Substance contain'd in it. In others the Lungs were swell'd and livid, the Liver tumified, and the Brain watery and putrid.

Upon observing the above-said state of the Blood in the Cattle Dead of this Distemper, he was desirous to see what condition it was in, while the sick Beasts were yet living. With which design having order'd several of them to be blooded, he found the Blood not to issue out of the Vessels in a continued Stream, as usual, but with a broken and interrupted Flux, one Part of the Blood not immediately succeeding another. Having caus'd the Blood to be receiv'd in proper Vessels, and suffer'd it to stand for some time, he found it intirely coagulated, without any Separation of the Serum, and attached to the sides of the Vessels, with a reticular Pellicle upon the Surface expos'd to the Air. All the Cattle which were blooded, being Eighteen in Number, died in a few days after the bleeding, one only excepted, in which the Vein was open'd upon its being first taken ill.

Having enumerated all the Symptoms of the Distemper, the Author concludes from the whole, that the Sickness among the Cattle was a Malignant Pestilential Fever, killing almost all those that were infected with it.

The immediate cause of this he takes to be a preternatural thickness of the Blood occasion'd by a beginning Coagulation of those parts of it, which constitute the Crassamentum, whereby the Globules of the Blood, and the particles of the Serum were imprison'd in a sort of Reticulum form'd by the Union of the Fibres of the Blood.

The occasional Cause of this Sickness he deduces from the cold and wetness of the Season, which reign'd all the preceeding Year from *October* 1710, to *November* 1711. Which Observation is worthy of remark, since the Season preceeding the Mortality among the Cattle here in *England* was remarkably dry, and yet the Symptoms of the Distemper agreed with those observ'd in *Italy*, as may appear from the Account given by the Learned Mr. *Bates*, Surgeon to his Majesty's Houshold in *Philosophical Transactions*, No. 358

For the particular manner in which this learned Gentleman endeavours to account Mechanically for the thickness of the Blood in these Animals, from the condition of the Season, and from that thickness of the Blood to deduce all the particular Symptoms of the Distemper, as likewise for his Conjectures concerning those Medicines, which might have been serviceable to the sick Cattle; he not having made tryal of any, we must refer the Curious to the Treatise it self.

F I N I S.

E R R A T A.

No. 363. Fig. 8. A B, which by mistake of the Graver, is shaded and made to represent a Hollow Cone, ought to be only two lines meeting in an Angle, to represent a perpendicular Section thro' two inclined Planes. No. 364. p. 24. l. 4. for *Biquadrate* read *square*.

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PHILOSOPHICAL TRANSACTIONS.

For the Months of *September, October, November* and
December, 1720.

The CONTENTS.

- I. **H**istoria portionis Intestini Coli propendentis
ex Vulnere per 14 annos. Communicante
Abrahamo Vater, Phil. & Med. Doct. Prof.
Public. Ord. Anat. & Botan. Substitut. Wittem-
berga, Natur. Curios. Coll.
- II. Observations upon the Bones and the Perio-
steum, in a Letter to the Royal Society, from
Mr. Leeuwenhoek, F. R. S.
- III. An Account of a præternatural Tumour on the
Loins of an Infant, attended with a Cloven
Spine. By Dr. Rutton, Fellow of the College of
Physicians, and of the Royal Society.
- IV. An Account of Two Observations in Gar-
dening, upon the change of Colour in Grapes and
Jessamine. In a Letter from Mr. Henry Cane.
- V. An Account of some new Electrical Experiments.
By Mr. Stephen Gray.

- VI. *A Letter to Dr. Halley, Astronomer Royal, R. S. Soc. in answer to some Objections made to the History of the Antiquity of the Venereal Disease. By Mr. Beckett, Surgeon, F. R. S.*
- VII. *An Experiment to compare the Paris Weights, as they are now us'd at Paris, with the English Weights. By the Reverend J. T. Desaguliers, LL. D. F. R. S.*
- VIII. *Some Remarks upon the Method of observing the differences of Right Ascension and Declination by Cross Hairs in a Telescope. By Dr. Edm. Halley, Astr. Reg. R. S. S.*
- IX. *A Proposal for measuring the height of Places, by help of the Barometer of Mr. Patrick, in which the Scale is greatly enlarged. By the same.*
- X. *Observations upon the Variation of the Needle made in the Baltick, Anno 1720. By Mr. William Sanderson. Communicated by Capt. Soame.*
- XI. *An Account of a large Quantity of Alcalious Salt produced by burning rotten Wood. By Mr. Robie of Harward College in Cambridge, New-England. Communicated by the Rev. Mr. Derham, Prebendary of Windsor, F. R. S.*

I. *Historia portionis Intestini Coli propendentis ex Vulnere per 14 annos. Communicante Abrahamo Vater, Phil. & Med. Doct. Prof. Public. Ord. Anat. & Botan. Substituto Wittembergæ, Natur. Curios. Coll.*

VIR quidam *Georgius Deppe Halberstadiensis* annorum xxxiv. castra secutus, anno MDCCVI. in prælio prope *Ramellies* in Belgio commisso, ab inflicto in sinistro hypochondrio vulnere, per hos xiv annos magnam portionem intestini coli, ictu hoc secti atque inversi, spithamæ magnitudine e corpore prominens gerit.

Intestinum hoc, per vulnus in illud simul penetrans, egressum, interiorem superficiem ad exteriora vertit, & ita in medio cohærens duas portiones efformat, quarum altera versus superiora extensa orificium monstrat, quod ad intestina tenuia spectat, & fæces alvinas educit; altera vero portio ad inferiora propendens orificium ad rectum apertum habet, ut Clysterem injectum per anum reddat. In interiori superficie, nunc ad extra vergente, glandulæ apparent copiosissimæ, albi ac cinerei coloris, verrucarum instar eminentes, & jucundissimum spectaculum exhibentes, quæ inclementius tactæ sanguinem fundunt. Intestinum ita prominens nunquam totum regreditur intra abdomen, ventriculo tamen vacuo quodammodo regreditur, sed ventriculo pleno cibo assumpto longius protruditur, imprimis spiritu retento. Utitur vir ille aqua gelidissima, imo glacie ac nive hyeme permista, ad abstergendas sordes, sine ullo incommodi

commodi sensu; fert etiam intestinum aerem frigidissimum; ab eodem tamen intra se retrahitur & indurescit, ac quodammodo pallefcit. Cibos omnis generis perferre potest vir iste; fructus tamen recentes & olera cibus reliquis non miscentur, sed indigesta exeunt, quemadmodum & juscula sine solidis cibus assumpta. Hæc sunt quæ ex relatione ipsius consignavi.

Wittembergæ,
Oct. I. 1720.

Abr. Vater.

Explicatio Figura.

A. B. C. D. Intestinum colon inversum, ex vulnere prominens, & versus superiora & inferiora porrectum.

a. Orificium superius ad intestina tenuia patens, per quod exeunt fæces alvinæ.

b. Orificium inferius, quod ducit ad rectum, & Clysterem per idem infusum per anum reddidit.

c. d. Vestigia vulneris à Chirurgo ampliati; per cujus medium adhuc apertum intestinum prominet.

e. Umbilicus.

f. f. f. Glandulæ copiosissimæ.

g. Regio Inguinalis.

b. b. Dorsum.

i. Coxendix.

K. Regio Lumbaris.

H. *Observations upon the Bones and the Periosteum, in a Letter to the Royal Society, from Mr. Leeuwenhoek, F. R. S.*

Delft, Nov. 20. 1720.

SOME Years ago, I communicated to the *Royal Society*, my Observations upon the Bones; what those Observations were, I do not now remember, nor have I endeavour'd to recollect them, lest I might be influenced by them in my later Enquiries, which I here present you with.

In my last Letter I acquainted you, that I had several times endeavour'd to discover, after what manner the *Periosteum* was united to those Vessels, that compose the Bone, but that I had not been able to observe it to my satisfaction. Since which time I have frequently employ'd myself in making Observations upon the Bones, and have found that the superficial part of them consisted of an inconceivable number of small Vessels, and some few of a larger size, which last, when they came to the surface of the Bone, appear'd to me to be cloathed with either a Membrane, or a bony Substance, that was perfectly transparent.

I once happen'd to discover in a small portion of a Shinbone, four or five Vessels of such a size, that a single filament of Silk might have been drawn thro' their Aperture. One of these appear'd to me to consist of two openings, each of which seem'd to be provided

vided with a Valve, which was disposed in such a manner, as to let out what was contain'd in the Vessel, but to suffer nothing to go in. In all my Observations upon the surface of the Bones, I never, but this one time, discover'd so many of these Vessels in so small a compass.

As for that Matter, which issues out from the Bone, and is carried into the *Periosteum*, I have discover'd the source of it to be the spongy or cellular Substance on the inside of the Bone, which is the repository for the Marrow.

This spongy Substance consists of long Particles closely united and link'd together, which Particles are compos'd of an infinite number of small Vessels, some running lengthwise, and others taking their course towards the sides of the bony Particles.

These bony Particles, notwithstanding their great number of Apertures, are yet exceeding hard, and lie some of them parallel, and others perpendicular to the length of the Bone.

Those Particles, that lie perpendicular to the length of the Bone, have Vessels proceeding from their ends; and from their sides, where they do not lie close together, proceed other Vessels, that compose the *Cortex*, or superficial part of the Bone. And those bony Particles that lie parallel to the length of the Bone, send out Vessels from their sides, that issue out thro' the side of the Bone. It is impossible for those, who have not seen this with their own Eyes, to conceive the prodigious number of small Vessels, of which the cortical part of the Bone consists; which in some places lies no thicker upon the spongy part of the Bone, than a thick Hair of a Man's Head, tho' in other places it has three or four times that thickness.

To the *Cortex* of the Bone, the *Periosteum* is united, not only on the outside, but even by entering in many places into the very substance of the Bone, and is join'd to it by the Vessels, which issue out from the Bone, in such a manner, that sometimes one cannot determine which is the Bone, and which belongs to the Membrane investing it, they both appearing in the Microscope to consist alike of exceeding small Vessels.

To make this be the better understood, I have given in Fig. 1. a representation of a small part of the Bone, with the *Periosteum* adhering to it, in which *ABCDEF* represents the bony part, whether taken from an Ox, or Sheep, I do not now remember. The *Periosteum* is mark'd with the Letters *BGHIE*, the thickness of which is design'd by *BG*, or *IE*, tho' in other places of the Bone, and even at no greater distance than two or three Hairs breadth, it is twice or thrice as thick. We see here, that all the small Vessels in the *Periosteum* are represented by so many Dots or Points; but in other places, where I had several times seen the Membrane of twice this thickness, the upper half of it has appear'd to be of a different make from the under part, for as much as in the upper part I could discover not only those Vessels, that had been cut transversly, and which consequently were represented by so many Points, but likewise a great number of other Vessels running lengthwise along the Membrane, as is represented in Fig. 2. by *LOPQNM*.

I am fully persuaded, that the part represented by *BGHIE*, Fig. 1. is not entirely membranous, but that some part of it is really bony. If we cut thro' the *Periosteum* so deep as to divide the part of the Bone mark'd with the Letters *ABCDEF*, in the same Figure, we find the same appearance of Pores in

the bony Substance, which are no other than the transverse Sections of small Vessels; and besides these, there are other Vessels running longways in the Bone. And we find just the same in those transparent parts, that lie between the bony Particles, which are represented thicker between *B C D E*, than they appear'd to me.

It is my Opinion, that the use of these bony Particles is, to convey an Oleaginous Liquor into the *Periosteum*, and that from the *Periosteum* it is carried by the intervention of the other Membranes into all parts of the Body, when in a healthful Condition.

In another place, I saw a great number of Vessels arising from a greater depth within the Bone, which drew closer together, so as to compose small *Fasciculi*, before they entered the *Periosteum*, in which they separated one from another, and dispers'd themselves again. It is difficult to determine, whether these Vessels bring any Liquor into the Bone, or carry it out; but I rather think they serve to carry it out of the Bone.

Having placed another piece of Bone before the Microscope, with the *Periosteum* adhering to it, I could discover a great number of Vessels, that I had cut thro' lengthways, as they ran along the *Periosteum*, and others that were cut thro' transversly. and appear'd as so many Points, as is represented in Fig. 2. by *K L O P Q N A*, where the bony part is mark'd by the Letters *K L M N A*, in which, tho' no Pores, or Vessels, are here represented, yet is it full of openings. That part, which is design'd by *L O P Q N M*, we must not take to be entirely membranous, for I am of opinion, that that part of it, which lies next the Bone, and which is represented by *L M N*, is of a bony Substance.

I had;

I had another small piece of Bone lying before a Microscope, of which I caus'd a part to be represented by *R S W X T V*, Fig. 3. in which *R S T V* is the Bone, and *S W X T* the *Periosteum*, which in this place was no thicker than a thick Hair of a Man's Beard, but in another part of the same Bone at a small distance, it was full four times that thickness

I placed another piece of Bone before a Microscope in such a manner, as that the Bone did not appear, but only the *Periosteum* and the Muscular Fibres, which were cut thro' transversly, and appear'd to be surrounded by the *Fibrillæ* of the *Periosteum*, as is represented by *T Z C D A B*, Fig. 4. where *T Z A B* is the *Periosteum*, and *Z C D A* are the fleshy Fibres cut thro' transversly. This piece of Bone was taken from one of the Ribs of a fat Ox, and I was surpris'd to find, that in this place, as I cut longways thro' the Rib, I could not discover any Particles of the Marrow, whereas in other parts the Rib abounded with them.

Notwithstanding the great number of Observations that I have made upon the Bones, and the Membrane that surrounds them, which is commonly call'd the *Periosteum*, I have never been able to satisfy myself entirely about them. I still imagin'd, that the part of the *Periosteum*, which immediately covers the Bone, and is strictly united to it, must have a degree of hardness approaching to that of the Bone, and that at a small distance from the Bone, the *Periosteum* must have a softness and flexibility like that of the carneous and adipose Membranes.

I had lying by me four pieces of Ribs of a fat Ox, which I had kept by me full two Months, and which were now grown very dry. From one of these I tore off the *Periosteum*, which I found stuck much harder:

harder to the Bone than I could have imagin'd, and I observ'd, that a great many Particles of this Membrane were left on cleaving to the Bone. This I did with design to make some Observations on the superficial part of the Bone, which is not near so hard as those bony Particles, that lie a little deeper. From this Bone I cut off some very thin slices, both along the Bone, and likewise transversly, one of which I placed before a Microscope, and gave it to the Painter, that for many Years has drawn all my Observations.

This Piece is represented by *ABKC*, Fig. 5. having been cut off transversly, and as thin as possible, from the Rib, with part of the *Periosteum*, as from *K* to *C*, still adhering to the Bone, and another part of it torn off from the Bone, as design'd by *BKD*, except that in some places the Bone and the Membrane are still united by Vessels torn out of their places, that run from one to the other. In this Figure *DEFC* represents the *Periosteum*, and the part design'd by *EGHIF*, is something lying upon it, which I could not tell what to make of, tho' it appear'd to me to be Membranous.

I had likewise some very thin slices shaven off from the Rib both of an Ox, and of a Calf, from which I tore off the *Periosteum* entirely, or at least as much of it, as possibly I could; after which I caused the edge of the Bone it had stuck to, to be represented by the crooked Line, *LMN*, Fig. 6.

In Fig. 7. *OPQ* represents the edge of another small slice of Bone, from which the *Periosteum* has been torn off, by which appearance it should seem, that the Union of the *Periosteum* with the Bone is so firm and strong, that, in separating it, some of the superficial Particles of the Bone are torn off with it.

I have likewise discover'd some Vessels running along within the Marrow-bone of the Shank of an Ox, that seem'd to be Blood-vessels.

Since now it appears from our Observations, which have been made with great Diligence and Care upon Bones of all kinds, that the Bones do for the most part consist of exceeding small Vessels, which Vessels arise from the inner, hollow, or spongy part of the Bone, and passing thro' the superficial or cortical Substance, enter the *Periosteum*, and are from thence continued farther into the Body, yea even into the utmost parts of the Body; we may from hence reasonably conclude, that, in a healthful Body, as there is a constant supply of an Oily Substance carried into the Bones, so this is again constantly carried out from the Bones by means of these Vessels, into all parts of the Body, even to the extremities of the Fingers. To give an evident proof of this, let any Man lay the ends of his Fingers upon a clean and bright Pewter Plate, and he shall find the Pewter appear soil'd in the place where he has touch'd it; for in reality this Soil is nothing else, but some Oleaginous Particles discharg'd from the ends of his Fingers. There is indeed something of a watery Substance mix'd with the Oily Particles, but this evaporates in a little time, and leaves the Oily Particles lying upon the Plate.

III. *An Account of a præternatural Tumour on the Loins of an Infant, attended with a Cloven Spine. By Dr. Rutty, Fellow of the College of Physicians, and of the Royal Society.*

THE curious Mr. *Ruyfch*, in an Observation on the *Spina Bifida*, takes notice that other Writers have describ'd it to be cloven into two equal parts longitudinally, as Butchers split the Back-bone of an Ox or Sheep; whereas out of ten Subjects, which he had an opportunity to examine, not one prov'd to be in that manner, the Body of the *Vertebræ* being entire, and the *Acute Processes* only divided. Pretty near this Description, is the *Spine* to be treated of, and I rather trouble this Society with it, because it don't appear by the Transactions, that it has as yet come before us.

The Back-bone is of a female Infant six Days old; whose Mother, when seven Weeks gone with Child, upon a fright occasion'd by her Husband's falling from a Horse and very much bruising his Loins, gave the *Embryo* this injury; but notwithstanding she went out her Time, and the Child was full grown.

There appeared upon the region of the Loins, in the same place where the Father receiv'd his hurt, a Tumour about the bigness of a small Turnep, with a broad *Basis*, around which the Skin was discolour'd, as by an *Ecchymosis*, but it grew immediately from thence pellucid, like a Vesicle rais'd by *Cantharides*, and continued so throughout, except just at its *Apex*, where was a Substance like a *Fungus*. This Bladder was fill'd with

with a Liquor, which in Scent and Colour resembled the true Urine: infomuch, that upon strictly examining the Linnen stain'd by what issu'd from hence, with that from the *Pudenda*, we cou'd perceive no sensible difference, and concluded there was a Communication between the left Kidney and the Orifice, into which the Surgeon's Probe pass'd obliquely upwards, about an Inch. Of the same opinion was Dr. *Pellet*, then present. Whether the same Similitude may hold, betwixt the Urine and this Liquor, in all Cases of the like Nature, I have not had opportunities to observe: but as Mr. *Ruyfch* has taken no notice of it, in such a Number of Subjects, that have been before him, I am apt to think, that it does not; being easily persuaded, that if it did, it would not so often have escap'd his inquisitive Researches. I cou'd have wish'd however, that this Fluid had been sav'd; to have try'd by Experiments, in what other Qualities, besides the Scent and Colour, it might correspond with the Urine; which wou'd have given us a farther Insight into the Cause of this Affinity between them, in the particular instance now before us.

Upon opening the Body, where were present Dr. *Pellet*, and Mr. *Stephens* an ingenious Surgeon, the Kidneys, contrary to Expectation, were *perfect*, and did not any way communicate with the outward Orifice.

But upon clearing away the Fungous Substance, which took up all the *Sulcus* or Hollow of the *Spine*, we found where the Perforation tended; a long Probe easily passing up the Channel, which contains the *Medulla Spinalis*. Throughout this *Fungus* were dispers'd a great many endings of small Nerves, from whence distill'd this, as it were, Urinous Liquor, which occasion'd the Tumour: the rest of the *Medulla* was more compact, and fill'd the Cavity of the *Spine*; tho' in some Subjects it has been wasted to such a Degree, that by blowing

P

into

into the Orifice you may inflate the *Dura Mater*; as was done by Mr. *Dobyns* a Surgeon, in a Case of this Nature. The Coat of the Tumour consisted at its *Basis* of the *Cuticula*, *Cutis*, and *Membrana Adiposa*: the two first of which forthwith terminating, the *Cuticula* only was continued; immediately under which appear'd the *Muscles* and *Fungus* above mention'd. In this it differs from those of Mr. *Ruyfch*, which received their Coats from the *Membranes* that cover the *Medulla*: as likewise from those taken notice of by *Tulpius*, which borrow'd theirs from the *Peritonæum*, as he says; tho' by his own Description, it is more likely they proceeded from the same Membranes.

These Tumours constantly attend the *Spina Bifida*: insomuch, that when any of them present themselves on the Loins or Back of a new-born Infant, we have good grounds to pronounce *This* to be the Cause; but we may be positive in it, if the Child can't move its lower Limbs; the want of which motion is an infallible Indication, that the *Medulla Spinalis* reaches no farther than the Swelling, whereby the Nerves are not distributed to those Parts. They appear differently in different Subjects. In some the whole Tumour is *Opake*; which proceeds from small Filaments of Nerves propagated in great numbers throughout its Coat, and not from the thickness of the Skin, as *Ruyfch* will have it. In others it is *pellucid*; and then the *Medulla* terminates at once at the Aperture of the *Spine*, and does not shoot out into any Ramifications. This before us is a Composite of both Species, the greater part of which was *pellucid*; the less, *viz.* the *Apex*, *opake*.

The Back-bone itself, you may observe, is not cleft, but has its *Vertebra*, with their *other Processes* entire, and is only defective in its *Spines* or *Acute ones*. But that

that Portion of the *Vertebra*, which should make an acute Angle, from whence their *Spinal Processes* naturally arise, in order to form the *Specus* or Passage for the Marrow, instead of that, gapes and lies almost in a strait Line on each side; whereby the *Medulla* is defrauded of its usual Guard from external Injuries. This Defect begins at the third *Vertebra* of the Loins, and is continued to the end of the *Os Sacrum*: which last is very extraordinary, it scarcely ever happening to this Bone; the reason of which I take to be the more than ordinary Compactness of its *Vertebra*, whereby it is less liable to be injur'd by any Impression. For we observe, that in Adults they grow so close together, as to unite and form one large and solid Bone.

As the Case before us is a *Vitium Conformationis* owing to the Mother's Imagination, so the Same sometimes is occasion'd by *Matter* lodging upon the *Spine*, and eroding the *Vertebrae* by its Acrimony: but then you find a *carious Bone*, whereas in these *praternatural* Cases, there remain no such Footsteps. This was observ'd in the Body of the late Lord *Peterborough*, as related by Mr. *Comper*: Upon the opening of which, He found a large Tumour, from whence flow'd a brownish colour'd Matter; and under it, the upper and fore part of the ninth, and the lower part of the eighth *Vertebra* of the *Thorax* were consumed and gone, the *Medulla Spinalis* being cover'd with its *Membranes only*. This gave rise to those Symptoms in that *morbid* Case, so nearly resembling the above recited in this *praternatural* One.

It is manifest from what has been said, that these Cases are incurable, and must in a little time kill the Patient. But it is almost immediate Death to open the Tumour; which every Surgeon will naturally do, that has not seen or read concerning it: the Fluid seeming

to require a Discharge. It is for the sake of those Gentlemen that I have taken upon me to describe it, who possibly may not have the above-mention'd Authors in their Hands; the only Ones that have observ'd it with any tolerable Accuracy.

IV. *An Account of Two Observations in Gardening, upon the change of Colour in Grapes and Jessamine. In a Letter from Mr. Henry Cane.*

Hillingdon near Uxbridge,
Oct. 31. 1720.

BEING a little curious in the Collection of all my sorts of Fruits, amongst other sorts, I have three or four of white Muscadine Grapes, very distinguishable by their Tastes, one of which pleased me above the rest; a Cutting of which about six Years since I planted against a Wall, on an Eastern Aspect, where it has the Sun from its rise till half an hour after Twelve. The Soil is a stiff Clay, but to make it work the better, I meliorated that, by mixing some Rubbish of the Foundation of an old Brickwall, where it now grows. In *January* last was twelve Month I pruned it, and the Figure was thus,

Left Hand T Right Hand *Black*

At time of Year it shot at both Hands about twenty two Inches of a side, before it came to a Joint; that on the Right was a very luxurious exuberant Branch, as big as the body of the Tree, the other side not half so thick or big, and the Leaves on the right were as big again as the other

on the Left-hand, and I fancy the largest that were ever seen. The Right-hand bears a very large and good black Grape, and large Bunches; the Left-hand very good white Grapes, and I had last Year more Bunches of the White, than of the Black; and whereas in all Vines bearing black and blue Grapes the Leaves die red, these died white on the black side as well as t'other. Last *January* I pruned the Tree again, but tack'd up more of the Right-hand (being Black) than I did on the Left, for which reason I had this Year a great many more of the Black, than I had of the White, and they ripen'd for the Season of the Year very well; divers Gentlemen of the Country both saw them last Year and this Year, and tasted of them. I gather'd the last about eight Days since, and the Leaves die White this Year also, being the second Year that ever it bore. I think to prune it pretty close on both sides this Year, and to plant out divers Cuttings of both sorts of it.

I will mention one thing more to you, which I have experienced about 28 Years since; I do it because Mr. *Lawrence* in his first Tract of Gardening, makes mention of the Plant, but I take mine to be a much different Case from his; I mean the yellow and green strip'd *Jessamine*. In the Month of *April, An. 1692*, having a small Plant of our common white *Jessamine*, which stood in the Ground, and was no bigger than a Tobacco-Pipe, I cut it off at two Joints above the Ground, and grafted it with a Cutting of the Yellow-strip'd; it took and shot a small weak Shoot, and in a Month or five Weeks after, it was blighted, and I perceived it had killed the Graft, and some part of the Stock below, so I took my Knife and cut it to the quick, which was near the next Knot or Joint to the Ground, and let it stand, thinking to graft it again at Spring, as before, but forgot it till the Season was past. At length going that way
by

by it, I saw it had broke out at the next Joint with several Shoots of they ellow and green strip'd; and not only there, but it had also made a strong Shoot from the Root, of yellow and green strip'd; after a while I took it up with Mold to the Root, and put it in a Pot, and it flourish'd all the Summer: And going to see my Son at *Magdalen College, Oxon*, I took it with me, and made a Present of it to the President of the College, where it flourish'd two or three Years, and then for want of shifting the Pot in time, it was matted so to the bottom and sides of the Pot, that it killed it; I also at that time gave the Fellows of that College, and several others of my Acquaintance an account of the Circulation and Descent of the Sap in that Plant, and I have try'd several other sorts of variegated Plants, but do not find any of them to transmute, as that Jessamine will do.

I am, &c.

H. Cane.

V. *An Account of some new Electrical Experiments.*
By Mr. Stephen Gray.

HAVING often observed in the Electrical Experiments made with a Glass Tube, and a Down Feather tied to the end of a small Stick, that after its Fibres had been drawn towards the Tube, when that has been withdrawn, most of them would be drawn to the Stick, as if it had been an Electrick Body, or as if there had been
been

been some Electricity communicated to the Stick or Feather; this put me upon thinking, whether if a Feather were drawn through my Fingers, it might not produce the same Effect, by acquiring some degree of Electricity. This succeeded accordingly upon my first trial, the small downy Fibres of the Feather next the Quill being drawn by my Finger when held near it: and sometimes the upper part of the Feather, with its Stem, would be attracted also; but not always with the same Success. I then proceeded to try whether Hair might not have the same Property, by taking one from my Wig, and drawing it 3 or 4 times through my Fingers, or rather between my Thumb and Fore-Finger, and soon found it would come to my Finger at the distance of half an Inch; and soon after I found that the fine Hair of a Dog's Ear was strongly Electrical; for upon taking the Ear and drawing it through my Fingers, great numbers of them would be attracted to my Fingers at once. I was at this time in the Country, and made no further trial till my return to *London*, (which I did about the middle of *November* last) and having repeated what I had before mentioned, the next thing which I thought of, was threads of Silk of several colours, and of several finenesses, which I found to be all Electrical, but sometimes I could not succeed; the reason of which I afterwards found, as will appear in the sequel of this Discourse.

Having succeeded so well in these, I proceeded to larger quantities of the same Materials, as pieces of Ribband both of coarse and fine Silk of several colours, and found that by taking a piece of either of these of about half a yard long, and by holding the end in one Hand, and drawing it through my other Hand between my Thumb and Fingers, it would acquire an Electricity, so that if the Hand were held near the
lower

lower end of it, it would be attracted by it at the distance of 5 or 6 Inches; but at some times the Electricity would be much weaker than at others, the reason of which I conjectur'd to be, that the Ribband might have imbibed some aqueous Particles from the moist Air, which I found to be upon trial the occasion of it; for when I had well warmed the Ribband by the Fire, it never failed to be strongly, Electrical.

After this I made trial of several other Bodies, as Linnen of several sorts, *viz.* Holland, Mussing, &c. and Woollen, as of several sorts of Cloth and other Stuffs of the same Materials. From these I proceeded to Paper, both white and brown, finding them, after they had been well heated before rubbing, to emit copiously their Electrick Effluvia. The next Body that I found the same Property in, was thin shavings of Wood; I have only as yet tried the Fir Shavings, which are strongly Electrical. The three last Substances which I found to have the same Property, are Leather, Parchment, and those thin Guts wherein Leaf-Gold is beaten.

All these Bodies will not only by their Electricity be drawn to the Hand, or any other solid Body that is near them; but they will, as other Electrick Bodies do, draw all small Bodies to them, and that to the distance of sometimes 8 or 10 Inches. Heating them by the Fire before rubbing very much increases their Force.

There is another Property in some of these Bodies, which is common to Glass, that when they are rubbed in the dark, there is a Light follows the Fingers through which they are drawn; this holds both in Silk and Linnen, but is strongest in Pieces of white pressing Papers, which are much the same with Card Paper; this not only yields a Light as above, but when the Fingers are held near it, there proceeds a Light from them with a crackling

ling Noise like that produced by a Glass Tube, though not at so great a distance from the Fingers; to perform this, the Paper before rubbing must be heated as hot as the Fingers can well bear.

