PHILOSOPHICAL
TRANSACTIONS:
GIVING SOME
ACCOMPT
OF THE PRESENT
Undertakings, Studies, and Labours
OF THE
INGENIOUS
IN MANY
CONSIDERABLE PARTS
OF THE
WORLD.

Vol I.
For Anno 1665, and 1666.

In the SAVOY,
Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar, and James Allestry in Duck-Lane,
Printers to the Royal Society.
A Philosophical Transactions giving some Account of the Present Undertakings, Studies, and Labours of the Ingenious Men in Many Considerable Part of the World.
TO THE
Royal Society.

I will not become me, to add any Attributes to a Title, which has a Fulness of Lustre from his Majesty's Denomination.

In these Rude Collections, which are only the Gleanings of my private diversions in broken hours, it may appear, that many Minds and Hands are in many places industriously employed, under Your Countenance and by Your Example, in the pursuit of those Excellent Ends, which belong to Your Heroical Undertakings.

Some of these are but the Intimations of large Compilments. And some Eminent Members of Your Society, have obliged the Learned World with Incomparable Volumes, which are not herein mention'd, because they were finish'd, and in great Reputation abroad, before I entred upon this Task. And no small Number are at present engaged for those weighty Productions, which
Epistle Dedicatory.

which require both Time and Assistance, for their due Maturity. So that no man can from these Glimpses of Light take any just Measure of Your Performances, or of Your Prosecutions; but every man may perhaps receive some benefit from these Parcels, which I guessed to be somewhat conformable to Your Design.

This is my Solicitude. That, as I ought not to be unfaithful to those Counsels you have committed to my Trust, so also that I may not altogether waste any minutes of the leisure you afford me. And thus have I made the best use of some of them, that I could devise; To spread abroad Encouragements, Inquiries, Directions, and Patterns, that may animate, and draw on Universal Assurances.

The Great God prosper You in the Noble Engagement of Dispersing the true Lustre of his Glorious Works, and the Happy Inventions of obliging Men all over the World, to the General Benefit of Mankind: So wishes with real Affections,

Your humble and obedient Servant

HENRY OLDENBURG.
The Contents.


The Introduction.

W

Hereas there is nothing more necessary for promoting the improvement of Philosophical Matters, than the communicating to such, as apply their Studies and Endeavors that way, such things as are discovered or put in practice by others; It is therefore thought fit to employ the Press, as the most proper way to gratifie those, whose engagement in such Studies, and delight in the advancement of Learning and profitable Discoveries, doth entitle them to the knowledge of what this Kingdom, or other parts of the World, do, from time to time, afford, as well
of the Progress of the Studies, Labors, and attempts of the Curious and Learned in things of this kind, as of their complete Discoveries and Performances: To the end, that such Productions being clearly and truly communicated, desires after solid and useful knowledge may be further entertained, ingenious Endeavors and Undertakings cherished, and those, addicted to and conversant in such Matters, may be invited and encouraged to search, try, and find out new things, impart their knowledge to one another, and contribute what they can to the Grand Design of improving Natural knowledge, and perfecting all Philosophical Arts, and Sciences. All for the Glory of God, the Honor and Advantage of these Kingdoms, and the Universal Good of Mankind.

An Accoount of the improvement of Optick Glasses.

There came lately from Paris a Relation, concerning the Improvement of Optick Glasses, not long since attempted at Rome by Signor Giuseppe Campani, and by him discoursed of, in a Book, Entitled, Ragguaglio di nuove Observationi, lately printed in the said City, but not yet transmitted into these parts; wherein these following particulars, according to the Intelligence, which was sent hither, are contained.

The First regardeth the excellency of the long Telescopes, made by the said Campani, who pretends to have found a way to work great Optick Glasses with a Turne-tool; without any Mould: And whereas hitherto it hath been found by Experience, that small Glasses are in proportion better, to see with, upon the Earth, then the great ones; that Author affirms, that his are equally good for the Earth, and for making Observations in the Heavens. Besides, he useth three Eye-Glasses for his great Telescopes, without finding any Iris, or such Rain-bow colors, as do usually appear in ordinary Glasses, and prove an impediment to Observations.

The Second, concerns the Circle of Saturn, in which he hath observed nothing, but what confirms Monsieur Christian Huygens de Zulichem his Systeme, of that Planet, published by that worthy Gentleman in the year, 1659.
The Third, respects Jupiter, wherein Campani affirms he hath observed by the goodness of his Glasses, certain protuberancies and inequalities, much greater than those that have been seen therein hitherto. He addeth, that he is now observing, whether those Stalis in the said Planet do not change their situation, which if they should be found to do, he judgeth, that Jupiter might then be said to turn upon his Axe; which, in his opinion, would serve much to confirm the opinion of Copernicus. Besides this, he affirms, he hath remarked in the Belts of Jupiter, the shades of his satellites, and followed them, and at length seen them emerge out of his Disk.

A Spot in one of the Belts of Jupiter.

The Ingenious Mr. Hook, did, some months since, intimate to a friend of his, that he had, with an excellent twelve foot Telescope, observed, some days before, he then spoke of it, (videl. on the ninth of May, 1664. about 9 of the Clock at night) a small Spot in the biggest of the 3 obscurer Belts of Jupiter, and that, observing it from time to time, he found, that within 2 hours after, the said Spot had moved from East to West, about half the length of the Diameter of Jupiter.

The Motion of the late Comet predicted.

There was lately sent, to one of the Secretaries of the Royal Society a Packet, containing some Copies of a Printed Paper, Entitled, The Ephemerides of the Comet, made by the same Person, that sent it, called Monsieur Auzout, a French Gentleman of no ordinary Merit and Learning. who desired, that a couple of them might be recommended to the said Society, and one to their President, and another to his Highness Prince Rupert, and the rest to some other Persons, nominated by him in a Letter that accompanied this present, and known abroad, for their singular abilities and knowledge in Philosophical Matters. The end of the Communication of this Paper was, That, the motion of the Comet, that hath lately appeared, having been predicted by the said Monsieur Auzout.
zout, after he had seen it (as himself affirms) but 4 or 5 times; the Virtuof of England, among others, might compare also their Observations with his Ephemerides, either to confirm the Hypothesis, upon which the Author had before hand calculated the way of this Star, or to undeceive him, if he be in a mistake. The said Author Dedicateth these his conceptions to the most Christian King, telling him, that he presents Him with a design, which never yet was undertaken by any Astronomer; all the World having been hitherto persuaded, that the motions of Comets were so irregular; that they could not be reduced to any Laws, and men having contented themselves, to observe exactly the places, through which they did pass; but no man, that he knows, having been so bold as to venture to foretell the places, through which they should pass, and where they should cease to appear: Whereas he exhibits here the Ephemerides, determining day by day, in what place of the Heavens this Comet shall be, at what hour it shall be in its Meridian, and at what hour it shall set; until its too great remoteness, or the approach of the Sun, hide it from our eyes. Descending to particulars, he faith, that this Star, being disengaged from the beams of the Sun, might have been observed, if his conjectures be good, ever since it hath been of 17 or 18 degrees Southern Latitude, and that about the middle of November last, and sooner, unless it have been too small: That however it hath been seen in Holland ever since the 2d. of December last, at which time, according to his reckoning, the Diurnal motion of the Comet should already amount to 17 or 18 minuts. He finds, that this Star moveth just enough in the Plan of a Great Circle, which inclineth to the Equinoctial about 30 degrees, and to the Ecliptick about 49 d. or 49 1⁄2, cutting the Equator at about 45 d. 1⁄2, and the Ecliptick at the 28 d. of Aries, or a little more. He faith just enough: because he thinks, there may perhaps be some parallaxe, which he wiseth could be determined.

Hence, (so he goes on) every one who pleaseth may see, in tracing the Comet upon the Globe, through, or by which Starres it hath passed and shall pass; adding, that there will be neither cause to wonder, that having descended to about 6. degr. beneath the Tropick of Capricorn, he hath remounted afterwards, and shall go on.
on ascending fo, as to pass the Equinoctial, and perhaps proceed to 15. degrees Northern Declination, if it do not disappear before that time, by reason of its remoteness; Nor to believe, that there have been two Comets, upon its being seen again the 31. of December; since, according to him, it ought to have been so, if it continue to move in a Great Circle.

Having hereupon shewed, how the motion is to be traced upon the Globe, he finds, that, according to his Calculation, this Comet was to pass the Tropic of Capricorn about the 16. of December, and being entered into the Sign of Virgo on the 20. of the same month, and having been in Quadrat with the Sun, it should still descend, until the 26. of December in the morning, and then enter into Leo; that having entered, the 28. of the same month, into Cancer, and been a little after that time, in its greatest Inclination to the Ecliptick, vid. in the 28. degree of Leo, it was to repass the Southern Tropic, over against the little Dogg, on the 29. of December about 9. or 10. of the clock in the morning, after it had been opposite to the Sun 2. or 3. hours before; and that on the 29. of December in the evening it should be in Gemini; and at the very beginning of the New year, enter into Taurus.

After this, our Author finds, that this Comet, according to his account, should pass the Equator on the 4. of January before noon, and that about 5. or 6. of the clock in the evening of that day it was to come into the Jaw of the Whale, and the 9. of the same, at 6. of the clock, it should come close to the small Starr of the Whale, which is in its way, a little below. At length, he finds that it was to enter into Aries on the 12. of January, and to cut the Ecliptick on the 16. of the same month about noon, at which time it was to be again in Quadrat with the Sun, whence drawing a little to above the Northern Line of Pisces; it should in his opinion cease to appear a little beyond that place, without going as far as to the middle of Aries; if so be that its remoteness make it not disappear sooner.

He continueth, and faith, that this Comet shall not arrive to the place over against the Line of Pisces, till the 10. of February, &c. that then its Diurnal motion shall not exceed 8. minutes; and not 5; minutes about the 20. of the same month: and that in the beginning,
ginning of March, if we see it so long, the said motion shall not exceed 4. minutes, and so shall be still diminishing; except the Comet become Retrograde, which, as very important, he would have well observed; as also, whether its motion will be about the end more or less swift, then he hath calculated it.

He subjoyneth, that the greatest way, which this Star could make in 24. hours, hath been 13. d. 25', and in one hour, about 34'; and thinking it probable, that about the time, when it made so much way, it should be nearest to the Earth, he concludes that its motion in 24. hours must be, in its least distance from the Earth, as about 3. to 14, or 1. to 4^5, and that its motion in one hour was to be the same least distance, as about 1. to 102^3.

But that, which he Judgeth most remarkable, is, that he found by his Calculation, that the said least distance should be on the 29. of December, when the Comet was opposite to the Sun; which he does not know whether it may not serve to decide the grand Question concerning the Motion of the Earth.

He taketh further notice, that the Tayl of the Comet was to turn Westward, with a point to the North, until the 29. of December, at which time it was to be opposite to the Sun, and that then the said Tayl was to look directly North; but that, after that time, the Tayl was to turn Eastward, and continue to do so, until it disappear; and that it shall draw a little towards the North, until the 8. or 10. of February, at which time the Tayl is to be parallel to the Equator, and if the Comet be yet seen for some time after, the Tayl shall go a little lower towards the South, but grow smaller.

He finds by his Hypothesis, that on the 2. of December, which is the first observation, that he hath heard of, this Star was to be about 7. times more remote from the Earth, then when it was in its Perigeeum; and that it will be again in an equal remoteness from the Earth, on the 27. of January, so that he is of opinion, that in case this Comet have not been seen before the 2. of December, it will not be seen any more after the 27. of January.

He wishes above all things, that it might be very exactly observed, at what Angle the way of the Comet cuts the Equator, and, most of all, the Ecliptick; that so it may be seen, whether there
there hath not been some Parallaxe in the Circle of his Motion; as also, that some observations could be had of its greatest descent beneath the Tropick of Capricorn in the more Southern parts, where he faith it would have been without Refractions; Moreover, of the Time, when it hath been in Quadrat with the Sun about the 20. of December, and that also very exact Observation might be made of the time of its being again in Quadrat with the Sun, which, according to him, was to be Januar. 16.

He wishes also, that some in Madagascar may have observed this Star; Seeing that it began to appear over the middle of that island, and palled twice over their Heads; he judgeth, that they have seen it before us. And he wishes lastly, that there were some intelligent person in Guiana to observe it there, seeing that within a few dates, according to his reckoning, it will pass over their Heads, and will not remove from thence but 8, or 10. degrees Northward, where he faith, it will disappear; thinking it improbable, that it can still appear, after the Sun shall have passed it.

This Account beareth date of the 2. January, new Style, 1665, and the Author thereof addeth this Note, That, seeing it could not be printed nor distributed so soon as he desired, he hath had the opportunity to verify it by some Observations, from which he affirms he hath found no sensible difference; or, if there be, that it proceeds only from thence, that the Stars have advanced, since his Globe was made. He concludeth, that if this continue, and the first Observations do likewise agree, or that the differences do arrive within the Times, ghesled by him, that he hopes, he shall determine both the Distance and the Magnitude of this Comet; and that perhaps one may be enabled to decide the Question of the Motion of the Earth. In the interim, he assureth, that he hath not changed the least number in his Calculations, and that Monseur Huygens, & several French Gentlemen, to whom, he faith, he hath given them long since, can bear him witness that he hath done so; as also many other friends of his, who saw upon his Globe, several daies before, the way of the Comet from day to day.

Thus far the Parisian Account of the Comet, which is here inserted at large, that the intelligent and curious in England may compare
compare their Observations therewith, either to verify these Predictions, or to shew wherein they differ; which is (as was also hinted above) the design of this Philosophical Prophet in dispersing his Conceptions, who declareth himself ready, in case he be mistaken in his reckoning, to learn another Hypothesis, to explicate these admirable appearances by

An Experimental History of Cold.

There is in the Press, a New Treatise, entitled, New Observations and Experiments in order to an Experimental History of Cold, begun by that Noble Philosopher, Mr. Robert Boyle, and in great part already Printed; He did lately very obligingly present several Copies of so much as was Printed, to the Royal Society, with a desire that some of the Members thereof might be engaged to peruse the Book, and select out of it for trial, the hints of such Experiments, as the Author therewithe might be either yet made or prosecuted. The Heads thereof are,

1. Experiments touching Bodies capable of Freezing others.
2. Experiments and Observations touching Bodies Disposed to be Frozen.
3. Experiments touching Bodies, Indisposed to be Frozen.
4. Experiments and Observations touching the Degrees of Cold in several Bodies.
5. Experiments touching the Tendency of Cold Upwards or Downwards.
6. Experiments and Observations touching the Preservation and Destruction of (Eggs, Apples, and other) Bodies by Cold.
7. Experiments touching the Expansion of Water and Aqueous Liquors by Freezing.
8. Experiments touching the Contraction of Liquors by Cold.
9. Experiments in Comfort, touching the Bubbles, from which the Levity of Ice is supposed to proceed.
10. Experiments about the Measure of the Expansion and the Contraction of Liquors by Cold.
12. Experiments touching a New way of estimating the Expansive
five force of Congelation, and of highly compressing Air without Engines.

13. Experiments and Observations touching the Sphere of Activity of Cold.

14. Experiments touching differing Mediums, through which Cold may be diffused.

15. Experiments and Observations touching Ice.

16. Experiments and Observations touching the duration of Ice and Snow, and the destroying of them by the Air, and several Liquors.

17. Considerations and Experiments touching the Primum Frigidum.

18. Experiments and Observations touching the Coldness and Temperature of the Air.

19. Of the strange Effects of Cold.

20. Experiments touching the weight of Bodies frozen and unfrozen.


This Treatise will be dispatched within a very short time, and would have been so ere this, if the extremity of the late Frost had not stopped the Press. It will be accompanied with some Discourses of the same Author, concerning New Thermometrical Experiments and Thoughts, as also, with an Exercitation about the Doctrine of the Antiperistasis: In the former whereof is first proposed this Paradox, That not only our Senses, but common Weather-glasses, may mis-inform us about Cold. Next, there are contained in this part, New Observations about the deficiencies of Weather-glasses, together with some considerations touching the New or Hermetical Thermometers. Lastly, they deliver another Paradox, touching the cause of the Condensation of the Air, and Ascent of water by Cold in common Weather-glasses. The latter piece of this part contains an Examen of Antiperistasis, as it is wont to be taught and proved: Of all which there will, perhaps, a fuller account be given by the Next.
An Account of a very odd Monstrous Calf.

By the same Noble person was lately communicated to the Royal Society an Account of a very Odd Monstrous Birth, produced at Limnington in Hampshire, where a Butcher, having caused a Cow (which cast her Calf the year before) to be covered, that she might the sooner be fatted, killed her when fat, and opening the Womb, which he found heavy to admiration, saw in it a Calf, which had begun to have hair, whose hinder Legs had no Joints, and whose Tongue was, *Cerberus*-like, triple, to each side of his Mouth one, and one in the midst; between the Fore-leggs & the Hinder-leggs was a great Stone, on which the Calf rid: The *Sternum*, or that part of the Breast, where the Ribs lye, was also perfect Stone; & the Stone, on which it rid, weighed twenty pounds and a half; the outside of the Stone was of Greenish colour, but some small parts being broken off, it appeared a perfect Freestone. The Stone, according to the Letter of Mr. David Thomas, who sent this Account to Mr. Boyle, is with Doctor Haughteyn of Salisbury, to whom he also referreth for further Information.

Of a Peculiar Lead-Ore of Germany, and the Use thereof.

There was, not long since, sent hither out of Germany from an inquisitive Physician, a List of several Minerals and Earths of that Country, and of Hungary, together with a Specimen of each of them; among which there was a kind of Lead-Ore, which is more considerable than all the rest, because of its singular use for Essays upon the Coppell, seeing that there is not any other Metal mixed with it. 'Tis found in the Upper-Palatinate, at a place called Freyung, and there are two sorts of it, whereas of one is a kind of Crystalline Stone, and almost all good Lead; the other not so rich, and more farinaceous. By the information, coming along with it, they are fetched, not from under the ground, but, the Mines of that place having lain long neglected, by reason of the Wars of Germany and the increase of Waters, the people, living there-
thereabout, take it from what their Forefathers had thrown away, and had lain long in the open Air. The use above-mentioned being considerable, the person, who sent it, hath been intreated, to inform what quantities may be had of it, if there should be occasion to send for some.

Of an Hungarian Bolus, of the same Effect with the Bolus Armenus.

The same person gave notice also, that, besides the Bolus Armenus, and the Terra Silesaca, there is an Earth to be found in Hungary about the river Tockay, thence called Bolus Tockaviensis, having as good effects in physic, as either of the former two, and commended by experience in those parts, as much as it is by Sennertus out of Crato, for its goodness.

Of the New American Whale-fishing about the Bermudas.

Here follows a Relation, somewhat more diverting, than the precedent Accounts; which is about the new Whale-fishing in the West-Indies about the Bermudas, as it was delivered by an understanding & hardy Sea-man, who affirmed he had been at the killing work himself. His account, as far as remembred, was this; That though hitherto all Attempts of mastering the Whales of those Seas had been unsuccessful, by reason of the extraordinary fierceness and swiftness of these monstrous Animals; yet the enterprise being lately renewed, and such persons chosen and sent thither for the work, as were resolved not to be baffled by a Sea-monster, they did prosper so far in this undertaking, that, having been out at Sea, neer the said Isle of Bermudas, seventeen times, and fastned their Weapons a dozen times, they killed in these expeditions two old Female-Whales, and three Cubs, whereof one of the old ones, from the head to the extremity of the Tayl, was 88. Foot in length, by measure; its Tayl being 23. Foot broad, the swimming Finn 26. Foot long, and the Gills three Foot long; having great bends underneath from the Nose to the Navil; upon her after-part, a Finn on the back; being within paved
paved (this was the plain Sea-mans phrase) with fat, like the Cawl of a Hog.

The other old one, he said, was some 60. Foot long. Of the Cubs, one was 33, the other two, much about 25, or 26. Foot long.

The shape of the Fish, he said, was very sharp behind, like the ridge of a house: the head pretty bluff, and full of bumps on both sides: the back perfectly black, and the belly white.

Their celerity and force he affirmed to be wonderful, infomuch that one of those Creatures, which he struck himself, towed the boat wherein he was, after him, for the space of six or seven Leagues, in 4 of an hours time. Being wounded, he faith, they make a hideous roaring, at which, all of that kind that are within hearing, come towards the place, where the Animal is, yet without striking, or doing any harm to the wary.

He added, that they struck one of a prodigious bigness, and by gues of above 100. foot long. He is of opinion, that this Fish comes neereft to that sort of Whales, which they call the *fin-bartes*; they are without teeth, and longer then the Greenland-Whales, but not so thick.

He said further, that they fed much upon Grass, growing at the bottom of the Sea; which, he affirmed, was seen by cutting up the great Bag, or Maw, wherein he had found in one of them about two or three Hogheads of a greenish grassy-matter.

As to the quantity and nature of the Oyl which they yield, he thought, that the largest sort of these Whales might afford seven or eight Tuns if well husbanded, although they had lost much this first time, for want of a good Cooper, having brought home but eleven Tuns. The Cubbs, by his relation, do yield but little, and that is but a kind of Jelly. That which the old ones render, doth candy, like Pork-Grease, yet burneth very well. He observed, that the Oyl of the Blubber is as clear and fair as any Whey: but that which is boil'd out of the Lean, interlarded, becomes as hard as Tallow, spattering in the burning; and that which is made of the Cawl, resembleth Hogs-grease.

One, but scarce credible, quality of this Oyl, he affirms to be, that though it be boiling, yet one may run ones hand into it without scalding; to which he adds, that it hath a very healing Vertue
Vertue for cuttings, lameness, &c. the part affected being anointed therewith. One thing more he related, not to be omitted, which is, that having told, that the time of catching these Fishes was from the beginning of March, to the end of May, after which time they appeared no more in that part of the Sea: he did, when asked, whither they then retired, give this Answer, That it was thought, they went into the Weed-beds of the Gulf of Florida, it having been observed, that upon their Fins and Tails they have store of Clams or Barnacles, upon which, he said, Rock-weed, or Sea-tangle did grow a hand long; many of them having been taken of them, of the bigness of great Oyster-shells, and hung upon the Governour of Bermudas his Pales.

A Narrative concerning the success of Pendulum-Watches at Sea for the Longitudes.

The Relation lately made by Major Holmes, concerning the success of the Pendulum-Watches at Sea (two whereof were committed to his Care and Observation in his last voyage to Guinea by some of our Eminent Virtuosi, and Grand Promoters of Navigation) is as followeth;

The said Major having left that Coast, and being come to the Isle of St. Thomas under the Line, accompanied with four Vessels, having there adjusted his Watches, put to Sea, and failed Westward, seven or eight hundred Leagues, without changing his course; after which, finding the Wind favourable, he steered towards the Coast of Africa, North-North-East. But having failed upon that Line a matter of two or three hundred Leagues, the Masters of the other Ships, under his Conduct, apprehending that they should want Water, before they could reach that Coast, did propose to him to steer their Course to the Barbadoes, to supply themselves with Water there. Whereupon the said Major, having called the Masters and Pilots together, and caused them to produce their Journals and Calculations, it was found, that those Pilots did differ in their reckonings from that of the Major, one of them eighty Leagues, another about an hundred, and the third, more; but the Major judging by his Pendulum-Watches, that they were only some thirty Leagues distant from the
the Isle of Fuego, which is one of the Isles of Cape Verd, and that they might reach it next day, and having a great confidence in the said Watches, resolved to steer their Course thither, and having given order so to do, they got the very next day about Noon, a sight of the said Isle of Fuego, finding themselves to fail directly upon it, and so arrived at it that Afternoon, as he had said. These Watches having been first Invented by the Excellent Monsieur Christian Hugens of Zulichem, and fitted to go at Sea, by the Right Honourable, the Earl of Kincardin, both Fellows of the Royal Society, are now brought by a New addition to a wonderfull perfection. The said Monsieur Hugens, having been informed of the success of the Experiment, made by Major Holmes, wrote to a Friend at Paris a Letter to this effect;

Major Holmes, at his return, hath made a relation concerning the usefullness of Pendulums, which surpriseth my expectation. I did not imagine that the Watches of this first Structure would succeed so well, and I had reserved my main hopes for the New ones. But seeing that those have already served so successfull, and that the other are yet more just and exact, I have the more reason to believe, that the Invention of Longitudes will come to its perfection. In the mean time, I shall tell you, that the States did receive my Proposition, when I desired of them a Patent for these New Watches, and the recompense set a part for the invention in case of success; and that without any difficulty they have granted my request, commanding me to bring one of these Watches into their Assembly, to explicate unto them the Invention, and the application thereof to the Longitudes; which I have done to their contentment. I have this week published, that the said Watches shall be exposed to sale, together with an Information necessary to use them at Sea: and thus I have broken the Ice. The same Objection, that hath been made in your parts against the exactness of these Pendulums, hath also been made here; to wit, that though they should agree together, they might fail both of them, by reason that the Air at one time might be thicker, then at another. But I have answered, that this difference, if there be any, will not be at all perceived in the Penduls, seeing that the continual Observations, made in Winter from day to day, until Summer, have shewed me, that they
they have always agreed with the Sun. As to the Printing of
the Figure of my New Watch, I shall defer that yet a while: but
it shall in time appear with all the Demonstrations thereof, togeth-
ner with a Treatise of Pendulums, written by me some days since,
which is of a very subtile Speculation.

The Character, lately published beyond the Seas, of an Eminent
Person, not long since dead at Tholouse, where he was a
Councillor of Parliament.

It is the deservedly famous Monsieur de Fermat, who was,
( faith the Author of the Letter ) one of the most Excellent
Men of this Age, a Genius so universal, and of so vast an ex-
tent, that if very knowing and learned Men had not given
testimony of his extraordinary merit, what with truth can be
said of him, would hardly be believed. He entertained a con-
stant correspondence with many of the most Illustrious Mathe-
maticians of Europe, and did excel in all the parts of Mathema-
tical Science: a Testimony whereof he hath left behind him in
the following Books.

A Method for the Quadrature of Parabola's of all degrees.
A Book De Maximis & Minimis, which serveth not only for
the determination of Problems of Plains and Solids, but also
for the invention of Tangents and Curve Lines, and of the Centres
of Gravity in Solids; and likewise for Numerical Questions.

An Introduction to the Doctrine of Plains and Solids, which
is an Analytical Treatise, concerning the solution of Plains and
Solids, which had been seen ( as the Advertiser affirms ) before
Monsieur Des Cartes had publish'd any thing upon this Subject.

A Treatise De Contactibus Sphericis, where he hath demonstrat-
ated in Solids, what Mr. Viet, Master of Requests, had but
demonstrated in Plains.

Another Treatise, wherein he establisbeth and demonstrateth
the two Books of Apollonius Pergaus, of Plains.

And a General Method for the dimension of Curve Lines, &c.
Besides, having a perfect knowledge in Antiquity, he was con-
sulted from all parts upon the difficulties that did emerge there-
in: he hath explained abundance of obscure places, that are
found
found in the Antients. There have been lately printed some of his Observations upon Athenæus; and he that hath interpreted Benedetto Castelli, of the Meafure of running waters, hath thence inserted in his Work a very handsome one upon an Epistle of Syntesius, which was so difficult, that the Jesuit Petavius, who hath commented upon this Author, acknowledges, that he could not understand it.

He hath also made many Observations upon Theon of Smyrne, and upon other Antient Authors; but most part of them are not found but scattered in his Epistles, because he did not write much upon these kinds of Subjects, but to satisfy the curiosity of his friends.

All these Mathematical Works, and all these curious searches in Antiquity, did not hinder this great Virtuoso from discharging the duties of his place with much affiduity, and with so much ability, that he hath had the reputation of one of the greatest Civilians of his Age.

But that, which is most of all surprising to many, is that with all that strength of understanding, which was requisite to make good these rare qualities, lately mentioned, he had so polite and delicate parts, that he composed Latin, French and Spanish Verses with the same elegance, as if he had lived in the time of Augustus, and passed the greatest part of his life at the Courts of France and Spain.

More particulars will perhaps be mention'd of the Works of this Rare person, when all things, that he hath publish'd, shall be recovered, and when liberty shall be obtained of his Worthy Son, to impart unto the World the rest of his Writings, hitherto unpublished.

Printed with Licence, For John Martin, and James Allifry, Printers to the Royal Society.
PHILOSOPHICAL
TRANSACTIONS.

Munday, April 3. 1665.

The Contents.

Extract of a Letter written from Rome, concerning the late Comet, and a New one. Extract of another Letter from Paris, containing some Reflexions on the precedent Roman Letter. An Observation concerning some particulars, further considerable in the Monster, that was mentioned in the first Papers of these Philosophical Transactions. Extract of a Letter written from Venice, concerning the Mines of Mercury in Friuly. Some Observations, made in the ordering of Silk-worms. An Account of Mr. Hook's Micrographia, or the Physiological descriptions of Minute Bodies, made by Magnifying Glasses.

Extract of a Letter, lately written from Rome, touching the late Comet, and a New one.

I cannot enough wonder at the strange agreement of the thoughts of that acute French Gentleman, Monsieur Arquon, in the Hypothesis of the Comets motion, with mine; and particularly, at that of the Tables. I have with the same method, whereby I find the motion of this Comet, easily found the Principle of that Author's Ephemerides, which he then thought not fit to declare; and 'tis this, that this Comet moves about the Great Dog, in so great a Circle, that that portion, which is descri-
bed, is exceeding small in respect of the whole circumference thereof, and hardly distinguishable by us from a straight line.

Concerning the New Comet you mention, I saw it on the 11. of February, about the 24. deg. of Aries, with a Northern latitude of 24. deg. 40. min. The cloudy weather hath not yet permitted me to see it in Andromeda, as others affirm to have done.

Extract of a Letter, written from Paris, containing some Reflections on part of the precedent Roman Letter.

As to the Hypothesis of Georg. Domenico Cassini, touching the motion of the Comet about the Great Dog in a Circle, whose Centre is in a straight line drawn from the Earth thorough the said Star, I believe it will shortly be publish'd in print, as a thought I lighted upon in discoursing with one of my Friends, who did maintain, that it turned about a Centre, because that its Perigee had been over against the Great Dog, as I had noted in my Ephemerides. This particular I did long since declare to many of my acquaintance, whereof some or other will certainly do me that right, as to let the world know it by the Press. I have added an Observation, which I find not, that Signior Cassini hath made, viz.: that there was ground to think, that the Comet of 1652. was the same with the present, seeing that, besides the parity of the swiftness of its motion, the Perigee thereof was also over against the Great Dog, if the Observations extant thereof, deceive not. But, to make it out, what ground I had for these thoughts, I said, that if they were true, the Comet must needs accomplish its revolution from 12. to 12. years, or thereabout. But, seeing it appears not by History, that a Comet hath been seen at those determinate distances of time, nor that over against the Perigee of all the other Comets, whereof particular observations are recorded, are always found Stars of the first Magnitude, or such others, as are very notable, besides other reasons, that might be alleged, I shall not pursue this speculation; but rather suggest
suggest what I have taken notice of in my reflexions upon former Comets, which is, that more of them enter into our Systeme by the sign of Libra and about Spica virginis, then by all the other parts of the Heavens. For, both the present Comet, and many others registred in History, have entred that way, and consequently passed out of it by the sign Aries; by which also many have entred.

I did found my Hypothesis upon three Observations only, viz. those of the 22, 26, and 31 of December. Nor have I done, as some have fancied of me, who having been able to observe the Comet, the 27, 28, 29, 30, and 31 of December, and to see the diminution of its motion, have judged, that I had only determined that diminution for the time to come, conform to the augmentation thereof in time pasted until the 29. of December. For January 1. (on which day I composd my Ephemerides) I knew not (nor any person here) that the motion of the Comet did diminish; but on the contrary, most men believed, it was not the same Comet. But Signior Caffini knows very well, that that was not necessary, seeing that two portions of a Tangent being given, and the Angles answering thereunto, it's ease to find the position and magnitude of its circle. The reason, which I think the true one, of the diminution of its Motion in Longitude, and of its Retrogradation, by me conjectured in my Ephemerides, I began to be assured of, Febr. 10. For until the sixth, the Comet had alwaies advanced, as Signior Caffini also hath very well noted: but after that day, I found that it returned in augmenting alwaies its Latitude. And I have constantly observed it, until March 8. between many Stars, which must be the same with these mention'd by Caffini, whereof the number was so great, that I think, I saw of them March 6. with one Aperture of my Glass, more then 40. or 50. and especially, above the head of Aries; but I did not particularly note the situation of more than 12. or 15; amongst which I have observed the position of the Comet since January 28. every day, when the weather did permit, viz. January 29. February 3, 6, 10, 19, 24, 26, 27. and March 6, 7.
7, 8. I left it on March 8. at the 18. of the Horn of Aries, almost in the same latitude; and I am apt to believe, it will be Eclipsed; which I wish I may be able to observe this evening, if it be not already passed.

If Signior Cassini hath observed it on those days that I have, he will be glad to find the conformity of our Observations. I shall only add, that on February 3. we were surprized, to see the Comet again much brighter than ordinary, and with a considerable Train. Some did believe, that it approach'd again to us. But having beheld it with a Telescope, I soon said, that it was joyned with two small Stars, whereof one was pretty bright, which I had already seen, on February 28. and 29: And this conjunction gave the Comet that brightness, as it happens to most of the Stairs of the fifth and sixth magnitude, where 2 or 3, or more are conjoined, which perhaps would shew but faintly single, though by reason of their proximity to one another, they appear but one Starr. Hence it was, that I assured my friends here, that the following days we should no more see it so bright, because I knew, that there were none such small bright Stars in the way, which by my former observations I conjectured it was to move.

An Observation imparted to the Noble Mr. Boyle, by Mr. David Thomas, touching some particulars further considerable in the Monster mentioned in the first Papers of these Philosophical Transactions.

Upon the strictest inquiry, I find by one, that saw the Monstrous Calf and stone, within four hours after it was cut out of the Cows belly, that the Breast of the Calf was not stony (as I wrote) but that the skin of the Breast and between the Legs and of the Neck (which parts lay on the smaller end of the stone) was very much thicker, then on any other part, and that the Feet of the Calf were so parted as to be like the Claws of a Dog. The stone I have since seen; it is bigger at one end
then the other, of no plain superficies, but full of little cavities. The stone, when broken, is full of small peble stones of an oval figure: its colour is gray like free-stone, but intermixt with veins of yellow and black. A part of it I have begg'd of Dr. Haughten for you, which I have sent to Oxford, whether a more exact account will be conveyed by the same person.

Extract of a Letter, lately written from Venice by the Learned Doctor Walter Pope, to the Reverend Dean of Rippon, Doctor John Wilkins, concerning the Mines of Mercury in Friuli; and a way of producing Wind by the fall of Water.

The Mines of Mercury in Friuli, a Territory belonging to the Venetians, are about a days journey and a half distant from Goritia Northwards, at a place call'd Idria, situated in a Valley of the Julian Alps. They have been, as I am inform'd, these 160. years in the possession of the Emperor, and all the Inhabitants speak the Slavonian Tongue. In going thither, we travell'd several hours in the best Wood I ever saw before or since, being very full of Firrs, Oker, and Beeches, of an extraordinary thickness, straitnes, and height. The Town is built, as usually Towns in the Alps are, all of wood, the Church only excepted, and another House wherein the Overseer liveth. When I was there, in August last, the Valley, and the Mountains too, out of which the Mercury was dugg, were of as pleasant a verdure, as if it had been in the midst of Spring, which they there attribute to the moistnes of the Mercury; how truly, I dispute not. That Mine, which we went into, the best and greatest of them all, was dedicated to Saint Barbara, as the other Mines are to other Saints, the depth of it was 125. paces, every pace of that Country being, as they inform'd us, more then 5 of our Feet. There are two ways down to it; the shortest perpendicular way is that, whereby they bring up the Mineral in great Buckets, and
by which oftentimes some of the workmen come up and down. The other, which is the usual way, is at the beginning not difficult, the descent not being much; the greatest trouble is, that in several places you cannot stand upright: but this holds not long, before you come to descend in earnest by perpendicular Ladders, where the weight of one's body is found very sensible. At the end of each Ladder, there are boards a cross, where we may breath a little. The Ladders, as was said, are perpendicular, but being imagined produced, do not make one Ladder, but several parallel ones. Being at the bottom, we saw no more then what we saw before, only the place, whence the Mineral came. All the way down, and the bottom, where there are several lanes cut out in the Mountain, is lined and propt with great pieces of Firr-trees, as thick as they can be set. They digg the Mineral with Pick-axes, following the veins: 'tis for the most part hard as a flone, but more weighty; of a Liver-colour, or that of Crocus Metallorum. I hope shortly to shew you some of it. There is also some soft Earth, in which you plainly see the Mercury in little particles. Besides this, there are oftentimes found in the Mines round stones like Flints, of several bignesses, very like those Globes of Hair, which I have often seen in England, taken out of Oxes bellys. There are also several Marcasites and stones, which seem to have specks of Gold in them; but upon tryal they say, they find none in them. These round stones are some of them very ponderous, and well impregnated with Mercury; others, light, having little or none in them. The manner of getting the Mercury is this: They take of the Earth, brought up in Buckets, and put it into a Sive, whose bottom is made of wires at so great a distance, that you may put your finger betwixt them: 'tis carried to a stream of running water, and wash'd as long as anything will pass through the Sive. That Earth which pafleth not, is laid aside upon an other heap: that which pafleth, reserved in the hole, in Fig. 1 and taken up again by the second Man, and so on, to about ten or twelve fives proportionally less. It often happens in the first hole, where the second Man takes up his Earth
Earth, that there is Mercur\(\text{y}\) at the bottom, but towards the farther end, where the intervals of the wires are less, 'tis found in very great proportion. The Earth laid aside is pounded, and the same operation repeated. The fine small Earth, that remains after this, and out of which they can wash no more Mercur\(\text{y}\), is put into Iron retorts and stop'd, because it should not fall into the Receivers, to which they are luted. The fire forces the Mercur\(\text{y}\) into the Receivers: The Officer unluted several of them to shew us; I observed in all of them, that he first poured out perfect Mercur\(\text{y}\), and after that came a black dust, which being wetted with water discover'd it self to be Mercur\(\text{y}\), as the other was. They take the Caput mortuum and pound it, and renew the operation as long as they can get any Mercur\(\text{y}\) out of it.

This is the way of producing the Mercur\(\text{y}\), they call Ordinary, which exceeds that, which is got by washing, in a very great proportion, as you will perceive by the account annex'd. All the Mercur\(\text{y}\) got without the use of Fire, whether by washing, or found in the Mines (for in the digging, some little particles get together, so that in some places you might take up two or three spoonfuls of pure Mercur\(\text{y}\)) is call'd by them Virgin-Mercur\(\text{y}\), and esteem'd above the rest. I inquir'd of the Officer what vertue that had more, then the other; he told me that making an Amalgama of Gold and Virgin-Mercur\(\text{y}\), and putting it to the fire, that Mercur\(\text{y}\) would carry away all the Gold with it, which common Mercur\(\text{y}\) would not do.

The Engins, employed in these Mines, are admirable; the Wheels, the greatest that ever I saw in my life; one would think as great as the matter would bear: all moved by the dead force of the water, brought thither in no chargeable Aqueduct from a Mountain, 3 Miles distant: the water pump't from the bottom of the Mine by 52 pumps, 26 on a side, is contriv'd to move other wheels, for several other purposes.

The Labourers work for a Julio a day, which is not above 6 or 7 pence, and indure not long; for, although none stay under
underground above 6 hours, all of them in time (some later, some sooner) become paralytic, and dye. We saw there a man, who had not been in the Mines for above half a year before, so full of Mercury, that putting a piece of Brass in his mouth, or rubbing it in his fingers, it immediately became white like Silver; I mean, he did the same effect, as if he had rubbed Mercury upon it, and so paralytic, that he could not with both his hands carry a Glass half full of Wine, to his mouth without spilling it, though he loved it too well to throw it away.

I have been since informed, that here in Venice, those that work on the back-side of Looking-glasses, are also very subject to the Palsy. I did not observe, that they had black Teeth; it may be therefore, that we accuse Mercury unjustly for spoiling the Teeth, when given in Venereal diseases. I confess, I did not think of it upon the place; but, black Teeth being so very rare in this Country, I think I could not but have marked it, had all theirs been so.

They use exceeding great quantity of Wood, in making and repairing the Engines, and in the Furnaces (whereof there are 16, each of them carrying 24 Retorts;) but principally in the Mines, which need continual reparation, the Fir-trees lasting but a small time underground. They convey their Wood thus: About four miles from the Mines, on the sides of two mountains, they cut down the Trees, and draw them into the interjacent Valley, higher in the same Valley, so that the Trees, according to the descent of the water, freeze betwixt it and Idria; with vast charges and quantities of Wood they make a Lock or Dam, that suffers not any water to pass; they expect afterwards, till there be water enough to float these Trees to Idria; for, if there be not a spring, (as generally there is,) Rain, or the melting of the Snow, in a short time, afford so much water, as is ready to run over the Dam, and which (the Flood-gates being open'd) carries all the Trees imperiously to Idria, where the Bridge is built very strong, and at very oblique Angles to the stream, on purpose to stop them, and throw them on shore near the Mines.
Thofe Mines cost the Emperor heretofore 70000. or 80000. Florens yearly, and yeilded less Mercury then at present, although it costs him but 28000. Florens now. You may see what his Imperial Majefty gets by the following account, of what Mercury the Mines of Idria have produced these last three years.

<table>
<thead>
<tr>
<th>Year</th>
<th>Ordinary Mercury</th>
<th>Virgin Mercury</th>
</tr>
</thead>
<tbody>
<tr>
<td>1661</td>
<td>198481</td>
<td>6194</td>
</tr>
<tr>
<td></td>
<td>204675</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1662</td>
<td>225066</td>
<td>9612</td>
</tr>
<tr>
<td></td>
<td>234678</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1663</td>
<td>244119</td>
<td>11862</td>
</tr>
<tr>
<td></td>
<td>255981</td>
<td></td>
</tr>
</tbody>
</table>

There are alwaies at work 280 persons, according to the relation I received from a very civil person, who informed me also of all the other particulars above-mentioned, whose name is Achatio Kappenjager; his Office, Contra-scrivano per sua Maefia Cefarea in Idria del Mercurio.

To give some light to this Narrative, take this Diagramme: F. is the water, C. B. a vessel, into which it runs. D.G.E.H.F.I. are streams, perpetually issuing from that vessel: D.E.F. three lines, the distance of whose wires at bottom lessen proportionably. C. the place, wherein the Earth, that pafs’d through the five D. is retained; from whence ’tis taken by the second man; and what paffes through the five E. is retained in H, and so of the rest. K.L. M. wants water, which is so much impregnated with Mercury, that it cureth Itches and fordid Ulcers. See Fig. 1.

I will trespass a little more upon you, in describing the contrivance of blowing the Fire in the Brassworks of Tivoli neer Rome (it being new to me) where the Water blows the Fire, not by moving the Bellows, (which is common) but by affording the Wind. See Fig. II. where A. is the River
River, B. the Fall of it, C. the Tub into which it falls, L. a Pipe, G. the orifice of the Pipe, or Nose of the Bellows, G. the Hearth, E. a hole in the Pipe, F. a stopper to that hole, D. a place under ground, by which the water runs away. Stopping the hole E, there is a perpetual strong wind, issuing forth at G; and G, being stop'd, the wind comes out so vehemently at E, that it will, I believe, make a Ball play, like that at Frescati.

An Extract of a Letter, containing some Observations, made in the ordering of Silk-worms, communicated by that known Vertuoso, Mr. Dudley Palmer, from the ingenuous Mr. Edward Digges.

I herewith offer to your Society a small parcel of my Virginian silk. What I have observed in the ordering of Silk-worms, contrary to the received opinion, is:

1. That I have kept leaves 24 hours after they are gathered, and flung water upon them to keep them from withering; yet when (without wiping the leaves) I fed the worms, I observed, they did as well as those fresh gathered.

2. I never observed, that the smell of Tobacco, or smells that are rank, did any waies annoy the worm.

3. Our Country of Virginia is very much subject to Thunders: and it hath thundered exceedingly when I have had worms of all sorts, some newly hatched; some half way in their feeding; others spinning their Silk; yet I found none of them concern'd in the Thunder, but kept to their business, as if there had been no such thing.

4. I have made many bottoms of the Brooms (wherein hundreds of worms spun) of Holly; and the prickles were so far from hurting them, that even from those prickles they first began to make their bottoms.

I did hope with this to have given you assurance, that by retarding the hatching of seed, two crops of Silk or more might
might be made in a Summer: but my servants have been remiss in what was ordered, I must crave your patience till next year.

An Account of Micrographia, or the Physiological Descriptions of Minute Bodies, made by Magnifying Glasses.

The Ingenious and knowing Author of this Treatise, Mr. Robert Hook, considering with himself, of what importance a faithful History of Nature is to the establishing of a solid Systeme of Natural Philosophy, and what advantage Experimental and Mechanical knowledge hath over the Philosophy of discourse and disputation, and making it, upon that account, his constant business to bring into that vast Treasury what portion he can, hath lately published a Specimen of his abilities in this kind of study, which certainly is very welcome to the Learned and Inquisitive world, both for the New discoveries in Nature, and the New Inventions of Art.

As to the former, the Attentive Reader of this Book will find, that there being hardly any thing so small, as by the help of Microscopes, to escape our enquiry, a new visible world is discovered by this means, and the Earth shews quite a new thing to us, so that in every little particle of its matter, we may now behold almost as great a variety of creatures, as we were able before to reckon up in the whole Universe itself. Here our Author maketh it not improbable, but that, by these helps the subtilty of the composition of Bodies, the structure of their parts, the various texture of their matter, the instruments and manner of their inward motions, and all the other appearances of things, may be more fully discovered; whence may emerge many admirable advantages towards the enlargement of the Active and Mechanick part of knowledge, because we may perhaps be enabled to discern the secret workings
workings of Nature, almost in the same manner, as we do those that are the productions of Art, and are managed by Wheels, and Engines, and Springs, that were devised by Humane wit. To this end, he hath made a very curious Survey of all kinds of bodies, beginning with the Point of a Needle, and proceeding to the Microscopical view of the Edges of Razors, Fine Lawn, Tabby, Watered Silks, Glass-canes, Glass-drops, Fiery Sparks, Fantastical Colours, Metallic line Colours, the Figures of Sand, Gravel in Urine, Diamonds in Flints, Frozen Figures, the Kettering Stone, Charcoal, Wood and other Bodies petrified, the Pores of Cork, and of other substances, Vegetables growing on blighted Leaves, Blew mould and Mushrooms, Sponges, and other Fibrous Bodies, Sea-weed, the Surfaces of Some Leaves, the Stinging points of a Nettle, Cowage, the Beard of a wild Oat, the seed of the Corn-violet, as also of Tyme, Poppy and Purslane. He continues to describe Hair, the scales of a Soal, the sting of a Bee, Feathers in general, and in particular those of Peacocks; the feet of Flies; & other Insects; the Wings and Head of a Fly; the Teeth of a Snail; the Eggs of Silk-worms; the Blue Fly, a water Insect; the Tufted Gnat; a White Moth; the Shepherds-spider; the Hunting Spider, the Ant; the wandering Mite; the Crab-like insect, the Book-worm, the Flea, the Louse, Mites, Vine-mites. He concludes with taking occasion to discourse of two or three very considerable subjects, viz. The inflexion of the Rays of Lights in the Air; the Fixt stars; the Moon.

In representing these particulars to the Readers view, the Author hath not only given proof of his singular skil in delineating all sorts of Bodies (he having drawn all the Schemes of these 60 Microscopical objects with his own hand) & of his extraordinary care of having them so curiously engraved by the Masters of that Art; but he hath also suggested in the several reflections, made upon these Objects, such conjectures, as are likely to excite and quicken the Philosophicall heads to very noble contemplations. Here are found inquiries concerning the Propagation of Light through differing mediums,
diurns; concerning Gravity, concerning the Roundness of Fruits, stones, and divers artificial bodies; concerning Springiness and Tenacity; concerning the Original of Fountains; concerning the dissolution of Bodies into Liquors; concerning Filtration, and the ascent of Juices in Vegetables, and the use of their Pores. Here an attempt is made of solving the strange Phenomena of Grass-drops; experiments are alleged to prove the Expansion of Glass by heat, and the Contraction of heated-Glass upon cooling; Des Cartes his Hypothesis of colours is examined: the cause of Colours, most likely to the Author, is explained: Reasons are produced, that Reflection is not necessary to produce colours, nor a double refraction: some considerable Hypotheses are offered, for the explication of Light by Motion; for the producing of all colors by Refraction; for reducing all sorts of colors to two only, Yellow and Blew; for making the Air, a dissolvent of all Combustible Bodies: and for the explicating of all the regular figures of Salt, where he alleges many notable instances of the Mathematicks of Nature, as having even in those things which we account vile, rude and coarse, shewed abundance of curiosity and excellent Geometry and Mechanism. And here he opens a large field for inquiries, and proposeth Models for prosecuting them; 1. By making a full collection of all the differing kinds of Geometrical figures of bodies; 2. By getting with them an exact History of their places where they are generated or found: 3. By making store of Tryals in Dissolutions and Coagulations of several Crystallizing Salts: 4. By making trials on-metals, Minerals and stones, by dissolving them in several Menstruums, and Crystallizing them, to see what Figures will arise from those several compositums: 5. By compounding & coagulating several Salts together into the same mass, to observe the Figure of that product: 6. By inquiring the closeness or rarity of the texture of those bodies, by examining their gravity, and their refraction, &c. 7. By examining what operations the fire hath upon several kinds of Salts, what changes it causes in their figures, Textures, or
Vertues. 8. By examining their manner of dissolution, or acting upon those bodies dissoluble in them; and the Texture of these bodies before and after the process. 9. By considering, by what and how many means, such and such figures, actions and effects could be produced, and which of them might be the most likely, &c.

He goes on to offer his thoughts about the Pores of bodies, and a kind of Valves in wood; about spontaneous generation arising from the Putrefaction of bodies; about the nature of the Vegetation of mould, mushrooms, moss, sponges; to the last of which he scarce finds any Body like it in texture. He adds, from the natural contrivance, that is found in the leaf of a Nettle, how the stinging pain is created, and thence takes occasion to discourse of the poisoning of Darts. He subjoins a curious description of the shape, Mechanism and use of the sting of a Bee; and shews the admirable Providence of Nature in the contrivance and fabrick of Feathers for Flying. He delivers those particulars about the Figure, parts and use of the head, feet, and wings of a Fly, that are not common. He observes the various ways of the generations of Insects, and discourses handsomely of the means, by which they seem to act so prudently. He taketh notice of the Mechanical reason of the Spider's Fabrick, and maketh pretty Observations on the hunting Spider, and other Spiders and their Webs. And what he notes of a Flea, Louse, Mites, and Vinegar-worms, cannot but exceedingly please the curious Reader.

Having dispatched these Matters, the Author offers his Thoughts for the explicating of many Phenomena of the Air, from the Inflexion, or from a Multiplicate Refraction of the rays of Light within the Body of the Atmosphere, and not from a Refraction caused by any terminating superficies of the Air above, nor from any such exactly defined superficies within the body of the Atmosphere: which conclusion he grounds upon this, that a medium, whose parts are unequally dense, and mov'd by various motions and transpositions as to one another, will produce all these visible
visible effects upon the rays of Light, without any other coefficient cause: and then, that there is in the Air or Atmosphere, such a variety in the constituent parts of it, both as to their density and rarity, and as to their divers mutations and positions one to another.

He conclueth with two Celestial Observations; whereof the one imports, what multitudes of Stars are discoverable by the Telescope, and the variety of their magnitudes: intimating with all, that the longer the Glasses are, and the bigger apertures they will indure, the more fit they are for these discoveries: the other affords a description of a Vale in the Moon, compared with that of Hevelius and Ricciolo; where the Reader will find several curious and pleasant Annotations, about the Pits of the Moon, and the Hills and Coverings of the same; as also about the variations in the Moon, and its gravitating principle, together with the use, that may be made of this instance of a gravity in the Moon.

As to the Inventions of Art, described in this Book, the curious Reader will there find these following:

1. A Baroscope, or an Instrument to shew all the Minute Variations in the Pressure of the Air, by which he affirms, that he finds, that before and during the time of rainy weather, the Pressure of the Air is less; and in dry weather, but especially when an Easterly Wind (which having passed over vast Tracts of Land, is heavy with earthy particles) blows, it is much more, though these changes be varied according to very odd Laws.

2. A Hygroscope, or an Instrument, whereby the Watery Steams, volatile in the Air, are discerned, which the Nose itself is not able to find. Which is by him fully described in the Observation touching the Beard of a Wild Oate, by the means whereof this Instrument is contrived.

3. An Instrument for graduating Thermometers, to make them Standards of Heat and Cold.

4. A New Engin for Grinding Optick Glasses, by means of which he hopes, that any Spherical Glasses, of what length, ever,
foever, may be speedily made: which seems to him most easie, because, if it succeeds, with one and the same Tool may be ground an Object Glass of any length or breadth requisite, and that with very little or no trouble in fitting the Engin, and without much skill in the Grinder. He thinks it very exact, because to the very last stroke the Glass does regulate and rectifie the Tool to its exact Figure; and the longer or more the Tool and Glass are wrought together, the more exact will both of them be of the desired Figure. He affirms further, that the motions of the Glass and Tool do so cross each other, that there is not one point of either's surface, but hath thousands of cross motions thwarting it, so that there can be no kind of Rings or Gutters made, either in the Tool or Glass.

5. A New Instrument, by which the Refraction of all kinds of Liquors may be exactly measured, thereby to give the Curious an opportunity of making Trials of that kind, to establish the Laws of Refraction, to wit, whether the Sines of the Angles of Refraction are respectivly proportionable to the Sines of the Angles of Incidence: This Instrument being very proper to examine very accurately, and with little trouble, and in small quantities, the Refraction of any Liquor, not only for one inclination, but for all; whereby he is enabled to make accurate Tables. By the same also he affirms to have found it true, that what proportion the Sine of the Angle of the one inclination has to the Sine of its Angle of Refraction, correspondent to it, the same proportion have all the other Sines of Inclination to their respective Sines of Refractions.

Lastly, this Author despair's not that there may be found many Mechanical Inventions, to improve our Senses of Hearing, Smelling, Tasting, Touching, as well as we have improved that of Seeing by Optick Glasses.

London, Printed with Licence for John Martyn, and James Allestry, Printers to the Royal Society.
The Contents.

Some Observations and Experiments upon May-Dew. The Motion of the Second Comet predicted, by the same person, who predicted that of the former. A Relation of the Advice, given by a French Gentleman, touching the Conjunction of the Ocean and the Mediterranean. Of the way of killing Rattle-snakes, used in Virginia. A Relation of Persons kill'd with Subterraneous Damps. Of the Mineral of Liege, yielding both Brimstone, and Vitriol, and the way of extracting them out of it, used at Liege. An Account of Mr. Boyle's Experimental History of Cold.

Some Observations and Experiments upon May-Dew.

That ingenious and inquisitive Gentleman, Master Thomas Henshaw, having had occasion to make use of a great quantity of May-dew, did, by several casual Essays on that Subject, make the following Observations and Tryals, and present them to the Royal Society.
That Dew newly gathered and filtered through a clean Linnen cloth, though it be not very clear, is of a yellowish Colour, somewhat approaching to that of Urine.

That having endeavoured to putrefy it by putting several proportions into Glass bodies with blind heads, and letting them in several heats, as of dung, and gentle baths, he quite failed of his intention: for heat, though never so gentle, did rather clarify, and preserve it sweet, though continued for two months together, then cause any putrefaction or separation of parts.

That exposing of it to the Sun for a whole Summer in Glasse, that hold about two Gallons, with narrow mouths, that might be stop'd with Cork, the only considerable alteration, he observed to be produced in it, was, that Store of green stuff (such as is seen in Summer in ditches and standing waters) floated on the top, and, in some places, grew to the sides of the Glass.

That putting four or five Gallons of it into a half Tub, as they call it, of Wood, and straining a Canvas over it, to keep out Dust and Insects, and letting it stand in some shady room for three weeks or a month, it did of itself putrefy and stink exceedingly, and let fall to the bottom a black sediment like Mudd.

That, coming often to see, what Alterations appeared in the putrefaction, He observed, that at the beginning, within twenty four hours, a slimy film floated on the top of the water, which after a while falling to the bottom, there came another such film in its place.

That if Dew were put into a long narrow Vessel of Glass, such as formerly were used for Receivers in distilling of 
Aqua Fortis, the slime would rise to that height, that He could take it off with a Spoon; and when he had put a pretty quantity of it into a drinking Glass, and that it had stood all night, and the water drained from it, if He had turned it out of his hand, it would stand upright in figure of the Glass, in substance like boiled white Starch, though something more transparent, if his memory (I think he) fail him not.

That having once gotten a pretty quantity of this gelly, and put it into a Glass-body and Blind-head, He set it into a gentle Bath;
Bath, with an intention to have putrefied it, but after a few days he found, the head had not been well luted on, and that some moisture exhaling, the gelly was grown almost dry, and a large 
Mushroom grown out of it within the Glass. It was of a loose watrish contexture, such an one, as he had seen growing out of rotten wood.

That having several Tubs with good quantity of Dew in them, set to putrefy in the manner above said, and coming to pour out of one of them to make use of it, he found in the water a great bunch, bigger than his fist, of those Insects, commonly called Hog-lace or Millepedes, tangled together by their long tails, one of which came out of every one of their bodies, about the bigness of a Horsehair: The Insects did all live and move, after they were taken out.

That emptying another Tub, whereon the Sun, it seems, had used sometimes to shine, and finding, upon the straining it through a clean linnen cloth, two or three spoonfulls of green stuff, though not so thick nor so green as that above mentioned, found in the Glasses purposely expos'd to the Sun, he put this green stuff in a Glass, and tied a paper over it, and coming some dayes after to view it, he found the Glass almost filled with an innumerable Company of small Flyes, almost all wings, such as are usually seen in great Swarms in the Aire in Summer Evenings.

That setting about a Gallon of this Dew (which, he faith, if he misremember not, had been first putrefied and strained) in an open Jarre-Glass with a wide mouth, and leaving it for many weeks standing in a South-window, on which the Sun lay very much, but the Cailments were kept close shut; after some time coming to take account of this Dew, he found it very full of little Insects with great Heads and small tapering Bodies, somewhat resembling Tadpoles, but very much less. These, on his approach to the Glass, would sink down to the bottom, as it were to hide themselves, and upon his retreat wriggle themselves up to the top of the water again. Leaving it thus for some time longer, he afterwards found the room very full of Gnats, though the Door and Windows were kept shut. He adds, that he did not at first suspect, that those Gnats had any relation
relation to the Dew, but after finding the Gnats to be multiplied
and the little watry Animals to be much lessened in quantity,
and finding great numbers of their empty skins floating on the
face of his Dew, he thought, he had just reason to persuade him-
self, the Gnats were by a second Birth produced of those little
Animals.

That vapouring away great quantities of his putrescied Dew
in Glass Basons, and other Earthen glazed Vessels, he did at
last obtain, as he remembers, above two pound of Grayish Earth,
which when he had washed with more of the same Dew out of
all his Basons into one, and vapoured to fixation, lay in leaves
one above another, not unlike to some kind of brown Paper,
but very friable.