A Down Feather being tied to the end of a fine thread of Raw Silk, and the other end to a small Stick, which was fixed to a Foot, that it might stand upright on the Table; there was taken a piece of brown Paper, which by the above-mentioned method was made to be strongly Electrical, which being held near the Feather, it came to the Paper, and I carried it with the same till it came near the Perpendicular of the Stick; then lifting up my Hand till the Paper was got beyond the Feather, the Thread was extended and stood upright in the Air, as if it had been a piece of Wire, though the Feather was distant from the Paper near an Inch. If the Finger were held near the Feather in this Position, the greatest part of the Fibres next the Paper would be repelled, when at the same time if a Finger were held to the Fibres that were more remote from the Paper, they would be drawn by it.

I then repeated this Experiment without the Feather, *viz.* by a single thread of Silk only of about 5 or 6 Inches long, which was made to stand extended upright as above-mentioned, without touching the Paper; then placing my Finger near the end, it wou'd avoid, or was repelled by it, but when I had placed my Finger at about the same distance from a part of the Thread, that was about two Inches from the end, it was then attracted by it.

An Enumeration of the several Bodies mentioned herein, that are found to be Electrical.

1. Feathers, 2. Hair, 3. Silk, 4. Linnen, 5. Woollen, 6. Paper, 7. Leather, 8. Wood, 9. Parchment, 10. Ox-Guts, wherein Leaf-Gold is beaten.

VI. *A Letter to Dr. Halley, Astronomer Royal, R. S. Soc. in answer to some Objections made to the History of the Antiquity of the Venereal Disease. By Mr. Beckett, Surgeon, F. R. S.*

S I R,

I Was of opinion that what I had said in my two former Letters, had been so full and satisfactory that I should have had no occasion to have given myself or any body else, any farther trouble upon this head: But forasmuch as I find there have been two Objections made against what I have advanced by one or two learned Gentlemen, I shall take upon me to answer them, and endeavour to prove they do not in the least invalidate the Authorities I before produced. The first is, that the *Venereal Disease* so well known among us now, and the *Leprosy* of former Ages, could not be the same Disease, because the *Leprosy* is not to be conquered by Salivation, which the other generally very readily yields to. In answer to this, I am to observe, that the *Leprosy*, which we have among us at this time, affects only the surface of the Body, the Skin generally appears scaley, with a certain deep red colour, or small Sores upon removing the Scales, and sometimes a Scabbiness, with a redness of the Skin, which affects different parts of the Body. I have known both the Cheeks only affected, both the Arms for the breadth of the Palm of the Hand, sometimes the Breast, the Legs, and other Parts; but this may continue upon the Patient during his Life, as it frequently does, and never make any farther Progress; which

which shews it to be a cuticular Disease: In these Cases upon Salivating the Patients, the Scales generally fall off, the redness disappears, and the Cure shall seem to be compleated; but in a Month or two, the same inconveniencies generally attend them as before. But one ought not to conclude, that because our *Leprosy* will but rarely be cured by Salivation, and the *Pox* generally will, that many of those Persons the Ancients judged to be *Leprous*, were not really *Venereal*; for their *Leprosy*, as they call'd it, was a quite different Disease from ours. Had there been any Proof brought that Persons had been Salivated in their *Leprosy*, and failed of Cure, it would have determin'd the Case; but on the contrary, we are assured by the learned Dr. *Pitcairn*, in his Dissertation concerning the Ingress of the *Lues Venerea*, That the *Leprosy*, before the *Neapolitan* Disease was talk'd of, was cured by *Mercury*, and now since it changed its Name, it is no longer heard of. Thus we find that their *Leprosy* and our *Venereal Disease* would be cured by the same Method, but their *Leprosy* and ours, being absolutely different Diseases, we by no means ought to expect the Success, from the same process of Cure, should be the same. I dare be positive that no body ever observed our *Leprosy* to be attended with falling of the Hair, hoarsness of the Voice, the Patient speaking as though he spoke through the Nose. Consumption of the Flesh, Ulcers all over the Body, corruption of the fleshy Parts, and of the Bones themselves, filthy Ulcers of the Throat, corrosion and falling of the Nose, all which are reckon'd as Symptoms of their *Leprosy*; on the contrary, ours is a mild and almost inoffensive Disease, which a Person may be affected with during his Life, and never become worse; whereas the other by displaying it self under

the Symptoms before enumerated, brings the Patient to the most miserable end; besides this, their Disease was got by Coition as their Authors assure us, but in our *Leprosy*, a diseased Husband may cohabit with his Wife as long as he lives, and he shall never be able either by Coition, or the immediate contact of the diseased Parts with those that are sound, to communicate any Evil. Had what our Predecessors called the *Leprosy* been the same Disease we call by that Name now, they had not been so solicitous of making such large Provision for them, or shutting them up from Humane Society; for one of our *Leprous* Persons might have been among them, and no body have known he laboured under any Infirmity at all. From hence it is evident the Disease so common among them, was entirely different from our *Leprosy*, the Appearances of which bear no manner of Analogy with the former: 'Tis from the Symptoms of the Disease, and the manner of its being received, that we generally know one Disease from another; but the Symptoms of most of their *Leprous* Persons, and the manner whereby the Disease was gotten, will be found in no other Disease that attacks the humane Body, but in the *Venercal Disease* only; for here they so exactly agree, that we must in a manner do violence to our own Reason, if we deny them to be the same. I proceed now to answer the second Objection, which indeed was long ago falsely asserted by Dr. *Fuller* the Historian; which is, that the *Leprosy* was brought into *England* from the Holy War, by some of our Countrymen, and that the Disease was altogether unknown among us before. This, as I take it, does not so immediately concern me, since all I take upon me to prove is, that what *They* called the *Leprosy*, is not the same Disease we call by that Name now, but another.

another. However, I shall in a few Words make it appear that this Objection is likewise groundless, by observing that the first *Englishmen* that went over to the Holy War, made their first Voyage in the Year 1096, as our Historians generally agree, and that some of them returned in 1098, two Years after that Expedition; but most certain it is, we had the *Leprosy* among us before, for *Wharton, de Episcopis Londinensibus*, and other Historians assure us, that *Hugo de Orivalle*, one of the Bishops of *London*, died here of the *Leprosy* in the Year 1084, which proves our Countrymen did not bring that Disease first from the Holy War, because we had it among us before. The account *William of Malmesbury* gives of this Bishop's Disease, is as follows. *Is post paucos ordinationis annos in morbum incurabilem incidit. Siquidem regia Valetudo totum corpus ejus purulentis ulceribus occupans ad pudendum remedium transmisit. Nam credens afferentibus unicum fore subsidium si vasa humorum receptacula, verenda scilicet, exsecantur, non abnuvit. Itaque & opprobrium spadonis tulit Episcopus, & nullum invenit remedium, quoad vixit leprofus.* Now its highly probable, had this been a new Disease the Bishop died of, the mention of it as such, would not have escaped our Historian, but on the contrary it seems to have been anciently known among us, because the Remedy made use of for it was so, it having been recommended by *Ætius*, and other Physical Writers several hundred Years before this time; and I think its very plain that the cutting off the *Testicles*, and with them the Vessels formed for the receiving the Humours as expressed in the former Case, was by them looked upon to be of peculiar Service, because its probable that observing the Disease to begin in these and the neighbouring Parts, they thought the very *Minera Morbi*,
would

would be by this means destroyed, and the Disease either cured or the spreading of it prevented.

I am, S I R,

Tours, &c.

William Beckett.

VII. *An Experiment to compare the Paris Weights as they are now us'd at Paris, with the English Weights. By the Reverend J. T. Desaguliers, LL.D. F. R. S.*

Finding the Accounts which we have of the *French* Weights different in different Books, I lent to a curious Gentleman for some *Paris* Weights exact to the Standard Weights at the *Chatelet*; and found upon tryal, the *Paris* Ounce, which contains 576 of their Grains, to be equal to 476 of our Grains Troy; from which Experiment all the other Proportions may be deduc'd.

The *French* Pound cont. 16 Ounces.

Ounce — 8 Drams, or 576 *Paris* Grains.

Dram — 3 Deniers.

Denier — 24 Grains.

VIII. *Some*

VIII. *Some Remarks upon the Method of observing the differences of Right Ascension and Declination by Cross Hairs in a Telescope.* By Dr. Edm. Halley, *Astr. Reg. R. S. S.*

Those that are curious in observing the Heavenly Motions, and particularly myself, whose Business it is, are greatly obliged to the late Signior *Cassini*, for his Thought of applying Threads at half Right Angles in the common *Focus* of a Telescope, to determine thereby the differences of Right Ascension and Declination of any two Stars, whose situation is such, that by their diurnal Motion they follow each other through the Aperture of the Telescope, so fixt as that the first of them may pass over the Centre of the Glais, and move exactly along one of the Threads, whilst the interval of time between the Transit thereof, and that of the following Star, is exactly measured by a *Pendulum* Clock well adjusted to the mean motion of the Sun, or else to the Revolution of the fixt Stars, whereby the difference of Right Ascension is given; as is the difference of Declination, by the time the following Star takes to pass from one diagonal Thread to the other. This manner of observing being long since published, will not in this place need any further Explanation; but it may not be amiss to say something of the sufficiency thereof, and of the exactness of which an Instrument of so little charge and *Apparatus* is capable; especially being at this time obliged to make use of it and the *Micrometer* only. for

my Observations. I need not say with what exactness Dr. *Pound*, and his Nephew Mr. *Bradley* did, myself being present, in the last Opposition of the Sun and *Mars*, this way demonstrate the extream minuteness of the Sun's Parallax, and that it was not more than 12", nor less than 9", upon many repeated Trials, it having been soon after the time laid before the Society. But being mindful that in *October* next, *Mars* would be again in Opposition to the Sun, about the tenth degree of *Taurus*, but would not come very near any fixt Star that has a place in Mr. *Flamsteed's* Catalogue; I was solicitous to see if there were any Telescopic Stars to which he would very nearly approach; and on the 28th of *February* last, the Heavens being very serene and clear in the Evening, and *Venus* having nearly the Declination in which *Mars* will move in *October* next, I fixt my Telescope on her, at 7^h. 28' equal time, and noted the moment she pass'd over the Center of my Glass, or rather the common intersection of the four cross Hairs; and in half an Hours time I noted eight very conspicuous Stars, four of which being within the compass of one Degree, fell very nearly in the said way of *Mars*, and from the intervals of Time I then observed, with their difference of Declination from *Venus*, I determined their Right Ascensions and Declinations, as well as her Place from my Tables, (which by Observation I found at this time needed no correction) would allow me; they all falling between the ninth and tenth degree of *Taurus*, with very little Latitude. But what confirm'd me that all was right was, that on *Tuesday* last *March* 21. *Mercury* appearing very fair, and newly past his greatest Elongation, I found by *Senex's* Zodiack that he was nearly in the same parallel that *Venus* had before described; and though the brightness of the *Crepusculum* effaced the smaller Stars, yet in a
quarter

quarter of an Hour I had one past $10' \frac{1}{4}$ more Southerly than the Planet, which in less than $3'$ of Time was succeeded by another, which was but one Minute more Northerly than the former; when after an interval of about 14 Minutes of time, in which I was surpris'd to find the Sky so void of Stars, the four before mentioned Stars past successively over my Glass, with the same interval of time in which I had seen them follow one another, on the 28th of *February*; whereupon I was desirous to try, whether, if the place of *Mercury* in my Tables were assumed, the same Right Ascensions and Declinations of those Stars would be deduced from him, as from *Venus*; and to my great Satisfaction, I found on trial by an exact *Calculus*, that I had the same Right Ascensions now as before, in none of the four differing fully half a Minute, so that these Stars may securely be added to the Catalogue, and the appulse of *Mars* to them be observed in very long Telescopes, in *October* next, to a further ascertaining the immense distance between the Sun and Earth.

Hence it will also appear that our *Mercurial* Numbers are, at least at this time, and in this part of his Orb, not less exact than those of *Venus*. And whereas this Planet scarce ever appears with us out of the Sun's Beams, and always low, and therefore under great Refraction; this way of observing takes off all the uncertainty, that accrues therefrom; and when once the *Zodiack* shall be compleated with the Stars that are wanting to fill up the vacant places, it will be easy at any time, by this method, to observe *Mercury* or a *Comet* within the Sun's Beams, with the same certainty, as if it were remote, and out of the neighbourhood of the Horizon, where the different Vapours that lie near the Earth, render the appearances of the Stars

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some-

somewhat dubious upon the account of the irregular Refractions.

March 23. 1721.

IX. *A Proposal for measuring the height of Places, by help of the Barometer of Mr. Patrick, in which the Scale is greatly enlarged. By the same.*

SINCE *Torricelli* first found that the *Mercury* in an inverted Tube was in *Æquilibrio* with the whole Column of Air that was over it, and that the weight of the incumbent Column was various according to the different Dispositions of the Air, in respect of serene fair Weather, and of rainy, windy, or otherwise tempestuous Weather: there have been several attempts and contrivances to make the minute variations thereof more sensible. And first the Wheel Barometer was thought of, which certainly shews these variations with great exactness, but is only proper for a fixt Station, nor easy to be removed; which Circumstance is required for the principal use this Instrument is applicable to, and which I would recommend it for.

The next thought for this purpose was that of Mr. *Hubin*, described in *Phil. Trans.* N^o 184, who returning the Tube of the Barometer, as an inverted Syphon, made a large dilatation in the ascending Leg thereof, wherein the *Mercury* ascended, as its Altitude in the other part thereof abated, and *è contra*: over this he drew out a narrow Glass Cane, which he fill'd with a tinged Spirit, and which being about fifteen times lighter

lighter than *Mercury*, would ascend about 15 times as much as the *Mercury* in the Barometer fell. This, besides that the Spirit would dilate and contract itself with heat and cold, had the inconvenience of the former, not to be easily removed without great danger of disorder and breaking, by reason of the smallness of the Tube in which the Spirit was to rise and fall.

This was succeeded by Dr. *Hook's* Marine Barometer, made of two Thermometers, the one the common seal'd Weather-glass, having no Communication with the outward Air, wherein the temper as to Heat and Cold was shewn by the swelling or shrinking of the included Spirit; the other the old Thermometer made with an inverted Bolt-head, in whose globular part was included Air somewhat rarer than the ambient, so as to make the Liquor which was to rise and fall in the shank of the Bolt-head, always to stand above the surface of the *Stagnum*, into which its end was immersed. This shew'd the heat of the Air by its own Dilatation; but at the same time, the different pressure of the Atmosphere mixed with it, so that the graduation of these two Thermometers being adjusted to any given height of the *Mercury*, they would at all times when the *Mercury* was at that height, both shew the same degree of heat: But at other times when the weight of the Air was different, that difference would shew itself by the disagreement of the degree of Heat shewed by them. This will be better understood from N^o 269, of the *Transactions*, wherein I have described this Instrument at large. This, tho' of admirable use at Sea, to give timely notice of approaching bad Weather, labours under the Objection that it supposes the Concave of the Tubes of the Thermometers to be Cylinders, or of equal Diameters throughout; and also that on account of Heat and Cold the Air and Spirit have a

proportional Dilatation and Contraction ; the first of which I take to be very hard to be found in ordinary Glass Canes, and the other I fear still wants to be made out by authentick Experiments.

The last contrivance for this purpose, is that of Mr. *Patrick*, who stiles himself the *Toricellian Operator*, by filling a small Glass Cane about five Foot long, and somewhat, but as little as may be, tapering upwards towards the close end of the Cane ; then inverting it, without a stagnant cistern of *Mercury*, so much of the *Mercury* as exceeds the length of the Column the Atmosphere can then support, will drop off, and leave its length equal to the then present height of the common Barometer : now when the Barometer rises, this length in the Cane becomes greater by the *Mercury's* being prest up into the upper and narrower part of the Tube ; and when it falls, on the contrary, it settles down into the wider part thereof, and becomes shorter, being always the same in Quantity. By this means, as the Angle of the Concave Cone of Glass, of which this Tube consists, is smaller, the different situation of the *Mercury*, will, upon the alteration of the Airs pressure, be nicely shewn by very large and distinct divisions.

Now the use I would apply this contrivance of the Barometer to, is to measure by it the different Levels of Places too remote to be come at by the ordinary Instruments for levelling, with the certainty one would desire. For this purpose let there be provided two small Glass Canes, as near as can be similar, growing very little taper or smaller at the closed end, so that being inverted, the *Mercury* may be suspended in them at the height it ought to have at the time of the Experiment. Let that height be duly noted, and then ascending the Monument, or some such Edifice where the ascent may be exactly measured, let the Scales annexed

annexed be divided into Parts by the descent of the *Mercury*, at every ten Foot, in both the pendent Barometers, which I conceive may be so chosen as to make the Divisions very distinct and sensible. These thus prepared, when it is desired to take the Level of two distant Places, let one of them be placed in the lower Place, at a time when the *Mercury* has the same height as when they were first inverted and graduated; and let the other be carried to the higher Place, where it will be found to stand at that division which answers the elevation of that Place above the other, the which had before been found by measure in ascending the Monument. Thus may 90 Foot ascent, which makes but one tenth of an Inch of *Mercury*, be represented by two or three Inches, or a space capable of being divided into 90 Parts: Whereas, if the distance of the two Places be 20 Miles, a minute of a Degree is equal to above 30 Foot; and by the usual Sights, whether Telescope or otherwise, of your Water Levels, I fear it will be very hard to convey a true Level without a greater Error than one Minute in the whole. This Proposal I humbly submit to the Examination of this Honourable Society.

X. *Observations upon the Variation of the Needle made in the Baltick, Anno 1720. By Mr. William Sanderson. Communicated by Capt. Soame.*

Wednesday, June the First 1720, being at Anchor near *Revell*, in the Latitude of $58^{\circ} 58'$ North, the Magnetical Amplitude at Sun-Set was

West ——— $64^{\circ} 30'$ North.

And the True was *West* ——— $49 \quad 37$ North.

Variation North ——— $14 \quad 53$ West.

Saturday, July 23, at the Isle *Gottsand*, in the Latitude of $58^{\circ} 21'$ North, at Sun Set, the Magnetical Amplitude was *West* ——— $49^{\circ} 50'$ North, and the True Amplitude, *West* — $35 \quad 00$ North, which

gives the Variation North $14 \quad 50$ West.

The Difference of Longitudes of the two foresaid Places by *Dead Reck.* is $1^{\circ} 50'$.

October the 24th at *Bornholme*, in the Latitude of $56^{\circ} 00'$, at Sun Rising, the Magnetical Amplitude was

East $43^{\circ} 15'$ South, and the True was

East $28 \quad 31$ which gives the Variation

North $14 \quad 44$ West.

XI. *An Account of a large Quantity of Alcalious Salt produced by burning rotten Wood. By Mr. Robie of Harward College in Cambridge, New-England. Communicated by the Rev. Mr. Derham, Prebendary of Windsor, F. R. S.*

A White Oak Tree, about two Foot Diameter, is in *Cambridge*, of so wonderful a nature, as that although about a third of it was decay'd, and seem'd really to be rotten Wood, yet this decay'd part, in burning, would turn almost wholly into a good white *Alkali*, and it would run down into hard Lumps white and clean. Such a Lump was brought us at College, to know what it was. We tasted it, and found it to be Salt. and very strong. We dissolved it in clean Water, and upon Decantation and Evaporation, without any Filtration, we produced a very clean, white Salt, exceeding in strength, and whiteness, any to be bought at the Shops. We tried it many of the ways of proving the goodness of an *Alkali*. Now although *Alkali's* may be extracted from common Ashes, yet what was peculiar in this, is,

1. That while it was Burning, the Wood it self would melt, and run down into hard Lumps of Salt; and none of the Wood that was found, would do this, but only that which was decay'd; and what was most decay'd, would yield the greatest Quantity of Salt. And,

2. Whereas

2. Whereas all other *Alkali's* of Wood made thus by Incineration, are blackish at first, and a *Lixivium* made of them, although often filtered, will yet be tinged with a brown colour, occasioned from a kind of Coal, or Ashes so inclosed, or closely united to the *Alkali* in burning, as not easily to be separated by Filtration, though often repeated; yet this *Alkali* was very white, even before Solution, and when dissolved, the *Lixivium* was not in the least tinged, but clear like pure Water, only a very small quantity of Ashes subsided to the bottom of the Vessel, in which the Solution was made. The *Lixivium* thence decanted needed no Filtration, but when boiled up to Dryness, the Salt remained fine and white. And

3. That in the burning of this Wood, as the heat of the Fire grew more intense, the Wood did, as it were, melt and clodder together in great Lumps, and did visibly bubble, and boil, with an hissing noise, like the Frying of Fat in a Pan.

4. That whereas the Weight of the *Alkali-Salt* produced from other Wood, in the common way of Incineration, is very inconsiderable, in proportion to the weight of the Wood producing it; yet this Salt nearly equalled in weight the Wood from whence it was taken.

5. Whereas the Ashes of other Wood are never so replete with Salt, as that Salt can be seen, or in the least cause the Ashes to lump or clodder together; yet this, the whole of it, would gather into hard and solid Lumps of white Salt, as easily to be distinguished from Ashes (tho' white) as the purest Salt of *Tartar* made with *Nitre* would be.

6. That although from other *Rotten Wood* much less of an *Alkali* can be produced, than from *Sound Wood*, yet here it is quite contrary, the decay'd part of this

Tree

Tree yielding, in quantity, as aforesaid; and the other, or sound part, yielding no more than other Wood.

Having thus given you a true and full Account of this strange and unusual Production, we shall give you our Thoughts respecting the Solution of it; which we should not attempt, but that being on the spot, we have examined the Tree, and considered what (by the Marks found on it) hath, in all probability, happened to it; and therefore suppose ourselves, in some measure, capable of giving as true or truer Judgment concerning it, than wiser and more ingenious Men can be, who have not had those Advantages. All which we do with humility and modesty submit to your Censure. And we would first premise, that the Tree, in all probability, was struck with Lightning many Years since, it being torn from the top of its Trunk to the bottom, on that side, which is now decay'd, and which yielded the aforesaid Salt, there being a Channel from top to bottom, about five Inches wide, as we suppose, at first, which the length of time had closed. And under this Bark, the Wood next to it was black; which we suppose was caused by the Lightning.

From which things we conjectured, that the Wood having been thus exposed to the Air and Water, for so long a time, this was the occasion of its becoming defective in that part; and that the Lightning having penetrated the Wood, had so altered and disposed the Parts and Pores of it (the Figure and Texture of the Parts appearing much different from other rotten Wood) to attract, receive in, and retain the *Nitrous* Salt of the Air, which through so long a space of time, could not but be in great abundance. Even as Salt of *Tartar*, or other *Alkali's*, being exposed to the Air for some considerable space of time, will be wholly reduced to a

S.

Nitrous

Nitrous Salt, (as *Glauber* says) and its Quantity also increased very considerably. Not that the Lightning had so calcined the Wood, as to reduce it to a perfect Salt, but yet by penetrating of it, it had calcined it in such measure, as to give it a like propriety or disposition, of attracting the *Nitrous Salt* of the Air, as aforesaid *Alkali's* of Wood that have been fully calcined.

Now if it should be objected, that the *Nitre* in this Wood being *Volatile*, would flee away in the burning of the Wood; we answer, That although *Nitre* can't be fixed, and reduced to an *Alkali-Salt*, by calcining it *per se*; yet it may so by the addition of the powder of Charcoal (as Chymists teach us.) And here we suppose the Wood so altered by Lightning, in which this *Nitrous Salt* was lodged, as served instead of Coal in the Burning of it.

F I N I S.

L O N D O N:

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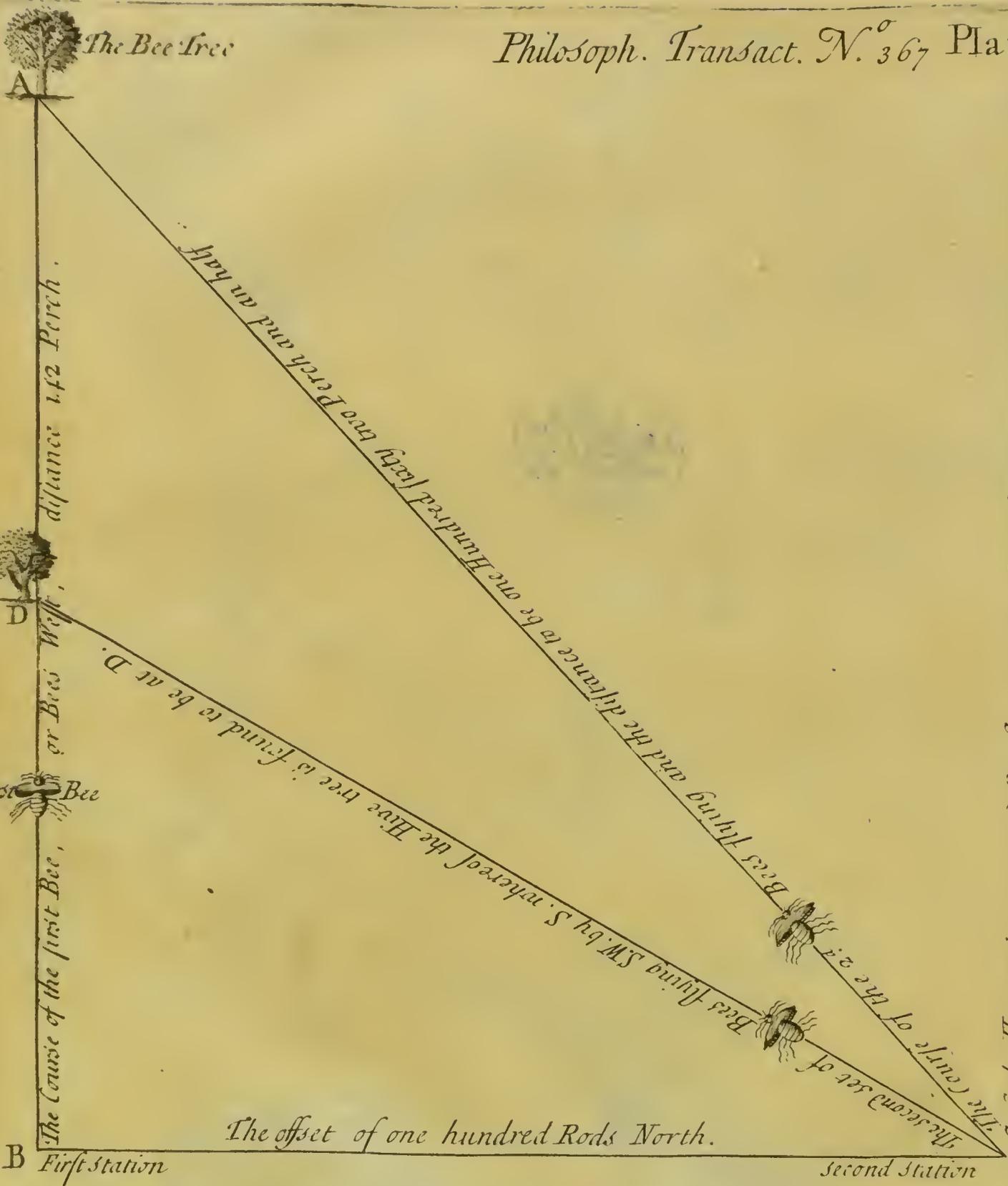


367.









At C the Hunter lets a second Bee fly (or more if necessary) and observes the Course of their flight and finds it to be South 54 degrees West.

At B the Hunter stands and having with his Sugar &c. procured one or more Bees, observes the Course of the first Bee & finds it to be due West.

N. B. If the Hunter is not satisfied of the Courses of (either first or second) by the flight of one Bee, he lets more fly untill he is very certain.



PHILOSOPHICAL TRANSACTIONS.

For the Months of *January, February, March, and April, 1721.*

The CONTENTS.

- I. **A** *N account of a Fœtus that continued 46 Years in the Mother's Body. Communicated by Dr. Steigertahl, Honorary Fellow of the College of Physicians, and Fellow of the Royal Society.*
 - II. *Observations upon the Membranes enclosing the Fasciculi of Fibres, into which a Muscle is divided. By Mr. Leeuwenhoek, F. R. S. Translated by Dr. Sprengell, F. R. S.*
 - III. *Observations upon the Vessels in several sorts of Wood, and upon the muscular Fibres of different Animals. By the same curious and inquisitive Person.*
 - IV. *Experiments relating to the Resistance of Fluids, made before the Royal Society on Thursday, March the 30th, 1721. By the Reverend J. T. Desaguliers, LL. D. F. R. S.*
 - V. *An account of the Poyson-Wood-Tree in New-England. By the Honourable Paul Dudley, Esq, F. R. S. Communicated by John Chamberlain, Esq;*
 - VI. *A farther account of the same Tree. By William Sherard, LL. D. R. S. S.*
 - VII. *An account of a Method lately found out in New-England, for discovering where the Bees Hive in the Woods, in order to get their Honey. By Mr. Dudley.*
 - VIII. *Propositiones aliquot de Projectilium motu Parabolico, scriptæ An. 1710. Per B. Taylor, LL. D. R. S. S.*
- T
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I. *An Account of a Fœtus, that continued 46 Years in the Mother's Body, Communicated by Dr. Steigertahl, Honorary Fellow of the College of Physicians, and Fellow of the Royal Society.*

A *Nna Mullern*, a Woman of the Village of *Leinzelle* near *Gemund* in *Suabia*, of a dry and lean Constitution, but otherwise healthful and robust, died at the Age of 94, after she had lived a Widow 40 Years.

Six and fourty Years before her death she declared herself to be with Child, and had all the usual Tokens of Pregnancy. At the end of her reckoning the Waters came away, and she was taken with the pains of Labour, which continued upon her about seven Weeks, and then went off upon the use of some Medicines given her by a Surgeon. Some time after this she recovered her perfect Health, except only that her Belly continued swell'd, and that now and then upon any Exercise she felt a little pain in the lower part of it. She was after this twice brought to bed, the first time of a Son, who is now a Huntsman at *Bischoffsheim*, and afterwards of a Daughter, who is married to a Soldier. But notwithstanding this, she was firmly persuaded, that she was not yet delivered of what she first went with, and desired *Dr. Wohnlix* the Physician of *Gemund*, and one *Knaussen* a Surgeon at *Heubach*, to open her Body after her death. Accordingly after her Death, which

happen'd

happen'd on the 11th of *March* 1720, after four days illness, her body was open'd by the Surgeon, the Physician before-mention'd being then dead. He found within her a hard mass of the form and size of a large Ninepin-Bowl, but had not the precaution to observe, whether it lay in the *Uterus*, or without it, and for want of better Instruments broke it open with a blow of a Hatchet. This Ball with the Contents of it, are expressed in the following Figures.