That taking this Earth out, and after he had well ground it
on a Marble, and given it a smart Fire, in a coated Retort of
Glass, it soon melted and became a Cake in the bottom, when
it was cold, and looked as if it had been Salt and Brimstone in
a certain proportion melted together; but, as he remembers,
was not at all inflammable. This ground again on a Marble, he
faith, did turn Spring water of a reddish purple Colour.

That by often calcining and filtering this Earth, he did at last
extract about two ounces of a fine small white Salt, which, look'd
on through a good Microscope, seemed to have Sides and Angles
in the same number and figure, as Rockpeeter.

The Motion of the Second Comet predicted,
by the same Gentleman, who predicted that
of the former.

Monsieur Augout, the same Person, that not long since com-
municated to the World his Ephemerides touching the course of
the former Comet, and recommended several Copies of them to
the Royal Society, to compare their Observations with his Ac-
count, and thereby, either to verify his Predictions, or to shew,
wherein they differ, hath lately sent another Ephemerides con-
cerning the Motion of the Second Comet, to the same end, that
invited him to send the other.
In that Tract he observes, first in General, that this second Comet is contrary to the precedent, almost in all particulars: seeing that the former moved very swift, this, pretty slow; that, against the Order of the Signs from East to West, this, following them, from West to East: that, from South to North, this, from North to South, as far as it hath been hitherto, that we hear off, observed: that, on the side opposite to the Sun, this, on the same side: that, having been in its Perigee at the time of its Opposition, this, having been there, out of the time of its Conjunction: where he taketh also notice, that this Comet differs in brightness from the other, as well in its Body, which is far more vivid and distinct, as in its Train, whose splendor is much greater, since it may be seen even with great Telescopes, which were useless in the former, by reason of its dimness. After this he descends to particulars, and informs us, that he began to observe this Comet April the second, and continued for some days following, and that as soon as he had made three or four Observations, he resolved to try again an Ephemerides; but that, having no instruments exact enough, and the Comet being in a place, destitute of Stars, and subject to Refractions, he feared to venture too much upon Observations so near one another, since in such matters a perfect exactness is necessary, and wished to see some precedent Observations to direct him: which having obtained, he thereby verified what he had begun, and resolved to carry on his intended Ephemerides, especially being urged by his Friends, and engaged by his former undertaking, that so it might not be thought a mere hazard, that made him hit in the former; as also, that he might try, whether his Method would succeed as well in flower, as in swifter Comets, and in those, that are near the Sun; as in such as are opposite thereunto, to the end, that men might be advertized of the determination of its use, if it could not serve but in certain particular Cases.

He relates therefore, that he had finish'd this New Ephemerides April the sixth, and put it presently to the Press; in doing of which, he hopes, he hath not disoblige'd the Publick: seeing that, though we should lose the light of this Star within a few days, by reason of its approach to the Sun, yet having found,
that it is always to rise before the Sun, and that we may again see it better, when it shall rise betimes, towards the end of May, and in the beginning of June, if the cleerness of the Day-break hinder us not; he thought it worth the while to try, whether the truth of this Ephemerides could be proved.

He affirms then, that the Line described by this Star resembles hitherto a Great Circle, as it is found in all other Comets in the midst of their Course. He finds the said Circle inclined to the Ecliptique about 26. d. 30'. and the Nodes, where it cuts it, towards the beginning of Gemini and Sagittary: that it declines from the Equator about 26. d. and cuts it towards the 11. d. and consequently, that its greatest Latitude hath been towards Piscers, where it must have been March 24. and its greatest Declination, towards the 25. d. of the Equator, where it was to have been April 11.

He puts it in its Perigee March 27. about three of the Clock in the Afternoon, when it was about the 15. degrees of Piscers, a little more Westerly than Marchab, or the Wing of Pegasus, and that it was to be in Conjunction with the Sun, April 9. Where yet he noteth, that according to another Calculation, the Perigee was March 27. more towards Night, so that the Comet advances a little more towards the East, and retards towards the West; which not being very sensible in the first days, differs more about the end, and in the beginning; which he leaves to Observation.

He calculateth, that the greatest Motion it could make in one day, hath been 4. d. and 8'. or 9'; in one hour, about 10'. and 25". so that its Diurnal Motion is to its left distance from the Earth a little more than as 1. to 14. and its Hourly Motion, as 1. to 330.

He wonders, that it hath not been seen sooner; the first Observations that he hath seen, but made by others, being of March 17. Whereas he finds, that it might have been seen since January, at least in the Months of February and March, when it rose at 2 of the Clock and before; because it is very likely, that, considering its bigness and brightness, when it was towards its Perigee, it was visible; since that towards the end of February it was not three times as much remote from the Earth, than when it was in its Perigee, and that towards the end of January it was not five times as much.
In the interim, faith he, the other Comet could be seen with the naked eye until January 31, when it was more than ten times further remote, than in its Perigee, although it was not by far so bright, nor its streamer shining as this hath appeared.

He wishes, that all the changes that shall fall out in this Comet, might be exactly observ'd; because of its not being swift, and the Motion of the Earth very sensible, unless the Comet be extreamly remote, we should find much more light from this, than the former Star, about the Grand Question, whether the Earth moves or not: this Author having all along entertained himself with the hopes, that the Motion of Comets would evince, whether the Earth did move or not; and this very Comet seemed to him to have by design appeared for that end, if it had had more Latitude, and that consequently we might have seen it before Day-break. He wishes also, that, if possible, it may be accurately observed, whether it will not a little decline from its great Circle towards the South; Judging, that some important truth may be thence deduced, as well as if its motion retarded more, than the place of its Perigee (which will be more exactly known when all the pasted Observations shall have been obtained) and its greatest Motion doe require.

He fears only, that it being then to rise at Break of Day, exact Observations cannot be made of it: but he would, at least have it sought with Telescopes, his Ephemerides directing where about it is to be.

April 10. it was to be over against the point of the Triangle; and from thence more Southerly by more than two degrees: and April 11. over against the bright Star of Aries; April 17. over against the Stars of the Fly, a little more Southerly, and May 4. it is to be over against the Pleiades, and about the fourth or fifth of the same Month, it is to be once more in Conjunction with the Sun; after which time, the Sun will move from it Eastward, and leave it towards the West; which will enable us to see it again at a better hour, provided the clearenfs of the Day-break be no impediment to us. He addeth, that this Star must have been the third time in Conjunction with the Sun, about the time when it first began to appear: and foresees, that from all these particulars many considerable consequences may be deduced.
It will cut the Ecliptick about the end of July, new Style, a little more Eastwards than the Eye of Taurus: at which time there will be no seeing of it, except it be with a Telescope.

It will be towards the End of April, new Style, twice as far distant as it was in its Perigee, thrice as far, May the fourth, four times, May the eighteenth, and five times, June the first, &c.

He would not have Men surprised, that there have been two Comets within so short a time; seeing, faith he, there were four, at least, three, in the Year 1618, and in other Years there have been two and more at the same time. What he adds about their signification, we leave to Astrologers to dispute it with him. He concludes with asking pardon, if he have committed mistakes, which he hopeth he shall obtain the sooner, because of the small time he hath had for these calculations: and he wishes that he could have made all the Observations himself, seeing that it is easy to fail, when one must trust to the Observations of others, whereof we know not the exactness: where he instanceth, that, according to his Observations, the way of the Comet should go neerer the Ecliptick than he hath marked it, even without having any great regard to the Refractions: but since he would subject himself to others, he hath made it pass a little higher, which, he faith, was almost insensibly so, in those few days that he was observing and writing, but that this may perhaps become sensible hereafter: which if it be so, he affirms that it will cut the Ecliptick and Equator sooner, than he hath marked it, &c. However, he thinks it convenient, to have given aforehand a common Notion of what will become of a Comet, to prepare men for all the Changes that may fall out concerning it: which he affirms he hath endeavoured to do; the rest being easy to correct, as soon as any good Observations, somewhat distant, have been obtained, considering, that there need but two very exact ones, a little distant when the Star is not swift, to trace its Way; although there must be at least three, to find out all the rest. But, then would he have it considered, that although his Method should be very exact, if there be not at hand Instruments big enough, and Globes good enough to trust to, nothing can be done perfectly in these kind of Predictions.

A Relation
A Relation of the advice given by Monsieur Petit, Intendant of the Fortifications of Normandy, touching the Conjun&tion of the Ocean and Mediterranean.

This Intelligent Gentleman, Monsieur Petit, having been consult'd with, touching the Conjun&tion of the Ocean and Mediterranean, delivers first the Proposition, and then giveth his thoughts upon it.

The Proposition is, That there being about two Leagues below Caftres in Languedoc a Rivolet, called Sor, passing to Re-bel, there may by the means thereof be made a Communication of the two Seas, by joyning the Waters of this Rivolet by a Channel (to be kept full all the year long) With those of St. Papoul, and others, which fall into Fresqueil (another small River) that runs into the Aude below Carcassonne, and go together to Narbonne, situated upon the Mediterranean.

Having given the Proposition, he adds some particulars, to illustrate the fame, before he declares his judgment upon it. For he relateth, that there is but one way, after the division of the Waters, to pass to the Mediterranean, which is by a Rivolet, called Fresqueil, that is conjoin'd with the Aude: But, to pass to the Ocean, there are three: One, by Riege, entering into the Garonne above Tholouse; the other, by Lers, passing on the side, and below the same Town; and the third, by Sor, falling into the River Apoast under Caftres, afterwards into the Tarne, and thence to Montauban, and lastly into the Garonne. And that, to compafs this design, all these Rivers and Rivolets are first to be made Navigable unto their Sluces; that of Aude and Fresqueil for the Mediterranean, and one of the others, such as shall be chozen, for the Ocean. He addeth, that, as to the several Ways passing to the Ocean, all of them commended as proper and convenient, and the three Countries concerned therein, speaking every one for their advantage: Those of Caftres and Montauban,
Montauban, are for the River Agouf: those of Tholoue, for Riege; and the rest, for Lers.

Now concerning his Opinion upon this Proposition, he thinks, that all that hath been represented touching this matter, can signify very little, seeing that the main thing is wanting, which is the assurance, and certain and positive mensuration of the height and quantity of the Waters, necessary to fall into both the Channels of the Aude and Garonne: that there must be plenty of that, to furnish at all times and always the highest and first Sluices, since what once issues thence, doth never enter again into them; and after some Boats are passed, if there should not be a sufficient supply for those that come after, either to go up, or to go down, all would stand dry, and Merchants and their Commodities would stay long enough expecting the supply of Rains, to their great detriment. He concludes therefore, that no knowing and discreet Person is able, in matters of this nature, to give a positive answer, without having before him a large and exact Topographical Map of those places, and of the sources of all the Rivolets, that are to supply the Water to the Head of the pretended Channel, together with a full account of the survey and mensuration of all the places, through which it is to pass; of the Nature of the Ground, whether it be stony, sandy, rocky, &c. of the exact level of all the places, where it is to be made, and of the several risings and depressions thereof, to be assured that the Water may be conveyed to the greatest rising, and to the highest Sluice; and lastly, of the quantity, that may be had at high, middle, and low Water, to have enough for all times; that all these things being first made out, 'tis then time enough to judge of the possibility of the thing, and to calculate the Charges necessary for Execution.

This Artist having thus prudently waved this Proposition, diverts himself with reflecting upon several others of the like nature, among which he insists chiefly upon two, whereof one is that so much celebrated in Egypt, the other, of Germany. And he is of Opinion, that the most important of all is that, of conjoining the Red Sea by the Nile with the Mediterranean, which he looks upon as the most excellent convenience to go into the East Indies, without doubling the Cape of Good-Hopes, and yet it could
could not be executed by those great Kings of Egypt, that raised so many stupendous Pyramids; although in his Opinion the reasons alleged by Historians to justify them for having abandoned that undertaking, are of no validity, and that the Red Sea cannot be, as they feared, higher than the Nile, and therefore not indanger the inundation of Egypt.

The other Proposition was made to Charles Magna, Anno 793, for joyning the Euxine Sea and the Ocean together, by a Channel, which was begun for that end, and designed to be 2000 paces long, and 100 paces broad, betwixt the River Altmull, falling into the Danube above Ratibone, and the River Roth, passing at Nurenberg, and thence running into the Main, and so into the Rhine. But yet this also proved abortive, though there was great appearance of success at first.

Of the Way of killing Ratle-Snakes.

There being not long since occasion given at a meeting of the Royal Society to discourse of Ratle-Snakes, that worthy and inquisitive Gentleman, Captain Silas Taylor, related the manner, how they were killed in Virginia, which he afterwards was pleased to give in writing, attested by two credible persons in whose presence it was done, which is, as follows.

The Wild Penny-royal or Ditany of Virginia, groweth straight up about one foot high, with the leaves like Penny-royal, with little blue tufts at the joyning of the branches to the Plant, the colour of the Leaves being a reddish green, but the Water distilled, of the colour of Brandy, of a fair Yellow: the Leaves of it bruised are very hot and biting upon the Tongue: and of these, so bruised, they took some, and having tied them in the cleft of a long stick, they held them to the Nose of the Ratle-Snake, who by turning and wriggling laboured as much as she could to avoid it: but she was killed with it, in less than half an hour's time, and, as was supposed, by the scent thereof; which was done Anno 1657, in the Month of July, at which season, they repute those creatures to be in the greatest vigour for their poison.
A Relation of Persons killed with subterraneous Damps.

This Relation was likewise made to the Royal Society, by that Eminent Virtuoso Sir R. Moray, who was pleased, upon their desire, to give it them in writing, as followeth,

In a Coal-pit, belonging to the Lord Sinclair in Scotland, where the Coal is some 18 or 20 foot thick, and antiently wast to a great depth: The Colliers, some Weeks agoe, having wrought as deep as they could, and being to remove into new Rooms (as they call them) did, by taking off, as they retired, part of the Coal that was left as Pillars to support the Roof and Earth over it, so much weaken them, that within a short space, after they were gone out of the Pitt, the Pillars falling, the Earth above them filled up the whole Space, where the Colliers had lately wrought, with its ruins. The Colliers being here-by out of work, some of them adventured to work upon old remains of Walls, so near the old waftes, that striking through the slender partition of the Coal wall, that separated between them and the place, where they used to work, they quickly perceived their Error, and fearing to be stifled by the bad Air, that they knew, possessed these old waftes, in regard not onely of the Damps, which such waftes do usually afford, but because there having for many years been a Fire in those waftes, that filled them with stifling fumes and vapours, retired immediately and saved themselves from the eruptions of the Damp. But next day some seven or eight of them came no sooner so far down the Staires, that led them to the place, where they had been the day before, as they intended, but upon their stepping into the place, where the Air was infected, they fell down dead, as if they had been shoot: And there being amongst them one, whose Wife was informed he was stifled in that place, she went down so far without inconvenience, that seeing her Husband near her, ventured to go to him, but being choked by the Damp, as soon as she came near him, she fell down dead by him.
This Story Sir R. Moray affirmed to have received from the Earl of Weymes, Brother in Law to the Lord Sinclair, as it was written to him from Scotland.

Of the Mineral of Liege, yielding both Brimstone and Vitriol, and the way of extracting them out of it, used at Liege.

The Account of this Mineral, and of the way of extracting both Brimstone and Vitriol out of it, was procured from Liege, by the lately mentioned Sir Robert Moray, and by him communicated to the Royal Society, as follows.

The Mineral, out of which Brimstone and Vitriol are extracted, is one and the same, not much unlike Lead-ore, having also oft times much Lead mingled with it, which is separated from it by picking it out of the rest. The Mines resemble our English Coal-Mines, dug according to the depth of the Mineral, 15, 20, or more fathoms, as the Vein leads the Workmen, or the subterranean waters will give them leave, which in Summer overflow the Mines, that the upper waters, by reason of the drought, not sufficing to make the Pumps goe, the Work ceases.

To make Brimstone, they break the Stone or Ore into small pieces, which they put into Crucibles made of Earth, five foot long, square and Pyramid-wise. The Entry is near a foot square. These Crucibles are laid floating, eight undermost, and seven above them, as it were betwixt them, that the Fire may come at them all, each having its particular Furnace or Oven. The Brimstone being dissolved by the violence of the heat, drops out at the small end of the Crucible, and falls into a Leaden-Trough or Receptacle, common to all the said Crucibles, through which there runs a continual Rivulet of cold water, conveyed thither by Pipes for the cooling of the dissolved Sulphur, which is ordinarily four hours in melting. This done, the Ashes are drawn out with a crooked Iron, and being put into an Iron Wheel-barrow, are carried out of the Hutt, and being
being laid in a heap, are covered with other elixed or drained Ashes, the better to keep them warm, which is reiterated, as long as they make Brimstone.

To make Coperas or Vitriol, they take a quantity of the said Ashes, and throwing them into a square planked pit in the Earth, some four foot deep, and eight foot square, they cover the same with ordinary water, and let it lye twenty four hours, or untill an Egge will swim upon the liquor, which is a sign, that it is strong enough. When they will boil this, they let it run through Pipes into the Kettles, adding to it half as much Mother-water, which is that water, that remains after boiling of the hardned Coperas. The Kettles are made of Lead, 4 ½ foot high, 6 foot long, and 3 foot broad, standing upon thick Iron Barrs or Grates. In these the Liquor is boiled with a strong Coal-fire, twenty four hours or more, according to the strength or weakness of the Lee or Water. When it is come to a just consistence, the fire is taken away, and the boyled liquor suffered to cool somewhat, and then it is tapp'd out of the said Kettles, through holes beneath in the sides of them, and conveyed through wooden Conduits into several Receptacles, three foot deep and four foot long (made and ranged not unlike our Tan-pits) where it remains fourteen or fifteen dayes, or so long till the Coperas separate it self from the water, and becomes icy and hard. The remaining water is the above-mentioned Mother-water, and the elixed or drained Ashes are the Dreggs, or Caput mortuum, which the Lee, whereof the Vitriol is made, leaves behind it in the planked Pitts.

A further Account of Mr. Boyle's Experimental History of Cold.

In the first Papers of these Philosophical Transactions, some promise was made of a fuller account, to be given by the next of the Experimental History of Cold, composed by the Honourable Mr. Robert Boyle; it being then supposed, that this History would have been altogether printed off at the time of publishing the Second Papers
Papers of these Transactions: but the Prets, employed upon this Treatise, having been retarded somewhat longer than was expected, the said promise could not be performed before this time: wherein it now concerns the inquiring World to take notice, that this subject, as it hath hitherto been almost totally neglected, so it is now, by this Excellent Author, in such a manner handled, and improved by near Two hundred choice Experiments and Observations, that certainly the Curious and Intelligent Reader will in the perusal thereof find cause to admire both the Fertility of a Subject, seemingly so barren, and the Author's Abilities of improving the same to so high a Degree.

But to take a short view of some of the particulars of this History, and thereby to give occasion to Philosophical men, to take this Subject more into their consideration, than hitherto hath been done; the Ingenious Readers will here see,

1. That not only all sorts of Acid and Alcalizate Salts, and Spirits, even Spirit of Wine; but also Sugar, and Sugar of Lead mixed with Snow, are capable of freezing other Bodies, and upon what account they are so.

2. That among the Substances capable of being frozen, there are not only all gross sorts of Saline Bodies, but such also as are freed from their grosser parts, not excepting Spirit of Urine, the Lixivium of Pot-ashes, nor Oyl of Tartar, per deliquium, it self.

3. That many very spirituous liquors, freed from their aqueous parts, cannot be brought to freeze, neither naturally, nor artificially: And here is occasionally mentioned a way of keeping Moats unpassable in very cold Countries, recorded by Olaus Magnus.

4. What are the wayes proper to estimate the greater or lesser Coldness of Bodies; and by what means we can measure the intenseness of Cold produced by Art, beyond that, which Nature needs to employ for the freezing of Water; as also, in what proportion water of a moderate degree of Coldness will be
be made to **fro**m by Snow and Salt, before it begin by Congela-
tion to **expand** it self; and then, how to measure by the differ-
ing Weight and Density of the same portion of Water, what
change was produced in it, betwixt the hottest time of Summer,
and first glaciating degree of Cold, and then the highest, which
our Author could produce by *Art*: Where an Inquiry is an-
nex’d, whether the making of these kind of Tryals with the
waters of the particular Rivers and Seas, men are to fail on, may
afford any useful estimate; whether or not, and how much, ships
may on those waters be safely loaden more in Winter, than in
Summer. To which is added the way of making exact Disco-
veryes of the differing degrees of Coldness in differing Re-
ions, by such Thermometers, as are not subject to the altera-
tions of the *Atmosphere’s* gravitation, nor to be frozen.

5. Whether in Cold, the diffusion from Cold Bodies be made
more strongly downwards, contrary to that of Hot Bodies. Where
is delivered a way of freezing Liquors without danger of
breaking the Vessel, by making them begin to freeze at the
bottom, not the top.

6. Whether that Tradition be true, that if frozen Apples or
Eggs be thaw’d near the Fire, they will be thereby spoil’d, but
if immersed in cold water, the Internal Cold will be drawn out,
as is supposed, by the External Cold; and the frozen Bodies
will be harmlesly thawed? *Item*, Whether Iron, or other Me-
tals, Glass, Stone, Cheese, &c. expos’d to the freezing Air, or
kept in Snow or Salt, upon the immersing them in Water will
produce any Ice? *Item*, What use may be made of what happens
in the different waies of thawing Eggs and Apples, by applying
the Observation to other Bodies, and even to Men, danger-
ously nipp’d by excessive Cold. Where is added not only a
memorable Relation, how the whole Body of a Man was suc-
cessfully thawed and cas’d all over with Ice, by being handled,
as frozen Eggs and Apples are; but also the Luciferousness of
such Experiments as these: and likewise, what the effects of
Cold may be, as to the Conservation or Destruction of the
Textures of Bodies: and in particular, how Meat and Drink
may
may be kept good, in very Cold Countries, by keeping it under Water, without glaciation? as also, how in extreme Cold Countries, the Bodies of Dead Men and other Animals may be preserved very many years entire and unputrefied? And yet, how such Bodies, when unfrozen, will appear quite vitiated by the excessive Cold? Where it is further inquired into; whether some Plants, and other Medicinal things, that have specific Vertues, will loose them by being throughly congealed and (several wayes) thawed? And also, whether frozen and thawed Harts-horn will yield the same quantity and strength of Salt and saline Spirit, as when unfrozen? Item, Whether the Electrical faculty of Amber, and the Attractive or Directive Virtue of Loadstones will be either impaired, or any wayes altered by intense Cold? This Head is concluded by some considerable remarks touching the operation of Cold upon Bones, Steel, Brasse, Wood, Bricks.

7. What Bodies are expanded by being frozen, and how that expansion is evinced? And whether it is caused by the intrusion of Air? As also, whether, what is contained in icy bubbles, is true and Springy Air, or not.

8. What Bodies they are, that are contracted by Cold; and how that Contraction is evinced? Where 'tis inquired, whether Chymical Oyles will, by Congelation, be, like expressed Oyles, contracted, or, like aqueous Liquors, expanded?

9. What are the ways of Measuring the Quantity of the Expansion and Contraction of Liquors by Cold? And how the Author's account of this matter agrees with what Navigators into cold Climats, mention from experience, touching pieces of Ice as high as the Mafts of their Ships, and yet the Depth of these pieces feems not at all answerable to what it may be supposed to be.

10. How strong the Expansion of freezing water is? Where are enumerated the several sorts of Vessels, which being filled
with water, and exposed to the cold Air, do burst; and where also the weight is expressed, that will be removed by the expansive force of Freezing? Whereunto an Inquiry is subjoined, whence the prodigious force, observed in water, expanded by Glaciation, should proceed? And whether this Phenomenon may be solved, either by the Cartesian, or Epicurean Hypothesis?

11. What is the sphere of Activity of Cold, or the Space, to whose extremities every way the Action of a cold Body is able to reach: where the difficulty of determining these limits, together with the causes thereof, being with much circumspection mentioned, it is observed, that the Sphere of Activity of Cold is exceeding narrow, not only in comparison of that of Heat in Fire, but in comparison of, as it were, the Atmosphere of many odorous Bodies; and even in comparison of the Sphere of Activity of the more vigorous Loadstones, inasmuch, that the Author hath doubted, whether the Sense could discern a Cold Body, otherwise then by immediate Contact. Where several Experiments are delivered for the examining of this matter, together with a curious relation of the way used in Persia, though a very hot Climate, to furnish their Conservatories with solid pieces of Ice of a considerable thickness: To which is added an Observation, how far in Earth and Water the Frost will pierce downwards, and upon what accounts the deepness of the Frost may vary. After which, the care is inculcated, that must be had, in examining, whether Cold may be diffused through all Mediums indefinitely, not to make the Trials with Mediums of two great thickness: where it is made to appear, that Cold is able to operate through Metalline Vessels, which is confirmed by a very pretty Experiment of making Icy Cans to drink in; whereof the way is accurately set down. Then are related the Trials, whether, or how, Cold will be diffused through a Medium, that some would think a Vacuum, and which to others would seem much less disposed to assist the diffusion of Cold, than Common Air itself. After which follows a curious Experiment, shewing whether a Cold Body can operate through a
12. How to estimate the solidity of the Body of Ice, or how strong is the mutual adhesion of its parts? and whether differing Degrees of Cold may not vary the Degree of the compactness of Ice. And our Author having proceeded as far as he was able towards the bringing the strength of Ice to some Estimate by several experiments, he communicateth the information, he could get about this matter among the Descriptions that are given us of Cold Regions: And then he relateth out of Sea-men's Journals, their Observations touching the insipidness of resolved Ice made of Sea-water; and the prodigious bigness of it, extending even to the height of two hundred and fourty Foot above water, and the length of above eight Leagues. To which he adds some promificuous but very notable Observations concerning Ice, not so readily reducible to the foregoing Heads: videlicet, Of the blew Color of Rocky pieces of Ice; and the horrid noise made by the breaking of Ice, like that of Thunder and Earthquakes, together with a Consideration of the Cause, whence those loud Ruptures may proceed.

13. How Ice and Snow may be made to last long; and what Liquor dissolves Ice sooner than others, and in what proportion of quickness the solutions in the several Liquors are made, where occasion is offered to the Author, to examine, whether Motion will impart a heat to Ice? After which, he relates an Experiment of Heating a Cold Liquor with Ice, made by himself in the presence of a Great and Learned Nobleman, and his Lady, who found the Glafs wherein the Liquor was, so hot that they could not endure to hold it in their Hands. Next, it is examined, whether the effects of Cold do continually depend upon the actual presence and influence of the manifest Efficient Causes, as the Light of the Air depends upon the Sun or Fire, or other Luminous Bodies. To this is annexed an Account of the Italian way of making Conservatories of Ice and Snow, as the Author had received it from that Ingenious and Polite Gentleman, Master J. Evelyn.
But want of time prohibiting the accomplishment of the intended account of this Rich Piece: what remains, must be referred to the next Occasion. It shall only be intimated for a Conclusion, that the Author hath annexed to this Treatise, an Examen of Master Hobs's Doctrine touching Cold; wherein the Grand Cause of Cold and its Effects is assigned to Wind, in so much that 'tis affirmed, that almost any Ventilation and stirring of the Air doth refrigerate.

LONDON,
Printed with License, By John Martyn, and James Allestry, Printers to the Royal-Society. 1665.
PHILOSOPHICAL
TRANSACTIONS.

Munday, June 5. 1665.

The Contents.

A Relation of some extraordinary Tydes in the West-Isles of Scotland, by Sr. Robert Moray. The judgment of Monsieur Auzout, touching the Apertures of Object-glasses, and their proportions in respect of the several lengths of Telescopes; together with a Table thereof. Considerations of the same Person upon Mr. Hook's New Engine for grinding of Optick-glasses. Mr. Hook's Thoughts thereupon. Of a means to illuminate an Object in what proportion one pleaseth; and of the distances, that are requisite to burn Bodies by the Sun. A further account by Monsieur Auzout of Signior Campani's Book, and Performances about Optick-Glasses. Campani's Answer thereunto; and Mr. Auzout's Animadversions upon that Answer. An account of Mr. Lower's newly published Vindication of Dr. Willis's Diatriba de Febribus.

A Relation of some extraordinary Tydes in the West-Isles of Scotland, as it was communicated by Sr. Robert Moray.

In that Tract of Isles, on the West of Scotland, called by the Inhabitants, the Long-Island, as being about 100 miles long from North to South, there is a multitude of small Islands, situated in a Fretum, or Frith, that passes between the Island of Eaf, and the Herris; amongst which, there is one called Berneray, some three miles long, and more...
more than a mile broad, the length running from East to West, as the Frith lies. At the East end of this Island, where I stayed some 16. or 17. days, I observed a very strange Reciprocal action of the Flux and Re-flux of the Sea, and heard of another, no less remarkable.

Upon the West side of the Long Island, the Tides, which came from the South-west, run along the Coaft, Northward; so that during the ordinary course of the Tides, the Flood runs East in the Frith, where Berneray lies, and the Ebb West. And thus the Sea ebbs and flows orderly, some 4. days before the full Moon, and change, and as long after (the ordinary Spring-tides rising some 14. or 15. foot upright, and all the rest proportionally, as in other places) ! But afterwards, some 4. days before the Quarter-moons, and as long after, there is constantly a great and singular variation. For then, (a Southerly Moon making there the full Sea) the course of the Tide being Eastward, when it begins to flow, which is about 9½ of the Clock, not only continues so till about 3½ in the afternoon, that it be high water, but, after it begins to ebb, the Current runs on still Eastward, during the whole Ebb; so that it runs Eastward 12 hours together, that is, all day long, from about 9½ in the morning, till about 9½ at night. But then, when the night-Tide begins to flow, the Current turns, and runs Westward all night, during both Floud & Ebb, for some 12. hours more, as it did Eastward the day before. And thus the Reciprocal actions continue, one Floud and Ebb, running 12 hours Eastward, and another twelve hours Westward, till 4. days before the New and Full-Moons, and then they resume their ordinary regular course as before, running East, during the six hours of Floud, and West, during the six of Ebb. And this I observed curiously, during my abode upon the place, which was in the Moneth of August, as I remember.

But the Gentleman, to whom the Island belongs at present, and divers of his Brothers and Friends, knowing and discreet persons, and expert in all such parts of Sea-matters, as other Islanders commonly are, though I shrewdly suspected their skill in Tides, when I had not yet seen what they told me, and I have now related of these irregular Courses of the Tides, did not confidently assure me, and so did every body I spake with about.
about it, that there is yet another irregularity in the Tides, which never fails, and is no less extraordinary, than what I have been mentioning: which is, That, whereas between the Vernal and Autumnal Equinoxes, that is, for six Moneths together, the Course of irregular Tides about the Quartermoons, is, to run all day, that is, twelve hours, as from about $\frac{9}{2}$ to $\frac{9}{4}$, $\frac{10}{4}$ to $\frac{10}{2}$ &c. Eastward, and all night, that is, twelve hours more, Westward: during the other six Moneths, from the Autumnal to the Vernal Equinox, the Current runs all day Westward, and all Night Eastward.

Of this, though I had not the opportunity to be an Eye-witness, as of the other, yet I do not at all doubt, having received so credible Information of it.

To penetrate into the Causes of these strange Reciprocations of the Tides, would require exact descriptions of the Situation, Shape, and Extent of every piece of the adjacent Coasts of Eust and Herus; the Rocks, Sands, Shelves, Promontorys, Bays, Lakes, Depths, and other Circumstances, which I cannot now set down with any certainty, or accurateness, seeing, they are to be found in no Map, neither had I any opportunity to survey them; nor do they now occur to my Memory, as they did some years ago, when upon occasion I ventured to make a Map of this whole Frith of Berneray, which not having copied, I cannot adventure to beat it out again.

Monsieur Auzout's Judgment touching the Apertures of Object-Glasses, and their Proportions, in respect of the several Lengths of Telescopes.

This Author, observing in a small French Tract lately written by him to a Countryman of his, Monsieur L'Abbe Charles, That great Optick-Glasses have almost never as great an Aperture as the small ones, in proportion to what they Magnifie, and that therefore they must be more dim; takes occasion to inform...
the Reader, that he hath found, that the Apertures, which Optick-Glasses can bear with distinctness, are in about a subduplicate proportion to their Lengths; whereof he tells us he intends to give the reason and demonstration in his Dioptricks, which he is now writing, and intends to finish, as soon as his Health will permit. In the mean time, he presents the Reader with a Table of such Apertures; which is here exhibited to the Consideration of the Ingenious, there being of this French Book but one Copy, that is known, in England.

A **TABLE of the Apertures of Object-Glasses.**

The Points put to some of these Numbers denote Fractions.

<table>
<thead>
<tr>
<th>Lengths of Glasses</th>
<th>For excellent ones.</th>
<th>For good ones.</th>
<th>For ordinary ones.</th>
<th>Lengths of Glasses</th>
<th>For excellent ones.</th>
<th>For good ones.</th>
</tr>
</thead>
<tbody>
<tr>
<td>4</td>
<td>4</td>
<td>4</td>
<td>3</td>
<td>25</td>
<td>2</td>
<td>4</td>
</tr>
<tr>
<td>6</td>
<td>5</td>
<td>5</td>
<td>4</td>
<td>30</td>
<td>3</td>
<td>7</td>
</tr>
<tr>
<td>9</td>
<td>7</td>
<td>6</td>
<td>5</td>
<td>35</td>
<td>4</td>
<td>4</td>
</tr>
<tr>
<td>10</td>
<td>8</td>
<td>7</td>
<td>6</td>
<td>40</td>
<td>4</td>
<td>7</td>
</tr>
<tr>
<td>1</td>
<td>6</td>
<td>8</td>
<td>7</td>
<td>45</td>
<td>4</td>
<td>10</td>
</tr>
<tr>
<td>2</td>
<td>11</td>
<td>10</td>
<td>8</td>
<td>50</td>
<td>5</td>
<td>2</td>
</tr>
<tr>
<td>3</td>
<td>10</td>
<td>0</td>
<td>11</td>
<td>65</td>
<td>5</td>
<td>4</td>
</tr>
<tr>
<td>4</td>
<td>6</td>
<td>1</td>
<td>2</td>
<td>70</td>
<td>5</td>
<td>8</td>
</tr>
<tr>
<td>5</td>
<td>1</td>
<td>3</td>
<td>1</td>
<td>75</td>
<td>5</td>
<td>10</td>
</tr>
<tr>
<td>6</td>
<td>1</td>
<td>4</td>
<td>1</td>
<td>80</td>
<td>5</td>
<td>14</td>
</tr>
<tr>
<td>7</td>
<td>1</td>
<td>5</td>
<td>1</td>
<td>90</td>
<td>6</td>
<td>4</td>
</tr>
<tr>
<td>8</td>
<td>1</td>
<td>6</td>
<td>1</td>
<td>100</td>
<td>6</td>
<td>8</td>
</tr>
<tr>
<td>9</td>
<td>1</td>
<td>7</td>
<td>1</td>
<td>120</td>
<td>7</td>
<td>12</td>
</tr>
<tr>
<td>10</td>
<td>1</td>
<td>8</td>
<td>1</td>
<td>150</td>
<td>8</td>
<td>16</td>
</tr>
<tr>
<td>12</td>
<td>2</td>
<td>10</td>
<td>2</td>
<td>6200</td>
<td>9</td>
<td>68</td>
</tr>
<tr>
<td>14</td>
<td>2</td>
<td>42</td>
<td>0</td>
<td>8250</td>
<td>10</td>
<td>69</td>
</tr>
<tr>
<td>16</td>
<td>2</td>
<td>62</td>
<td>2</td>
<td>9300</td>
<td>11</td>
<td>610</td>
</tr>
<tr>
<td>18</td>
<td>2</td>
<td>82</td>
<td>4</td>
<td>11350</td>
<td>12</td>
<td>610</td>
</tr>
<tr>
<td>20</td>
<td>3</td>
<td>102</td>
<td>6</td>
<td>1400</td>
<td>13</td>
<td>411</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Considerations of Monsieur Auzout upon Mr. Hook's New Instrument for Grinding of Optick-Glasses.

In the above-mentioned French Tract, there are, besides several other particulars, to be represented in due place, contained some Considerations of Monsieur Auzout upon Mr. Hook's New Engine for grinding Optick-Glasses. Where he premies in General his thoughts touching the working of Great Optick-Glasses, and that by the help of a Turn lathe; affirming first of all, that not only the Engin is to be considered for giving the Figure, but the Matter also, which ought to be brought to greater perfection, than it hath been hitherto. For, he finds it not so easie (at least, where he is) to procure Great pieces of Glass without Veins, and other faults, nor to get such, as are thick enough without Blebs; which, if they be not, they will yield to the pressure and weight, either when they are fitted to the Cement, or wrought.

Secondly, He finds it difficult to work these Great Glasses of the same thickness, which yet is very necessary, because, that the least difference in Figures so little convex, can put the Center out of the Midle, 2 or 3 Inches; and if they be wrought in Moulds, the length of time, which is required to wear and to smooth them, may spoil the best Mould, before they be finished. Besides, that the strength of Man is so limited; that he is unable to work Glasses beyond a certain bigness, so as to finish and polish them all over so well, as small Glasses; whereas yet, the bigger they are, the more compleat they ought to be: And if any weight or Engine be used to supply strength, there is then danger of an unequal pressure, and of wearing away the Engine; In the mean time, the preciseness and delicateness is greater,
greater than can easily be imagined. Wherefore he could never, having some experience of this precision, conceive, that a **Turn-lathe**, wherein must be two different, and in some manner contrary motions, can move with that exactness and steadiness, that is required, especially, for any considerable length of time.

Having premised this, he discourses upon Mr. **Hook** his **Turre**, intimating first of all, that he was impatient to know what kind of **Turre** this was, imagining, that it had been tried, and had succeeded, as coming from a Society that professeth, they publish nothing but what hath been maturely examin'd. But that he was much surprized when he saw the **Micrography** of Mr. **Hook**, and found there, that his **Engine** was published upon a mere **Theory**, without having made any Experiment, though that might have been made with little charge and great speed; expense of Money and Time being the onely thing, that can excuse those who in matter of **Engines** impart their inventions to the publick, without having tried them, to excite others to make trial thereof.

Whereupon he proposes some difficulties, to give the Inventor occasion to find a way to remove them. He affirms therefore, that though it be true in the **Theory**, that a **Circle**, whose **Plain** is inclined to the **Axis** of the **Sphere** by an **Angle**, whereof half the **Diameter** is the **Sine**, and which touches the **Sphere** in its **Pole**, will touch in all its parts a spherical **Surface**, that shall turn upon that **Axe**. But that it is true also, that that must be but a **Mathematical Circle**, and without **Breadth**, and which precisely touches the **Body** in its middle: Whereas in the practice, a **Circle** capable to keep **Sand and Putty**, must be of some **breadth**; and he knows not whether we can find such a dexterity of keeping so much of it, and for so long a time, as needs, upon the Brim of a **Ring** that is half an Inch broad. He adds, that it is very difficult to contrive, that the middle of the **Glass** do always precisely answer to the Brim of this **Ring**, seeing that the position of the **Glass** does always change a little in respect of the **Ring**, in proportion as tis worn, and as it must be pressed because of its inclination. He believes it also very hard, to give to the **Axis** or to the **Mandril**, which holds the **Glass**, that little...
Inclination, that would be necessary for great Glassés, and to make the two Mandrils to have one and the same Plain, as is necessary. And, having done all this, he persuades himself, that it is exceeding difficult, if not impossible, for two contrary motions, where so many pieces are, to rest for a long time steady and firm, as is requisite for the not swarving from it a hair’s thickness, since less than that can change all.

He goes on, and, seeing that this Inventor speaks of Glassés of a thousand, & ten thousand foot, which he supposed not impossible to be made by this Engine, discourses of what is necessary for the making Glassés of such bigness; which he believes this Inventor may perhaps not have thought of. Wherefore he affirms, that if the Table, made by himself for the Apertures of Glassés (which is that, that is above delivered) be continued unto a thousand feet, by taking always the Subduplicate proportion of Lengths, it will be found, that for pretty good ones, the Aperture must be of 15. Inches; for good ones, more than 18. and for such as are excellent, more than 21. Inches: whence it may be judged, what piece of Glass, and of what thickness it must be, to effects the working. But he proceeds to speak of the Inclination, which the Mandril must have upon the Plain of the Ring, when the Ring should have 10. or 12. Inches; and finds, that it would make but 6. or 7. minutes of inclination, and that a Glasses would have less Convexity, and consequently, less difference from a Glasses perfectly plain, than the 7. or 8. part of a Line. And then he leaveth it to be judged, whether a Glasses of such a Length being found, we ought to hope, that a Turn can be firm enough to keep such a piece of Glass in the same Inclination, so that a Mandril do not recede some Minutes from it, and, though even the Glass could be fastned perfectly perpendicular to the Mandril, that these two Mandrils could be put in one, and the same place, & that that little Inclination, which is requisite, could be given, and the Mandril be continued to be pressed in that same Inclination, according as the Glass is worn. All which particulars, he conceives to be very hard in the practice; not to mention, that the weight of the Glass, that should be inclined to the Horizon, as ‘tis represented by Mr. Hook, would make it slide upon the Cement, and so chance
change the Center; and that the Glass is not pressed at the same
time by the Ring but in one part on the side, *vid.* about a fourth;
and that the parts of the Glass are not equally worn away, &c.
What then, *faith be,* would becom of a Glass of 10000 feet,
which, according to the said Table, would have more than four
feet, or four feet and nine inches, or five feet, seven inches *A-
perture,* and of which the Ring, though it were two feet nine inches,
would have but one minut of *Inclination,* and the Glass of 5
feet *Aperture* would have but 4 minutes, and the curvity of it
would be less than the hundred part of a Line.

But, *faith be,* let us consider, only a Glass of 300 foot, to see,
what is to be hoped of that, and to know at least the difficulty,
to be met with in making a Glass only of that Length. A Glass
then of 300 foot, according to his Table, must have more than
8 inches *Aperture,* which maketh but 16 minutes of its *Circle,*
and it should have more than 11 inches, if it be an excellent one.
If Mr. *Hook* (*adds he*) did use but his *Ring* of 6 inches, which
he would use from twelve to an hundred foot Glass, the *Incli-
nation,* which the *Axis,* or *Mandril,* that bears his Glass, should
have, should be but 16 minutes, and the *Curvity* of the Glass
would be less than the eighth part of a Line, and if he should
use a bigger, the *Inclination* would be proportionable.

Whence it may be judged (*continues he*) that we are yet
very far from seeing Animals &c. in the Moon, as *Monfieur Des*
Cartes gave hope, and Mr. *Hook* despears not of. For, he be-
lieves by what he knows of *Telescopes,* that we are not to look
for any above 300 or 400 foot at most; and he fears, that nei-
ther Matter nor Art will go even so far.

When therefore (*faith be*) a *Glass* of 300 foot should bear
an *Eye-glass* of 6 inches (which would appear wonderful) it
would magnifie but 600 times in *Diameter,* that is, 360000
times in *Surface,* but suppose, that such could be made, as would
magnifie a 1000 times in *Diameter,* and 1000000 of times in
*Surface,* admitting there were but 60000 leagues from the Earth
to the Moon, and that the smallness of the *Aperture* of the Glasses
(which yet would diminish the Light more than 36 times)
and the obstacle of the Air were not considered, we should not see
see the Moon, but as if we were a 100, or at least, 60, leagues distant from her without a Glass. He here wishes, that those, that promise to make us see Animals and Plants in the Moon, had thought on what our naked Eyes can make us discern of such Objects, only at 10 or 12 leagues distance.

But this he would not have understood as a discouragement from searching with all care and earnestness after the means of making long Telescopes, or of facilitating the working thereof; but only as an Advertisement to those, who light upon the Theory of any Engine, not to expose it presently as possible and useful, before they have tried it, or if it have succeeded in small, not to endeavour to persuade, that it will also succeed in great.

As it may happen (faith be) that the Engin of Mr. Hook may, by using all necessary precautions, succeed in the making of Eye-Glasses, or small Optick-Glasses, but not in making great ones; as we see, that an instrument composed of two Rulers, where-with are traced Portions of Circles, succeeds well enough in small, but when there is no more than half a Line, a quarter of a Line, or less convexity, it will be no longer just at all, as he tells us to have made the proof of it in Circles drawn by the means of one of these Instruments, made by one of the best Workmen in his time, who, whilst he lived, esteemed them above price, although they be not just; as others and my self (faith be) have by tryal found, when we endeavoured to make Moulds by their means, & as those, who by the like Instrument laboured to trace portions of Circles of 80 or 100 foot, &c. Diameter, can attest.

But, notwithstanding all this, he hath thought upon two or three things, which he thinks may remedy some inconveniencies of Mr. Hook his Turn. The first is, to invert the Glafs, and to put it under the Ring, that so not only the Glafs may be placed more Horizontally, and not slide upon the Cement, but that the Sand also, and the Putty may stay upon the Glafs.

The other is, that there must be two Poppetheads, into which the Mandril must pass, where the Ring is to be fastned; and the Mandril must be perfectly Cylindrical, that so it may advance upon the Glafs as it wears away by the means of its weight, or by the means of a spring, pressing it, without wrigling from one place to another, as it would presently happen in the fashion,
as the Turn is composed. For, when the Glasses do wear, especially when they are very convex, it cannot be otherwise, but the Mandril will play and wrigle, before the Scrue be made firm.

But he doubts, whether all can be remedied, which he leaves to the industry of Mr. Hook, considering what he faith in the Preface of his Micrography, touching a Method, he knows, of finding out as much in Mechanicks, as can be found in Geometry by Algebra.

Besides this, he taketh notice, that most of those that medle with Optick Glasses, give them not as much Aperture, nor charge them so deep as they ought. And he instances in the Telescope, which His Majesty of Great Britain presented the Duke of Orleans with, videlicet that it did bear but 2 inches, and 9 lines French, for its greatest Aperture, though there be 5 or 6 lesser Apertures, of which it seems (faith he) the Artificer would have those, that use it, serve themselves more ordinarily, than of the greatest; which conveys but almost half as many Rays as it should do, according to his Calculation, which is, as 9 to 16; Whereas, according to his Table of Apertures, an excellent 35 foot Telescope should bear 4 inches Aperture in proportion to excellent small ones. He notes also, that the Eye-glass of the said Telescope, composed of 2 Glasses, hath no more effect, when it is most charged, than a Glass of 4½ inches; which makes it magnifie not a 100 times. And he finds by Mr. Hook, that he esteems a Telescope made in London of 60 feet, (which amount to about 57 feet of France, the foot of France being to that of England as about 15 to 16) because it can bear at least 3 English inches Aperture, and that there are few of 30 feet, that can bear more than 2 inches, (which is but 22½ Lines French) although he (M. Augoult) gives no less Aperture, than 50, to a 15 foot Telescope, and his of 21 feet hath ordinarily 2 Inches, 4 Lines, or 2 inches, 6 Lines Aperture.

This Discourse he Concludeth with exhorting those, that work Optick Glasses, to endeavor to make them such, that they may bear great Apertures and deep Eye-glasses; seeing it is not the length that gives esteem to Telescopes; but on the contrary renders them less estimable, by reason of the trouble accompanying
panying them, if they perform no more, than shorter ones. Where, by the by, he takes notice, that he knows not yet, what. Aperture Signor Campani gives to his Glasses, seeing he hath as yet signified nothing of it; but that the small one, sent by him to Cardinal Antonio, hath no more Aperture, than ordinary ones ought to have.

He promises withall, that he will explicate this way in his Treatise of the usefulness of Telescopes, where he intends to assign the Bigness of the Diameter of all the Planets, and their proportion to that of the Sun; as also, that of the Stars, which he esteemes yet much les; than all those have done, that have written of it hitherto; not believing, that the Great Dog, which appears to be the fairest Star of the Firmament, hath 2 Seconds in Diameter, nor that those, which are counted of the sixth Magnitude, have 20 thirds; nor thinking, that all the Stars, that are in the Firmament, do enlighten the Earth as much as a Luminous Body of 20 seconds in Diameter would do, or, because there is but one half of them at the same time above our Horizon, as a Body of 14 seconds in Diameter; and as the 18432th part of the Sun would enlighten us, or as the Sun would do, if we were 14 times more distant from it, than Saturn, and 137 times further, than the Earth: Which, he saith, would not be credible, if he did not endeavor to evince it both by Experience and Reason. And he doubts not, but that Venus, although the sends us no Light but what is reflected, does sometimes enlighten the Earth more, than all the Stars together. Yet he would not have us imagine, from what he hath spoken of the smallness of the Stars, that Telescopes do not magnifie them by reason of their great distance, as they do Planets; for this he judgeth a Vulgar Error, to be renounced. Telescopes magnifie the Stars (saith he) as much in proportion, as they do all other Bodies, seeing that the demonstration of their magnifying is made even upon Parallel rays, which do suppose an infinite distance, though the Stars have none such: And if the Telescopes did not magnifie the Stars, how could they make us see some of the fiftieth, and it may be some of the hundredth, and two hundredth Magnitude, as they do, and as they would shew yet much lesser ones, if they did magnifie more?
Mr. Hook's Answer to Monsieur Auzout's Considerations, in a Letter to the Publisher of these Transactions.

SIR,

Together with my most hearty thanks for the favour you were pleased to do me, in sending me an Epitome of what had been by the ingenious Monsieur Auzout animadverted on a description, I had made of an Engine for grinding spherical Glasses; I thought myself obliged, both for your satisfaction, and my own Vindication, to return you my present thoughts upon those Objections. The chief of which seems to be against the very Proposition itself: For it appears, that the Objection is somewhat unsatisfied, that I should propound a thing in Theory, without having first tried the Practicableness of it. But first, I could wish that this worthy Person had rectified my mistakes, not by speculation, but by experiments. Next, I have this to answer, that (though I did not tell the Reader so much, to the end that he might have the more freedom to examine and judge of the contrivance, yet) it was not mere Theory I propounded, but somewhat of History and matter of Fact: For, I had made trials, as many as my leisure would permit, not without some good Success; but not having time and opportunity enough to prosecute them, I thought it would not be unacceptable to such, as enjoyed both, to have a description of a way altogether New, and Geometrically true, and seemingly, not unpracticable, whereof they might make use, or not, as they should see reason. But nothing surprized me so much, as, that he is pleased (after he had declared it a fault, to write this Theory, without having reduced it to practice) to lay it, as he seems to do, in one place of his book, p. 22 upon the Royal Society. Truly, Sir, I should think my self most injurious to that Noble Company, had I not endeavoured, even in the beginning of my Book, to prevent such a misconstruction. And therefore I cannot but make this interpretation of what Monsieur Auzout saith in this particular, that either he had not so much
much of the Language wherein I have written, as to understand all what was said by me, or, that he had not read my Dedication to the Royal Society, which if he had done, he would have found, how careful I was, that that Illustrious Society should not be prejudiced by my Errors, that could be so little advantaged by my Actions. And indeed, for any man to look upon the matters published by their Order or Licence, as if they were Their Sense, and had Their Approbation, as certain and true, 'tis extremely wide of their intentions, seeing they, in giving way to, or encouraging such publications, aim chiefly at this, that ingenious conceptions, and important philosophical matter of Fact may be communicated to the learned and enquiring World, thereby to excite the minds of men to the examination and improvement thereof. But, to return; As to his Objections against the Matter, I do find that they are no more against mine, than any other way of Grinding Glasses, nor is it more than I have taken notice of myself in this Passage of the same Paragraph, of which sort are also those difficulties he raises about Long Glasses, which are commonly known to such, as are conversant in making them. It would be convenient also (these are my words) and not very chargeable, to have four or five several Tools: One, &c. And, if curiosity shall ever proceed so far, one for all lengths, between 1000 and 10000 foot long; for indeed, the Principle is such, that supposing the Mandrils well made, and of a good length, and supposing great care be used in working and polishing them, I see no reason, but that a Glass of 1000, nay 10000 foot long may be made, as well as one of 10. For, the reason is the same, supposing the Mandrils and Tools be made sufficiently strong, so that they cannot bend; and (supposing also, that the Glass out of which they are wrought) be capable of so great a regularity in its parts, as to its Refraction. But next, I must say that his Objections to me, seem not so considerable, as perhaps he imagines them. For, as to the possibility of getting Plates of Glasses thick and broad enough without veins, I think that not now so difficult here in England, where I believe is made as good, if not much better Glasses for Optical Experiments, than ever I saw come from Venice. Next, though it were better, that the thickest part of a long Object-Glass were exactly in the middle, yet I can assure Monsieur Auxout, that it may be a very good
good one, when it is an Inch or two out of it. And I have a
good one by me at present, of 36. foot, that will bear an Aperture,
if Saturn or the Moon in the twilight, be look'd on with it,
of 3½ Inches over; and yet the thickest part of the Glass is a great
way out of the middle. And I must take the liberty to doubt,
whether ever my Animaduver for saw a long Glass, that was otherwise:
as he might presently satisfy himself by a way I could shew
him’ (if he did not know it) whereby the difference of the thick-
ness of the sides might be found to the hundreth part of a Line.

As to the exceeding exactness of the Figure of Long Object-
Glasses, this is not doubted, but that it is a matter difficult enough to
be attained in any way; but yet, I think, much easier by Engine, than
by Hand; and of all Engines, I conceive, none more plain and
simple, than that of a Mandril. And for making Spherical Glasses
by an Engine, I am apt to think, there hardly can be any way more
plain, and more exact, than that which I have described; where-
in there is no other motion, than that of two such Mandrils,
which may be made of sufficient strength, length, and exactness,
to perform abundantly much more, than I can believe possible
to be done otherwise than by chance, by a man’s hands or
strength unassisted by an Engine, the motion and strength being
much more certain and regular. I know very well, that in mak-
ing a 60. foot Glass by the strength of the hand, in the common
way, not one of ten that are wrought, will happen to be good, as
I have been assured by Mr. Reeves; who, I am apt to think, was
the first that made any good of that length. For the Figure of
the Tool in that way is presently vitiated by the working of the
Glases, and without much guessing will not do any thing con-
siderable. Besides, the strength of a man’s hands, applied to it for the
working and polishing of it, is very unequal, and the motions
made, are very irregular; but in the way, I have ventured to pro-
pose, by Mandrils, the longer the Glass and Tool are wrought to-
gether, the more exact they seem to be, and if all things be or-
dered, as they should be, the very polishing of the Glass, does
seem most of all to rectifie the Figure.

As to what he objects, that the Tool does only touch the Glass
in a Mathematical Circle; that is true, perhaps, at first, but before
the Glass is wrought down to its true Figure, the Edge of the Tool
will
will be worn or ground away, so as that a Ring of an inch broad may be made to touch the Spherical Surface of the Glass; nay, if it be necessary (without much trouble, especially in the grinding of longer Glasses) the whole Concave Surface of the Tool may be made to touch a Glass. Besides, that as to the keeping a quantity of the same sand and Powders of several finesses, according as the glass wears, the same is possible to be done, as with the same Sand wrought finer by working in the Ordinary way.

The giving the Inclination to the Mandrils, is not at all difficult; though perhaps to determine the length exactly which the Glass so made shall draw, is not so easy: But 'tis no matter, what length the Glass be of, so it be made good, whether 60 or 80 foot, or the like. Nor is it so very difficult, to lay them both in the same Plain. And to keep them steady, when once fix'd, is most ease.

As to the Calculation of the propriety of a Glass of a thousand foot, perhaps for that particular Length, I had not, nor have as yet calculated, that the Convexity of one of eighteen inches broad, will not be above a seventh part of a Line. But it does not thence follow, that I had not considered the difficulties, that would be in making of it. For, I must tell him, that I can make a Plano-convex Glass, though its convexity be of a smaller sphere than is usual for such a length, to be an Object-Glass of about 150 foot in Length, nay of 300 foot, and either longer or shorter, without at all altering the convexity. So that, if he will by any Contrivance he hath, give me a Plano-convex Glass of 20, or 40 foot Diameter, without Veins, and truly wrought of that Figure, I will presently make a Telescope with it, that with a single Eye-glass shall draw a thousand foot. Which Invention, I shall shortly discover, there being, I think, nothing more easy and certain. And if a Plano-convex Glass can be made of any Sphere between twenty and forty foot radius, so as that both the Convex and Plain side of the Glass be exactly polished of a true Figure, I will shortly shew, how therewith may be made a Telescope of any Length, supposing the Glass free from all kind of Veins, or inequality of Refraction.

As for the sliding of the Glass upon the Cement, I see no reason at all for it, at least in the Cement, I make use of, having never observed any such accident in hard Cement.
And for the Bearing of the Ring against one side of the Glass only at a time, I cannot see, why that should produce any inequality, since all the sides of the Glass have successively the same pressure.

His ratiocination concerning a Glass of 300 foot, is much the same with the former, about the difficulty of working a true surface of a convenient figure; which how considerable both that and his Conclusion thereupon (vid. That we are not to expect Glasses of above 300 or 400 foot long at most, and that neither Matter nor Art will go so far) is, may be judged from what I have newly told you of making any Object-Glasses of any Length.

And for his good wishes, that those, who promise to make him see Plants or Animals in the Moon (of which I know not any, that has done so, though perhaps there may be some, notwithstanding his Objections, that do not yet think it impossible to be done) had considered, what a Man is able to see with his bare Eye at 60 Leagues distance: I cannot but return him my wishes, that he would consider the difference between seeing a thing through the Gross and Vaporous Air near the Earth, and through the Air over our heads: Which, if he observe the Moon in the Horizon, and near the Zenith with a Telescope, he will experimentally find; and, having done so, he will perhaps not be so diffident in this matter.

Concerning his Advertisement to such, as publish Theories, I find not, that he hath made use of it in his own case. For, in his Theory about Apertures he seems to be very positive, not at all doubting to rely upon it, vid. that the Apertures must be thus and thus in great Glasses, because he had found them so or so in some small ones.

For his Proposal of amendments of some inconveniences in this way, I return him my thanks; but as to his first I believe, that the matter may be contained as well in the Concave Tool, as on the convex Glass. And as to that of 2 Poppet-heads, I do not well understand it, if differing from mine; and the keeping of the Tool upon the Glass with a spring or weight, must quickly spoil the whole; since, if either of the Mandrils will easily yield backwards, the regularity of all will be spoiled: and as to the wrigling and playing of the Mandril, I do not at all apprehend it.

His
His Theory of Apertures, though he seems to think it very authentic, yet to me it seems not so clear. For, the same Glass will endure greater or lesser Apertures, according to the lesser or greater Light of the Object: If it be for the looking on the Sun or Venus, or for seeing the Diameters of the Fix'd Stars, then smaller Apertures do better; if for the Moon in the daylight, or on Saturn, or Jupiter, or Mars, then the largest. Thus I have often made use of a 12 foot Glass to look on Saturn with an Aperture of almost 3 inches, and with a single Eye-glass of 2 inches double convex; but, when with the same Glass I looked on the Sun or Venus, I used both a smaller Aperture, and shallower Charge. And though M. Augout seems to find fault with the English Glass of 36 foot, that had an Aperture of but 2½ inches French; as also, with a 60 foot Tube, used but with an Aperture of 3 inches; yet I do not find, that he hath seen Glasses of that length, that would bear greater Apertures, and 'tis not impossible, but his Theory of Apertures may fail in longer Glasses.

Of a means to illuminate an Object in what proportion one pleaseth; and of the Distances requisite to burn Bodies by the Sun.

One of the means used by M. Augout to enlighten an Object, in what proportion one pleaseth, is by some great Object-glasses, by him called a Planetary one, because that by it he shews the difference of Light, which all the Planets receive from the Sun, by making use of several Apertures, proportionate to their distance from the Sun, provided that for every 9 foot draught, or thereabout, one inch of Aperture be given for the Earth. Doing this, one sees (faith he) that the Light which Mercury receives, is far enough from being able to burn Bodies, and yet that the same Light is great enough in Saturn to see clear there, seeing that (to him) it appears greater in Saturn, than it doth upon our Earth, when it is overcast with Clouds: Which (he adds) would scarce be believed, if by means of this Glass it did not sensibly appear so; Whereof he promises to discourse more fully in his Treatise.
Treatise of the usefulness of great Optick-Glasses, where he also intends to deliver several Experiments, by him made. 1. Touching the quantity of Light, which a Body, that is 10, 15 and 20 times, &c. remoter than Saturn, would yet receive from the Sun. 2. Touching the quantity of Light, by which the Earth is illuminated even in the Eclipses of the Sun, in proportion of their bigness. 3. Touching the quantity of Light, which is necessary to burn Bodies: he having found, that not abating the Light, which is reflected by the Surfaces of the Glass (whereof he confesseth, he doth not yet exactly know the quantity) there would be necessary about 50 times as much Light, as we have here, for the burning of Black Bodies; and nearer 9 times more for the burning of White Bodies, than for the burning of Black ones: and so observing the immediate proportions between these two, for burning Bodies of other Colors. Whence (he tells us) he hath drawn some consequences, touching the distance, at which we may hope, to burn Bodies here, by the means of great Glasses and great Looking-glasses. So that (faith he) we must yet be seven times nearer the Sun, than we are, to be in danger of being burned by it. Where he mentions, that having given Instructions to certain persons, gone to travel in Hot Countries; he hath among other particulars recommended to them, to try by means of great Burning-glasses, with how much less Aperture they will burn there, than here, to know from thence, whether there be more Light there than here; and how much; since this perhaps may be the only means of trying it, supposing, the same matters be used: although the difference of the Air already heated, both in hot Countries, and in the Planets, that are nearer than we, may alter, if not the quantity of Light, at least that of the Heat, found there.

A further Account, touching Signor Campani’s Book and Performances about Optick-glasses.

In the above-mentioned French Tract, there is also contained M. Auzout’s Opinion of what he had found New in the Treatise of Signor Campani, which was spoken of in the first Papers of these Transactions, concerning both the Effect of the Telescopes, contrived after a peculiar way by the said Campani at Rome, and his
his New Observations of \textit{Saturn} and \textit{Jupiter}, made by means thereof.

First therefore, after that M. \textit{Augout} had raised some scruple against the Contrivance of Signor \textit{Campani} for making Great Optick-Glasses without Moulds, by the means of a \textit{Turn-lath}, he examines the Observations, made with such Glasses: Where, having commended \textit{Campani}'s sincerity in relating what he thought to have seen in \textit{Saturn}, without accommodating it to M. \textit{Hugens}'s Hypothesis, he affirms, that supposing, there be a \textit{Ring} about \textit{Saturn}, Signor \textit{Campani} could not see in all those different times, that he observed it, the same Appearances, which he notes to have actually seen. For, having seen it sometimes in \textit{Trine Aspect} with the Sun, and \textit{Oriental}; sometimes, in the same \textit{Aspect}, but \textit{Occidental}; sometimes in \textit{Sextil Aspect}, and \textit{Occidental}; at another time, again in \textit{Trine}, and \textit{Oriental}, this Author cannot conceive, how \textit{Saturn} could in all these different times have no difference in its \textit{Phasis}, or keep always the same \textit{Shadow}: seeing that, according to the Hypothesis of the \textit{Ring}, when it was \textit{Oriental}, it must cast the \textit{Shadow} upon the \textit{left side} of the \textit{Ring} beneath, without casting any on the \textit{right side}; and when it was \textit{Occidental}, it could not but cast it on the \textit{right side} beneath, and nothing of it on the other.

Concerning the \textit{Shadow above}, which \textit{Campani} affirms to be made by the \textit{Ring} upon the Body of \textit{Saturn}, M. \textit{Augout} judges, that there could be no such \textit{Phenomenon}, by reason of its \textit{Northern Latitude} at the times, wherein the Observations were made, \textit{vid.} in April 1663; in the midst of \textit{August}, and the beginning of \textit{October}, next following, and in April 1664, except it were in \textit{October}, and the \textit{Shadow} strong enough to become visible.

But as to the \textit{Shadow below}, he agrees with \textit{Campani}, that it does appear, yet not as he notes it, seeing that it must be sometimes on the one side, sometimes on the other; and towards the \textit{Quadrat} with the Sun it must appear biggest, as \textit{indeed} he affirms to have seen it himself this year, insomuch that sometimes it seemed to him, that it covered the whole \textit{Ring}, and that the \textit{Shadow}, joyning with the obscure space between both, did interrupt the circumference of the \textit{Ring}; but beholding it at other times in a clear Sky, and when there was no \textit{Trepidation} of the \textit{Air},
he thought, that he saw also the Light continued from without, although very slender. But he acknowledges, that he could never yet precisely determine, by how much the largeness of the Ring was bigger than the Diameter of Saturn's Body. As for the proportion of the Length to the Breadth, he affirms, to have always estimated it to be two and a half, or very nearly so; and to have found in his Observations, that in January last, one time, the length of Saturn was 12 Lines, and the breadth 7. Another time, the length was 12 Lines, and the breadth 4. and this by a peculiar method of his own. But yet he acknowledges also, that sometimes he hath estimated it as 7, to 3. and at other times as 13, to 5; and that if there do not happen a change in the magnitude of the Ring (as it is not likely there does) that must needs proceed from the Constitution of the air, or of the Glass's having more or less Aperture, or from the difficulty of making an exact estimate of their proportions. However it is not much wide (faith he) of two and an half, although Campani make the length of the Ring but double to its breadth.

Monsieur Auxout believes, that he was one of the first that have well observed this shadow of Saturn's Body upon its Rings, which he affirms happened two years since; when, observing in July, for the first time, with a Telescope of 21, and then another of 27 foot, he perceived, that the Angle of the obscure space on the right side beneath, was bigger and wider, than the three other Angles, and that some interruption appear'd there, between the Ring, and the Body of Saturn; of which he saith to have given notice from that time to all his friends, and in particular, as soon as conveniently he could, to Monsieur Hugens.

He confesseth, that he hath not had the opportunity of observing Saturn in his Oriental Quadrat; yet he doubts not, but that the shadow appears on the Left-side, considering, that the Existence of the Ring can be no longer doubted of, after so many Observations of the shadow cast by Saturn's Body upon it, according as it must happen, following that Hypothesis, there being no reason, why it should cast the said shadow on one side, and not on the other.

Concerning the Observation of Jupiter and its satellites, the famous Astronomer of Bononia, Cassinu, having published,
lished, that on the 30. day of July, 1664. at 2½ of the clock in
the morning, he had observ'd, with Campani's Glasse, that
there passed through the broad obscure Belt of Jupiter two ob-
scurer spots, by him esteemed to be the shadows of the Satellites,
moving between Jupiter & the Sun, and eclipsing him, and emerging
from the Occidental Brim thereof: This Author did first
conceive, that they were not shadows, but some Sallies, or Pro-
minencies in that Belt; which he was induced to believe, because
he perceived not, that that Prominency, which he there saw, was so
black, nor so round as Caffini had represented his spots; where-
fore, seeing it but little differing in colour, from the Belt, and so
not judging it round, because it did stand only about half its dia-
meter out of the Belt, he persuaded himself, that it was rather a
Sally, or Prominency of the Belt, than a round shadow, as that of a
Satellite of Jupiter must have bin. But having been since inform-
ed of all the Observations made by Caffini and Campani, with the
New Glasse, and seen his Figure, he candidly and publickly wiseth,
that he had not spoken of that Sally, or Prominency; ad-
vowing that he can doubt no longer, but that it was the shadow of
the Satellite between Jupiter and the Sun, having seen the other
emerge, as soon as with a 20. foot Glass he made the Observati-
on, and having not perceiv'd these shadows with a 12. foot Glass:
But although he grants that they did ghes better than he, yet he
doeth it with this proviso, vid. In case they made that Observa-
ton of July 30. not with their 36. but 12. or 17. foot Telescope.
If it be wondered at, that Monsieur Augout did not see this sha-
dow move, he allegeth his indisposition for making long Observa-
tions, and addeth, that it may be much more wondred at, that nei-
ther Campani nor himself did see upon the obscure Belt the Bo-
dies of the Satellites, as parts more Luminous than the Belt. For
(fait he) although the Latitude was Meridional, it being no more
than of 9. or 10. minutes, the Body of the Satellites should, thinks
he, pass between us and the Belt, especially according to Campa-
ni, whomaketh the Belt so large, and puts the shadows far enough
within the same. This maketh him conclude, that either they
have not observ'd well enough, or that the motion of the Satel-
lites doth not exactly follow the Belts, and is inclin'd unto them.
Whereupon he resolves, that when he shall know that they are to
pass between Jupiter and us, and to be over against the Belt, that
then:
then he will observe, whether he can see them appear upon the
Belt, as upon a darker ground, especially, the third of them, which
is sensibly greater, and more Luminous, than the rest. He hopeth
also, that in time, the shadow of Saturn's Moon will be seen upon
Saturn, although we are yet some years to stay for it, and to pre-
pare also for better Glasses.

From this rare Observation, he infers the Proportion of the
Diameter of the Satellites to that of Jupiter; and judgeth, that no
longer doubt can be made of the turning of these 4. Satellites,
or Moons about Jupiter, as our Moon turns about the Earth, and
after the same way as the rest of the Celestial Bodies of our
Systeme do move: whence also a strong conjecture may be made,
that Saturn's Moon turns likewise about Saturn.

Hence he also taketh occasion to intimate, that we need not
scruple to conclude, that if these two Planets have Moons wheel-
ing about them, as our Earth hath one that moves about it, the
conformity of these Moons with our Moon, does prove the confor-
mitv of our Earth with those Planets, which carrying away their
Moons with themselves, do turn about the Sun, and very proba-
ibly make their Moons turn about them in turning themselves
about their Axis; and also, that there is no cause to invent per-
plex'd and incredible Hypotheses, for the receding from this
Analogie since (faith he) if this be truth, the Prohibitions of pub-
lishing this doctrine, which formerly were caused by the offence
of Novelty, will be laid aside; as one of the most zealous Doctors
of the contrary Opinion hath given cause to hope, witness Eu-
stachius de Divinis, in his Tract against Monsieur Hugen's Systeme
of Saturn, p. 49. where we are inform'd, that that learned Jesuit,
P. Fabry, Penitentiary of S. Peter in Rome, speaks to this purpose:

* Ex veftris, iijque Coryphæis non femel quæstionem eft, utrum
aliquam habent demonstrationem pro Terra mox additru-
endo. Nunc quam aui funt id
aferere Nil igitur obiatur, quin
loca illa in fententia literali Ecclési-
sia intelligat, & intelligenda
effe declarat, quamfù nulla de
monstratone contrarium evin-

* It hath been more than once asked of your
Chieftains, whether they had a Demonstration
on for affenting the motion of the Earth? They
durst never yet affirm they had; wherefore noth-
ing binders, but that the Church may under-
stand those Scripture-places, that speak of
this matter, in a literal sense, and declare
they should be so understood, as long as the con-
trary is not evinced by any demonstration;
which
which, if perhaps it should be found out by you
(which I can hardly believe it will) in this case
the Church will not at all scruple to declare,
that these places are to be understood in a fi-

gurative and improper sense, according to
that of the Poet, Terræque Urbisque reced-
cedunt.

Whence this Author concludes, that the said Jesuite affur-
ging us that the Inquisition hath not absolutely declared, that those Scrip-
ture-places are to be understood literally, seeing that the Church
may make a contrary declaration, no man ought to scruple
to follow the Hypothecis of the Earths motion, but only forbear to
maintain it in publick, till the prohibition be called in. But to
return to the matter in hand, this Author, upon all these obser-
vations and relations of Cassini and Campani, doth find no reason to
doubt any more of the excellency of the Glass used by them,
above his; except this difference may be imputed to that of
the Air, or of the Eys. But yet he is rather inclined to ascribe it
to the goodness of their Glasses, and that the rather, because,
he would not be thought to have the vanity of magnifying his
own; of which, yet he intimates by the by, that he caused
one to be wrought, of 150 Parisian feet; which though it
proved none of the best, yet he despairs not to make good ones of
that, and of far greater Length.

Signor Campani's Answer: and Monsieur
Auzout's Animadversions thereon.

The other part of this French Tract, containing Campani's An-
swer, and Mr. Auzout his Reflections thereon, begins with the pre-
tended Shadows of the Ring upon Saturn, and of Saturn upon the
Ring. Concerning which, the said Campani declareth, that he ne-
ever believed them to be shadows, made by the Ring upon the Disk
of Saturn, or by the body of Saturn upon the Ring, but the Rims
of these bodies, which being unequally Luminous, did shew these
appearances. In which Explication, forasmuch as it represents,
that the said Campani meant to note only the Inequality of the
Light, which, he saith, his Glasses did discover, Mr. Auzout does
so far acquiesce, that he only wishes, that his own Glasse would shew him those differences. Next to the Objection, made by Monsieur Auxout, against Signor Campani, touching the Proportion of the Length of the Ring to its breadth, Campani replyeth, that the Glasse of Monsieur Auxout shew not all the particulars, that his do, and therefore are unfit for determining the true Figure and breadth of the apparent Ellipsis of the Ring. To which M. Auxout rejoyns, that he is displeased at his being destitute of better Glasse, but that it will be very hard for the future to convince Campani touching the Proportion of the Ring, seeing that the breadth of the Ellipsis is always diminishing, although, if the declination of the Ring remains always the same, one can at all times know, which may have been its greatest breadth. But he assures, that the breadth of the Ring is not the half of its length, and that it doth not spread out so much beyond Saturn's Body, as he hath alleged. And withal desir's to know, what can be answered by Sig. Campani to M. Hugens, who being persuaded, that the Declination of the Ring is not above 23 deg. 30' having seen the Ring to spread out above the Body of Saturn, concludes, in a Letter to M. Auxout, that the length of the Ring is more than treble the Diameter of Saturn's body, which, according to Campani, is only as about 67 to 31. Which difference yet doth not appear to M. Auxout to be so great; but that M. Hugens perhaps will impute it to the Optical reason, which he (Auxout) hath alleged of the Advance of the light upon the obscure space; although he is of Opinion, he should not have concluded so great a length, if he had not seen the breadth spread out more, than he hath done: for (faith he) if the length of the Ring be to the body of Saturn, as 2 ½ to 1, and the Inclination be 23 deg. 30', the Ring will be just as large, as the body, without spreading out; but if the Ring be bigger, it will a little spread out; and if it were treble, it must needs spread out the half of its breadth, which hath not so appeared to him.

Further, to M. Auxout's change of Opinion, and believing, that the Advance or Sally, seen by him in Jupiter, was the Shadow of one of his Moons, Campani declares, that he would not have him guilty of that change: Whereupon M. Auxout wonders, why Campani then hath not marked it in his Figure; and would gladly
ly know whether that Sally be more easie to discover, than the
shadows of the Satellites, which Campani believes, Ascout hath not
seen; and whether he be assured, that those obscure parts, which
he there distinguishes, do not change: for if they should not
change, then Jupiter would not turn about his Axis, which yet, he
faith, it doth, according to the Observation made by Mr. Hook,
May 9 1664. inserted in the first papers of these Transactions.
The full Discovery of which particular also he makes to be a
part of Caffini's and Campani's work, seeing that they so distinct-
ly see the inequalities in the Belts, and see also sometimes other
spots besides the shadows of the Satellites: where he exhorts
all the Curious, that have the conveniency of observing, to en-
deavor the discovery of a matter of that importance, which
would prove one of the greatest Analogies for the Earth's Motion.

An Account of Mr. Richard Lower's newly
published Vindication of Doctor Willis's
Diatriba de Febribus.

The Title of this Curious piece, is, Diatriba Thomæ Willi-ś
Med. Doct. & Profess. Oxon. De Febribus Vindicatio, Authore Ri-
charde Lower, &c. In it are occasionally discussed many con-
siderable Medical and Anatomical inquiries, as, Whether a
Fever does consist in an Effervescence of Blood? And if so,
of what kind? Whether there be a Nervous and Nutritious
Juice? Whether the office of sanguification belongs to the
Blood it self, existing before those Viscera (at least) that are
commonly esteemed to be the Organs of sanguification? How
Nutrition is performed, and the nourishing substance assimila-
ted? Whether the Blood affords both the Matter for the struc-
ture of the Body, and such parts also, as are fit for the nourish-
ment of the same? Whether the Pulse of the Heart ceasing,
there remains yet a certain Motion in the blood, arguing, that
Pulse and Life do ultimately rest in the Blood? Whether the
Umbilical Vessels convey the blood of the Mother to the
Child, or whether the Fœtus be for the most part form'd and
acted by the circulating blood, before the existence of the Umbilical Vessels, or before the connecting of the Fœtus with the Uterus? A new Experiment to prove, that the Chyle is not transmuted into Blood by the Liver. A discourse of the Nature of the Blood, and what difference there is between the Venal and Arterial blood, and for what Uses both the one and the other are particularly designed. Where it is considered, what Life is, and whence the Soul of Brutes, and its subsistence, and operations do depend. It is also inquired into, what the uses of the Lungs are in hot Animals? And many other such material disquisitions are to be found in this small, but very Ingenious and Learned Treatise.

A Note touching a Relation, inserted in the last Transactions.

In the Experiment of killing, Rattle-Snakes, mentioned in the last of the precedent Papers (wherein, by a mistake, these words, The way, were put for A way, or An Experiment) it should have been added, that the Gentleman there mention'd, did affirm, that, in those places, where the Wild Penny-Royal or Dittany grows, no Rattle-Snakes are observed to come.

Errata.

Pag. 59. line 11. read, bignesses, l. 20. r. endure, for, resist. l. 30. r. those, for, these. l. 31. r. Plain, for, place.

L O N D O N,
Printed with Licence, By John Martyn, and James Allestry, Printers to the Royal-Society, at the Bell in St. Pauls Church-Yard. 1665.
The Contents.

An Account, how Adits and Mines are wrought at Liege without Air-shafts, communicated by Sir Robert Moray. A way to break easily and speedily the hardest Rocks; imparted by the same Perlon, as he received it from Monsieur Du Son, the Inventor. Observables upon a Monstrous Head. Observables in the Body of the Earl of Belcarres, sent out of Scotland. A Relation of the designed Progress to be made in the Breeding of Silkworms, and the Making of Silk, in France. Enquiries touching Agriculture, for Arable and Meadows.

An Account, how Adits & Mines are wrought at Liege without Air-shafts, communicated by Sir Robert Moray.

It is well known to those conversant in Mines, that there is nothing of greater inconvenience in the working or driving, as they call it, of Mines or Adits under ground, for carrying away of Water, or such Minerals as the Mine affords, than the Damps, want, and impurity of Air, that occur
cur, when such Adits are wrought or driven inward upon a Level, or near it, 20, 30, or 40. fathom, more or less: As with because of the expence of money, as of time also, in the ordinary way of preventing or remedying those inconveniences, which is, by letting down shafts from the day (as Miners speak) to meet with the Adit; by which means the Air hath liberty to play through the whole work, and so takes away bad vapours and furnishes good Air for Respiration. The Expence of which shafts, in regard of their vast depth, hardness of the Rock, drawing of water &c. doth sometimes equal, yea exceed the ordinary charge of the whole Adit.

Amongst the expedients that have been devised to remedy this, there is one practiced in the Coal-mines, near the Town of Liige (or Luyck) that seems preferable to all others for Efficacy, Ease, and Cheapness: the description whereof followeth.

At the mouth or entry of the Adit, there is a structure raised of Brick, like a Chimney, some 28. or 30. foot high in all; at the bottom, two opposite sides are (or may be) some 5½ foot broad; and the other two, 5. foot: the wall 1½ Brick thick. At the lower part of it, is a hole, some 9. or 10. inches square, for taking out of the Ashes, which when it is done, this Ash-hole is immediately stopped so close, as Air cannot possibly get in at any part of it. Then, some 3. foot above ground or more, there is on that side, that is next to the Adit or Pit, a square hole of 8. or 9. inches every way, by which the Air enters to make the Fire burn: Into this hole there is fixed a square Tube or Pipe of Wood, whereof the Joints and Chinks are so stopped with Parchment pasted or glewed upon them, that the Air can no where get in to the Pipe but at the end: And this Pipe is still lengthened, as the Adit or Pit advances, by fitting the new Pipes so, as one end is always thrust into the other, and the Joints and Chinks still carefully cemented and stopped as before. So the Pipe or Tube being still carried on, as near as is necessary, to the wall or place, where fresh Air is requisite; the Fire within the Chimney doth still attract.
Air through the Tube, without which it cannot burn, which yet it will do, as is obvious to conceive, (all Illustrations, and Philosophical Explications being here superfluous,) and so, while the Air is drawn by the fire from the farthest or most inward part of the Mine or Adit, fresh Air must needs come in from without, to supply the place of the other, which by its motion doth carry away with it all the vapors, that breath out of the ground; by which means the whole Adit will be always filled with fresh Air, so that men will there breath as surely as abroad, and not only Candles burn, but Fire, when upon occasion there is use for it for breaking of the Rock.

Now that there may be no want of such fresh Air, the Fire must always be kept burning in the Chimney, or at least as frequently as is necessary; For which purpose there must be two of the Iron Grates or Chimneys, that when any accident befals the one, the other may be ready to be in its place, the Coals being first well kindled in it; but when the fire is neer spent, the Chimney or Grate being haled up to the dore, is to be supplied with fresh fuel.

The Figure of the Fabrick, Chimney, and all the parts thereof being hereunto annexed, the rest will be easily understood.

**Figure 1.**

_A. The Hole for taking out the Ashes._

_B. The Square-hole, into which the Tube or Pipe for conveying the Air is to be fixed._

_C. The Border or Ledge of Brick or Iron, upon which the Iron-grate or Cradle, that holds the burning Coles, is to rest, the one being exactly fitted for the other._

_D. The Hole where the Cradle is set._

_E. The woorden Tube, through which the Air is conveyed towards the Cradle._

_F. The Dore, by which the Grate and Cradle is let in, which is
to be set 8, or 10 foot higher than the Hole D and the Shutter made of Iron, or Wood that will not shrink, that it may shut very close; this Door being made large enough to receive the Cradle with ease.

G. The Grate or Cradle, which is narrower below than above, that the Ashes may the more easily fall, and the Air excite the Fire; the bottom being barred as the sides.

H. The Border or Ledge of the Cradle, that rests upon the Ledge C.

I. Four Chains of Iron fastned to the four corners of the Cradle, for taking of it up, and letting of it down.

K. The Chain of Iron, to which the other are fastned.

L. The Pulley of Iron or Brass, through which the Chain paffeth.

M. A Hook, on which the end of the Chain is fastned by a Ring, the Hook fixed being placed in the side of the Door.

N. A Barr of Iron in the Walls, to which the Pulley is fastned.

The higher the Shaft of the Chimney is, the Fire draws the Air the better. And this Invention may be made use of in the Pits or Shafts, that are Perpendicular, or any wise inclining towards it, when there is want of fresh Air at the bottom thereof, or any molestation by unwholesom Fumes or Vapours.

A way to break easily and speedily the hardest Rocks, communicated by the same Person, as he received it from Monsieur Du Son, the Inventor.

Though the Invention of breaking with ease, and dispatch, hard Rocks, may be useful on several occasions, the benefit is incomparably great, that may thereby accrue to those, who have Adits or Passages to cut through hard Rocks, for making passage for Water to run out by, in Mines of Lead, Tin, or any other whatsoever; these Adits appearing to be the surest, cheapeft, and most advantageous way imaginable, for draining of the same.
That which is here to be described, was invented by one of the most Excellent Mechanicks in the World, Monsieur du Son, who lately put it in practice himself in Germany, at the desire of the Elector of Mentz. The manner is, as followeth.

The Mine or Adit is to be made seven or eight foot high, which though it seem to make more work downwards, yet will be found necessary for making the better dispatch by rendering the Invention more effectual.

There is a Tool of Iron well-fueled at the end, which cuts the Rock, (of the shape shewed by Fig. 2, here annexed;) 20. or 22. Inches long or more, and some 2½ Inches Diameter at the fueled end, the rest being somewhat more slender. The fueled end is so shaped, as makes it most apt to pierce the Rock, the Angles at that end being still to be made the more obtuse, the harder the Rock is. This Tool is to be first held by the hand, in the place, where the Hole, to be made for the use, which shall here be shewed, is to be placed; that is, in the middle between the sides of the Rock, that is to be cut, but as near the bottom as may be. The Tool being placed, is to be struck upon with an Hammer, the heavier the better, either suspended by a Shaft turning upon a Pin, or otherwise, so as one man may manage the Hammer, while another holds the Tool or Piercer. If it be hung in a Frame, or other convenient way, he that manageth it hath no more to do, but to pull it up at first as high as he can, and let it fall again by its own weight, the motion being so directed, as to be sure to hit the Piercer right. After the stroke of the Hammer, he that holds the Piercer, is to turn it a little on its point, so that the Edges or Angles at the point may all strike upon a new place; and so it must still be shifted after every stroke, by which means, small Chipps will at every stroke be broken off, which must from time to time be taken out, as need requires. And thus the work must be continued, till the Hole be 18. or 20. Inches deep, the deeper the better. This Hole being made as deep as is required, and kept as straight and smooth in the sides, as is possible, there is then a kind of double Wedge to be made, and
fitted exactly for it; the shape whereof is to be seen in the annexed 3. Figure.

This double Wedge, being 12. or 13. Inches long, each piece of it, and so made, as being placed in their due position, they may make up a Cylinder, cut Diagonal-wise. The two flat sides, that are contiguous, are to be greased or oyled, that the one may slip the more easily upon the other; and one of them, which is to be uppermost, having at the great end a hollow Creafe cut into it round about, for fastning a Cartridge, full of Gunpowder, to it with a thred, the round end of the Wedge being pared as much, as the thickness of the Paper or Paftboard, that holds the Powder, needs to make the outside thereof even with the rest of the Wedge. This Wedge must have an Hole drilled through the longest side of it, to be filled with priming Powder, for firing of the Powder in the Cartridge; which needs have no more, than half a pound of Powder, though upon occasion a greater quantity may be used, as shall be found re-

quisite.

Then this Wedge, being firft thrust into the Hole with the Car-
tridge, the round fide, where the Priming hole is, being upper-
moft, the other Wedge is to be thrust in, home to the due posi-
tion, care being taken, that they fit the Hole in the Rock as exa-
ctly as may be. Then the end of the lower Wedge being about an Inch longer, than that of the upper outwardly, and flatned, priming Powder is to be laid upon it, and a piece of burning Match or Thread dipt in Brimfome or other fuch pre-
pared combustible Matter, fastned to it, that may burn fo long, before it fire the Powder, as he, that orders it, may have time enough to retire quite out of the Pit or Adit, having firft placed a piece of Wood or Iron fo, as one end thereof, being set against the end of the lower Wedge, and the other againft the fide-wall, fo as it cannot flip. Which being done, and the Man retired, when the Powder comes to take fire, it will firft drive out the uppermoft Wedge, as far as it will go; but the flaun-
ting figure of it being fo made, as the farther it goes backward,
the thicker it grows, till at the laft it can go no farther, then the fire
fire tears the Rock to get forth, and so crackes and breaks it all about, that at one time a vast deal of it will either be quite blown out, or so crackt and broken, as will make it easy to be remov'd: And according to the effect of one such Cartridge, more may be afterwards made use of, as hath been said.

Observables upon a Monstrous Head.

This was the Head of a Colt, represented in the annexed Figure 4, first viewed by Mr. Boyle, who went into the Stable where the Colt lay, and got the Head haftily and rudely cut off, the Body thereof appearing to his Eye compleatly formed, without any Monstrofity to be taken notice of in it. Afterwards he caused it to be put into a Vessel, and covered with Spirit of Wine, thereby chiefly intending, to give good example, together with a proof, that by the help of the said Spirit, (which he hath recommended for such Properties in one of his Essays of the Usefulness of Natural Philosophy) the parts of Animals, and even Monsters, may in Summer it self be preserved long enough, to afford Anatomists the opportunities of examining them.

The Head being opened, and examined, it was found,

First, That it had no sign of any Nose in the usal place, nor had it any, in any other place of the Head, unless the double Bagg CC, that grew out of the midft of the forehead, were some rudiment of it.

Next, That the two Eyes were united into one Double Eye, which was placed just in the middle of the Brow, the Nose being wanting, which should have separated them, whereby the two Eye-holes in the Scull were united into one very large round hole, into the midft of which, from the Brain, entred one pretty large Optick Nerve, at the end of which grew a great Double Eye; that is, that Membrane, called Sclerotis, which contained both, was one and the fame, but seemed to have a Seam, by
by which they were joined, to go quite round it, and the fore or pellucid part was distinctly separated into two Cornea’s by a white Seam that divided them. Each Cornea seemed to have its Iris, (or Rain-bow-like Circle) and Apertures or Pupils distinctly; and upon opening the Cornea, there was found within it two Balls, or Crystalline Humours, very well shaped; but the other parts of it could not be so well distinguished, because the eye had been much bruised by the handling, and the inner parts confused and dislocated. It had four Eye-browes, placed in the manner express in Figure 4. by a a, b b; a a representing the lower, and b b, the upper Eyelids.

Lastly, That just above the Eyes, as it were in the midst of the Forehead, was a very deep depression, and out of the midst of that grew a kind of double Purse or Bagg, C C, containing little or nothing in it; but to some it seemed to be a production of the matter designed for the Nose, but diverted by this Monstrous Conception; perhaps the Processus mammillares joined into one, and covered with a thin hairy skin.

**Observables in the Body of the Earl of Balcarres.**

These following Observations, were a while since sent out of Scotland by an ingenious person, an Eye-witnes, to Sir Robert Moray.

1. That the Belly of this Nobleman being opened, the Omentum or Net was found lean and small: his Liver very bigg; the Spleen bigg also, filled with a black and thick humour. His Stomach and Entrails all empty, of a Saffron-colour, distended with wind onely. The Bladder of Gall swelled with a black humour: the Kidneys filled with a kind of grumous blood.

2. That in the Thorax or Chest, the Lobes of the Lungs were all entire, but of a bad colour; on the left side somewhat black and blue, and on the right, whitish; with a yellowish knob under one of the Lobes.

3. That
3. That the Pericardium or the Case of the Heart being opened, there appeared none of that water, in which the Heart uses to swim; and the external Surface of it, from the Base to the Tip, was not smooth, but very rough. It being cut afunder, a quantity of white and insipidate liquor run out, and beneath the Base, between the right and left Ventricle, two stones were found, whereof the one was as bigg as an Almond, the other, two Inches long and one broad, having three Auricles or crisped Angles: And in the Orifice of the right Ventricle, there was a fleshy fattish Matter.

4. That the whole Body was bloudless, thin, and emaciated, of a black and bluish Colour.

5. The Scull being opened, both the Cerebrum and Cerebellum were bigg in proportion to the Body; and out of it run much more Bloud, than was seen in both the other Regions together.

Of the designed Progress to be made in the Breeding of Silkworms, and the Making of Silk, in France.

The French King Henry the Fourth, having made a general Establishment all over France, of planting and propagating of Mulberry-trees, and Breeding of Silkworms, in order to set up and entertain a Silktrade there; and having prospered so well in that Design, that in many parts of his Dominions great store of such Trees were rais'd, and Multitudes of Silk-works propagated, to the great benefit of the French people, forasmuch as it was a considerable beginning to avoid the transport of several Millions abroad for buying of Silks, and withall an excellent means of well-employing abundance of poor Orphans and Widows, and many old, lame, and other indigent and helpless people; The present French King, hath lately revived and seconded that Undertaking, by giving express order, that it should be promoted by all possible means, and particularly in the Metropolis of that Kingdom, and round about it; and that for that end the whole way concerning that Work and Trade
Trade should be fully and punctually communicated in Print; which hath also been executed by one Monseur Isnard, in a Treatise published at Paris, in French, Intitled, 

Instructions for the Planting of White Mulberryes, the Breeding of Silkworms, and the Ordering of Silk in Paris, and the circumjacent Places. In which Book, the Method being represented, which that Great Prince Henry IV. used in establishing the said Work and Trade, together with the success thereof, and the advantages thence derived to his Subjects, the Author, from his own Experience, and long Practice, delivers (and seems to do it candidly) all what belongs in this business in four main heads. 

First, he teaches the Means of sowing, planting, and raising White Mulberryes (as the Foundation of Silkworks) shewing how many several ways that may be done. Secondly, The Breeding of Silkworms, the choosing of good Eggs, and their hatching, as also the Feeding of the Worms, and preserving them from Sickness, and Curing them of it, together with the way of making them spin to best advantage. Thirdly, The manner of winding their Silk from their Bottoms, adding the scheme of the Instrument serving for that purpose. Fourthly, The way of keeping Silkworms Eggs for the ensuing year.

Through the whole Book are scattered many not inconsiderable particulars, though perhaps known to most. The White Mulberry Tree, as it is in other qualities preferable to the Black, to this Author esteems it the best, not only for the durableness of the wood, and its large extent of usefulness in Carpentry and Joyners work; but also for the fitness of its leaves (besides their principal use for the food of Silkworms) to fatten Sheep, Goats, Cowes, and Hoggs, only by boiling and mingling them with Bran. The Berryes themselves he commends as very excellent to fatten Poultry, and to make them lay Eggs plentifully. In the Changes, Working, and Generation of this Insect, he is very curious to observe many things. Their Metamorphoses, as is known, are four, whereof the form of the one hath no conformity with any of the rest. The first from an Egge (of the bigness of a Mustard-seed, and of a darkish Gray Colour, when good) to a Worm or Caterpillar, but of a dome-Rick, noble, and profitable kind; Black, when it first comes forth
forth, but growing white at last; having 24. feet, 8. on each side of the body, and 4. besides, close to each side of the head. During this form, they undergo constantly 4. Sicknesses, in which they cast their Skins, each sickness lasting about 4. days, wherein they feed not at all; but grow clearer, shorter, and thicker. The second, from a Worm to an Aurelia or Chrysalis, having the shape of a small Plum, whereunto it is transformed after its spinning time is past; in which state it lyes shut up, in hot Countries, for 14. or 15. dayes; in more temperate ones, 18. or 20. without any Food or Air, known to us. During which time this Insect leaves two Coats, both that of a Worm, whence 'tis changed into an Aurelia, and that of an Aurelia, whence it becomes a Papilio or Butterfly, in the Theca or Case. The third is, from an Aurelia to a Butterfly, coming out of the Theca with a head, legs, and horns; for which passage it makes way by a whitish water, it casts upon the Silk, which moistening, and thereby in a manner putrefying it, the new creature thrusts out its head through the sharp end of the Case, by a Hole as big as itself. There is found no Excrement in the Case, but the two Skins onely, just now mentioned.

Before they begin to spin, and about the latter end of their feeding, they must, faith the Author, be often changed, and have Air enough, by opening the Windows of the Room, they are in, if it be not too ill Weather; else, faith he, the Silk that is in their Belly, will cause to extraordinary a heat in them, that it burns their guts, and sometimes bursts them; and the same (being a substance that resembleth Gum or Burgundy Pitch) will putrefy and turn into a yellowish matter.

He maketh the best marks of their maturity for spinning to be, when they begin to quit their white Colour, and their green and yellow Circles, and grow of the Colour of Flesh, especially upon the tail; having a kind of consistent softness, shewing that they have something substantial in their Stomachs.

As for their Working, he gives this account of it, that the first day they make only a Webb; the second, they form in this Webb their Cases, and cover themselves all over with Silk; the third day, they are no longer seen, and the dayes following they thicken their Cases, always by one end or thread, which they
never break off, themselves. This, he affirms, they put out with so much quickness, and draw it so subile and so long, that, without an Hyperbole, the end or thread of every Case may have two Leagues in length. Headvertiseth, that they must be by no means interrupted in their work, to the end, that all the Silk, they have in their bellies, may come out.

Some eight days after they have finished their Work, as many of the best Cases, as are to serve for seed, viz. the first done, the hardeft, the reddeft and best coloured, must be chosen, and put a-part; and all diligence is to be used to winde off the silk with as much speed, as may be, especially if the Worms have nimbly dispatched their work.

Here he spends a good part of his Book, in giving very particular Instructions, concerning the way of winding off the silk, letting also down the form of the Oven and Instruments necessary for that work, which is the painfuleft and niceft of all the rest.

Touching their Generation, he prescribeth that there be chosen as many male as female Cases (which are discerned by this, that the males are more pointed at both ends of the Cases, and the females more obtufe on the ends, and bigger-bellyed) and that care be had, that no Cases be taken, but such wherein the Worms are heard rolling; which done, and they being come forth in the form of Butterflyes, having four wings, six feet, two horns, and two very black eyes, and put in a convenient place, the males fluttering with their wings, will joyn and couple with the females, after that these have first purged themselves of a kind of reddish humour by the fundament: in which posture they are to be left from Morning (which is the ordinary time of their coming forth) till evening, and then the females are to be gently pulled away, whereupon they will lay their eggs, having first let fall by the Fundament another humour, esteemed to proceed from the seed of the males; but the males are then thrown away as useless. He advertiseth, that if they be coupled longer then 9, or 10 hours, (which they will be, and that sometimes for 24 hours together, if they be let alone) either the female will receive very great hurt by it, or much seed will remain in her belly.
The feed at first coming out is very white, but within a day it becomes greenish, then red, at last by little and little gray, which colour it retains always, the most coloured of an obscure gray, being the best; those grains, which never quit their whiteness, having no fecundity in them.

Each female emits ordinarily some 300 grains, more or less, some of them not being able to render them all, and dying with them in their belly. One ounce of feed will require an hundred pair of Cafes, of as many Males as Females.

Care must be taken, that no Rats, Mice, Ants, or other Vermin, nor any Hens, nor any Birds come near the Seed, they being very greedy to eat them.

This is the substance of what is contained in this French Author, published at Paris on purpose to promote the Making of Silk there, as well as it is practised already in other parts of that Kingdom: which is represented here, to the end; that from this occasion the design, which the English Nation once did entertain of the increasing of Mulberry-trees, and the Breeding of Silk-worms, for the making of Silk within themselves, may be renewed, and that encouragement, given by King James of Glorious memory for that purpose (witness that Letter which he directed to the Lords Lieutenants of the several shires of England) and seconded by his Most Excellent Majesty, that now is, be made use of, for the honour of England and Virginia, and the increase of wealth to the people thereof: especially since there is cause of hope, that a double Silk-harvest may be made in one Summer in Virginia, without hindring in the least the Tobacco-Trade of that Countrie.

Enquiries concerning Agriculture.

Whereas the Royal Society, in prosecuting the Improvements of Natural knowledge, have it in design, to collect Histories of Nature and Arts, and for that purpose have already, according to the several Inclinations and Studies of their Members, divided themselves into divers Committees, to execute the said design: Those Gentlemen, which do constitute the Committee for considering of Agriculture, and the History and Improvement thereof, have begun their work with drawing up certain Heads
Heads of Enquiries, to be distributed to persons Experienced in Husbandry all over England, Scotland, and Ireland, for the procuring a faithful and solid information of the knowledge and practice already obtained and used in these Kingdoms; whereby, besides the aid which by this means will be given to the general End of collecting the aforementioned History, every place will be advantaged by the helps, that are found in any, and occasion ministered to consider, what improvements may be further made in this whole matter. Now to the End, that those Enquiries may be the more universally known, and those who are skilful in Husbandry, publicly invited to impart their knowledge herein, for the common benefit of their Country, it hath been thought fit to publish the effect of them in Print, and withall to desire, that what such persons shall think good from their own Knowledge and Experience to communicate hereupon, they would be pleased to send it to the Printers of the Royal Society, to be delivered to either of the Secretaries of the same. The Enquiries follow.

1. For Arable.

1. The several kinds of the Soyls of England, being supposed to be, either Sandy, Gravelly, Stony, Clayey, chalky, Light-mould, Heathy, Marish, Boggy, Fenny, or Cold weeping Ground; information is desired, what kind of Soyls your Country doth most abound with, and how each of them is prepared, when employed for Arable?

2. What peculiar preparations are made use of to these Soyls for each kind of Grain; with what kind of Manure they are prepared; when, how, & in what quantity the Manure is laid on?

3. At what seasons and how often they are ploughed; what kind of Ploughs are used for several sorts of Ground?

4. How long the several Grounds are let by fallow?

5. How, and for what productions, Heathy Grounds may be improved? And who they are (if there be any in your Country) that have reduced Heaths into profitable Lands?

6. What ground Marle hath over head? How deep generally it lieth from the surface? What is the depth of the Marle itself? What the colour of it? Upon what grounds it is used?
What time of the year it is to be laid on? How many loads to an Acre? What Grains Marled Land will bear, and how many years together? How such Marled Land is to be used afterwards, &c?

7. The kinds of Grain or Seed, usual in England, being supposed to be either Wheat, Miscelane, Rye, Barley, Oats, Peafe, Beans, Fitches, Buck-wheat, Hemp, Flax, Rape; We desire to know, what sorts of Grains are sown in your Country, and how each of these is prepared for Sowing? Whether by steeping, and in what kind of Liquor? Or by mixing it, and with what?

8. There being many sorts of Wheat, as the White or Red Lammas, the bearded Kentifh Wheat, the gray Wheat, the red or gray Pollard, the Ducks-bill Wheat, the red-eared-bearded Wheat, &c. And so of Oats, as the common Black, Blue, Naked, Bearded in North-wales: and the like of Barley, Peafe, Beans, &c. The Enquiry is, which of these grow in your Country, and in what Soyl; and which of them thrive best there; and whether each of them require a peculiar Tillage; and how they differ in goodness?

9. What are the chief particulars observable in the choice of Seed corn, and all kinds of Grain; and what kinds of Grain are most proper to succeed one another?

10. What Quantity of each kind is sown upon the Statute-Acre? And in what season of the Moon and year it is sowed?

11. With what instruments they do Harrow, Clod and Rowl, and at what seasons?

12. How much an Acre of good Corn, well ordered, generally useth to yield, in very good, in less good, and in the worst years?

13. Some of the common Accidents and Diseases befalling Corn in the growth of it, being Meldew, Blasting, Smut; what are conceived to be the Causes thereof, & what the Remedies?

14. There being other Annoyances, the growing Corn is exposed to, as Weeds, Worms, Flies, Birds, Mice, Moles, &c. how they are remedied?

15. Upon what occasions they use to cut the young Corn in the Blade, or to feed it, and what are the benefits thereof?

16. What are the seasons and waies of Reaping and Ordering each sort of Grain, before it be carried off the Ground?

17. VVhat
What are the several ways of preserving Grain in the Straw, within and without doors, from all kind of Annoyance, as Mice, Heating, Rain, &c?

What are the ways of separating the several sorts of Grain from the Straw, and of dressing them?

What are the ways of preserving any stores of separated Grain, from the Annoyances they are obnoxious to?

2. For Meadows.

1. How the above-mentioned sorts of Soyl are prepared, when they are used for Pasture or Meadow?

2. The common Annoyances of these Pasture or Meadow Grounds being supposed to be, either Weeds, Moss, Sour-graffs, Heath, Fern, Bushes, Bryars, Brambles, Broom, Rushes, Sedges, Gorse or Furzes; what are the Remedies thereof?

3. What are the best ways of Drayning Marshes, Boggs, Fens, &c?

4. What are the several kinds of Grafs, and which are counted the best?

5. What are the chief circumstances observable in the Cutting of Grafs; and what in the making and preserving of Hay?

6. What kind of Grafs is fittest to be preserved for winter-feeding? And what Grafs is best for Sheep, for Cows, Oxen, Horses, Goats, &c.

Advertisement.

The Reader is hereby advertised, that by reason of the present Contagion in London, which may unhappily cause an interruption as well of Correspondencies, as of Publick Meetings, the printing of these Philosophical Transactions may possibly for a while be intersmitted; though endeavors shall be used to continue them, if it may be.

LONDON,

Printed with Licence, By John Martyn, and James Allestry, Printers to the Royal-Society, at the Bell in St. Pauls Church-Yard. 1665.
Monday, Novemb. 6. 1665.

The Contents.

An Account of a not ordinary Burning Concave, lately made at Lyons, and compared with several others made formerly.

Of Monfieur Hevelius his promise of communicating to the World his Invention of making Optick Glasses; and of the hopes, given by Monfieur Christian Hugens of Zulichem, to perform something of the like nature; as also of the Expectations, conceived of some Persons in England, to improve Telescopes. An Intimation of a way of making more lively Counterfeits of Nature in Wax, then are extant in Painting, and of a new kind of Maps in a low Relievo, or Sculpture, both practised in France. Some Anatomical Observations, of Milk found in Veins instead of Blood; and of Grass, found in the Wind-pipes of some Animals. Of a place in England, where, without petrifying Water, Wood is turn’d into Stone. Of the nature of a certain Stone, found in the Indies in the head of a Serpent. Of the way, used in the Mogol’s Dominions, to make Salt-petre. An Account of Hevelius his Prodromus Cometicus, and of some Animadversions made upon it by a French Philosopher; as also of the Jesuit Kircher’s Mundus Subterraneus.

An Account of a not ordinary Burning Concave, lately made at Lyons, and compared with several others made formerly.

An opportunity being presented to revive the publishing of these Papers, which for some Months hath been discontinued
discontinued by reason of the great Mortality in London, where they were begun to be Printed; it hath been thought fit to embrace the fame, and to make use thereof for the gratifying of the Curious, that have been pleased to think well of such Communications: To re-enter whereupon, there offers it self first of all a Relation of an un-common Burning Glass, not long since made in France, in the City of Lyons, by one called Monsieur de Vilette, as it was sent to the Publisher of these Tracts, in two Letters, whereof the one was in Latin, the other in French, to this effect:

Concerning the Efficacy of Monsieur de Vilette his Burning Glass, all what the P. Bert. hath written of it, is true. We have seen the Effects of it repeated over and over again, in the Morning, at Noon, and in the Afternoon, always performing very powerfully; burning or melting any Matter, very few excepted. The Figure of it is round, being thirty Inches, and somewhat better, in Diameter. On one side it hath a Frame of a circle of Steel, to the end that it may keep its just Measure: 'Tis easy to remove it from place to place, though it be above an hundred weight, and it's easily put in all sorts of postures. The burning Point is distant from the Centre of the Glass, about three Feet. The Focus is about half a Louis d'or large. One may pass ones hand through it, if it be done nimbly; for if it stay there the time of a second Minute, there is danger of receiving much hurt.

Greenwood takes fire in it, in an instant, as do also many other Bodies.

A small piece of Pot-Iron was melted, and ready to drop down, in

A Silver Piece of 15 Pence was pierced, in
A gross Nail (called le Clou de paisan) was melted, in
The end of a Sword-blade of Olindo, was burn'd, in
A Brass Counter was pierced, in
A piece of red Copper was melted, ready to drop down, in

A piece
A piece of a Chamber Quarry-stone was vitrified, and put into a Glass-drop, in Steel, whereof Watch-makers make their springs, was found melted, in A Mineral-stone, such as is used in Harquebusses around, was calcin'd and vitrified, in A piece of Mortar was vitrified, in

In short, there is hardly any Body, which is not destroyed by this Fire. If one would melt by it any great quantity of Mettal, that would require much time, the Action of Burning not being perform'd but within the bigness of the Focus, so that ordinarily none but small pieces are expos'd to it. One Monsieur d'Alibert buys it, paying for it Fifteen hundred Livres.

Since this Information, there were, upon occasion given from hence, upon the same subject, further communicated from Paris the following Particulars:

I see by two of the Letters, that you incline to believe, the Glasses of Maginus and Septalium do approach to that of Lyons: but I can assure you, they come very far short of it. You may consult Maginus his Book, where he describes his; and there are some Persons here that have seen one of his best, which had but about twenty Inches diameter; so that this of Lyons must perform at least twice as much. As to Septalium, we expect the Relations of it from Intelligent and Impartial Men. It cannot well be compared to that of Lyons, but in bigness; and in this case, if it have five Palms (as you say) that would be about 3½ feet French, and so it were a Foot bigger, which would make it half as much greater in surface: But as to the Effects, seeing it burns so far off, they cannot be very violent. And I have heard one say, that had seen it, that it did not set Wood on Fire but after the time of saying a Misère. You may judge of the difference of the Effects, since that of Lyons gathers its Beams together within the Space of seven or eight Lines; and
and that of Septimius must scatter them in the compass of three Inches. Some here do intend to make of them, yea and bigger ones; but we must stay till they be done, &c.

Of Monsieur Hevelius’s Promise of imparting to the World his Invention of making Optick Glasses; and of the hopes given by Monsieur Hugens of Zulichem, to perform something of the like nature; as also of the Expectations, conceived of some Ingenious Persons in England, to improve Telescopes.

That eminent Astronomer of Danzig, Monsieur Hevelius, writes to his Correspondent in London, as followeth:

What hath been done in the grinding of Optick glasses in your parts, and how those beginnings, mention’d by you formerly, do continue and succeed, I very much covet to hear. ’Tis now above ten Years, since I myself invented a peculiar way of grinding such Glasses, and reduced it also into practice; by which ’tis easie, without any considerable danger of failing, to make and polish Optick glasses of any Conic Section, and that (which is most notable) in any dish of any Section of a Sphere: which Invention I have as yet discovered to none, my purpose being, for the Improvement of Natural Knowledge, to describe the whole method thereof in my Celestial Machine, and to propose it to the Examination and Judgement of the Royal Society; not doubting at all, but they will finde the way true and practicable, my self having already made several Glasses by it, which many Learned Men have seen and tried.

Monsieur Hugens, inquiring also in a Letter, newly written by him to a Friend of his in England, of the success of the attempts made by an Ingenious English Man for perfecting such Glasses, and urging the prosecution of the same, to
so as to shew by the Effects the practicableness of the Invention, mentions thereupon, That he intends very shortly to try something in that kind, of the success whereof he declares to have good hopes.

Monseur du Son, that excellent Mechanician, doth also at this very present employ himself in London, to bring Telescopes to perfection, by grinding Glasses of a Parabolical Figure, by the means whereof he hopes to enable the Curious to discover more by a Tube of one Foot long, or thereabout, furnished with Glasses thus figured, then can be done by any other Tubes of very many times more that length: The success hereof will (its thought) shortly appear.

An Advertisement of a way of making more lively Counterfaits of Nature in Wax, then are extant in Painting: And of a new kind of Maps in a low Relievo. Both practised in France.

This was communicated by the Ingenious Mr. John Evelyn, to whom it was sent from Paris in a Letter, as followeth: Here is in our Neighbourhood a French-man, who makes more lively Counterfaits of Nature in Wax, then ever I yet saw in Painting, having an extraordinary address in modeling the Figures, and in mixing the Colours and Shadows; making the Eyes so lively, that they kill all things of this Art I ever beheld: He pretends to make a visit into England with some of his Pieces.

I have also seen a new kind of Maps in low Relievo, or Sculpture: For example, the Isle of Antile, upon a square of about eight Foot, made of Boards, with a Frame like a Picture: There is represented the Sea, with Ships and other Vessels Artificially made, with their Canons and Tackle of Wood fixed upon the surface, after a new and most admirable manner. The Rocks about the Island exactly form'd,
as they are upon the Natural Place; and the Island itself, with all its inequalities, and Hills and Dales; the Town, the Fort, the little Houses, Platform, and Canons mounted; and even the Gardens and Platforms of Trees, with their green Leaves standing upright, as if they were growing in their Natural Colours: In fine, Men, Beasts, and whatever you may imagine to have any protuberancy above the level of the Sea. This new, delightful, and most instructive form of Map, or Wooden Country, you are to look upon either Horizontally, or Sidelong, and it affords equally a very pleasant object.

Some Anatomical Observations of Milk found in Veins, instead of Blood; and of Grass, found in the Wind-pipes of some Animals.

A curious Person wrote not long since from Paris, that there they had, in the House of a Physician, newly opened a Man's Vein, wherein they found Milk, instead of Blood. This being imparted to Mr. Boyle at Oxford, his Answer was, That the like Observation about white Blood, had been made by a Learned Physician of his acquaintance, and the thing being by him look'd upon as remarkable, he was desirous to have it very circumstantially from the said Physician himself, before he would say more of it: The next Moneth may bring us in this Account.

The other Particular, mention'd in the Title of this Head, came in a Letter, sent also by Mr. Boyle, in these words:

I shall acquaint you, That two very ingenious Men, Dr. Clark, and Dr. Lower, were pleased to give me an account of a pretty odd kind of Observation: One of them assuring me, That he had several times, in the Lungs of Sheep, found considerable quantity of Grass in the very Branches of the Astern Asterna: And the other relating to me, That a few Weeks since, He, and a couple of Physicians,
tians, were invited to look upon an Ox, that had for two or three days almost continually held his Neck straight up, and was dead of a Disease, the Owner could not conjecture at; whereupon, the parts belonging to the Neck and Throat, being open’d, they found, to their wonder, the Aspera Arteria in its very Trunk all stuffed with Grass, as if it had been thrust there by main force: which gives a just cause of marvelling and inquiring, both how such a quantity of Grass should get in there; and how, being there, such an Animal could live with it so long.

Of a place in England, where, without petrifying Water, Wood is turned into Stone.

The same Searcher of Nature, that was allledged in the immediately precedent Observations, did impart also the following, in another Letter from Oxford, where he faith:

I was a while since visited by a Gentleman, who tells me, That he met with a Place in these parts of England, where, though there be no petrifying Spring (for that I particularly asked) Wood is turned into Stone in the Sandy Earth it self, after a better manner then by any Water I have yet seen: For I had the Curiosity to go to look upon pieces of Wood, he brought thence, and hope for the opportunity of making some tryals to examine the matter a little further, then I have yet been able to do. Thus far that Letter.

Since which time, He was pleased to give this further Information of the same matter, with a Mantissa of some other Particulars, belonging to this Subject, in these Words.

I was lately making some Tryals with the petrified Wood I told you off, which I finde to be a very odde substance, wonderfully hard and fixed. If I had opportunity to Re-print the History of Fluidity and Firmness, I could adde divers things about Stones, that perhaps would not be disliked; and I hope, if God vouchsafe me a little leisure,
to insert several of them in fit places of that History, against the next Edition. Here is a certain Stone, that is thought to be petrified Bone, being shap’d like a Bone, with the Marrow taken out; but with a fit Menumnum, I found that I could easily dissolve it, like other soft Stones: and possi-

bly it may prove as fit as Osteocolla, for the same Medicinal Uses.

Of the nature of a certain Stone, found in the Indies, in the head of a Serpent.

There was, some while ago, sent by Sir Philiberto Vernatti from Java major, where he resides, to Sir Robert Moray, for the Repository of the Royal Society, a certain Stone, affirmed by the Presenter to be found in the Head of a Snake, which laid upon any Wound, made by any venomous Creature, is said to stick to it, and to draw away all Poison: and then, being put in Milk, to void its Poison therein, and to make the Milk turn blaw: in which manner it must be used, till the Wound be cleansed.

The like Relations having been made, by several others, of such a Stone, and some also in this City affirming, to have made the Experiment with success, it was thought worth while, to inquire further into the truth of this Matter: since which time, nothing hath been met with but an Information, delivered by that Ingenious Parisian, Monseur Thevenot, in his second Tome, of the Relations of divers considerable Voyages, whereof he lately presented some Exemplars to his Friends in England. The Book being in French, and not common, ’tis conceived it will not be amiss to insert here the said Information, which is to this effect:

In the East Indies, and in the Kingdom of Quamsiy in China, there is found a Stone in the Head of certain Serpents (which they call by a name signifying Hairy Serpents,) which heals the bitings of the same Serpent, that else would kill in 24 hours. This Stone is round, white in the middle, and about the edges
edges blew or greenish. Being applied to the Wound, it adheres to it of itself, and falls not off, but after it hath sucked the Poyson: Then they wash it in Milk, wherein 'tis left a while, till it return to its natural condition. It is a rare Stone, for if it be put the second time upon the Wound, and stick to it, 'tis a sign it had not sucked all the Venome during its first application; but if it stick not, 'tis a mark that all the Poyson was drawn out at first. So far our French Author: wherein appears no considerable difference from the written Relation before mentioned.

Of the way, used in the Mogol's Dominions, to make Saltpetre.

This is delivered in the same Book of Monsieur Thevenot, and the manner of it having been inquired after, by several curious Persons, to compare it with that which is used in Europe, 'tis presum'd, they will not be displeased to find it inserted here in English, which is as followeth:

Saltpetre is found in many places of the East Indies, but chiefly about Agra, and in the Villages, that heretofore have been numerously inhabited, but are now deserted. They draw it out of three sorts of Earth, black, yellow, and white: the best, is that which is drawn out of the black, for it is free from common Salt. They make two Pits, flat at the bottom, like those wherein common Salt is made; one of them having much more compass then the other, they fill that with Earth, upon which they let run Water, and by the feet of People they tread it, and reduce it to the consistancy of a Pap, and so they let it stand for two days, that the Water may extract all the Salt that is in the Earth: Then they pass this Water into another Pit, in which it christallizes into saltpetre. They let it boil once or twice in a Caldron, according as they will have it whiter and purer. Whilst it is over the Fire, they scum it continually, and fill it out into great Earthen Pots, which hold
hold each 25 or 30 pounds, and these they expose to clear Nights; and if there be any impurity remaining, it will fall to the bottom: Afterwards they break the Pots, and dry the Salt in the Sun. One might make vast quantities of Salt-petre in these parts; but the Country People seeing that we buy of it, and that the English begin to do the same, they now sell us a Maon of 6 pounds for two Rupias and a half, which we had formerly for half that price.

An Account of Hevelius his Prodromus Cometicus, together with some Animadversions made upon it by a French Philosopher.

This excellent Dantiscan Astronomer, Hevelius, in his Prodromus (by him so call'd, because it is as a Harbinger to his Cometography, which hath already so far passed the Press, that of twelve Books there are but three remaining to be Printed) gives an Account of the Observations he hath made of the first of the two late Comets; reserving those he hath made of the second, for that great Treatise, where he also intends to deliver the Matter of this first more particularly and more fully, then he hath done here.

In this Account he represents the Rise, Place, Course, Swiftness, Faces and Train of this Comet, interweaving his Conceptions both about the Region of Comets in general (whether it be the Air, or the Ether?*) and the Causes of their Generation: In the search of which latter, he intimates to have received much assistance from his Telescope.

He observed this Comet nor before Decemb. 22, (though he conceives it might have been seen since Novem. 23. f. n.) & he saw it no longer then Febr. 2: though several others have seen it both sooner, and later: and though himself continued to look out for it til March 7. f. n. but fruitlessly, where-of he thinks the reason to have been its too great distance and tenuity.