Plate I. Fig. I.

- A* A part of the Tegument, which had adher'd to a spongy fleshy Substance. This at first seem'd to be a Mass of Cartilage, but was afterwards found to be entirely bony.
- B. B. B. C. C. C.* Shews the membranous Part, which was bloody.
- D. D. D. D.* The opening made by the Hatchet.
- E. E. E. E. E.* Another part of the Tegument appearing entirely bony, with several prominences.
- F.* A Contusion occasion'd by the rude manner of opening it, where there appear'd some putrified Membranes.

Fig. II. *A. A. A.* The Tegument, or Substance enclosing the *Fœtus*.

- B.* The *Fœtus*.
- C.* A depression, or hollowness on the right Cheek.
- α.* The Nose turning up.
- β.* The Mouth flattened, but not so wide, as it is here represented.
- γ.* The Eye closed up.
- δ.* The Ear.
- D. D.* The Arms, of which the right was the larger, and the two Joints of that Thumb were plainly to be seen.
- E.* The

- E. The protuberance of the Knee.
 F. Part of the *Funiculus Umbilicalis* torn, but still adhering to the Navel.
 G. G. Part of the same *Funiculus*, fasten'd to the bony part of the Tegument.
 H. The Breast.
 I. The mark of an Incision into the left side, where the Flesh appear'd red, but was dry, and look'd like smoaked Beef.

Fig. III. Represents two Ribs from the left Side, of their natural Substance, colour and bigness.

- A. A. The part joining to the *Vertebrae*.
 B. B. to the *Sternum*.

This Piece is preserv'd in the Cabinet of Rarities of his most Serene Highness the Duke of *Wirtemberg*.

Dr. *Camerarius*, Professor at *Tubingen*, in a Letter upon this Subject, takes notice, that the Surgeon found this Mass in a cavity on the Woman's left side, and that it adhered to the Membranes of that cavity by the intervention of a spongy, fleshy Substance. From which particular, and the Woman's having had two Children during the time that this large Mass lay in her, that learned Professor conjectures, that it was not lodged in the Womb, but in the left *Fallopian Tube*, which by this means had been very much dilated and thickned in its Substance.

II. *Observations upon the Membranes enclosing the Fasciculi of Fibres, into which a Muscle is divided.* By Mr. Leeuwenhoek, F. R. S. Translated by Dr. Sprengell, F. R. S.

Delft, Jan. 9. 1720.

Sometime ago I mis'd a small piece of Flesh, which I had left standing before a Microscope, and endeavouring to procure such another, I cut off several thin slices from a piece of Beef; in doing which, whenever I cut the fleshy Fibres thro' transversly, I could plainly discover the Membrane, as it is commonly call'd, which runs between and envelops the fleshy Fibres, and especially the larger *Fasciculi* of them, as they run lengthwise along the Muscle. Between these *Fasciculi* the Membrane is of a considerable thickness, but spreads out every way into Ramifications exceedingly small. I had observ'd some Weeks before, that this Membrane was composed of an inconceivable number of very small Vessels, which were plainly to be discern'd not only where the Membrane appear'd of some considerable breadth, but even where it was not so broad as a single Muscular Fibre; but how far this held, I could not determine, forasimuch as these small Ramifications of the Membrane, did again spread themselves into other Ramifications so exceedingly fine, especially where they enclosed the single Muscular

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Fibres,

Fibres, that they were in a manner invisible even through my best Microscopes.

The very small Vessels, which compose this Membrane, (as it is call'd) are doubtless framed to convey some nutritious Juices, yet they are so small, that the Globules of Blood cannot pass through them.

That this might be the better understood, I caused a small piece of the Membrane to be designed, as at Fig. 1. *A. B. C. D.* which, with the adjoining fleshy Parts, is cut thro' transversly, and since it was impossible for the Designer to draw the extraordinary number of Vessels, which composed it, on account of their being so exceeding small, he has represented them only by Points.

Altho' in my former Letters I gave some Draughts of the Carnous Fibres cut thro' cross-way along with the Membranes, yet for the satisfaction of those who have not seen my other Letters, I have given them here another Draught of the same. See Fig. 1. between *E. F. G.* and *H. I.*

These carnous Fibres, when wet, lay so close to each other, that the space between *E. F. G.* and *H. I.* was quite fill'd up; but when dry'd, the Fibres were so shrunk, that one might see such Spaces between them as are here delineated.

Now as we see, upon the drying of the Membranes *A. F. G.* and *D. E. G.* with the Muscular Fibres between them, what a number of small Ramifications proceed from the Membranes, as is here represented between the muscular Fibres; we must not imagine, that these Ramifications proceed only from the points here represented, but that they are continued the whole length of the Fibres, and subdividing

viding themselves into still finer Ramifications, they enclose every single Fibre in the whole Muscle.

Amongst several pieces of Flesh, where the car-nous Fibres were cut transversly, I happen'd on one piece with its Branches so plain, that the Membranes and Fibres look'd like so many Boughs of Trees, with the Leaves on them, as may be seen at Fig. 2. *K. L. M. N.* where *M.* shews the so call'd Membrane torn off from another, as also how many Branches it runs into, and the many Fibres it covers.

All these carnous Fibres, with the so call'd Membranes, lay very compact together, when I cut them off from the piece of Flesh, as likewise when I laid them on the Glass, and moistened them; but as the moisture dry'd away, they shrunk again, in the manner here represented, and altho' the Designer could plainly distinguish the small Vessels which were cut thro', the largest of which appear'd at *M.* Fig. 2. yet he was obliged to mark them only with Points. Here you may observe, that all the carnous Fibres, having been closely tied together by the said Membranes, by which they were enveloped, which are nothing but a congeries of Vessels, could not be separated from each other upon drying, but by tearing asunder those Membranes.

The carnous Fibres along with the so call'd Membrane, at Fig. 2. *K. L. M. N.* do not take up so much room, but that a grain of Sand may cover it, and yet one might very distinctly observe, in some of those carnous Fibres, the parts of which they were composed.

This Observation I was resolv'd to pursue in the Flesh of a Whale, of which I had kept two pieces by me, for about 7 or 8 Years, of about a Span long,

and two Inches thick; from these I cut several Slices transversely, but found that the carnous Fibres so cut through did easily separate from one another, so that I could not find my account in this, but thought that the Membranes were rotten. Therefore I did cut off the outside with a Table-Knife, and then with a very sharp Knife I cut the inner part into very fine Slices, and there I found the Excrements of Mites, which were very small, but globular, and some of them as small as I had ever seen before, and so going on, I found these Excrements every where, especially where the Membranes were thickest; then looking into such places where the Membranes were thinnest, insomuch that I was satisfied, that a Mite, tho' just come from the Egg, could hardly find room there, there it was that I discover'd in the so nam'd Membranes, the aforesaid Vessels, and that in as great a number as I had seen them in the Oxe's Flesh, and as plain as one can see the holes in a Thimble with the naked Eye.

After the former Discoveries that I had made concerning the Circulation of the Blood, particularly that the Blood Vessels had no Endings, I began to consider how the Fat-Particles could be formed, since I did not think that they were separated from the Blood, and came out of the Blood-Vessels. But having now plainly discover'd, that the so call'd Membranes were nothing but very small Vessels, and believing that they were created for no other end but to transport Nutriment, as also that there was no Circulation in these Vessels, I imagin'd that the Matter which we call Fat, was brought into them, which, when there was too great a supply of Nutriment, so that it could not be forced farther on, must
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be driven out of these Vessels; for all the Particles of Fat, that I have as yet observ'd, are inclosed in small Films.

This Original of the Fat is to me much more credible, than that it should be forced out of the Blood Vessels; and yet how these fatty Particles, which consist of small globules, and those out of still smaller globules (as it appears to me) are made and formed, I cannot as yet determine: As also where these Vessels, which constitute what we call Membranes, have their beginning, and how this Fat is brought into them.

I had in my Drawer a piece of Oxe's-flesh, that I believe had lain there about four Years, wrapp'd up in a Paper, which Piece I found in some places to be cover'd with a Membrane; from this I cut off several small Slices along with the Membrane, and I found that near the Membrane, there lay about 16 or 18 Nervous Fibrils, which, in the drying of the Flesh, were so squeez'd together, that they were almost twice as long as they were broad. In some of which I saw very distinctly those Vessels, which are in the Nerves.

These Nervous *Fibrillæ* were enclosed by a sort of half round, separating them from the Muscular Fibres, which half round consisted of a row of small Tendinous *Fibrillæ*, each of which was about twice as thick as a Hair of a Man's Beard. Without these Tendinous *Fibrillæ* lay the Muscular Fibres, that had been cut through transversly, and in this part of the half round there were several Apertures, which seem'd in the Microscope to be big enough for Hemp-seed to pass through them, which might well be taken for Vessels, but that there lay so many of
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them together. But considering that the Nerves are commonly cover'd with fatty Particles, I concluded that these Apertures were no Vessels, but meer fatty Particles, which I found to be true when I had cut thro' them, and discover'd that the inward Fat was eaten out by the Mites, which had left only the Husks, or *Cortices*, of the Fat Globules behind: Which *Cortices* I never had as yet been able to discover, because the *Cortices* of the Fat Globules would, upon any heat, melt away as fast as the inward Fat.

III. *Observations upon the Vessels in several sorts of Wood, and upon the Muscular Fibres of different Animals. By the same Curious and Inquisitive Person.*

Delft, Jan. 24. 1721.

SEEING some reddish Boards carry'd into a House in my Neighbourhood, and enquiring what use they were design'd for, I was answer'd that they were bought of the *India* Company to make Cabinets of, and that they came from the Island *Amboina*.

I procured a piece of that Wood saw'd off at the end of a Board, as likewise some of the Chips, in order to observe the Vessels therein; and, cutting the Wood through all manner of ways, I found that in one place the Wood appear'd whitish, that at a small distance

distance it was red, and in another place blackish. Upon cutting it transversely, I saw the Orifices of the ascending Vessels, which ran along the length of the Wood, and which appear'd of such a size in the Microscope, that one would have judg'd a Pea might pass thro' them. Where the Wood look'd reddish, I found these great Vessels fill'd with a Substance of a fine red colour, so that I imagin'd, that these great Vessels carried a red Sap into the Horizontal Vessels, which appear'd so very numerous, and so thick together, that they caus'd the Wood to appear of the same colour with the red Substance, which was contain'd in those Vessels likewise.

I afterwards cut off some very thin slices transversely from this Wood, and putting them into a *China* Cup, I pour'd some hot Water upon them, and suffer'd them to lie in it for some time; then viewing them with a Microscope, I observ'd that the red Substance was extracted by the Water, and no red colour was now to be found in any of the Vessels.

What seem'd the strangest to me in this Wood, was that cutting thro' the Wood lengthways, as I frequently did, I observ'd it to be of a fine red colour for one Hair's breadth, and a Hair's breadth farther it appear'd white; and the ascending Vessels seem'd to be smaller, where the Wood was red, than where it was white: which narrowness of the red Vessels I judg'd to proceed from the Sap contain'd in them.

I made several other Remarks upon this Wood, which I shall pass by at this time, and shall proceed to some Observations I have made upon the ascending Vessels in Oak, and other Wood.

In viewing the ascending Vessels in Oak, I found some other Vessels, which enter'd into their sides, and appear'd to me like so many small round holes, especially where the Horizontal Vessels lay, which I judged to be united to the ascending Vessels, by means of those small Orifices, and thereby to discharge part of their Sap into them.

Taking a small Twig of an Oak, which in seven Years growth was grown to about the thickness of ones Finger, I cut it thro' according to the length both of the ascending and horizontal Vessels, which last I saw lying in great Numbers very close together, and proceeding directly from the Pith of the Twig.

I have likewise made some Observations upon Fir Wood, in which the ascending Vessels consist of so very fine and thin a Substance, that they exhibit a very delightful Spectacle in the Microscope. In these ascending Vessels I imagin'd that I saw some Globules, with a small opening in their middle, which seem'd to be of a closer and denser Substance than the rest of the Wood. But I afterwards found myself mistaken, and that these supposed Globules were nothing else but the Orifices, whereby the ascending and horizontal Vessels were united together, and through which the Sap was carry'd from the one to the other.

From these Observations I turn'd my Thoughts to the fleshy Fibres of Animals, and began to consider with myself, that, since the Author of Nature usually observes the same Frame and Structure in a great variety of his Creatures, perhaps the fine Membranes, with which every Muscular Fibre is invested, and which are provided with an innumerable

nable multitude of small Vessels, might carry Nourishment in the same manner, thro' every carnous Fibre in a healthful Body.

In this view I cut off some very small thin Slices from the flesh of an Ox, directly across the length of the Fibres, and having placed them upon Glasses, and moistned them with clean Rain-water, I observ'd them with a very good Microscope, and continued viewing them so long, that the fleshy Fibres began to grow dry. I then saw, that in some Places the exceeding small and fine Vessels, which compounded the Membranes, wherewith the fleshy Fibres were enclosed, were broken off from the fleshy Fibres, by the unequal shrinking of the thin slice of Flesh upon the Plate of the Microscope. I saw at the same time some other of these small Vessels, which were something stronger than the former, and were not broken off from the fleshy Fibres, but yet were stretched and drawn from them to the distance of the Diameter of a Blood Globule. I saw likewise some fleshy Fibres, which adhered so close to other Fibres, that the small Vessels of Communication were not broken off or stretched, so that nothing was to be seen there, but only the Membrane encompassing the Fibres.

I likewise placed before the same Microscope several other carnous Fibres, which I had separated according to their length from the Flesh of an Ox. In each of these I observ'd a great number of extremely small Apertures, by which I judged that the small Vessels of the Membranes had enter'd the Fibres; which Vessels having been moistned with Water, as soon as the little moisture, which had

been left in those Apertures, was evaporated, I could see them very plain and distinct.

Now since of late there have been two Persons of note, who have maintain'd, that the Blood circulated thro' the carnous Fibres; in order to examine into the Truth of this Hypothesis, I prick'd my Thumb with a fine Needle, and placed a little Blood upon the Glafs, where the carnous Fibres lay, with design to observe with my Microscope, what was the proportion between the Diameters of a Globule of Blood, and of the abovesaid Apertures, which I had seen in a Fibre.

While I was employ'd in these Observations, in came my Painter, who for these many Years has drawn all my Discoveries, and not being willing to trust too much to my own Eyes, he being much younger and better sighted than I, I placed before him the small Apertures in the Fibres, which he said were plain enough to be seen, and when he had view'd them to his satisfaction, I placed before him likewise the Globules of Blood, which lay together in great numbers, and yet so distinct and separate one from another, as one shall seldom see them. I then ask'd him, what he thought to be the proportion between the Diameter of a Globule of Blood, and the Diameter of one of those Apertures in a fleshy Fibre. After a little pause, he gave me for answer, that the Diameter of a Blood-Globule, was four times as large as the Diameter of one of those Apertures. If so, then according to the known Rule, a Globule of Blood must be divided into 64 parts, before it can enter through one of these Apertures into a fleshy Fibre.

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This discovery appear'd to me very wonderful; and I am apt to think, that it will be very difficult to penetrate any deeper into the hidden Structure of the muscular Fibres, and the manner by which they receive their Nourishment. I must confess that this Observation gave me an inward pleasure and satisfaction, which made me amends for some Reflections lately thrown upon me by a certain Foreign Gentleman, who, if this should come to his knowledge, would perhaps write to me again, as he did once before, that I related things which no Eye had ever seen.

Having committed these things to paper, my Thoughts ran again upon the muscular Fibres; and considering what I had said formerly concerning them, that they were composed of long, small Filaments, now I was not satisfy'd therewith, and therefore endeavour'd to discover, whether these small Filaments, which compose a carnous Fibre, might not really be so many small Vessels. With this design, I took part of the flesh of a Whale, which I had kept some Years by me, and cut it into very thin Slices directly across the Fibres, and having moistned these thin Slices with fair Water, I placed them upon several Glasses, and before several Microscopes, when I observ'd that what I had formerly taken for small Threads or Filaments, were in reality exceeding small Vessels. I then cut part of the Whale's Flesh lengthwise, in order to discover the Vessels, which convey the nutritious Juice out of the Membranes into the muscular Fibres, which Vessels then appear'd to me in great plenty and very distinct.

I afterwards took another piece of the flesh of a very fat Ox, which I cut thro' transversly, and looking upon it with some of my best Microscopes,

I could plainly see, that how small soever these Fibres were, they were still vascular, for I could see the light thro' the apertures of these Vessels, as I had done before in those of a Whale; but if I happen'd to cut the Fibres never so little obliquely, instead of cutting them directly across their length, the light was not to be seen thro' them.

I had in a Drawer the hind Quarter of a Mouse, which had lain there some Years; from the largest Muscle of which I cut off transversely some small Slices, as thin as possibly I could. Then placing these before my Microscope, I not only saw, that the carnos Fibres were of the same thickness with those of an Ox, but besides I could see the apertures of the Vessels composing the carnos Fibres, as plainly as in the flesh of a Whale. The Vessels in the Muscular Fibres of a Whale, are indeed six times more in number, than in those of an Ox, or a Mouse, but then the Fibre of a Whale is also six times as thick as the other.

Hereupon I consider'd after what manner the Vessels, of which the muscular Fibres mostly consist, receiv'd their nourishment from the Vessels of the Membranes; since the muscular Fibres, when they are at rest, have many alternate corrugations, by which I judged, that the Vessels in the Fibres must have their sides press'd together, and their cavities closed up. But if we call to mind, that in walking a Man may move both his Feet above 3600 times in an Hour, (for he may make two steps in the time of one pulsation of the Artery,) and that in that space of an Hour the muscular Fibres must be so many times extended and contracted; and will therefore require great Supplies; we shall likewise find that this is sufficiently

ciently provided for, since upon every extension of the Muscle the apertures of those small Vessels are free and open for the carriage of Nourishment into the Fibres. This wonderful structure of the Membranes, and the vast number of Vessels they consist of, as likewise the small Vessels of which the muscular Fibres are composed, has never yet to my knowledge enter'd into the Thoughts of any Man, and with many will hardly find Credit. Perhaps they will say, they have as good Glasses as I, and yet cannot see what I have related.

I am, &c.

P. S. I humbly desire that what I have here said, may be receiv'd with your usual Candour, and that a favourable allowance may be made for my great Age. A certain Gentleman, who was with me some Months ago, intreated me to go on in making Observations, adding that the Fruit which ripen'd in Autumn, was the most lasting. This is now the Autumn of my Life, I being arriv'd to the age of $88\frac{1}{4}$ Years, and having committed these Observations to Paper in the Months of *November* and *December* last.

IV. *Experiments relating to the Resistance of Fluids,*
made before the Royal Society on Thursday,
March the 30th, 1721. By the Reverend J. T.
Desaguliers, LL. D. F. R. S.

I Took a Ball of Gold of an Inch in Diameter, that had a little Stem of the same Metal, with a place on it to fasten a String to ; and having suspended it by a silken Thread too strong to lengthen by stretching, I made the Distance between the Center of the Ball, and the Point of Suspension equal to 12,5 Inches, then causing the Ball to vibrate in a Trough full of Water, (which had an upright Piece of Wood in the middle of one side with Pins or Keys from which the Ball hung, that the Center of Suspension might always be in the same place) I observ'd by looking from a Pin on one side of the Trough to a mark made opposite to it on the other side, whereabouts the String of the *Pendulum* (just above the Surface of the Water ; in which the Ball was quite immers'd) went after 14 Vibrations ; and by another Pin and opposite mark, also observ'd where it went to, after 28 Vibrations. Taking out the Water, I fill'd the Trough with Mercury, the length of the *Pendulum*, Point of Suspension and all other things remaining as before: then letting go the Ball in the Mercury from the same place whence it was let down when the Trough was full of Water ; (which was mark'd by a String stretched a cross to prevent mistakes) after
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one whole Vibration, it came very little short of the same mark as it had come to in Water after fourteen Vibrations, and when it vibrated twice in Mercury, it came to the same place it had done after between 26 and 28 Vibrations in Water; and this it did exactly several times.

Afterwards filling an upright Copper Pipe of four Inches Diameter with Mercury to the height of 3 Foot 10 Inches, and suspending the golden Ball in it by a short String about an Inch long, so as to have the Ball just immers'd under the middle of the surface of the Mercury; I caus'd it to be let down suddenly, and observing how long it was falling down to the bottom of the Tube, I found that the Experiment was disturb'd by the Ball's striking against the sides of the Tube, which retarded the fall of the Ball, and the more so the oftner the Ball struck. When the Ball was least retarded, it was only two Seconds and a half in falling, which must be taken as the true time of the fall of the Ball in an height of Quicksilver equal to 3 Foot 10 Inches; because when I try'd the Experiment again at home the first Day of *April* following, the Ball fell in the Mercury once or twice without striking the sides of the Tube at all, but not in less time than $2\frac{1}{2}$ Seconds.

I also repeated the other Experiments at home, making the Golden *Pendulum* 39,2 Inches long, so as to make it vibrate but once in a Second, and then I found that it wou'd vibrate 5 or 6 times in the Mercury before the Vibrations became so small as not to be observ'd; and then the first Vibration in the Mercury ended very near where the 14th in Water had done; the second in Mercury ended where the 27th in Water had done, and observing the third Vibration
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in Mercury, it ended exactly at the mark where the 40th in Water ended; and this was observ'd by several Persons as well as myself.

Then I weigh'd 14 penny-weight of the Mercury (in which I made the Experiments) first in the Air, then in Water, where it lost only one penny-weight and one Grain of its Weight; that is, it weigh'd in Air 336 Grains, and in Water 311, so that its specific gravity was to that of Water as 13,44 to 1.

As to the golden Ball which had Varnish and Cement upon it to keep the Mercury from sinking into it, I found it to weigh as follows,

	Ounces	dwt.	gr.	
It weigh'd in Mercury	1	00	18	or 498 gr.
in Water	5	01	00	or 2424 gr.
in Air	5	07	09	or 2577 gr.

I took the Wire and *Pendulum* of a long *Pendulum* Clock, and having fasten'd the golden Ball at the end of the Wire under the pendulous Weight that serv'd for the Clock, in order to make the Vibrations of the golden Ball in the Mercury continue longer, I did not find it to keep on the motion above one swing or two the longer for that help; neither did a round Ball of Lead placed upon the said Wire, just above the surface of the Mercury, help any more; and as I found some Inconveniencies in these two last ways of making the Experiment, I rather chuse to rely upon those made with the Golden Ball hanging by a silken Thread of 39,2 Inches long, measuring from the point of suspension to the Center of the Ball.

V. *An account of the Poyson Wood Tree in New-England.* By the Honourable Paul Dudley, Esq, F. R. S. Communicated by John Chamberlain, Esq;

THE Poyson-Wood-Tree grows only in Swamps, or low wet Grounds, and is something like a small Ash, but much more like a *Sumach*, and therefore is by some called the *Swamp Sumach*, for the Twiggs, Leaves, and Shape are exactly like the *Sumach*, and it likewise bears a dry Berry.

It never grows bigger than a Man's Leg, nor taller than Alder, but spreads much, and several together, especially about the Stump or Roots of one that is cut down; as it is of quick growth, so it does not last long; the inside of the Wood is yellow and very full of Juice, as glutinous as Honey or Turpentine; the Wood itself has a very strong unfavory Smell, but the Juice stinks as bad as Carrion. Having thus described the Tree, we shall now proceed to give an account of its Poisonous Quality, &c.

1. And first, it must be observed, that it poysons two ways, either by touching or handling of it; or by the Smell; for the Scent of it, when cut down in the Woods, or on the Fire, has poisoned Persons to a very great degree. One of my Neighbours was blind for above a Week together, with only handling

ling it. And a Gentleman in the Country, sitting by his Fire-side in the Winter, was swelled for several Days with the Smoak or Flame of some Poyson-Wood that was in the Fire.

2. A second thing to be remarked of the Poyson-Wood is, that it has this effect only on some particular Persons and Constitutions; for I have seen my own Brother not only handle, but chew it without any harm at all. And so by the same Fire one shall be poysoned and another not at all affected.

3. But then Thirdly, this sort of Poyson is never Mortal, and will go off in a few Days of it self, like the Sting of a Bee; but generally the Person applies Plantain Water, or Sallet-Oyl and Cream.

4. As to its Operation, within a few Hours after the Person is poysoned, he feels an itching Pain that provokes a Scratching, which is followed by an Inflammation and Swelling; sometimes a Man's Legs only have been poysoned, and have run with Water.

My Neighbour that was so sadly poysoned with handling it, told me one thing very remarkable of the Wood, and that is, that when he touched it, he plainly perceiv'd it to differ from the other Wood, that he was throwing up into his Cart; for it was as cold as a piece of Ice; and withal assured me, he could distinguish it blindfold, or in the dark, from any other Wood in the World, by its Coldness; but the poor Man is as much afraid of it, when he goes into the Woods, as of a Rattle-Snake. He further tells me, that he felt an itching in a few Hours after he had handled the Wood, but the swelling did not come on till in about three Days.

Paul Dudley.

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VI. *A farther Account of the same Tree.* By
William Sherard, LL. D. R. S. S.

THE Account I had of the Poyson-Tree from Mr. More (which probably he had from Mr. Dudley) is as follows.

The Poyson-Tree grows to the bigness of Elder; I never saw the Leaf; the Wood is as cold as Ice; when laid on the Fire, of 5 or 6 Persons sitting by it, some will fall a swooning, fainting, or yawning, continuing so for some Days, others but a few Hours, and others of the Company not at all. I handle, cut and burn it with impunity; and so it is with several others, I suppose, according to their several Constitutions. It was never known to kill any body, but only to do hurt to some Persons. I have sent you all the Seeds of it, I can get.

The Seeds he sent were but few, but I had a good quantity from Mr. Catesby in Carolina. He calls it a Water Shrub, of which he never saw Leaf or Flower. Tis a species of *Toxicodendron*, tho' not nam'd by Dr. Tournefort in his Institutions, p. 610. but I believe it to be *Arbor Americana alatis foliis, succo lacteo venenata*. Pluknet. Almag. 45. Tab. 145. Fig. 1. which is a Species of *Toxicodendron*, that grew formerly at Chelsea Garden. What makes me think it to be this, is Mr. Dudley's writing 'tis like a *Sumach*, and that it is by some call'd the *Swamp Sumach*; this in its manner of growing and alated Leaves, very

much resembles the *Sumach* or *Rhus*; the Fruit is a white roundish dry Berry, growing in Clusters, so like that of *Toxicodendron triphyllon folio sinuato, pubescente, Inst. R. Herb. 611. Hederæ trifoliæ Canadensi affinis planta: Arbor venenata quorundam H. R. Paris.* as scarce to be distinguish'd from it.

VII. *An Account of a Method lately found out in New-England, for Discovering where the Bees Hive in the Woods, in order to get their Honey. By the same Mr. Dudley.*

THE Hunter in a clear Sun-shiny day, takes a Plate or Trencher, with a little Sugar, Honey or Molasses spread on it, and when got into the Woods, sets it down on a Rock or Stump in the Woods: this the Bees soon scent and find out; for 'tis generally supposed a Bee will scent Honey or Wax above a Mile's distance. The Hunter secures in a Box or other Conveniency, one or more of the Bees as they fill themselves, and after a little time, lets one of them go, observing very carefully the Course the Bee steers; for after he rises in the Air, he flies directly, or upon a streight Course to the Tree where the Hive is.

In order to this, the Hunter carries with him his Pocket Compass, his Rule, and other Implements, with a Sheet of Paper, and sets down the Course; suppose it be West; by this he is sure the Tree must be somewhere in a West Line from where he is, but wants to know the exact Distance from his Station,

tion; in order to determine that, he makes an off-set either South or North (we'll suppose North) an hundred Perch or Rod, (if it be more, it will still be more exact, because the Angle will not be so acute) then he takes out another Bee and lets him go, observing his Course also very carefully, for he being loaded will, as the first, (after he is mounted a convenient height) fly directly to the Hive; this second Course, (as I must call it) the Hunter finds to be South, 54 Degrees West; then there remains nothing but to find out where the two Courses intersect, or, which is the same thing, the Distance from *B* to *A*, or from *C* to *A*, as in the Figure, Tab. 3d. for there the Honey-Tree is.

For which Reason, if the Course of the second Bee from *C* had been South-west, and by South, *viz.* to *D*, then the Hive-Tree must have been there, for there the Lines are found to intersect.

The Foundation of all this is the streight or direct motion of Bees, when bound home with their Honey, and this is found to be certain by the Observation and Experience of our Hunters every Year, and especially of late Years, since this Mathematical way of finding Honey in the Woods has been used with such Success.

An ingenious Man of my Acquaintance the last Year took two or three of his Neighbours that knew nothing of the matter, and after he had taken his Bees, set the Courses the first and second Bee steered, made the off-set, and taken the Distance from the two Stations to the Intersection, he gave orders to cut down such a Tree, pointing to it; the Labourers smiled, and were confident there was no Honey there, for they could not perceive the Tree to be hollow, or to have

have any hole for the Bees to enter by, and would have dissuaded the Gentleman from felling the Tree, but he insisted on it, and offered to lay them any Wager that the Hive was there, and so it proved to the great surprize of the Country-men.

I cannot dismiss this Subject, without acquainting you, that all the Bees we have in our Gardens, or in our Woods, and which now are in great numbers, are the produce of such as were brought in Hives from *England* near a hundred Years ago, and not the natural produce of this part of *America*; for the first Planters of *New England* never observed a Bee in the Woods, until many Years after the Country was settled; but that which proves it beyond question is, that the *Aborigines* (the *Indians*) have no word in their Language for a Bee, as they have for all Animals whatsoever proper to, or aboriginally of the Country, and therefore for many Years called a Bee by the name of *English Man's Fly*.

Our People formerly used to find out Honey in the Woods; by surprizing and following one Bee after another by the Eye, till at length they found out where the Bees lived.

I will mention another thing with respect to Bees, tho' I don't know but it may have been commonly observed; and that is, when they Swarm they never go to the Northward, but move Southward, or inclining that way.

I should have taken notice in the proper place, that when one Bee goes home from the Sugar-plate, he returns with a considerable number from the Hive.

VIII. Propositiones aliquot de Projectilium motu
Parabolico, Scriptæ An. 1710. Per B. Taylor,
LL. D. R. S. S.

PROPOSITIO I.

*Vi gravitatis, ejusq; directione datis, motus corporis
projecti, in medio non resistente, fit in Parabolâ.*

DEMONSTRATIO.

Projiciatur corpus de loco A (Fig. 1.) in directi-
one lineæ AB , sitque ejus trajectoria curva
 ACD . Ad trajectoriæ punctum quodlibet C , duc
rectam CB in directione vis Gravitatis, rectæ AB
occurentem in B ; atq; resolvetur motus projectilis
per AC in partes AB , BC , quarum AB oritur a
motu projectionis uniformi, atq; BC a vi gravitatis
accelerante. Est ergo recta descripta AB tempori
proportionalis ab initio motûs in A , atq; est BC in
duplicatâ ratione ejusdem temporis, sicut olim de-
monstravit Galilæus; adeoque in duplicatâ ratione
rectæ AB . Cum ergo sit BC in duplicatâ ratione
rectæ AB , constat curvam ACD esse Parabolam.
Q. E. D.

PROP.

P R O P. II.

Velocitas corporis projecti in quolibet puncto trajectoriae, ea est, quam corpus acquirere potest cadendo per altitudinem æqualem quartæ parti parametri Parabolæ ad punctum illud.

D E M O N S T R A T I O.

Sit Trajectoria ACD (Fig. 2.) Ad punctum quodlibet A ducantur tangens AB , & diameter AE . In tangente AB fiat AB æqualis dimidio parametri ad verticem A , & diametro AE parallela ducatur BC , trajectoriae occurrens in C , & ad punctum C duci intelligatur tangens CG , tangenti AB occurrens in F , atq; diametro AE in G . Tum ex naturâ parabolæ erunt AG , CB æquales, adeoq; & AF , FB ; & quoniam est AB æqualis dimidio parametri ad punctum A , erit BC quarta pars ejusdem parametri, & proinde æqualis ipsi BF . Ipsi BC proximam & parallelam duc bc , parabolæ occurrentem in c , & duc lineæ Bb parallelam $C\beta$, ipsi bc occurrentem in β . Tum quoniam spatium Cc , adeoque & spatium βc , finguntur perexigua, velocitates quibus describuntur erunt æquabiles quamproximè; adeoq; spatia Bb , seu $C\beta$, Cc , cum eodem tempore describantur, erunt ut velocitates quibus describuntur, & vicissim velocitates erunt ut spatia. Coincidant puncta C , c , atq; erunt hæ rationes accuratæ. Sed in isto casu propter similia triangula $C\beta c$, FBC , fit $C\beta$ ad βc , sicut FB ad BC ; ideoq; velocitates quibus describuntur Bb , βc sunt ut FB , BC , hoc est, sunt æquales. Velocitas autem, quâ describitur βc ,

ea

ea est, qua movetur projectile in puncto A , & velocitas altera qua describitur βc , ea est quam corpus acquirit cadendo per altitudinem BC quartæ partis parametri ad punctum A . Est ergo velocitas projectilis in quolibet puncto A æqualis velocitati, quam corpus acquirere potest cadendo per altitudinem quartæ partis parametri ad punctum illud. *Q. E. D.*

P R O P. III.

Datis velocitate & directione projectionis, invenire Trajectoriam corporis projecti.

1. Projiciatur corpus de loco A (Fig. 3.) in directione rectæ AB . Duc AC in directione vis gravitatis, (hoc est Horizonti perpendicularem,) ejus longitudinis, ut sit C punctum, unde corpus cadendo acquirere potest velocitatem in A , quâ fit projectio. Duc AF æqualem AC , angulum FAB constituentem cum lineâ directionis AB , æqualem angulo CAB . Duc CD perpendicularem ad AC (hoc est horizonti parallelam,) eiq; occurrentem FD , ipsi AC parallelam. Biseca FD in E ; atq; erit EF axis, atq; E vertex principalis Parabolæ, per quam movetur projectile. Unde describetur Trajectoria per notas proprietates Parabolæ. *Q. E. F.*

DEMONSTRATIO.

Est enim AC quarta pars parametri ad verticem A . Unde constant cætera ex conicis.

2. Ad punctum Trajectoriæ quodvis G , duc GH ipsi AC parallelam, & ipsi CD occurrentem in H ; atque iter HG altitudo, per quam corpus cadendo acquirere potest

Z

potest velocitatem, quâ movetur projectile in puncto G . *Q. E. F.*

Hoc item constat ex Prop 2. & ex conicis.

Scholium. Si ad puncta A , & C (Fig. 2.) ducantur tangentes AB , CG occurrentes rectis horizonti perpendicularibus CB , AG , in B & G ; velocitates in A & C erunt inter se ut tangentium partes interceptæ, AB , CG .

P R O P. IV.

Unico factò experimento invenire velocitatem projectionis.

Projiciatur corpus de loco A (Fig. 2.) in directione qualibet AB , atq; observetur punctum percussum C . In directione vis gravitatis ducatur CB , ipsi AB occurrens in B , atque ipsis CB , AB , fiat tertia proportionalis L . Erit quarta pars longitudinis L altitudo, per quam corpus cadendo acquirere potest velocitatem projectilis in A . *Q. E. I.*

D E M O N S T R A T I O.

Est enim L parameter Trajectoriæ ad punctum A ; unde constat solutio per propositionem secundam.

Scholium. Commodissimè instituitur experimentum, erectâ ad horizontem perpendiculari AG , & directionem sumendo AB , quæ bifecet angulum CAG , rectâ etiam AC existente horizonti parallelâ. Nam in isto casu altitudo quæsitâ, æqualis est dimidio distantia AC .

PROP.

P R O P. V.

Datis directione & velocitate projectionis; invenire occursum Trajectoriæ cum rectâ transeunte per punctum unde fit projectio.

Projiciatur corpus de loco A (Fig. 4.) in directione rectæ AB . In directione gravitati contrariâ, fiat AC æqualis altitudini, per quam corpus cadendo acquirere potest velocitatem, quâ fit projectio, atq; ducatur CE ipsi AC perpendicularis. Fiat FA æqualis ipsi CA , atq; angulum constituens FAB æqualem angulo CAB . Sit AK recta, cujus occursum cum Trajectoriâ quæritur. Duc FI ipsi AK perpendicularem, atq; ipsi CE occurrentem in D . In CE sume ED æqualem CD , atq; ducatur ipsi CE perpendicularis EK , ipsi AK occurrens in K . Erit K punctum quæsitum.

D E M O N S T R A T I O.

In FI productâ fiat fI æqualis FI , atq; ducantur fA , fE , FE , FK . Quoniam est angulus FIA rectus, atq; fI æqualis FI , est etiam fA æqualis FA . Sed per constructionem est FA æqualis CA , atque angulus DCA rectus. Sunt ergo puncta C , F , f ad circulum centro A descriptum, quem tangit recta DC in C . Sunt ergo FD , CD , fD , continuè proportionales. Sed est ED æqualis CD (per constructionem). Sunt ergo FD , ED , fD continuè proportionales; adeoque ob angulum communem ad D , triangula FED , EfD sunt similia, atque angulus DEF æqualis angulo EfF .

Puncta itaq; tria F , E , f sunt ad circulum, quem tangit recta ED in E . Sed quoniam est fI æqualis FI , atq; angulus FIK rectus, centrum istius circuli est in rectâ IK ; item quoniam est angulus DEK rectus, centrum illud est etiam in rectâ EK . Est ergo K centrum istius circuli, adeoq; FK æqualis est ipsi EK . Jam (*per Prop. 3.*) sunt F focus Trajectoriæ, atq; C quarta pars parametri ad punctum A . Unde cum sit CE ad AC & EK perpendicularis, atq; FK æqualis EK , erit punctum K ad Trajectoriam (*per conica*). *Q. E. D.*

P R O P. VI.

*Hisdem datis, invenire occursum Trajectoriæ cum rectâ,
quâlibet positione datâ.*

Projiciatur corpus de loco A (Fig. 5.) in directione AB , sitq; GH recta cujus occursum cum Trajectoriâ quæritur. Duc AC in directione gravitati contrariâ, atq; æqualem altitudini, per quam corpus cadendo acquirere potest velocitatem, quâ sit projectio; & duc AF æqualem ipsi AC , ita ut sit angulus FAB æqualis angulo CAB ; & ducatur CE perpendicularis ipsi CA . Ducatur FI ipsi GH occurrens ad angulos rectos in I , atq; ipsi CE in D ; & in FI fiat fI æqualis FI . In CE fiat ED media proportionalis inter FD & fD ; & ipsi CE ducatur perpendicularis EK , ipsi GH occurrens in K . Erit K punctum quæsitum. *Q. E. I.*

D E M O N S T R A T I O.

Conjungendo fE demonstratur ad modum propositionis præcedentis.

Scholium.

Scholium. Quoniam punctum E sumi potest ad utramlibet partem puncti D , duo sunt puncta K, k , ubi recta GH occurrit Trajectoriæ.

P R O P. VII.

Datâ velocitate projectionis, invenire directionem, quæ faciat, ut Trajectoria transeat per punctum datum.

Projiciatur corpus de loco A , (Fig. 4.) & sit K punctum, per quod transire debet Trajectoria quæsitâ. Fiat AC , in directione gravitati contrariâ, æqualis altitudini, per quam corpus cadendo acquirere potest velocitatem projectionis. Ducatur CE ipsi AC perpendicularis, & ad eam duc KE perpendicularem. Centris A & K , & radius CA, EK describantur duo circuli sibi mutuo occurrentes in F . Duc FA , & biseca angulum CAF rectâ AB . Erit AB directio quæsitâ, in quâ fieri debet projectio, ut transeat Trajectoria per punctum K . *Q. E. F.*

D E M O N S T R A T I O.

Est CA æqualis quartæ parti parametri ad punctum A (per *Prop. 2.*) Et per constructionem sunt FA, CA æquales, item FK, EK . Est ergo F focus Parabolæ per puncta A, K , descriptæ: Sed illam tangit recta AB in A , propter angulos FAB, CAB æquales. Corpore itaq; projecto de puncto A , in directione AB , eâ cum velocitate, quam corpus acquirere potest cadendo per altitudinem CA , transibit Trajectoria per punctum K . *Q. E. D.*

NB. Cum

NB. Cum circulorum centris A, K , & radiis CA, EK , descriptorum duo sint concursus, F, f , bisectis angulis FAC, fAC , duo etiam erunt directiones, quæ faciant, ut Trajectoria transeat per punctum datum K .

P R O P. VIII.

Datâ directione projectionis, invenire velocitatem, quæ faciat ut Trajectoria transeat per punctum datum.

Projiciatur corpus de loco A (Fig. 6.) in directione rectæ AB , & faciendum sit ut transeat Trajectoria per punctum K . Duc AK , eamq; biseca in C , & in directione gravitatis duc CB , ipsi AB occurrentem in B ; & junge BK . Duc AD, KE , ipsi CB parallelas, & ducantur AF, KF sibi mutuo occurrentes in F , ita ut sint anguli FAB, DAB æquales, item FKB, EKB . Erit FA æqualis altitudini, per quam corpus cadendo acquirere potest velocitatem quæsitam, quâ projectio fieri debet in directione AB , ut transeat Trajectoria per K . *Q. E. F.*

D E M O N S T R A T I O.

Quoniam CB est in directione gravitatis, est diameter Parabolæ; & quoniam CA æqualis est CK , est CB diameter ad ordinatam AK . Unde cum sit AB tangens ad parabolam in A , erit etiam KB tangens ad punctum K . Hinc quoniam AD est in directione diametrorum, atq; angulus FAB æqualis angulo DAB , transit AF per focum parabolæ. Eodem argumento transit etiam KF per focum. Est ergo

ergo F focus parabolæ, adeoq; FA quarta pars parametri ad punctum A , quæ proinde æqualis est altitudini, per quam corpus cadendo acquirere potest velocitatem ad hoc necessariam, ut projecto corpore de A , in directione AB , transeat Trajectoria per punctum K .

P R O P. IX.

Invenire velocitatem minimam & directionem ei congruam, quâ fieri potest, ut transeat Trajectoria per punctum datum.

Projiciendum sit corpus de loco A (Fig. 7.) cum velocitate omnium minimâ & directione ei congruâ, quâ fieri potest ut perveniat in locum K , hoc est ut transeat Trajectoria per punctum K . Ductis AC , KE in directione gravitati contrariâ, & ductâ AK , biseca angulos CAK , EKA , rectis AB , KB , sibi mutuo occurrentibus in B . Duc BC ipsi AC occurrentem ad angulos rectos in C , atq; erit CA altitudo, per quam corpus cadendo acquirere potest velocitatem quæsitam; eritq; AB directio quæsitâ. $Q. E. F.$

D E M O N S T R A T I O.

Ducatur BF ipsi AK occurrens ad angulos rectos in F , atque occurrat CB ipsi KE in E . Propter angulos CAB , BAF , item angulos EKB , BKF , æquales atq; angulos rectos in C , E , & F , erunt æquales CA , FA , item EK , FK . Hinc constat puncta A , K esse ad parabolam, quam tangit recta AB in A , cujusq; parameter ad punctum A est quadruplum altitudinis CA , foco existente F . Corpore itaque projecto

projecto de A in directione AB , eâ cum velocitate, quam corpus acquirere potest cadendo per altitudinem CA , Trajectoria erit dicta Parabola (per Prop. 2.) Dico autem illam esse velocitatem minimam, seu esse CA partem quartam parametri omnium minimæ, quâ Parabola describi potest, quæ transeat per puncta A, K .

Si fieri potest, in CA capiatur altitudo cA minor, quæ sit quarta pars parametri ad punctum A . Duc ipsi CA perpendicularem ce , ipsi KE occurrentem in e , & centro A & radio Ac describatur circulus ipsi AK occurrens in f . Quoniam cA dicitur quarta pars parametri ad punctum A , focus Parabolæ erit punctum aliquod p , in circumferentia circuli cpf , centro A & radio Ac descripti. Si ergo sit punctum K ad parabolam illam, erit pK æqualis eK . Est vero FK æqualis EK . Unde cum sit eK minor ipsâ EK , erit etiam pK minor ipsâ FK . Sed est pK major ipsâ fK , atq; est fK major ipsâ FK , (quoniam est fA minor ipsâ FA per hyp.) unde fit pK major ipsâ FK . Sed jam dicebatur pK minor ipsâ FK ; quæ repugnant. Nequit ergo Parabola describi, quæ transeat per puncta A, K , minori parametro quam in solutione definitum est. *Q. E. D.*

P R O P. X.

Datâ velocitate projectionis, invenire directionem, quæ faciat, ut corpus projiciatur ad distantiam omnium maximam in plano dato; atq; distantiam illam definire.

Sit planum datum AK (Fig. 8.) atq; invenienda sit distantia maxima AK , ad quam corpus projici potest in plano illo. Duc

Duc AC in directione gravitati contrariâ, æqualem quartæ parti parametri ad punctum A . Tum bisecto angulo CAK rectâ AB , erit AB directio projectionis quæsitâ. Duc CB ipsi CA perpendicularem, rectæ AB occurrentem in B , atque in CB productâ fiat BE æqualis ipsi BC . Tum ductâ EK , ipsi CA parallelâ, quæ occurrat plano AK in K , erit AK distantia maxima quæsitâ.

D E M O N S T R A T I O.

Centro A & radio AC describe circumulum, ipsi AK occurrentem in F , & ducantur BF , BK . Quoniam anguli CAB , BAF sunt æquales (per constructionem) atque AF æqualis CA , erit BF æqualis CB , æqualis BE (per constructionem) atq; anguli ad F recti. Unde etiam fit FK æqualis EK . Sunt ergo puncta A , K ad parabolam foco F descriptam, quam tangit AB in A (propter angulos CAB , FAB æquales) quartâ parte parametri ad punctum A existente CA . Corpore igitur projecto de loco A , in directione AB , eâ cum velocitate, quam corpus acquirere potest cadendo per altitudinem CA , Trajectoriâ transibit per punctum K (per Prop. 2.) *Q. E. D.*

Dico autem, quod fit KA distantia omnium maxima, ad quam corpus projici potest de loco A eâdem cum velocitate.

Si fieri potest, eâdem parametro, ad A describatur parabola, quæ transeat per punctum distantius k , hoc est projiciatur corpus ad distantiam majorem kA . Duc Bk , atq; ipsi KE parallelam ke , ipsi CE occurrentem in e . Quoniam FB , EB , item FK , EK sunt æquales, sunt etiam anguli FBK , EBK æquales. Angulus ergo FBk major est angulo kBe ; unde

A a.

fit

fit kF major ipsa ke . Sed quoniam est AC quarta pars parametri ad punctum A , focus parabolæ erit alicubi in circumferentiâ circuli centro A , & radio CA descripti. Sit focus ille p , & ducatur pk . Tum quoniam pk major est ipsâ Fk , erit etiam pk major ipsâ ke . Sed ut parabola transeat per punctum k , debet esse pk æqualis ke . Nequit ergo parabola duci in circumstantiis propositis, quæ transeat per punctum k distantius puncto K ; adeoq; nec corpus projici ad distantiam majorem ipsâ KA . *Q. E. D.*

P R O P. XI.

Iisdem positis, invenire locum puncti K, seu Curvam describere, quæ tangat omnes parabolas eodem vertice A & eâdem parametro descriptas.

Sit A (Fig. 9.) vertex datus, atq; in directione gravitati contrariâ ducatur AC æqualis quartæ parti parametri datæ. Tum descriptâ parabolâ, cujus vertex principalis sit C , atq; focus A ; erit ea curva quæsitâ.

D E M O N S T R A T I O.

Duc quamlibet AK , atq; in eâ sume FA æqualem CA , & ducatur CB ad CA perpendicularis, sitq; K punctum in propositione præcedente inventum. In AC productâ, factâ Cc æquali CA , ducatur ce parallela ipsi CE ; ducatur etiam KE parallela ipsi AC , ipsis CE , ce occurrens in E & e . Per propositionem præcedentem est KE æqualis ipsi FK ; unde cum sit etiam FA æqualis ipsi AC , æqualis ipsis Cc , Ee (per con-

constructionem) est ergo Ke æqualis KA ; unde est punctum K ad parabolam foco A & vertice principali C descriptam. *Q. E. D.*

Bifecto autem angulo AKE à rectâ KB , tanget hæc utramq; parabolam, tam foco F per A & K , quam foco A per K descriptam. Unde se mutuo tangunt parabolæ. *Q. E. D.*

Errat. Pag. 152. l. ult. pro $\beta c.$ l. $Bb.$

F I N I S.

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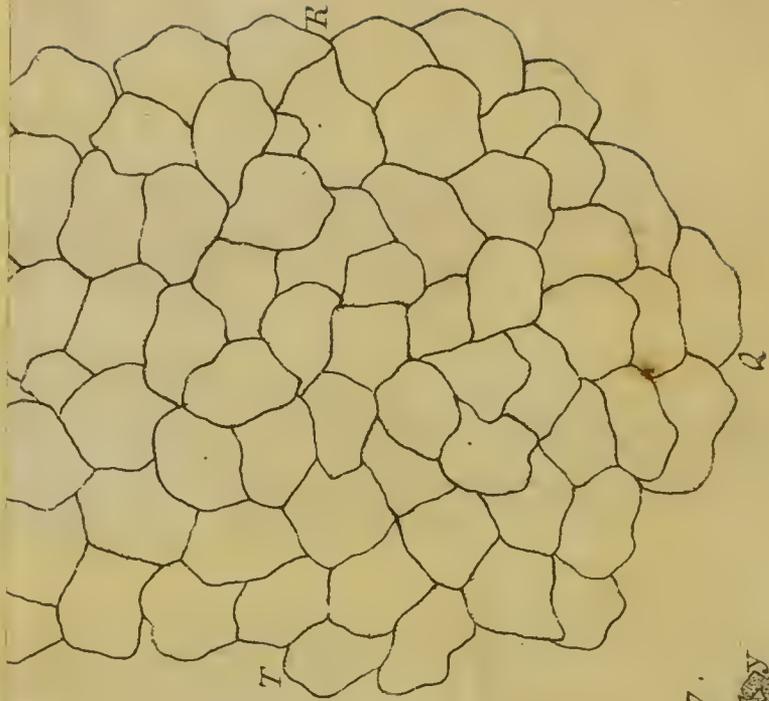


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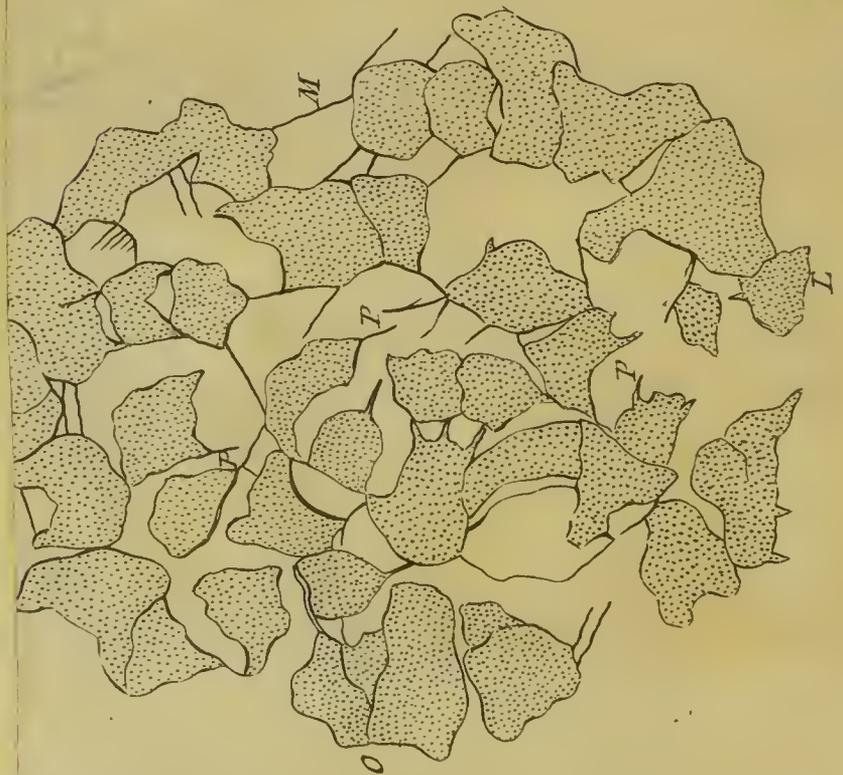


Fig: 7.

PHILOSOPHICAL TRANSACTIONS.

For the Months of *May, June, July, and August,*
1721.

The CONTENTS.

- I. *A Description of the Moose-Deer in America:*
By the Honourable Paul Dudley, Esq; F. R. S.
Communicated by John Chamberlayne, Esq;
- II. *Some Remarks on the Allowances to be made in
Astronomical Observations for the Refraction
of the Air.* By Dr. Edm. Halley, R. S. S.
Astronomer Royal: *With an accurate Table of
Refractions.*
- III. *The Variation of the Magnetical Compass, ob-
served by Capt. Rogers, Commander of the Ship
Duke, in his Passage from Cape St. Lucar in
California to the Isle of Guam or Guana, one
of the Ladrones, with some Remarks thereon.*
Communicated by the same.

IV. *An*

- IV. *An Addition to the Description of the Art of Living under Water, publish'd in Phil. Transact. No. 349. By the same.*
- V. *An Account of an Aurora Borealis, observed at Dublin, on Monday the sixth of February 1720-1. By J. W.*
- VI. *A Description of an Aurora Borealis, seen on the same Day at Cruwys-Morehard, in Devonshire. By Samuel Cruwys, Esq; F.R.S.*
- VII. *Observations on the Muscular Fibres of Fish. By Mr. Leeuwenhoek, F. R. S.*
- VIII. *Observations upon the Seeds of Plants. By the same. Translated by John Chamberlayne, Esq;*
- IX. *Extract of a Letter from Dr. Brook Taylor, F. R. S. to Sir Hans Sloan, dated 25. June, 1714. Giving an Account of some Experiments relating to Magnetism.*

I. *A Description of the Moose-Deer in America.*
 By the Honourable Paul Dudley, Esq; F. R. S.
 Communicated by John Chamberlayne, Esq;

Among other things the *Royal Society* have had from *America*, I find, in Mr. *Lowthorp's* *Abridgment of the Philosoph. Transact.* a short Account of the Moose, taken from Mr. *Joselyn's* *History of New England*, but in my opinion so imperfect, that I shall not scruple to present you with the Natural History of that fine Creature, partly from my own Knowledge, and partly from the Information of Men of Ingenuity and Probity, that are better acquainted with it.

The Moose is a Creature, not only proper, but it is thought peculiar, to North *America*, and one of the noblest Creatures of the Forest; the *Aborigines* have given him the Name of *Moose*, *Mooseuk* in the Plural.

Of Moose there are two sorts, the *Common light grey Moose*, by the *Indians* called *Wampoose*; these are more like the ordinary Deer, spring like them, and herd sometimes to thirty in a Company. And then there are the *large, or black Moose*, of which I shall now give you the following Account.

First, That he is the Head of the Deer-kind, has many things in Common with other Deer, in ma-
 ny

ny things differs, but in all very superiour. The Moose is made much like a Deer, parts the Hoof, chews the Cudd, has no Gall, his Fars large and erect. The Hair of the black Moose is a dark Grey, and upon the Ridge of his Back to ten and twelve Inches long, of which the *Indians* make good Belts. He has a very short Bob for a Tail. Mr. *Neal*, in his late History of this Country, speaking of the Moose, says they have a long Tail; but that Gentleman was imposed on, as to other things besides the Moose.

Our Hunters have found a Buck, or Stag-Moose, of fourteen Spans in height from the Withers, reckoning nine Inches to a Span; a quarter of his Venison weighed more than two hundred pounds. A few Years since, a Gentleman surprized one of these black Moose, in his Grounds within two miles of *Boston*; it proved a Doe or Hind of the fourth Year: After she was dead, they measured her upon the Ground, from the Nose to the Tail, between ten and eleven Feet, she wanted an Inch of seven Foot in height.

The Horns of the Moose, when full grown, are between four and five Foot from the Head to the Tip, and have seven Shoots or Branches to each Horn, and generally spread about six Foot. When the Horns come out of the Head, they are round, like the Horns of an Ox; about a Foot from the Head, they begin to grow a palm broad, and further up still wider, of which the *Indians* make good Ladles, that will hold a Pint. When a Moose goes through a Thicket, or under the Boughs of Trees, he lays his Horns back on his Neck, not only that he may make his way the easier, but to cover his Body from the Bruise

Bruise, or Scratch of the Wood. These mighty Horns are shed every Year. The Doe-Moose has none of these Horns.

A Moose does not spring, or rise in going, as an ordinary Deer, but shoves along side-ways, throwing out the Feet, much like a Horse in a racking pace. One of these large black Moose, in his common Walk, has been seen to step over a Gate, or Fence, 5 Foot high. After you unharbour a Moose, he will run a Course of 20, or 30 miles, before he turns about, or comes to a Bay; when they are chased, they generally take to the Water; the common Deer, for a short space, are swifter than a Moose, but then a Moose soon outwinds a Deer.

The Meat of a Moose is excellent Food, and tho' it be not so delicate as the common Venison, yet it is more substantial, and will bear salting: The Nose is look'd upon as a great Dainty; I have eat several of them my self; they are perfect Marrow. The *Indians* have told me, that they can travel three times as far after a Meal of Moose, as after any other Flesh of the Forest.

The Black Moose are not very gregarious, being rarely found above four or five together; the young ones keep with the Dam a full Year,

A Moose calves every Year, and generally brings two. The Moose bring forth their young ones standing, and the young fall from the Dam upon their Feet. The time of their bringing forth is generally in the Month of *April*. See *Job* Cap. 39. V. 1, 2, 3. *Canst thou mark when the hinds do calve, or knowest thou the time when they bring forth? They bow themselves, they bring forth their young ones, they cast forth their sorrows.*

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The Moose being very tall, and having short Necks, do not graze on the Ground, as the common Deer, Neat Cattel, &c. do, and if at any time they eat Grass, 'tis the top of that which grows very high, or on steep rising Ground. In the Summer they feed upon Plants, Herbs, and young Shrubs, that grow upon the Land, but mostly, and with greatest delight on Water-Plants, especially a sort of wild Colts-foot and Lilly, that abound in our Ponds, and by the sides of the Rivers, and for which the Moose will wade far and deep, and by the noise they make in the Water, our Hunters often discover them. In the Winter they live upon Browse, or the tops of Bushes and young Trees, and being very tall and strong, they will bend down a Tree as big as a Man's Leg, and where the Browse fails them, they will eat off the Bark of some sorts of Trees, as high as they can reach. They generally feed in the Night, and lie still in the Day.

The Skin of the Moose, when well dress'd, makes excellent Buff; the *Indians* make their Snow-shoes of them: Their way of dressing it, which is reckoned very good, is thus: After they have hair'd and grain'd the Hide, they make a Lather of the Moose's Brains in warm Water, and after they have soaked the Hide for some time, they stretch and supple it.

II. *Some Remarks on the Allowances to be made in Astronomical Observations for the Refraction of the Air.* By Dr. Edm. Halley, R. S. S. Astronomer Royal. With an accurate Table of Refractions.