He
He finds, its apparent Motion was not made in a just great Circle, but deviating considerably from it; and conceives, that every Comet falls to this deviation, when this apparent Motion grows slow, and the Star becomes Stationary (which, as he faith, it doth in respect of the Ecliptick, not its own orbite.) Here he observes, That from Decemb. 28. to Decemb. 30. Jan. 9. its course was almost in a great Circle: but, that then it began to deflect from that Circle towards the North; so that afterwards, with a very notable and conspicuous Curvity, it directed its course towards Primam Arietis: Of which deflection, he ventures to assign the cause from the Cometical Matter, the various position and distance of the Comet from the Earth and the Sun, the annual Motion of the Earth, and the impressed Motion, and the inclination of the discus of the Cometical Body.

He is pretty positive, that without the annual Motion of the Earth, no rational Account can be given of any Comet, but that all is involved with perplexities, and deform'd by absurdities.

He inquires, since all Comets have their peculiar ingenite Motion, what kinde of Line it is, they describe by that Motion of their own? whether circular, or straight, or curve, or partly straight and partly curve? And if curve, whether regular or irregular? if regular, whether Elliptick, or Parabolar, or Hyperbolical? He answers, That this Motion is Conical; and judgeth, that by the Comet path all the phenomena of Comets can, without any inconveniency, be readily solved; even of that, which (by History) in fifty days, past through more then the 12 Signs of the Zodiac; And of that, which in two days run through eight Signs: and of another, which in 48 days past through all the Signs, contra seriem. Which how it can be explicated upon the supposition of the Earths standing still, and upon the denying of the annual Motion thereof, he understands not at all.
He refers to his Cometography these Disquisitions: whether all Comets (in their innate Motion) move equal Spaces in equal Times? which is the swiftest, and which the slowest Motion they are capable of? what the cause of this acceleration and retardation of their true Motion?

He puts it out of doubt, that they are in the sky itself, producing Reasons for it that are very considerable, and alluding among others, That the Parallaxes doe clearly evince it, which he finds far less in Comets, than in the Moon, yea then sometimes in the Sun itself. Where he also represents, That he hath deduced the Horizontal Parallax of this very Comet from one onely Observation, made Febr: 4., 31. by which he found, That then it was distant from the Earth 5000 Semidiameters of the same, or 4300000 German miles. From this its distant from the earth, he deduces, That on that Day when it was so remote from the Earth, its true Diameter was 2560 German miles; which is three times bigger than the Diameter of the Earth, and almost six times bigger than that of the Moon, whose Diameter, according to his Theory, is 442 German miles.

He finds the Matter of Comets to be in the Ethere itself, making the Ethere and the Air to differ onely in purity, and esteeming, That the Planets do emit their Exhalations, and have their Atmospheres like unto our Earth. Where he affirms, That the Sun alone may cast out so much Matter at any time in one Year, as that thence shall be produced not one or two Comets, equalling the Moon in Diameter, but very many 5 which if so, what contribution may not be expected from the other Planets?

Of this Cometical Matter, he thinks, That first it is by little and little gathered together, then coagulated and condensed, and thereby reduced to a less Diameter; but then, after a while, it resolves again, and grows dilute and pale, and at last is dissipated. And accordingly he affirms, That he hath observed the Head of this Comet at first more confused, thin and pale, afterwards clearer and clearer.
He conceives, That all Comets do respect the Sun as their King and Centre, as Planets do, making them a kinde of Spurious Planets, that emulate the true ones in their Motion almost in all things.

The Train, he makes nothing else but the Beams of the Sun, falling on the head of the Comet; and passing through the same, refracted and reflected. And amongst his Observations and Schemes of this Comet, there occurs one, wherein the Tail is curve, so seen by him Decemb. 11. He assigns the causes why the Trains do so much vary, and shews also, on what depends their length.

Whether the same Comets return again, as the Spots in the Sun? and, Whether in the time of great Conjunctions they are more easily generated? and whether they can be certainly foretold? with several other Inquiries, he refers for to his great Book.

As to Prognostications, he somewhat complains, That Men do more inquire what Comets signify, then what they are, or how they are generated and moved; professing himself to be of the minde of those that would have Comets rather admired then feared; there appearing indeed no cogent reason, why the Author of Nature may not intend them rather as Monitors of his Glory and Greatness, then of his Anger or Displeasure; especially seeing that some very diligent Men (among whom is Gemma Frisius) take notice of as great a number of good as bad Events, consequent to Comets. Seneca also relating, That that Comet which appeared in his Time, was so happy, that it did Cometis defirahere insamiam, it cleared the credit of Comets, and made People have good thoughts of them.

Having given some Account of what may be look'd for in this Prodrorus, it follows, That some also should be rectified of the Animadversions mention'd to have been made upon the same. This was done by that Parisian Philosopher Monsieur Auzout, in a Letter of his to his Country-man Monsieur Petit; in which he strongly conceives, That this

Irodoros
(108)

Prodromus contains some mistakes, of which he chiefly singles out one, as most considerable, in Hevelius's Observation of Febri., and declares thereupon, That he, and several very intelligent Astronomers of France and Italy concurring with him therein, (whereas M. Hevelius to him seems to stand single, as to this particular) found by their Observations, That this Comet could not, on that day of February, be there where M. Hevelius placed it, viz. in Prima Arietis; unless it be said, That it visited that Star of Aries on the 18th, and returned thence the 19th, into its ordinary course; in which, according to his, and his several Correspondents Observations, the Comet on Febr. 17. was distant from that first Star of Aries at least 1 degree and 17 minutes; and on February 19, (he having missed, as well as his other Friends, the Observation on Febr. 18.) was advanced in its way 12 or 13 minutes, but yet distant from the said Star some minutes above a whole degree, and consequently far from having then passed it. After which time M. Anzout affirms to have seen it, as well as several others, for many days, and that until March 2; observing, That about Febr. 26. or 27, when the Comet was nearest to the often-mentioned first of Aries, it approached not nearer thereunto, then at the distance of 50 minutes.

This important Difference between two very Learned, and very deserving Persons, being come to the knowledge of some of the ablest Philosophers and Astronomers of England, hath been by them thought worthy their Examination: and they being at this very present employed in the discussion thereof, by comparing what hath been done and publish'd by the Dissenters, and by confronting with them their own Domestick Observations, are very likely to discern where the mistake lies; and having discern'd it, will certainly be found highly impartial and ingenuous in giving their sense of the same.
Of the Mundus Subterraneus of Athanasius Kircher.

This long expected Subterraneous World, is now come to light; dedicated (at least the Exemplar, that hath been pursued by the Publisher of these Papers, who hears, That other Copies bear Dedication to other Great Princes) both to the present Pope, as being esteemed by the Author to have a part of his Apostolical Kingdom there; and to the Roman Emperor now Regnant, who indeed in his Kingdom of Hungary, and in several Provinces of Germany, hath very many and very considerable things, worthy to be observed, under Ground.

To give the Curious a taste of the Contents of this Volume, and thereby to excite them to a farther search into the recesses of Nature, for the composure of a good Natural History; they may first take notice, That the Author, having given an account in the Preface, what encouragement he received, for writing this Book, from the opportunity of Travelling with the Cardinal of Hassia into Sicily (in which Voyage, he saith, He met with, as it were, an Epitome of what may be observable in the Subterraneous parts of the Earth; and in particular, with an Earth-quake of 14 days duration, very instructive to him concerning several great Secrets of Nature;) having, I say, thus Prefaced, he divideth his Work into 12 Books, wherein he affirms not only to have explicated the Divine Structure of the under-ground World, and the wondrous distribution of the Work-houses of Nature, and her Majesty and Riches therein; but also to have opened the Causes of her Effects and Productions; whence, by the Marriage of Nature and Art, a happy Issue may follow for the use and benefit of Humane Life.

In the first Book, he considers the nature of the Centre of the Earth, where he delivers several Paradoxes touching the same, and Discourses of the Motion of heavy Bodies, of Pendulems, of Projectils.
In the second, he treats of the Fabric of the Terrestrial Globe, of the Influences it receives from the Celestial Bodies, especially the Sun and Moon, of both which Luminaries he gives a Scheme; of the proportion of the Earth to the Sun and Moon; of the external conformation of the Earth, its Mountains, and their concatenations, decrease and increase, together with the strange transformation thereof. Further, of the Waters encompassing the Earth, and their various Communications by hidden Passages; as also of the height of Mountains, and of the depth of Seas; the dimension of the Sicilian Straights; the Magnetical Constitution of the Earth, its Heterogeneous Nature, Interior Frame, Laboratories, Caves, Channels, &c.

In the third: Of the Nature of the Ocean, and the diversity of its Motions; of its general Motion from East to West, Currents, Reciprocations, Gulfs, Whirlpools, Salt-nels, &c.

In the fourth: Of the Nature of the Subterraneous Fire, its necessity, diffusiveness, food, prodigious Effects through ignivomous Mountains; as also of the Nature of Air and Winds, their power and variety; of the general Wind, how and whence generated; of Periodical and Anniversary Winds, and their Causes; as also of the production of Artificial Winds, for refreshment and other advantages. To which he subjoins a Discourse, tending to prove, That all Meteors owe their Nativity to the Fires of the Subterraneous World.

In the fifth: Of the Original of Springs, Rivers, Lakes; various differences and qualities of Waters, and the marks where they are to be met with under Ground; of Waters Medical, hot Baths, and their Differences, Causes, Vertues; together with the wonderful Qualities and Properties of some Springs; as to their Colour, Taste, Smell, Weight, Saliubrity, Flux and reflux, Petrifying power, &c.

In the sixth: Of the Earth itself, and the great variety contained in the Womb thereof; of the manifold Productions
editions made therein, by the virtue of Salt and its Auxiliaries, the differences whereof are largely dicsourfed of, together with the way of extracting the same. In particular of Saltpeter, its Generation, Nature, Vertues; of the way of making Gunpowder, and the various uses thereof; as also the Nature, Qualities, Preparation, Medical and other uses of Alume and Vitriol.

In the seventh: Of some Fossils, as Sand, Gravel, Earths, and their various Differences, Qualities, uses Economical, Chymical, Medical: together with the strange varieties and changes happening in the Earth, and their causes; as also the requisits to Agriculture.

In the eighth: First, of Stones, their Origine, Concretion, difference of Colours; and in particular, of Gems and their variety, causes of generation, transparency in some and colours in others; as also of their various Figures and Pictures, by Nature formed both in common and precious Stones, with their Causes. Secondly, of the Transformation of Juices, Salts, Plants, yea of Beasts and Men turn'd into Stone: together with the generation of Bony Substances under Ground, by many esteemed to be the Bones of Giants; and of Horny Substances, taken for Unicorns horns: as also of Fossil wood and Coals. Thirdly, of Bituminous Flowers, lapis Asbestos, Amber, and its Electrical vertue; together with the way how Insects, little Fishes, and Plants are Intombed therein. Fourthly, of Subterraneous Animals, Moles, Mice, Birds, Dragons; where is also treated, of those Animals that are found in the midst of Stones.

In the ninth: First, of Poysons, their primeval Origine from Minerals, and their accidental Generation in Vegetable and Animal Bodies, together with their differences; where it is dicsourfed, not onely how Poysons may be bred in Men, but also, how the Poysons of some Animals do infect and kill Men; and, where the Venom of Vipers lodges, and how mad Dogs and Tarantula's so communicate their Poyson, as that it exerts not its noxioufnness, till after some
time: Where also occasion is taken to discourse of the Original of Diseases, and cure of Poysonous ones. Secondly, of the wonderful Nature of Sulphur, Antimony, Quick-silver, their origine and qualities; together with the productions of Corals and Pearls.

In the tenth: First of Metallurgy, and the way how that unctuous Body, out of which Mettals are produced, is elaborated by Nature, and what therein are Sulphur, Salt, and Mercury; besides, what it is that renders Mettals fluid in the Fire, but not Stones and Vegetables &c. Secondly, of the Requisits to a perfect knowledge of the Metallick Art, and of the Qualities of the Mine-master; then of the Diseases of Mine-men, and their Cure, and the ways of purging the Mines of the Airs malignity; as also of Metallognomy, or the signs of latent Mettals, and by what Art they may be discovered. Thirdly, several Accounts sent to the Author, upon his Inquiries by the Mine-masters themselves, or other chief Over-seers of the Mine-works, touching the variety, nature and properties of Minerals, and the many Accidents happening in Mines, particularly the Hungarian ones at Schemnitz, and those of Tyrol. Fourthly, of several both Hydraulick and Wind-Engines, to free the Mines from Water and noxious dampes. Fifthly, Of the way of working Mettals, Gold, Silver, Copper, Iron, and particularly of the method used at Potosi in Peru, of extracting the Silver out of the Mineral: to which is added, a Discourse of Salt-pits, and the way of making Salt.

In the eleventh, First, of Alchimy, its Original and Antiquity, the Vessels and Instruments belonging thereunto. Secondly, of the Philosophers Stone, what is meant by it, and whether by means thereof true Gold can be produced? And in general, whether there be any such thing, as a true and real Transmutation of one Metal into another? Where are delivered the several Procedes of the reputed Adepti, Raymund Lulle, Azoth, Arnold de Villanova, Paracelsus, Sen. divogius, &c. but all exploded as false and deceitful. Thirdly, of
of the decisions in Law concerning Chymical Gold, true or false. Fourthly, what the celebrated Philosophers Stone was among the Ancients, and what they understood by the same?

In the twelfth: First, Of the Seminal Principle of all things, its origine, nature and property; of the way how Nature proceeds in the Generation of Minerals, Vegetables, Animals; of spontaneous Generation; of Zeophyts, Insects of all sorts, and particularly of the Worms bred in Men; together with the causes why Nature would produce such swarms of infinite sorts of Insects. Secondly, of the variety and differences of Vegetables; of the requisites to know the verities of Plants, and of the several ways of Engrafting. Thirdly, of the Art of Distilling, whereby Nature is imitated, as doing all her under-ground Works, in the Opinion of this Author, by Distillation. Fourthly, of the Laboratories of various Arts, in which, according to Nature's pattern, used in her Subterraneous Operations, strange things may be performed: where treating of Chymical Secrets, the truth of the Preparation of Aurum potabile is discussed, and the Magisteries of Gold, Silver, Iron, Tin, Copper and Lead, examined; to which is subjoyned an Appendix, furnishing such Rules, whereby Students in Chymistry may be directed in their work, and true Operations distinguished from false ones. Fifthly, of Metallostaticks, whereby the mixture of Metals and Minerals may be certainly known; together with a way of weighing the Proportions of moist and dry, existent in every Compound, as well Vegetable and Animal, as Mineral. Sixthly, of Glass-making, where is treated of the Nature of Glass; of the Artificial Production of all sorts of Precious Stones, partly from the Authors own Experiments, partly from the Communication of his Friends, and the Collection of the best Writers upon that subject. Seventhly, of Fire-works, where the Invention and Preparation of Gun-powder is largely discoursed of, and the ways of making Squibs, Fires burning in Water.
and many others, used in Publick Festivities, are described. Eighthly, of some Mechanical Arts, as that of Gold-smiths, Black-smiths, Copper-smiths, Wyre-drawers, in the last where-of he resolves this Problem; a certain weight of Metal, and the bigness of the hole, through which the Wyre is to be drawn, being given, to finde into what length so much Metal can be spun out.

Thus you have a view of this whole Volume; to which it may perhaps not be amiss to add, for a Conclusion, some of those Particulars which are esteemed by the Author to out-shine the rest, and are here and there inter-woven as such. For example, in the First Part.

The use of Pindules, for knowing by their means the state of ones Health, from the different beatings of the Pulse, Pag. 51.

The Chain of Mountains, so drawn over the Earth, that they make, as it were, an Axis, passing from Pole to Pole; and several transverse ductus, so cutting that Axis, as to make, in a manner, an Equator and Tropicus of Mountains: by which concatenation he imagines, That the several parts of the Earth are bound together for more firmness, pag. 69.

A Relation of a strange Diver, by his continual converse in Water, so degenerated from himself, That he was grown more like an Amphibium, then a Man, who, by the command of a Sicilian King, went down to the bottom of Charybdis, and brought a remarkable account of the condition of that place, pag. 98.

A Description of the Origine of the Nile, as this Author found it in a certain MS of one of his own Society, called Peter Pais, whom he affirmes to have been an Eye-witness, and to have visited the Head of the Emperor of Ethiopia himself Anno 1618. which Manuscript, he faith, was brought to Rome, out of Africa, by their Procurator of India and Ethiopia, pag. 72.
The Communication of the seas with one another by Subterraneous Passages, viz., of the Caspian, with Pont Euxin and the Persian Gulf; of the Mare Mortuum, with the Mare Rubrum, and of this latter with the Mediterranean; as also of Scylla with Charybdis, pag. 85. 101.

The Subterraneous Store-houses (in all the four parts of the Earth) of Water, and Fire, and Air; together with their important Uses, pag. 111.

An Account of the state of the Earth about the Poles; how the Waters are continually swallowed up by the Northern, and running along through the Bowels of the Earth, do regurgitate at the Southern Pole, pag. 159.

A Description of Mount Vesuvius and Ætna, both visited by the Author himself, Anno 1638. their Dimensions, Communication, Incendiums, Paths of Fiery Torrents cast out by them, &c. as also of the Vulcans in Iceland and Groenland, and their Correspondence and Effects, p. 180.

An Account of that famous and strange Whirl-pool upon the Coast of Norway, commonly call'd, The Maelstrom; which this Author fancies to have a Communication, by a Subterraneous Channel, with another such Whirl-pool in the Boddick Bay; by which commerce, according to him, the Waters, when, upon their accumulation and crowding together in one of these places, they are swallowed up by the Gulf there, carrying along with them whatsoever is in the way, and lodging it in a certain receptacle at the bottom thereof, are conveyed through the said under-ground Channel to the other Gulf; where again, upon the like conflux and returnescence of Waters, they are absorbed, and through the same Channel do reciprocally run to the former Gulf, and meeting in their impetuous Passage with the things formerly sunk down into the Repository, carry them aloft; with themselves, and cast them up again on the Coast of Norway, p. 146.

A Relation of strange Earth-Quakes, p. 220.
An Enumeration of all the celebrated Medical Water and hot Baths, in all the parts of the World, p. 263. et seq.

In the Second Part, some of his special Observations, are,
How Stones are coloured and figured under ground, p. 13.
Natures skill in Painting of Stones, p. 22.
A whole Natural Alphabet represented upon Stones; and all sorts of Geometrical Figures, naturally Imprinted upon them, p. 23.
The cause of the variety of Colours in Prismes, and the Authors severe Judgement concerning those, that hold them to be meerly Phantastical, pag. 15, 16, 17. Where he also delivers an Experiment, by him counted wonderful, exhibiting all sorts of Colours by the means of Mercury, coagulated by the vapour of Lead, and put in a Brass Spoon upon burning Coals.
The cause of the curious Colours in Birds, p. 17.
The way of Nature in the Generation of Diamonds, p. 21.
A way of preparing such a Liquor, that shall sink into, and colour the whole Body of Marble, so that a Picture made on the surface thereof, shall, the stone being cut through, appear also in the inmost parts of the same, p. 43.
A Story of a whole Village in Africa turned into Stone, with all the People thereof, p. 50.
An Experiment, representing the Generation of the Stone in the Bladder, p. 52.
An Asbestos Paper, that shall last perpetually, p. 74.
Several Relations of numerous Societies of People living under ground, and their Economy; whereof a strange one is alleged to have been found in England, attested by an English Author, p. 97, 98, 99.
A Relation of a Man, that bred a Serpent in his Stomach, which came from him of the length of one Foot and a half, affirmed by the Author to have been seen by himself, p. 126.
Of whole Forests of Coral at the bottom of the Red Sea, pag. 159.
The vanity of the Virga Divinatoria, p. 181.

A peculiar way of washing out very small Dust-gold, p. 198.

Of some extraordinary big pieces of perfect Natural Gold and Silver, p. 203.

Of a very rare Mineral, sent to the Author out of the Hungarian Mines, which had pure Silver branching out into Filaments, and some splendid yellow parts, which was pure Gold, and some dark parts, which was Silver mixed with Gold, p. 189.

Salt, the Basis of all Natural Productions, and the admirable variety of Salts, p. 299.

Strange Figures of Plants, p. 348.

The way of reproducing Plants, p. 414.

In how much time a Swallow can fly about the World, p. 418. &c.

This may suffice, to give occasion to the Searchers of Nature, to examine this Book, and the Observations and Experiments contained therein, together with the Ratiocinations raised thereupon, and to make severer and more minute Inquiries and Disquisitions of all.

A farther Account of an Observation above-mentioned, about white Blood.

Since the Printing of the former Sheet, there is this farther account from the same hand. Mr. Boyle,

I have at length, according to your desire, receiv'd from the Ingenious Dr. Lower, an account in Writing of the Observation about Chyle found in the Blood; which though you may think strange, agrees well with some Experiments of his and mine, not now to be mention'd. The Relation, though short, comprizing the main Particulars of what he had more fully told me in Discourse, I shall give it you with little or no variation from his own Words.

A Maid,
A Maid, after eating a good Breakfast, about seven in the Morning, was let Blood about eleven the same day in her Foot; the first Blood was receiv'd in a Porringer, and within a little while it turn'd very white; the last Blood was received in a Sawcer, which turned white immediately, like the white of a Custard. Within five or six hours after, he (the Physitian) chanced to see both, and that in the Porringer was half Blood and half Chyle, swimming upon it like a Serum as white as Milk, and that in the Sawcer all Chyle, without the least appearance of a drop of Blood; and when he heated them distinctly over a gentle Fire, they both harden'd: As the white of an Egge when 'tis heated, or just as the Serum of Blood doth with heating, but far more white. This Maid was then in good health, and onely let Blood because she never had her Courses, yet of a very florid clear Complexion.

Note.

The Reader of these Papers is desired, that in those of Numb. 4. pag. 60. lin. 10. he would please to read eight, instead of hundred; this latter word having been put in by a great over-sight, and, without this Corre&tion, injuring that Author, whose Considerations are there related. This Advertisement should have been given in Numb. 5. but was omitted for haste.


Oxford, Printed by Leonard Lichfield, for Richard Davis. 1665.
The Contents.

Monsieur de Sons progress in working Parabolar Glasses. Some speculations of Monsieur Auzout concerning the changes, likely to be discovered in the Moon. The instance of the same Person to Mr. Hook, for communicating his contrivance of making with Glasses of a few feet diameter, telescopes drawing several hundred feet; together with his offer of recompening that secret with another, which teacher, How to measure with a telescope the distances of objects upon the Earth. The experiment of Kircher, of preparing a liquor, that shall sink into, and colour the whole body of marble, delivered at length. An intimation of a way found in Europe, to make good China-Dishes. An account of an odd spring in Westphalia, together with an information touching salt-springs; and a way of straining salt-water. Of the rise and attempts of a way to convey liquors immediately into the mass of blood.

Of Monsieur de Sons Progress in working Parabolar Glasses.

Since what was mentioned in the immediately precedent tract, touching Monsieur de Son’s noble attempt of grinding glasses of a parabolical figure, the publisher of these papers had himself seen two eye-glasses of that shape, about one inch and a half deep, and one inch and a quarter broad, wrought by this eminent artist with a rare steel-instrument of his own contrivance and workmanship, and by himself also polished to admiration. And certainly it will be wondred at by those, who...
who shall see these Glasses, how they could be truly wrought to such a Figure, with such a Cavity; & yet more, when they shall hear the Author undertake to excavate other such Eye-Glasses to above two inches, and Object-glasses of five inches Diameter. He hath likewise already begun his Object-glasses for the mentioned two Ocular ones, of the same Figure of about two inches Diameter, which are to be left all open, yet without causing any colours. Of all which 'tis hoped, that shortly a fuller and more particular account will be given.

Monsieur Auzout's Speculations of the Changes, likely to be discovered in the Earth and Moon, by their respective Inhabitants.

This Inquisitive Philosopher in a letter of his lately written to his correspondent in London, takes occasion to discourse of his considerations concerning those Changes, mentioned in the Title, as follows:

I have (said he) sometimes thought upon the Changes, which 'tis likely, the supposed Inhabitants of the Moon might discover in our Earth, to see, whither reciprocally I could observe any such in the Moon. For example, me-thinks, that the Earth would to the people of the Moon appear to have a different face in the several seasons of the year; and to have another appearance in Winter, when there is almost nothing green in a very great part of the Earth; when there are Countries all covered with snow, others, all covered with water, others, all obscured with Clouds, and that for many weeks together: Another in Spring, when the Forrests and Fields are green. Another in Summer, when whole Fields are yellow &c. Me-thinks, I say, that these Changes are considerable enough in the force of the reflexions of Light to be observed, since we see so many differences of Lights in the Moon. We have Rivers considerable enough to be seen, and they enter far enough into
into the Land, and have a breed capable to be observed. There are luxes in certain places, that reach into large Countries, enough to make there some apparent change; & in some of our Seas there float sometimes such bulky masses of ice, that are far greater, than the Objects, which we are assured, we can see in the Moon. Again, we can down whole Forests, and drain Marshes, of an extent large enough to cause a notable alteration: And men have made such work, as have produced Changes great enough to be perceived. In many places also are Vulcans, that seem big enough to be distinguished, especially in the shadow: And when Fire lights upon Forests of great extent, or upon Towns, it can hardly be doubted, but these Luminous Objects would appear either in an Eclipse of the Earth, or when such parts of the Earth are not illuminated by the Sun. But yet, I know no man, who hath observed such things in the Moon; and one may be rationally assured that no Vulcans are there, or that none of them burn at this time. This it is (so be goes on) which all Curious men, that have good Telescopes, ought well to attend; and I doubt not; but, if we had a very particular Map of the Moon, as I had designed to make one with a Topography, as it were, of all the considerable places therein, that We or our Posterity would find some changes in it. And if the Maps of the Moon of Hevelius, Drumi, and Riccioli, are exact, I can say, that I have seen there some places considerable enough, where they put parts that are clear, whereas I there see dark ones. 'Tis true that if there be Seas in the Moon, it can hardly fall out otherwise, than it doth upon our Earth, where Alluvium's are made in some places, and the Sea gains upon the Land in others. I say, if those Spots we see in the Moon, are Seas, as most believe them to be; whereas I have many reasons, that make me doubt, whether they be so; of which I shall speak elsewhere. And I have sometimes thought, whether it might not be, that all the Seas of the Moon, if there must be Seas, were on the side of the other Hemisphere, and that for this cause it might be that the Moon turns not upon its Axis, as our Earth.
wherein the Lands and Seas are, as it were, balanced: That thence also may proceed the non-appearance of any Clouds raised there, or of any Vapors considerable enough to be seen, as there are raised upon this Earth; and that this absence of Vapors is perhaps the cause, that no Crepuscle is there, as it seems there is none, my selfe at least not having hitherto been able to discerne any mark thereof: For, me thinks, it is not to be doubted, but that the reputed Citizens of the Moon might see our Crepuscle, since we see, that the same is without comparison stronger, than the Light afforded us by the Moon, even when she is full; for, a little after Sun-set, when we receive no more the first Light of the Sun, the sky is far clearer, than it is in the fairest night of the full Moon. Mean while, since we see in the Moon, when she is increasing or decreasing, the Light, the receives from the Earth, we cannot doubt, but that the People of the Moon should likewise see in the Earth that Light, where-with the Moon illuminates it, with perhaps the difference, there is betwixt their bigness. Much rather therefore should they see the Light of the Crepuscle, being, as we have said, incomparably greater. In the mean time we see not any faint Light beyond the Section of the Light, which is every where almost equally strong, and we there distinguish nothing at all, not so much that clearest part, which is called Aristarchus, or Porphyrites, as I have often tried; although one may there see the Light, which the Earth sends thither, which is sometimes so strong, that in the Moon's decrease I have often distinctly seen all the parts of the Moon, that were not enlightened by the Sun, together with the difference of the clear parts, and the Spots, so far as to be able to discern them all. The Shaddows also of all the Cavities of the Moon seem to be stronger, than they would be, if there were a Second Light. For, although a far off, the Shaddows of our Bodies, environed with Light, seem to us almost dark; yet they doe not so appear so much, as the Shaddows of the Moon doe; and those that are upon the Edge of the Section, should
should not appear in the like manner. But, I will determine
nothing of any of these things. When I shall hereafter have
made more frequent Observations of the Moon with my
great Telescopes, in convenient time, I shall then perhaps learn
more of it, than I know at present; at least it will excite the
Curious to endeavor to make the like Observations; and it
may be, others, that I have not thought of.

The Instance of the same Person to Mr. Hook, for
communicating his Contrivance of making, with a
Glass of a Sphere of 20 or 40 foot diameter, a Tele-
scope, drawing several hundred foot; and his of-
fer of recompening that Secret with another, teaching To measure with a Telescope the Distances
of Objects upon the Earth.

In Numb. 4. Of these Papers, pag. 67. Mr. Hook had inti-
mated, that he would shortly discover a way of his, with a
Plano-convex Glasse of a Sphere of 20. or 40. feet Diameter,
without Veines, and truly wrought of that Figure, to make
a Telescope, that with a single Eye-glasse should draw 300,
400, yea 1000 feet, without at all altering the Convexity:
Monseur Auzout returns this consideration, and offer upon
it, which follows:

To perform (saith he) with a lesser Object-glasse the effect of
a great Telescope, we must find out a way to make
such an Object-glasse to receive as many Rayes as one will,
without their being sensibly distant from one another; to
the end, that by applying to it a stronger Eye-glasse, there
may be still Beams enough to see the Object, and to obli-
mate the small specks and imperfections of the Eye-glasse.
And if Mr. Hook hath this Invention, I esteem it one of the
greatest, that can be found in the matter of Telescopes. If
he please to impart it to us, we shall be obliged to him; and
I wish,
I wish, I had a secret in Opticks to encourage him to that communication. If I did believe, that this would be esteemed one, To measure with a great Telescope the distance of Objects upon the Earth; which I have found long since, and proposed to some by way of Paradox; Locorum distantias ex unica statione, absque ullo Instrumento Mathematico, metiri; I do here promise to discover it to him, with the necessary Tables, as soon as He shall have imparted his to me: which I will use, as he shall order me. For, although the Practise does not altogether answer the Theory of my Invention, because that the length of the Telescopes admits of some Latitude; yet one comes near enough, and perhaps as Just, as by most of the ways, ordinarily used with Instruments. That, which I am proposing, I doubt not but M. Hook will soon understand, and see the determination of all Cases possible. I shall only say, that if we look upon the sole theory, we may make use of an ordinary telescope, whereof the Eye-glasse is to be Convexe: for, by putting the Glasses at a little greater distance, than they are, proportionably to the distance for which it is to serve, and by adding to it a new Eye-glasse, the Object will be seen distinct, though obscure; and if the Eye-glasse be Convexe, the Object will appear erect. They may be done two manner of ways; either by leaving the telescope in its ordinary situation, the Object-glasse before the Eye-glasse; or by inverting it, and putting this before that. But if any will make use of two Object-glassses, whereof the Focuses are known, the distance of them will be known. If it be supposed, that the Focus of the first be B, and that of the second C. and the distance given, B + 2D, and that D minus C be equal to F; for this distance will be equal to B + C + F - r F - C². And if you have the Focus of the first Object-glasse, equal to B the distance, where you will put the second Glass equal to B + C + D. the Focus of the 2d Glass will be found equal to \( \frac{CD}{GD} \). And if you will that the Object shall be magnified as much with these two Glasses, as it would be with a single one, whereof the Focus should
should be of the distance given, having the Focus of the Object-glass given equal to B, and the distance given to \( B + D \); the distance between the first and the second Glass will be equal to \( \frac{B + D}{B} \); whence subducting B (the Focus of the Object-glass given) there remains \( \frac{BD}{B+D} \); and if this sum be supposed equal to C, we shall easily know, by the precedent Rule, the Focus of the second Glass.

So far M. Auzout, who, I trust, will receive due satisfaction to his desire, as soon as the happy end of the present Contagion shall give a beginning and life again to the Studies and Actions of our retired Philosophers.

I shall only here add, That the Secret he mentions ['Of measuring the distance of Places by a Telescope (fitted for that purpose) and from one station'] is a thing already known (if I am not misinformed) to some Members of our Society; who have been a good while since considering of it, and have contrived ways for the doing of it: Whether the same with those of Mr. Auzout, I know not. Nor have I (at the distance that I am now from them) opportunity of particular Information.

**An Experiment of a way of preparing a Liquor, that shall sink into, and colour the whole Body of Marble, causing a Picture, drawn on a surface, to appear also in the inmost parts of the Stone.**

This Experiment, having been hinted at in the next foregoing Papers, out of the *Mundus Subterraneus* of Athanassius Kircher, and several Curious Persons, who either have not the leisure to read Voluminous Authors, or are not readily skilled in that Learned Tongue wherein the said Book is written, being very desirous to have it transferred hither, it was thought fit to comply with their desire herein.

The Author therefore of the *Mundus*, &c., having seen some
some stones reputed to be natural that had most lively pictures, not only upon them, but passing thorow their whole substance, and thereupon finding an artist, skilful to perform such rare workmanship, did not only pronounce such stones to be artificial, but when that artist was unwilling to communicate unto him his secret, did join his study and endeavors with those of one Albertus Gunter a Saxon, to find it out themselves: wherein having succeeded, it seems, they made the Experiments which this industrious and communicative Jesuit delivers in this manner:

The Colours, faith he, are thus prepared; I take of Aqua fortis and Aqua Regis, two ounces and a half of Sal Armoniack, one ounce of the best spirit of Wine, two drachms; as much gold as can be had for nine Julio's (a julio being about six pence English) of pure silver, two drachmes. These things being provided, let the silver, when calcined, be put into a vial; and having powdered upon it the two drachmes of Aqua fortis, let it evaporate, and you shall have a water yielding first a blew colour, and afterwards a black. Likewise put the gold, when calcin'd, into a vial, and having powdered the Aqua Regis upon it, let it by to evaporate: then put the spirit of Wine upon the Sal Armoniack, leaving it also till it be evaporated; and you will have a golden coloured water, which will afford you divers colours. And, after this manner, you may extract many pictures of colours out of other mettals. This done, you may, by the means of these two waters, paint what picture you please upon white Marble, of the softer kind, renewing the figure every day for several days with some fresh superadded liquor, and you shall find in time, that the picture hath penetrated the whole solidity of the stone, so that cutting it into as many parts as you will, it will always represent unto you the same figure on both sides.

So far, which how far it answers expectation, is referred to the trial of ingenious artists. In the mean time there are not wanting experienced men that scruple the effect, but they
yet are far from pronouncing any thing positively against it, so that they do not discourage any that have conveniencies, from trying.

But whether the way there mentioned will succeed or not, according to expectation: Sure it is that a Stone-cutter in Oxford, Mr. Bird, hath many years since found out a way of doing the same thing, in effect, that is here mentioned; and hath practised it for many years. That is, he is able to apply a colour to the outside of polished Marble, as that it shall sink a considerable depth into the body of the stone, and there represent like figures or images as those are on the outside; (deeper or shallower according as he continues the application, a longer, or shorter while.) Of which kind there be divers pieces to be seen in Oxford, London, and elsewhere. And some of them being shewed to his Majesty, soon after his happy restauration, they were broken in his presence, and found to answer expectation. And others may be daily seen, by any who is curious, or desirous to see it.

An Intimation of a Way, found in Europe to make
China-dishes.

Notice was lately given by an inquisitive Parisian to a friend of his in London, that by an Acquaintance he had been informed, that Signor Septalio, a Canon in Milan, had the Secret of making as good Porcelain as is made in China itself, and transparent; adding that he had seen him make some. This as it deserves, so it will be further inquired after, if God permit.

An Account of an odd Spring in Westphalia, together
with an Information touching Salt-Springs and the straining of salt-water.

An observing Gentleman did lately write out of Germany, that in Westphalia, in the Diocess of Paderborn, is a Spring, which looses itself twice in 24 hours; coming always, after 6 hours, back again with a great noise and so forcibly, as
to drive 3 Mills not far from its source. The Inhabitant
call it the Bolderborn, as if you should say, the Boysterous
Spring.

The same Person, having mentioned the many Salt-Springs
in Germany, as those at Luneburg, at Hall in Saxony, at
Saltzwedel in Brandenburger Mark, in Tyrol, &c. observes,
that no Salt-water, which contains any Metal with it, can
well be sodden to Salt in a Vessel of the same Metal, which
it itself contains, except Vitriol in Copper Vessels.

He adds, that, to separate Salt from Salt-water, without
Fire, if you take a Vessel of Wax, hollow within, and every
where tight; and plunge it into the Sea, or into other Salt-
water, there will be made such a separation, that the ves-
sel shall be full of sweet water, the Salt staying behind: but,
though this water have no saltish taste, yet, be faith, there
will be found a Salt in the Essay, which is the Spirit of Salt,
subtile enough with the water to penetrate the Wax.

An Account of the Rise and Attempts, of a Way to
convey Liquors immediately into the Mass of
Blood.

Whereas there have lately appeared in publick some
Books, printed beyond the Seas, treating of the Way
of Injecting Liquors into Veins; in which Books the Original
of that Invention seems to be ascribed to others; besides
him, to whom it really belongs; It will surely not be thought
amiss, if something be said, whereby the true Inventor's right
may beyond exception be ascertained & preserved; To which
end, there will need no more, than barely to represent the
Time when, and the Place where, &c among whom it was first
started and put to tryal. To joyn all these circumstances
together, 'Tis notorious, that at least six years since (a good
while before it was heard off, that any one did pretend to
have so much as thought of it) the Learned and Ingenious
Dr. Christopher Wren did propose in the University of Oxford
(where he now is the Worthy Savilian Professor of Astronomy,
and where very many Curious Persons are ready to at
tell this relation) to that Noble Benefactor to Experimental Philosophy, Mr. Robert Boyle, Dr. Wilkins, and other deserving Persons. That he thought, he could easily contrive a Way to convey any liquid thing immediately into the Mass of Blood: *vide:* By making Ligatures on the Veines, and then opening them on the side of the Ligature towards the Heart, and by putting into them slender Syringes or Quills, fastened to Bladders (in the manner of Cystern-pipes) containing the matter to be injected; performing that Operation upon pretty big and lean doggs, that the Vessels might be large enough and easily accessible.

This Proposition being made, Mr. Boyle soon gave order for an Apparatus, to put it to Experiment; wherein at several times, upon several Doggs, Opium & the Infusion of Crocus Metallicum were injected into that part of the hind-legs of those Animals, whence the larger Vessels, that carry the Blood, are most easy to be taken hold of: whereof the success was, that the opium, being soon circulated into the Brain, did within a short time stupify, though not kill the Dog; but a large Dose of the Crocus Metallicum, made another Dog vomit up Life and all: All which is more amply and circumstantially delivered by Mr. Boyle in his Excellent Book of the Usefulness of Experimental Philosophy, Part. 2. Essay 2. pag. 53. 54. 55. Where 'tis also mention'd, that the same of this Invention and of the succeeding Tryals being spread, and particularly coming to the knowledge of a foreign Ambassador, that was Curious, and then resided in London, it was by him tryed with some Crocus Metallicum, upon a Malefactor, that was an inferior Servant of his; with this success, that the Fellow, as soon as ever the Injestion began to be made, did, either really or craftily, fall into a swoon; whereby, being unwilling to prosecute so hazardous an Experiment, they desisted, without seeing any other effect of it, save that it was told the Ambassador, that it wrought once downwards with him. Since which time, it hath been frequently practised both in Oxford & London as well before the Royal Society, as elsewhere. And particularly that Learned Physitian,
Phyitian, Dr. Timothy Clerk, hath made it part of his business, to pursue those Experiments with much industry, great accurateness, and considerable observations thereon, which above two years since, were by him produced and read before the Royal Society, who thereupon desired him, as one of their Members, to compleat, what he had proposed to himself upon that subject, and then to publish the same: the Effect whereof 'tis hoped, will now shortly appear, and not prove unwelcome to the Curious.

Some whereof, though they may conceive, that liquors thus injected into veins without preparation and digestion, will make odd, commotions in the Blood, disturb Nature, and cause strange Symptoms in the Body, yet they have other thoughts of Liquors, that are prepared of such things, as have passed the Digestion of the Stomach; for example, of Spirit of Urine, of Harts-borne, of Blood &c. And they hope likewise, that besides the Medical Uses, that may be made of this Invention, it may also serve for Anatomical purposes, by filling, after this way, the vessels of an Animal as full, as they can hold, and by exceedingly distending them, discover New Vessels, &c: But not now to enlarge upon the Uses, the Reader may securely take this Narrative, as the naked real Matter of Fact, whereby 'tis as clear, as Noon-day (both from the Time, and irrefragable Testimony of very many considerable Persons in that University, who can jointly attest it; as well as from that particular unquestionable one of Mr. Boyle and his worthy Company, who were the first Eye-witnesses of the Tryals made, that to Oxford, and in it, to Dr. Christopher Wren, this Invention is due; and consequently, that all others, who discourse or write of it, doe either derive it from Him, or are fallen upon the same Device several years after Him:

Published with License.

The Contents.

An Account of the Tryalls, made in Italy of Campani's new Optick Glasses. A further Relation of the Whale-fishing about the Bermudas, and upon the Coast of New England, and New Netherland. Of a remarkable Spring of Paderborn in Germany. Of some other uncommon Springs at Basel and in Alsatia. Of the richest Salt-springs in Germany. Some Observations of Strange Swarms of Insects, and the mischiefs done by them: as also of the Brooding of Snakes and Vipers. Observations of odd Constitutions of humane Bodies. Of a way, used in Italy, of preserving Ice and Snow by Chaffe. Directions for Sea-men bound for far Voyages, drawn up by Master Rook, late Geometry Professor of Gresham College. Some Observations of Jupiter's Eclipsed by one of his Satellites: and of his Conversion about his Axis. Of some Philosophical and Curious Books, that are shortly to come abroad.

An Account of the Tryalls, made in Italy of Campani's new Optick Glasses.

An Inquisitive Parfian writes to his Correspondent in London, as follows:

We received lately news from Rome, from a very Curious Person of our acquaintances, importing, that Campani hath had the advantage of Divini. The Great Duke of Toscany, and Prince Leopold, his Brother, upon Tryal, made of both their Glasses, have found those of Campani excel the other, and with them they have been able, easily to distinguish people at
at 4 Leagues distance: Of which I intend you more particulars hereafter.

Among them are expected the Length of these Telescopes, and the Largeness of the Aperture of their Object-glasses. In the mean time, the Parabolical-glasses, formerly mentioned to be in hand here at London, are finishing with all possible care and industry.

A Further Relation of the Whale-fishing about the Bermudas, and on the Coast of New-England and New-Netherland.

The same Person, that communicated the particulars about the new Whale-fishing near the Bermudas, mentioned in the first of these Tracts, gives this further Information; That there have been since taken by order of the Bermudas Company, sixteen of those Whales, the Oyle whereof, to the quantity of 50 or 60 Tuns arrived in Ireland at Limrick, some few months agoe.

He adds, that about two years since, there stranded upon the Coast of New-England a dead Whale, of that sort, which they call Trumpo, having Teeth resembling those of a Mill, and its mouth at a good distance from, and under the Nose or Trunk, and several boxes or partitions in the Nose, like those of the Tailes in Lobsters; and that that being open’d there run out of it a thin oily substance, which would candy in time; after which, the remainder, being a thick fatty substance, was taken out of the same part, with a scoope. And this substance he affirmed to be the Sperma Ceti; adding further, that the Blubber, as they call it, it self, of the same sort of Whales, when stewed, yields on the top a creamy substance, which taken off, and thrown upon white lime, lets fall a dirty heterogeneous sediment, but what remains aloft, affords a Sperma-Ceti-like matter.
He concluded his relation with observing, that these whales were to be met with, between the Coast of New-England, and New-Netherland, where they might be caught eight or nine months in the year, whereas those about the Bermudas are to be found there only in the Months of February, March and April.

Concerning the death of the Whale, which hath been related to have stranded upon New-England, it is not very improbable, but, (that Fish having also more than one Enemy, whereof a small Fish called the Tresker, is one, who, by Mr. Terry's Relation in his East-Indian Voyage, with his nimbleness vexes him as much, as a Bee does a great Beast on the land; and a certain Hornly Fish another, who runs its horn into the Whale's belly,) it may have been kill'd by the latter of these two; which kind of Fish is known, sometimes to run its horn into Ships (perhaps taking them for Whales) and there snapping it afunder; as hapned not long since to an English Vessel in the West-Indian Seas; the broken piece of that Horn being by the Master of that ship presented to the King, and now kept in His Majesties Repository; the like wherof befel a French Vessel, falling towards the East-Indies, according to the Relation, made by Monsieur Thevenot in his Second Tome of Curious Voyages.

Of a remarkable Spring, about Paderborn in Germany.

An inquiring Gentleman of those parts writes to his Friend in London, as follows;

In this Diocess of Paderborn, about 2 leagues from that Town, is a treble Spring call'd Methorn, which has three streams, two whereof are not above one foot and and a half distant from one another, and yet of so differing qualities; that whereas one of them is limpid, blewish, lukewarm, bubling, and holding Sal-ammoniac, Ochra, Iron, Vitriol, Allum,
Allum, Sulphur, Niter, Orpiment, used against Epilepsies, bad Spleens, and the Wormes; the other is Ice-cold, turbid and which, much stronger in taste, and heavier than the former, holding much Orpiment, Salt, Iron, Niter, and some Sal-Armoniack, Allum and Vitriol; Of this all Birds, observed to drink of it, doe dye; which I have also privately experimented by taking some of it home, and giving it to Hens, after I had given them Oates, Barley and Bread-crumes: For, soon after they had drunk of it, they became giddy, reeled, and tumbled upon their backs, with convulsion-fits, and so dyed, with a great extention of their legs. Giving them common-salt immediately after they had drunk, they dyed not so soon; giving them vinegar, they dyed not at all, but seven or eight days after were troubled with the Pitt. Those that dyed, being open'd, their Lungs were found quite shrivelled together. Yet some men, that are troubled with Worms, taking a little quantity of it, and diluting it in common water, have been observed by this means to kill the Worms in their bellies; so that a great number of worms come from them; whereupon though they are sick, yet they dye not. As to the third stream, that lyes lower than the other two, about 20 paces distant from them, it is of a greenish colour, very clear, and of a sweeter taste, pleasing enough. It hath about a middle weight between the other two; whence we guess, that it is mixed of them both, meeting there together: to confirm which, we have mixed equal quantities of those two, with an addition of a little common well-water, and have found that they, being stirred together and permitted to settle, made just a water of the same colour and taste of this third stream.

Of some other not-common Springs at Basel and in Aetia.

A Curious Person writes from those Places in manner following:

At
At Basel the Spring, running in the Gerbergasse (or Tanner's-street) from St. Leonard's Hill, is of a Blewhish colour, and somewhat troubled, holding Copper, Bitumen, and Antimony, about 3 parts of the first, one of the second, and two of the last, as has been examined by skilful Persons. Our Tanners do water their Skins in it; and being a well-taunted and wholesome Water, it is both much drunk, and used to Bath in. It mingles with another Spring water, call'd the Birseck, and with it, between the Salt tower and the Rhine-gate, runs into the Rhine.

In the same Town (which abounds with Spring-waters) there are two, among the rest, call'd Bandolph's well, and Brun Zum Brunnen, that are more observable than the other; the former of them having a Camphor and drying Quality, and used against Hydropical Distempers; the latter containing some Sulphur, Saltpeter and Gold, and being an excellent Water to drink; much used in the principal Tavern of the City, where the Chief of the Town do resort, and near which it runs.

In Alsatia in the Valley, called Leberthal, near Giesbach (an ancient Mine-work) there runs out of a Cavern a foul, fattish, oily Liquor, which, though the Country-men of that place employ to the vile use of greasing their Wheels, instead of ordinary Wheel-grease; yet doth it afford an excellent Balsom, by taking a quantity of it, and putting it in an Earthen Pot well luted, that no steam may exhale; and then with a gentle Fire at first, but a stronger afterwards, boyl it for three hours together; in which space it will boil in a fourth part, and an Earthen Matter, like Pitch, will settle it self at the bottom: but on the top thereof, when cold, there will swim a fatty Substance, like Lyne-Oyl, limped and somewhat yellowish, which is to be decanted from the thick Sediment, and then gently distilled in an Alembick in an Arena; by which means, there will come over two differing Liquors, one Phlegmatick, the other Oily, which
which latter swimming on the Phlegm, is to be severed from it. The Phlegm is used as an excellent Resister and Curer of all the Putrefactions of the Lungs and Liver, and it heals all foul Wounds and Ulcers. The Oily part, being diluted with double its quantity of distilled Vineger, and brought three times over the Helm, yields a rare Balsom, against all inward and outward Corruptions, stinking Ulcers, hereditary Scurfs and Scabs; 'Tis also much used against Apoplexies, Palfies, Consumptions, Giddineses, and Head-aches. Inwardly they take it with Succory-water against all corruptions of the Lungs. It is a kind of Petroleum, and contains no other Mineral Juice, but that of Sulphur, which seems to be thus distilled by Nature under ground; the distillation of an Oyl out of Sulphur by Art, being not so easie to perform.

Of the richest Salt-Springs in Germany.

An Account having been desired of those two chief Salt-Springs in Germany, at Hall and Lunenburg, it was lately transmitted thus:

The Salt-Springs at Hall in Saxony are four, called Gutiaar, the Dutch-Spring, the Mettritz, and the Hackeldorn; whereof the three first hold near the same proportion of Salt; the last holds less, but yields the purest Salt. The three first hold about seven parts of Salt, three of Marcasit, and fourteen of Water: They are, besides their Oeconomical use, employed Medicinally to Bath in, and to draw a Spirit out of it, exhibited with good success against Venom, and the putrefaction of the Lungs, Liver, Reins, and the Spleen.

The Salt-Water at Lunenburg, being more greenish then white, and not very transparent, is about the same nature and hold with that of Hall. It hath a mixture of Lead with it, whence also it will not be sod in Leaden Pans; and if it held no Lead at all, it would not be so good, that Mettal being judged to putrefie the Water: whence also the Salt of Lunenburg
Some Observations of swarms of strange Insects, and the Mischief done by them.

A great Observer, who hath lived long in New England, did upon occasion relate to a Friend of his in London, where he lately was, That some few Years since there was such a swarm of a certain sort of Insects in that English Colony, that for the space of 200 Miles they poison'd and destroyed all the Trees of that Country; there being found innumerable little holes in the ground, out of which those Insects broke forth in the form of Maggots, which turned into Flies that had a kind of tail or stings, which they struck into the Tree, and thereby envenomed and killed it.

The like Plague is said to happen frequently in the Country of the Cossacks or Ukrains, where in dry Summers they are infested with such swarms of Locusts, driven thither by an East, or South-East Wind, that they darken the Air in the fairest weather, and devour all the Corn of that Country; laying their Eggs in Autumn, and then dying; but the Eggs, of which every one layeth two or three hundred, hatching the next Spring, produce again such a number of Locusts, that then they do far more mischief than afore; unless Rains do fall, which kill both Eggs and the Insects themselves, or unless a strong North or North-West Wind arise, which drives them into the Euxin Sea: The Hogs of that Country loving these Eggs, devour also great quantities of them, and thereby help to purge the Land of them; which is often so molest'd by this Vermine, that they enter into their Houses and Beds, fall upon their Tables and into their Meat, insomuch that they can hardly eat without taking down some of them; in the Night when they repose themselves upon the ground, they cover it three or four Inches thick, and if a Wheel pass over...
over them, they emit a stench hardly to be endured: All which, and much more may be fully seen in the French Description of the Countries of Poland, made by Monsieur de Beauplan, and by Monsieur Thevenot, in his Relation of the Cossacks, contained in the First part of his Curious Voyages.

An Observation touching the Bodies of Snakes and Vipers.

Several have taken notice, that there is a difference between the brooding of Snakes and Vipers, those laying their Eggs in Dung-hills, by whose warmth they are hatched; but these (Vipers) brooding their Eggs within their Bellies, and bringing forth live Vipers. To which may be added, That some affirm to have seen Snakes lye upon their Eggs, as Hens sit upon theirs.

Some Observations of odd Constitutions of Bodies.

A very curious Person, studying Physick at Leyden, to whom had been imparted those Relations about a Milky Substance in Veins, heretofore asledged in Numb. 6, returns, by way of gratitude, the following Observations.

There was (faith he) not many Years since, in this Country a Student, who being much addicted to the study of Astronomy, and spending very many Nights in Star-gazing, had, by the Nocturnal wet and cold temper of the Air, in such a manner obstructed the pores of his skin, that little or nothing exhaled from his Body; which appeared hence, because that the shirt he had worn five or six weeks, was then as white as if he had worn it but one day. In the mean while he gathered a subcutaneous Water, of which yet he was afterwards well cured.

We have also (faith the same) seen here a young Maid, of about thirteen Years of age, which from the time that she was but six Years old, and began to be about her Mother in
the Kitchin, would, as often as she was bid to bring her Salt, or could else come at it, fill her Pockets therewith, and eat it, as other children doe Sugar: whence she was so dried up, and grown so stiff, that she could not stirre her limbs, and was thereby starved to death.

That Learned and Observing Doctor John Beal, upon the perusal of the forementioned Numb. 6. was pleased to communicate this Note.

To your Observation, of Milk in Veines, I can add a phenomenon of some resemblance to it, which I received above 20. years agoe from Thomas Day, an Apothecary in Cambridge; vide. That himself let a man blood in the arme, by order of Doctor Eade, a Physitian there. The mans blood was white as Milk, as it run out of his arme, it had a little dilute redness, but immediately, as it fell into the Vessell, it was presently white; and it continued like drops of Milk on the pavement, where ever it fell. The conjecture which the said Physitian had of the cause of this appearance, was, that the Patient had much fed on Fish; affirming withall, that he had soon been a Leper, if not prevented by Physick.

A way of preserving Ice and Snow by Chaffe.

The Ingenious Mr. William Ball did communicate the relation hereof, as he had received it from his Brother, now residing at Livorne, as follows:

The Snow, or Ice-houses are here commonly built on the side of a steep hill, being only a deep hole in the ground, by which means, they easily make a passage out from the bottom of it, to carry away all the water, which, if it should remain stagnating therein, would melt the Ice and Snow: but they thatch it with straw, in the shape of a Saucepan-cover, that the rain may not come at it. The sides (supposing it dry) they line not with any thing; as is done in St. James's Park, by reason of the moistness of the ground. This Pit they fill
full of Snow or Ice (taking care that the Ice be made of the purest water, because they put it into their wine) over-spraying first the bottom very well with chaff; by which I mean not any part of the straw, but what remains upon the winnowing of the Corn; and I think, they here use Barley-chaff. This done, they further, as they put in the Ice, or the Snow, (which latter they ram down,) line it thick by the sides with such chaff, and afterwards cover it well with the same; and in half a year lying so, 'tis found not to want above an eight part of what it weighed, when first put in. When ever they take it out into the Aire, they wrap it in this chaff, and it keeps to admiration. The use of it in England would not be so much for cooling of drinks, as 'tis here generally used; but for cooling of fruit, sweet-meats &c. So far this Author.

The other usual way both in Italy and other Countries, to conserve Snow and Ice with Straw or Reed, is set down so punctually by Mr. Boyle in his Experimental History of Cold, pag. 408, 409. that nothing is to be added. It seems Pliny could not pass by these Conservatories, and the cooling of drinks with Ice, without passing this severe, though elegant and witty, Animadversion upon them: Hi Nives, illi glaciem potant, pandique montium in voluptatem gula vertunt: Servare algor aestibus, excogitaturre ut alienis mensibus nix algeat, lib. 19. cap. 4. But the Epigrammatist sports with it thus:

Non potare nivem, sed aquam potare rigentem

Directions for Sea-men, bound for far Voyages.

It being the Design of the R. Society, for the better attaining the End of their Institution, to study Nature rather than Books, and from the Observations, made of the Phenomena and Effects she presents, to compose such a History
ry of Her, as may hereafter serve to build a Solid and Useful Philosophy upon; They have from time to time given order to several of their Members to draw up both Inquiries of things Observable in foreign Countries, and Directions for the Particulars, they desire chiefly to be informed about. And considering with themselves, how much they may increase their Philosophical stock by the advantage, which England enjoys of making Voyages into all parts of the World, they formerly appointed that Eminent Mathematician and Philosopher Master Rooke, one of their Fellowes, and Geometry Professor of Greenwich College (now deceased to the great detriment of the Commonwealth of Learning) to think upon and set down some Directions for Sea-men going into the East & West-Indies, the better to capacitate them for making such observations abroad, as may be pertinent and suitable for their purpose; of which the said Sea-men should be desired to keep an exact Diary, delivering at their return a fair Copy thereof to the Lord High Admiral of England, his Royal Highness the Duke of York, and another to Trinity-house to be perused by the R. Society. Which Catalogue of Directions having been drawn up accordingly by the said Mr. Rooke, and by him presented to those, who appointed him to expedite such an one, it was thought not to be unseasonable at this time to make it publick, the more conveniently to furnish Navigators with Copies thereof. They are such, as follow:

1. To observe the Declination of the Compass, or its Variation from the Meridian of the place, frequently; marking withal, the Latitude and Longitude of the place, wherever such Observation is made, as exactly as may be, and setting down the Method, by which they made them.

2. To carry Dipping Needles with them, and observe the Inclination of the Needle in like manner.

3. To remark carefully the Ebbings and Flowings of the Sea, in as many places as they can, together with all the Accidents,
dents, Ordinary and Extraordinary, of the Tides; as, their precise time of Ebbing and Flowing in Rivers, at Promontories or Capes; which way their Current runs, what Perpendicular distance there is between the highest Tide and lowest Ebb, during the Spring-Tides and Neap-Tides; what day of the Moon's age, and what times of the year, the highest and lowest Tides fall out: And all other considerable Accidents, they can observe in the Tides, chiefly neer Ports, and about Islands, as in St. Helena's Island, and the three Rivers there, at the Bermudas &c.

4. To make Plotts and Draughts of prospect of Coasts, Promontories, Islands and Ports, marking the Bearings and Distances, as neer as they can.

5. To sound and mark the Depths of Coasts and Ports, and such other places near the shoar, as they shall think fit.

6. To take notice of the Nature of the Ground at the bottom of the Sea, in all Soundings, whether it be Clay, Sand, Rock, &c.

7. To keep a Register of all changes of Wind and Weather at all hours, by night and by day, shewing the point the Wind blows from, whether strong or weak: The Rains, Hail, Snow and the like, the precise times of their beginnings and continuance, especially Hurricanes and Spouts; but above all to take exact care to observe the Trade-Winds, about what degrees of Latitude and Longitude they first begin, where and when they cease, or change, or grow stronger or weaker, and how much; as near and exact as may be.

8. To observe and record all Extraordinary Meteors, Lightnings, Thunders, Ixgesfatui, Comets, &c., marking still the places and times of their appearing, continuance, &c.

9. To carry with them good Scales, and Glasse-Violls of a pint or so, with very narrow mouths, which are to be fill'd with Sea-water in different degrees of Latitude, as often as they
they please, and the weight of the Vial full of water taken exactly at every time, and recorded, marking within all the degree of Latitude, and the day of the Month: And that as well of water near the Top; as at a greater Depth.

**Some Observations concerning Jupiter. Of the shadow of one of his Satellites seen; by a Telescope passing over the Body of Jupiter.**

I have received an Account from very good hands, That on the 26th of September last, at half hour after seven of the Clock, was seen, both in Holland and in France (by curious Observers, with very good Tele-scopes) the shadow of one of the Satellites of Jupiter, passing over his Body. One of those small Stars moving about his Body (which are therefore called his Satellites) coming between the Sun and it, made a small Eclipse, appearing in the Face of Jupiter as a little round black Spot. The Particulars of those Observations, when they shall come to our Hands, we may (if need be) make them public: Which Observations, as they are in themselves very remarkable, and argue the Excellency of the Glasses by which they were discovered; So are we, in part, beholding to Monsieur Cassini for them, who giving notice before-hand of such Appearances to be expected, gave occasion to those Curious Observers to look for them.

**Of a permanent Spot in Jupiter: by which is manifested the conversion of Jupiter about his own Axis.**

Besides that Transient Shadow last mentioned, there hath been observed, by Mr. Hook first (as is mentioned in Numb. I. of these Transact.) and since by M. Cassini, a permanent Spot in the Disque of Jupiter; by the help whereof, they have been able to observe, not only that Jupiter turns about upon his own Axis, but also the Time of such conversion; which he estimates
estimates to be, 9 hours and 56 minutes.

For as Kepler did before conjecture, from the motion of
the Primitive Planets about the Sun as their Center, that the
Sun moved about its own Axis, but could not prove it, till
by Galileo and Shiner the Spots in the Sun were discovered;
so it hath been thought reasonable, from the Secondary Plan-
ets moving about Jupiter, that Jupiter is also moved about
his Axis: yet, till now, it hath not been evinced by Obser-
vation, That it doth so move; much less, in what Period of
Time. And the like reason there is to judge so of Saturn, be-
cause of the Secondary Planet discovered by Monsieur Hu-
gens de Zulichem to move about it; (though such motion be
not yet evinced from Observation:) as well as that of the
Earth, from its Attendant the Moon.

Whether the same may be also concluded of the other
Planets, Mars, Venus, and Mercury, (about whom have not
yet been observed any Secondary Planets to move,) is not
so evident. Yet there may be somewhat of like probability
in those. Not onely, because it is possible they may have
Secondary Planets about them, though not yet discovered;
(For, we know, it was long after those of Jupiter, before that
about Saturn was discovered; and who knows, what after
times may discover about the rest?) But because the Primary
Planets being all in like manner inlightned by the Sun, and
(in all likely hood) moved by it; it is likely that they be
moved by the same Laws and Methods; and therefore, turn'd
about their own Axis, as it is manifeft that some of them
are.

But, as for the Secundary Planets, as well those about
Jupiter, as that about Saturn; it is most likely that they have
no such Rotation upon their Axis. Not so much, because,
by reason of their smallness, no such thing hath been yet ob-
served, (or, indeed, could be, though it were true;) But be-
cause they being Analogical to our Moon, it is most likely
that they are moved in like manner. Now, though it be
true,
true, that there is some kind of Libration of the Moon's body, so that we have not precisely just the same part of it looking towards us; (as is evident by Hevelius' observations, and others;) yet is there no Revolution upon its Axis; the same part of it, with very little alteration, always respecting us, as is to be seen in Hevelius his Treatise de Motu Luna Libratorio; and, indeed, by all those who have written particularly of the spots in the Moon; and is universally known to all that have with any curiosity viewed it with Telescopes.

Of some Philosophical and curious Books, that are shortly to come abroad.

1. Of the Origin of Forms and Qualities, deduced from Mechanical Principles; by the Honorable Robert Boyle Esq.  
2. Hydrostatical Paradoxes, by the same. Both in English.  
3. A Treatise of the Origin of the Nile, by Monsieur Isaac Vossius, opposed to that of Monsieur de la Chambre, who is maintaining, That Niter is the principal cause of the Inundation of that River.  
4. A Dissertatio of Vipers, by Signor Redi, an Italian.  
5. A Discourse of the Anatomy of a Lyon, by the same.  
6. Another, De Figuris Salium, by the same.  
7. A Narration of the Establishment of the Lyncei, an Italian Academy, and of their Design and Statutes: the Prince Cés being the Head of them, who did also intend to establish such Philosophical Societies in all parts of the World, and particularly in Africa and America, to be by that means well informed of what considerable productions of Nature were to be found in those parts. The Author yet Anonymus.  
8. To these I shall add, a Book newly Printed in Oxford (and not yet dispersed) being, A Catalogue of Fixed Stars with their Longitudes, Latitudes, and Magnitudes, according to the Observations of Uleg-Beig (a King, and famous Astronomer, who was Great-Grand-child to the famous Tamerlane.
lane; and one of his Successors in some of his Kingdoms) made at Samarcand, his chief seat, (for the year of the Hegira 841, for the year of Christ 1437,) who not finding the Tables of Ptolemy to agree sufficiently with the Heavens, did with great diligence, and expense, make observations anew; as Tycho Brahe hath since done. It is a small part of a larger Astronomical Treatise of his, whereof there be divers Persian Manuscript Copies in Oxford. Out of which this is Translated and Published, both in Persian and Latine, by Mr. Thomas Hyde, now Library Keeper to the Bodleian Library in Oxford: (with Commentaries of his annexed:) Like as another part of it hath formerly been by Mr. John Graves. And it were a desirable work that the whole were Translated, that we might be the better acquainted with what was the Eastern Astronomy at that time.

Published with License.

Oxford, Printed by A: & L: Lichfield, for Ric; Davis. 1666.
PHILOSOPHICAL
TRANSACTIONS.


The Contents.

An Appendix to the Directions for Seamen, bound for far voyages.

Of the judgment of some of the English Astronomers, touching the difference between two learned men, about an Observation made of the first of the two late Comets. Of a Correspondency, to be procured for the finding out of the true distance of the Sun and Moon from the Earth. Of an Observation not long since made in England of Saturn. An Account of some Mercurial Observations, made with a Barometer, and their Results. Some Observations of Vipers, made by an Italian Philosopher.

An Appendix to the Directions for Seamen, bound for far Voyages.

Hereas it may be of good use, both Naval and Philosophical, to know, both how to sound depths of the sea without a Line, and to fetch up water from any depth of the same; the following wares have been contrived by Mr. Hook to perform both; (which should have been added to the lately printed Directions for Seamen, if then it could have been conveniently done.)
First, for the sounding of depths without a Cord, consider Figure 1, and accordingly take a Globe of Firr, or Maple, or other light Wood, as A: let it be well secured by Vernish, Pitch, or otherwise, from imbibing water; then take a piece of Lead or Stone, D, considerably heavier then will sink the Globe: let there be a long Wire-staple B, in the Ball A, and a springing Wire C, with a bended end F, and into the said staple, press in with your fingers the springing Wire on the bended end: and on it hang the weight D, by its ring E, and so let Globe and all sink gently into the water, in the posture represented in the first Figure, to the bottom, where the weight D touching first, is thereby stopp’d, but the Ball, being by the Impetus, it acquired in descending, carried downwards a little after the weight is stopp’d, suffers the springing wire to fly back, and thereby lets it self at liberty to reascend. And, by observing the time of the Ball’s stay under water (which may be done by a Watch, having minutes and seconds, or by a good Minut-glass, or best of all, by a Pendulum vibrating seconds) you will by this way, with the help of some Tables, come to know any depth of the sea.

Note, that care must be had of proportioning the weight and shape of the Lead, to the bulk, weight, and figure of the Globe, after such a manner, as upon experience shall be found most convenient.

In some of the Tryals already made with this Instrument, the Globe being of Maple-wood, well covered with Pitch to hinder soaking in, was 5½ inches in diameter, and weighed 2⅛ pounds: the Lead of 4½ pounds weight, was of a Conical figure, 11 inches long, with the sharper end downwards, ½ inches at the top, and ¼ at the the bottom in diameter. And in those Experiments, made in the Thames, in the depth of 19 foot water, there passed between the Immerision and Emerision of the Globe, 6 seconds of an hour; and in the depth of 10 foot water, there passed 3½ seconds or thereabout: From many of which kind of Experiments, it will likely not be hard to finde
out a method to calculate, what depth is to be concluded from any other time of the like Globes stay under water.

In the same Tryals, made with this Instrument in the said River of Thames, it has been found, that there is no difference in time, between the submerriages of the Ball at the greatest depth, when it rose two Wherries length from the place where it was let fall (being carried by the Current of the Tide) and when it rose within a yard or so of the same place where it was let down.

The other Instrument, for Fetching up water from the depth of the sea, is (as appears by Figure 2.) a square wooden Bucket C, whose bottoms EE, are so contrived, that as the weight A sinks the Iron B, (to which the Bucket C, is fastned by two handles DD, on the ends of which are the moveable bottoms or Valves EE,) and thereby draws down the Bucket; the resistance of the water keeps up the Bucket in the posture C; whereby the water hath, all the while it is descending, a clear passage through: whereas, as soon as the Bucket is pulled upwards by the Line F, the resistance of the water to that motion beats the Bucket downward, and keeps it in the posture G, whereby the Included water is preserved from getting out, and the Ambient water kept from getting in.

By the advantage of which Vessel; it may be known, whether sea water be Salter at and towards the bottom, then at or near the top: Likewise, whether in some places of the sea, any sweet water is to be found at the bottom; the Affirmative whereof is to be met with in the East Indian Voyages of the industrious John Hugh Van Linschoten, who page 16 of that Book, as 'tis Englished, records, that in the Persian Gulph, about the Island Barem, or Baharem, they fetch up with certain Vessels (which he describes not) water out of the sea, from under the salt-water, four or five fathom deep, as sweet, as any Fountain water.
Of the Judgement of some of the English Astronomers, touching the difference between two learned men, about an Observation made of the First of the two late Comets.

Whereas notice has been taken in Num. 6. of these Transactions, that there was some difference between those two deservedly celebrated Philosophers, Monsieur Hevelius and Monsieur Augout, concerning an Observation, made by the former of them, on the 13 of February 1665, & that thereupon some Eminent English Astronomers, considering the importance of the dispute, had undertaken the examination thereof; it will, 'tis conceived, not be unacceptable to such, as saw those Papers, to be informed, what has been done and discerned by them in that matter. They having therefore compared the Printed Writings of the two Diffenters, and with all consulted the observations made with Telescopes at home, by some of the most intelligent Astronomers amongst them, who have attentively observed the Position of that Comet to the Telescopic Stars, that lay in its way; Do thereupon Joyntly conclude, that, whatever that Appearance was, which was seen near the First star of Aries, by Monsieur Hevelius (the truth of whose relation concerning the same, they do in no wise question) the said Comet did not come near that Star in the left Ear of Aries, where the said M. Hevelius supposes it to have passed, but took its course near the Bright Star in its Left Horn, according to Bayers Tables. And since that the Observations of judicious both French, Italian, &c Dutch Astronomers (as many of them, as are come to the knowledge of the English) do in the main fully agree with theirs, they do not at all doubt, but that, there being such an unanimous con-
Of a Correspondency, to be procured for the Finding out the True distance of the Sun and Moon from the Earth, by the Paralax, observed under (or near) the same Meridian.

Seeing that the knowledge of this distance may prove of important use, for the Perfecting of Astronomy, and for the better establishing the doctrine of Refractions; it is in the thoughts of some very curious Persons in England, for the finding out the same, to settle a Correspondency with some others abroad, that are understanding in Astronomical matters, and live in places far distant in Latitude, and under (or near) the same Meridian.

To perform which, the following Method is proposed to be observed; viz. That at certain times agreed on by two Observatours, making use of Telescopes, large, good and well fitted for this purpose, by a measuring rod, placed within the Eye-glass at a convenient distance, that it may be distinctly seen, and serve for measuring small distances by minutes and seconds (which is easy enough in large Telescopes) that, I say, each of such observers, thus furnish'd, shall observe the visible way of the Moon among the Fixt stars, (by taking her exact distance from any Fixt star, that lies in or very near her way, together with the exact time of her so appearing) and the then apparent Diameter of her Disk; continuing these Observations every time for two or three hours, that so,
if possible, two exact observations of her Apparent place among the Fixt stars being made, at two places thus distant in Latitude, and as near as may be under the same Meridian, by these Observators concurring at the same time, her true and exact distance may be hence collected, not only for that time, but at all other times, by any single Observator’s viewing her with a Telescope, and measuring exactly her Apparent Diameter. It were likewise desirable, that as often as there happens any considerable Eclipse of the Sun, that this also might be observed by them, noting therein the exact measure of the greatest Obscurcation compared with the then Apparent Diameter of his Disk. For by this means, after the distance of the Moon hath been exactly found, the distance of the Sun will easily be deduced.

As for the time, fittest for making Observations of the Moon, that will be, when she is about a Quarter or somewhat less illuminated, because then her light is not so bright, but that with a good Telescope she may be observed to pass close by, and sometimes over several Fixt stars; which is about four or five days before or after her Change: Or else at any other time, when the Moon passes near or over some of the bigger sort of Fixt stars, such as of the first or second Magnitude; which may be easily calculated and foreseen: Or best of all, when there is any Total Eclipse of the Moon; for then the smallest Telescopical stars may be seen close adjoyning to the very body of the Moon. Of all which particulars the two Correspondents are to agree, as soon as he, that is to joyne abroad, shall be found out; whereupon they are mutually to communicate to each other, what they shall have thus observed in each place.

Of an Observation, not long since made in England, of Saturn.

This Observation was made by Mr. William Ball, accompanied
panied by his brother, Dr. Ball, October 13, 1665, at six of
the Clock, at Mainhead near Exeter in Devonshire, with a very
good Telescope near 38 foot long, and a double Eye-glass, as
the observer himself takes notice, adding, that he never saw
that Planet more distinct. The observation is represented by
Figure 3. concerning which, the Author faith in his letter to a
friend, as follows; This appear'd to me the present figure of
Saturn, somewhat otherwise, than I expected, thinking it
would have been decreasing, but I found it full as ever, and a
little hollow above and below. Whereupon the Person, to
whom notice was sent hereof, examining this shape, hath by
Letters desir'd the worthy Author of the Système of this Pla-
net, that he would now attentively consider the present Figure
of his Anses or Ring, to see whether the appearance be to him, as
in this Figure, and consequently whether he there meets with
nothing that may make him think, that it is not one body of a
Circular Figure, that embraces his Diske, but two.
And to the end that other Curious men, in other places might
be engaged, to joyn their Observations with him, to see, whether
they can find the like appearance to that, represented here,
especially such Notches or Hollandinesses, as at A and B, it was
thought fit to insert here the newly-related Account.

A Relation of some Mercurial Observations,
and their Results.

Modern Philosophers, to avoid Circumlocutions, call that In-
strument, wherein a Cylinder of Quicksilver, of between 28.
and 31. Inches in Altitude, is kept suspended after the manner
of the Torricellian Experiment, a Barometer or Baroscope, first
made publick by that Noble Searcher of Nature, Mr. Boyle,
and implored by Him and others, to detect all the minut
variations in the Pressure and weight of the Air. For these more
curious
curious and nice distinguishing of which small changes, Mr. Hook in the Preface to his Micrography, has described such an Instrument with a Wheel, contrived by himself, and, by these two last years trials of it, constantly found most exact for that purpose: which being so accurate, and not difficult to be made, it were desirable, that those who have a Genius and opportunities of making Observations of this kind, would furnish themselves with such of these Instruments, as were exactly made and adjusted according to the Method, delivered in the newly mentioned place.

To say something of the Observations, made by this Instrument, and withal to excite studious Naturalists to a sedulous prosecution of the same, the Reader may first take notice, that the lately named Mr. Boyle hath (as himself not long since did intimate to the Author of these Tracts) already made divers Observations of this kind in the year 1659, and 1660, before any others were publick, or by him so much as heard of; though he has hitherto forbore to divulge them, because of some other Papers (in whose company they were to appear) which being hindered by other studies and employments, he hath not as yet finished.

Next, that, besides several others, who, since have had the curiosity of making such observations, the Worthy and Inquisitive Dr. John Beal, is doing his part with much affiduity (of which he hath by several Letters acquainted his Friends in London) both by observing himself, and by procuring many Correspondents in several places in England for the same purpose; judging it of great importance, that Observations of this kind be made in parts somewhat distant from one another, that so from many of those, accurately made and then compared, it may be discovered, whether the Aire gravitates more in the parts of the Earth lying more East or West, North or South? whether on such as lie nearer to the Sea, or further up into the Mainland? in hotter or colder weather? whether in high
Whether high Winds or Calms? whether in wet weather or dry? whether most when a North, or when a South, when an East or a West wind blows? and whether it keeps the same seasons of Changes? and whether the seasons and changes of the Air and Weather can be thereby discover'd, and the now hidden causes of many other Phenomena detected?

The said Doctor is so much pleased with the discovery already made by the help of this Instrument, that he thinks it to be one of the most wonderful that ever was in the World, if we speak of strangeness, and just wonder, and of Philosophical importance, separate from the interest of lucre. For (faith he, in one of his Letters) who could ever expect, that we men should find an Art, to weigh all the Air that hangs over our heads, in all the changes of it, and, as it were, to weigh, and to distinguish by weight, the Winds and the Clouds? Or, who did believe, that by palpable evidence we should be able to prove, the serenest Air to be most heavy, and the thickest Air, and when darkest Clouds hang neerest to us, ready to dissolve, or dropping, then to be lightest. And though (so he goes on) we cannot yet reach to all the Uses and Applications of it; yet we should be entertain'd for a while, by the truly Honourable Mr. Boyle, as the leading person herein, upon the delight and wonder. The Magnet was known many hundreds of years before it was applied to find out New Worlds. To me (faith he) is a wonderful delight, that I have alwaies in my Study before my eye such a Curious Ballance.

Having thus in General expressed his thoughts about this Invention, and the singular pleasure, he takes in the Observations made therewith, he descends to particulars, and in several Letters communicates them to his Correspondent, as follows:

1. My Wheel-barometer I could never fill so exactly with Mercury, as to exclude all Air; and therefore I trust more
to a *Mercurial Cane*, and take all my Notes from it. This Cane is but 3½ Inches long, of a very slender Cavity, and thick Glass. This may easily be conveyed to any place, for Trials. The Vessel for the stagnating Mercury, into which the said Cane is immersed, is about two Inches wide. The Mercury so well fill’d, that for some daies it would not subside, but hung to the top of the Glass-cane. I keep it in a Closet pretty close, 9. foot high, 8. foot broad, 15. foot long; near a Window. This I note, because possibly the closeness of the room may hinder, that it gives not the full of all Changes, as it might in a more passable Air.

2. In all my Observations from May 28. 1664, to this present (December 9. 1665,) the Quicksilver never ascended but very little above 30½ Inches.

3. It ascended very seldom so high (videl. to 36½ Inches) chiefly in Decemb. 13. 1664, the weather being fickle-fair, Evening.

4. I find by my Calendar of June 22. 1664. at 5. in the Morning, in a time of long settled fair weather, that the Mercury had ascended about half an Inch higher then 30: but I fear some mistake, because I then took no impression of wonder at it; yet for 3, or 4. daies, at that time it continued high, in well-setled, fair and warm weather; most part above 30. Inches. So that I may note, the Mercury to rise as high in the hottest Summer, as in the coldest Winter-weather.

5. Yet surely I have noted it ascend a little higher for the Coldness of the Weather; and very frequently, both in Win-
6. Generally in settled and fair weather both of Winter and Summer, the Mercury is higher, than a little before or after, or in Rainy weather.

7. Again, generally it descended lower after Rain, than it was before Rain.

8. Generally also it falls in great winds; and somewhat it seem'd to sink, when I open'd a wide door to it, to let in stormy winds; yet I have found it to continue very high, in a long stormy wind of 3. or 4. daies.

9. Again, generally it is higher in an East and North-wind. (Ceteris paribus) than in a South and West-wind.

10. I tried several times, by strong fumes and thick smoaks to alter the Air in my Closet, but I cannot affirm, that the Mercury yielded any more, than might be expected from some increas of heat. Such as have exact Wheel-Barometers, may try whether Odors or Fumes do alleviate the Air.

11. In this Closet, I have not in all this time found the extreme st changes of the Quicksilver to amount to more, than to 2$\frac{1}{4}$, or to 2$\frac{3}{4}$ inches, at most.

12. Very often I have found great changes in the Air, without any perceptible change in the Barometer; as in the dewy nights, when the moisture descends in a great quantity, and the thickness sometimes seems to hide the Stars from us: In the days foregoing and following, the Vapors have been drawn
drawn up so *Invisibly*, that the Air and Sky seem'd very clear all day long. This I account a great change between ascending and descending Dews and Vapors (which import Levity and Weight,;) and between thick Air and clear Air: which changes do sometimes continue in the Alternative course of day and night, for a week or fortnight together: and yet the Baroscope holding the same.

13. Sometimes (I say not often) the Baroscope yields not to other very great changes of the Air. As lately (December 18.) an extraordinary bright and clear day: and the next following quite darkned, some Rain and Snow falling; but the Mercury the same: so in high winds and calms the same.

14. I do conceive, that such as converse much *Sub die*, and walk much abroad, may find many particulars much more exactly, then I, who have no leisure for it, can undertake. To instance in one of many, December 16. last, was a clear cold day, very sharp and strong East wind, the Mercury very near 30. inches high, about three in the afternoon, I saw a large black cloud, drawing near us from the East and South-East, with the East-wind. The Mercury changed not that day nor the day following; the Stars and most of the sky were very bright and clear till Nine of the Clock; and then suddenly all the sky was darkned, yet no change of weather happened: December 17. the frost held, and 'twas a clear day, till about two of the clock in the afternoon; and then many thick clouds appear'd low in the West; yet no change of the weather here; the Wind, Frost, and Quick-silver, the same, December 18. the Mercury fell almost 4 of an inch, and the sky and Air so clear and bright and cold with an East-wind, that I wondered what could cause the Mercury to descend. I Expected, it should have ascended, as usually it does in such clear skys. Casually I sent my servant abroad, and he discovered the remote Hills, about 20. miles off, cover'd with snow
This seem'd to manifest, that the Air, being discharged of the clouds by snow, became lighter.

15. I have seldom seen the change to be very great, at any one time. For, though I do not now take a deliberate view of my Notes, yet I wonder'd once to see, that in one day it subsided about \(\frac{3}{4}\) of an inch.

16. Of late I have altered my Method upon the Barometer, observing it, as it is before my Eyes, all day long, and much of the night, being watchful for the moments of every particular change, to examine, what cause in the Air and Heavens may appear for such changes. And now my wonder is, to see, how slow it is, it holding most between the nine and twentieth and thirtieth inch of late.

17. I must now (January 13. 1662) tell you, that the Mercury stands at this time (as it did also yesterday) a quarter above 30. inches; yet both days very dark and cloudy, sometimes very thick and misty Air; which seldom falls out. For, for the most part, I see it higher in clearest settled weather, than in such cloudy and misty Fogs. This thick Air and darkness hath lasted above a week; lately more Cold, and East and North-East wind.

Thus far the Notes of this Observing Divine; of which Mr. Boyle, to whom they were also communicated, entertains these thoughts, that they seem to him very faithfully made, and do for the main, agree well enough with his observations, as far as he remembers, not having them, it seems, at that time, when he wrote this, at hand; and though it be wished by him, that the Observer's Glass-Cane had been somewhat bigger; yet his diligence in fitting it so carefully, or rather so skilfully, as is above-mentioned, is much by him commended.

Some
A curious Italian, called Francesco Redi, having lately had an opportunity, by the great number of Vipers, brought to the Grand Duke of Tuscany for the composing of Theriac or Treafe, to examine what is vulgarly delivered and believed concerning the Poyfon of those Creatures, hath, (according to the account, given of it in the French Journal des Scavans, printed January 4. 1665) performed his undertaking with much exactness, and publish'd in an Italian tract, not yet come into England, these Observations.

1. He hath observed, that the poyfon of Vipers is neither in their Teeth, nor in their Tail, nor in their Gall; but in the two Vesicles or Bladders, which cover their teeth, and which coming to be compressed, when the Vipers bite, do emit a certain yellowish Liquor, that runs along the teeth and poyfons the wound. Whereof he gives this proof, that he hath rub'd the wounds of many Animals with the Gall of Vipers, and pricked them with their Teeth, and yet no considerable ill accident follow'd upon it, but that as often as he rubbed the wounds with the said yellow Liquor, not one of them escaped.

2. Whereas commonly it hath hitherto been believed, that the poyfon of Vipers being swallowed, was present death; this Author, after many reiterated Experiments, is said to have observed, that in Vipers there is neither Humour, nor Excrement, nor any part, not the Gall itself, that, being taken into the Body, kills. And he assures, that he hath seen men eat, and hath often made Bruit Animals swallow all that is esteem'd most poyfonous in a Viper, yet without the least mischief to them. Whence he shews, that it needs not so much to be wondred at, that certain Empiricks swallow the juyce of the most
most venomous Animals without receiving any harm thereby; adding, that, which is ascribed to the virtue of their Antidote, ought to be attributed to the nature of those kinds of Poylons, which are no poisons, when they are swallow'd, (for which Doctrine he also alleges Celsus) but onely when they are put into wounds. Which also has been noted by Lu-cean, who introduces Cato thus speaking:

Noxia serpantium est admisto sanguine pestis,  
Morsu virus habent, & fatum dente minantur;  
Pocula morte carent.

And what also some Authors have affirm'd, videl. That it is mortal, to eat of the Flesh of creatures killed by Vipers; or to drink of the Vine wherein Vipers have been drown'd: or to suck the wounds that have been made by them, is by this Authour observed to be wide of truth. For he assures, that many persons have eaten Pullets and Pigeons, bitten by Vipers, without finding any alteration from it in their health. On the contrary, he declares, That it is a soveraign Remedy against the biting of Vipers, to suck the wound; alleging an Experiment, made upon a Dog, which he caus'd to be bitten by a Viper at the nose, who by licking his own wound sav'd his life. Which he confirms by the example of those people, celebrated in History by the name of Marfi and Pfilli, whose Employment it was, to heal those, that had been bitten by Serpents, by sucking their wounds.

3. He adds, that although Galen and many modern Physici-ans do affirm, that there is nothing, which causeth so much thirst, as Vipers-flesh, yet he hath experimented the contrary and known divers persons, who, did eat the flesh of Vipers at all their meals, and yet did assure him, they never were less dry, then when they observed that kind of Diet.

4. As for the Salt of Vipers, whereof some Chymists have fo
so great esteem, he faith, that it hath no Purging vertue at all in it: adding that even of All Salts, none hath more vertue than another, as he pretends to have shew'd in an other Book of his, De natura salium; which also hath not been yet transmitted into these parts.

5. He denies, what Aristotle assures, and what Galen faith to have often tryed, that the Spittle of a Fasting person kills Vipers, and he laughs at many other particulars, that have been delivered concerning the Antipathy of Vipers unto certain things; and their manner of Conception and Generation, and several other properties, commonly ascribed to them; which the alleged French Author affirms to be refuted by so many experiments made by this Italian Philosopher, that it seems to him, there is no place left for doubting, after so authentick a testimony.

Advertisement.

The Reader of these Transactions is desired to correct these Errata in Number 8. viz. page 132. line penult. read Wine for Lime; and page 133. line 10. read Thresber for Treger, as some Copies have it; and page 136. line ult. read purifie for putrisie.

LONDON,
Printed for John Martyn and James Allestrée, Printers to the Royal Society. 1666.
The Contents.

Observations continued upon the Barometer, or Ballance of the Air.

A Relation concerning the Earth-quake near Oxford; together with some Observations of the sealed Weatherglass and Barometer thereupon by Dr. Wallis. A more full and particular Account of these Observations about Jupiter, that were mention'd in Numb. 8. An Account of some Books, lately published, videl. Mr. Boyle's Hydrostatical Paradoxes; Steno de Musculis & Gladulis; De Graeff de Natura & Usu Succi Pancreatici.

Observations continued upon the Barometer, or rather Ballance of the Air.

These Transactions being intended, not only to be (by parcels) brief Records of the Emergent Works and Productions in the Universe; Of the Mysteries of Nature of later discoveries; And, of the growth of Useful Inventions and Arts; but also, and chiefly, to solicitate in all parts mutuell Ayds and Collegiate endeavours for the farther advancement thereof: We shall begin this Second year of our Publications in this kind (in which, for 3 moneths the Printing-preffes were interrupted by the publick Calamity) with a few more particular Observations upon the Ballance of the Air, as they are most happily invented and directed by Mr. Boyle; and deserve to be prosecuted with care and diligence in all places.

But it is to be premised, that that Worthy person, who was alleged as the Author of the Observations, delivered of this kind in the last of these Tracts (Dr. Beale) gives notice, That...
he did not pretend to exactness, but only to excite the carefulness of others in the several distant places, and chiefly such, as can have the assistance of a Wheel Balance perfectly filled: without both which aids he hopes not to obtain all the benefits and mysteries of this Invention.

This being thus briefly intimated, the Account of the Observations themselves, as they were extracted out of a late Letter of the same Person, are, as follows:

1. As I have fitted and filled the Single Cane, I can say in the general, That I have not yet found any such infallible Prognostick of these changes of weather, which do follow a long Serenity, or settled weather. And perchance in brighter Climates it may be constantly infallible. In these Northern Islands, the Clouds are so short, and narrow, and by fickle changes are sometimes emptied upon us, sometimes so near, as may make so little variation in the weight of the whole Atmophere of Air, as may sometimes deceive us, or smother and hide from us the causes of fixedness, or of changes. I wish I could see a good Calendar or Journal taken in Tangier, and in some of our Northern and most Southern parts of America. I have store of Hygroscopes of divers kinds, and I do remark them, and the Sweating of Marble, and as many other famed Prognosticks, as I can hear of, but can find nothing so near, as indicative of the change of weather, as this Ballance. Those others are often changed by Dews, which do not at all alter the Ballance, nor alter the state of the weather: And the open Weather-glass is known to signify nothing at certainty, having a double obedience to two Masters, sometimes to the Weight of the Air, sometimes to Heat, as the service is commanded.

2. And in further confirmation of this Note, I may add to the former, That in January last, from the fourth, and more especially from the seventh day, for many days it continued very dark, so that all men expected daily great rain; yet the Mercury held very high, neer to the greatest height: And though in those days sometimes thick mists arose, and some small rain fell, yet the Quick-silver held at a great height: which did indicate to me, there could then be no great change of weather. As the small rain fell, it yeilded somewhat, not much, and that does more
confirm the indication. And more lately, in very dark days, I had the same confidence upon the same ground, and I was not disappointed.

3. Again, if the Mercury ascends to a good height after the fall of rain (as sometimes, but less often it does) then I look for a settled serenity; but if it proceeds after rain in a descending motion, then I expect a continuance of broken and showry weather. But in all, as I only say, For the most part, so I dare not positively declare it an affirmative result, but do refer it to the remarks of others. And this may explicate the Notes 6. and 14. of Num. 9. into more clearness.

4. That we find the Weather and our Bodies more chill, cold, and drooping, when the Mercury is lowest, and the Air lightest, besides other causes, I guess, That as Air is to us the breath of life, as water is to Fishes; so, when we are deprived of the usual measure of this our food, 'tis the same to us, as when the water is drawn ebb from Fishes. But I would much rather be instructed by others, then offer much in this kind.

5. The lowest descent of the Mercury in all the time, since I have observed it, was Octob. 26. 1665. in the Evening, when it was very near at 27 1/2 Inches. Which I find thus circumstances with the weather in my notes.

Inch.

Oct. 25. Morning; Mercury at 28 1/2. Great storms and much rain.
Oct. 26. Evening; Merc. at 27 1/2. That day, and some days following, the weather was variable, frequent rain, and as you see, the Mercury lower, than usual.

6. Over the place, where this Mercurial Cane stands, I have set a Wind vane, with purpose of exactness, of a Streamer in Brass so large, and pointing to a Board indented in the Margin, that I can at a sure Level upon the Vane, take every of the 32. points of the Wind, half points, and quarter points, at good distance. Otherwise we may find our guesses much deceived, as the best guessers, upon trial, do acknowledge. And this exactness may become the Wheel-balance, which shews the minutest variations almost beyond imagination. And thus any servant, at the approach of a thick Cloud, or other Meteor, higher or lower, or at the rising of a storm or fresh wind in the night, or day, may bring a report of the Weight of the Air, as certainly and almost as easily,
easily, as of the Sun from the Dial in a Sunshine. It were good to have an Index of Winds, that discover’d as well their Ascent and Descent, as their Side-coastings.

A Relation concerning the late Earthquake near Oxford; together with some Observations of the sealed Weather-glass, and the Barometer, both upon that Phænomenon, and in General.

This Relation was communicated by the excellently learned Dr. Wallis, as follows:

On the 19. of January 1665. Stylo Angliae (or Jan. 29. 1666. Stylo novo) at divers places near Oxford, was observ’d a small Earthquake (as at Blechington, Stanton-St. John, Bril, &c.) towards evening. In Oxford itself, I doe not hear, that it was observ’d to be an Earthquake; yet I remember about that time (whether precisely then or not, I cannot say) I took notice of some kind of odd shaking or heaving, I observ’d in my study, but did impute it to the going of Carts or Coaches, supposed to be not far off; though yet I did take notice of it, as a little differing from what is usual on such occasions; (and wondered the more, that I did not hear any:) But not knowing, what else to refer it to, I thought no more of it. And the like account I have had from some others in Oxford, who yet did not think of an Earthquake; it being a rare thing with us. Hearing afterwards of an Earthquake observed by others; I looked on my Notes concerning my Thermoscope and Baroscope, to see if any alteration considerable had then happened.

My Thermoscope consists of a round large Glass, containing about half a pint or more; from whence issues a long Cylindrical neck of Glass, about two foot and a half in length, and less than a quarter of an inch diameter; which neck was hermetically sealed at the top, to exclude communication with the External Air; but before the sealing of it, the whole Glass was filled with Spirit of Wine (tinged with Cochineal, to make it the more discernable to the Eye) so warmed, that it filled the whole content of the Glass; but afterwards, as it cooled, did so subside, as to leave a void space in the upper part of the Neck. Which Instrument, so prepared, both by the rising or falling of the tinged liquor in the neck (consequent upon the expanding or contracting of the whole liquor contained in it and the Ball below) give a very nice account of the Temperature of the Air,
as to Heat or Cold: Even so nice, as that my being or not being in my Study I find to vary its height sometimes almost a quarter of an inch.

My Baroscope, I call another Instrument for estimating the Weight or Pressure of the Incumbent Air, consisting of a long Glass tube of about 4 foot in length, and about a quarter of an inch Bore: which tube (hermetically sealed at the one end) being filled with Quicksilver (according to the Torricellian Experiment) is inverted, so as to have the open end of it immersed in Stagnant Quicksilver, contained in a larger Glass under it, exposed to the pressure of the outward Air: Out of which open end (after such immersion) the Quicksilver in the Tube being suffered to run out, as much as it will, into the Stagnant Quicksilver, in which that mouth or open end is immersed, there is wont to remain (as is commonly known to those acquainted with this Experiment) a Cylinder of Quicksilver suspended in the Tube, about 28, 29, or 30, inches high; measuring from the surface of the Stagnant Quicksilver perpendicularly: (but more or less, within such limits, according as the Weight or Pressure of the Air incumbent on the External Stagnant Quicksilver exposed to it, is greater or less:) leaving the upper part of the Tube void. (Both which Instruments being the contrivance of the Honourable Robert Boyle, they are by him more particularly described in his Physico-Mechanical Experiments touching the Air, Exper. 17. and 18. and in his Thermometrical Discourses, prefixed to his History of Cold.)

Now, according to both these Instruments, having kept a daily Register of Observations for more than a whole year (having when I have been for some short time absent from home) I find my Notes for that day to be these.

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Day.</td>
<td>Hour.</td>
<td>inches.</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

So that, there being in the morning (January 19.) a hard frost (which began the day before about 4. of the Clock in the afternoon
noon (Jan. 18.) and continued (with us) till about 5. of the Clock in the afternoon of that day, Jan. 19. with some fierceness) and the weather, Jan. 19. being in the morning, close; and cloudy all the day, with little of Sun-shine; the Liquor in the Thermoscope was very little raised; by 4. of the Clock afternoon, that is, but ¾ of an inch (which, had the Sun shone, would, it's likely, have been near an Inch:) and after that time (or somewhat before) had there been no considerable change of weather, it would upon the Sun's setting have fallen (and probably so it did, till about 5. of the Clock, though I took no Observation in the interim.) But, contrary to what would have been expected, it was at 9. of the Clock at night, higher by ½ of an inch, than it had been at 4. occasioned by the change of weather, the Frost suddenly breaking, with us, between 5. and 6. of the Clock; about which time also it began to rain, and continued raining that Evening and good part of the Night. And the next morning I found the Liquor yet higher by half an inch, vid. 1½ inches: (by reason of the Air that night being so much warmer, than it had been the day before;) whereas commonly it is considerably lower in the morning, than over night.

As to the Baroscope, for the Weight or Pressure of the Air; I find, that for the 11, 12, 13, 14, 15, 16, and 17. day's, the Mercury in the Tube, was (by the ballancing Pressure of the incumbent Air on the stagnant Quicksilver, exposed to it) kept up to the height of near 30. Inches above the surface of the External Quicksilver, (though with some little variation, as 30, 29½, 29¾, 29⅞, but never so low, all that time, as 29⅝;) which is the greatest height I have known it at, (for I do not find, that I have ever, till then, observed it to be, in my Glasses, full 30. Inches, though it have been very near it: the Weather having been almost continually Foggy, or very thick Mists, all that time. January 18. it came down to 29½, in the forenoon; and afternoon to 29¾, about the time the frost began: And Jan. 19. it was, at 8. in the morning, come down to 29½; at 4. in the afternoon, to 29¾. But at 9. in the evening (when the Earthquake had intervened) it was risen half an inch, vid. to 29¼. And, by the next morning, fallen again a whole inch, vid. to 28¼; which fall I attribute (at least in part) to the rain that fell in the night.

This being what I observed out of my Register of these Instruments, (which, if I had then thought of an Earthquake, I should
should have more nicely watched) what I have further gathered from Reports, is to this purpose.

I hear, it was observed at Blechington, above 5. miles to the North of Oxford, and so along by Boftol, Horton, Stanton-St. John, and so towards Whately, which is about 4. miles Eastward from Oxford. Not at all these places at the same time, but moving forward from Blechington towards Whately. For it was at Stanton about 6. of the Clock or later (as I understand from Mr. Boyle, who was there at that time;) but had been at Blechington a good while sooner. And I am told, that it was taken notice of by Doctor Holder (a Member of our Society) who was then at Blechington, to be observed by those in the further part of the Garden, some very discernable time before it was observed by those in the Houle; creeping forward from the one place to the other. What other places in the Country it was observed at, I have not been informed: but at Oxford (which, it seems, was about the skirts of it) it was so small, as would have been hardly noted at all, had not the notice, taken of it abroad, informed us of it.

Upon this Occasion, it will not be unseasonable to give some General accounts of what I have in my Thermoscope and Baroscope observed.

My Thermoscope, being fitted somewhat at adventures, I have found at the lowest to be somewhat more than 12. inches high, in the fiercest time of the long Frost in the beginning of the last year 1665, and about 27. Inches high, at the highest, in the hottest time of the last Summer: (which I mention, that it may appear at what temperature in proportion, the Air was at the time above-mentioned.) But I must add withal, that this standing so, as never to be exposed to the Sun, but in a room, that has a window only to the North, it would have been raised much higher than 27. inches, if it were put in the hot Sun-shine in Summer; this, as it is placed, giving therefore an account only of the Temperature of the Air in general, not of the immediate heat of the Sun-shine.

This Instrument, thus situated, when it is about 15. inches, or lower, is for the most part hard frost; but seldom a frost, if higher than 16. Yet this I have often observed, that the Air by the Thermoscope has appeared considerably colder (and the liquor lower) at sometimes when there is no Frost, than at some other times, when the Frost hath been considerably hard.
In my Baroscope, I have never found the Quicksilver higher than 30 inches, nor lower than 28; (at least, scarce discernably, not \( \frac{1}{10} \) of an inch higher than that; or lower than this:) which I mention, not only to shew the limits, within which I have observed mine to keep, vid. full 2 inches, but likewise as an Estimate of the Clearness of the Quicksilver from Air. For, though my Quicksilver were with good care cleansed from the Air, yet I find that which Mr. Boyle useth, much better: for, comparing his with mine at the same times, and both in Oxford, at no great distance; I find his Quicksilver to stand alwaius somewhat higher than mine (sometimes neer a quarter of an Inch:) which I know now how to give a more probable account of, than that my Quicksilver is either heavier than his; or else, that his is better cleansed from Air; (unless, possibly, the difference of the Bore, or other circumstances of the Tube, may cause the alteration; mine being a taller Tube, and a bigger Bore, than his.) And upon like reason, as his stands higher than mine; so another, less cleansed from Air, may at the same time be considerably lower, and consequently under 28. Inches at the lowest.

In thick foggy weather, I find my Quicksilver to rise; which I ascribe to the heaviness of the Vapours in the Air. And I have never found it higher, than in the foggy weather above-mentioned.

In Sunshiny weather, it riseth also (and commonly the clearer, the more;) which, I think, may be imputed partly to the Vapors raised by the Sun, and making the Air heavier; and partly to the Heat, increasing the Elastick or Springy power of the Air. Which latter I the rather add, because I have sometimes observed in Sunshiny weather, when there have come Clouds for some considerable time (suppose an hour or two) the Quicksilver has fallen; and then, upon the Sun's breaking out again, it has risen as before.

In Rainy weather, it useth to fall (of which the reason is obvious, because the Air is lightned, by so much as falls;) In Snowy weather, likewise, but not so much as in Rain. And sometimes I have observed it, upon a Hoar-frost, falling in the night.

For Windy weather, I find it generally to fall; and that more universally, and more discernably, than upon Rain: (which I attribute to the Winds moving the Air collaterally, and thereby not suffering it to press so much directly downwards: the like of which
which we see in swimming, &c.) And I have never found it lower than in high Winds. *

I have divers times, upon discerning my Quicksilver to fall without any visible cause at home, looked abroad, and found (by the appearance of broken Clouds, or otherwise) that it had rained not far off, though not with us: Whereupon, the Air being then lightened, our heavier Air (where it rained not) may have, in part, discharged it self on that lighter.

A more particular Account of those Observations about Jupiter, that were mentioned in Numb. 8.

Since the publishing of Numb. 8. of these Transactions, where, among other particulars, some short Observations were set down touching both the Shadow of one of Jupiter's Satellites, passing over his Body, and that Permanent Spot, which manifests the Conversion of that Planet about his own Axis; there is come to hand an Extract of that Letter, which was written from Rome, about those Discoveries, containing an ample and particular Relation of them, as they were made by the Learned Caffini, Professor of Astronomy in the University of Bononia. That Extract, as it is found in the French Journal des Scavans of Febr. 22. 1666. we thus English.

Monsieur Caffini, after he had discovered (by the means of those Excellent Glasses of 50 palmer, or 35 feet, made by M. Campani) the Shadows, cast by the 4 Moons or Satellites of Jupiter upon his Diske, when they happen to be between the Sun and Him; after he had also distinguished their Bodies upon the Diske of Jupiter; made the last year some Predictions for the Months of August and September, noting the days and hours, when the Bodies of the said Satellites and their Shadows should appear upon Jupiter, to the end that the Curious might be convinced of this matter by their own Observations.

Some of these Predictions have been verified not only at Rome, and in other places of Italy, but also at Paris by M. Augout, the most Celebrated and the most Exact of our Astronomers; and in Holland, by M. Huygens. And we can now doubt no longer, of the rotation of the Satellites about Jupiter, as the Moon turns about the Earth; nor believe, that Jupiter or his Attendants have any other Light, than that, which they receive from the Sun; as some did a-
Sure before these Observations. There remained to find by Experience, whether Jupiter did turn about his Axis, as many believe, that the Earth turns about her’s. And although most Astronomers had conjectur’d, it did so, either by this Analogy, or by other Congruities; yet it was much with’d, that we might be assured thereof by Observations. And this it is, for which we are obliged to M. Caffini, who, having by the advantage of the same Glasses discover’d several changes, as well in the three obscure Belts, commonly seen in Jupiter, as in the rest of his Diske, and having also observed Spots in the midst of that Planet, and sometimes Brightnesses, such as have bin formerly seen in the Sun, hath at length discover’d a Permanent Spot in the Northern part of the most Southern Belt; by the means whereof, he hath concluded, that Jupiter turns about his Axis in 9. days, 56. minutes, and makes 29. whole circumvolutions in 12. days 4. minutes of ours, and 360. in 149. days. For he has found, that this Spot was not caused by the Shadow of any Satellit, as well by reason of its Situation; as because it appeared, when there could be no Shadow. Besides, that its motion differed from that of the Shadows, which is almost equal, as well towards the Edges as towards the Middle of Jupiter: Whereas, on the contrary, this Spot hath all the accidents, that must happen to a thing, which is upon the surface of a round Body moving; for example, to move much more slowly towards the Edges, than towards the Middle, and to pass over that part, which is in the middle of the Diske, equal to the half of the Diameter, in the sixth part of the time, it takes to make the whole revolution; he having seen this half pass’d over, in 99 or 100 minutes just, as it must happen, supposing the whole circumrotation is made in 9. hours 56. minutes.

He hath not yet been able to determine the Situation of the Axis, upon which this motion is made; because the Belts, according to which it is made, have for some years appeared straight, though in the precedent years, other Astronomers have seen them a little crooked: Which sheweth, that the Axis of the diurnal motion of Jupiter is a little inclined to the plain of the Ecliptick. But in time we may discover, what certainty there is in this matter.

After this excellent Discovery, he hath calculated many Tables, whereof he gives the Explication and Use in the Letters by him addressed to the Abbot Falconieri. By the means of them, one may know, when this Spot may be seen by us: For, having first considered
fidered it in relation to the Sun, in respect whereof, its motion is regular, he considers the same in relation to the Earth, where we observe it; and shews by the means of his Tables, what is to be added or substracted, to know, at what time the said Spot is to come into the middle of Jupiter's Diske, according as he is Oriental or Occidental. He hath also considered it in relation to an unmovable point, which he has supposed to be the first point of Aries, because we thither refer here upon Earth the beginning of all the Celestial motions, and there is the Primum mobile, that one would imagine, if we were in Jupiter, as we do here imagine Ours of 24 hours.

The Discovery is one of the best, that have been yet made in the Heavens; and those, that hold the Motion of the earth, find in it a full Analogy. For, Jupiter turning about the Sun, does nevertheless turn about his Axis; and although he be much bigger than the Earth, he does nevertheless turn much more swiftly than it, since he makes more than two Turns, and a third part, for its one; and carries with him 4 Moons, as the Earth does one.

This Observation ought to excite all Curious persons to endeavour the perfecting of Optick Glasses, to the end that it may be discovered, whether the other Planets, as Mars, Venus and Mercury, about whom no Moon hath as yet been discovered, do yet turn about their Axes, and in how much time they do so; especially Mars, in whom some Spot is discover'd, and Venus, wherein Mr. Burattini hath signified from Poland, he has observ'd Inequalities, as in the Moon.

It will be worth while, to watch for the seeing of Jupiter again this Spring, that this happy Observation may be confirmed in divers places, and endeavours used to make new ones.

An Account of some Books, lately published.

I. Hydrostatical Paradoxes, made out by New Experiments (for the most part Physical; and Easie) by the Honourable Robert Boyle. This Treatise promised in Numb. 8. of these Papers, is now come forth: And was occasioned by the perusall of the Learned Monsieur Paschalls Tract, of the Equilibrium of Liquors, and of the Weight of the Air: Of which two Subjects, the latter having been more clearly made out in England by Experiments, which could not be made by Monsieur Paschal and others, that wanted the advantage of such Engines and Instruments, as have here been frequently made use

A a 2

off;
Our Noble Author insists most upon giving us his thoughts of the former, *vide* the *Equilibrium of Liquors: Which Discourse consisting partly of Conclusions; and partly of Experiments, the former seem to Him, to be almost all of them consonant to the Principles and Laws of the *Hydrostaticks*; but as for the latter, the Experimental proofs, offered by M. Pasthall for his Opinions, are by our Author esteemed such, that he confesses, he hath no mind to make use of them: for which he alleges more reasons than one; which, doubtless, will appear very satisfactory to Intelligent Readers.

Wherefore, instead of those Pasthallian Experiments, there is in this Treatise deliver'd a far more Expeditious way to make out, not only most of the Conclusions, agreed on by these two Authors, but others also, that M. Pasthall mentions not; and that with so much more ease and clearness, that persons, but ordinarily versed in the common Principles of *Hydrostaticks*, may readily apprehend, what is deliver'd, if they will but bring with them a due Attention, and Minds disposed to prefer Reason and Experience to Vulgar opinions and Authors.

It not being our Author's present Task, to deliver a Body of *Hydrostaticks*, but only some Paradoxes, which he conceives to be proveable by his New way of making them out, he delivers them in as many distinct Propostitions; after each of which, he endeavours, in a Proof, or an Explication, to show, both that it is true, and why it ought to be so.

The Paradoxes themselves (after a premised *Postulatum*) are these:

1. That in Water, and other Fluids, the Lower parts are pressed by the Upper.
2. That a lighter Fluid may gravitate or weigh upon a heavier.
3. That, if a Body, contiguous to the Water, be altogether, or in part, lower than the highest level of the said Water, the lower part of the Body will be pressed upward by the Water, that touches it beneath.
4. That in the Ascension of Water in Pumps, &c. there needs nothing to raise the Water, but a Competent weight of an External Fluid.
5. That the pressure of an External Fluid is able to keep an Heterogeneous Liquor suspended at the same height in several Pipes, though these Pipes be of very different Diameters.
6. If a Body be placed under Water, with its uppermost Surface parallel to the Horizon; how much Water soever there may be on this or that side above the Body, the direct pressure sustained by the Body (for we now consider not the Lateral nor the Receding pressure, to which the Body may be exposed, if quite environed with Water) is no more than that of a Column of water, having the Horizontal Superficies of the Body for its Basis, and the Perpendicular depth of the Water for its height.

And so likewise,

If the Water, that leans upon the Body, be contained in Pipes open at both ends; the pressure of the Water is to be estimated by the weight of a pillar of Water, whose Basis is equal to the lower Orifice of the Pipe (which we suppose to be parallel to the Horizon) and its height equal to a perpendicular, reaching thence to the top of the Water; though the Pipe be much inclined towards the Horizon, or though it be irregularly shap’d, and much broader in some parts, than the said Orifice.

7. That a Body immersed in a Fluid sustains a Lateral pressure from the Fluid; and that increased, as the depth of the immersed Body, beneath the Surface of the Fluid, increaseth.

8. That Water may be made as well to depress a Body lighter than itself, as to buoy it up.

9. That, whatever is said of Positive Levity, a parcel of Oyl lighter than Water, may be kept in Water without ascending in it.

10. That the cause of the Ascension of Water in Syphons, and of its flowing through them, may be explicated without having a recourse to Nature’s abhorrensy of a Vacuum.

11. That a Solid Body, as ponderous as any yet known, though near the Top of the water it will sink by its own weight; yet if it be placed at a greater depth, than that of twenty times its own thickness, it will not sink, if its descent be not assisted by the weight of the incumbent Water.

These are the Paradoxes, evinced by our Author, with much evidence and exactness, and very likely to invite Ingenious men to cultivate and to make further disquisitions in so excellent a part of Philosophy, as are the Hydrostaticks; an Art deserving great Elogiums, not only, upon the account of its Theorems and Problems, which are most of them pure and handsome productions of Reason, very delightful and divers of them surprizing; and besides, much conducing to the clear explication and thorough understanding,
ing of many both familiar and abstruse Phenomena of Nature; but
also, upon the score of its Practical use, since the Propositions, it
reaches, may be of great importance to Navigation, and to those
that inquire into the Magnitudes and Gravities of Bodies, as also
to them, that deal in Salt-works: Besides, that the Hydrostaticks
may be made divers waies serviceable to Chymists, as the Author
intimates, and intends to make manifest, upon several occasions, in
his yet unpublish'd part of the Usefulness of Natural and Experimen-
tal Philosophy.

These Propositions are shut up by two important Appendixes,
whereof the one contains an Answer to seven Objections by a late
learned Writer, to evince, that the upper parts of water pres not
upon the lower; the other solves that difficult problem, why Urina-
tors or Divers, and others, who descend to the bottom of the Sea,
are not oppressed with the weight of the incumbent water? where,
among other solutions, that is examined, which occurs in a printed
Letter of Monsieur des Cartes, but is found unsatisfactory.

I I. Nicolai Stenonis de Musculis & Glandulis Observationum Speci-
men; cum duas Epistolis Anatomicis. In the specimen itself, the Au-
thor, having described in general, both the Structure and the Function
of the Muscles, applies that description to the Heart, to demonstrate
that that is also a true Muscle: Observing first, that in the substance of
the Heart there appears nothing but Arteries, Veins, Nerves, Fibres,
Membranes; and that that, & nothing else is found in a Muscle; affir-
m ing with all, that which is commonly taught of the Muscles, and
particularly of the Heart's Parenchyma, as distinct from Fibres, is
due, not to the Senses, but the Wit of Anatomists: so that he will not
have the Heart made up of a substance peculiar to itself, nor con-
sidered as the principle of Innate heat, or of Sanguification, or of vi-
tal spirits. He observes next, that the Heart performs the like opera-
tion with the Muscles, to wit, to contract the Flesh; which action
how it can have a different cause from that of the Contraction
made in the Muscles', where there is so great a parity and agree-
ment in the Veins, he sees not. And as for the Phenomena, that
occur, of the Motion of the Heart, he undertakes to explicate them
all, from the Duibus or Position of the Fibres; but refers for the per-
formance of this undertaking to another Treatise, he intends to
publish.

As to his Observations about Glandulis, he affirms, that he has
been the First, that has discover'd that Vessels, which by him is call'd
Salivare,
Salivare Externis, passing from the Parotides (or the two chief Arteries that are on the right and left side near the Throat) into the Mouth, and conveying the Spittle: Where he also gives an account of several other Vessels and Glanduls, some about the Lips; others under the Tongue; others in the Palate &c. To which he adds the Vessels of the Eye-lids, which have their root in the Glanduls that are about the Eyes, and serve for the shedding of Tears. He mentions also several things about the Lymphatrick vessels, and is of opinion, that the knowledge thereof may be much illustrated by that kind of Glanduls that are called Conglobate, and by their true insertion into the veins; the mistake of the latter whereof, he conceives to have very much misled the Noble Ludo vicius de Bills, notwithstanding his excellent method of dissection. And here he observes first, that all the Lymphatrick vessels have such a commerce with the Glanduls, that none of them is found in the body, which either has not its origine from, or is insetted into a Glandule: And then, that Glanduls are a kind of Strainers, so form’d, that whilst the Blood passes out of the Arteries into the Veins through the small Capillary vessels, the Serous parts thereof, being freed from the Sanguineous, are by virtue of the heat expell’d through fit pores into the Capilaries of the Lymphatics; the direction of the Nerves concurring.

Of the two annex’d Epistles, the First gives an account of the dissection of two Raja’s or Skates, and relates that the Author found in the bellies of these Fishes a Haddock of 1 1/2 span long, and a Sole, a Plaice, and nine middle-sized Sea-craFishes, whereof not only the three former had their flesh, in the fishes stomack, turn’d into a fluid, and the Gristles or Bones into a soft substance, but the CraFishes had their shells comminuted into very small particles, tinging here and there the Chyle near the Pylorus; which he judges to be done not so much by the heat of the Fishes stomack, as by the help of some digesting juice. Coming to the Uterus of these Fishes, he takes occasion to examine, with what ground several famous Naturalists and Anatomists have affirm’d, that Eggs are the uterus expos’d or ejected out of the body of the Animal. Taking a view of their Heart, he there finds but one ventricle, and discourses of the difficulty arising from thence. As for the Lungs, he saw no clearer footsteps of them in these, than he had done in other Fishes: but within the mouth he trac’d several gaping fissures, and found the recesses of the Gills so form’d, that the water taken in at the mouth, being let out by these nores, cannot by them re-enter, by reason of a skin, outwardly pass’d over every hole, and covering it. Where he intimates, that though Fishes have not true Lungs, yet they want not a Succedaneous thereto; to wit, the Gills; and if water may be to Fishes, what Air is to terrestrial Animals, for Respiration: assert’ing, that whereas nothing is so necessary for the conservation of Animal life, as a reciprocal A ceifs and Reccefs of the Ambient to the sanguineous vessels, this is to one, whether that be done by receiving the Ambient within the body, or by its gentle passing by the prominent vessels of the Gills.

The other Epistle contains some Ingenious Observations, touching the way, by which the Chicken, yet in the shell, is nourish’d, videlicet, not by the conveyance of the Yolk into the Liver by the Umbilical vessels, nor into the Stomach by the

Mouth,
Mouth, but by a peculiar ductus, by him described, into the Intestins, where, according to his alleged experience, it is turn'd into Chyle: which he affirms, he hath discover'd, by taking an Egg from under a brooding Hen, when the Chicken was ready to break forth, and when he was looking for the passage of the Yolk, out of its integument into the Liver, by finding it pass thence into the Intestins, as he found the white to do by the mouth into the belly. Whence he inclines to infer, that since every fæces takes in at the mouth the liquor it swims in, and since the Chicken receives the white of the Egg into the mouth, and the yolk by the new discover'd ductus into the Intestins, it cannot be certainly made out, that a part of the Chyle is conveyed into the Liver, before it passeth into the Heart: Exhorting in the mean time the Patrons of the Liver, that they would produce Experiments to evince their Ratiocinations.

III. Regneri de Graeff, de Succi Pancreatici Natura & usu, Exercitatio Anatomico-medica. In this Tract, the Indulgentious Author, after he has enumerated the various opinions of Anatomists concerning the use of that kernelly substance, call'd Pancreas (in English, the Sweetbread) endeavours to prove experimentally that this Glandule was not form'd by Nature, to separate any Excrementitious humor, and to convey it into the Intestins, but to prepare an useful juice out of the Blood and Animal spirits, of a somewhat Acid taste, and to carry the same into the Gut, call'd Duodenum, to be there mixt with the Aliment, that has been in some degree already fermented in the Stomach, for a further fermentation, to be produced by the conflux of the said acid Pancreatic juice and some Bilious matter, abounding with volatile Salt, causing an Effervescence; which done, that juice is, together with the purer part of the nourishment, carried into the Milkie veins, thence into the common receptacle of the Chyle and Lymphatic liquor, and so through the ductus Thoracicus into the right Ventricle of the Heart.