WERE the *Medium* of our *Air* much more in Quantity, or the Force of *Gravity* much greater than it is, or in a word, were the Refractive Power of the *Air* much more sensible than we find it, nothing could have been a greater Impediment to Discoveries in Astronomy: For all Objects appearing by Refraction higher than really they are, till such time as the Laws and Quantity of that Refraction had been ascertained, it would have been impossible to have been secure of the true observed Place of any Cœlestial Object. But as it falls out to be so little, that none but nice Instruments can perceive its Effects, it was not discovered to be at all, till *Bernard Walther's* time, about the Year 1500; nor brought to any sort of Rule till *Tycho Brahe*; nor ascertained, till our worthy *President* made the first accurate Table thereof: The *Curve* which a Beam of *Light* describes, as it approaches the *Earth*, being one of the most perplext and intricate that can well be proposed, as Dr. *Brook Taylor* in the last Proposition of his *Methodus Incrementorum* has made it evident. By this Table it follows that the *ratio*

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of the Sine of the Angle of Incidence to that of the Refracted Angle, encreasing as the Beam approaches, makes a very notable difference in the place of an Object near the Horizon: but in Objects that are much elevated, the Refractions become small, and their Differences scarce exceed a Second *per* Degree; so that they are sufficiently the same, as if the Incident and Refracted Angles were on the Surface of a Sphere of Air of the same uniform Density close adjoining to the Eye.

When therefore the Stars are twenty degrees or upwards elevated above the Horizon, we may take it for granted, without sensible Error, that the Sines of the true and apparent Distances from the *Vertex*, are in the same constant *ratio*. Hence it will appear that the Distances of all the Stars are seen less than they really are, in whatever position they are taken, and that not less than a Second *per* Degree of the distance; that is, a distance of 30 degrees, for example, is contracted at least so many seconds, and one of 60 gr. no less than a Minute, if the distances be taken by an Instrument that is truly divided. So that when Mr. *Hevelius*, to shew the exactness of his Observations, brings eight Distances, as taken by his *Sextant*, which exactly compleat the Circle, both in Longitude and Right Ascension; the consequence is really quite opposite to his Design: for if those distances were the true ones, they being all contracted by appearing through a refracting *Medium*, the Sum of the eight differences of both Longitude and Right Ascension, ought to fall short of a whole Circle or 360 degrees by at least six minutes; so that I am inclined to believe that the sixty degrees of Mr. *Hevelius*'s *Sextant* wanted about a Minute of its true quantity.

Such

Such an allowance as this may perhaps be a proper Expedient to avoid accounting for Refraction in cœlestial Observations, provided the Objects be nearly parallel to the Horizon, or at a good height above it. For all distances of Stars are contracted by Refraction, when they are parallel to the Horizon, by the same constant quantity, be they high or low, that is by about one Second *per* Degree; the Chords of the Arches of the real and visible distances being always in the same *ratio* as is the Sine of the Angle of Incidence to that of the refracted Angle.

And this is the case wherein the *Refraction* of the *Air* does least affect the distances of the *Stars*, which Distances are still more and more contracted, as they are nearer to a perpendicular Situation: So that a Distance, for Example, of thirty Degrees loses but half a Minute in a horizontal Site; but if the one Star be 20 degrees high, and the other fifty, it will be lessened by above three times as much, or by 1 m. 41 sec. If the one be 30 and the other 60 Degrees high, the same distance will appear less than 30 Degrees by about one Minute; the difference still decreasing as the Objects are more elevated above the Horizon. But in all cases to account for the effect of the *Refraction* upon the Distances of the Stars, requires, besides some Trigonometrical Work, the help of the afore-mentioned Table, which I here subjoyn for the use of the Curious, such as I long since received it from its Great Author; it having never yet, that I know of, been made publick.

*Tabula Refractionum Siderum ad Altitudines
apparentes.*

Alt. Appar. deg. m.	Refractio. m. sec.	Alt. Appar. deg.	Refractio. m. sec.	Alt. Appar. deg.	Refractio. m. sec.
0 0	33 45	16	3 4	46	0 52
0 15	30 24	17	2 53	47	0 50
0 30	27 35	18	2 43	48	0 48
0 45	25 11	19	2 34	49	0 47
1 0	23 7	20	2 26	50	0 45
1 15	21 20	21	2 18	51	0 44
1 30	19 46	22	2 11	52	0 42
1 45	18 22	23	2 5	53	0 40
2 0	17 8	24	1 59	54	0 39
2 30	15 2	25	1 54	55	0 38
3 0	13 20	26	1 49	56	0 36
3 30	11 57	27	1 44	57	0 35
4 0	10 48	28	1 40	58	0 34
4 30	9 50	29	1 36	59	0 32
5 0	9 2	30	1 32	60	0 31
5 30	8 21	31	1 28	61	0 30
6 0	7 45	32	1 25	62	0 28
6 30	7 14	33	1 22	63	0 27
7 0	6 47	34	1 19	64	0 26
7 30	6 22	35	1 16	65	0 25
8 0	6 0	36	1 13	66	0 24
8 30	5 40	37	1 11	67	0 23
9 0	5 22	38	1 8	68	0 22
9 30	5 6	39	1 6	69	0 21
10 0	4 52	40	1 4	70	0 20
11 0	4 27	41	1 2	71	0 19
12 0	4 5	42	1 0	72	0 18
13 0	3 47	43	0 58	73	0 17
14 0	3 31	44	0 56	74	0 16
15 0	3 17	45	0 54	75	0 15

III. *The Variation of the Magnetical Compass, observed by Capt. Rogers, Commander of the Ship Duke, in his Passage from Cape St. Lucar in Calefornia to the Isle of Guam or Guana, one of the Ladrones, with some Remarks thereon. Communicated by the same.*

HAVING lately had the Opportunity of perusing Capt. *Woods Rogers's* original Journal, who in 1709-10, in eight Weeks time traversed the great *South-Sea*, or *Pacifick Ocean*, I was highly pleased to find the Care he had taken to set down the Variations of the *Magnetical Compass* in his Passage from the South Cape of *Calefornia* to the Island of *Guana*, being about seven Hours or 105 Degrees of Longitude. This might have been long since expected from Capt. *Dampier*, who had three times made the *Tour* of the *World*, and thrice gone this very same Track.

It were to be wisht that the *French*, who have had frequent Opportunities to do it, would bestow upon us an account of the Variations they have lately found in their Voyages from *Peru* and *Chili* to *China*; and that the *Spaniards* would tell us how the Needle varies at this time in the North Part of that great Sea, through which they return from the *Manilla's* to *New Spain*. With these helps, having three Points in each Curve, we might be enabled

abled with a tolerable certainty to compleat the System of the Magnetick Variations, which I was forced to leave unfinished, as to this part of the Ocean, in my General Chart thereof, for want of the Observations requisite.

In the mean time, please to take the following Account extracted from Capt. *Rogers's* Journal; wherein the first Column gives the correct Latitude of the Place; the second, the Longitude West from *London*, as estimated by Reckoning; and the third the Variation, which in this whole Track is *Easterly*.

Variations observed in the Great South-Sea, from the South Cape of California to the Island of Guana or Guam, one of the Ladrões.

January 1709-10.	Lat. correct. every	Day	Long. West from London	Variation Easterly.
12	22	16	114 09	03 00
	21	18	114 42	02 50
	20	24	115 15	02 50
15	19	25	115 45	02 50
	18	56	116 24	02 45
	18	00	117 06	02 45
	17	11	117 30	02 15
	16	32	118 05	02 00
20	15	44	118 54	01 50
	15	00	120 15	01 30
	14	49	122 05	01 10
	14	36	124 25	00 50
	14	24	126 45	00 40
25	14	14	129 05	00 45
	13	50	131 23	00 50
	13	29	132 58	01 00
	13	29	134 41	01 10
	13	22	136 48	01 15
30	13	27	139 21	01 25
	13	32	142 07	01 30
Feb. 1	13	32	144 37	01 40
	13	36	147 32	01 50

Variations observed in the SOUTH-SEA.

1709-10.	Lat. North correct. daily	Long. West from London.	Variation Easterly.
Feb. 3.	13 26	150 18	02 00
	13 26	153 02	02 10
5	13 26	155 19	02 25
	13 26	157 43	02 30
	13 25	160 31	02 50
	13 41	163 00	03 00
	13 41	165 18	03 20
10	13 44	167 26	03 30
	13 36	169 56	03 45
	13 33	172 27	04 00
	13 36	175 00	04 30
	13 32	177 21	05 20
15	13 40	179 28	06 30
	13 47	181 24	07 00
	13 54	183 22	07 30
	13 52	185 37	09 00
	13 40	187 42	10 15
20	13 28	189 49	11 00
	13 21	191 30	11 30
	13 12	193 25	12 00
	13 07	194 37	11 50
	13 10	195 51	11 00
25	13 03	197 51	10 00
	13 00	199 03	09 50
	12 57	200 16	09 30
	12 54	202 20	09 00
March 1.	12 58	204 12	08 40
	13 04	206 06	08 20
	13 05	207 33	08 00
	13 05	209 04	07 50
5	13 02	211 54	07 30
	13 07	212 42	07 10
	13 07	214 07	07 00
	13 03	215 28	06 50
	13 08	217 11	06 30
10	13 16	218 27	05 40

Island of *Guana* in Sight.

By.

By this it appears, that at about 250 or 300 Leagues West from the South-head of *California*, the *East Variation* diminishes to about $\frac{3}{4}$ of a degree; That for 1300 Leagues from thence, the same *Easterly Variation* gradually encreases to about 12 degrees, where it becomes greatest. And that at the Isle of *Guam*, five hundred Leagues still more *Westerly*, it is again decreased to 5 degrees 40 minutes.

As far as this single Instance can direct us, I am inclinable to think, that in all that space of Sea which lies to the Northwards of our Track, between *Japon* and *California*, there reigns an *Easterly Variation*, which is still greater and greater as the North Latitude encreases. But that to the Southward of our Track, and especially to the Southward of the Equinoctial, a *Westerly Variation* arises, of no great extent or quantity, but which is greatest about 1000 Leagues West from the Coasts of *Peru* and *Chili*, about the same Meridians where Capt. *Rogers* found the *East Variation* smallest. This is agreeable to the *Theory* of the *Variation* I laid down in No. 148. of these *Transactions*, about 40 Years since; and I then expressly mentioned, in my seventh Remark on the Observations there cited, that there was undoubtedly such a Tract of *West Variation* in the Southern Parts of the *South-Sea*, it being the necessary Consequence of the Site of the four Magnetical Poles there supposed, though at that time I wanted Experiments to prove it.

IV. *An Addition to the Description of the Art of Living under Water, publish'd in Phil. Transact. No. 349. By the same.*

IN No. 349. of the *Philosophical Transactions*, I did, as I suppose, sufficiently explain the Method I had practiced and found effectual to furnish Air at any reasonable Depth under Water, and in any quantity desired, for the Subsistence of men that shall have occasion to work on Wrecks, or otherwise at the bottom, under a great Pressure of Water. This I did by means of the Diving-Bell, which, being from time to time replenished with fresh Air, I had found sufficient to maintain five Men for near two Hours together in ten fathom Water, without the least Hurt or Inconvenience. But the Bell being not to be moved from place to place, but by moving the Vessel from which it hung suspended, was a great Impediment to the Work that was to be done below; and therefore I bethought my self how to enable the Diver to go out from the Bell to a considerable distance, and to stay a sufficient time without it, with full freedom to act as occasion served. And considering that the Pressure being greater on the Surface of the Water in the Bell, than on any other Surface that was higher than it, the Air would by a Pipe pass from the Bell into a Cavity of Air over that higher Surface; I concluded, that putting on a Cap of Lead made weighty enough to

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sink

sink empty, and in form resembling the Bell it self, I might by flexible Pipes, which a man might carry coiled on his Arm, receive a constant Stream of Air from the Magazine thereof in the great Bell, so long as the Surface of the Water in the Caps was above the Level of that in the Bell.

Following this Idea, I procured Pipes to be made, which answered all that was hoped from them. They were secured against the Pressure of the Water, by a spiral brass Wire, which kept them open from end to end, the Diameter of the Cavity being about the sixth part of an Inch. These Wires we coated with thin Glove-leather, curiously sowed on, and then dipt the Leather into a Mixture of Oyl and Bees-Wax hot, which, filling up the Pores of the Leather, made it impenetrable to Water. Then we drew several Folds of Sheeps Guts over them, which when dry, we painted with a good coat of Paint, and then secured the whole with another coat of Leather, to keep them from fretting. The Pipes of which we made several, were much about forty Foot long, the size of a half Inch Rope; the one end thereof being fixt in the Bell, at some height above the Water, and the other end fastned to a Cock, which opened into the Cap. The use of the Cock being to stop the return of the Air, whenever there was occasion to stoop down, or go below the Surface of the Air in the Bell, which was necessary as often as there was occasion to go out or return into the Bell.

The Diver therefore putting on his Cap, and coyling his Pipe on his Arm, like a Rope, as soon as he is discharged from the Bell, opens his Cock, and marches on the bottom of the Sea, vearing out the
Coiles

Coiles of his Pipe, which serve as a Clue to direct him back again : and this I have seen practis'd, without any ill Incident attending it.

But there are two things to be remark'd in this affair ; first, That the Weight of a Man being very little more than that of his Bulk in Water, he cannot act with any Strength, nor stand with any Firmity, especially where any thing of a Stream runs, without a considerable addition of Weight ; and therefore the leaden Caps were made to weigh about half a hundred weight, to which I added a Girdle of large Weights of Leads, of about the same Weight in the whole, this being to be worn about the Waist ; and two Clogs of Lead for the Feet, of about 12 pound each. With this Accession of Weight I found a Man could stand well in an ordinary Stream, and even go against it. The other thing necessary to be provided against, was the Cold of the Water, which though it could not be wholly taken off, so that a Man could endure it long, yet it was much eas'd by Habits of Wastecoat and Drawers, made close to the Body, of that thick sort of woollen Stuff they make Blankets of : This being full of Water, would be a little warm'd by the Heat of the Body, and keep off the Chill of new cold Water coming on it.

As to Sight under Water, as long as the Water is not turbid, things are seen sufficiently distinct ; but a small degree of Thickness makes perfect Night, in no great depth of Water : In my leaden Caps, which from their use I call'd *Caps of Maintenance*, I at first fixt a plain Glass before the Sight, but soon found that the Vapour of the Breath would make such a Dew on the Surface of the Glass, that it hindred

its Transparency : To remedy which, I found it necessary to prolong that Side of the Cap that was before the Eyes, and thereby enlarged the Prospect of what was under us.

V. *An Account of an Aurora Borealis, observed at Dublin, on Monday the sixth of February, 1720-1. By J. W.*

THE *Royal Society* having done me an unexpected Honour, in causing a short Account, which I sent 'em of the *Aurora Boreales*, that were seen the 10th and 24th of *November*, in the Year 1719, to be printed in their Journals ; I now present them with an Extract of what was observed on *Monday* the 6th of *February* last. The Air was all that Day, as it had been for some time before, very clear and sharp ; about half an Hour past four in the Evening, some flying Clouds appear'd, and the Sky was ting'd with a very unusual yellowish Colour, which perhaps might be reflected from a great Quantity of Snow, that soon after fell for near a quarter of an Hour. However that might be, I'm willing to date the beginning of the ensuing *Phænomena* from the first appearance of this uncommon Light. About a quarter past six, a thin Vapour, which was as yet very ill defin'd, and in all appearance resembl'd an exceeding black Cloud, had fix'd it self in the Northern *Hemisphere* ; its Edges were ting'd with a reddish Yellow, that by degrees, as it approach'd the

Vertex,

Vertex, grew more dilute, till at last it ended in a faint Whiteness. That in reality it was no Cloud, but only a Vapour exceeding pure and limpid, was manifest, because several of the fix'd Stars shone thro' it, without having their Light in any degree effac'd. In the midst of this dark *Basis*, about half an Hour past six, a lucid *Area* shew'd it self due N. East, about 35 deg. above the *Horizon*, and in less than a Minute from the time I first discover'd it, emitted a very large *Pyramidal* Stream of shining Vapour, which with an incredible Swiftnefs ascended obliquely towards S. S. West, so as to leave the *Zenith* considerably to the Westward, and very soon after, about the same Place, six others arose at the same instant almost to the *Zenith*. From this time till 48 Minutes past six, we had repeated Projections of these lucid Rays, without any order as to Time, Place, or Magnitude. They did not only arise from behind the dark *Basis*, but sometimes as it were out of the pure Sky; and tho' some of them continu'd visible more than a Minute, yet the greater part of them only just shew'd themselves and died away. I had now got to the Top of a convenient Observatory, where (though destitute of Instruments) I had a free Prospect of the *Horizon*; and in company of another Gentleman, fix'd my self with great Attention, to expect the ensuing *Phases* of this *Phenomenon*.

About 6 h. 55 m. between N. W. by North, and W. N. West, we found the Representation of a very bright *Crepusculum*, such as that which appears about 20 Minutes after Sun-set; from which arose several very large Beams of Light, not exactly erect towards the *Vertex*, but somewhat declining to the South;

among;

among these, one which arose about N. West, and in three or four second Minutes pass'd over 50 or 60 degrees of a great Circle, was above all others that had preceded, the most splendid: Its sides were inclin'd to each other with an Angle of about 8 or 10 Degrees, and were ting'd with a brisk lively Red, which by degrees, as it approach'd the *Axis*, became more intense and dirty: On the other hand, receding from the *Axis*, its Colour was a pale Yellow, that soon lost it self in a faint Whiteness.

From this time no Moment pass'd without such Variety of different *Phases*, that it was impossible for the Eye of any single Person to pursue it thro' the suddenness of its Alteration. While some of the lucid Beams seem'd to stand fix'd, as it were, among the Stars, others mov'd slowly from East to West, by which they seem'd to meet each other, sometimes to recede from each other, and sometimes by a kind of apposition, great ones were produc'd from others of an inferior order.

The lucid *Area*, which I first discover'd in the N. East, had now form'd it self into a *Parallelogram*, whose upper and lower Edges were 5 or 6 Degrees distant from each other, and nearly parallel to the *Horizon*: In this, as if behind a Curtain, vast Waves of Light, whose Extremities did not reach the *Periphery* of the dark *Basis*, seem'd to meet and pervade each other; at other times, while some of 'em, with a remarkable Velocity, mov'd Eastward; others, as if behind them, would fly towards the West; by which variety of different Motions, as often as any Interval pass'd between the Collision of these erect Waves, a beautiful Undulation was produc'd, and its Pulses, by the adjoining Parts of the Fluid, were propagated to a vast distance.

While

While we stood amaz'd at this surprizing Sight, the *Axis* of the colour'd *Pyramid*, which arose in the N. West, had mov'd considerably toward the West, and at 7h. 25m. was about 23 or 25 Degrees to the North of *Venus*. The dark *Basis* of this Meteor had now extended almost to the East Point of the *Horizon*, and at half an Hour past seven, between E. N. E. and E. by N. several large Columns ascended in an instant to the *Zenith*; the most Eastward whereof was remarkably convex toward the South, and ting'd with a pale Red, as were most of those which ascended with it. They were met by others, that arose at the same time between the North and West, and in the *Zenith* form'd a vast Collection of Vapour, that pretty much resembl'd Smoke inlightned by the Sun's Beams; its Waves reflected a brisk, lively red Colour, and in some places a pale Yellow; they roll'd indifferently any way; and in little more than a Minute, when the first Efforts of their Congress were spent, and all seem'd fix'd and serene, the *Corona* projected several small Rays, which with a slow uniform Velocity descended between W. by N. and N. W. soon after which it died away.

We had not much time to lament the Absence of our *Spectrum*, for at 7 h. 40 m. several other *Striæ* were discharged from behind the dark *Basis*, which intersecting with others, that at the same time arose about the East and West Points, form'd in the *Zenith*; or rather 6 or 8 Degrees to the South thereof, a second much more elegant and surprizing than the former, and indeed than any thing that had yet appeared: it was not only ting'd with different Orders of red and yellow, but also with blue and violet, the last of which, by a Mixture with the white Light, appear'd.

appear'd faint and inclin'd to Purple. Tho' the Vapour, of which this and the preceding *Corona* were form'd, was so exceeding thin and pure, that several of the fix'd Stars were visible thro' it, yet it reflected a Light so copious, that I could thereby perfectly distinguish the time of night by a small Gold Watch. While thus delighted, our *Phænomenon* ejected four or five large Columns toward the N. W. (besides others toward the South) which appear'd pointed at the Top, and their Sides inclin'd to each other with an Angle of 5 or 6 Degrees. When their Bases were extended about 30 or 35 Degrees from the *Vertex*, the lower Parts of two or three of 'em broke, as it were, by the meer Weight of the Vapour, separated from the upper, and descended with a slow Motion, in the Form of truncate Cones: they were gradually follow'd by their upper Parts, and in about a Minute were lost in a large Body of Light that was settl'd between the N. W. by N. and W. N. W. The *Corona*, as if exhausted by these great Discharges, became immediately more dilute and languid, its lively Colours faded, and were succeeded by a whitish vibrating Light, that in less than two Minutes intirely disappear'd.

The dark Vapour, which continu'd to possess the Polar Regions, had now extended it self from the East to the N. W. by N. point of the *Horizon*, and was form'd in a large Segment of a Circle, whose Center was about 20 Degrees below the *Horizon*; its upper Edge was ting'd with a pale Red, which was soon lost in a florid Yellow, and this again, as it approach'd the *Zenith*, became more effoete and languid. In this dark Segment several lucid *Areas* frequently discover'd themselves, with a vibrating Light, which instantly disappear'd, as if a Curtain were drawn over them; and

and from it Rays of very different Magnitudes continu'd to ascend without any Uniformity as to time and place, till 48 or 49 minutes past seven, when a third *Corona*, very little, if at all, inferior to the preceding ones, either in the Variety of its Colours, or in the quantity of Light it emitted, was form'd in the *Zenith*. As the preceding were both produc'd by the Northern *Striæ*, so this was augmented by two or three large ones, that arose due South, out of the pure Sky, and were, in all probability, part of the Vapour, which had been projected beyond the *Zenith*, or which had subsided from the two former: they caus'd the Vapour, of which this Image was compos'd, to move with great Violence, in different Directions, not unlike Waves of Smoke, confin'd in a reverberating Furnace; this Motion being abated, the Vapour acquir'd a kind of Stagnation, in which State it continu'd but a very short time, before it projected several lucid Beams, (an inevitable Fore-runner of its approaching Dissolution,) between the North and West, and soon after, pardon the levity of the Expression, *Nocti se immiscuit atra*.

About this time, the great Beam, which arose in the N. W. and had preserv'd its Colours in their original Beauty, for more than three quarters of an hour, began to fade, and at 7 h. 53 m. was absorb'd in a vast body of Light, which seem'd fix'd in that part of the *Horizon*: it had mov'd in that time 15 or 20 degrees to the Westward of the Place from whence it arose. The *Impetus* of the Vapour being now pretty much abated, we had nothing extraordinary but successive Discharges of pointed Rays between the N. West, and E. N. East; without any Order

or Uniformity as to time or place ; setting aside these, there was very little difference in the general face of affairs for 20 minutes ; neither had we much reason to hope for any, because the severity of the Cold was such, that it oblig'd us to remove to a better Climate, and by that means we unfortunately lost the ensuing *Phases* of our dying *Meteor*.

Dublin, June
28th. 1721.

I am, &c.
J. W.

VI. *A Description of an Aurora Borealis, seen on the same Day at Cruwys-Morehard, in Devonshire. By Samuel Cruwys, Esq; F.R.S.*

Monday the 6th of Feb. 1720-21, a little before 7 in the Evening, there arose out of the North, or a little towards the East, a bright *Crepusculum*, which soon spread it self a great way through the Northern part of the Hemisphere. About 7 (when I first saw it) it began to leave behind it, at due North, or a few Degrees to the East, part of a very clear Sky (which look'd like a black Cloud, but the Stars shone in it clearly) being a Segment of a Circle, into which Figure, the *Crepusculum* (or expanded Body of lucid Vapour) had now formed its upper Limb also, making a kind of broad *Iris*, terminated at each end by the Horizon.

All this while the streaming Lights appear'd in great Variety as to Figure, Place, Magnitude and Colour, but for the most part of a redder Colour (especially towards the West) than the *Crepusculum* itself, out of which they seem'd to be formed, and tho' for the most part the greatest Appearances had been within 20 Degrees of the North on each side, yet at due N. W. there were very many considerable ones.

About 8, this *Crepusculum*, (which had been constantly, though slowly, carried further from the North) had, with the upper part of its outer Limb, reached to about 10 or 12 Degrees beyond the Pole-Star towards the Zenith, being now above 30 deg. broad, with a circular Segment of black clear Sky to the North, of about 25 deg. when the whole *Crepusculum* or Vapour was all suddenly formed into aggregate Bodies like Vapours, and gave one of the most pleasing Appearances, that perhaps has been seen of this kind. The *Bases* of the Cones seem'd to rest on the upper Limb of the Segment of clear Sky (which was extended near 60 deg. on each side the N.) and the Vertices of the Cones, pointing all towards the Zenith, approached within a few Degrees of it, and terminating there, formed the greatest part of a Semicircle inclosed, as it were, with Golden Pallisadoes, which shined all at once as bright almost as Flame, and being of a prodigious Length and Number, exhibited a most agreeable Spectacle.

This last *Phænomenon* convinced us, that these Cones were Collections of the very same Particles, whereof the *Crepusculum* had consisted: because when it appeared every where alike and equal, the great Stars shined through it but very faintly; where-

as afterwards, those Stars, that remained between the Cones, suddenly appeared very bright, whilst those, that were covered by them, could hardly be perceived: and indeed all the streaming Lights this Evening seem'd to flow from this *Crepusculum* downwards, as from a Fountain or Store, and not to arise from the Horizon, few approaching it nearer than 10 Degrees, and many not within 20 or 30 deg.

After this fine Appearance had continued about 2 Minutes, the Matter seem'd to be exhausted, and the Scene almost at an end, the streaming shining Lights being mostly extinguished, and the remaining Parts of Vapour left, like broken Clouds; when the flashing Lights began to appear of a most prodigious Swiftnes, both from N. E. and N. W. pointing to the Zenith, or a little more to the South. 'Twas observable, that over the Tracts, where these flashing Lights pass'd, the remaining parts of Vapour (which now lay scattered every where, like white broken Clouds) pointed, or seem'd to have a Tendency, conformable to the same Motion; whereas, towards the due North, where no Flashes appeared, these whitish Clouds lay confused and irregular as before.

This continued about 20 or 25 Minutes, when the Wind began to arise a little at N. E. and the Scene was quite at an end, dark Clouds succeeding all over the North, and by nine a Clock we had a severe Storm of Snow.

N. B. That all the time of these Appearances, many broken parts of the extinguished Vapours, like white Clouds, were carried beyond the Zenith, some 50 or 60 deg. and others, even to the Horizon it self, at S. S. W.

During

During the whole Continuance, there seemed to be a small, easy breath of Wind, scarce perceivable, at N. N. E. which the Motion of the Clouds above-mentioned also confirms, but as soon as it began to blow a little brisker, the remaining parts of the Vapour were all dissipated.

*An Account of the Weather both before and after this
Phænomenon.*

- January 30. **H**ARD Snow in the Morning, and Frost all Night.
- Jan. 31. and
February 1, 2, 3 4. Pleasant Sun-shine Days; very calm, but the Snow still lying, and at Nights very hard Frosts.
5. Very violent Snow in the Morning, and some Thaw in the Afternoon, hard Frost at Night.
6. Hard Frost in the Morning, and the Wind exceeding cold and sharp, but not hard; the afternoon pleasant, Sun-shine and calm, but it froze all Day out of the Sun, and continued to do so all the Evening, and the Ground was still covered with Snow.
7. Very hard Frost in the Morning, and Frost and Storms of Snow all Day. Wind N. N. W.
- 8, 9, 10, 11. Pleasant calm Days, but hard Frost, and very hard Frost at Night.
12. Hard Frost. Exceeding cold Wind at S. E.

The

The 12th at Night these Lights are said to have appeared again, as also on *Saturday* the 18th, to a very great degree, but I saw them not; the Weather still frosty with little Snow.

Feb. 22. At
Night.
23, 27.

Hard Snow.

A Thaw and some Rain, and but little Frost afterwards, only dry cold Winds, till the 27th, when the Frost returned very sharp, with exceeding cold Winds, at N. E. and S. E. for a Fortnight or more, without any Snow, and did a great deal of hurt.

VII. *Observations on the Muscular Fibres of Fish.*
By Mr. Leeuwenhoeck, F. R. S.

Delft, April 11. 1721.

IN my Letter to this Honourable Society of the 24th of *January* last, I treated of the great Number of Vessels that I had observed in the Fibres of the Muscles. I now take the liberty of presenting you with the following Observations, hoping there may be something in them for your Entertainment.

It has been affirmed to me by several Persons, and in particular, by a certain very learned Foreigner, that Nature, in all her various Productions, constantly observes the same Course and Manner of Operation. To this Assertion my Observations do by no means agree; neither those, that I have made upon the Generation

neration of Animals, and the Seeds of Plants; nor yet those that I have made upon the Muscles and Muscular Fibres of different Animals, forasmuch as the Muscles of Fishes are not provided with any Tendons. On this occasion, I have lately examin'd anew the Muscles of the Cod-fish, upon which I have made the following Observations.

After the late Discoveries I had made of the small Vessels in the Muscular Fibres of the Whale, the Ox, the Sheep, and the Mouse, I was apt to imagine, that the same Fabrick would hold in the Muscular Fibres of Fish likewise: but as this could not be certainly concluded, having at that time in my house a part of a fresh Cod, I cut off a piece of the Fish, with Intent to examine it with my Microscope some Days afterward.

This piece of Fish I cut into small Slices, some according to the length of the Fibres, and others directly across them, in order to observe, whether these Muscular Fibres were composed of great Numbers of small Vessels running according to the length of the Fibre. And in effect I found, that, when I had cut the Fibres dexterously through, there appear'd in the Microscope as great a number of small Vessels running along these Fibres, as I had formerly seen in the Muscular Fibres of a Whale.

But what appear'd to me the most remarkable, was, that in a great number of Fibres, in which I was not able to discover any Vessels running according to their length, I observed abundance of small Vessels, which seem'd to me to proceed from the Membranes encompassing the Fibres. For in one Fibre these Vessels appear'd to come out of the Circumference, or circular Tunicle of the Fibre, and to pass on to the opposite

opposite part of the Tunicle. And in another Fibre cut transversly, I saw Vessels arising from the Circumference, and dividing themselves into smaller Branches about the middle of the Fibre; all which, as far as I could perceive, ended again in the Circumference of the Fibre. In one Fibre I saw at least fifty of these Vessels running through one another.