This Assertion first advanced (faith the Author) partly by Gothofredus Mebius, partly by Franciscus de le Boe Sylvius, he undertakes to prove by experiments; which, indeed, he has with much industry, tried upon several Animals, to the end that he might collect some of this juice of the Pancreas for a taste: which having at last obtained, and found it somewhat acid, he thereupon proceeds to deliver his opinion both of the constitution and quantity of this Succe in healthy Animals, and the vices thereof, in the unhealthy: deriving most diseases partly from its too great Acidity, or from its saltiness, or harshness; partly from its paucity or redundancy: but especially, endeavouring to reduce from thence, as all intermittent Fevers (of all the Phenomena whereof he ventures to assign the causes from this Hypothesis) to also the Gout, Syncope's, Stranguries, Oppilations, Diarrhæas, Dysenterie, Hysterical and Colick passions, &c. All which he concludes with mentioning the ways and remedies to cure the manifold peccancy of this juice by Evacuations and Alterations:

This seeming to be a new as well as a considerable discovery, it is hop'd, that others will by this intimation be invited to prosecute the same by further experiments, either to confirm what this Author has started, if true, or to rectifie it, if he be mistaken.

NOTE.

In Fig. 1. of Num. 9. of these Tracts, the Graver hath placed the bended end of the springing Wire C F, above the Wire-staple B, between it and the Ring E, of the weight D, whereas that end should have been so express'd, as to pass under the Wire-staple, betwixt its two Wires, into the said Ring.

PHILOSOPHICAL TRANSACTIO NS.

Munday, April 2. 1666.

The Contents.

A Confirmation of the former Account touching the late Earth-quake near Oxford, and the Concomitants thereof, by Mr. Boyle. Some Observations and Directions about the Barometer, communicated by the same Hand. General Heads for a Natural History of a Country, small or great, proposed by the same. An Extra of a Letter, written from Holland, about Preserving Ships from being Worm-eaten. An Account of Mr. Boyle's lately publish'd Treat, entituled, The Origine of Forms and Qualities, illustrated by Considerations and Experiments.

A Confirmation of the former Account touching the late Earth-quake near Oxford, and the Concomitants thereof.

His Confirmation came from the Noble Mr. Boyle in a Letter, to the Publisher, as followeth:

As to the Earth-quake, your curiosity about it makes me sorry, that, though I think, I was the first, that gave notice of it to several of the Virtuosi at Oxford; yet the Account, that I can send you about it, is not so much of the Thing itself,
as of the Changes of the Air, that accompanied it. To inform you of which, I must relate to you, that riding one Evening some-
what late betwixt Oxford & a Lodging, I have at a place, 4 miles distant from it, the weather having been for a pretty while Frofty, I found the Wind so very cold, that it reduced me to put on some defensives against it, which I never since, nor, if I forget not, all the foregoing part of the Winter was obliged to make use of. My unwillingness to stay long in so troublesome a Cold, which continued very piercing, till I had got half way home-ward, did put me upon galloping at no very lazy rate; and yet, before I could get to my Lodgings, I found the Wind turned, and felt the Rain falling; which, considering the shortness of the time, and that this Accident was preceded by a settled Frost, was surprising to me, and induced me to mention it at my return, as one of the greatest and suddainest Alterations of Air, I had ever observ'd: And what changes I found, have been taken notice of in the Gravity of the At-
mosphere at the same time by that Accurate Observer * Dr: Wallis, who then suspected nothing of what follow'd; as I suppose, he has ere this told you himself. Soon after, by my guess about an hour, there was a manifest Trembling in the House where I was (which stands high in comparison of Oxford.) But it was not there so great, but that I, who chance'd to have my thoughts busied enough on other matters, than the weather, should not have taken notice of it as an Earth-quake, but have imputed it to some other cause, if one, that you know, whose hand is employed in this Paper, and begins to be a diligent observer of Natural things, had not advertis'd me of it; as being taken notice of by him and the rest of the people of the House. And soon after there hapned a brisk Storm: whereupon I sent to make inquiry at a place call'd Brill, which standing upon a much higher ground, I suppos'd might be more obnoxious to the effects of the Earth-quake (of which, had I had any suspicion of it, my hav-
ing formerly been in one near the Lacus Lemanus, would have made me the more observant:) But the person I sent to, being disabled
disabled by sickness to come over to me (which he promised to do, as soon as he could) wrote me only a Ticket, whose substance was. That the Earthquake was there much more considerable, than where I lodged, and that at a Gentleman's house, whom he names (the most noted Person, it seems, of the neighbourhood) the House trembled very much, so as to make the Stones manifestly to move to and fro in the Parlour, to the great amazement and fright of all the Family. The Hill, whereon this Brill stands, I have observ'd to be very well stor'd with Mineral substances of several kinds; and from thence I have been inform'd by others, that this Earthquake reach'd a good many miles, but I have neither leisure, nor inclination to entertain you with uncertain reports of the Extent and other Circumstances, especially since a little further time an inquiry may enable me to give you a better warranted account.

Some Observations and Directions about the Barometer, communicated by the same Hand, to the Author of this Tract.

These shall be set down, as they came to hand in another Letter; vide l. As to the Barometrical Observations (as for brevities sake I use to call them) though you * guessed aright, that, when I saw those of the Learned and Inquisitive Dr. Beale, I had not Mine by me, (for I left them, some years since, in the hands of a Virtuoso, nor have I now the leisure to look after those Papers;) yet since by the Communication, you have made publick, 'tis probable, that divers Ingenious men will be invited to attempt the like Observations, I shall (notwithstanding my present haste) mention to you some particulars, which perhaps will not appear unseasonable, that came into my mind upon the reading of what you have presented the Curious.

When I did, as you may remember, some years agoe, publickly express and desire that some Inquisitive men would make
make Baroscopic Observations in several parts of England (if not in foreign Countries also;) and to assist them, to do so, presented some of my Friends with the necessary Instruments: The declared reason of my desiring this Correspondence was (among other things) that by comparing Notes, the Extent of the Atmospheric Changes, in point of Weight, might be the better estimated. But not having hitherto received some account, that I hoped for, I shall now, without staying for them, intimate thus much to you: That it will be very convenient, that the Observers take notice not only of the day, but, as near as they can, of the Hour wherein the height of the Mercurial Cylinder is observ'd: For I have often found, that within less than the compass of one day, or perhaps half a day, the Altitude of it has so considerably vary'd, as to make it in many cases difficult, to conclude any thing certainly from Observations, that agree but in the day.

It will be requisite also, that the Observers give notice of the Situation of the place, where their Barometers stand, not only, because it will assist men to Judge, whether the Instruments were duly perfected, but principally, because, that though the Baroscope be good (nay, because it is so) the Observations will much disagree, even when the Atmosphere is in the same state, as to Weight, if one of the Instruments stand in a considerably higher part of the Countrey, than the other.

To confirm both the foregoing admonitions, I must now inform you, that, having in these parts two Lodgings, the one at Oxford, which you know stands in a bottom by the Thames side, and the other at a place, four miles thence, set upon a moderate Hill, I found, by comparing two Baroscopes, that I made, the one at Oxford, the other at Stanton St. Johns, that, though the former be very good, and have been noted for such, during some years, and the latter was very carefully fill'd; yet by reason, that in the Higher place, the incumbent part of the Atmosphere must be lighter, than in the Lower, there is almost al
ways between 2 and 3 Eights of an Inch difference betwixt them. And having sometimes order’d my servants to take notice of the Dilpacity, and divers times carefully observ’d it my self, when I pass’d to and fro between Oxford and Stanton, I generally found, that the Oxford Barometer and the other, did, as it were by common consent, rise and fall together so, as that in the former the Mercury was usually higher, than in the latter.

Which Observations may teach us, that the Subterraneous Steam, which ascend into the Air, or the other Causes of the varying Weight of the Atmosphere, do, many times, and at least in some places, uniformly enough affect the Air to a greater height, than, till I had made this tryall, I durst conclude.

But, as most of the Barometrical observations are subject to exception, so I found the formerly mentioned to be. For (to omit lesser variations) riding one Evening from Oxford to Stanton, and having, before I took horse; look’t on the Baroscope in the former of these 2 places, I was somewhat surpris’d, to find at my coming to the latter, that in places no farther distant, and notwithstanding the shortness of the time (which was but an hour and a half, if so much) the Barometer at Stanton was short of its usual distance from the other, near a quarter of an Inch, though, the weather being fair and calm, there appear’d nothing of manifest change in the Air, to which I could ascribe so great a Variation; and though also, since that time, the Mercury in the two Instruments hath, for the most part, proceeded to rise and fall as before.

And these being the only Observations, I have yet met with, wherein Baroscopes, at some Distance of Place, and Difference of Height, have been compar’d (though I cannot now send you the Reflexions, I have elsewhere made upon them;) as the opportunity I had to make them my self, rendred them not unpleasant to me, so perhaps the Novelty will keep them from being unwelcome to you. And I confess, I have had some flying suspicions, that the odd Phenomena of the Baroscope, which have hitherto more pos’d, than instructed us, may in time, if a
competent number of Correspondents do diligently prosecute the Inquiries (especially with Baroscopes, accommodated with Mr. Hooks ingenious additions) make men some Luciferous discoveries, that possibly we do not yet dream off.

I know not, whether it will be worth while to add, that since I was oblig'd to leave London, I have been put upon so many lesser removes, that I have not been able to make Baroscopical Observations with such a constancy, as I have wished, but, as far as I remember, the Quick-silver has been for the most part, so high, as to invite me to take notice of it; and to desire you to do me the favour to inquire among your correspondents whether they have observ'd the same thing. * For, if they have, this lastling (though not uninterrupted) Altitude of the Quick-silver, happening, when the Seasons of the year have been extraordinary dry (so much as to become a grievance, and to dry up, as one of the late Gazettes informs us, some springs near Waymouth, that used to run constantly) it may be worth inquiry, whether these obstinate Droughts, may not by cleaving of the ground too deep, and making it also in some places more porous and as it were, spongy, give a more copious Vent, than is usual, to subterraneal streams, which ascending into the Air, increase the gravity of it. The inducements I have to propose this inquiry, I must not now stay to mention. But perhaps, if the Observation holds, it may prove not useless in reference to some Diseases.

Perhaps it will be needful to put you in mind of directing those Virtuosi, that may desire your Instructions about Baroscopes, to set down in their Diaries not only the day of the month, and the hour of the day, when the Mercuries height is taken, but (in a distinct Column) the weather, especially the Winds, both as to the Quarters, whence they blow (though that be not always so easy nor necessary,) and as to the Violence or Remisness, wherewith they blow. For, though it be more difficult, than

* This hath been inquired into, and is found, that several Accurate and Curious persons (as the Most Noble President of the Royal Society, the Lord Viscoun Broukner, Doctor Beale, Mr. Hook &c.) have observed the same.
than one would think, to settle any general rule about the rising and falling of the quicksilver; yet in these parts one of those, that seem to hold offneff, is,

* that when high winds blow, the mercury is the lower; and yet that it self does sometimes fail: For, this very day (March 3.) though on that hill, where I am, the somewhat westerly winds have been blustering enough, yet even since morning the quicksilver has been rising, and is now risen near \( \frac{3}{4} \) of an inch.

I had thoughts to add something about another kind of baroscope (but inferior to that in use) whereof I have given some intimation in one of the Preliminaries to the History of cold. But you have already too much of a letter, and my occasions, &c.

So far that letter. Since which time, another from the same noble observer intimates, that, as for that cause of the height of the quicksilver in droughts, which by him is suspected to be the elevation of streams from the crust or superficial parts of the earth, which by little and little may add to the weight of the atmosphere, being not, as in other seasons, carried down from time to time by the falling rain, it agrees not ill with what he has had since occasion to observe. For, whereas about March 12th, at Oxford, the quicksilver was higher, than, for ought he knew, had been yet observed in England, viz. above \( \frac{3}{4} \) above 30. inches, upon the first considerable showers, that have interrupted our long drought, as he affirms, he foretold divers hours before that the quicksilver would be very low, (a blustering wind concurring with the rain) so he found it at Stanton to fall \( \frac{3}{4} \) beneath 29. inches.*

* Dr. Beale concurs with this observation, when he saith, in a late letter of March 19. to his correspondent in London; By change of weather and wind, the mercury is sunk more than an inch, since I wrote to you on Monday last, March 12. This last night, by rain and south wind, tis sunk half an inch.
General Heads for a Natural History of a Countrey, Great or small, imparted likewise by Mr. Boyle.

It having been already intimated (Num. 8. of Phil. Transact. p. 140, 141.) that divers Philosophers aim'd, among other things, at the Composing of a good Natural History, to superstruct, in time, a Solid and Useful Philosophy upon; and it being of no slight importance, to be furnished with pertinent Heads, for the direction of Inquirers; that lately named Benefactor to Experimental Philosophy, has been pleased to communicate, for the ends above said, the following Articles, which (as himself did signify) belong to one of his Essays of the unpublish'd part of the Usefulness of Nat. and Experimental Philosophy.

But first, he premises, that what follows, is design'd only to point at the more General heads of Inquiry, which the proposer ignores not to be Divers of them very comprehensive, in so much, that about some of the Subordinate subjects, perhaps too, not the most fertile, he has drawn up Articles of inquisition about particulars, that take up near as much room, as what is here to be deliver'd of this matter.

The Heads themselves follow;

The things, to be observ'd in such a History, may be variously (and almost at pleasure) divided: As, into Supraterraneous, Terrestrial, and Subterraneous; and otherwise: but we will at present distinguish them into those things, that respect the Heavens, or concern the Air, the Water, or the Earth.

1. To the First sort of Particulars, belong the Longitude and Latitude of the Place (that being of moment in reference to the observations about the Air &c.) and consequently the length of the longest and shortest days and nights, the Climate, parallels &c. what fixed stars are and what not seen there: What Constellations 'tis said to be subject to? Whereunto may be added other Astrological matters, if they be thought worth mentioning.

2. About
2. About the Air may be observ'd, its Temperature, as to the first four Qualities (commonly so call'd) and the Measures of them: its Weight, Clearness, Refractive power: its Subtlety or Grossness: its abounding with, or wanting an Eau-vive Salt: its variations according to the seasons of the year, and the times of the day; What duration the several kinds of Weather usually have: What Meteors it is most or least wont to breed; and in what order they are generated; and how long they usually last: Especially, what Winds it is subject to; whether any of them be stated and ordinary, &c. What diseases are Epidemical, that are supposed to flow from the Air: What other diseases, wherein that hath a share, the Country is subject to; the Plague and Contagious sicknesses: What is the usual salubrity or insalubrity of the Air; and with what Constitutions it agrees better or worse, than others.

3. About the Water, may be observ'd, the Sea, its Depth, degree of Saltiness, Tydes, Currents, &c. Next, Rivers, their Bigness, Length, Course, Inundations, Goodness, Levity (or their Contraries) of Waters, &c. Then, Lakes, Ponds, Springs, and especially Mineral waters, their Kinds, Qualities, Vertues, and how examined. To the Waters belong also Fishes, what kinds of them (whether Salt or Fresh-water) are to be found in the Country; their Store, Bigness, Goodness, Seasons, Haunts, Peculiarities of any kind, and the ways of taking them, especially those that are not purely Mechanical.

4. In the Earth, may be observed,

1. It self.
2. Its Inhabitants, and its Productions, and these External, and Internal.

First, in the Earth it self, may be observ'd, its dimensions, situation, East, West, North, and South: its Figure, its Plains, and Valleys, and their Extent: its Hills and Mountains, and the height of the tallest, both in reference to the neighbouring Valleys or Plains, and in reference to the Level of the Sea: As
also, whether the Mountains lie scattered, or in ridges, and whether those run North and South, or East and West, &c. What Promontories, fiery or smoaking Hills, &c. the Country has, or hath not: Whether the Country be coherent, or much broken into Islands. What the Magnetical Declination is in several places, and the Variations of that Declination in the same place (and, if either of those be very considerable, then what circumstances may assist one to guess at the Reason as Subterraneal fires, the Vicinity of Iron-mines, &c.) what the Nature of the Soyle is, whether Clays, Sandy, &c. or good Mould; and what Grains, Fruits, and other Vegetables, do the most naturally agree with it: As also, by what particular Arts and Industries the Inhabitants improve the Advantages, and remedy the Inconveniences of their Soyl: What hidden qualities the Soyl may have (as that of Ireland, against Venemous Beastis, &c.)

Secondly, above the ignoble Productions of the Earth, there must be a careful account given of the Inhabitants themselves, both Natives and Strangers, that have been long settled there: And in particular, their Stature, Shape, Colour, Features, Strength, Agility, Beauty (or the want of it) Complexions, Hair, Dyet, Inclinations, and Customs that seem not due to Education. As to their Women (besides the other things) may be observed their Fruitfulness or Barrenness; their hard or easy Labour, &c. And both in Women and Men must be taken notice of what diseases they are subject to, and in these whether there be any symptome, or any other Circumstance, that is usual and remarkable.

As to the External Productions of the Earth, the Inquiries may be such as these: What Grasses, Grains, Herbs, (Garden and Wild) Flowers, Fruit-trees, Timber-trees (especially any Trees, whose wood is considerable) Coppices, Groves, Woods, Forrefts, &c. the Country has or wants: What peculiarities are observable in any of them: What Soyles they most like or dislike; and with what Culture they thrive best. What Animals the Country has or wants, both as to wild Beasts, Hawks, and other Birds of Prey; and as to Poultery, and Cattle.
Cattle of all sorts, and particularly, whether it have any Animals, that are not common, or any thing, that is peculiar in those, that are so.

The Internal Productions or Concealments of the Earth are here understood to be, the riches that lie hid under the Ground, and are not already refer'd to other Inquiries.

Among these Subterraneous observations may be taken notice of, what sorts of Minerals of any kind they want, as well as what they have; Then, what Quarries the Country affords, and the particular conditions both of the Quarries and the Stones: As also, how the Beds of Stone lye, in reference to North and South, &c. What Clays and Earths it affords, as Tobacco-pipe-clay, Marles, Fullers-earths, Earths for Potters wares, Bolus's and other medicated Earths: What other Minerals it yields, as Coals, Salt-Mines, or Salt-springs, Allom, Vitrial, Sulphur, &c. What Mettals the Country yields, and a description of the Mines, their number, situation, depth, signs, waters, damps, quantities of ore, goodness of ore, extraneous things and ways of reducing their ores into Mettals, &c.

To these General Articles of inquiries (faith their Proposer) should be added; 1 Inquiries about Traditions concerning all particular things, relating to that Country, as either peculiar to it, or at least, uncommon elsewhere. 2 Inquiries, that require Learning or Skill in the Answerer: to which should be subjoined Proposals of ways, to enable men to give Answers to these more difficult inquiries.

Thus far our Author, who, as he has been pleased to impart these General (but yet very Comprehensive and greatly Directive) Articles; so, 'tis hoped from his own late intimation, that he will shortly enlarge them with Particular and Subordinate ones. These, in the mean time, were thought fit to be publish'd, that the Inquisitive and Curious, might, by such an Assistance, be invited not to delay their searches of matters, that are so highly conducive to the improvement of True Philosophy, and the well-fare of Mankind.
An Extract
Of a Letter, Written from Holland, about Preserving of Ships from being Worm-eaten.

This Extract is borrowed from the French journal des Scavans of Febr. 15. 1666. and is here inserted, to excite Inventive heads here, to overtake the Proposer in Holland. The letter runs thus:

Although you have visited our Port (Amsterdam) I know not whether you have noted the ill condition, our ships are in, that return from the Indies. There is in those Seas a kind of small worms, that fasten themselves to the Timber of the ships, and so pierce them, that they take water everywhere; or if they do not altogether pierce them thorow, they so weaken the wood, that it is almost impossible to repair them. We have at present a Man here, that pretends to have found an admirable secret to remedy this evil. That, which would render this secret the more important, is, that hitherto very many ways have been used to effect it, but without success. Some have employed Deal, Hair and Lime, &c. and therewith lined their ships, but besides that this does not altogether affright the worms, it retards much the ships Course. The Portugals scorch their ships, insomuch that in the quick works there is made a coaly crust of about an Inch thick. But as this is dangerous, it happening not seldom, that the whole vessel is burnt; so the reason why worms eat not thorow Portuguese ships, is conceived to be the exceeding hardness of the Timber, employed by them.

We expect with impatience the nature and effect of this Proposition. Many have already ventur’d to give their thoughts concerning it. Some say, there needs no more, but to build ships of a harder kind of Wood, than the usual. Others having observed, that these Worms fasten not to a kind of wild Indian Pear-tree, which is highly bitter, do thereupon suggest,
gest, that the best Expedient would be, to find out a Wood having that quality. But certainly there being now no Timber, fit for Ships, that is not known, tis not likely that any will be found either more hard, or more bitter, than that, which has been hitherto employed. Some do imagine, that the Proposer will, by certain Lixiviums, give to the ordinary Wood such a quality and bitterness, as is found in the already mentioned Indian Pear-tree. But this also will hardly succeed, since it will be requisite not only to make Lixiviums, in great quantities at an easie rate, and strong enough to penetrate the thick sides of a Ship, but also to make them durable enough, not to be wash't out by the Sea. Yet notwithstanding, in these matters one ought to suspend on's judgement, untill experience do shew, what is to be believed of them.

So far the Extract. To which it may perhaps not be unseasonable to add, that a very worthy person in London, suggests the Pitch, drawn out of Sea coles, for a good Remedy to scare away these noysome insects.

An Account

Of a Book, very lately publish't, entituled, The Origine of Forms and Qualities, illustrated by Considerations and Experiments, by the Honourable Robert Boyle.

This Curious and Excellent Piece, is a kind of Introduction to the Principles of the Mechanical Philosophy, explicating, by very Considerable Observations and Experiments, what may be, according to such Principles, conceived of the Nature and Origine of Qualities and Forms; the knowledge whereof, either makes or supposes the Fundamental and Useful part of Natural Philosophy. In doing of which, the Author, to have his way the clearer, writes rather for the Corpuscularian Philosophers (as he is pleased to call them) in General, than any Party.
Party of them, keeping himself thereby disengaged from adopting an Hypothesis, in which perhaps he is not so thoroughly satisfied, and of which he does not conceive himself to be necessitated to make use here; and accordingly forbearing to employ Arguments, that are either grounded on, or suppose Atoms, or any Innate Motion belonging to them; or that the Essence of Bodies consists in Extension; or that a Vacuum is impossible; or that there are such Globuli Celestes, or such a Materia Subtilis, as the Cartesians imploy to explicate most of the Phenomena of Nature.

The Treatise consisting of a Speculative, and an Historical part, the Author, with great modesty leaves the Reader to judge; Whether in the First part he hath treated of the Nature and Origin of Forms and Qualities in a more Comprehensive way, than others; Whether he has by fit Examples, and other means, rendered it more intelligible, than they have done: Whether he has added any considerable number of Notions and Arguments towards the compleating and confirming of the proposed Hypothesis: Whether he has with reason dismissed Arguments unfit to be relied on; and Whether he has proposed some Notions and Arguments so warily, as to keep them from being liable to Exceptions and Evasions, whereto they were obnoxious, as others have proposed them. And, as to the Second and Historical part, he is enclin'd to believe, that the Reader will grant, he hath done that part of Physicks, he is treating of, some service, by strengthening the doctrines of the New Philosophy (as 'tis call'd) by such particular Experiments, whose Nature and Novelty will render them as well Acceptable as Instructive.

The summe of the Hypothesis, fully and clearly explicated in the First Part, is this;

That all Bodies are made of one Catholic matter, common to them all, and differ but in Shape, Size, Motion or Rest, and Texture of the small parts, they consist off; from which Affec-
tions of Matter, the Qualites, that difference particular Bodies, result: whence it may be rationally concluded, that one kind of Bodies may be transmuted into another; that being in effect no more, than that one Parcel of the Universal Matter, wherein all Bodies agree, may have a Texture produced in it, like the Texture of some other Parcel of Matter, common to them both.

To this Hypothesis, is subjoin'd an Examination of the Scholastick opinion of Substantial Forms: where the Author, First, States the Controversie: next, gives the Principal reasons, that move him to oppose that Opinion; then, answers the Main arguments employed to evince it; further, assigns both the First Cause of Forms (God:) and the Grand Second Cause thereof (Local Motion:) and, lastly, proves the Mechanical Production of Forms: grounding his proof, partly upon the Manner, by which such a Convention of Accidents, as deserve to pass for a Form, may be produced: as that the Curious Shapes of Salts (believed to be the admirablest Effects and strongest Proofs of Substantial Forms) may be the Results of Texture; Art being able to produce Vitriol as well as Nature: partly, upon the possibility of Reproducing Bodies by skill, that have been deprived of their reputed Substantial Forms: Where he alleges the Redintegration of Saltpetre, succensfully performed by himself; though his Attempts, made upon the dissipation and re-union of Amber, Album, Sea-Salt, and Vitriol, proved (by reason of accidental hindrances rather, than of any impossibility in the Nature of the Thing) less successful.

In the Second and Historical Part, the Author, appealing to the Testimony of Nature, to verifie his Doctrine, sets down, both some Observations, of what Nature does without being over-ruled by the power and skill of man; and some Experiments, wherein Nature is guided, and as it were, mattered by Art,

The Observations are four: I. The First is taken from what happens in the Hatching of
an Egg; out of the White whereof, which is a Sub stance Similar, insipid, soft, diaphanous, colourless, and readily dissoluble in cold water, there is by the New and Various contrivement of its small parts, caused by the Incubation of the Hen, an Animal produced, some of whose parts are opa- cious, some red, some yellow, some white, some fluid, some consistent, some solid and frangible, others tough and flexible, some well, some ill-tasted, some with springs, some without springs, &c.

2. The Second is fetched from Water, which being fluid, tasteless, inodorous, diaphanous, colourless, volatile, &c. may by a Differing Texture of its parts, be brought to constitute Bodies, having qualities very distant from these; as Vegetables, that have firmness, opacity, odors, tastes, colours, Medicinal virtues; yielding also a true Oyle, that refuses to mingle with Water, &c.

3. The Third, from Inoculation; wherein, a small Bud is able so to transmute all the sap, that arrives at it, as to make it constitute a Fruit quite otherwise qualified, than that, which is the genuine production of the Tree, so that the same sap, that in one part of the Branch constitutes (for Instance) a Cluster of Haws, in another part of the same Branch, may make a Pear. Where the Author mentions divers other very considerable Effects of Inoculations, and inserts several Histories, all countenancing his doctrine.

4. The Fourth, from Putrified Cheese; wherein, the rotten part, by the alteration of its Texture, will differ from the Sound, in colour, odor, taste, consistency, vermination, &c.

The Experiments are ten.

1. A Solution of Vitriol and Camphire; in which by a change of Texture, appeared the Production of a deep colour from a white
white Body, and a clear Liquor without any external heat: The destruction of this Colour, by adding only some fair water: The change of an Odorous Body, as Camphire, into an Inodorous, by mixing it with a Body, that has scarce any sensible odour of its own: The sudden restauration of the Camphire to its native scent and other qualities, by common water, &c.

2. Sublimate, distill'd from Copper and Silver, which both did wholly loose their Metalline forms, and were melted into brittle lumps, with colours quite differing from their own; both apt to imbibe the moisture of the Air, &c.

3. A solution of silver into Luna Cornea: Whereby the opacous, malleable and hardly fusible Body of Silver, was, by the addition of a little spirit of salt, reduced into Chryftals, differing from those of other Mettals; diaphanous also, and brittle, and far more easily fusible, than Silver; wholly unlike either a Salt or a Mettal, but very like to a piece of Horn, and withall insipid, though the Solution of Silver, be very bitter, and the spirit of salt, highly sour, &c.

4. An Anomalous Salt; (which the Author had not, it seems the liberty to teach the Preparation of) whose Ingredients were purely Saline, and yet the Compound, made up only of salt, sour, and strongly tasted Bodies, was rather really sweet, than of any other taste, and when a little urged with heat, its odour became stronger, and more insupportable than that of Aqua fortis, distilled Urine, and even spirit of salt Armoniack, but yet when these Fumes settled again into salt, their odour would again prove inoffensive, if not pleasant, &c.

5. A Sea-salt, whence Aqua fortis had been distilled: Where the Liquor, that came over, proved an Aqua Regis: the substance in the bottom, had not onely a mild taste, and

D d affected
affected the Pallet much more like salt peter, than Common salt; but was also very fusible, and inflammable, though produced of two un-inflammable bodies; and the Same Sub-
stance, consisting of Acid salts, by a certain way of the Au-
thor, produced a Fixt salt.

6. Oyle of Vitriol poured upon a Solution of Bay-salt: whence
was abstracted a liquor, that by the Smell and Taste ap-
peared to be a Spirit of salt. In which operation, the mixture,
by working a great change of Texture, did so alter the na-
ture of the compounding Bodies, that the Sea-salt, though a
considerably fixt Body, was distill'd over in a moderate
Fire of Sand, whilst the Oyl of Vitriol, though no such
gross salt, was by the same operation so fixt, as to stay be-
hind: Besides that the same, by a competent heat yielded
a substance, though not insipid, yet not at all of the Taste
of Sea-salt, or of any other pungent one, much less having
the highly corrosive acidity of Oyl of Vitriol, &c.

7. A dissolvent, made by pouring a strong Spirit of Nitre on
the rectified Oyl of the Butter of Antimony, and then distilling off
all the liquor, that would come over, &c. This Menstruum (cal-
led by the Author Peracutum) being put to highly refined
Gold, destroyed its Texture, and produced, after the me-
thod prescribed in the book, a true Silver, as its whiteness
in colour, dissolubleness in Aqua fortis, and odious Bitterness,
did manifest: which change of a Mettal, commonly esteemed
to be absolutely indestructible by Art, though it be far from
being Luciferous, is yet exceedingly Instructive; as is also
the way, the Author here adds, of Volatilizing Gold, by the
power of the same Dissolvent.

8. Aqua fortis, concoagulated with differing Bodies, produced
very differing Concretes: And the same Numeral Saline
Corpuscles, that being associated with those of one Mettal,
had already produced a Body eminent in one Taste, did af-
fer-
terwards, being freed from that Body, compose a Liquor of a very differing taste; and after that too, being combin'd with the parties of another Mettal, did with them constitute a Body of a very eminent Taste, as opposite as any one can be to both the other Tasts; and yet these Saline Corpuscles, being instead of this second Mettal, associated with such a one as that, they are driven from, did therewith exhibite again the first of the three mention'd Tasts.

9. Water transmuted into Earth, though the Author faith of this Transmutation, that it was not so perfect, as he wish'd, and as he hopes to make it.

10. A mixture of Oyle of Vitriol and Spirit of Wine. These two Liquors, being of odd Textures in reference to each other, their conjunction and distillation made them exhibite these Phenomena: vid. That, whereas Spirit of Wine has no great, nor good scent, and moderately dephlegm'd Oyl of Vitriol is wont to be inodorous; the Spirit, that first came over from their mixture, had a scent not only very differing from Spirit of Wine, but from all things else, that the Author ever smelt; the Odor being very fragrant & pleasent, and so subtle, that in spite of the care taken in luting the Glasses exactly together, it would perfume the neighbouring parts of the Laboratory, and afterwards smell strongly at some distance from the Viol, wherein it was put; though stop't with a close Cork, covered with two or three several Bladders. But, after this volatile and odoriferous Spirit was come over, and had been follow'd by an Acid Spirit, it was at last succeeded by a strongly flinking Liquor, &c.

But Manum de Tabula: the Book itself will certainly give a satisfaction far beyond what here can be said of it.
Some New observations about the Planet Mars, communicated since the Printing of the former sheets.

There was very lately produced a Paper, containing some observations, made by Mr. Hook, about the Planet Mars; in the Face whereof he affirmed to have discovered, in the late months of February and March, that there are several Maculae or Spotted parts, changing their place, and not returning to the same Position, till the next ensuing night near about the same time. Whence it may be collected, that Mars (as well as Jupiter, and the Earth, &c.) does move about his own Axis, of which a fuller account will be given hereafter, God permitting. This short and hasty intimation of it, is intended only to invite others, that have opportunity, timely to make Observations, (either to confirm, or rectify) before Mars gets out of sight.
The Contents.

A way of Preserving Birds taken out of the Egg, and other small Fetus's; communicated by Mr. Boyle. An Extract of a Letter, lately sent to Sr. Robert Moray out of Virginia, concerning an unusual way of Propagating Mulberry-trees there, for the better improvement of the Silk-Work; together with some other particulars, tending to the good of that Royall Plantation. A Method, by which a Glass of a small Plano-Convex Sphere may be made to refract the Rays of Light to a Focus of a far greater distance, than is usuall. Observations about Shining Worms in Oysters. Observations of the Effects of Touch and Friction. Some particulars, communicated from foreign Parts, concerning the Permanent Spott in Jupiter; and a contest between two Artists about Optick-Glasses, &c. An Account of a Book written by Dr. Thomas Sydenham, entitled, Methodus Curandi Febres, propriis Observationibus superstructa.

A way

Of preserving Birds taken out of the Egg, and other small Fetus's; communicated by Mr. Boyle.

His was imparted in a Letter, as follows;

The time of the year invites me to intimate to you, that among the other Uses of the Experiment, I long since presented the Society, of preserving Whelps taken out of the Dams womb, and other Fetus's, or parts of them, in Spirit of Wine; I re-

member
member, I did, when I was solicitous to observe the Process of Nature in the Formation of a Chick, open Hens Eggs, some at such a day, and some at other daies after the beginning of the Incubation, and carefully taking out the Embryo's, embalmed each of them in a distinct Glass (which is to be carefully stoppt) in Spirit of Wine: Which I did, that so I might have them in readiness, to make on them, at any time, the Observations, I thought them capable of affording; and to let my Friends at other seasons of the year, see, both the differing appearances of the Chick at the third, fourth, seventh, fourteenth, or other daies, after the Eggs had been set on, and (especially) some particulars not obvious in Chickens, that go about; as the hanging of the Guts or the Abdomen, &c. How long the tender Embryo of the Chick soon after the Punctum saliens is discoverable, and whilst the Body seems but a little Organized Gelly, and some while after That, will be this way preserv'd, without being too much shrivel'd up, I was hindred by some mischances to satisfy my self: but when the Fetus's, I took out, were so perfectly formed as they were wont to be about the seventh day, and after, they so well retain'd their shape and bulk, as to make me not repent of my curiosity: And some of those, which I did very early this Spring, I can yet shew you. I know I have mention'd to you an easie application of what I, some years since, made publick enough; but not finding it to have been yet made by any other, and being perswaded by Experience, that it may be extended to other Fetus's, which this season (the Spring) is time to make provision off, I think the Advertisement will not seem unseasonable to some of our Friends; though being now in haste, and having in my thoughts divers particulars, relating to this way of Preserving Birds taken out of the Egge, and other small Fetus's, I must content my self to have mention'd that, which is Essential, leaving divers other things, which a little practice may teach the Curious, unmention'd. Notwithstanding which, I must not omit these two Circumstances; the one, that when the Chick was grown big, before I took it out of the Egge, I have (but not constantly) mingled
mingled with the Spirit of Wine, a little Spirit of Sal Armoniack, made (as I have elsewhere delivered) by the help of Quick-lime: which Spirit I choose, because, though it abounds in a Salt not Sore, but Urinous, yet I never observed it (how strong soever I made it) to coagulate Spirit of Wine. The other circumstance is, that I usually found it convenient, to let the little Animals I meant to imbleme, lie for a little while in ordinary Spirit of Wine, to wash off the looser filth, that is wont to adhere to the Chick, when taken out of the Egg; and then, having put either the same kind of Spirit, or better upon the same Bird, I suffer'd it to soak some hours (perhaps some days, pro renata) therein, that the Liquor, having drawn as it were what Tincture it could, the Blood being remov'd into more pure and well dephlegm'd Spirit of Wine, might not discolour it, but leave it almost as limpid, as before it was put in.

An Extract

Of a Letter, sent lately to Sir Robert Moray out of Virginia, concerning an unusual way of propagating Mulberry trees there, for the better improvement of the Silk-Work; together with some other particulars, tending to the good of that Plantation.

I am disappointed at this time of some Rarities of Minerals, Mettals, and Stones; but you may have them any other time, as conveniently, &c. I have planted here already ten thousand Mulberry trees; and hope, within two or three years, to reap good Silk of them. I have planted them in a way unusual here, which advances them two or three years growth, in respect of their being sown in seed: And they are now, at writing thereof all holding good, although this has been a very long and bitter winter with us, much longer and colder, than ever I did find it in Scotland or England. I intend likewise to plant

E e 2 them
them all, as if they were Currants or Gooseberries, so thick as
hedges; whereby one man may gather as many of them, as oth-
erwise, when they are planted in trees at distance, four
persons may do. Expedient is the benefit of this Trade. Hav-
ing discoursed of this new way to all here; they are generally
inclining to it; considering that the Planting their Trees, as
before, at distance, and letting them grow high, has been the
main obstruction of that work hitherto, and the loss of their
time and gain: but being in hedges, they will be always young
tender plants; and consequently will be easily cut in great
quantities with a pair of Garden Sizzers. But there may be
suggested yet another, and perhaps a better way; which is, to
sowe some Acres with Mulberry seed, and to cut it with a sith,
and ever to keep it under. I have also betheathed myself a
new way, for a few hands to serve many Worms, and that
more cleanly than before; which also will be a means, without
more trouble or pains, to separate unhealthy worms from
healthful; and by which a great many more may be kept in a
room, than otherwise upon shelves, as is usual here. Besides
this, I have sown a little French Barley and Rice seed, and am
thinking on a way of un-husking them with expedition, and to
preparing them for the Merchant, as they use to be: But if you
can inform me, how they are prepared, you may save me
some labour. If I had any Coffee in husks, or any other ve-
getable commodity, from the Streights to try, I would here
make trial with them. Its like, that some of those Merchants
that are of your Society, and keep a Correspondency there, may
afflict in procuring them. By the latter ships I intend to send
you a New sort of sweet sented Tobacco, which I have not yet
had time to improve.

A Method, by which a Glass of a small Plano-convex Sphere
may be made to refract the Rays of light to a Focus of
a far greater distance, than is usual.

This is proposed by Mr. Hook, in consequence of what was
mention'd from him in Numb. 4 pag. 67, of these Transactions.

Prepare (for he) two Glasses, the one exactly flat on both sides, the other flat on the one side, and convex on the other, of what Sphere you please. Let the flat Glass be a little broader than the other. Then let there be made a Cell or Ring of Brass, very exactly turn'd, into which these two Glasses may be so fastned with Cement, that the plain surfaces of them may lye exactly parallell, and that the Convex-side of the Plano-convex Glass may lye inward; but so, as not to touch the flat of the other Glass. These being cemented into the Ring very closely about the edges, by a small hole in the side of the Brass-ring or Cell, fill the interposed space between these two with Water, Oyl of Turpentine, Spirit of Wine, Saline Liquors; &c; then stop the hole with a screw: and according to the differing refraction of the interposed Liquors, so shall the Focus of this compound Glass be longer or shorter.

But this (adds the Proposer) I would only have look'd upon, as one instance of many (for there may be others) of the Possibility of making a Glass, ground in a smaller Sphere, to constitute a Telescope of a much greater length: Though (not to raise too great Expectation) I must add, That of Spherical Object glasses, those are the best, which are made of the greatest Sphere, and whose substance hath the greatest refraction.

Observations
About Shining Worms in Oysters.

These Observations occur in the French journal of April 123, 1666, in two letters, written by M. August to M. Dela Voye; whereof the substance may be reduced to the following particulars.

I. That M. Dela Voye having observed, as he thought,
some shining Worms in Oysters; M. Augout, being made acquainted with it, did first conceive, they were not Worms (unless they were crushed ones) that shin’d, as having not been able then to discern any parts of a Worm; but only some shining clammy moisture, which appeared indeed like a little Star of a blewish colour, and stuck to the Oyster-shell; being drawn out, shine in the Air its whole length (which was about four or five lines,) and when put upon the Observers hand, continued to shine there for some time.

2. That M. Augout afterwards, causing more than 20. dozen of Oysters to be open’d at Candle-light, really saw, in the dark, such shining worms in them; and those of three sorts. One sort was whitish, having 24. or 25. feet on each side, forked; a black speck on one side of the head (taken by him for a Chryalitin) & the back like an Elec; stript off her skin. The second, red, and resembling the common Glow-worms, found at Land, with folds upon their backs, and feet like the former; and with a nose like that of a dog, and one eye in the head. The third sort was speckled, having a head like that of a Sole, with many tufts of whitish hair on the sides of it.

3. That, besides these, the Observer saw some much bigger, that were grayish, with a big head, and two horns on it, like those of a Snayl, and with 7. or 8. whitish feet, but these, though kept by him in the night, shin’d not.

4. That the two first sorts are made of a matter easily resoluble, the least shaking or touch turning them into a viscus and aqueous matter; which falling from the shell, stuck to the Observers fingers, and shine there for the space of 20. seconds: and if any little part of this matter, by strongly shaking the shell, did fall to the ground, it appear’d like a little piece of a flaming Brimstone; and when shaken off nimbly, it became like a small shining Line, which was dissipated before it came to the ground.

5. That
5. That this shining matter was of different colour: some whitish, some reddish; but yet that they afforded both, a light which appear'd a violet to his eye.

6. That it is very hard to examine these worms entire (especially the white ones) because that at the least touch they doe burst, and resolve into a glutinous moisture; whence also if it were not for their feet, that are discover'd in their matter, none would judge them to be Worms.

7. That among those, which be observed, he saw two more firm, than the rest, which shone all over; and when they fell from the Oyster, twinkled like a great star, shining strongly, and emitting rays of a violet-light by turns, for the space, (as touch'd above) of 20. seconds. Which Scintillation the observer imputes to this, that those worms being alive, and sometimes raising their head, sometimes their tayle, like a Carpe, the light increased and lessened accordingly; seeing that, when they shone not, he did, viewing them by a Candle, find them dead.

8. That forcibly shaking the Oyster-shells in the dark, he sometimes saw the whole shell full of lights, now and then as big as a fingers end; and abundance of this clammy matter, both red and white, (which he judges to have been Worms); burst in their holes.

9. That in the shaking he saw all the Communications of these little Verminulous holes, like to the hole of Worms in Wood.

10. That in more than 20 douzen of Oysters he shook now shell (10. or 12. excepted) but it emitted light: And found some of this light in sixteen of the Oysters themselves.

11. That this light occurs more frequently in big, than small Oysters; in those that are pierced by the Worm, oftner, than in
in those that are not, and rather upon the Convex-side, than the other; and more in fresh ones, than in the stale.

12. That having somewhat scaled the Convex-side of the shell, and discover'd the Communication of the holes, wherein the often-mention'd viscous moisture; that has any form of insects, is found; he smelt a scent, that was like the water of a squeezed Oyster.

13. That the Worms give no light, when irritated, but if they do, the light lasts but a very little time, whereas that which appears in those, that were not angered before, continues a great while; the Observer affirming to have kept of it above 2 hours.

So far the Journal des Scavans; which intimates withal, that if the Observers had had better Microscopes, they could have better examin'd this matter.

But since the curious here in England are so well furnish'd with good ones, 'tis hoped, that they will employ some of them for further and more minute Observations of these Worms; it being a matter, which, joyned with other Observations, already made by some excellent persons here, (especially Mr. Boyle) upon this subject of Light, may prove very luciferous to the doctrine of it, so much yet in the dark.

Some Observations
Of the Effects of Touch and Friction.

The Operations and Effects of Touch and Friction having been lately much taken notice off, and being lookt upon by some, as a great Medical Branch, for the curing of many diseases and infirmities; it will perhaps not be unseasonable to mention (here also) some Observations relating thereunto; which may give an occasion to others, to consider this subject more; than has been done heretofore, and to make further
ther Observations and Tryals concerning the power of the fame.

And First, the Illustrious Lord of Verulam, in his History of Life and Death; Hist. c. §. 3. observes, That Motion and Warmth (of which two, Friction consists) draws forth, into the parts, New Juyce and Vigour. And Canon. XIII: he affirms, That Frictions conduce much to Longevity. See the same, Connex. ix. §. 26. &c.

Secondly, The Honourable Robert Boyle, in his Usefulness of Experimental Philosophy, sect. 2. ch. 15. considering the Body of a Living man or any Animal, as an Engine, so composed, that there is a conspiring communication betwixt its parts; by virtue whereof a very slight impression of adventitious matter upon some one part, may be able to work, on some other distant part, or perhaps on the whole Engine, a change far exceeding, what the same adventitious matter could do upon a Body not so contrived: Representing, I say, an Animal in this manner, and thence inferring, how it may be alter'd for the better or worse by motions or impulses, confessedly Mechanicall, observes, How some are recover'd from swooning fits by pricking; others grow faint and do vomit by the bare motion of a Coach; others fall into a troublesome sickness by the agitation of a Ship, and by the Sea-air (whence they recover by rest, and by going a shore.) Again, how in our Stables a Horse well-curried is half-fed: How some can tell by the Milk of their Asses, whether that day they have been well curried or not: Arguing hence, that if in Milk the alteration is so considerable, it should be so likewise in the Blood, or other Juyces, of which the Blood is elaborated, and consequently in divers of the principal parts of the Body. Where also (upon the authority of Piso) he refers the Reader to the Brasillian Empiricks, whose
Thirdly, the learned Dr. John Beale, did not long since communicate by some Letters, First, that he could make good proof of the curing or killing a very great and dangerous Wens (that had been very troublesome for two or three years,) by the application of a dead man's hand, whence the Patient felt such a cold stream pass to the Heart, that it did almost cause in him a fit of swooning. Secondly, that, upon his brothers knowledge, a certain Cook in a Noble Family of England (wherein that brother of his then lived) having been reproached for the ugliness of his Warty hands, and return'd for an answer, that he had tried many remedies, but found none, was bid by his Lord, to rub his hand with that of a dead man; and that this Lord dying soon after, the Cook made use both of his Lords advise and hand, and speedily found good effect. (Which is also confirm'd by what Mr. Boyle relates in his lately mentioned Book, of Dr. Harvey's frequently successfull trial, of curing some Tumors or Excrencies, by holding on them such a Hand.) Here is Friction or Touch, to mortifie Wens, to drive away swellings and Excrencies: And why not to repel or dissipate Spirits, that may have a dangerous influence upon the Brain, or other parts; as well as to call forth the retired ones into the habit of the Body, for Invigoration? Thirdly, that a Gentleman, who came lately out of Ireland, lay at his House, and inform'd him of an aged Knight there, who having great pain in his feet, insomuch that he was unable to use them, suffered, as he was going to bed, a loving Spaniel to lick his feet; which was for the present very pleasing to him, so that he used it mornings and evenings, till he found the pain appeased, and the use of his feet restored. This, faith the
the Relator, was a gentle touch, and transpiration; for he found the Spirits transpire with a pleasing Kind of Titillation. Fourthly, that he can assure of an honest Blacksmith, who by his healing hand converted his Bars of Iron into Plates of Silver, and had this particular faculty, that he caused Vomitings by stroaking the Stomack; gave the Stool by stroaking the Belly; appeased the Gout, and other paines, by stroaking the parts affected.

Some particulars, communicated from foreign parts, concerning the Permanent Spott in Jupiter; and a Contest between two Artists about Optick Glasses, &c.

Eustachio de Divinis (faith the Informer,) has written a large Letter, wherein he pretends, that the Permanent Spot in Jupiter hath been first of all discovered with his Glasses; and that the P. Gotignies is the first that hath thence deduced the Motion of Jupiter about his Axis; and that Signior Caffini opposed it at first; to whom the said Gotignies wrote a letter of complaint thereupon.

See Numb. 1. of these Translations; by the date whereof it will appear, that that Spot was observed in England, a good while before any such thing was so much as heard of.

The same Eustachio pretends likewise, that his great Glasses excell those of Campani; and that in all the tryals, made with them, they have performed better; and that Campani was not willing to do, what was necessary for well comparing the one with the other. viz. To put equall Eyeglasses in them, or to exchange the same Glasses.

The said Divini affirms also, that he hath found a way to know
know, whether an Object-glass be good or not, only by looking upon it, without trying. This would be of good use, especially if it should extend so far as to discern the goodness of such a glass, whilst it is yet on the Cement.

An Account

Of Dr. Sydenham’s Book, entitled, Methodus Curandi Febres, Propriis observationibus superstructa.

This Book undertakes to deliver a more certain and more genuine Method of curing Feavers and Agues, than has obtained hitherto: And it being premised, First, that a Fever is Nature’s Engine, she brings into the field, to remove her enemy: or her handmaid, either for evacuating the impurities of the blood, or for reducing it into a New State: Secondly, that the true and genuine cure of this sickness consists in such a tempering of the Commotion of the Blood, that it may neither exceed, nor be too languid: This, I say, being premised by the Author, he informs the Reader:

In the First Section, of the different Method, to be employed in the cure of Feavers, not only in respect of the differing constitutions and ages of the patients, but also in regard of the differing seasons of one and the same year, and of the difference of one year from another. As to the Former, he shews, in what sorts of Patients, and at what time of the Fever, Phlebotomy, or Vomiting, or both, are to be used; and when and where not: In what space of time the Depuration, if nature be not disturbed or hindered in her work, will be perform’d: When Purgatives are to be administered: How that Diarrhea’s happen, if the Patient had in the beginning
ning of the Feaver an inclination to vomit, but no vomit was given; and that those symptoms, which commonly are imputed to a malignity, do, for the most part, proceed from the Relaxation of the tone of the Blood, caused by Medicines too refrigerating, or by the unseasonable use of Glisters in the declination of the disease. As to the Latter, he observes, that one of the chief causes, rendring the Cure of Feavers so uncertain and unsuccessful, is, that Practitioners do accommodate their observations, they take from the successful cure of some Feavers in one season of the year, or in some one year, to that of all Feavers in any season, or in any year whatsoever. And here he observes, first, how vigorous the blood is in the Spring, and how dispirited in Autumn; and thence regulates the letting of blood, and Vomiting, and the giving of Glisters. Next, how difficult it is, to assign the cause of the difference between the Feavers of several years; and to prognosticate of the salubrity or insalubrity of the following part of the year: where yet he insinuates, that, when Insects do swarm extravagantly, and when Feavers and Agues (especially Quartan) appear very early, as about Midsummer, then Autumn commonly proves very sickly. Lastly, what method and Cautions are to be used in the Cure of Epidemical Feavers.

In the Second Section, he treats of the Symptoms, accompanying Continued Feavers; as Phrenies, Pleurisies, Coughs, Hicoughs, Fluxes, &c. Shewing, both whence they are caused, and how they are to be cured: Where having inserted a considerable Paragraph, touching a certain Symptomatical Feaver in the Spring, to be cured like Plurisy; he mentions among many Observables, this, as a chief one, that Laudanum, or any other Narcotick given against the Phrensy, in the beginning, progress, or height of a Fever, does rather hurt, than good, but in the declination thereof, is used with good success. To all which he subjoins a particular ac-
compt of the Iliac Passion (esteem'd by him to be sometimes a Symptome also of Feavers;) not only discoursing of its cause (a preposterous inversion of the Intestins, proceeding either from Obstruction, or Irritation,) but adding also a very plain way of Curing the same; and that not by the use of Quick-silver or Bullets (by him judged to be frequently noxious) but only by Mint water; and the application of a Whelp to the Patients stomach; to strengthen the same, and to reduce it again to its natural motion.

In the Third Section, he treats of Intermittent Feavers, or of Agues: Where he discourses of the times of the Cold and Hot Fits, and of that of the Separation of the subdued aquish matter: Finds difficulty in giving a satisfactory account of the return of Fits: distinguishes Agues into Vernal and Autumnal: Takes notice, that as there are few Continued Feavers, so generally there are only Quotidian and Tertians, in the Spring; and only Tertians and Quartans in Autumn; Of which having offered Reasons, that seem considerable, he proceeds to his Method of curing them; and, laying much weight upon the said difference, he prescribes and urges different ways to be used in that cure: Interferring among other things these notes; First, that the Period of Fermentation in Feavers, both Continued and Intermittent, is (if left to Nature's own conduct, and well regulated, if need be, by Art) performed in about 336 hours or 14 days; deducting in Intermittent ones, the hours of intermission, and counting $\frac{1}{2}$ hours for every Paroxism; and imputing the excursion beyond that time to the disturbance given to nature by the error of Practitioners. Secondly, that whoever hath had a Quartan formerly, though many years be pass'd, shall, if he chance to have another, be soon freed from it; and that a Physician knowing that, may confidently predict this.
In the Fourth Section, the Author, in conformity to the Custom of those that write of Feavers, discourses of the Small-pox: and First, examining the cause of this sickness and its universality, delivers his peculiar opinion of the blood's endeavouring a Renovation or a New Texture (once at least in a Man's life) and is inclin'd to preferr the fame to the received doctrine of its malignity. Then, having laid down, for a foundation of the Cure, the two times, of separation and Expulsion, he argues as well against too high an Ebullition or too hafty a separation (by a hot diet or high Cordials) as against too languid a one (by Blood- ing, Purges, and Cooling medicines.) The like he does to the Time of Expulsion, forbidding both immoderate Heat(whereby Nature's expelling operation is disturbed by a precipitated and too thick a crowd of the protruded pustules) and too much Cooling, whereby due Expulsion is hindred. In short, he advises, to permit Nature to do her own work, requiring nothing of the Physician, but to regulate her, when she is exorbitant, and to fortifie her, when she is too weak. He concludes all, with delivering a Model of the Method, he would use for his own only Son, if he should fall into this Sickness.

Advertisement.

Whereas 'tis taken notice of, that several persons persuade themselves, that these Philosophical Transactions are publish't by the Royal Society, notwithstanding many circumstances, to be met with in the already publish't ones, that
that import the contrary; The Writer thereof hath thought fit, expressly here to declare, that that persuasion, if there be any such indeed, is a mere mistake; and that he, upon his Private account (as a Well-wisher to the advancement of useful knowledge, and a Furtherer thereof by such Communications, as he is capable to furnish by that Philosophical Correspondency, which he entertains, and hopes to enlarge) hath begun and continues both the composition and publication thereof: Though he denies not, but that, having the honour and advantage of being a Fellow of the said Society, he inserts at times some of the Particulars that are presented to them; to wit, such as he knows he may mention without offending them, or transgressing their Orders; tending only to administer occasion to others also, to consider and carry them further, or to Observe or Experiment the like, according as the nature of such things may require.

Printed with Licence for John Martyn, and James Allestry, Printers to the Royal Society. 1666.
A gentle line, do not allow dilating the...
PHILOSOPHICAL
TRANSACTIONS.

Munday, June 4. 1665.

The Contents.

Certain Problems touching some Points of Navigation. Of a new Contrivance of Wheel-Barometer, much easier to be prepar'd than others. An account of Four Suns which lately appeared in France; and of two, unusually posited, Rainbows, seen in the same Kingdom. A Relation of an Accident, by Thunder and Lightning, in Oxford. An Experiment, to examine, what Figure or Celerity of Motion begetheth or increaseth Light and Flame. Some Considerations touching a Letter in the Journal des Scavans of May 24. 1666.

Certain Problems
Touching some Points of Navigation.

These Problems are presented by the Learned and Industrious Nicolaus Mercator, for the advancing of that Excellent and Beneficial Science, Navigation, as follows:

The line of Artificial Tangents, or the Logarithmical Tangent-line, beginning at 45. deg. and taking every half degree for a whole one, is found to agree pretty neer with the Meridian-line of the Sea-Chart; they both growing, as it were, after the same Proportion. But the Table of Meridional degrees being calculated onely to every sexagesimal minute of a degree, shews some small difference from the said Logarithmical Tangent-line. Hence it may be doubted, whether that difference do not arise from that little error, which is committed by calculating the Table of Meridional degrees onely to every minute.
Mr. Oughtred in the VI. Chap. of his Navigation, annexed to the Book, entituled, The Circles of Proportion, and the Horizontal Instrument, &c. mentions an Artifice, by himself discover'd, by which it may be effected, that the small parts of the Meridian be not one minute (which on the face of the Earth answers to above an English Mile) but the hundred-thousandth, or, if need be, the millioneth part of a minute, scarce exceeding one fifteenth part of an Inch: Which thing, he saith, he is able to perform in Tables unto the Radius 10000000; yet nothing at all differing either in their form or manner of working from those that are now commonly in use.

But which way this is to be done, this Author hath not made known to the Publick. And, though such Tables unto the Radius 10000000 had been brought to light, yet would they not be sufficient to prove the identity or sameness of the said two Lines, as to continue the comparison between them as far, as the one of them, videlicet, the Logarithmical Tangent-line, is already calculated, that is, to Ten places, besides the Characteristic.

Now therefore, if a certain Rule could be produced, by which the Agreement or Disagreement of the said two Lines might be shew'd, not only to that Extent of places, to which that Tangent-line is already calculated, but also to as many more, as the same may be yet further extended unto, in infinitum usque: surely that rule would not only save us the labour of making Tables unto the Radius 10000000; but also the Helix or Spiral Line of the Ships Course would be reduced to a more precise exactness, than ever was pretended by Him: and this most Noble and Useful Science (as he justly calls it) which is the Bond of most disjunct Countries, and the Consociation of Nations farthest remote, would attain its full luster and perfection.

Besides, that the same Rule would also discover a far easier way of making Logarithmes, than ever was practised or known; and therefore might serve, when ever there should be occasion, to extend the Logarithmes beyond that number of places, that is already extant.

Moreover such a Rule would enable men to draw the Meridian line Geometrically, that is, without Tables or Scales: which indeed might.
might also be done, by setting of the Secants of every whole
or half degree, if there were not this Inconveniency in it (which
is not in my Rule:) That a Line composed of so many small
parts, would be subject to many errors, especially in a small
compass.

The same Rule also will serve, to find the Course and Distance
between two Places assigned, as far, as practice shall require it;
and that, without any Table of Meridional parts, and yet with
as much ease and exactness.

And seeing all these things do depend on the solution of this
Question, Whether the Artificial Tangent-line be the true Meridian-
line? It is therefore, that I undertake, by God's assistance, to
resolve the said Question. And to let the world know the readiness
and confidence, I have to make good this undertaking, I am willing to lay a Wager against any one or more persons that
have a mind to engage, for so much as another Invention of mine
(which is of less subtlety, but of far greater benefit to the pub-
lick) may be worth to the Inventor,

For, the great advantage, that all Merchants, Marriners, and
consequently the Commonwealth, may receive from this other
Invention, is, in my judgment, highly valuable; seeing it will
oftentimes make a ship faile, though, according to the common
way of failing, the wind be quite contrary, and yet as near to
the place intended, as if the wind had been favourable: Or, if
you will, it will enable one to gain something in the intended
way, whether the wind be good or no (except only when you
go directly South or North,) but the advantage will be most,
where there is most need of it, that is, when the wind is contrary:
So that one may very often gain a fifth, fourth, third part, or
more of the intended voyage; according as it is longer or shorter;
Viz. always more in a longer Voyage, where the gain is
more considerable, and more welcome; not only by saving
Time, but also Victuals, Water, Fuel, Men's health, and so much
Room in the ship.

All this, which is here pretended, the Proposer is to make
good by the Verdict of some Able Men, who also may give a
guess, what this latter Invention may be worth to the owner:
And for so much, and no more, he will stand engaged against a-
ny one or more Persons, that he will and shall resolve the Question above-mention'd, viz. Whether the Artificial Tangent-line be the true Meridian-line, yea or no? And if he do not, that then he will lose, and transport to the other Party the whole benefit of the last mentioned invention. But if, on the contrary, he do prove or disprove the Identity of the said two lines, to the Judgment of some able Mathematicians, that then so much money be paid him by the other Party, as the said Invention was valued.

And, whereas there are often Wagers laid about things that concern the Engagers little or nothing; 'tis thought, that it would concern all Merchants, Marriners, and all Lovers of the common good, rather to lay wagers against one another about things of this Nature, where the Gainer doth gain as well, as if he had laid his wager about something else; and the Looser hath so far the benefit as well as the Gainer, that, he hath thereby promoted the thing, that concerns them both alike.

Now therefore, to the end, that the Looser may have his benefit by it, as well as the Gainer, it would not be amiss, that the condition were made thus; that the latter should grant the moiety of his gain to the Proposer; that thereby he might be enabled to bring to light both those, and some other useful inventions, for the Service of Mankind. And to manifest, that it is not for his own interest only, that the Proposer mentions this; he is willing to impart from that moiety, so received, the full moiety again to any other person within His Majesty's Dominions, who shall first of all give notice of his Undertaking to prove or disprove the said Identity, and perform it accordingly within the space of two Months, to be computed from the present Date. Those that have a mind to engage, may repair to the Printers of these Tracts, where they may know further.

A new Contrivance of Wheel-Barometer, much more easy to be prepared, than that, which is described in the Micrography: imparted by the Author of that Book.

This is only an easy way of applying an Index to any Common Baroscope, whether the Glasses be only a Single Cane, or have a round Bolt-head at the top. And by the means thereof, the Variation.
Variation of the Altitude of the Mercurial Cylinder, which at most is hardly three Inches, may be made as distinguishable, as if it were three Foot, or three Yards, or as much more, as is desired.

The manner hereof is visible enough by Figure I: where A B C represents the Tube, which may be either Blunt, or with a Head, as A B C (by which latter shape, more room is allow'd for any remainder of Air, to expand the better.) This is to be filled with Quicksilver, and inverted as commonly; but into a Vessel of Stagnant Mercury, made after the fashion of I K, that is, having its sides about 3 or 4 inches high, and the Cavity of it equally big both above and below; and if it can be (besides that part, which is fill'd by the end of the Mercuriall Tube, that stands in it) of equal capacity with the hollow of the Cane about B: For then the Quicksilver rising as much in the hollow of I, as it descends at B, the difference of the height in the Receiver I, will be just half the usual difference. And if the receiving Vessel I K have a bigger Cavity, the difference will be less, but if less, the difference will be greater: But, whether the difference be hereby made bigger or less, 'tis no great matter, since by the contrivance of the Wheel and Index (which is more fully described in the Preface to the Micrography) the least variation may be made as sensible as is desired, by diminishing the bigness of the Cylinder E, and lengthening the Index F G, according to the Proportion requisite.

An Account
Of Four Suns, which very lately appear'd in France, and of two Raine-bows, unusually posited, seen in the same Kingdom, somewhat longer agoe.

These Phenomena are thought worthy to be inserted here, for the Speculation of the Curious in those Kingdoms; as they were publish'd in the French Journal des Scavans, of May 10. 1666. viz.

The 9th of April of this present year, about half an hour past nine, there appear'd three Circles in the sky. One of them was very great, a little interrupted, and white every where, without
the mixture of any other colour. It passed through the midst of the Sun’s Disk, and was parallel to the Horizon. Its Diameter was above a hundred degrees, and its Center not far from the Zenith.

The Second was much less and defective in some places, having the Colours of a Rainbow, especially in that part, which was within the great Circle. It had the true Sun for its Center.

The Third was less, than the first, but greater than the second; it was not entire, but only an Arch or Portion of a Circle, whose Center was far distant from that of the Sun, and whose circumference did, by its middle, joyn to that of the least Circle, intersecting the greatest Circle by its two extremities. In this Circle were discerned also the Colours of a Rainbow, but they were not so strong, as those of the Second.

At the place, where the circumference of this Third Circle did close with that of the Second, there was a great brightness of Rainbow-Colours, mixt together: And at the two extremities, where this Second Circle intersected the First, appear’d two Parhelia’s or Mock-suns, which shone very bright, but not so bright, nor were so well defined, as the true Sun. The False Sun, that was towards the South, was bigger, and far more luminous, than that towards the East. Besides those two Parhelia’s, which were on the two sides of the true Sun, in the intersection of the First and Third Circle, there was also upon the First great Circle, a third Mock-sun, situated to the North, which was less and less bright, than the two others. So that at the same time there were seen Four Suns in the heavens.

Figure II. will illustrate the Position of this Phenomenon.

A. The Zenith or the Point Vertical of the place of Observation.

R. The true Sun.

SCHN. The great Circle, altogether White, almost parallel to the Horizon, which pass’d through the True Sun’s Diske, and upon which were the False Suns.

DEBO. A Rain-bow about the Sun, forming an entire Circle, but interrupted in some places.

HDN. A portion of a Circle, that was Excentrick to the Sun, and greater than the Circle DEBO, which touch’d DEBO, and was confounded with it in the point D.
HN. The two Mock-Suns, in the intersecition of the Semicircle HDN, and the Circle SCHN: The midft of which two False-Suns was white and very luminous; and their Extreme towards D I were tinged with the Colours of a Rainbow. The False Sun, mark'd N, was fainter than that, which is mark'd H.

C. The Mock-Sun, all white, and far less shining, than the two others.

I. A space very dark between R, and D.

This Appearance is look't upon as one of the notableft, that can be seen, by reason of the Excentricity of the Circle HDN, and because that the Parhelia * were not in the Interception of the Circle DEBO with the great Circle SCHN, but in that of the Semi-circle HDN.

As for the two odd Rainbows; they appear'd at Chartres the 10. of August, 1665. about half an hour past six in the Evening; and did crofs one another almost at right Angles, as may be seen by Fig. IV.

The Rainbow, which was oppofite to the Sun, in the usual manner, was more deeply colour'd, than that, which crofs'd it; though even the Colours of the first Iris were not fo strong, as they are now and then seen at other times.

The greatest height of the stronger Rainbow, was about 45. degrees; the feeble Rainbow lof't one of its Legs, by growing fainter, about 20. degrees above the stronger; and the Leg below appear'd continued to the Horizon.

These Rainbows did not juft decussate one another at right Angles; there was some 6. or 7. degrees difference. The fainter, feem'd to be a Portion of a great Circle; and the stronger was but a Portion of a small Circle, as usually.

The Sun, at their appearance, was about 6. degrees high above the Horizon, and towards the 17. Azimuth of the West, Northward.

*Those Five Suns, that appeared the 29 March, A. 1629. at Rome, between 2. or 3. of the clock in the afternoon, were thus pofted; that the two of them, which were in the interception of two Circles, appear'd in that of a Circle, which passed through the Sun's Disk, with another, that was Concentrick to the Sun: as may be seen in Figure III. borrow'd (for the easier comparing them together) out of Des-Cartes his Meteors, cap. X.
The Observer, M. Estienne, notes, that, when he made this Observation, the River of Chartres, which runs very near from South to North, was betwixt him and the Rainbow; and that he stood Level with this River, whence he was distant not above 150 paces: which he adds, that the Curious may the better judge of this Observation.

A Relation

Of an Accident by Thunder and Lightning, at Oxford.

This was imparted by Dr. Wallis in a Letter, written at Oxford, May 12, 1666, to the Publisher, as follows:

I should scarce have given you so soon the trouble of another Letter, were it not for an Accident which hapned here May 10. I had that afternoon, about 4. of the clock heard it thunder at some distance. About 5. of the clock the Thunder coming nearer to Us, it began to rain, and soon after (the rain with all increasing) the Thunder grew very loud, and frequent, and with long rattling Claps (though not altogether so great, as I have some other times heard;) and the Lightning with flashes very bright (notwithstanding the clear day-light) and very frequent, (when at the the fastest, scarce a full minute between one flash and another; many times not so much, but a second flash before the Thunder of the former was heard;) The Thunder for the most part began to be heard about 8. or 10. second minutes after the flash; as I observ'd for a great part of the time by my Minute-Watch: but once or twice I observ'd it to follow (in a manner) immediately upon it, as it were in the same moment; and the lightning extremely red and fiery. I do not use to be much apprehensive of Thunder and lightning, but I was at this time (I know not well, why?) very apprehensive, more than ordinary, of mischief to be done by it, for it seem'd to me to be very low and near Us (which made me so particular, as to observe the distance of the flash by the noise) and very frequent, and bright, so that, had it been by night as it was by day, it would have been very terrible. And, though I kept within doors, yet I sensibly discover'd a stinking sulphureous smell in the Air. About 7. of the clock it ended, before which time I had news brought me of a Sad Accident upon the water.
water at Medley, about a mile or somewhat more distant from
hence. Two Schollars of Wadham-Colledge, being alone in a
Boat (without a water-man) having newly thrust off from shore, at Medley, to come homewards, standing near the head of the boat, were presently with a stroke of Thunder or Lightning, both struck off out of the boat into the water, the one of them stark dead, in whom, though presently taken out of the water, having been by relation, scarce a minute in it) there was not discerned any appearance of life, sense, or motion: the other was stuck fast in the mud (with his feet downwards, and his upper parts above water) like a post not able to help himself out; but, besides a present spinning or numness, had no other hurt; but was for the present so disturb'd in his senses, as that he knew not, how he came there out of the boat, nor could remember either thunder or lightning, that did effect it: and was very feeble and faint upon it; which (though presently put into a warm bed) he had not thoroughly recover'd by the next night; and whether since he have or no, I know not.

Others in another boat, about 10 or 20 yards from these (as by their description I estimate) felt a disturbance and shaking in their boat, and one of them had his Chair struck from under him, and thrown upon him; but had no hurt. Those immediately made up to the others, and (some leaping into the water to them) presently drew them either into the boat or on shore; yet none of them saw these two fall into the water (not looking that way) but heard one of them cry out for help presently upon the stroke, and smelt a very strange stinking smell in the Air, which, when I asked him, that told it me, what kind of stink? he said, like such a smell, as is perceived upon the striking of Flint's together.

He that was dead (when by putting into a warm bed, and rubbing, and putting strong waters into his mouth, &c. no life could be brought into him) was the next morning brought to town; where, among multitudes of others, who came to see, Dr. Willis, Dr. Mollington, Dr. Lowder, and myself, with some others, went to view the Corps: where we found no wound at all in the skin; the face and neck swart and black, but not more, than might be ordinary, by the setting of the blood: On the right side of the neck was a little blackish spot about an inch long, and
about a quarter of an inch broad at the broadest, and was, as if it had been fear'd with a hot iron; and, as I remember, one somewhat bigger on the left side of the neck, below the Ear. Straight down the breast, but towards the left side of it, was a large place about three quarters of a Foot in length, and about two inches in breadth, in some places more, in some less, which was burnt and hard, like Leather burnt with the fire, of a deep blackish red colour, not much unlike the scorched skin of a roasted pig: And on the fore-part of the left shoulder such another spot about as big as a shilling; but that in the neck was blacker and seem'd more fear'd. From the top of the right shoulder, sloping downwards towards that place in his breast, was a narrow line of the like scorched skin; as if somewhat had come in there at the neck, and had run down to the breast, and there spread broader.

The buttons of his Doublet were most of them off; which, some thought might have been torn off with the blast, getting in at the neck; and then bursting its way out: for which the greatest presumption was (to me) that, besides 4 or 5 buttons wanting towards the bottom of the breast, there were about half a dozen together clear off from the bottom of the collar downwards, and I do not remember, that the rest of the buttons did seem to be near worn out, but almost new. The Collar of his doublet just over the fore-part of the left shoulder was quite broken asunder, cloth and stiffening, straight downwards, as if cut or chop'd asunder, but with a Blunt tool; onely the inward linnen or suftian lineing of it was whole, by which, and by the view of the ragged Edges, it seem'd manifest to me, that it was by a strock inwards (from without) not outwards from within.

His Hat was strangely torn, not just on the Crown, but on the side of the hat, and on the brim. On the side of it was a great hole, more than to put in ones fist through it: some part of it being quite struck away, and from thence divers gashes every way, as if torn, or cut with a Dull tool, and some of them of a good length, almost quite to the edges of the brim. And, besides these, one or two gashes more, which did not communicate with that hole in the side. This also I judged by a stroke inwards: not so much from the view of the edges of those gashes (from which there was scarce any judgment to be made either way,) but be-
cause the lining was not torn, only ript off from the edge of the
hat (where it was sow'd on) on that side, where the hole was
made. But his hat not being found upon his head, but at some
distance from him, it did not appear, against what part of the
head that hole was made.

Upon the rest of his Cloaths, I do not know of any further
effect, nor did we smell any sulphurous scent about them: which
might be, Partly because it was now a good while after the time,
and Partly by reason of their being presently drenched in the
water into which he fell.

The night following, the three Doctors above mentioned, & my
self, with some Chirurgions (besides a multitude of others) were
present at the opening of the head, to see if anything could be
there discover'd; but there appear'd no sign of contusion, the
brain full and in good order: the nerves whole and sound, the
vessels of the brain pretty full of blood. But nothing was by any
of them discern'd to be at all amiss. But it was by candle-light,
and they had not time to make very nice Observation of it (the
Body being to be buried by and by,) and the crowd of people
was a further hindrance. But if anything had been considerably
out of order to the view, it would surely have been by some of
them discovered. Some of them thought, they discern'd a small
fissure or crack in the skull; and some who held it, while it was
fawing off, said, they felt it Jarring in their hands, and there
seem'd to the eye something like it, but it was so small, as that by
Candle-light we could not agree it certainly so to be.

Some of the Hair on the right Temples was manifestly singed,
or burnt; and the lower part of that Ear blacker, than the parts
about it, but soft; and it might be only the setting of the Blood.
The upper part of the left Shoulder, and that side of the neck,
were also somewhat blacker, than the rest of the Body, but whether
it were by the blow, which broke the collar, and scorch'd
the round red spot thereupon, or only by setting of the Blood,
I cannot say; yet I think, it might very well be, that both on
the head, and on this side of the neck, there might be a very
great blow, and a contusion upon it (and seems to have been so,
by the tearing of the hat, and breaking the collar, if not also
cracking of the skull,) and yet no sign of such contusion, because
dying so immediately, there was not time for the blood to gather

H h 2 to
to the part and stagnate there (which in bruises is the cause of blackness) and it was but as if such a blow had been given on a Body newly dead; which does not use to cause such a symptom of a bruise, after the blood ceases to circulate.

Having done with the Head, they open'd the Breast, and found that burning to reach quite through the skin, which was in those scorched places hard and horney, and shrunk up, so as it was not so thick as the soft skin about it: but no appearance of any thing deeper than the skin; the Muscles not at all disordered or discolour'd (perhaps, upon the reason, that was but now said of the Head, Neck and Shoulder.) Having then taken off the Sternum, the Lungs and Heart appeare'd all well, and well-coloured without any disorder.

This is the sum of what was observed; only that the whole Body was, by night, very much swell'd, more than in the morning; and smelt very strong and offensively: Which might be by the hotness of the weather, and by the heat of the place occasion'd by the multitude of people.

**An Experiment**

*To examine, what Figure, and Celerity of Motion begetteth, or encreaseth Light and Flame.*

This was communicated by Dr. Beale, as follows:

May 5. 1665. fresh Mackrels were boil'd in Water, with salt and sweet herbs; and, when the Water was perfectly cold, the next morning, the Mackrels were left in the Water for pickle.

May 6. more fresh Mackrels were boil'd in like water; and May 7. both water and Mackrels were put into the former water, together with the former Mackrels. (Which circumstances I do particularize, because, whether, the mixture of the pickle of several ages, and a certain space of time, or whatever else was necessary, and wanting, the trial did not succeed with like effect at other times.)

But now on the next Munday (May 8.) evening, the Cook stirring the water, to take out some of the Mackrels, found the water at the first motion become very luminous, and the Fish shining through the water, as adding much to the Light, which the water yielded. The water by the mixture of Salt and Herbs, in
in the boyling, was of it self thick and rather blackish, than of any other clear colour: yet being stirr'd, it shin'd, and all the fish appear'd, more brightly luminescent in their own shapes.

Wherever the drops of this water (after it was stirr'd) fell on the ground, or benches, they shin'd: And the children took drops in their hands, as broad as a penny, running with them about the house, and each drop, both near and at distance, seem'd by their shining as broad as a six pence, or a shilling, or broader.

The Cook turn'd up the side of the fish, which was lowest, and thence came no shining: and after the water was for some good time settled, and fully at rest, it did not shine at all.

On Tuesday night (May 9.) we repeated the same Trial, and found the same effects. The water, till it was stirr'd, gave no light, but was thick and dark, as we saw by day-light, and by candle-light. As soon as the Cook's hand was thrust into the water, it began to have a glimmering; but being gently stirr'd by the hand moving round (as the Dairy-maids do to gather the Curds for Cheesef) it did so shine, that they, who look'd on it at some distance, from the further end of another room, thought verily, it was the shining of the Moon through a Window upon a Vessel of Milk; and by brisker Circulation it seem'd to flame.

The Fish did then shine as well from the Inside, as the Outside, and chiefly from the throat, and such places, as seem'd a little broken in the boyling.

I took a piece, that shin'd most, and fitted it, as well as I could devise in the night, both to my great Microscope, and afterwards to my little one; but I could discern no light by any of these Glasses; nor from any drops of the shining water, when put into the Glasses. And May 10. in the brightest rays of the Sun, I examin'd, in my great Microscope, a small broken piece of the Fish, which shin'd most the night before. We could find nothing on the surface of the Fish very remarkable. It seem'd whitish, and in a manner dried, with deep inequalities. And others, as well as myself, thought, we saw a stream, rather darkish, than luminous, arising, like a very small dust, from the fish: And rarely here and there, a very small, and almost imperceptible sparkle in the Fish. Yet of these sparkles we are certain; we numbred them, and agreed in the number, order, and place. Of the Steam I am not confident, but do suspect our eyes in the
bright Sun, or that it might be some dust in the Aire.

The great Microscope being fitted in the day-light for this piece of Fish, we examin’d it that night, and it yielded no light at all, either by the view of the Glass, or otherwise.

Finding it dry, I thought that the moisture of Spittle, and touching of it, might cause it to shine: and so it did, though but a very little, in a few small sparks, which soon extinguish’t. This we saw with the bare eye; not in the Glass.

The Fish were not yet fetide, nor insipid to the best discerning palats: And I caused two Fish to be kept for further Tryall, two or three daies longer, till they were fetide in very hot weather, and then I expected more brightness, but could find none, either in the water, by stirring it, or in the Fish, taken out of the water.

And some Trials I made afterwards with other boyld Mackrels (as is above said) with like pickle, but fail’d of the like success.

This season serves for many Trials in this kind, and by better Microscopes, or better ordered. And in these Vulgarities we may perhaps as well trace out the cause and nature of Light, as in Jewels of greatest value, &c.

Some Considerations


In Num. 9. of these Transactions were publish’d the Schemes and Descriptions of certain Ways of Sounding the Depth of the Sea without a Line; and of Fetching up Water from the bottom of it; together with some Experiments already made with the former of these two Contrivances. The Author of the French Journal des Scavans found good, to insert them both in his Journal of May 3; but in another of May 24. intimates, that the said Schemes and their Descriptions are not very clear and intelligible (he means, that they were not well understood by French Readers;) proposing also some Difficulties, relating to that Subject, and esteemed by him necessary to be satisfied, before any use could be made of the said Instruments.

Upon this occasion, the Author of these Tracts thinks fit, here to represent.

First,
First, That English-men and such others, as are well versed in the English tongue, find no difficulty in understanding the descriptions of these Engines, nor in apprehending their structure, exhibited by the Figures, especially if notice be taken of the Emendation, expressed at the end of Num. 10, about the misgiving the Bended end of the Springing Wire; (which it seems has not been noted in France, though the said Num. 10 is known to have been seen there a pretty while before their Journal of May 24, was publish'd.) And as for the particular of the Bucket, fetching water from the bottom of the Sea, both the Figure and the annexed Description thereof are so plain and clear, that 'tis some wonder here, that any difficulty of understanding them is pretended by any, that hath but ordinary skill in Cutts and the English language. Mean while, that way, which the French Author recommends for this purpose as more simple, Videt. a Brass Pump with double Valves, is not at all unknown in England, nor has bin left untried there; but was found inconvenient, in respect that the Valves in descending did not fully open, and give the water a free passage through the Cavity of the Vessel, nor in ascending shut so close, as to hinder the water from coming in at the top: Whereas by the way, proposed in Num. 9, both is perform'd with great ease and security.

Secondly, Whereas the French Author is of opinion, that 'tis unknown, How much time a Heavy Body requires to sink in water, according to a certain depth; He may please to take notice, that that hath been made out in England by frequent Experiments; by which, several Depths, found by this Method of founding without a Line, were examin'd by trying them over again in the same place with a Line, after the common way. And as to that Query of his, Whether a heavy Body descends in the same Proportion of Swiftness in Water, that it would do in Air? The answer is, that it does not; but that, after it is sunk one or two fathoms into the Water, it has there arrived to its greatest Swiftness, and keeps, after that, an equal degree of velocity; the Resistance of the water being then found equal to the Endeavour of the heavy Body downwards.

Thirdly, When the same Author alleages that it must be known, when a Light Body reascends from the bottom of the water to the
the top, in what proportion of time and swiftness it rises. He seems not to have consider'd, that in this Experiment, the times of the descent and ascent are both taken and computed together; so that, for this purpose, there needs not that nicety, he discourses of.

Fourthly, Whereas 'tis further excepted, That this way of Sounding depths is no new invention; The answer is ready, that neither is it pretended to be so, in the often quoted Tract; it being only intimated there, that the manner of performing it, as 'tis in that place represented and described, is new.

Lastly, To rectifie the said Author's mistake, as if the instrument of Fetching up water from the bottom of the Sea, were chiefly contriv'd, to find out, Whether in some places of the Sea any Sweet water is to be met with at the bottom: There will need no more, than to direct him to the Book itself Num. 9. where p. 149. towards the end, the First use of this Bucket is expressed to be, to know the degrees of Saltness of the water according to its nearness to the top or bottom; or rather to know the constitution of the Sea-water in several depths of several Climates, which is a matter, much better to be found out by Triall, than Discourse. Neither is it any where argued in that Book (as the French Journal insinuates) that, because sweet water is found at the bottom of the Sea of Babarem, therefore it must be found there, but only that it may, be found so elsewhere. And since the same Journal admits, that those Sweet water-springs, which yield the sweet water, that is found at the said place, have been formerly on the Continent, far enough from the sea, which hath afterwards cover'd them: It will be, 'tis presum'd, lawful to ask, Why in many other places there may not be found the like? And besides, how we do know, but that there may be in other parts, Eruptions of large springs at the bottom of the Sea, as well as there.

Printed with Licence for John Martyn, and James Allestry, Printers to the Royal Society. 1666.
The Figures of the Observations made in London - Transact Apr 1700

The Observation of Jupiter.

The Figures of the Italian Observations.

The late Observ of Saturne.
The Contents.

An Account of a New kind of Baroscope, which may be call'd Statical; and of some Advantages and Conveniencies it hath above the Mercurial; communicated by Mr. Boyle. The Particular Observations of the Planet Mars, formerly intimated to have been made by Mr. Hook in February and March last. Some Observations, made in Italy, confirming the former; and with all fixing the Period of the said Planet's Revolution. Observations, lately made at London, of the Planet Jupiter; as also of Saturn. A Relation of a sad Effect of Thunder and Lightning. An Account of some Books, lately publish'd; videlicet: The Relations of divers Curious Voyages, by Mons. Thevenot: A Discourse about the Cause of the Inundation of the Nile, by Mons. de la Chambre, both French; De Principiis & Ratiocinatione Geometrarum, Contra Faustum Professorum Geometrice, by Mr. Hobbes: King Salomon's Portrait of Old Age, by J. Smith, M.D.

An Account

Of a New kind of Baroscope, which may be called Statical; and of some Advantages and Conveniencies it hath above the Mercurial: Communicated some while since, by the Honourable Robert Boyle.

S for the New kind of Baroscopes, which, not long agoe, I intimated to you, that my haste would not permit me to give you an account of, since your Letters acquaint me, that you still design a Communicating to the
Curious as much Information, as may be, in reference to Baroscopes; I shall venture to send you some Account of what I did but name (in my former Letter) to you.

Though by a Passage, you may meet with in the 19th and 20th Pages of my Thermometrical Experiments and Thoughts, you may find, that I did some years agoe think upon this New kind of Baroscope; yet the Changes of the Atmosphere’s Weight not happening to be then such, as I wish’d, and being unwilling to deprive my self of all other use of the exactest Ballance *, that I (or perhaps any man) ever had, I confess to you, that successive avocations put this attempt for two or three years out of my thoughts; till afterwards returning to a place, where I chanc’d to find two or three pairs of Scales, I had left there, the sight of them brought it into my mind; and though I were then unable to procure exacter, yet my desire to make the Experiment some amends for so long a neglect, put me upon considering, that if I provided a Glass-buble, more than ordinary large and light, even such Ballances, as those, might in some measure perform, what I had tried with the strangely nice ones above-mention’d.

I caused then to be blown at the Flame of a Lamp some Glass-bubles as large, thin and light, as I could then procure, and choosing among them one, that seem’d the least unfit for my turn, I counterpoised it in a pair of Scales, that would loose their Equilibrium with about the 30th part of a Grain, and were suspended at a Frame. I placed both the Ballance and the Frame by a good Baroscope, from whence I might learn the present weight of the Atmosphere. Then leaving these Instruments together; though the Scales, being no nicer than I have express’d, were not able to shew me all the Variations of the Air’s weight, that appear’d in the Mercurial Baroscope, yet they did what I expected, by shewing me variations no greater, than alter’d the height of Quicksilver half a quarter of an Inch, and perhaps much smaller, than those: Nor did I doubt, that, if I had had either tender Scales, or the means of supplying the Experiment with convenient accommodations, I should have discerner
Far small' rer Alterations of the Weight of the Air, since I had the pleasure to fee the Buble sometimes in an *equilibrium* with the counterpoise; sometimes, when the Atmosphere was high, preponderate so manifestly, that the Scales being gently stirr'd, the Cock would play altogether on that side, at which the Buble was hung; and at other times (when the Air was heavier) that, which was at the first but the Counterpoise, would preponderate, and, upon the motion of the Ballance, make the Cock vibrate altogether on its side. And this would continue sometimes many daies together, if the Air so long retain'd the same measure of gravity; and then (upon other changes) the Buble would regain an *equilibrium*, or a preponderance: so that I had oftentimes the satisfaction, by looking first upon the Statical Baroscope (as for distinctions fake it may be call'd) to foretell, whether in the *Mercurial* Baroscope the Liquor were high or low. Which Observations though they hold as well in Winter, and several times in Summer (for I was often absent during that season) as the Spring, yet the frequency of their Vicissitudes (which perhaps was but accidental) made them more pleasant in the latter of these seasons.

So that, the matter of Fact having been made out by variety of repeated Observations, and by sometimes comparing several of those new Baroscopes together, I shall add some of these Notes about this Instrument, which readily occur to my memory, reserving the rest till another opportunity.

And *First*, if the ground, on which I went in framing this Baroscope, be demanded, the answer in short may be; 1. That, though the Glass-buble, and the Glass-counterpoise, at the time of their first being weigh'd, be in the Air, wherein they both are weigh'd, exactly of the same weight; yet they are nothing near of the same bulk; the Buble, by reason of its capacious cavity (which contains nothing but Air, or something that weighs less than Air) being perhaps a hundred or two hundred times (for I have not conveniency to measure them) bigger than the Metalline counterpoise. 2. That according to a *Hydrostatical Law* (which you know I have lately had occasion to make out) If two Bodies of equal gravity, but unequal bulk come to be weigh'd in another *Medium*, they will be no longer equi-
equiponderant; but if the new Medium be heavier, the greater Body, as being lighter in Specie, will loose more of its weight, than the lesser and more compact; but if the new Medium be lighter than the first, then the bigger Body will outweigh the lesser: And this disparity, arising from the change of Medium's, will be so much the greater, by how much the greater inequality of bulk there is between the Bodies formerly equiponderant.

3. That, laying these two together, I consider'd, that 'twould be all one, as to the effect to be produced, whether the Bodies were weighed in Medium's of differing gravity, or in the same Medium, in case its (specific) gravity were considerably alter'd: And consequentially, that since it appear'd by the Baroscope, that the weight of the Air was sometimes heavier, and sometimes lighter, the alterations of it, in point of gravity, from the weight, it was off at first counterpoising of the Buble of it, would unequally affect so large and hollow a Body, as the Buble, and so small and dense a one, as a Metallin weight: And when the Air by an increase of gravity should become a heavier Medium, than before, it would buoy up the Glass more than the Counterpoises; and if it grew lighter, than it was at first, would suffer the former to preponderate: (The Illustrations and Proof can scarce be added in few words; but, if it be desired, I may, God permitting, send you them at my next leisure:) And though our English Air be about a thousand times lighter, than water, the difference in weight of so little Air, as is but equal in bulk to a Buble, seem'd to give small hopes, that it would be sensible upon a Ballance; yet, by making the Buble very large and light, I supposed and found the Event, I have already related.

Secondly, The hermetically seal'd Glass-buble, I employed, was of the bigness of a somewhat large Orange, and weigh'd about 1. drachme and 10. grains. But I thought it very possible, if I had been better furnish'd with conveniencies (wherein I afterwards found, I was not mistaken) to make (among many, that might be expected to miscarry) some, that might be preferable to this, either for capacity or lightnes, or both; especially if care be taken, that they be not seal'd up, whilst they are too hot. For, though one would think, that it were advantagous.
racious to rarify and drive out the Air as much as is possible, because in such seal’d Bubles the Air itself (as I have elsewhere shewn) has a weight; yet this advantage countervails not the inconvenience of being obliged to increase the weight of the Glass, which when it includes highly rarified Air, if it be not somewhat strong, will be broken by the pressure of the External Air, as I have sufficiently tried.

Thirdly, I would have tried, whether the Dryness and Moisture of the Air would in any measure have alter’d the weight of the Buble, as well as the Variation of Gravity produced in the Atmosphere by other causes; but the extraordinarily constant absence of Fogs, kept me from making Observations of this kind; save that one morning early, being told of a mist, I sent to see (being myself in bed) whether it made the Air so heavy as to buoy up the Buble; but did not learn, that that mist had any sensible operation on it.

Fourthly, By reason of the difficulties and casualties, that may happen about the procuring and preserving such large and light Bubles, as I have been lately mentioning; it may in some cases prove a convenience to be inform’d, That I have sometimes, instead of one sufficiently large Buble, made use of two, that were smaller. And, though a single Buble of competent bigness be much preferable, by reason that a far less quantity and weight of Glass is requisite to comprise an equal capacity, when the Glass is blown into a single Buble, than when it is divided into two; yet I found, that the employing of two instead of one, did not so ill answer my expectations, but that they may for a need serve the turn instead of the other; than which they are more easier to be procured: And if the Ballance be strong enough to bear so much Glass, without being injur’d: by employing two or a greater number of large Bubles, the effect may be more conspicuous, than if only a single Buble: (though a very good one) were employed.

This instrument may be much improved by divers Accommodations. As

First, There may be fitted to the An/a (or Checks of the Ballance) an Arch (of a Circle) divided into 17, or 20. deg; (more or less, according to the goodness of the Ballance) that the Cock resting over against these Divisions, may readily and
and without Calculation shew the quantity of the Angle, by which, when the Scales propend either way, the Cock declines from the Perpendicular, and the Beam from its Horizontal parallelism.

Secondly, Those, that will be so curious, may, instead of the Ordinary Counterpoise (of Brass) employ one of Gold, or at least of Lead, whereof the latter being of equal weight with Brass, is much less in Bulk, and the former amounts not to half its bigness.

Thirdly, These parts of the Balance, that may be made of Copper or Brass, without any prejudice to the exactness, will, by being made of one of those Mettals, be less subject, than Steel, (which yet, if well hardned and polish'd, may last good a great while) to rust with long standing.

Fourthly, Instead of the Scales, the Buble may be hung at one end of the Beam, and only a Counterpoise to it at the other, that the Beam may not be burthen'd with unnecessary weight.

Fifthly, The whole Instrument, if placed in a small Frame, like a square Lanthorn with Glass-windows, and a hole at the top for the Commerce of the internal and external Air, will be more free from dust, and irregular agitations; to the latter of which, it will otherwise be sometimes incident.

Sixthly, This instrument being accommodated with a light Wheele and an Index (such as have been applyed by the excellent Dr. Cbr. Wren to open Weather glasses, and by the ingenious Mr. Hook to Baroscopes) may be made to shew much more minute variations, than otherwise.

Seventhly, And the length of the Beam, and exquifitness of the Ballance, may easily, without any of the foregoing helps (and much more with them) make the instrument far exacter, than any of those, I was reduced to employ. And to these Accommodations divers others may be suggested by a farther consideration of the nature of the thing, and a longer practice.

Though in some respects this Statical Baroscope be inferior to the Mercurial; yet in others it has its own advantages and conveniencies above it.

And 1. It confirms ad oculum our former Doctrine, that the falling and rising of the Mercury depends upon the varying weight of the Atmosphere; since in this Baroscope it cannot be
be pretended, that a Fuga vasui, or a Funiculus, is the cause of
the changes, we observe. 2. It shews, that not only the Air has
weight, but a more considerable one, than some Learned men;
who will allow me to have prov'd, it has some weight, will
admit; since even the variation of weight in so small a quantity
of Air, as is but equal in bulk to an Orange, is manifestly dis-
coverable upon such Ballances, as are none of the nicest. 3. This
Statical Baroscope will oftentimes be more parable, than the
other: For many will finde it more easie, to procure a good
pair of Gold-scales, and a Buble or two, than a long Cane
feal'd, a quantity of Quick-silver, and all the other requisits of
the Mercurial Baroscope; especially if we comprize the trouble
and skill, that is requisite to free the deserted part of the Tube
from Air. 4: And whereas the difficulty of removing the Mer-
curial Instrument has kept men from so much as attempting to
do it, even to neighbouring places; the Essentia parts of the
Scale-Baroscope (for the Frame is none of them;) may very
easily in a little room be carried, whither one will, without the
hazard of being spoil'd or injur'd. 5. There is not in Statical
Baroscopes, as in the other, a danger of uncertainty, as to the
goodness of the Instruments, by reason, that in these the Air is,
in some more, and in some less perfectly excluded; whereas in
these, that consideration has no place. (And by the way, I have
sometimes, upon this account, been able to discover by our
new Baroscope, that an esteem'd Mercurial one, to which I com-
pared it, was not well freed from Air.) 6. It being, as I former-
ly intimated, very possible to discover Hydrostatically, both the
bigness of the Buble, and the Contents of the cavity, and the
weight and dimensions of the Glassie substance (which toge-
 ther with the included Air make up the Buble,) much may be
discover'd by this Instrument, as to the Weight of the Air, abso-
lute or respective. For, when the Mercury in the Mercurial Ba-
roscope is either very high, or very low, or at a middle station
between its greatest and leaft height, bringing the Scale-Baro-
meter to an Exact Equilibrium; (with very minute divisions
of a Graine,) you may, by watchfully observing, when the
Mercury is risen or fallen just an inch, or a fourth, or half an inch
&c. and putting in the like minute divisions of a Grain to the
lighter Scale; till you have again brought the Ballance to an
exquisite.
exquisite Equilibrium; you may, I say, determine, What known weight in the Statical Baroscope answers such determinate Altitudes of the ascending and descending Quick-silver in the Mercurial. And if the Ballance be accommodated with a divided Arch, or a Wheel and Index, these Observations will assist you for the future to determine readily, by seeing the inclination of the Cock or the degree mark’d by the Index, what pollency the Buble hath, by the change of the Atmospheres weight, acquired or lost. Some Observations of this nature I watchfully made, sometimes putting in a 64th sometimes a 32th sometimes a 16th: and sometimes heavier parts of a Grain, to the lighter Scale. But one, that knew not, for what uses those little papers were, coming to a window, where my Baroscopes stood, so unluckily shook them out of the Scales, and confused them, that he robb’d me of the opportunity of making the nice Observations I intended, though I had the satisfaction of seeing, that they were to be made. 7. By this Statical Instrument we may be assisted to compare the Mercurial Baroscopes of several places (though never so distant) and to make some Estimates of the Gravities of the Air therein. As if, for instance, I have found by Observation, that the Buble, I employ, (and one may have divers Bubles of several sizes, that the one may repair any mischief, that may happen to another) weigh’d just a Drachme, when the Mercurial Cylinder was at the height of 29½ inches (which in some places I have found a moderate altitude;) and that the Addition of the 16th part of a gr. is requisite to keep the Buble in an Equilibrium, when the Mercury is risen an 8th, or any determinate part of an inch above the former station: When I come to another place, where there is a Mercurial Barometer, as well freed from Air as mine (for that must be supposed) if taking out my Scale instrument, it appears to weigh precisely a Drachme, and the Mercury in the Baroscope there stand at just 29½ inches, we may conclude the Gravity of the Atmosphere not to be sensibly unequal in both those two places, though very distant. And though there be no Baroscope there, yet if there be an additional weight, as for instance, the 16th part of a Grain requisite to be added to the Buble, to bring the scales to an Equilibrium, it will appear that the Air at this second place is, at that time
More heavier, than the Air of the former place was, when the Mercury stood at \(29\frac{1}{2}\) inches.

But in making such comparisons, we must not forget to consider the Situation of the several places, if we mean to make Estimates not only of the weight of the Atmosphere, but of the weight and density of the Air. For, though the Scales will shew (as has been said) whether there be a difference of weight in the Atmosphere at the two places; yet, if one of them be in a Valley or bottom, and the other on the top or some elevated part of a Hill, it is not to be expected, that the Atmosphere, in this latter place, should gravitate as much, as the Atmosphere in the former, on which a longer Pillar of Air does lean or weigh.

And the mention, I have made of the differing Situation of Places, puts me in mind of something, that may prove another use of our Statical Baroscope, and which I had thoughts of making trial of, but was Accidentally hindered from the opportunity of doing it. Namely, that by exactly pouring the Bubble at the foot of a high Steeple or Hill, and carrying it in its close Frame to the top, one may, by the weight requisite to be added to Counterpoise there to bring the Beam to its Horizontal position, observe the difference of the weight of the Air at the bottom, and at the top; and, in case the Hill be high enough, at some intermediate Stations. But how far this may assist men, to estimate the Absolute or Comparative height of Mountains, and other elevated Places; and what other Uses the Instrument may be put to, when it is duly improved; and the Cautions, that may be requisite in the several cases, that shall be proposed, I must leave to more leisure, and farther Consideration.

The Particulars.

Of those Observations of the Planet Mars, formerly intimated to have been made at London in the Months of February and March A. 1666.

To perform, what was promised Num. 11. of these Papers, pag. 198; 'tis thought fit now to publish the Particular Observations, concerning the spots in Mars, and their motion, as they were made with a 36 foot Telescope, and produced in writing
writing before the Royal Society, the 28 March 1666. by Mr. Hook, as follows;

Having a great desire (faith he) to observe the Body of Mars, whilst Acronyca and Retrograde (having formerly with a Glass of about 12. foot long, observ’d some kind of Spots in the Face of it,) though it be not at present in the Perihelium of its Orbe, but nearer its Aphelium, yet I found, that the Face of it, when near its Opposition to the Sun (with a Charge, the 36. foot-glafs, I made use of, would well bear) appeared very near as big, as that of the Moon to the naked eye; which I found, by comparing it with the Full Moon, near adjoyning to it, March 10.

But such had been the ill disposition of the Air for several nights, that from more than 20. Observations of it, which I had made since its being Retrograde, I could find nothing of satisfaction, though I often imagin’d, I saw Spots, yet the Influent veins of the Air (if I may so call those parts, which, being interspers’d up and down in it, have a greater or less Refractive power, than the Air next adjoyning, with which they are mixt) did make it so confus’d and glaring, that I could not conclude upon any thing.

On the third of March, though the Air were still bad enough yet I could see now and then the Body of Mars appearing of the form A: which I presently described by a Scheme: and about 10. minutes after, as exactly representing what I saw through the Glass, as I could, I drew the Scheme B. This I was sufficiently satisfied (by very often observing it through the Tube, and changing my Eye into various positions, so there might be no kind of Fallacy in it) could be nothing else, but some more Dusky and Spotted parts of the Face of this Planet.

March 10. finding the Air very bad, I made use of a very shallow Eye-glass, as finding nothing Distinct with the greater Charge; and saw the appearance of it as in C, which I imagin’d, might be the Representation of the former Spots by a lesser charge. About 3 of the Clock the same morning, the Air being very bad (though to appearance exceeding clear, and causing all the Stars to twinkle, and the minute Stars to appear very thick) the Body seem’d like D; which I still suppos’d to be the
the Representation of the same Spots through a more confused and glaring Air.

But observing March 21. I was surprized to find the Air (though not so clear, as to the appearance of small Stars) so exceeding transparent, and the Face of Mars so very well defined, and round, and distinct, that I could manifestly see it of the shape in E. about half an hour after Nine at night. The Triangular spot on the right side (as it was inverted by the Telescope, according to the appearances, through with all the preceding Figures are drawn) appear'd very black and distinct, the other towards the left more dim; but both of them sufficiently plain and defined. About a quarter before 12. of the Clock the same night, I observ'd it again with the same Glass, and found the appearance exactly, as in F; which I imagin'd to shew me a Motion of the former triangular spot: But designing to observe it again about 3. of the Clock the same Morning, I was hindered by cloudy weather.

But March 22. about half an hour after 8. at night, finding the same Spots in the same posture, I concluded, that the preceding Observation was only the appearance of the same Spots at another height and thickness of the Air: And thought my self confirm'd in this Opinion, by finding them in much the same posture, March 23. about half an hour after 9, though the Air was nothing so good as before.

And though I desired to make Observations, about 3. of the Clock those mornings; yet something or other interven'd, that hindered me, till March 28. about 3 of the Clock, the Air being light (in weight) though moist and a little hazy, when I plainly saw it, to have the form, represented in I; which is not reconcileable with the other Appearances, unless we allow a Turbinated motion of Mars upon its Center: Which, if such there be, from the Observations made March 21. 22. and 23. we may guess it to be once or twice in about 24. hours unless it may have some kind of Librating motion; which seems not so likely. Now, whether certainly so or not, I shall endeavour, as oft as I have opportunity, further to observe.

A particular direction to the Figures mentioned in the precedent discourse.

A. March 3d. 00h. 20m. in the morning, the Air having many inflec-
ing parts dispersed up and down in it: by the Wheel Barometer, heavy.

B. Another Scheme, which I drew from my Observation, about 10. minutes after, the same morning. Both these were observed with a very deep Eye-glass.

C. March 10th. 00h. 20m. in the morning: the Air heavy and insulphite. Use was made of a shallow or ordinary Charge.

D. March 10th. 3h. 00m. in the Morning: the Air very heavy and Insulphite, which made it glare and radiate, and be more confused than about 3. hours before. A shallow Charge.

E. March 21st. 9½h. post merid: the Air light (in weight) and clear, without insulphiting parts; the Face appear'd most distinctly of this Forme. A shallow Charge.

F. March 21st. 11½h. post merid: the Air continuing very light and clear, without insulphiting vapours. A shallow Charge.

G. March 22nd. 8½h. post mer. the Air clear, with few insulphiting veins in it, and indifferent light. A shallow Charge.

H. March 23rd. 9½h. post mer. the Air pretty light, but moist, and somewhat thick and hazy, but seem'd to have but few veins, or insulphiting parts.

I. March 28th. 3h. p. m. much the same kind of Air with that of March 23; light, moist, and a little hazy, with some very few veins.

Observations
Made in Italy, confirming the former, and withall fixing the Period of the Revolution of Mars.

These Observations we shall summarily present the Curious in these parts with, as they were lately presented (by Letter from his Excellency the Ambassadour of Venice, now residing at the Court of France) to the Royal Society, in some printed sheets of Paper, entituled, MARTIS, circa Axem proprium Revolubilis, Observationes, BONONIÆ à DOMINICO CASSINO habitæ; come to hand June 3. 1666.

In these Papers the Excellent Caffini affirms:
1. That with a Telescope of 24. Palmes, or of about 16 Foot, wrought after S. Campani's way, he began to observe February 6. 1666 (ft.-n.) in the morning, and saw two dark Spots in the first Face of Mars.

2. That
2. That with the same Glass he observ'd Febr. 14, in the Evening, in the other Face of this Planet, two other Spots, like those of the first, but bigger.

3. That afterwards continuing the Observations, he found the Spots of these two Faces to turn by little and little from East to West, and to return at last to the same situation, wherein he had seen them first.

4. That S. Campani, having also observ'd at Rome with Glasses of 50. Palmes or about 35 Foot, likewise of his own contrivance, had seen in the same Planet the same Phenomena.

5. That sometimes he hath seen, during the same night, the two Faces of Mars, one, in the Evening, the other in the Morning.

6. That the Motion of these Spots in the inferior part of the apparent Hemisphere of Mars, is made from East to West, as that of all the other Celestial Bodies, and is performed by Parallels, that decline much from the Equator, and little from the Ecliptick.

7. That the Spots return the next day to the same situation, 40 minutes later, than the day before; so that in every 36. or 37. days, about the same hour, they come again to the same place.

8. He promises shortly to give us the particular Tables of this Motion and of its Inequalities, together with the Ephemerides themselves.

9. He represents, that some other Astronomers have also made at Rome several Observations of these Spots of Mars, from March $\frac{14}{44}$ to March $\frac{38}{44}$, with Glasses, wrought by Eustachio Divini, of 25. and 45. Palmes. Which Spots he makes little differing from his own, of the first Face; as will by and by appear, by the direction to the Schemes.

10. But he adds, that those other Roman Astronomers, that have observ'd with Divini's Glasses, will have the Conversion of Mars to be performed, not in 24 h. 40 m. (as he maintains it is) but in about 13 h.

11. And to evince, that they are mistaken in these Observations of theirs; he alludes, That they assure that the Spots, which they have seen in this Planet, (by an Eustachian Telescope) the $\frac{28}{44}$ of March, were small, very distant from one another, remote from the middle of the Disk, and the Oriental Spot was less than the Occidental (as is represented by the Fig. O, like that of the first Face of Mars.) whereas, on the contrary,
He (Cassini) pretends to evidence by his Observations made at the same time at Bononia, that, the same day and hour, those Spots were very large, near one another, in the midst of the Disk, the Oriental bigger than the Occidental (as appears by Fig. P, which is that of the second Face of Mars.)

12. Besides, he declares, that those Astronomers were too hasty in determining, after 5 or 6 Observations only, in how much time Mars finisht his Revolution, and denies it to be perform'd in 13 hours: adding, that, though Himself had observ'd for a much longer time, than they; yet he durst not for a great while define, Whether Mars made but one Turn in 24 hours 40 minutes or two; and that all, that he could, for a long time affirm, was only this, that after 24 h. 40 m. this Planet appear'd in the same manner he did before.

13. But since those first Observations, He affirms to have found cause to determine, that the Period of this Conversion is made in the said space of 24 h. 40 m; and not oftner than once within that time; Alledging for proof,

1. That, whereas Febr. 6. (ft.n.) he saw the Spots of the first Face of Mars, moving from eleven of the Clock in the night, until break of day, they appear'd not afterwards in the Evening after the rising of that Planet (witness several intelligent persons, which he names, that were present at the Observations) Whence he infers, that after 12 hours and 20 minutes, the same Spots did not come about; since that the same, which in the morning were seen in the middle, upon the rising of Mars; after 13 or 14 hours, might have appear'd near the Occidental Limb. But, because he might be imposed upon by Vapors, whilst Mars was yet so near the Horizon, he gives this other determination, vid.

2. Whereas he saw the first Face of Mars, the 6 of February at 11 of the clock of the night following; he did not see the same after 18 days at the same hour, as he ought to have done, if the Period were absolved in the space of 12 h. 20 m.

3. Again, whereas he saw Febr. 24. in the Evening, the other Face of Mars, he could not see the same, the 13. and 15. day of March, to wit after 17 and 19 days; as he should have done, if the Revolution were made in the newly mention'd time.

4. Again, whereas the 27. of March in the Evening he saw the
the second Face of Mars, he could not see it the 14. and 16. of April.

From all which Observations he Judges it to be evident, that the Period of this Planet's Revolution is not perform'd in the space of 12. hours 20. minutes, but in about 24 hours 40 minutes; more exactly to be determin'd by comparing distant Observations: And that those who affirm the former, must have been deceived by not well distinguishing the two Faces, but that having seen the second, taken it for the first:

All which he concludes with this Advertisement, that, when he defines the time of the Revolution of Mars, he does not speak of its Mean Revolution, but only of that, which he observ'd, whilst Mars was opposite to the Sun; which is the shortest of all.

The Figures of the Principal Observations, represented in the Book here discoursed of, may be seen in the annexed Scheme; videlicet.

K. One of the Faces of Mars, as S. Caffini observ'd it March 3. (f. n.) 1666. in the Evening, with a Glass of 24 Palmes.

L. The other Face, as he saw it Febr. 14. in the Evening.

M. The first Face, as S. Campani saw at Rome, March 3. 1666. in the Evening, with a Glass of 50 Palmes.

N. The second Face, as the same Campani observ'd it March 15. in the Evening.

O. The Figure of Mars, as it was seen at Rome by a Telescope of Divini of 45 Palmes, March 16.

P. The Figure of the said Planet, as it was seen the same day and hour at Bononia by Caffini; being that of the second Face.

Some Observations

Lately made at London concerning the Planet Jupiter.

These, as they were made, so they were imparted, by Mr. Hook, as follows:

A. 1666. June 26. between 3. and 4. of the Clock in the morning, I observed the Body of Jupiter through a 60. foot-glass; and found the apparent Diameter of it through the Tube, to be somewhat more than 2. degrees, that is, about four times
times as big, as the Diameter of the Moon appears to the naked Eye. I saw the Limb pretty round, and very well defined without radiation. The parts of the Phases of it had various degrees of Light. About 1. and 3. the North and South poles of it, (in the Fig. 2.) it was somewhat darker, and by degrees it grew brighter towards b. and e. two Belts or Zones; the one of which (b) was a small dark Belt crossing the Body Southward; adjoining to which was a small Line of a somewhat lighter part; and below that again, Southwards, was the great black Belt c. Between that, and e, the other smaller black Belt, was a pretty large and bright Zone; but the middle d, was somewhat darker than the edges. I perceive'd, about 3 h. 15 m. near the middle of this, a very dark round Spot, like that represented at g, which was not to be perceive'd about half an hour before. And I observed it, in about 10 minutes time to be gotten almost to d, keeping equal distance from the Satellites h, which moved also Westwardly, and was joyn'd to the Disk at i, at 3 h. 25 m. After which, the Air growing very hazy, and (as appeared by the Baroscope) very light also (in weight) I could not observe it: So that it was sufficiently evident, that this black Spot was nothing else, save the shadow of the Satellites h, Eclipsing a part of the Face of Jupiter. About two hours before, I had observed a large darker Spot in the bigger Belt about k, which in about an hour or little more (for I did not exactly observe the time, nor draw the Figure of it) moving Westwards, disappear'd. About a week before, I discover'd also, together with a Spot in the Belt c, another Spot in the Belt e, which kept the same way and velocity with that of the Belt c. The other three Satellites in the time of this Eclipse, made by the Satellites, were Westwards of the Body of Jupiter, appearing as bright through the Tube, as the Body of Jupiter did to the naked Eye, and I was able to see them longer through the Tube, after the daylight came on, than I was able to see the Body of Jupiter with my naked eye.

A late Observation about Saturn made by the same.

June 29 1666. between 11. and 12. at night I observed the Body of Saturn through a 60. foot Telescope, and found it exactly
of the shape represented in the Figure R. The Ring appear'd of a somewhat brighter Light than the Body; and the black lines a a, crossing the Ring, and b b crossing the Body (whether Shadows or not, I dispute not) were plainly visible, whence I could manifestly see, that the Southern-most part of the Ring was on this side of the Body, and the Northern part, behind, or covered by the Body:

A Relation

Of a sad effect of Thunder and Lightning:

This Relation was written by that worthy Gentleman, Thomas Neale Esquire, (the then High Sheriff of the County of Hampshire, when this disaster hapned) to a Friend of his in London, as follows;

On the 24 of January 1662, one Mr. Brooks of Hampshire, going from Winchester towards his house near Andover in very bad Weather, was himself slain by Lightning, and the Horse, he rode on, under him. For about a mile from Winchester he was found with his Face beaten into the ground, one leg in the stirrup, the other in the Horse's mane; his Cloaths all burnt off his back, not a piece as big as a handkerchief left entire, and his hair and all his body sing'd. With the force, that struck him down, his nose was beaten into his face, and his Chin into his Breast; where was a wound cut almost as low, as to his Navel; and his cloaths being, as aforesaid, torn, the pieces were so scatter'd and consum'd, that not enough to fill the crown of a hat could be found. His gloves were whole, but his hands in them sing'd to the bone. The hip-bone and shoulder of his Horse burnt and bruised; and his saddle torn in little pieces. This was what appear'd to the Coroners inquest, and so is likely to be as near truth, as any is to be had.

So far this Letter: Which, if it had come soon enough to the hands of the Publisher, would have been joyned to a like Relation, inferred in the next foregoing Papers (Num., 13.) of an accident hapned at a later time. With both which may be compared the Account, formerly published in Latin by the Learned Dr. Charleton, concerning the Boy, that was Thundered.
struck near Nantwich in Cheshire; the Title of the Book being, Anatome Pueri de Calo tači: such Relations, when truly made, well deserving to be carefully recorded for farther consideration.

Of some Books lately publish'd.

RELATIONS OF DIVERS CURIOUS VOYAGES, by Mons. Thevenot, the third Tome, in French. This Book contains chiefly, the Ambassage of the Dutch into China, translated out of the Dutch manuscript: A Geographical description of China, translated out of a Chinese Author by Martinus: And the Account, which the Directors of the Dutch East-India Company made to the States General, touching the state of affairs in the East-Indies, when their late Fleet parted from thence. To touch some things of a Geographical and Philosophical nature, contained therein, we shall take notice;

1. How the Kingdom of China is peopled; there being according to the best computation (which is there made with singular care) above 58 millions of Men, not counting Magistrates, Soldiers, Priests, Eunuchs, Women and Children; so that it may not be altogether strange, if one should affirm, there were 200 millions of people, of all sorts, in that Kingdom.

2. That Catay is nothing else but the Six Northern Provinces of China, separated from the other Nine, by the great River Kang; and that the City Cambalu is the same with that of Peking: the Tartars, who carry every three years their Tribute to the Emperor of China, constantly calling the said Provinces and City by those names of Catay, and Cambalu.

3. That China is so well furnish'd with Rivers, and cut Channels, that men may go from the most Southern to the most Northern part thereof by water, except one day's journey; as the Dutch Ambassadors did, embarking at Canton, which is 23 d. 43 m. Northern Latitude, and landing at Peking, which is about 40 d; having only travelled one day's journey over some Mountains of the Province Kiamf.

4. That the people of China are exceeding industrious Husband
bandmen, making, among other ways of improving their soils, great use of Flouding.

5. That the Physicians of China do cure Sicknesses with much ease, and in a short time: That they have very ancient Books of the nature and virtues of Herbs, Trees and Stones: That their Modern Physicians (as well as their Ancient ones did) write of the Prognosticks, Causes, Effects, &c. of Diseases. That their Remedies consist for the most part of Simples and Decoctions, Cauterries, Frictions; without the use of Blood-letting: That they have such an excellent skill and method in feeling the Pulse, that by the means thereof they discover even the most latent causes of Diseases; taking a good half-hour, when they visit a Patient, in feeling and examining his Pulse: That they prescribe much the use of The; and the drinking always warme, whatever they drink: To the custom of both which it's imputed, that the inhabitants of China do spit very little, nor are subject to the Stone or Gout: That they prize highly the Root Ginseng, as an extraordinary Restorative and Cordial, recovering frequently with it agonizing persons; one pound of it being paid with 3 pounds of silver. As for their Chymists, (of which they have also good store) they go beyond ours, promising not only to make Gold, but to give Immortality.

6. That their Nobility is raised from Learning and Knowledge, without regard to Blood or Parentage, excepting the Royall Family.

7. That in CHEKIAN, a maritime Province, whence is the shortest cut of China to Japan, is the best and plentifullest Silk-trade in the world: And that there every year the Mulberries are cutt, and kept down, that they grow not into Trees for the easier gathering of the Leaves, there being a double Silk-harvest in that Country, as there is in several other parts of the East-indies; (both which there is hope, will shortly be imitated in Virginia.)

8. That the way of making Porcelaine is this: (Which is the rather inserted here, because it agrees so well with an Account, we received a while since from a very Curious and intelligent Person of Amsterdam.) There is in the Province of Nankin a Town, call'd
Goefifoh whence they draw the Earth for Porcelaine, which is found between the Rocks of Mountains. This Earth they beat very small, and stamp it to a very fine Powder, and then put it into Tubs fill’d with water; where the finest part sinks to the bottom. Afterwards ’tis kneaded in the form of small Cubes, of the weight of about 3 Catti (a Catti being 20 Ounces.) These pieces thus wrought are sold to the people, that commonly in great numbers fetch them, coming from the Town Sintefmo (otherwise Fontio) in the Province of Kiansy, being about 50 miles distant from Woting, near the City KIANSY; which people transport them to their homes, and there bake them in this manner: They heat their Ovens well, for the space of 15 daies successively, and then keep them so close, that no Air may get in; and after 15 other daies are pass’d, they open the Oven in the presence of an Officer, who takes every fifth vessel of each fashion for the service of the Emperor: Which done, the rest is sold to those of Ucienien, whence it is transported all over the Country. So that the Earth is not prepared, in Nankin, where ’tis found, because the people of that Province have not the skill of working it, as the other above-mention’d; who also alone have the Art of coloring it, which they keep as a great Secret, not teaching it to any, but their Children and next Kindred.

That Musk is nothing else, but the Testicles of a Beast like a Dear, found in the Province of Honan; and that, when tis good and unmixt, as it comes from the Animal, they sell it even in Nankin and Pekin, for 30. or 35. Teyls (that is, about so many Crowns) the Catti.

Many other curious informations might be borrow’d from this Author, concerning the Customs, Studies, Exercises of the Chinese; of the number of the people of each Province; of the Natural productions of the Earth and Rivers there; of the Structure and Antiquity of their Wall; of the Magnificence of their Porcelain Tower &c; but, remitting for these things to the Book itself, we shall only add a piece of Oeconomy, used by the Holland-Merchants in their Commerce with China; which is, that they dry abundance of Sage-leaves, role them up, and prepare.
prepare them like The, and carrying it to China, as a rare drogue, get for one pound of it, fourtimes as much The.

A DISCOURSE ABOUT THE CAUSES OF THE INUNDATION OF THE NILE, in French. The Author of this Book is Monsieur dela Chambre, who, being perswaded from several Circumstances, that accompany the Overflowing of this River, that it cannot proceed from Rain, ventures to assign for a Cause of it, and of all the other effects that happen at the time of its swelling, the Niter, wherewith that water abounds.