Upon this Discovery, I found I had been mistaken in what I had at first imagin'd, which was, that the Vessels, which arose from the Membranes, proceeded no farther, than just through the Tunicle of the Fibre, and so discharged the Fluid into the Fibre for its Nourishment. Whereas, now I perceiv'd, that the Vessels, which arose from the Membrane, and entred into the Fibre, did not end there, but spread themselves into smaller Branches proceeding every way from the inside to the Tunicle of the Fibre. This caus'd me to think, that the nutritious Juice might circulate in these small Vessels, just as the Blood does in the Veins and Arteries; and that what the Muscular Fibres received from them, might be no more than what ouzed thro' the Tunicles of these small Vessels, as I have said of the small Vessels in Land-Animals, which have no other end than the Artery coming from the Heart, and the Vein terminating in the Heart; the Artery and Vein thus making one continued Vessel.

Having now a great number of Fibres lying before me, in which I could see very plainly the Vessels just now treated of, yet I could not discern in the transverse Sections of the Fibres any appearance of those Vessels, which run along their length, and compose the greatest Part of the Body of each Fibre. This I imputed to the cutting of those Vessels not directly

rectly across, but something obliquely, by which their Apertures had been clos'd in such a manner, that I could not perceive them, nor the least Resemblance of them.

I have several times observ'd, between the Muscular Fibres of the Fish, a great number of Vessels lying together, which compos'd what is commonly call'd a Membrane, which Vessels surrounded the Muscular Fibres, and lay so many of them together, that the thickness of the whole *Fasciculus* of Vessels was equal to that of a Muscular Fibre, and, as I imagin'd, was afterwards to be dispers'd in smaller Ramifications between the Fibres.

In taking a View of an entire Muscle of a Codfish, and the Fibres of which it was compos'd, I found the thick end of the Muscle to equal the Back of an ordinary Knife, and the thinner end not to exceed the thickness of a single Fibre. Many of these Fibres are twice as long as the thickness of the Muscle, and between the Muscles lie what are commonly call'd Membranes, which are nothing else but a *Congeries* of Vessels. These Vessels do not only run between the Fibres, but into the very Substance of every Fibre, as we see, when the Fibres are cut transversly. By these Vessels the Muscular Fibres, and the entire Muscles themselves are so firmly bound together, that they serve instead of Tendons to one another.

In like manner the Muscular Fibres are united to the Bones, by the Vessels proceeding from the Bones, which Vessels compose what in Land-Animals is called the *Periosteum*.

In order to give a clear notion of what I mean, when I speak of the Muscles of a Cod-fish, I have here caus'd two of those Muscles to be delineated, lying close together, as they are united to one another, and separated from the other Muscles, as is represented by *A, B, C, D*, Fig. 1. the Part design'd by *A, B, C*, having been cover'd with the Skin near the Head of the Fish. And it is my Opinion, that the Body of the Cod-fish, from Head to Tail, consists of a continued *Series* of such Muscles.

I have likewise caus'd a single Muscle of the Fish to be represented by *E, F, G, H*, Fig. 2. where *E, H, G*, shews the Thickness of the Muscle; and its thin Edge, which is no thicker than the Edge of a Knife, is marked by *E, F, G*.

When these Muscles had lain several days upon a Paper, yet they were not dry'd so hard, but that I could split them into thin Shivers, one of which is design'd in Fig. 3. between the Letters *I* and *K*, in order to shew the oblique Course of the Fibres, which are represented by small Lines.

I now turn'd my Thoughts to the River-Fish, and particularly to the Pearch; and, as I imagin'd that an old Pearch had no greater number of Muscular Fibres than a young one, but only that the Fibres increas'd in bigness during the Growth of the Fish, and that the larger these Fibres were, the more plain and distinct must be the small Vessels, of which the Fibres were composed; I sent orders, upon a Market-day, to bring me the largest Pearch that was to be found in the Market; and accordingly, I had one brought me the largest that I had ever seen, weighing three pounds and an half, and seventeen Inches
and

and an half in length *Delft* Measure, which is the same with the *Rhinland*.

I cut off four pieces from this Fish, as two from the Back near the Head, and two others from the Belly in the thick part of the Fish, with design to make my Observations upon them the next Day.

Accordingly, at that time, I took a view of the Muscular Fibres both in length and breadth, and found that the Fibres of this great Pearch were not so thick as those of the Cod-fish. Upon cutting them thro' lengthwise, I saw the Apertures of the small Vessels in so great a number, as I could hardly have believ'd, if I had not seen them. I next cut some of the Fibres transversly, and plainly found them thinner in this Pearch, than in a middling Cod-fish, and saw the small Vessels, that compose the greatest part of the Bulk of the Fibre, lying as close together, as ever I saw them in any kind of Fish or Flesh.

To give a better notion of these Muscular Fibres, and of the great number of small Vessels, of which they are chiefly compos'd; I had, some Weeks before, placed some of them cut transversly in pieces before a Microscope, with design to have them drawn by my Painter, but had been obliged to defer it, by reason of the great Severity of the Weather. These had been a little moistned before they were placed upon the Glass, in order to make them stick to it the better; and I have caused a small portion of these Muscular Fibres of the Fish, cut thro' transversly, after they were grown dry, and in their shrinking had been torn off from the small Vessels, that encompass them, to be represented, as at *L, M, N, O, Fig. 4*: The openings of the small Vessels in these Fibres were distinctly to be seen, but appeared in such great numbers, and were

so exceedingly small, that it was impossible for the Painter to represent them any otherwise than by Points.

In this Figure are represented what we call the Membranes, but which indeed are nothing else but a *Congeries* of small Vessels, which not only surround the Fibres, but enter into their very Substance. These, in the drying and shrinking of the Object upon the Plate, had been torn off from the Fibres, as may be seen at *P, P, P.*

When this was done, I put a small Drop of Water, about the size of a Pin's head, on this small Portion of Fibres, into which it immediately insinuated, and swell'd them to the same bigness, as when they were first laid upon the Plate: After which, I desir'd the Painter to draw them, as they then appear'd to him, but to omit representing the small Vessels, and only to design the Circumference of every Fibre, which he did, as appears at *Q, R, S, T, Fig. 5.*

I then clove a grain of Millet thro' the middle, and placing one half of it upon the Glass, beside the portion of Fibres represented in Fig. 4. I desir'd the Painter to observe the difference in bigness between the half grain of Millet-feed, and that portion of Fibres; who told me, that the half grain appear'd larger than the portion of Fibres, and so said likewise a second Person that view'd them. By which one may easily imagine, in how small a space that number of Fibres is comprehended, each of which consists of so many Vessels. I caus'd the Painter to represent the half grain of Millet at Fig. 6.

I likewise made my Observations upon the Muscular Fibres of a Pike, a Roach, Schar, and Flounder,
in

in each of which I found the Fibres to be composed of small Vessels, like those of a Cod and Pearch.

I had now a mind to examine the Muscular Fibres of the Smelt, to see whether they were likewise composed of Vessels, but not being able at that time to meet with any Smelts, it came into my Head to observe the same in dried Sprats. Accordingly I took the largest of these I could light on, which was a little more than five Inches in length, and I found that the Fibres of the Sprat were but little thinner than those of the large Pearch spoken of before, and that the Vessels of which the Fibres were composed, were nearly as numerous as in the Fibres of the Pearch.

From these Observations some Persons may be apt to conclude, that the Muscular Fibres of Land-Animals are of the same thickness with those of Fish. But for the satisfaction of those, who have not seen the Objects here spoken of, I have caused a small portion of the Muscular Fibres of a large Ox to be delineated, as they appear'd through the same Microscope with the former, to shew the thickness of the dried Fibres, and the Vessels that compose them, as is represented in *Fig. 7.* by *XY Z.*

I desir'd the Painter to tell me, how many Vessels he could see in the transverse Section of one of these Fibres; who, after some pause, reply'd, That he counted five and twenty Vessels in one Fibre.

Some time after this, I had a small Smelt brought me, of the length of about two Joints of my Finger; and cutting some of its Muscular Fibres transversly, I placed them before a Microscope, and saw not only that these Fibres were twice as thick as those of an Ox, but likewise that they were provided with as great a number of Vessels as the Fibres of other Fish.

Upon

Upon thus observing that the muscular Fibres of Fishes were much larger than those of Beasts, I set my self to consider, for what Reasons the great Creator of the Universe had made this Disproportion between them. All the Satisfaction I could meet with, in my Thoughts upon this Subject was that, as the Fish swim in the Water, their muscular Fibres need to exert very little force, in order to support their Bodies in the Water, because they are very nearly of the same specifick Gravity with the Element, in which they swim. All the force they exert is in their progressive Motion, in pursuit of their Food. Whereas the muscular Fibres of Land-Animals exercise a great force, not only in supporting and moving their own Bodies, but in carrying Burthens and other Labour they are put to. And we must allow, that the smaller and finer the Fibres are, to make a Body of any determinate thickness, the stronger will be the Composition, and therefore the Muscles in Flesh must be stronger than those of Fish. But this I leave to better Judgments.

It justly claims our Admiration, to see the wonderful and amazing Structure of these minute Parts in the Muscles of Beasts and Fish, which never enter'd into our Thoughts; and no doubt there are many other surprizing things there inclosed, which will perhaps for ever escape our Penetration.

I am, &c.

P. S.

P. S.

I Happen'd yesterday to take up a boil'd Grey Pea, out of which I took a little of the mealy Substance, and laid it before a Microscope, where it appear'd to consist of such like Parts as are found in Rats Dung, every one of which Parts consisted of a great number of very small Particles. But I could not discover any Membranes enveloping those Parts, from whence I concluded, that those Membranes were destroy'd and dissolv'd by the hot Water.

Upon this, I took another Grey Pea, which had not been boil'd, and cut it into very thin Slices, when I not only saw the Membranes, in which the Parts of the mealy Substance had been inclosed, but found likewise, that those Membranes consisted of nothing else but a great number of very small Vessels, like the Membranes, as they are commonly call'd, which surround the Muscles and muscular Fibres in Beasts and Fish.

I intend to prosecute this Subject farther for my own Diversion.

VIII. *Observations upon the Seeds of Plants.* By
the same. Translated by John Chamberlayne,
Esq;

Delft in Holland, 27. June, 1721.

I Have often turn'd my Thoughts to the observing the so called Membranes, in which the Substance of Meal, or Flower, is inclosed, like little Packets in Cells or Boxes, which is also the case of all kinds of Beans, Pease, Wheat, Barley and other Grain. I at length, with astonishment, discover'd very plainly, that what I call the Membranes, were endued with an unspeakable number of little Holes, thro' which, in many places, one might perceive the Light; which Holes we must suppose to be nothing else but little Vessels, which had been torn or cut off, and which do partly compose the Membranes, which I call Little Cells, and which partly serve for the Production of the *Farina*, of which there are an infinite number of Particles in a Pea or Bean; which, as small as they are, I imagine that each of those mealy Particles receives its increase from a little Vessel, which proceeds from the foresaid Cell, and that those Vessels are imperceptible through their Smallness.

These Vessels, of which the little Cells, or Cases, do mostly consist, are more easy to be discover'd in Beans and Pease, than in any sort of Legumens or Grains; but in Wheat the Vessels are difficultly traced

ced in the Cells, and I have been obliged to make very many Observations and Experiments, before I could fully satisfy my self, that I saw the torn or broken Vessels; the Reason of which is, that the little Vessels, of which the Cells or Skins of the Grains of Wheat are composed, are exceeding thin and brittle.

Moreover, I have found, upon observing the Vessels, of which the Cells are composed, that several of the Globules in Wheat were broken in pieces in the Operation, and that in one of those single Globules, there were other small Globules enclosed.

I have likewise observed that the Membranes, or little Cells, in *Barley*, in which the Globules, or Parcels of the Meal are shut up, and receive their increase, are thicker and stronger than those of *Wheat*.

Although I conclude, that almost all Seeds and Grains, as well as their Membranes, or Skins, are of one and the same Texture and Configuration, yet for Experiment sake, I took a large *Almond*, and cut off several thin Slices from it, and dug out of those Slices, as well as I could, the Substance that lay in the little Cells, and viewing them, as nicely as possible, with a Microscope, I observed that those Cells, in which the Oyl of the said *Almond* was for the most part contain'd, consisted also of nothing but little Vessels.

My Intentions were indeed, if it had been practicable, to view the smaller sorts of Seeds, in order to find out, whether the little Cells, in which the farinaceous matter might lie, were likewise composed of small Vessels: but I changed my mind, believing

that my Endeavours therein would be ineffectual, By reason of the Smallness of those little Cells, and consequently of the Vessels, of which those Cells were made up; though I do not question but what we discover in the greater Seeds, is analogous to the Structure of the small ones.

Now when I perceiv'd; that the before-mention'd little Cells proceeded from the Bark, or Skin, which surrounds the Kernel of the Seed, or Grain, I was thinking that, as the mealy Substance receives its increase from the Vessels, which are in the little Cells, and as the Plant is formed between the Cells, during the time that the Seed lies in the Earth, and as the little Orifices in the Skin of Animals and Fruits, are formed in order to discharge thereby the Superfluity of their Moisture, and do shut in such a manner, that no Moisture, nor common Air, can get into the same, as I have formerly advanced: So on the contrary, the Orifices of Seeds are so form'd, that many of their little Vessels do admit Moisture to pass inwards, and accordingly Water is driven into them by the pressure of the Air, and causes the Seed to swell; upon which, a Warmth and Fermentation succeeding in the Seed, it requires a greater space, and by the particular Formation of the Particles, which lie in the Cells, and which have derived their Increase from the Cells, the mealy Substance, of which they consist, is partly driven out of them into the body of the young Plant, which by this means increases so much in bulk, that the Root is now able to supply it with Nourishment from the Earth, at which time the Seed is found to be diminished in its bigness.

As often as I have view'd Seeds, for several Years past, with the Microscope, yet I never imagin'd, that the little Cells were endued with so many Vessels, tho' I have often been considering, how the Intrusion of the Particles of the Meal, or Flower, into the Membranes was effected; nor should I ever have attained thereto, but by continual Labour in the investigation of things, which are concealed from our naked Eyes, and towards which I have a much greater inclination, than what I observe in most other Men.

Ff 2

IX. *Extra*

IX. *Extract of a Letter from Dr. Brook Taylor, F. R. S. to Sir Hans Sloan, dated 25. June, 1714. Giving an Account of some Experiments relating to Magnetism.*

AFTER having given an Account of an Experiment made with the large Magnet in the Repository of the *Royal Society*, (which Experiment is described in *Philosoph. Transact.* No. 344. Article 4.) The Letter goes on with the same Subject as follows. -- If it were known what point within the Stone, and what point in the Needle are the Centers of the Magnetical power, it would be easy to find the true powers of the Magnet at all the distances observed. For want of that Knowledge, I have computed the Forces from the Center of the Needle, and the Extremity of the Loadstone, and find, that at the distance of nine Feet, the Power alters faster, than as the Cubes of the distances, whereas at the distances of one and two Feet, the Power alters nearly as their Squares. To try whether the Law, by which the Magnetism alters, could be reduced at all distances to any one certain power of those distances, I sought those points in the Needle and Stone, which being used as the Centers of the power, might have that property. But in that case, I found the Center of the Stone must be carried quite out of its Figure, to make the distances large enough for this purpose. From whence it seems to appear, that the power of
Magnetism

Magnetism does not alter according to any particular power of the distances, but decreases much faster in the greater distances, than it does in the near ones.

This seems to be confirmed by other Experiments I made. The first Experiment was thus; I made a Needle $\frac{3}{4}$ of an Inch long, of very fine Steel-wire (a Foot length of which weigh'd but a Grain) which I lengthen'd by sticking a light piece of Rush to it, so that I could observe the Direction of the Needle in all the trials with a *Radius* of two Inches. Instead of a Magnet I used a touch'd Needle of Steel-wire, which I set on a perpendicular to the Horizontal Plane I made the Observations on, by means of a Frame I made to transport it from one place to another; the North end of the Needle being placed downwards, and made a little sharp, that it might mark the Paper it was set upon in every position, by pressing the top of the Needle gently with the Finger. The Observations were made in this manner; after having taken notice of the natural direction of the small Compass Needle, I brought the perpendicular Needle as near to it as I conveniently could, setting it in such a manner, that a Line from the upright Needle to the Center of the Compass might be perpendicular to the Compass Needle. Then observing the same caution (which was convenient to make the Center of the Compass serve sufficiently well to be esteem'd its Center of Power) I placed the upright Needle at several greater distances, every time marking the place in the manner already described, and observing the Variation of the Compass. By this means I got a Curve pretty regularly and fairly drawn by points on the paper. And by examining this

Curve

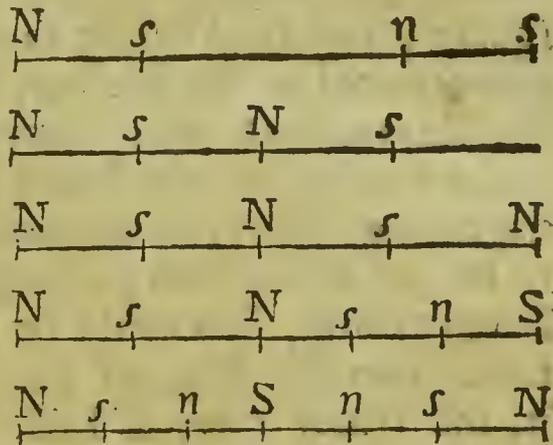
Curve, compared with the Variations of the Compass corresponding to its respective points, I found that the Magnetical power decreased faster at the greater distances than at the nearer. It is of little use to be very particular in the account of the several Observations. I shall only take notice, that at about two Inches and a quarter distance, the Force did not alter so fast as the Squares, and at ten Inches distance (where the Variation was one degree only) it alter'd faster than the Cubes, the Index of the Power being about $3\frac{1}{4}$. The Needle of the Compass was so short, that to suppose its Center of Force to be either in the middle or at the extremity of it, would not alter the Index of the Powers of the distances $\frac{1}{4}$ of an Unite.

I made another Experiment to the same purpose, with a Compass Needle made of a slight piece of Straw, with a small piece of Steel-wire fasten'd to one end of it, which was always kept in the same position, being balanced between two perpendicular Needles, one of which was moveable, and the other fix'd. The Event was much the same as in the former Experiment.

Endeavouring to find the true Poles, or Centers of the magnetical Power in touch'd Needles, I made a Needle of two Inches long, of the fine Steel-wire, which I touch'd with the South point of a small cap Loadstone, applying the point of the Cap only to the Extremity of the Needle, without drawing it along. The Needle so touch'd, being laid gently on the Surface of a stagnant Water, floated. I then applied to it successively the two ends of a touch'd Needle,

Needle, as near as I could, without letting the Needles touch. The result was, that the floating Needle rested under the respective Poles of the other Needle mark'd

with the small Letters *s*, *n*, *s*. So that by one Touch with the Loadstone, which gave the Needle a North-pole at *N*, where it was touch'd, it acquir'd three other Poles, *s*, *n*, *s*, which we may not therefore improperly call its consequential Poles.



Having discover'd these consequential Poles, I made some other Experiments to discover more of the Nature of them, as they are describ'd in the Scheme annexed. The Needles were all of them two Inches long, made of the same fine Steel-wire, and the Letters *N*, or *n*, and *S*, or *s*, denote the Character of North or South belonging to the points mark'd; the great Letters signifying the points the Loadstone was applied to, and the small Letters shewing the consequential Poles.

There are two other Experiments described in the same Letter; relating to the Attraction of Fluids, one of which (*viz.* that of the *Hyperbola*, made by the Surface of the Water between two Glafs-planes) being already described in these *Transactions* (No. 336.) we shall only transcribe the Account given of the other.

I took several very thin pieces of Fir-board, and having hung them successively in a convenient manner to a nice pair of Scales, I tried what Weight

was

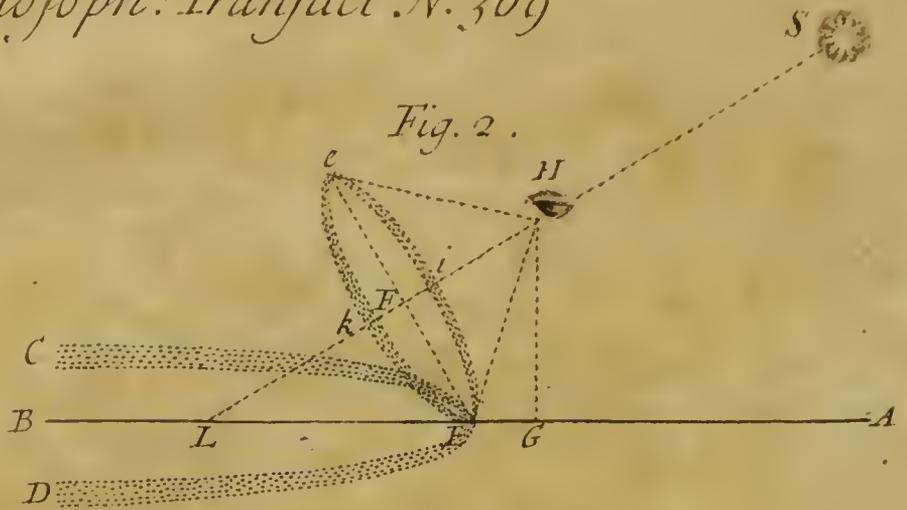
was necessary, (over and above their own, after they had been well soak'd in Water) to separate them at once from the Surface of stagnating Water. I found 50 Grains to separate a Surface of one Inch square; and the Weight in every trial being exactly proportional to the Surface, I was encourag'd to think the Experiment well made. The distance of the under Surface of the Board from the Surface of the stagnating Water, at the time they separated, I found to be $\frac{1}{16}$ of an Inch; though I believe it would be found greater, if it could be measured at a greater distance from the Edge of the Board, than I could do it, the Water rising a little before it came quite under the Edge of the Board.

E I N I S.

L O N D O N:

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Western
Parhelion

Fig. 5.

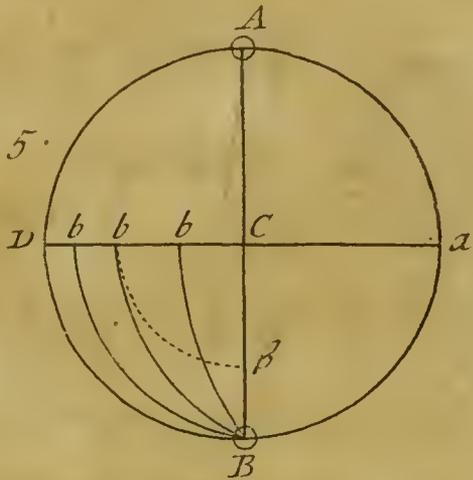
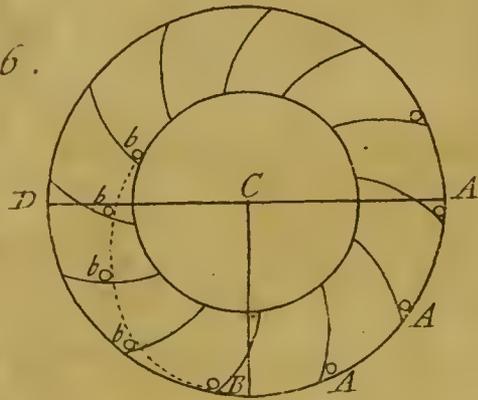


Fig. 6.



117

PHILOSOPHICAL TRANSACTIONS.

For the Months of September, October, November,
and December, 1721.

The CONTENTS.

- I. *On the Method of determining the Places of the Planets by observing their near Appulses to the Fixed Stars.* By Edmund Halley, L. L. D. *Astron. Reg. & Reg. Soc. Soc.*
- II. *Observation of a Parhelion, Oct. 26th, 1721.* By the same.
- III. *An Account of two Mock-Suns, and an Arc of a Rainbow inverted, with an Halo, and its brightest Arc, seen on Sunday and Monday, Octob. 22, & 23. 1721. at Lyndon, Comitatus Rutland; communicated by the Rev. Mr. William Whiston, M. A. sometime Professor of the Mathematicks in the University of Cambridge.*
- IV. *Observations upon the Generation of Plants, in a Letter to Sir Hans Sloane, Bar. Pr. Coll. Med.* By Patrick Blair, M. D. F. R. S.

V. Ob

- V. *Observation of an extraordinary Height of the Barometer, December 21. 1721. By Mr. George Graham, Watchmaker, F. R. S.*
- VI. *A Caution to be used in examining the Specifick Gravity of Solids, by weighing them in Water. By James Jurin, M. D. R. S. Secr.*
- VII. *A Letter from Mr. Edward Naisb, Surgeon in York, to Claudius Amyand, Esq; Serjeant-Surgeon to his Majesty, -and F. R. S. Concerning an Ossification of the Crural Artery.*
- VIII. *An Account of a Rainbow seen on the Ground. Communicated in a Letter from the Reverend Benjamin Langwith, D. D. Rector of Petworth, to Dr. Jurin, Secr. R. S.*
- IX. *Epistola Domini Antonij van Leeuenhoek, R. S. S. de Osculis, sive Spiraculis Foliorum Buxi, item de lanugine Malorum Persicorum & Cydoniorum.*
- X. *Remarks on some Attempts made towards a perpetual Motion, by the Reverend Dr. Desaguliers, F. R. S.*
- XI. *A Method for rowing Men of War in a Calm. Communicated by Monsieur Du Quet.*
- XII. *Part of a Letter from the Reverend Mr. Rowlands, to the Reverend Mr. Derham, Prebendary of Windsor, and F. R. S. Concerning the stocking of the River Mene with Oysters.*

1. *On the Method of determining the Places of the Planets by observing their near Appulses to the Fixed Stars.* By Edmund Halley, LL. D. *Astron. Reg. & Reg. Soc. Soc.*

OF all the Cælestial Observations that have hitherto been made, none are so capable of perfect Exactness, as the near Appulses of the Moon and Planets to the Fixed Stars, such as lately we had of *Jupiter* to two small Stars in *Gemini*, and of *Mars* to the Forehead of the *Scorpion*; for though the Places of the Stars have not as yet attained an ultimate Precision, yet these Sorts of Observations are ever good, and the Places of the Planets are thereby ascertain'd, in Proportion to the Correctness of the Catalogues that may hereafter be made: But the ordinary Number of the Stars, with which the Planets may be thus compared, being small, the Opportunities of observing are consequently rare: Whence appears the great Use of a full Catalogue of Telescopical Stars, at least within the Limits of the Zodiack; *viz.* that thereby these Opportunities may be more frequent: And wherever such Observations have formerly been made to these small Stars, we may be enabled to find them out, and by determining their Places, to be certain of the Places of the Planets also: Of which I have given a notable Instance in finding the Place of the great Comet of 1680, in its first Appearance, even before it had a Tail visible to the naked

Eye, of which an Account is given in *Numb.* 342. of the *Transactions*. And since the *Royal Observatory* at *Greenwich* has been put under my Care, I have endeavoured to put myself into a Condition to supply the many and great Vacancies to be met with in the present Zodiack; and particularly I have sought out and settled the Places of two Telescopic Stars, to one of which, *Jupiter* was observed to apply by *Galileo* at the Beginning of *March* 1610, *New Stile*, and which is the very first Observation of that kind that was made with the Telescope. (a) On the 28th of *February*, one Hour after Sun-set, a small fix'd Star was in Conjunction with the fourth Satellite, (as it since appears to have been) being then Eastwards of the Planet. The next Day, *Mart.* 1°. at the same Hour, the Center of 4 was in the Angle of an equilateral Triangle with the fourth Satellite and the Star: And again, *March* 2°. *Jupiter* being retrograde, had past the Conjunction of the Star, and a Line from the Star, perpendicular to that of the Satellites, fell on the first Satellite then two Minutes to the West of the Planet, and in Latitude the Star was more Southerly than the Satellite eight Minutes. This Star, by the Direction of the Place of *Jupiter* at that Time, I found out, and, by comparing it with others in the Catalogue, having nearly the same Declination, I settled its Place in π $13^{\circ} 24' \frac{1}{2}$ to the Time of the *British* Catalogue with $0^{\circ} 25'$ South Latitude.

Another remarkable Observation of *Saturn* is recorded in *Riccioli* (b), said to have been made at *Modena* by the Marquis *Malvazzo*, on *July* 3° N. S. 1662, when the Eastern *Ansa* of *Saturn* touched a fix'd Star. By the then Place of *Saturn*, I look'd out for this Star, to which

(a) *Nuncius Syder*, pag. 27. Edit. prin. 1610.

(b) *Astron. Reform.* pag. 286.

which *Saturn* is at this Time very near, and after the same Method I settled its Place, *ineunte Anno* 1690, (the Epoch of the *British Catalogue*) in $29^{\circ} 34'$ of *Scorpio* with $2^{\circ} 0' \frac{1}{2}$, North Latitude. By this it will appear, how defective the observed Place of *Saturn* is stated in *Riccioli*, there being above seven Minutes erred in the Latitude thereof.

II. *Observation of a Parhelion, Oct. 26th, 1721.*
By the same.

THIS Morning, 26th of *October*, being on the River coming up to *London*, about half an Hour past Ten, the Sun being then about twenty Degrees high, I observed a Circle about the Sun, which is by no means unusual, when the Air in chilly Weather, such as it is now, is replete with snowy Particles; which Circle was of the Size it always appears in, about 23 Degrees from the Sun, and faintly ting'd with the Colours of the *Iris*. When this Circle happens, I always look out, to see whether any other of the Phenomena that sometimes attend it did at that Time appear, such as *Parbelia*, and other colour'd Circles, concentrick with the Sun, and sometimes, as once I saw it, excentrick; as also a white Circle round the *Zenith*, in equal Altitude with the Sun: But this Time, the Air being thickned with a hazy Vapour, and the Smoke of the Town, I could only see to the Eastward a luminous white Patch, which for about twenty Minutes shone through the thick Air very conspicuously, of about two Degrees diameter, as near as I could estimate it, and about the same Altitude with the Sun: and from it, towards the Sun, there seemed to proceed a long

white Tail, much narrower than the Mock-Sun, but which I took to be a Segment of the white Circle which I once saw entire in *London*. Had the Air been clear, I doubt not but much more of the Phenomena of the *Parbelia* might this Time have been observed: and I hope, that from our Neighbourhood some Member of the *Society* may furnish us with a fuller Relation. But how to explain these Appearances, and account for the Magnitude of these Circles, is what seems still wanting.

III. *An Account of two Mock-Suns, and an Arc of a Rainbow inverted, with an Halo, and its brightest Arc, seen on Sunday and Monday, Octob. 22, & 23. 1721. at Lyndon, Comitatus Rutland, communicated by the Rev. Mr. William Whiston, M. A. sometime Professor of the Mathematicks in the University of Cambridge.*

ABOUT Ten o'Clock in the Morning, on *Sunday, Octob. 22. 1721.* being at the House of *Samuel Barker, Esq;* of *Lyndon* in the County of *Rutland*, after an *Aurora Borealis* the Night before (Wind W. S. W.) I saw an Attempt towards two *Mock-Suns*, as I had done sometimes formerly, of which I immediately inform'd Mr. *Barker*, though without any great Expectations of what followed. About $\frac{1}{2}$ or $\frac{3}{4}$ of an Hour after, I went to view the Heavens, and then found the Appearance compleat; and when Mr. *Barker* and others of the Family were call'd, we all saw it, and all
saw

saw indeed what we had none of us seen before; I mean two plain *Parbelia*, or *Mock-Suns*, tolerably bright and distinct; and that in the usual Places, *viz.* in the two Intersections of a strong and large Portion of an *Halo*, (*Fig. 1.*) with an imaginary Circle, parallel to the Horizon, passing through the true Sun. I call this Circle here *imaginary*, because it was not itself visible, as it sometimes has been at such Appearances. Each *Parbelion* had its Tail, of a white Colour, and in direct Opposition to the true Sun; that towards the East was 20 or 25 Degrees long; that towards the West about 10 or 12 Degrees; but both narrowest at the remote Ends. The *Mock-Suns* were evidently red towards the Sun, but pale or whitish at the opposite Sides, as was the *Halo* also. Upon casting our Eyes upward, we saw an Arc of a curious *inverted Rainbow*, about the Middle of the Distance between the Top of the *Halo* and our *Vertex*. I mean this, when Allowance is made for the usual Inequality, that appears between the same Number of Degrees, nearer to and remoter from that *Vertex*. This Arc was as distinct in its Colours as the common Rainbow; and, with the like Allowance as before, of the same Breadth. The red Colour was on the Convex, and the blue on the Concave of the Arc; which seemed to be about 90 Degrees long: Its Center in or near our *Vertex*. On the Top of the *Halo* was a kind of inverted bright Arc, though its Bend was not plain. The lower Part of the *Halo* was among the Vapours of the Horizon, and not visible. The Angles, especially as more exactly measured on *Monday*, near Noon, when the same Appearance return'd again, but more faintly, were as follows: Sun's Altitude $22^{\circ} \frac{1}{3}$; perpendicular Semidiameter of the *Halo* $23^{\circ} \frac{1}{3}$; Distance of the *Rainbow* from the Top of the *Halo* $23^{\circ} \frac{1}{3}$; Semidiameter of the Arc of
 the

the *Rainbow*, if our *Vertex* be suppos'd its Center, 21°. The *Phænomenon* lasted each Day for an Hour and an half, or two Hours. What was most remarkable on *Monday* was that the Wind, which on *Sunday* had been almost insensible, was now become sensible, and changed to N. N. E. that the *Halo* was sensibly become oval; its shorter Axis parallel to the Horizon; and the two *Mock-Suns*, which were then but just visible, especially that on the East, were not in the *Halo*, but a Degree or two without it, which I ascribe to the unusual Shortness of the Horizontal Diameter; which Position of the *Mock-Suns* does not appear to have been hitherto taken Notice of by any, though it was now very sensible.

On *Thursday* Morning, *Octob.* 26. as I was coming in the *Northampton* Coach towards *London*, about 9 o'Clock, the *Halo* returned larger and clearer than before; and the two *Mock-Suns* just attempted an Appearance therein, as on *Sunday*; but the Air becoming thicker and thicker towards Rain, I saw them no more. I add nothing to this Account, but only, that *Aug.* 30. before, I saw at the same Place *Rutland* a remarkable *Halo*, whose upper Part had its inverted Arc reddish within, and pale without, but brighter and more vivid than ever I saw in my Life: That we had there, *Sept.* 11. in the Evening, the lightest and most remarkable *Aurora Borealis*, with its unaccountable Motions and Removals, that ever I saw; excepting that original one, *March* 6, 1715: That it was seen in *Northamptonshire*, at the *Bath*, and elsewhere: That the *Vertex* of the Columns which shot upwards, was not our *Vertex*, but evidently 15 or 20 Degrees distant towards the South; and that the Wind was in *Rutland* North, as I observed myself; at the *Bath* West, as Mr. *Molyneux* observed; and, as I am inform'd by Sir *Robert Clarke*, in *Northamptonshire*

amptonsbire South ; all at the same Time, which deserves particular Reflection. But then, if any Reader expects here the *Solution* of all these *Phænomena*, he is to know, that as to these *Northern Lights*, Dr. *Halley* has communicated his Thoughts to the Publick in the *Philosophical Transactions* soon after the first Appearance ; and I communicated mine about the same Time in a small Pamphlet. And as to the *Halo's*, *Mock-Suns*, *Inverted Arcs of Rainbows*, and other *Phænomena* of the like Nature, *Monf. Huygens* has most accurately explained them in his *Posthumous Works*, from p. 293 to p. 366. and *Sir Isaac Newton* himself has touched upon them in his *Opticks*, 1st Edit. p. 134. to which the *Inquisitive Reader* may have Recourse for his Satisfaction. Only if any enquire farther, Why the *Northern Lights* have of late been so unusually frequent, I must declare, I am far from having satisfied myself, and so shall not pretend to offer any Thing for the Reader's Satisfaction.

London, Nov. 6. 1721,

Will. Whiston.

IV. *Observations upon the Generation of Plants, in a Letter to Sir Hans Sloane, Bart. Pr. Coll. Med. By Patrick Blair, M. D. F. R. S.*

Boston, Dec. 31. 1721.

Honoured Sir,

IT is no small Satisfaction, that what I advanced in my *Botanick Essays* is now so fully confirm'd by Experiments made by some curious Gardeners, among whom is Mr. *Philip Miller*, who writes me,

November 11, 1721.

I. That in Pursuance of my Advice he separated the Male Plants of the Spinage from the Female; the Consequence was, that the Seeds did swell to the usual Bigness; but when he sow'd it, it did not grow afterwards. He searched into the Seed, and found it wanted the *Punctum Vitæ*, which perhaps might have been the Case with Mr. *Geoffroy*; but if not, the female *Embryones* might have been impregnated another Way, as he experimented with twelve Tulips, which he set by themselves about six or seven Yards from any other, and as soon as they blew, he took out the *Stamina* so very carefully, that he scattered none of the Dust, and about two Days afterwards, he saw Bees working on Tulips, in a Bed where he did not take out the *Stamina*, and when they came out, they were loaded with the Dust on their Bodies and Legs: He saw them fly into the Tulips, where he had taken out the *Stamina*, and when they came out, he went and found they had left behind them

them sufficient to impregnate these Flowers, for they bore good ripe Seed ; which persuades him, that the *Farina* may be carried from Place to Place by Insects, and when they happen upon a Flower, whose *Uterus* is capable to be impregnated by such a Dust, it may be thus effected.

I am of Opinion, this will not suit with Mr. *Morland's* Scheme. For tho' we may suppose the *Stamina* of every Flower to be loaded with a due Proportion of the *Farina*, yet this accidental Conveyance of it to a neighbouring Flower, may be rather less than greater than is necessary : So that, if wanting, then those *Embryones*, which had not received its determined Particle into their Bosom, must be defective in Bulk, or barren in growing, but here all were equally fill'd.

2. By a Second Letter, *October 19. 1721.* he informs me, that he bought a Parcel of *Savoy* Seeds of a Neighbour, which he sowed, and planted out the Plants ; but was surprized to see the Production : For he had half of them red Cabbages, and some white Cabbages, and some Savoys with red Ribs, and some neither one Sort nor other, but a Mixture of all Sorts together in one Plant. He went to the Gardiner and told him his Tale, who shew'd him, that he was in the same Condition, but did not know how it should come to pass, for he was sure he took special Care in saving of the Seed. Being ask'd how and where he planted them for Seed, he shew'd him them under a *South-West* Hedge, and told him the Manner in which he planted them : First, a Dozen of white Cabbages, then a Dozen of Savoys, and then a Dozen of Red. Then he immediately thought how it came to pass, by the *Effluvia*

impregnating the *Uterus* of one another; and it is very common for our Gardiners to plant white and red Cabbages together for Seed, and they are as often disappointed by having a Degeneracy of both Kinds, which they attribute to the Soil, and think that is the Cause: They send to *Holland* for a fresh Supply of Seeds, and say our Soil will not continue that Sort Good. He told them his Opinion, and they laugh at him for it, and will not be turn'd out of their Road, although they should have never so many Experiments shew'd them.

This Experiment is a most convincing Argument for the *Effluvia*; for did each Grain of the *Farina* enter the *Pistillum* to its proper *Uterus*, this mongrel Kind would never be produced. For if the individual Plant be in each Grain of the Male *Farina*, how can it be so far dismember'd, as that one Part shall go to the making up of the Ribs of red Cabbage, and another to compose the rest of a Savoy Plant. Analagous to this, is what I lately observ'd in a Spaniel Bitch, of so good a Kind, that when she became proud, Care was taken to let her have good Dogs. The Litter she produced, consisted of Puppies some Piebald, like one of the Dogs that had lin'd her, of the same Shape, Colour and Spots; others like another; and a third partaking of both, with Spots from the Bitch interspers'd. This is a farther Confirmation of what I have advanc'd, Essay 4. where Page 310. I only assert, that several Foetus's partake equally of Male and Female; but here two Males concur with one Female in the Composition of a fourth Body, made up of all the three: And one Seed produces a Cabbage consisting of three different Species, which could never happen, did these organiz'd *Animalcula*, or Granules of the *Farina*, become

come a *Fœtus*, or contain the *Folia Seminalia* of a Plant. This methinks is sufficient to answer what the ingenious Mr. *Bradley* has so strenuously contended for, *Works of Nature*, p. 9. & seq. But since that worthy Gentleman has not thought fit to answer what I have already advanc'd upon that Subject, I may hereafter answer his Objections more at large.

I could descant yet more upon this Observation, and consider how far this may lead us into the infinite Variations and Stripes, in not only annual Flowers, such as Poppies, *Consolida Regalis*, and Bottles, but also in perennial Roots; such as Auricula's, Couflips, &c. of a lower Size, which is hinted by Mr. *Bradley*; he having received that Notion from the ingenious Mr. *Du Bois*, as I have been credibly informed; and in Plants of a larger Size, not of a Bulbous, but Carnous Root, such as Columbines; where there is a vast Variety: And in this Plant it is most especially to be observed, that though the indigenious one, from which all the other seem only to be Variations, and not determinate Species, be of a blue Colour, consisting of ten Alternate Petala, viz. five corniculate, and five plain; yet into how many other Kinds of Flowers is it subdivided; such as pale yellow, with bluish red, purple, dark Stripes vastly double, blue, blackish red, &c. Some with Corniculate *Petala*, and some only with plain, and how in single Flowers it imitates all the Colours we see Pigeons endow'd with. I say it is worthy of Consideration, whether the *Farina* may do this, since I do not understand there has been much Art used in making these Flowers break, as Tulips, or to cultivate a Set of Breeders; but that a richer Soil may produce a double Flower; and a suitable Loam may produce the Variety of Colours; the *Farina*

from several Flowers may occasion the Stripes, and the *Stamina* arising from the plain *Petala*, rather than the *Cornicula*, pouring out the *Farina*, may cause the Flowers with the plain *Petala*. So that were I to extend this to a great many other Plants, and were there proper Observations made upon them, considerable Improvements might be made upon this Doctrine of the Sexes of Plants. For after the Flowers, we come next to the Variegation of the Seed of some Plants, particularly the *Phaseoli*, whose various Spots and Colours, and even the Bigness too, may very much depend upon the *Effluvia* from the *Farina*, when several Kinds are sown together. For do but consider three plain Colours, a White, Red, and dark Blue, and you may observe how many Descendants, and what a Variety of Spots may proceed from them. The *Lupines* also in some Measure may be brought in here, and I know not but that the *Medica Cochleata Falcata Lunata*, may be multiply'd in its Variations after the same Manner. But it is Time to proceed to another Experiment of my Correspondent Mr. Miller.

Being persuaded to it by an ingenious Gardiner, he pull'd off all the Male-Flowers of some Melon Plants so soon as they appeared; but instead of finding, as his Friend informed him, that these Flowers exhausted the Nourishment from the Fruit; he found that, without these Flowers, none of the Melons would grow, so that he was deprived of the Fruit which he expected.

As this Experiment is a plain Indication of the Necessity of the *Farina*, so it confirms the Use I have assign'd to the Leaves, *viz.* that by entering the Capillaries of the Leaves, and returning, the nutritive Particles may be more attenuated; So here, the *Petala*

of the Male-Flowers may serve for the same Purpose; for by the Largeness of the *Tubuli* in these *Pomiferæ Scandentes*, a gross viscid Sap is received, which even the Leaves themselves are not sufficient to attenuate, so as to be fit for composing the more subtile Part of the Fruit; until by repeated Circulation through the *Petalà* of the Male-Flowers, it may be render'd fit for such a Purpose. Indeed, the Female-Flowers upon the Top of the *Rudimentum Fructus*, may in some Measure serve for this Purpose. But as the Male-Flowers are, generally speaking, more numerous than the Female, so their being remov'd must deprive the *Embryones* of a very great Assistance towards its being perfected: I may add, that the Orifices of the Pedicles, when the Flowers are pull'd off, must lose so much of the Sap, that the whole Plant must be thereby so impoverish'd, as not to be able to bring forth the design'd Fruit; all this, beside the Want of the considerable Supply of the *Farina Fæcundans*.

I design'd to have given a few Thoughts concerning the Variagation of Leaves and Flowers, being unwilling to admit of Mr. *Bradley's* Sickness or Weakness of the Sap: But I shall reserve that to a more convenient Opportunity, being at present intent upon making some farther Improvements upon the Generation and Nourishment of Plants, which I hope to have well confirm'd by Experiments made by my good Correspondents the Gardiners, especially this Mr. *Mil-ler* and my self. I shall expect your Sentiments of these, which will be a great Encouragement to proceed, to

Your Most Obliged,

P. A. B L A I R.

V. *Observation of an extraordinary Height of the Barometer, December 21. 1721. By Mr. George Graham, Watchmaker, F. R. S.*

Jan. 11th, 1721.

UPON *Thursday* the 21st of *December* last, observing the Barometer much higher than usual; that Evening, between Seven and Eight a Clock, I fill'd a Tube with very clean Quick-silver, and found the Height a little to exceed $30,7\frac{1}{2}$ Inches. By Eight the next Morning, a Wheel-Barometer, which hung in the same Room, had risen One tenth of an Inch higher than it was the Night before, when the Experiment was made; at Ten a Clock, One fifth of an Inch more: At which Time it was at the highest, being a little above $30,8\frac{1}{2}$ Inches; for about Twelve at Noon it was sensibly lower, and continued falling all the rest of the Day.

When the lower End of the Tube was first immers'd in the Cistern, the Quick-silver for some Time adher'd to the Crown of the Glass, but upon shaking, it fell to the Height above-mention'd.

VI. *A Caution to be used in examining the Specific Gravity of Solids, by weighing them in Water.* By James Jurin, M. D. R. S. Secr.

AS it is oftentimes of good Use to know the Specific Gravity of solid Bodies, a great Number of Experiments have been made upon this Subject by Members of the *Royal Society*, and other Curious Persons; the Result of which has been publish'd in several Tables in the *Philosophical Transactions*, and elsewhere. But, as it is necessary that Experiments of this Nature should be made with great Exactness, if we would so far depend upon them, as to draw any Inferences from them in Natural Philosophy, it may not be amiss to mention a Caution, which is oftentimes necessary in the making of them, and which I have Reason to think has been generally very little regarded. It is this; That when a dry, porous Body is to be weigh'd in Water, in order to discover its Specific Gravity, it is necessary, by some means or other, to extricate the Air out of all the small Pores and Cavities within it, that the Water may have free Liberty to enter and pervade them. Unless this Care be taken, it must needs happen, that the Air, which possesses those small Cavities, and keeps the Water out, will render the Solid of less Weight in the Water, and consequently of less apparent Specific Gravity than it really is. The best way of avoiding this Inconvenience, is, to set the Vessel of Water, in which the solid

lid Body is immersed, under the Receiver of an Air-Pump, and to extract the Air out of the Body by that means; which will be more easily and exactly done, if the Water be first heated over the Fire. And where the Conveniency of an Air-Pump cannot be had, the same Thing may be done almost as well, by letting the solid Body continue some Time in boiling Water over the Fire.

But no solid Body must ever be put into hot Water, that will in any measure dissolve, or give a Tincture to the Water.

One Instance of the Neglect of this Caution, may be seen in the Accounts we have of the Specifick Gravity of the Stones taken out of Human Bladders, which have been commonly found to be but about one half, and some of them have been no more than a fourth Part heavier than an equal Bulk of Water. From this it has been too hastily concluded, that these Stones are very improperly call'd by that Name, as not at all approaching to the Specifick Gravity of even the lightest real Stones, that we have any Account of.

Whereas it is much more reasonable to suppose, that those Stones, which have been found to be so light, were such as had been a considerable Time taken out of the Bladder, and consequently had lost much of their Weight by the Evaporation of the Urine, with which they had at first been saturated, and that they had afterwards been tried without the Caution above-mention'd. I would therefore beg Leave to recommend it to those, who shall examine the Specifick Gravity of the Human *Calculus*, that they will either try the Experiment upon Stones fresh taken out of the Bladder, or else that they will be pleas'd to use the
 2 above-

abovesaid Method, to extricate the Air out of their Cavities. If they do this, I am confident they will meet with some *Calculi* (as I have done) exceeding the Weight of some Sorts of burnt Earthen Ware and Alabaster, and approaching very near to that of Brick, and the softer Sort of paving Stone. But it is not to be expected, that they should entirely equal the Specifick Gravity of Stone, found in the Earth; because the Mixture of some Portion of the Animal Oil and Volatile Salt, with the stony Substance of the Human *Calculi*, must needs lessen the Specifick Gravity of the whole Concrete.

I shall mention one other Observation, relating to this Subject; which, however trivial it may seem, yet to me was very surprizing, when I accidentally discover'd it. It is, That the Substance of all Wood (as Oak, Fir, &c.) is specifically heavier than Water. To prevent being misunderstood, I must observe, that in Wood, and other Vegetables, there are two Sorts of Vessels; one of which convey the Sap, and the other contain only Air, for which Reason they are call'd Air-Vessels. When Wood floats, or swims in Water, this Effect is not owing to the Lightness of the Substance of the Wood, but only to its being buoy'd up by the Air contain'd in the Vessels before-said. For when the Air is extracted out of those Vessels, and instead thereof the Water has insinuated it self into them, the Wood will sink to the Bottom. As is very easily shewn in small Chips, or Shavings of Wood, by means of the Air-Pump, or an Infusion in boiling, or even in cold Water for a sufficient Time. And the same is found to succeed in the Roots, Stalks, Leaves, and Seeds of as many other Vegetables as I have yet try'd; Cork only excepted; in which last I had no Reason to expect it, con-

sidering the particular Structure of that Substance, as describ'd by the late Learned Dr. *Hook*, in his *Micrographia*.

VII. *A Lettrr from Mr. Edward Naish, Surgeon in York, to Claudius Amyand, Esq; Serjeant-Surgeon to his Majesty, and F. R. S. Concerning an Ossification of the Crural Artery.*

York, Sept. 11. 1721.

MR. *Conssett*, of *Cleveland* in *Yorkshire*, a Gentleman of Sixty seven Years of Age, who all his Life before had enjoy'd a perfect good State of Health, sent for me on account of a Mortification, which began about a Month before on one of his Toes, and by gradual Advances in that Time had reach'd half way his Leg; and this without any manifest Cause. This was the State I found him in; *viz.* a perfect Mortification, or *Sphacelus* of his Foot, and half his Leg. In such a Case, what was to be done? The Gentleman saw himself dying daily by Piece-meal; but Heart-whole, as he express'd it, and had a pretty good Pulse. I propos'd Amputation, as the only Remedy, which (I told him) would give him some Chance for his Life; tho' the Odds was against him. This he readily consented to; and as soon as I could get my Dressings ready, I went about the Operation; assisted by Mr. *Mitford*, a Surgeon of *Northalerton*, and Mr. *Moon* of *Stockton*, who before had attended the Gentleman.

The Leg being taken off at the usual Place, (which was four Inches above the Mortification) about two or three Ounces of Blood issued out from the Muscular Part; but upon slackening the Turniket, in order to look for, and tye the Artery, not one Drop of Blood flow'd out; to my no small Surprize. And upon feeling the Extremity of the Artery, I found it hard and callous; however, I secur'd it by a Ligation, as usual, and dress'd the Stump. The Patient (who had born the Operation with the greatest Resolution) being put to Bed, I was desirous to examine the Leg; and having dissected the Artery, with its two considerable Branches as far as the *Tarsus*, I found them for the most part ossified; that is to say, the Trunk, where it was amputated, was ossified about two thirds of its Circumference. About a Quarter of an Inch lower, the whole was bony, leaving so small an Orifice, that it would only admit of a Hog's Bristle; not a very fine Probe, that I endeavour'd to introduce. A little lower, it was on one side bony, on t'other membranous; then again an entire Case of Bone. Here and there, for the Breadth of a Barly-Corn, there would be no Bone at all. I open'd about two Inches of the internal Branch immediately above the *Malleolus*, it appearing blacker than the rest. After it had been wash'd, I found in it about two or three Drops of coagulated Blood; and now 'tis expanded and dry'd, 'tis one entire *Lamina* of Bone, as thick as the Shell of a Pigeon's Egg, and of an unequal Surface. I dissected three Ramifications of this internal Branch into the Foot; only one of them had a very small Bit of Bone in it, about half an Inch from the Trunk. The other great Branch, that runs

on the Ligament that ties the Fociles together, was not so much ossified as that which I have described.

This Ossification (which is the compleatest of any I have yet heard of) was, no doubt, the Cause of the Mortification, and of the Death of my Patient, which follow'd four Days after the Amputation, as Mr. *Mitford* inform'd me, being oblig'd to leave him the next Morning; though I should have been curious, had I been present, to have seen how high the Ossification reach'd.

I must observe to you, That this bony Shell, or *Lamina*, was contain'd within the Tunicles, or Coats of the Artery. I don't doubt, but these Cases are more common than we imagine. For when we see Mortifications seize the Extremities of Aged People, which we commonly attribute to a Decay of Nature, or an Extinction of the Vital Warmth; this, I believe, is often the Cause. And I am the more inclin'd to think so, from two or three parallel Cases I have been concern'd in, since I came into *Yorkshire*.

I am, &c.

Edward Naisb.

VIII. *An Account of a Rainbow seen on the Ground. Communicated in a Letter from the Reverend Benj. Langwith, D. D. Rector of Petworth, to Dr. Jurin, Secr. R. S.*

Petworth, Dec. 17. 1721.

ON the 7th of *September* last, about Nine in the Morning, I was riding with some Friends over *Port-Mead* near *Oxford*. The Morning had been Misty, and the Grais was very wet with the Dew. We had not been long out, before the Air clear'd up, and the Sun began to shine very bright. We soon after had the Satisfaction of seeing a Rainbow upon the Ground, whose Colours were very near as lively as those of the common *Iris*: This was extended upon the Ground for some Hundreds of Yards, and the Colours were so strong, that it might have been seen much farther, had it not been terminated by the Bank, and Hedge of the Field. It is hardly worth while to observe, that it continually chang'd its Place as we mov'd along, since this is no more than happens in other Rainbows. The more remarkable Particulars were these:

1st. That the Figure of it was not round, but oblong; being as I conceive, a Portion of an *Hyperbola*.

2. That the Convex Part of it was turn'd towards the Eye, and the *Vertex* was at a small Distance before us.

3. That

3. That the Colours took up less Space, and were much more lively in those Parts of the *Iris* that were near us, than in those at a Distance.

These *Phænomena* may easily be accounted for, by comparing this *Iris* D C E, Fig. 2. with the common *Iris* k i E e form'd by Drops falling in the Air at a small Distance from the Eye of the Spectator, H, and touching the Ground with the lower Part of its Arch in E, the vertical Point of the *Iris* D C E. Produce the Cone H k i E e : Its Intersection with the Plane of the Horizon will give the Figure of the *Iris* D C E. Hence it follows,

1st. That as the Angle e H G happens to be greater, equal to, or less than 90 deg. the Figure will be an *Hyperbola*, *Parabola*, or *Ellipsis*.

2. That as the Sun was about 30 deg. high, when we view'd the *Phænomena*, the *Iris* was an *Hyperbola*.

3. That the Arches of the same *Iris*, consisting of Colours of different Refrangibility, may also in some Cases be different Sections of the Cone.

4. That since the Angle e H F is always given ; from the Height of the Point of View H G, and the Sun's Altitude S L A, the Dimensions of these *Iris*'s are easily determin'd.

I am, &c.

Ben. Langwith.

IX. *Epistola Domini Antonij van Leeuwenhoek,*
R. S. S. de Osculis, sive Spiraculis Foliorum
Buxi, item de lanugine Malorum Persicorum
& Cydoniorum.

Delphis, 15. Januarij 1721.

NUPER Cogitationem meam converti ad Frondes Arborum nostratium, tacitusque meditabar num & illæ forsitan Osculis sint instructæ ; cum igitur in Area mea duas haberem Arbusculas, sive Plantas illius Buxi, quæ Vulgo *Palma Ceres* appellatur, folium ex iis unum decerpsi, & in partes divulsum ope Microscopij contemplatus sum. Tum vero partes illas, per quas Transpiratio vel Exhalatio sit, clarissime visu distinxi. Adhæc complures percepi exiguissimos Hiatus, qui lucem transmittebant : Quos tamen commodius majorique numero percepi, cum partes prædicti folij aliquanto essent sicciores.

Cum præterea supra porticum Domus meæ, quæ Porticus plum bocontacta est, alia staret Arbuscula Buxi ; quædam illius Foliola, partim adhuc virentia, partim exsiccata, mihi adferenda curavi, ut illorum Texturam, quantum possibile esset, investigarem : Quod eo Successu feci ; ut istiusmodi Oscula, sive Spiracula, in foliis istis clarius Visu perceperim, quam in ullis unquam Fructibus ante percepisssem. Ut autem Multitudinem Osculorum, quæ in tali Foliolo percepi, velut Oculis expositam haberem ; Folium Buxeum Lineali imposui cupero, quod

quod in varias partes distinctum erat: Comperique Longitudinem Folij parem esse octonis partibus Pollicis, in decem partes distributi; Folij vero Latitudinem cum medietate pollicis, sive quinque decimis partibus exæquari.

Jam vero ponamus tali Foliolo Figuram esse Ovalam; adhæc Latitudinem ejus atque Longitudinem jungamus: Tum exsurget numerus 13, cujus dimidium sit $6\frac{1}{2}$. Dein ponamus idem Foliolum, post illam Latitudinis atque Longitudinis conjunctionem, instar Circuli esse rotundum; illiusque Diametrum $6\frac{1}{3}$ decimis Pollicis partibus ex æquo respondere.

Exinde juxta Foliolum ante dictum, locavi Pilum Porcinum; quem adhibito Microscopio contemplatus, judicavi duodecim Buxi Oscula, si sibi contigua jacerent, cum Diametro Pili Porcini Longitudine exæquari; Sexaginta vero Pilos Porcinos judicabam Magnitudinis esse pollicaris. Sequitur decimam quamque Pollicis partem sex Diametris Pilorum Porcinatorum Longitudine parem esse; dimidiatam vero Diametron Folioli Buxei cum $19\frac{1}{7}$ Diametris Pilorum Porcinatorum exæquari. Quæ $19\frac{1}{2}$ Diametri, si duodecies, id est juxta numerum Osculorum, multiplicentur; efficitur numerus 234, quam Longitudinem dimidiatus Folioli Buxei circulus ex antedictis habet.

Ut autem quid tali Circulo contineatur supputemus, primo cum Geometris statuendum est; quæ proportio est numeri 14 ad numerum 11, eandem Proportionem esse inter numerum quadratum Diametri cujusvis Circuli, & ea quæ Circulo ipso continentur. Sequitur unam Folij Buxei Superficiem Osculis 172090 præditam esse. Cum autem altera Superficies haud paucioribus instructa sit; tandem exsurget numerus

Osculorum 344180, quorum Ope Perspiratio & Exhalatio fiat.

Cum Lanuginem illam, quæ Mala Persica, vulgo Montana convestit, nupera Æstate sine Microscopio considerarem; Frustrula quædam Mali Persici, ex cortice excisa, ante Microscopium collocavi. Tum vero judicabam Floccos illos Laneos Multitudine pares esse Osculis per cutem Mali Persici diffusis. Et quemadmodum Oscula Fructuum velut inter duo labiola patescere, nec plane rotunda sed aliquantulum oblonga esse, jam ante monui; sic Flocci Lanei, ex Osculis dictis efflorescentes, non omnino rotundi sunt, sed nonnihil plani. Sed & complures in medio Ruga quadam notatos esse videbam.

Ut autem Multitudinem Osculorum halitus exspirantium palam proponam; unàque ingentem numerum exhalantium humorum, qui Aerem ingressi in particulas oblongas, sed alias aliis longiores spissescunt, Oculis subjiciam; exiguam Mali Persici portionem delineari & in Icone 3 per A B C D E F G indicari curavi. Ubi quidem A B F G perpusillum est Frustrum Mali et Corticis Persici, per B C D E F lanugo Malo adhærescens designatur.

Magnitudinem dicti Frustruli, per Iconem designati, sic æstimare poteris: Tres Vultus mei Pilos Microscopio applicatos habebam, quos Pictori post Iconis modo dictæ Delineationem intuendos exhibui. Cum deinde ejusdem Iconis Longitudinem, a G ad A in 16 Partes distribuisssem; rogavi quot latorum pilorum Diametros in illa Iconis Longitudine contineri judicaret; qui, *ne octo quidem* respondit. Quod si verum esse statuamus, quam incredibili Floccorum Laneorum Multitudine necesse est Malum Persicum circumvestiri!

Cum postea Mala sive Pyra Cydonia permatu-
 ruissent, quasdam etiam Mali Cydonij particulas ad
 Microscopium applicavi ; & Lanuginem, quæ ex Malo
 Cydonio exhalatur, neque Lanugini Mali Persici Copiâ
 cedit, delineandam curavi ; quæ omnia in Icone 4 de-
 signata vides per H I K L M N O. Ubi H I N O
 perparva est portio Mali Corticisque Cydonij, per
 I K L M N Lanugo ex Malis Cydoniis exsudans in-
 dicatur. Quæ Lanugo, licet in Cydoniis longior quam
 in Persicis, non tamen in illis erigitur, sicut in istis ; sed
 crispando sibi invicem implectitur.

X. *Remarks on some Attempts made towards a
 perpetual Motion, by the Reverend Dr. Desagu-
 liers. F. R. S.*

THE Wheel at *Hesse-Cassel*, made by Monsieur *Or-*
fireus and by him called a perpetual Motion, has
 of late been so much talk'd of, on Account of its wonder-
 ful *Phænomena*, that a great many People have believed
 it to be actually a self-moving Engine ; and according-
 ly have attempted to imitate it as such. Now as a great
 deal of Time and Money is spent in those Endeavours ;
 I was willing (for the Sake of those that try Experi-
 ments with that View) to shew that the Principle,
 which most of them go upon is false, and can by no
 Means produce a perpetual Motion.

They take it for granted, that if a Weight descend-
 ing in a Wheel, at a determinate Distance from the
 Centre, does in its Ascent approach nearer to it ; such
 a Weight

a Weight in its Descent will always preponderate, and cause a Weight equal to it to rise, provided it comes nearer the Centre in its Rise; and accordingly as it self-rises, will be overbalanced by another Weight equal to it; and therefore they endeavour by various Contrivances to produce that Effect, as if the Consequence of it would be a perpetual Motion.

But I shall shew, that they mistake one particular Case of a general *Theorem*, or rather a *Corollary* of it, for the *Theorem* it self. The *Theorem* is as follows :

Theor. If one Weight in its Descent, does by Means of any Contrivance, cause another Weight to ascend with a less *Momentum* or Quantity of Motion than it self, it will preponderate and raise the other Weight.

Cor. 1. Therefore if the Weights be equal, the descending Weight must have more Velocity than the ascending Weight, because the *Momentum* is made up of the Weight multiplied into the Quantity of Matter.

Cor. 2. Therefore if a Leaver or Balance, have equal Weights fasten'd or hanging at its Ends, and the *Brachia* be ever so little unequal, that Weight will preponderate, which is farthest from the Centre.

SCHOLIUM.

This Second *Corollary* causes the Mistake; because those, who think the Velocity of the Weight is the Line it describes, expect that that Weight shall be overpois'd, which describes the shortest Line, and therefore contrive *Machines*, to cause the ascending Weight to describe a shorter Line than the descending Weight.

As for Example, in the Circle A D B a (*Fig. 5.*) the Weights A and B being supposed equal, they imagine, that if (by any Contrivance whatever) whilst the Weight A describes the *Arc* A a, the Weight B is carried in any *Arc*, as B b, so as to come nearer the Centre in its rising, than if it went up the *Arc* B D; the said Weight shall be overpois'd, and consequently, by a Number of such Weights, a perpetual Motion will be produced.

This is attempted by several Contrivances, which all depend upon this false Principle; but I shall only mention one, which is represented by *Fig. 6.* where a Wheel having two parallel Circumferences, has the Space between them divided into Cells, which being carv'd, will, (when the Wheel goes round) cause Weights plac'd loose in the said *Cells*, to descend on the Side A A A, at the outer Circumference of the Wheel; and on the Side D to ascend in the Line B b b b, which comes nearer the Centre, and touches the inner Circumference of the Wheel. In a *Machine* of this Kind, the Weights will indeed move in such a Manner, if the Wheel be turn'd round, but will never be the Cause of the Wheels going round. Such a *Machine* is mentioned by the Marquis of *Worcester*, in his *Century of Inventions* in the following Words, N^o. 56.

“ To provide and make that all the Weights of the
 “ descending Side of a Wheel, shall be perpetually
 “ farther from the Centre, than those of the mount-
 “ ing Side, and yet equal in Number, and best to
 “ the one Side as the other. A most incredible thing,
 “ if not seen; but tried before the late King (of blef-
 “ sed Memory) in the *Tower* by my Directions, two
 “ extraordinary Ambassadors accompanying his Ma-
 “ jesty,

“ jesty, and the Duke of *Richmond*, and Duke of *Ha-*
 “ *milton*, with most of the Court attending him. The
 “ Wheel was fourteen Foot over, and had forty Weights
 “ of fifty Pounds a Piece. Sir *William Balfore*, then
 “ Lieutenant of the *Tower*, can justify it, with
 “ several others. They all saw, that no sooner these
 “ great Weights passed the Diameter Line of the lower
 “ Side, but they hung a Foot farther from the Centre ;
 “ nor no sooner passed the Diameter Line of the upper
 “ Side, but they hung a Foot nearer. Be pleased to
 “ judge the Consequence.

Now the Consequence of this, and such like *Ma-*
chines, is nothing less, than a perpetual Motion ; and
 the Fallacy is this. The Velocity of any Weight is
 not the Line, which it describes in General, but the
 Height that it rises up to, or falls from, with respect
 to its Distance from the Centre of the Earth. So that
 when the Weight (*Fig. 5.*) describes the *Arc A a*, its
 Velocity is the Line *A C*, which shews the perpen-
 dicular Descent (or measures how much it is come
 nearer to the Centre of the Earth) and likewise the
 Line *B C* denotes the Velocity of the Weight *B*, or the
 Height that it rises to, when it ascends in any of the
Arcs B b, instead of the *Arc B D* : So that in this Case,
 whether the Weight *B* in its Ascent be brought nearer
 the Centre or not, it loses no Velocity, which it ought
 to do, in order to be rais'd up by the Weight *A*. Nay,
 the Weight in rising nearer the Centre of a Wheel,
 may not only not lose of its Velocity, but be made
 to gain Velocity, in Proportion to the Velocity of its
 counterpoising Weights, that descend in the Circum-
 ference of the opposite Side of the Wheel ; for if we
 consider two *Radij* of the Wheel, one of which is

Horizontal, and the other (fasten'd to and moving with it) inclin'd under the Horizon in an Angle of 60 degr. (*Fig. 7.*) and by the Descent of the End B of the *Radius* B C, the *Radius* C D by its Motion causes the Weight at D, to rise up the Line p P, which is in a Plane that stops the said Weight from rising in the Curve D A, that Weight will gain Velocity, and in the Beginning of its Rise, it will have twice the Velocity of the Weight at B; and consequently, instead of being rais'd, will overpoise, if it be equal to the last mentioned Weight. And this Velocity will be so much the greater, in Proportion as the Angle A C D is greater, or as the Plane P p (along which the Weight D must rise) is nearer to the Centre. Indeed if the Weight at B, *Fig. 5.* could by any Means be lifted up to β , and move in the *Arc* βb , the End would be answer'd; because then the Velocity would be diminished, and become βC .

Experiment (*Fig. 7.*)

Take the Leaver B C D, whose *Brachia* are equal in Length, bent in an Angle of 120 degr. at C, and moveable about that Point as its Centre: In this Case, a Weight of two Pounds hanging at the End B of the horizontal Part of the Leaver, will keep in *Æquilibrio* a Weight of Four Pounds hanging at the End D. But if a Weight of one Pound be laid upon the End D of the Leaver, so that in the Motion of D along the *Arc* p A, this Weight is made to rise up against the Plane P p (which divides in half the Line A C equal to C B) the said Weight will keep in *Æquilibrio* two Pounds at B, as having twice the Velocity of it, when
the

the Leaver begins to move. This will be evident, if you let the Weight 4 hang at D, whilst the Weight 1 lies above it : For if then you move the Leaver, the Weight 1 will rise four times as fast as the Weight 4.

XI. *A Method for rowing Men of War in a Calm. Communicated by Monsieur Du Quet.*

TO perfect the Art of Navigation; Two Things seem principally wanting ; viz. An easy Method for finding the Longitude at Sea ; and a Way to give a Vessel its Course, when there's no Wind stirring.

I flatter my self to have found the last ; and hope to make it appear, by Reason and Experiment, That a Man of War may make a League an Hour in a Calm, by Means of revolving Oars, which are easily apply'd to the Sides of the Ship, without occasioning any Incumbrance : As I shall make appear by the following Account, after having deliver'd my Notion of the Motion of Bodies in Fluids.

A Body swims upon Water, when it weighs less than the Volume of Water, whose Place it takes up ; and it sinks more or less in the Water, only in proportion as its Volume is more or less increas'd.

A Body lying in still Water, is as it were in *Æquilibrium* ; the least Effort gives it Motion, and makes it lose that *Æquilibrium*. If the Effort be continued, tho' ever so little, the Motion it communicates will be very

ry sensible. How great soever the Weight of the Body be, when once it is in Motion, it will always continue so, if nothing hinders.

Upon these Principles, I consider the Motion a Vessel receives by means of Oars, and the Application of Hands that set it a-going. The *Impetus* of the Hand, apply'd at one End of the Oar, and the Resistance the Water makes against the other End, are both impress'd upon the Point, where the Oar rests upon the Vessel. This Point is like the *Fulcrum* of the common Leaver, which always bears the Sum of the Weights at both Ends, besides the Weight of the Leaver it self: So that the greater the Effort is at one of the Oars, and the Resistance at the other, so much the greater is the Impression, which the Point or *Fulcrum* receives, in order to its being put in Motion. A Galley, with two Oars only, would go as fast as it does with the usual Number; provided the same Number of Hands were apply'd with equal Vigour to the two Oars, and the Oars were strong and broad enough to make the necessary Resistance: Because then the *Fulcrum* of the two Oars would receive as much Impression, as all the *Fulcra* of the common Oars taken together.

This Consideration put me at first upon contriving a Way, how to apply a greater Number of Hands to the common inclined Oars; but, after several Tryals, I threw them aside, and made use of perpendicular Oars; because the first do only skim the Water, and (when the Sea is rough, and the Waves run high) they don't take Water oftentimes, and so become useless. For, in this Case, the Rowers are tript up, for want of meeting a Resistance.

This

This Inconvenience is avoided by the revolving Oars ; because they take the Water perpendicularly, and enter far enough not to miss it : And if the Water should happen to evade the Stroke, the Rowers would not be so incommoded ; because they would be supported at every Vibration, which is only of three Foot. Besides, in the Use of inclined Oars, more than half the Time is lost, in raising and recovering the Oar, before they give the Stroke ; which makes the Vessel move by fits and jerks, so that the People aboard feel (as it were) every Stroke of the Oars when they play ; whereas, the revolving Oars always move equally, and succeed one another without Loss of Time ; which makes the Vessel move uniformly, without affecting those who are aboard.

It is to be observ'd too, that a Gally built on purpose for the Use of inclined Oars, would not be so proper as another Vessel for perpendicular Oars ; because the Gally has a considerable Length and but little Height above the Water.

Having propos'd this Invention to the Court of *France*, I was sent to *Havre de Grace*, to make a Tryal, which had the Approbation of the Intendant. He made his Report, That the Officers at first object-ed to the Invention ; but as for his own part, the more particularly he consider'd it, the more he was convinc'd of its Usefulness. I was afterwards sent to *Marseilles*, where I made several Tryals on board a Gally ; the Swiftnes of which was compar'd with that of another Gally, equipp'd as usual. *M. de Chazelles*, a Member of the *Royal Academy of Sciences*, and Engineer of the King's Gallies, had Orders to make his

Observations, and send 'em to Court: A Copy of which he gave me, sign'd with his own Hand, and is as follows.

The Report of Monsieur de Chazelles.

An Experiment of the Swiftnefs of a Gally, with perpendicular revolving Oars, invented by Monsieur Du Quet; compar'd with that of a common Gally. Made at Marseilles, &c.

At 10 h. 3 min. in the Morning, the *Superbe Gally* quitted her Station over-against the *Augustins*, in order to fall down to the *Chain*.

At 10 h. 11 m. she came to the *Chain*.

At 10 h. 6 m. the *Machine Gally* quitted her Station; at the innermost Part of the Port. She had three *Machines* on each side.

10. 13. She came to the *Chain*.

10. 19. The two Gallies abreast. Both row with their whole Crews.

10. 25. The *Superbe* passes; and then rows only with the hinder part of her Crew.

10. 27. The *Machine Gally* passes.

10. 28. Both row with their whole Crews.

10. 30. The *Superbe Gally* passes; and then rows only with the fore part of her Crew.

10. 32. The *Machine Gally* passes; upon which the *Superbe Gally* claps on more Oars, till such Time as she has acquir'd the same Velocity with the *Machine Gally*: And it appear'd, that with seven or eight Oars less than her Complement
on

on each side, she kept up with the *Machine* Gally ; making about 200 Rowers, which was the Number of the *Machine* Gally's Crew. There was a little Wind a-head, which retarded the *Superbe* something more than it did the *Machine* Gally ; because the *Superbe* had her Masts and Yard-Arms standing, and the other not.

10. 43. Came to the Moorings of the *Iles*. The Sign given for turning.

10. 47. The *Superbe* was come about. It appear'd, that the *Machine* Gally was considerably quicker in turning than the *Superbe*.

10. 30. They came again into Port.

By this it appears, that the *Machine* Gally has a considerable Advantage over the common one, in quitting her Station, and acquiring her first Motion : For, in seven Minutes, she ran the whole Length of the Port ; having quitted her Station by means of her Oars, without towing her self by her Moorings ; which is what another Gally would not have effected, but very slowly. And the *Superbe* Gally, after she had mov'd from her Station, was eight Minutes in going a less Distance than the Length of the Port.

But if we consider the Experiment made without the Harbour, it seems to prove the common Gally to have the Advantage over the *Machine* Gally, tho' the Number of Hands be equal. For, with eight Oars less than her Complement on each side, she kept up with the *Machine* Gally, notwithstanding the greater Resistance of the Wind against her Masts. However, if we consider, that the Crew of the *Superbe* was a great deal better than that of the *Machine* Gally ;

that the *Superbe* is acknowledg'd to be one of the best Sailors the King has ; whereas that which had the *Machines*, is an old decay'd Gally, and reckon'd a very bad Sailor ; besides that the Crew of the *Superbe* are much better acquainted with the common Oar, than the others are with the new way of rowing ; and that in the common Gally there's no Improvement to be made, either with respect to the Proportion of the Oars, their Length, the Breadth of the Pallets, the Height of the Point of Rest, &c. or with respect to the Construction of the Vessel ; whereas in the *Machine*, there are several Things to be improv'd and alter'd in the Oars, the Hand-spikes, and in disposing the Men to the best Advantage. These Things, I say, consider'd, it seems reasonable to believe, that a Vessel with the *Machines* might go faster than one with the common Oars ; because the Loss of Time is avoided, which happens in the ordinary way of rowing.

This Experiment, however defective it be, for the Reasons above, will prove, That the Velocity is greater in this way of rowing than in the other, when the Circumstances on both sides are equal. For, by my Journal, I find, that the *Patronne*, in Company with fourteen other Gallies, left the Port of *Marseilles* at 50 min. past three ; and rowing all in a Calm, came to the *Isles* at 4 h. 23 min. ; which made 33 m. in going from the *Chain* to the *Isles*. But the *Machine* Gally made the same way, with 200 Men, in 30 min. having left the *Chain* at 10 h. 13 m. and arriv'd at the *Isles* at 10 h. 43 min. altho' there was some Wind a-head.

Sign'd CHAZELLES.

This

This Experiment shews, that the *Impetus* does not depend upon the Number of Oars, but the Number of Men.

A Vessel charg'd with revolving Oars, will go as fast in a Calm with 100 Men, as it would do if towed by a Gally of 200 Men ; because there will be one Gally less to draw along.

Another Memoire of Monsieur Chazelles, concerning the Usefulness of perpendicular revolving Oars, invented by Monsieur Du Quet.

The Experiment made of the New *Machine*, altho' defective by reason of the Difference there was with respect both to the Crew and the Vessels, does yet leave room to expect a considerable Advantage from this Invention, in giving the Ship way : For tho' the common Gally should keep up with the *Machine* Gally at their first setting out, with equal Number of Hands ; 'tis evident, the *Machine* Gally will get the better at long Run, when the others Crew are so fatigu'd, as to be oblig'd to row by turns. For here the Men will hold out a longer Time, their Action not being so great, nor so violent. Besides, having only 200 Men employ'd, and being equally mann'd with the other Gally, fresh Hands may be supply'd, and so they will continue to go at the same Rate : For in case of need, the Marines may be employ'd in this Service ; which they will perform with as little Reluctance, or Trouble, as they work at the Capstane.

The Reason of this Increase of Velocity appears plain, if we consider the Difference between the com-
mon

mon way of rowing, and that by perpendicular Oars : The last is done by an uninterrupted Application of Force, in the same Direction ; the other acts by Jerks. And, of the three Parts of Action that are employ'd, in order to give the Strokes ; one in raising the Oar out of the Water, the second in advancing the Hands forwards, and the third in pressing against the Water ; only the last turns to Account : And that still loses something of its Efficacy ; for the Crew, by their falling back all together, make the Vessel plunge, and render its Motion oblique, which contributes very much to its Decay.

These are not the only Defects of the common Oars ; for, in order to augment their Force, the Number is to be increas'd, and consequently, the Vessel must have a greater Length ; by which means, it is render'd weaker and less able to resist the force of the Sea. Besides, the Vessel must be low-built, and uncover'd, (and so more expos'd to the beating in of the Waves) by reason they are obliged to proportion the Length of the Oar to the Strength and Size of the Men. And tho' the Crew should be under some Covert, as they are in a Galeass ; an Opening must be left for the Oars to play, by which the Waves may beat in.

Both these Inconveniencies are avoided, by the perpendicular Oars ; because the Addition of Force may be obtain'd, by only applying more Hands to the *Machine* ; so that with two or three *Machines* on a side, there will be more or less Force, in proportion to the Number of Men employ'd, and the Length of the Vessel may be lessen'd at Discretion. And to guard against the Sea, another Deck may be made, shut close

close on all sides, even where the *Axis* of the *Machine* passes through.

The chief Objections against this Invention, seem to me sufficiently obviated by Mr. *Du Quet's Memoir*: But tho' the whole of what is objected should indeed prove, That a Vessel made for sailing, as the common Gally, would be so incumber'd with the *Machines*, as to make the Use of Sails impracticable; yet if it still holds true, that she will move faster; as appears, both by Reason and Fact; it must be allowed, that a Vessel might be so commodiously constructed, to carry these *Machines*, as to go as fast as a Gally in a Calm, and better endure the Weather when under Sail.

Such a Vessel would have several Advantages above a Gally, both in Sailing, and in Fight; not to mention the Conveniencies of lodging the Crew. She may put off to Sea any where, and thereby avoid the Dangers attending the Coast-Winds, which Gallies find to be a-head as soon as they have doubled certain Capes; and so they find themselves between two Winds, which there would be no Danger of, farther out at Sea. With respect to Fight, she may mount Cannon fore and aft, and on each side; and even Mortar-pieces. In Time of Battel, she would be of wonderful Use; for she would take and maintain her Post without Assistance, either at the Head, or the Rear of the Enemy's Line, and there make use of her Bombs: Besides the Advantages of towing off other Vessels from their Danger in a Calm, and of boarding, or making off from the Enemy. And this holds in Ships of any Rate; provided the Length of the Oars, the
Breadth

Breadth of the Pallets, and the Strength of the Handspikes be proportionable. And the moving Force will always be in proportion to the Strength and Number of the Men employ'd, and not to the Number of *Machines*, as in the common Oars, which too are impracticable in Ships above the fourth Rate, by Reason of their great Length, which will be disproportionate to the ordinary Bulk of a Man.

By this means the Crew will be free from the Fatigue of towing, and the Vessel will move incomparably faster than if it was towed ; because the Chaloups which tow, are subject to the Inconveniencies of the common Way of towing, by losing two thirds of the Time ; and besides, they can't act all together : And the Vessel that is towed, pulling them back after the Oar has made its Stroke, they have so much of the Space to regain by the next Stroke. Besides, the Cable by which they tow, sinking into the Water by its own Gravity, the Resistance the Water makes to its Return, is to be overbalanced ; all which Circumstances together considerably diminish the towing Force.

Besides, this Invention is not such as is destructive to Mankind, and becomes useless to the Nation that first puts it in Practice, when generally known ; on the contrary, it may be greatly advantagious to the Inventors at the Beginning, and every where serviceable on many Occasions, when it is put in Practice by those who use the Sea.

Signed *CHAZELLES*.

Mr. *de Chazelles* might have added, that the Chaloups that tow, are in close Fight liable to be sunk by
the

the Enemy's Cannon, and are exposed to the Waves by their having so little Height above Water.

It may be proper to explain the Advantages there are in boarding, or getting clear of the Enemy, by means of the new Oars.

The chief Advantage, and which includes all the rest, is, that let a Vessel crowd as much Sail as possible, the perpendicular Oars are always capable of increasing her Swiftnes, because the Rowers have only a Motion of three Foot to make one Way, and as much the contrary Way, in order to make the Oars describe four and fifty Foot Space in the Water, and that Motion of six Foot might be perform'd in two Seconds of Time, if the Oars met with no Resistance; consequently the Vessel must run four and fifty foot in two Seconds, that is, about six Leagues an Hour, before those revolving Oars be unserviceable, for then the Vessel would go as fast as the Oars could possibly move with a Diameter of eighteen Foot; and if it was necessary to make them move faster, it is only lengthning out their Diameter, and they would move so much the faster, without obliging the Rowers to increase their own Motion.

Mr. *Arnoult* was order'd to examine the new Oars; and he made his Report to the Court, that the Officers of the Gallies found, that they interfered with the Use of the Sails in a Gally, but might be of Use in other Vessels and Bomb-ketches; in Consequence of which, I was sent to *Toulon* to make the Experiment on Board a Bomb-ketch.

At the Time when the Experiment was made, Mr. *de Vauvre*, and the Officers of the *Marine* were at Sea, and only some Officers of the *Port* were pre-

sent, who sent a Verbal Process to the Court, without acquainting me with it, or offering any Objection, although I had very much press'd them to it, in order to obviate the Prejudices might be conceiv'd against this Novelty.

At my Return to *Paris*, Mr. *de Salabery*, surpriz'd at my knowing nothing of that Account, gave it me to answer, which I did Paragraph by Paragraph: The whole was given to a general Officer then at Court to examine, and make a Report of it, the Result of which was, That this Invention ought to be put in Practice.

XII. *Part of a Letter from the Reverend Mr. Rowlands, to the Reverend Mr. Derham, Prebendary of Windsor, and F. R. S. Concerning the stocking of the River Mene with Oysters.*

THE River of *Mene*, that divides *Anglesey* from *Carnarvonshire*, near which I live, has at present the Bottom of its Channel for some Miles in length; all bedded with good Oysters, in such Plenty, that in the Season, several Boats are daily employ'd to dredge them up, and have done so these eight or nine Years last past to their great Profit; but what I recommend as observable, is, that about twenty four Years ago, we have good Assurance, that there were none to be found on that Bottom: but that a Gentleman about that Time, caused three or four hundred large

large Oysters to be dropp'd into the Channel, just under his Land; from the Spat or Seed of which, it is most probable, the Flux and Reflex of Tides dispersing it, all the Bottom at length, where small Stones and a large Cultch received the Sperm, became cover'd with Oysters. And what favours this Conjecture, that they are a Brood of Oysters begun at that Time, is, that at the first finding, they appeared young and small, but have since yearly increased in Bulk and Plenty, though prodigious Quantities have been taken up of them.



I N D E X

To the *Thirty first* Volume of the *Philosophical Transactions*.

A.

- A**ir's Refraction allow'd for, n. 368, p. 169.
Appulses of the Planets to the fix'd Stars, n. 369, p. 209.
Artery Crural, ossify'd, n. 369, p. 226.
Aurora Borealis, Seen at Cambridge, n. 365, p. 66. At Dublin, n. 368, p. 180. At *Cruwys-Morehard* in Devonshire, n. 368, p. 186.

B.

- Barometer Pendent*, to measure the Height of Places by it, n. 366, p. 116. The *Quicksilver* seen at an extraordinary Height, n. 369, p. 222.
Bees, A Method of finding their Hives by *Trigonometry*, n. 367, p. 148.
Books, Accounts of them; I. *Geometria Organica Colini Maclaurin*, n. 364, p. 38. *Conghietture sopra l'infermità dell'Animali Bovini*, &c. n. 365, p. 83.

C.

- Cap of Lead*, To go out of the diving Bell with, at the Bottom of the Sea, n. 368, p. 177.
Colon, Part of it hanging out at a Wound in the Belly fourteen Years, n. 366, p. 89.
Cross Hairs in a Telescope, their Exactness in determining the right Ascension and Declination, n. 366, p. 113.

N n

Diving.

I N D E X.

D.

Diving-Bell improv'd, n. 368, p. 177.

E.

Experiments. To prove that Bodies of the same Bulk, do not contain equal Quantities of Matter, n. 365, p. 81. Electrical, n. 366, p. 104. To compare the Weights of *London* and *Paris*, n. 366, p. 112. Of the Resistance of Fluids, n. 367, p. 142. Relating to Magnetism, n. 368, p. 204. On the Adhesion of Fir-wood to Water, n. 368, p. 207.

F.

Fasting, an incredible Instance of it, n. 364, p. 28.
Fœtus, continuing 46 Years in the Body, n. 367, p. 126.

G.

Glands Salivary described, n. 364, p. 5.
Grapes, their Colour changed, n. 366, p. 102.
Guts, Part of the Colon hanging out at a Wound. n. 366, p. 126.

H.

Jessamine, its Colour changed, n. 366, p. 102.

L.

Leprosy. The *Pox* formerly call'd by that Name, n. 365, p. 55. n. 366, p. 108.
Ligament of the *Patella* broken, n. 365, p. 44.
Lymphaticks, their Course and Injections, n. 364, p. 5.

M.

Magnetical Needle, its Variation in the *Baltick*, n. 366, p. 120. In the *South-Sea*, n. 368, p. 173.
Maple-tree, Sugar made from its Juice, n. 364, p. 27.
Medicines, Their Operations, n. 365, p. 71.
Meteor, seen at *Dublin*, n. 364, p. 21. n. 368, p. 180.
At *Cambridge*, n. 365, p. 66. At *Cruwys-Morehard*, n. 368, p. 186.

Micro-

K N D E X.

Microscopical Observations, On the Bones and the *Periosteum*, n. 366, p. 91. On the Membrane enclosing the Muscular Fibres, n. 367, p. 129. On the Vessels of Wood, and the Muscular Fibres, *ibid*, p. 134. On the Muscular Fibres of Fish, n. 368, p. 190. On the Seeds of Plants, *ibid*. p. 200. On the Vessels in the Leaves of a Sort of Box, and on the Down of Peaches and Quinces, n. 369, p. 231.

Mock-Sun, seen near London, n. 369, p. 211. The same in Northamptonshire, *ibid*. p. 212.

Moose-Deer described, n. 368, p. 165.

O.

Offification of the Crural Artery observ'd, n. 369, p. 226.

Oysters, the River *Mene* stock'd with them, *ibid*. p. 250.

P.

Parhelion observ'd near London, n. 369, p. 211. In Northamptonshire, *ibid*. p. 212.

Plague at Constantinople, a Description of it, n. 364, p. 14.

Planets, their Places determin'd by their near Appulses to the fixed Stars, n. 369, p. 209.

Plants, a Method of discovering their Virtues by their external Structure, n. 364, p. 30. Observations upon their Generation, n. 369, p. 216.

Pox, its Antiquity in Europe, n. 365, p. 47. n. 366, p. 108.

Projectiles, some Propositions concerning their Motion, n. 367, p. 151.

R.

Rainbow seen on the Ground, n. 369, p. 229.

Refraction of the Air, what Allowance to be made for it, n. 368, p. 169. A Table for that purpose, *ibid*. p. 172.

S. Salt.

I M D E X.

S.

- Salt* Alkalious from rotten Wood, n. 366, p. 122.
Ships, a new Method of rowing them, n. 369, p. 239.
Sirius, his Parallax mistaken by M. Cassini, n. 364. p. 1.
Specifick Gravity of solid Bodies. A Caution to be observ'd in trying it, n. 369, p. 223.
Spine Cloven, n. 366. p. 98.
Stars fix'd, the Infinity of their Sphere, n. 364, p. 22.
Their Number, Order, and Light, *ibid.* p. 24.
Sugar made from the Juice of the Maple-tree, n. 364, p. 27.
Sun's Parallax, n. 366, p. 114.
Surgery. Two extraordinary Cases, n. 365, p. 79.

T.

- Telescope*. Exactness of the Method of observing the Right Ascension and Declination, by cross Hairs in it, n. 366. p. 113.
Tumour in the Loins of an Infant attended with a cloven Spine, n. 366, p. 98.

V.

- Variation* of the Needle in the *Baltick Sea*, n. 366, p. 120. In the *South Sea*, n. 368. p. 173.
Venercal Disease, Its Antiquity, n. 365, p. 47. n. 366, p. 108.

W.

- Wood* rotten producing Alcalious Salt, n. 366. p. 122.
Wood specifically heavier than Water, n. 369, p. 225.

F I N I S.

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