The discourse having six parts, the Author endeavours to shew in the

First, that the Waters of the Nile are Nitrous, explicating the Nature of Salt, and Saltpeter, and imputing the fertility of the Earth, as well as the secundity of Animals, to Salt. Where he shews, that all things, that serve to improve Land, are full of Salt; and that it is observ'd, that grain steep'd in Vrine, before sowing, rises sooner, and becomes fuller and stronger, than else. Adding, that that, which renders the Seed of Animals prolific, is, that one of the Spermatick veins hath its Origine from the Emulgent, through which the Nitrous and Saline Soporities, that discharge themselves into the Kidneys and Bladder, do pass.

In the Second, he examines, what is Fermentation, and how 'tis perform'd; affirming, that, what thrulfs forth Plants in the Spring, is, that the Earth being fermented by the Niter, it harbours, the Nitrous Spirits insinuate themselves into their Pores.

In the Third he treats of all the Circumstances, observable in the Inundation of the Nile. 'Tis affirm'd, that 3 or 4 days before that River begins to overflow, all its water is troubled: that then there falls a certain Dew, which hath a fermenting vertue, and leavens a Paste exposed to the Air: that the Mud, which has been drawn out of the water, grows heavier, when the overflowing begins, then it was before, and that by the increase of the weight of that Mud, they Judge of the greatness of the approaching inundation. The Author pretends, that...
the Niter, which the Nile is stored with, is the cause of all these strange effects, and of many others, by him alleged. For, *faith he*, when the Nitre is heated by the heat of the Sun, it ferments, and mingling with the water, troubles it, and swells it, and makes it pass beyond its banks; after the same manner, as the Spirits in new Wine render it troubled, and make it Boyle in the vessel. And it seems not likely to him, that the Mud, found in the Nile, should come a far off; for then it would at last so raise the banks of this River, that it would not be able to overflow them any longer. Whereas 'tis more than 2000 years, that the banks thereof are not grown higher, there being now requisite but 16 cubits for overflowing the Land, no more than there was in the time of Herodatus. Which shews, *faith he*, that this Mud is nothing but a volatile Niter, which exhaling, doth not increase the Earth. As for the Egyptian Dew, and the increase of the weight of the Mud, he ascribes them to the same Cause. For, the Spirits of Nitre abounding in the Nile, when railed into the Air with the vapors, that exhale continually from this River, there is made out of their mixture, a Dew, that refreshes the Air, makes sickness to cease, and produces all those admirable effects, that make the Egyptians wish for it so passionately. And the same Spirits of Niter, being joyned to the Paste, and to the Mud, raise the one, and augment the weight of the other. That, which Mr. Buratini observes, that at the time of this inundation, the Niter-pits of the neighbouring places vomit out liquid Niter, and that one may see issue out of the Earth abundance of Chrystals of Nitre, is alleged to fortify this conjecture; Which is yet more confirm'd by the Fertility, communicated to the Earth by the Mud of this River. For, plants do grow there in such abundance, that they would choke one another, if it were not remedied by throwing Sand upon the Fields; insomuch that the Egyptians must take as much pains to spread Sand to lessen the fattens of their Land, as other Nations do, to spread dung or other manure upon theirs to increase the fattens.

In the Fourth and Fifth, the Author undertakes to prove, that all those strange effects cannot be attributed to Rain or Snow, and
and that the overflowing of the Nile always happens at a certain day.

In the Last, he alledges some Relations, serving to confirm his Opinion: Which are too long here to insist upon.

**DE PRINCIPiIS ET RATIOCCINATIONE GEMETRARUM; Contra Faustum Professorum Geometricarum**

Author Tho: Hobbes. It seems, that this Author is angry with all Geometricians, but himself; yea he plainly faith in the dedication of his Book, that he invades the whole Nation of them; and unwilling, it seems, to be call'd to an account for doing so. He will acknowledge no judge of this Age; but is full of hopes, that posterity will pronounce for him. Mean while he ventures to advance this Dilemma; Eorum qui de ipsdem rebus mecum aliquid ediderunt, aut solus insanio Ego, aut solus non insanio; tertium enim non est, nisi (quod dicet forte aliquis) insaniamus omnes. Doubtless, one of these will be granted him.

As to the Book itself, he professes, that he doth not write it against Geometry, but Geometers: and that his design in it, to shew, that there is no less uncertainty and falsity in the writings of Mathematicians, than there is in those of Naturalists, Moralists, &c. though he judges, that Physicks, Ethicks, Politicks, if they were well demonstrated, would be as certain as the Mathematics.

Attacking the Mathematical Principles as they are found in Books, and withall some Demonstrations, he takes to task Euclid himself, instead of all; as the Master of all Geometricians, and with him his best interpreter, Clavius, examining in the First place, the Principles of Euclid: Secondly, Declaring false, what is superfrusted upon them, whether by Euclid, or Clavius, or any Geometer whatsoever that hath made use of those or other (as he is pleased to entitle them) false Principles. Thirdly, Pretending, that he means to to combat all, both Principles and Demonstrations, undertaken by him, as that he will substitute better in their room, leaft he should seem to undermine the Science it-selfe.
The particulars, which he undertakes to reform, are,

Punctum.
Linea.
Terminus.
Linea Recta.
Superficies.
Superficies Termini.
Superficies Plana,
Angulus (Where he is large upon the Angulus Contactus.)
Petitio prima Elem. i. Euclidis.
Ratio.

Radix & Latus.
Prop. 16. El. 3.
Dimensio Circuli.
Magnitudo Circuli Hugeniana, Seatio Anguli.
Ratio, quam habet recta composita ex Radio & Tangente
30. grad, ad Radium ipsum,
Propos. 47. Elem. i. Demonstration.
Addita est Appendix de Medii proportionalibus in genere.

KING SALOMON'S PORTRAITURE OF OLD AGE; by John Smith, M.D. This Treatise being a Philosophical Discourse, though upon a sacred Theme, may certainly claim a place among Philosophical Transactions. Not here to mention the many other learned Notes, this Worthy Author gives upon that Hieroglyphical Description of Old Age, made by that Royal Pen-man of Ecclesiastes, cap. 12. We shall only take notice of that surprizingly Ingenious one, there to be met with, concerning the Antiquity of the Doctrine of the Blood's Circulation: King Salomon, who lived near 2700 years ago, using such expressions, as may, to a considering Reader, very probably denote the same Doctrine, which the Sagacious Dr. Harvey has of late years so happily brought to light, and introduced into all the most Ingenious Societies of Learned men: The Pitcher, mention'd in the quoted place, being Interpreted for the Veines, and the Fountain, for the Right Ventricle of the Heart, as the Gisern, for the Left; the Wheel, there spoken off, manifestly importing a Circulation, made by the Great Artery with its Branches, the principal Instrument thereof.

Printed with Licence for John Martyn, and James Allestry, Printers to the Royal Society. 1666.
The Contents.

A new Experiment, shewing, how a considerable degree of Cold may be suddenly produced without the help of Snow, Ice, Haile, Wind, or Niter, and that at any time of the year. An Account of two Books, lately printed in London; whereof the one is entituled, EUCLIDI S ELEMENTA GEOMETRICA, novo ordine ac Methodo demonstrata; the Author, Anonymus. The other, THE ENGLISH VINEYARD VINDICATED, by JOHN ROSE.

A new Frigoristical Experiment shewing, how a considerable degree of Cold may be suddenly produced without the help of Snow, Ice, Haile, Wind, or Niter, and that at any time of the year.

His subject will it self, as presumed, without any other Preamble, speak the Cause, why this present Paper is publish'd at this (unusual) time of the Month: though, by the by, it may not be amiss to add on this occasion, that the Publisher of these Treatises never meant to confine himself to a Set time, as not to retain the Liberty of taking any other, when there is occasion. And there being one given him, before another Month is come in, he does without any scruple or delay comply therewith, presenting the Curious with an Experiment which he thinks is both seasonable, and will not be unwelcome to them; furnishing out of the Ample Magazine of that Philosophical Benefactor, the Noble Mr. Boyle; Concerning which, thus much is further thought requisite to intimate on this occasion, that it, and some others of the same Gentlemen, that have been, and may be, mentioned in the Transactions, belong to certain Treatises, the Author hath lying by him; but that yet he denies not
to communicate them to his Friends, and to allow them to dis-
pose thereof, upon a hope, that equitable Readers will be ready
to excuse, if hereafter they should appear also in the Treatises
they belong to, since he consents to this Anticipation, but to
comply with those, that think the imparting of real and practi-
cal Experiments, may do the Publick some Service, by excite-
ing and assisting mens Curiosity in the interim.

As for the Experiment, you saw the other day at my Lodg-
ings, though it belongs to some Papers about Cold, that (you
know) could not be Publish't, when the rest of the History came
forth, and therefore was reserved for the next Edition of that
Book; yet the Weather having been of late very hot, and
threatning to continue so, I presume, that to give you here in
compliance with your Curiosity an Account of the Main and
Practical part of the Experiment, may enable you to gratify
not onely the Curious among your Friends, but those of the
Delicate, that are content to purchase a Coolness of Drinks
at a somewhat chargeable rate.

You may remember, that the Spring before the last, I shew'd
you a particular Account of a way, wherein by a certain sub-
stance obtain'd from Sal Armoniack, I could presentlv produce
a considerable degree of Cold, and that with odd Circumstances,
without the help of Snow, Ice, Niter &c. But that Experiment
being difficult and costly enough, and design'd to afford men
Information, not Accommodations, I afterwards tried, what
some more cheap and facil mixtures of likely Bodies with Sal
Armoniack would do towards the Production of Cold, and af-
terwards I began to consider, whether to that purpose alone
(for my first experiment was design'd to exhibite other Pheno-
mena too) those mixtures might not without inconvenience be
omitted: and I was much confirm'd in my conjecture, by an
accident, which was casually related to me by a very Ingenious
Physician of my acquaintance, but not to be repeated to you
in few words; though he complain'd, he knew not what to make
of it.

Among the several ways, by which I have made insfrigidating
Mixtures with Sal Armoniack, the most simple and facil is this:
Take one pound of powder'd Sal Armoniack, and about three
Pints (or pounds) of Water, put the Salt into the Liquor, ei-
ther altogether, if your design be to produce an intense, though

but a short coldness; or at two, three, or four several times, if you desire, that the produced coldness should rather last somewhat longer than be so great: Stirre the powder in the Liquor with a stick or whalebone (or some other thing, that will not be injur'd by the fretting Brine, that will be made) to hasten the dissolution of the Salt; upon the quickness of which depends very much the intensity of the Cold, that will ensue upon this Experiment. For the clearing up whereof, I shall annex the following particulars.

1. That a considerable degree of Cold is really produced by this operation, is very evident: First, to the touch; Secondly, by this, that if you make the Experiment (as for this reason I sometimes chuse to do) in a Glass-Body or a Tankard, you may observe, that, whilst the Solution of the Salt is making, the outside of the Metalline Vessel will, as high as the mixture reaches within, be bedew'd (if I may so speake') with a multitude of little Drops of Water, as I have * elsewhere shown that it happens, when mixtures of Snow and Salt, being put into Glasses or other Vessels, the aqueous vapors, that swim to and fro in the Air, and chance to glide along the sides of the Vessels, are by the coldness thereof condens'd into Water. And in our Armoniack Solution you may observe, that if you wipe off the Dew from any particular part of the outside of the Vessel, whilst the Solution does yet vigorously goe on, it will quickly collect fresh Dew, which may be sometimes copious enough to run down the sides of the Vessel. But Thirdly, the best and surest way of finding out the Coldness of our Mixture is that, which I shew'd you by plunging into it a good seal'd Weatherglass, furnisht with tincted Spirit of Wine. For, the Ball of this being put into our frigorifick mixture, the Crimson Liquor will nimbly enough descend much lower, than when it was kept either in the open Air, in common Water, of the same temper with that, wherein the Sal Armoniack was put to dissolve. And if you remove the Glass out of our Mixture into common water, the tincted Spirit will, (as you may remember, it did) hastily enough reascend for a pretty while, according to the greater or leffer time, that it continued in the Armoniack Solution. And this has succeeded with me, when instead of removing the Mixture into Common Water, I removed it into water newly impreguated with Salt-peter.

2. The
2. The Duration of the Cold, produc'd by this Experiment, depends upon several Circumstances; as First, upon the Season of the year, and present temperature of the Air; For, in Summer and Hot weather the Cold will sooner decay and expire. Secondly, upon the Quantity of the Salt and Water: For, if both these be great, the effect will be as well more lasting, as more considerable. Thirdly, for 'tis yet known, we may here add the Goodness & Fitness of the particular parcel of Salt, that is employ'd: for, though it be hard to discern beforehand, which will be the more, and which the less proper; yet some trials have tempted me to suspect, that there may be a considerable disparity, as to their fitness to produce Cold, betwixt parcels of Salt, that are without scruple lookt upon as Sal Armoniack: Of which difference it were not perhaps very difficult to assign probable reasons from the Nature of the Ingredients of this compound Concrete, and the waies of preparing it. But the Duration of the Cold may be conceived to depend also, Fourthly, upon the Way of putting in the Salt into the Water. For, if you cast it in all at once, the Water will sooner acquire an intense degree of Coldness, but it will also the sooner return to its former temper; Whereas, if you defire but an inferior degree of that Quality, but that may last longer (which will usually be the most convenient for the Cooling of Drinks,) then you may put in the Salt by little and little. For, keeping a long Weather-glass for a good while in our impregnated Mixture, I often purposely try'd, that, when the tinted liquor subsided but slowly, or was at a stand, by putting in, from time to time, 2. or 3. Spoonfuls of fresh Salt, and stirring the Water to quicken the Dissolution, the Spirit of Wine would begin again to descend, if it were at a stand or rising, or subside much more swiftly than it did before. And if you would lengthen the Experiment, it may not be amiss, that part of the Sal Armoniack be but grofly beaten, that it may be the longer in dissolving, and consequently in Cooling the Water. Whilst there are dewy drops produced on the outside of the Vessel, 'tis a sign, that the Cold within continues pretty strong; for, when it ceases, these drops especially in warm weather, will by degrees vanish. But a surer way of measuring the duration of the Cold, is, by removing from time to time the Seal'd Weather-glass out of the Saline Mixture into the same common Water, with part of which it was made. And thought it be not easie to determine any thing particularly about this matter, yet it may somewhat assist you in your Estimates, to be informed, That I have in the Spring by a good Weather-glass found a sensible advantageous Cold, made by a pound of Sal Armoniack at the utmost, to last about 2. or 3. hours.

3. To cool Drinks with this Mixture, you may put them in thin Glasses, the thinner the better; which (their orifices being stopp'd, and still kept above the Mixture) may be mov'd to and fro in it, and then be immediately pour'd out to be drunk; Though, when the Glafs, Imployed, was conveniently shape'd, as, like a Sugar-loaf, or with a long Neck, I found it not amiss to drink it out of that, without pouring it into any other; which can scarce be done without lessening the Coolness. The refrigeration, if the Glafs viall be convenient, is quickly perform'd: And if one have a mind to cool his hands, he may readily do it by applying them to the outside of the Vessel, that contains the refrigerating Mixture, by whose help, pieces of Chryystal, or Bulletts for the cooling of the
the Mouths or Hands of those patients, to whom it may be allow'd, may be potently cool'd, and other such refreshments may be easily procur'd.

4. How far Sal Armoniack, ming'd with Sand or Earth, and not dissolv'd, but only moist'n'd with a little Water sprinkl'd on it, will keep Bottles of Wine or other liquors more coole, than the Earth or that Sand alone will do, I have not yet had opportunity by sufficient trials fully to satisfie my self, and therefore resign that Enquiry to the Curious.

5. For the cooling of Air, and Liquors, to adjust Weather-glasses (to be able to do which, at all times of the year, was one of the chief aims, that made me bethink my self of this Experiment;) or to give a small quantity of Beer &c. a moderate degree of coolness, it will not be requisite, to employ near so much as a whole pound of Sal Armoniack at a time. For, you may easily observe by a seal'd Weather-glass, that a very few ounces, well poud'er'd and nimbly dissolv'd in about 4. times the weight of Water, will serve well enough for many purpofes.

6. And that you may the less scruple at this, I shall tell you, that even before and after Midsummer, I have found the Cold-producible by our Experiment to be considerable and useful for refrigerating of Drinks, &c. but if the Sal Armoniack be of the fittest sort (for I intimated above, that I suspected, 'tis not equally good) and if the season of the year do make no disadvantageous difference, the degree of Cold, that may be produced by no more than one pound (if not by less) of Sal Armoniack, may, within its own Sphere of Activity, be much more vehement, than, I presume, you yet imagine, and may afford us excellent Standards to adjust seal'd Weather glafl'es by; and for several other purpofes. For I remember that in the Spring, about the end of March, or beginning of April, I was able with one pound of Sal Armoniack and a requisite proportion of Water, to produce a degree of Cold much greater, than was necessary the preceding Winter, to make it frozly Weather abroad; nay. I was able to produce real ice in a space of time, almost incredibly short. To confirm which particulars, because they will probably seem strange to you, I will here annex the Transcript of an entry, that I find in a Note book of the Phenomena and Succes of one of those Experiments, as I then try'd it; though I should be ash'm'd to expose to your perufal a thing fo rudely pen'd, if I did not hope, you would consider, that 'twas hastily written only for my own Remembrance. And that you may not stop at any thing in the immediately annex'd Note, or the two, that follow, it will be requisite to premise this Account of the seal'd Thermoscope, (which was a good one) wherewith these Observations were made; That the length of the Cylindrical pipe was 16. Inches, the Ball, about the bigness of a somewhat large Walnut, and the Cavity of the Pipe by guefs about an eight or ninth part of an inch Diameter.

The First Experiment is thus registered. March the 27th, in the Seal'd Weather glafl'es, when first put into the Water, the tinged Spirit refted at 8 & inches: being suffer'd to stay there a good while, and now and then stirr'd to and fro in the Water, it descended at length a little beneath 7 & inches; then the Sal Armoniack being put in, within about a quarter of an hour or a little more, it descended to 2 & inches, but before that time, in half a quar-
ter of an hour it began manifestly to freeze the vapours and drops of water on the outside of the Glafs. And when the frigorifick power was arriv'd at the height, I several times found, that water, thinly plac'd on the outside, whilst the mixture within was nimbly stirr'd up and down, would freeze in a quarter of a minute (by a Minute-watch.) At about $\frac{1}{4}$ of an hour after the infrigidating Body was put in, the Thermoscope, that had been taken out a while before, and yet was risen but to the lowest freezing mark, being again put in, the liquor, fell an inch beneath the mark. At about $2\frac{1}{2}$ hours from the first Solution of the Salt I found the tinted liquor to be in the midst between the freezing marks, whereof the one was at $5\frac{1}{2}$ inches (at which height when the Tincture ran'd, it would usually be, some, though but a small, Frost abroad,) and the other at $4\frac{1}{2}$ inches; which was the height, to which strong and durable Frosts had reduced the liquor in the Winter. At 3 hours after the beginning of the Operation, I found not the Crimfon-liquor higher than the upper Freezing mark newly mention'd; after which, it continued to rise very slowly for about an hour longer, beyond which time I had not occasion to observe it.

Thus far the Note-book; wherein there is mention made of a Circumstance of some former Experiments of the like kind, which I remember was very conspicuous in this newly recited. For, the frigorifick mixture having been made in a Glafs-body (as they call it) with a large and flatifh bottom, a quantity of water, which I (purposely) spilt upon the Table, was by the operation of the mixture within the Glafs, made to freeze, and that strongly enough, the bottom of the Cucurbit to the Table, that stagnant liquor being turn'd into solid Ice, that continued a considerable while unhaw'd away, and was in some places about the thickness of a half Crown piece.

Another Observation, made the same Spring, but lefs solemn, as meant chiefly to shew the Duration of Cold in a high degree, is recorded in these terms: The first time the Seal'd Weather-glafs was put in, before it touch'd the common water, it stood at $8\frac{1}{2}$, having been left there a considerable while, and once or twice agitated the water, the tinted liquor sunk but to $7\frac{1}{2}$, or at furthest, $7\frac{3}{4}$, then the frigorifick liquor being put into the water with circumstances disadvantageous enough, in (about) half a quarter of an hour the tinted liquor fell beneath $\frac{3}{4}$; and the Thermoscope, being taken out, and then put in again, an hour after the water had been first infrigidated, subsided beneath $5\frac{1}{2}$ inches, and consequently within $\frac{1}{4}$ of an inch of the mark of the strongly freezing weather.

7. Whereas the grand thing, that is like to keep this Experiment from being as generally Useful, as perhaps it will prove Luciferous, is the Dearnness of Sal Armoniack; two things may be offered to lessen this inconvenience. For first, Sal Armoniack might be made much cheaper, if instead of fetching it beyond sea, our Country-men made it here at home; (which it may easily be, and I am ready to give you the Receipt, which is no great Secret.) But next, I consider'd, that probably the infrigidating vertue of our mixture might depend upon the peculiar Texture of the Salt Armoniack, whereby, whilst the Water is dissolving it, either some Frigorifick particles are extricated and excited, or (rather) some particles, which did before more agitate the minute parts of the water, are expell'd (or invited out by the ambient Bodies) or come
come to be clogg'd in their motion: Whence it seem'd reasonable to expect that upon the Reunion of the Saline particles into such a Body, as they had constituted before, the redintegrated Sal Armoniack having, near upon, the same Texture, would, upon its being redissolv'd, produce the same, or a not much inferior degree of Coldness: And hereupon, though I well enough foresaw that an Armoniack Solution, being boil'd up in Earthen vessels (for Glass ones are too chargeable) would, by piercing them, both lose some of the more subtle parts, and thereby somewhat impair the texture of the rest; yet I was not deceiv'd in Expecting, that the dry Salt, remaining in the pipkins, being redissolv'd in a due proportion of water, would very considerably infrigidate it, as may further appear by the Notes, which for your greater satisfaction you will find here subjoin'd, as soon as I have told you, that, though for want of other vessels I was first reduc'd to make use of Earthen ones, and the rather, because some Metallin Vessels will be injur'd by the dissolv'd Sal Armoniack, if it be boil'd in them; yet I afterwards found some conveniences in Vessels of other Mettall, as of Iron; whereof you may command a further Account.

March the 29th, the Thermoscope in the Air was at 8½ inches; being put into a somewhat large evaporating glass, fill'd with water; it fell (after it had a pretty while, and had been agitated in the liquor) to 8 inches; then about half the Salt, or less, that had been used twice before, and felt much less cold than the water, being put in and stirr'd about, the tinted Spirit subsided with a visible progress, till it was seen manifestly beneath 4 inches; and then having caused some water to be freshly pump'd and brought in, though the newly mention'd Solution were mixt with it, yet it presently made the Spirit of Wine manifestly to ascend in the Instrument, much faster, than one would have expected, &c.

And thus much may suffice for this time concerning our Frigorific Experiment; which I scarce doubt but the Carthesians will lay hold on as very favourable to some of their Tenents; which you will easily believe, it is not to the Opinion, I have elsewhere expos'd, of those Modern Philosophers, that would have Salt-petre to be the Primum Frigidum: (though I found by trial, that, whilst 'tis actually dissolving, it gives a much considerable degree of Cold, than otherwise.) But about the Reflexions, that may be made on this Experiment, and the Variations, and Improvements, &c. of it, though I have divers things lying by me; yet, since you have been several of them already, and may command a fight of the rest, I shall forbear the mention of them here, not thinking it proper, to swell the bulk of this Letter with them.

An Account of two Books lately printed in London.

1. Euclidis Elementa Geometrica, nova ordine as methodo demonstrata. In this compendious and pretty Edition, the Anonymous Author pretends to have rendred these Elements more expeditious, by bringing all together into one place, what belongs to one and the same Subject: Comprising 1. What Euclid hath said of Lines, Straight, Intersecting one another, and Parallel. 2. What he hath demonstrat'd of a Single Triangle, and of Triangles Compared one with another. 3. What of the Circle, and its Properties. 4. What of Proportions in Triangles and other Figures. 5. What of Quadrats and Rectangles, made of Lines diversify
6. What of Plane Superficies's. 7. What of Solids. After which follow the Problems. The Definitions are put to each Chapter as need requireth. The Axioms because they are few, and almost every where necessary, are not thus distributed in Chapters. The Postulate, are not subjourn'd to the Axioms, but reserv'd for the Problems, the Author esteeming, that they being practical Principles, had only place in Problems.

This for the Order. As to the Manner of Demonstrating, One and the same is observ'd in most Propositions; all with much brevity; to the end, that what is not of it self difficult, may not be made so, by multitude of Words and Letters.

II THE ENGLISH VINE-YARD VINDICATED. The Author (Mr. John Rose, his Majesties Gardener at his Royal Garden in St. James's) makes it his business in this small Tract (a very thin Pocket-book) by a few short Observations made by himself, to direct Englishmen in the Choice of the Fruit, and the Planting of Vine-yards, heretofore very frequently cultivated, though of late almost quite neglected by them.

He discourses skillfully, 1. Of the several sorts of Vines, and what Grapes are most fuitable to the Climate of England; where he chiefly commends the small Black-grape, or Cluster-grape; the Parsley grape; the White Muscadine; the Frontiniack; and a new White-grape, with a red Wood and a dark green Leaf: All these being early ripe fruit. 2. Of the Soyle, and Situation of a Vine-yard in England: Where, as to the First, he pitches upon a Light Soile, having a bottom of Chalk or Gravel; and given to Brambles, observing, that no Plant whatsoever is so consonant to the Vine for Soyle, as the Bramble. As for the Situation he chooses that side or declivity of a Hill, that lies to the South or South-west, and is favoured with other Hills somewhat higher, or Woods on the North and East, to break the rigour of those quarters. This direction he thinks of that importance, that he affirms, that the discouragement of the Culture of Vines in England, has only proceeded from men's misinformation on this material article of Choice of Soyle and Situation. 3. How to prepare the Ground for the Plantation, vid. by plowing up the Swath in July, and by disposing the Turf in small heaps, and so burning them, and spreading the ashes over the Land; care being taken, that by keeping too much materials together, the Earth be not over burnt by the excessive heat and fire, which they require to reduce them to ashes.

What is added, of the Manner of planting the Sets: of Dressing, Pruning and Governing the Plantation: of the Ordering and Cultivating the Vine-yard after the first four years, till it needs renewing; as also of the manner and time, how and when to manure the Vine-yard, with Compost, will be better understood from the Book itself, than can be here described; the Author pretending, that, those few observations of his, as the native production of his own Experience, being practised with care, the Vine-yards in England may be planted, governed and perpetuated with undoubted success; and offering withall to furnish those, that have a desire to renew this Culture, and to flore their grounds with Sets and Plants of all those sorts, which he recommends, he having a plentiful flock of them all.

Printed with Licence for John Martyn, and James Allestry, Printers to the Royal Society. 1666.
The Contents.

An Essay of Dr. John Wallis, exhibiting his Hypothesis about the Flux and Reflux of the Sea, taken from the Consideration of the Common Center of Gravity of the Earth and Moon; together with an Appendix of the same, containing an Answer to some Objections, made by several Persons against that Hypothesis. Some Animadversions of the same Author upon Master Hobbs's late Book, De Principis & Ratiocinacione Geometrarum.

An Essay

Of Dr. John Wallis, exhibiting his Hypothesis about the Flux and Reflux of the Sea.

How abstruse a subject in Philosophy, the Flux and Reflux of the Sea hath proved hitherto, and how much the same hath in all Ages perplexed the Minds even of the best of Naturalists, when they have attempted to render an Account of the Cause thereof, is needless here to represent. It may perhaps be to more purpose, to take notice, that all the deficiencies, found in the Theories or Hypotheses, formerly invented for that End, have not been able to deterre the Ingenious of this Age from making farther search into that Matter: Among whom that Eminent Mathematician Dr. John Wallis, following his happy Genius for advancing reall Philosophy, hath made it a part of his later Inquiries and Studies, to contrive and deduce a certain Hypothesis concerning that Phenomenon, taken

N n
from the Consideration of the Common Center of Gravity of the Earth and Moon, This being by several Learned Menlookt
upon, as a very rational Notion, it was thought fit to offer it by the Press to the Publick, that other Intelligent Persons also
might the more conveniently and at their leisure examine the Conjecture (the Author, such is his Modesty, presenting it no o-
therwise) and thereupon give in their Sense, and what Diffi-
culties may occur to them about it, that so it may be either
confirm'd or laid aside accordingly: As the Proposer himself
expressly desires in the Discourse; we now, without any more
Preamble, are going to subjoin, as it was by him addrested, by
way of Letter, from Oxford to Mr. Boyle April 25, 1666, and
afterwards communicated to the R. Society, as follows:

You were earnest with me, when you last went from hence,
that I would put in writing somewhat of that, which at divers
times, these three or four years last past, I have been discours-
ing with your self and others concerning the Common Center of
Gravity of the Earth and Moon, in order to salving the Phenomena
as well of the Seas Ebbing and Flowing; as of some perplexities
in Astronomical Observations of the Places of the Celestial Bo-
dies.

How much the World, and the great Bodies therein, are
manag'd according to the Laws of Motion, and Statick Principles,
and with how much more of clearness and satisfaction, many
of the more abstruse Phenomena have been salved on such
Principles, within this last Century of years, than formerly they
had been; I need not discourse to you, who are well versed in
it. For, since that Galileo, and (after him) Torricelli, and o-
thers, have applied Mechanick Principles to the salving of Phi-
losophical Difficulties; Natural Philosophy is well known to have
been rendered more intelligible, and to have made a much
greater progress in less than an hundred years, than before for
many ages.

The Seas Ebbing and Flowing, hath so great a connexion with
the Moons' motion, that in a manner all Philosophers (whatever
other Causes they have joyned with it), have attributed much
of its caufe to the Moon, which either by some occult quality,
or particular influence, which it hath on moist Bodies, or by some Magnetic virtue, drawing the water towards it, (which should therefore make the Water there highest, where the Moon is vertical) or by its gravity and pressure downwards upon the Terraqueous Globe (which should make it lowest, where the Moon is vertical) or by whatever other means (according to the several Conjectures of inquisitive persons,) hath so great an influence on, or at least a connexion with, the Sea’s Flux and Reflux, that it would seem very unreasonable, to exclude the consideration of the Moons motion from that of the Sea: The Periods of Tides (to say nothing of the greatness of them near the New-moon and Full-moon) so constantly waiting on the Moon’s motion, that it may be well presumed, that either the one is governed by the other, or at least both from some common cause.

But the first that I know of, who took in the consideration of the Earth’s motion, (Diurnal and Annual) was Galileo, who in his Systeme of the World, hath a particular discourse on this subject: Which, from the first time that I ever read it, seemed to me so very rational, that I could never be of other opinion, but that the true Account of this great Phenomenon was to be referred to the Earths motion, as the Principal cause of it: Yet that of the Moon (for the reasons above mentioned) not to be excluded, as to the determining the Periods of Tides, and other circumstances concerning them. And though it be manifest enough, that Galileo, as to some particulars, was mistaken in the account which there he gives of it; yet that may be very well allowed, without any blemish to so deserving a person, or prejudice to the main Hypothesis: For that Discourse is to be looked upon only as an Essay of the general Hypothesis; which as to particulars was to be afterwards adjusted, from a good General History of Tides; which it’s manifest enough that he had not; and which is in a great measure yet wanting. For were the matter of Fact well agreed on, it is not likely, that several Hypotheses should so far differ, as that one should make the Water then and there at the Highest, where and when the other makes it at the Lowest; as when the Moon is Vertical to the place.
And what I say of Galileo, I must in like manner desire to be understood of what I am now ready to say to you. For I do not profess to be so well skilled in the History of Tides, as that I will undertake presentley to accommodate my general Hypothesis to the particular cases; or that I will indeed undertake for the certainty of it, but onely as an Essay propose it to further consideration; to stand or fall, as it shall be found to answer matter of Fact. And truly had not your importunity (which is to me a great Command) required me to do it, I should not so easily have drawn up any thing about it, till I had first satisfied my selfe, how well the Hypothesis would answer Observation: Having for divers years neglected to do it, waiting a time when I might be at leisure throughly to prosecute this design.

But there be two reasons, by which you have prevailed with me, at least to do something. First, because it is the common Fate of the English, that out of a modesty, they forbear to publish their Discoveries, till prosecuted to some good degree of certainty and perfection; yet are not so wary, but that they discourse of them freely enough to one another, and even to Strangers upon occasion; whereby others, who are more hasty and venturous, coming to hear of the notion, presentely publish something of it, and would be reputed thereof, to be the first Inventers thereof: though even that little, which they can then say of it, be perhaps much less, and more imperfect, than what the true Authors could have published long before, and what they had really made known (publikely enough, though not in print) to many others. As is well known amongst us as to the business of the Lymphatick Vessels in Anatomy: the Injection of Liquors into the veins of Living animals; the Exhibiting of a straight line equal to a crooked; the Spot in Jupiter, whence his motion about his own Axis may be demonstrated; and many other the like considerable Inventions.

The other Reason (which, with me, is more really of weight, though even the former be not contemptible) is, because, as I have been already for at least three or four years past past diverted from prosecuting the inquiry or perfecting the Hypothesis, as I had thoughts to do; so I do not know, but like Emergencies may divert me longer; and whether I shall ever so
do it, as to bring it to perfection, I cannot determine. And therefore, if as to my self any thing should humanitatem accidere; yet possibly the notion may prove worth the preserving to be prosecuted by others, if I do it not. And therefore I shall, at least to your self, give some general account of my present imperfect and undigested thoughts.

I consider therefore, that in the Tides, or the Flux and Reflux of the Sea, besides extraordinary Extravagancies, or Irregularities, whence great Inundations or strangely high Tides do follow, (which yet perhaps may prove not to be so meerly accidental as they have been thought to be, but might from the regular Laws of Motion, if well considered, be both well accounted for, and even foretold;) there are these three notorious Observations made of the Reciprocation of Tides. First, the Diurnal Reciprocation: whereby twice in somewhat more than 24 hours, we have a Flood and an Ebb; or a High-water and Low-water. Secondly, the Menstrual; whereby in one Synodical period of the Moon, suppose from Full-moon to Full-moon, the Time of those Diurnal Vicissitudes doth move round through the whole compass of the Nodusbusen, or Natural day of twenty-four hours. As for instance, if at the Full-moon the full Sea be at such or such a place just at Noon, it shall be the next day (at the same place) somewhat before One of the clock; the day following, between One and Two; and so onward, till at the New-moon it shall be at midnight; (the other Tide, which in the Full-moon was at midnight, now at the New-moon coming to be at noon;) and so forward, till at the next Full-moon, the Full-sea shall (at the same place) come to be at Noon again. Again, that of the Spring-tides and Neap-tides (as they are called;) about the Full-moon and New-moon the Tides are at the Height; at the Quadratures the Tides are at the Lowest; and at the times intermediate, proportionably. Thirdly, the Annual; whereby it is observed, that at sometimes of the year, the Spring-tides are yet much higher than the Spring-tides at other times of the year: Which Times are usually taken to be at the Spring and Autumn; or the two Equinoxes; but I have reason to believe (as well from my own Observations, for many years, as of others who have been much
much concerned to heed it, whereof more will be said by and by; that we should rather assign the beginnings of February and November, than the two Equinoxes.

Now in order to the giving account of these three periods, according to the Laws of Motion and Mechanick Principles; We shall first take for granted, what is now adayes pretty commonly entertained by those, who treat of such matters; That a Body in motion is apt to continue its motion, and that in the same degree of celerity, unless hindered by some contrary Impediment; (like as a Body at rest, to continue so, unless by some sufficient mover, put into motion:) And accordingly (which daily experience testifies) if on a Board or Table, some loose incumbent weight, be for some time moved, & have thereby contracted an Impetus to motion at such a rate: if that Board or Table chance by some external obstacle, or otherwise, to be stopped or considerably retarded in its motion, the incumbent loose Body will shoot forward upon it: And contrariwise, in case that Board or Table chance to be accelerated or put forward with a considerably greater speed than before, the loose incumbent Body, (not having yet obtained an equal Impetus with it,) will be left behind, or seem to fly backward upon it. Or, (which is Galileo's instance,) if a broad Vessel of Water, for some time evenly carried forward with the Water in it, chance to meet with a stop, or to flack its motion, the Water will dash forward and rise higher at the fore part of the Vessel: And contrariwise, if the Vessel be suddenly put forward faster than before, the Water will dash backwards, and rise at the hinder part of the Vessel. So that an Acceleration or Retardation of the Vessel, which carries it, will cause a rising of the Water in one part, and a falling in another: (which yet, by its own weight, will again be reduced to a Level as it was before.) And consequently, supposing the Sea to be but as a loose Body, carried about with the Earth, but not so united to it, as necessarily to receive the same degree of Impetus with it, as its fixed parts do: The acceleration or retardation in the motion of this or that part of the Earth, will cause (more or less, according to the proportion of it,) such a dashing of the Water, or rising at one part, with a falling at another, as is that, which we call the Flux and Reflux of the Sea.

Now
(269)

Now this premised, we are next, with him, to suppose the Earth carried about with a double motion; the one Annual, as (Fig. 1.) in B E C the great Orb, in which the Center of the Earth B, is supposed to move about the Sun A.

The other Diurnal, whereby the whole moves upon its own Axis, and each point in its surface describes a Circle, as D E F G.

It is then manifest, that if we suppose, that the Earth moved but by any one of these motions, and that regularly, (with an equal swiftness;) the Water, having once attained an equal Impetus thereunto, would still hold equal pace with it; there being no occasion, from the Quickening or Slackening of the Earths motion, (in that part where the Water lyeth) for the Water thereon either to be cast Forward or fall Backward, and thereby to accumulate on the other parts of the Water: But the true motion of each part of the Earths surface being compounded of those two motions, the Annual and Diurnal; (the Annual in B E C being, as Galileo there supposeth, about three times as fast as a diurnal motion in a great Circle, as D E F;) while a Point in the Earths surface moves about its Center B, from G to D. and E. and at the same time, its Center B, be carried forwards to C; the true motion of that Point forwards, is made up of both those motions; to wit, of B to C, and of G to E; but while G moves by D to E, E moves backward by F to G, contrary to the motion of B to C; so that the true motion of E, is but the difference of B C, and E G: (for, beside the motion of B; the Center G is also put forward as much as from G to E; and E put backward as much as from E to G:) so that the Diurnal motion, in that part of the Earth, which is next the Sun, as E F G, doth abate the progress of the Annual, (and most of all at F;) and in the other part, which is from the Sun, as G D E, it doth increase it, (and most of all at D.) that is, in the day time there is abated, in the night time is added to the Annual motion, about as much as is G E, the Earths Diameter. Which would afford us a Cause of two Tides in twenty-four hours; the One upon the greatest Acceleration of motion, the Other upon its greatest Retardation.

And thus far Galileo's Discourse holds well enough; But then...
in this it comes short; that as it gives an Account of two Tides; so those two Tides are always to be at F and D; that is, at Noon and Midnight; whereas Experience tells us, that the Time of Tides, moves in a moneths space through all the 24 hours. Of which he gives us no account. For though he do take notice of a Menstrual Period; yet he doth it only as to the Quantity of the Tides; greater or less; not as to the Time of the Tides, sooner or later.

To help this, there is one (Vid. * Jo. Baptista Balianus) who makes the Earth to be but a secondary Planet; and to move, not directly about the Sun, but about the Moon, the Moon meanwhile moving about the Sun; in like manner as we suppose the Earth to move about the Sun, and the Moon about it.

But this, though it might furnish us with the foundation of a Menstrual Period of Accelerations and Retardations in the compound motion of several parts of the Earths surface; yet I am not at all inclined to admit this as a true Hypothesis, for divers Reasons, which if not demonstrative, are yet so consonant to the general Sysyme of the World, as that we have no good ground to disbelieve them. For 1. The Earth being undeniably the greater Body of the two (whereof there is no doubt to be made) it cannot be thought probable, that this should be carried about by the Moon, lesser than it self: The contrary being seen, not onely in the Sun, which is bigger than any of the Planets, which it carrieth about; but in Jupiter, bigger than any of his Satellites; and Saturne, bigger than his. 2. As the Sun by its motion about its own Axis, is with good reason judged to be the Physical cause of the Primary Planets moving about it; So there is the like reason to believe, that Jupiter and Saturne moving about their Axes, are the Physical cause of their Satellites moving about them, which motion of Jupiter hath been of late discover'd, by the help of a fixed Spot discern'd in him; and we have reason to believe the like of Saturne. Whether Venus and Mercury (about whom no Satellites have been yet observed) be likewise so moved; we have not yet the like ground to determine: But we have of Mars; from the

* Vid. Riccioli Almagest. novum, Tom. I. lib. 4. cap. 10. n. 111. pag. 216, 2.
the Observations of Mr. Hook made in February and March last, and by him communicated to the Royal Society; and since printed in the Transactions, published Apr. 2. 1666, consonant to the like observations of Jupiter, made by him in May 1664, and since communicated to the same Society; and then published in the Transactions, of March. 6. then next following. Now that the Earth hath such a motion about its own Axis (whereby it might be fitted to carry about the Moon) is evident by its Diurnal motion. And it seems as evident that the Moon hath not; because of the same side of the Moon always turned towards us; which could not be, if the Moon carried the Earth about. Unless we should say, that it carries about the Earth in just the same Period, in which it turns upon its own Axis: Which is contrary to that of the Sun carrying about the Planets: the shortest of whose Periods, is yet longer than that of the Sun's moving about its own Axis. And the like of Jupiter, shorter than the Period of any of his Satellites; if at least the Period of his revolution about his Axis, lately said to be observed, prove true. (Of Saturn we have not yet any Period assigned; but it's likely to be shorter, than that of his Satellites.) And therefore we have reason to believe, not that by the Moons motion about its Axis the Earth should be carried by a contemporary Period (whereby the same face of the Moon should be ever towards us;) but that by the Earths revolution about its Axis in 24 hours, the Moon should be carried about it in about 29 days, without any motion on its own Axis: And accordingly, that the Secondary Planets about Jupiter and Saturn, are not (like their Principals) turned about their own Axis. And therefore I am not at all inclined to believe, that the Mensural Period of the Tides with us, is to be salved by such an Hypothesis.

In stead of this, that Surmise of mine, (for I dare not yet, with confidence give it any better name,) of what I have spoken to you heretofore, (and which hath occasioned this present account which I am now giving you,) is to this purpose. The Earth and Moon being known to be Bodies of so great connexion (whether by any Magnetick, or what other Tye, I will not determine; nor need I, as to this purpose;) as that
The motion of the one follows that of the other; (The Moon observe the Earth as the Center of its periodic motion: ) may well enough be looked upon as one Body, or rather one Aggregate of Bodies, which have one common center of Gravity; which center (according to the known Laws of Staticks) is in a freight Line connecting their respective Centers, so divided as that its parts be in reciprocal proportion to the Gravities of the two Bodies. As for Example: Suppose the Magnitudes (and therefore, probably, the Gravity) of the Moon to be about an one and fortyeth part of that of the Earth; (and thereabouts as Hevelius in his Selenography page 203 doth out of Tycho, estimate the proportions, and an exact certainty is not necessary to our present businesse.) And the distance of the Moons Center from the Center of the Earth, to be about fifty six semidiameters of the Earth, (as thereabouts he doth there estimate it, in its middle distance; and we need not be now very accurate in determining the numbers; wherein Astronomers are not yet very well agreed.) The distance of the Common Center of Gravity of the two Bodies, will be from that of the Earth, about a two and fortyeth part of fifty six Semidiameters; that is, about \( \frac{1}{40} \) or \( \frac{3}{5} \) of a Semidiameter; that is about \( \frac{3}{4} \) of a Semidiameter of the Earth, above its surface, in the Air, directly between the Earth and Moon.

Now supposing the Earth and Moon, jointly as one Body, carried about by the Sun in the great Orb of the Annual motion, this motion is to be estimated, (according to the Laws of Staticks, in other cases,) by the motion of the common Center of Gravity of both Bodies. For we use in Staticks, to estimate a Body, or Aggregate of Bodies, to be moved upwards, downwards, or otherwise, so much as its Common Center of Gravity is so moved, howsoever the parts may change places amongst themselves.

And accordingly, the Line of the Annual motion, (whether Circular or Elliptical; of which I am not here to dispute,) will be described, not by the Center of the Earth (as we commonly estimate it, making the Earth a Primary and the Moon a Secondary Planet,) nor by the Center of the Moon, (as they would do, who make the Moon the Primary and the Earth a Secondary.)
Secondary Planet, against which we were before disputing:)
But by the Common Center of Gravity of the Bodies, Earth and Moon,
as one Aggregate.

Now supposing A B C D E to be a part of the great Orb of the Annual motion, described by the Common Center of Gravity, in so long time as from a Full-Moon at A to the next New-Moon at E; (which, though an Arch of a Circle or Ellipse, whose Center we suppose at a due distance below it, yet being but about \(\frac{1}{10}\) of the whole, may well enough be here represented by a straight Line:) the Center of the Earth at T, and that of the Moon at L, must each of them (supposing their common Center of Gravity to keep the Line A E) be supposed to describe a Periphery about that Common Center, as the Moon describes her Line of Menstrual motion. (Of which I have (in the Scheme) only drawn that of the Earth; as being sufficient to our present purpose; parallel to which, if need be, we may suppose one described by the Moon; whose distance is also to be supposed much greater from T than in the figure is expressed, or was necessary to express.) And in like manner E F G H I, from that New-moon at E, to the next Full-moon at I.

From A to E (from Full-moon to New-moon,) T moves (in its own Epicycle) upwards from the Sun: And from B to I, (from New-moon to Full-moon,) it moves downwards, toward the Sun. Again, from C to G, (from last quarter to the following first quarter,) it moves forwards according to the Annual motion; But from G forward to C, (from the first Quarter to the ensuing last Quarter,) it moves contrary to the Annual motion.

It is manifest therefore, according to this Hypothesis, that from Last quarter to First quarter (from C to G, while T is above the Line of the Annual motion,) its Menstrual motion in its Epicycle adds somewhat of Acceleration to the Annual motion; and most of all at E, the New-moon: And from the first to the last quarter (from G forward to C, while T is below the Line of the Annual motion,) it abates of the Annual motion; and most of all at I, or A the Full-moon.

So that in pursuance of Galileo's Notion, the Menstrual add-
ing to or detra&ting from the Annual motion, should either leave behinde, or cast forward, the loose waters incumbent on the Earth, (and thereby cause a Tide, or accumulation of Waters;) and most of all at the Full moon and New-moon, where those Accelerations or Retardations are greatest.

Now this Menstrual motion, if nothing else were superadded to the Annual, would give us two Tides in a moneth, and no more; (the one upon the Acceleration, the other on the Retardation;) at New-moon and Full-moon; and two Ebbs, at the two Quarters; and in the Intervals, Rising and Falling water.

But the Diurnal motion superadded, doth the same to this Menstrual, which Galileo supposeth is to do to that Annual; that is, doth Add to, or Subtract from, the Menstrual Acceleration or Retardation; and so gives us Tide upon Tide.

See For in whatsoever part of its Epicycle, we suppose Fig. 4. T to be; yet because, while by its Menstrual motion the Center moves in the Circle L T N; each point in its surface, by its diurnal motion moves in the Circle L M N: whatever effect (accelerative or tardative;) the Menstrual would give, that effect by the Diurnal is increased in the parts L M N (or rather L M N. the Semicircle) and most of all at M: but diminished in the parts N O L (or rather N O L) and most of all at O. So that at M, and O, (that is when the Moon is in the Meridian below or above the Horizon;) we are to have the Diurnal Tide or High-water, occasioned by the greatest Acceleration or Retardation, which the Diurnal Arch gives to that of the Menstrual: which seems to be the true cause of the Daily Tides. And withall gives an account, not only why it should be every day; but likewise, why at such a time of the day, and why this time should in a moneth run through the whole 24. hours; viz. because the Moons coming to the Meridian above and below the Horizon, (or as the Seamen call it, the Moons Southing, and Northing;) doth so: As likewise of the Spring tides and Neap-tides. For, when it so happens, that the Menstrual and Diurnal Accelerations or Retardations, be coincident, (as at New moons and Full-moons they are,) the effect must needs be the greater. And although (which is not to be dissimuled) this happen but
but to one of the two Tides; that is, the Night-tide at the New-
moon (when both motions do most of all Accelerate,) and the
Day-tide at Full-moon (when both do most Retard the Annual
motion.) Yet, this tide being thus raised by two concurrent
causes; though the next Tide have not the same cause also, the
Impetus contracted will have influence upon the next Tide.
Upon a like reason, as a Pendulum let fall from a higher Arch,
will (though there be no new cause to occasion it) make the
Vibration on the other side (beyond the Perpendicular) to
be also greater: Or, of water in a broad Veſsel, if it be so jogg-
ged, as to be cast forward to a good height above its Levell, will
upon its recoyling, by its own gravity, (without any addi-
tional cause) mount so much the higher on the hinder part.

But here also we are to take notice, that though all parts of
the Earth by its Diurnal motion do turn about its Axis, and de-
scribe parallel Circles; yet not equal Circles; but greater near
the Equinoxial, and leffer near the Poles, which may be a cause:
why the Tides in some parts may be much greater than in oth-
ers. But this belongs to the particular considerations, (of
which we are not now giving an Account:) not to the general
Hypothesis.

Having thus endeavored to give an account of the Diurnal
and Menſtrual Periods of Tides; It remains that I endeavou-
the like as to the Annual: Of which there is, at least, thus much-
agreed; That, at some times of the year, the Tides are noted
to be much higher, than at other times.

But here I have a double task; First, to rectify the Obser-
vation; and then, to give an account of it:

As to the First: It having been observed (grossly) that those
high Tides have used to happen about the Spring and Autunm;
it hath been generally taken for granted (without any more
nice observation) that the two Equinoxes are the proper times,
to which these Annual high Tides are to be referred: And such
causes sought for, as might best fute with such a Supposition.

But it is now, the best part of twenty years, since I have had
frequent occasions to converse with some Inhabitants of Rums-
ney-marsh in Kent; where the Sea being kept out with great
Earthen walls, that it do not at high water overflow the Levell;
and the Inhabitants livelihood depending most on grazing, or feeding Sheep; they are (as you may believe they have reason to be) very vigilant and observant, at what times they are most in danger of having their Lands drowned. And I find them generally agreed, by their constant Observations, (and Experience dearly bought) that their times of danger are about the beginning of February and of November: that is, at those Spring Tides which happen near those times; to which they give the names of Candlemas-stream and Aithaloniid-stream: And if they escape those Spring-tides, they apprehend themselves out of Danger for the rest of the year. And as for March and September (the two Equinoxes) they are as little solicitous of them, as of any other part of the year.

This, I confess, I much wondered at, when I first heard it; and suspected it to be but a mistake of him, that first told me, though he were indeed a person not likely so to be mistaken, in a thing wherein he was so much concerned: But I soon found, that it was not only his, but a general observation of others too; both there, and elsewhere along the Sea coast. And though they did not pretend to know any reason of it, (nor so much as to enquire after it;) Yet none made doubt of it; but would rather laugh at any that should talk of March and September, as being the dangerous times. And since that time, I have myself very frequently observed (both at London and elsewhere, as I have had occasion) that in those months of February and November, (especially November) the Tides have run much higher, than at other times: Though I confess, I have not been so diligent to set down those Observations, as I should have done. Yet this I do particularly very well remember, that in November 1660, (the same year that his Majesty returned) having occasion to go by Coach from the Strand to Westminster, I found the Water so high in the middle of King-street, that it came up, not only to the Boots, but into the Body of the Coach; and the Palace-yard (all save a little place near the West-End) overflow’d, as likewise the Market-place; and many other places; and their Cellars generally filled up with Water. And in November last, 1665, it may yet be very well remembered, what very high Tides there were, not only on the Coasts of England, (where much hurt was done
done by it) but much more in Holland, where by reason of those inundations, many Villages and Towns were overflow'd. And though I cannot so particularly name other years, yet I can very safely say, that I very often observed Tides strangely high about those times of the year.

This Observation did for divers years cause me much to wonder, not only because it is so contrary to the received opinion of the two Æquinoxes; but because I could not think of anything signal at those times of the year: as being neither the two Æquinoxes, nor the two Solstices, nor the Sun's Apogæum and Perigæum; (or Earths Apælium and Perælium;) nor indeed, at contrary times of the year, which at least, would seem to be expected. From Alhollandtide to Candlemas being but three months; and from thence to Alhollandtide again nine months.

At length it came into my mind, about four years since, that though there do not about these times happen any single signal Accident, which might call it on these times, yet there is a compound of two that may do it: Which is the Inequality of the Natural day (I mean that of 24 hours, from noon to noon) arising at least from a double cause; either of which singly would call it upon other times, but both jointly on those.

It's commonly thought, how unequal forever the length be of the Artificial dayes as contradistinguished to nights, yet that the Natural Day, reckoning from noon to noon, are all equal: But Astronomers know well, that even these dayes are unequal.

For, this Natural Day is measured not only by one intire conversion of the Æquinoctial, or 24 Æquinoctial hours, (which is indeed taken to be performed in equal times,) but increases by so much, as answers to that part of the Sun's (or Earths) Annual motion as is performed in that time. For, when that part of the Æquinoctial, which (with the Sun) was at the Meridian yesterday at noon, is come thither again to day, it is not yet Noon (because the Sun is not now at the place where yesterday he was, but is gone forward about one degree, more or less) but we must stay till that place, where the Sun now is, comes to the Meridian before it be now Noon.

Now this Additament (above the 24 Æquinoctial hours, or intire conversion of the Æquinoctial) is upon a double account unequal;
equal. First, because the Sun, by reason of its Apogaeum and Perigeum, doth not at all times of the year dispatch in one day an equal Arch of the Ecliptick; but greater Arches neer the Perigeum, which is about the middle of December; and lesser neer the Apogaeum, which is about the middle of June: As will appear sufficiently by the Tables of the Sun's Annual motion. Secondly, though the Sun should in the Ecliptick move always at the same rate; yet equal Arches of the Ecliptick do not in all parts of the Zodiac answer to equal Arches of the Equinoctial, by which we are to estimate time: Because some parts of it, as about the two Solstitial Points, lie nearer to a parallel position to the Equinoctial, than others, as those about the two Equinoctial points, where the Ecliptick and Equinoctial do intersect; whereupon an Arch of the Ecliptick, neer the Solstitial points answers to a greater Arch of the Equinoctial; than an Arch equal thereunto neer the Equinoctial points: As doth sufficiently appear by the Tables of the Sun's right Ascension.

According to the first of these causes, we should have the longest natural daies in December, and the shortest in June, which if it did operate alone, would give us at those times two Annual High-waters.

According to the second cause, if operating singly, we should have the longest daies at the two Solstices in June and December, and the two shortest at the Equinoxes in March and September, which would at those times give occasion of four Annual High-waters.

But the true Inequality of the Natural Days, arising from a Complication of those two causes, sometimes crossing and sometimes promoting each other: though we should find some increases or decreases of the Natural daies at all those seasons answerable to the respective causes (and perhaps of Tides proportionably thereunto:) yet the longest and shortest natural daies absolutely of the whole year (arising from this complication of Causes) are about those times of Allhallowtide and Candlemas; (or not far from them) about which those Annual High-tides are found to be: As will appear by the Tables of Equation of Natural daies. And therefore I think, we may with very good reason cast this Annual Period upon that cause, or rather complication.
(279)

plication of causes. For (as we before shewed in the *Menstrual* and *Diurnal*) there will, by this inequality of Natural daies, arise a Physical Acceleration and Retardation of the Earths Mean motion, and accordingly a casting of the Waters backward or forward; either of which, will cause an Accumulation or High-water.

'Tis true, that these longest and shortest daies, do (according to the Tables, some at least) fall rather before, than after Al-hallontide and Candlemas (to wit the ends of October and January;) but to do also (sometimes) those high Tydes: And it is not yet so well agreed amongst Astronomers, what are all the Causes (and in what degrees) of the Inequality of Natural daies; but that there be diversities among them, about the true time: And whether the introducing of this New Motion of the Earth in its Epicycle about this Common Center of Gravity, ought not there-in also to be accounted for, I will not now determine: Having already said enough, if not too much, for the explaining of this general Hypothesis, leaving the particularities of it to be adjusted according to the true measures of the motions; if the General Hypothesis be found fit to be admitted.

Yet this I must add, (that I be not mistaken) that whereas I cast the time of the daily Tydes to be at all places, when the Moon is there in the *Meridian*; it must be understood of open Seas, where the water hath such free scope for its motions, as if the whole Globe of Earth were equally covered with water: Well knowing, that in Bays and In-land-Channels, the position of the Banks and other like causes must needs make the times to be much different from what we suppose in the open Seas: And likewise, that even in the Open Seas, Islands, and Currents, Gulfs and Shallows, may have some influence, though not comparable to that of Bays and Channels. And moreover, though I think, that Seamen do commonly reckon the time of High-water in the Open Seas, to be then, when the Moon is there in the *Meridian* (as this Hypothesis would cast it;) Yet I do not take myself to be so well furnished with a History of Tides, as to assure myself of it; much less to accommodate it to particular places and cases.

Having thus dispatched the main of what I had to say con-
cerning the Seas Ebbing and Flowing: Had I not been already too tedious, I should now proceed to give a further reason, why I do introduce this consideration of the Common Center of Gravity in reference to Astronomical Accounts. For indeed, that which may possibly seem at first to be an Objection against it, is with me one reason for it.

It may be thought perhaps, that if the Earth should thus describe an Epicycle about the Common Center of Gravity, it would (by this its change of place) disturb the Celestial motions, and make the apparent places of the Planets, especially some of them, different from what they would otherwise be. For though so small a removal of the Earth, as the Epicycle would cause (especially if its semidiameter should not be above \( \frac{1}{3} \) of the Earth's Semidiameter) would scarce be sensible (if at all) to the remoter Planets; yet as to the nearer it might.

Now though what Galileo answers to a like Objection in his Hypothesis: (that its possible there may be some small difference, which astronomers have not yet been so accurate, as to observe) might here perhaps serve the turn; Yet my answer is much otherwise; to wit, that such difference hath been observed, and hath very much puzzled astronomers to give an account of. About which you will find Mr. Horrocks (in some of his Letters, whereof I did formerly, upon the Command of the Royal Society, make an Extract) was very much perplexed; and was fain, for want of other relief, to have recourse to somewhat like Kepler's amicable Fibres, which did according to the several positions of the Moon, accelerate or retard the Moon's motion; which amicable Fibres he had no affecction to at all (as there appears) if he could any other waies give account of those little inequalities; and would much rather (I doubt not) have embraced this Notion of the Common Center of Gravity, to solve the Phenomenon, had it come to his mind, or been suggested to him. And you find, that other astronomers have been seen to bring in (some upon one supposition, some upon another) some kind of Menstrual Equation, to solve the inequalities of the Moon's motion, according to her Synodical Revolution, or different Aspects (of New-moon, Full Moon, &c.) beside what concerns her own Periodical motion.

For
For which, this consideration of the Common Center of Gravity of the Earth and Moon, is so proper a remedy (especially if it shall be found precisely to answer those Phenomena, which I have not Examined, but am very apt to believe) that it is so far from being, with me, an Objection against it, that it is one of the reasons, which make me inclinable to introduce it.

I must before I leave this, add one Consideration more, That if we shall upon these Considerations think it reasonable, thus to consider the Common Center of Gravity of the Earth and Moon; it may as well be thought reasonable, that the like Consideration should be had of Jupiter and his four Satellites, which according to the Complication of their several motions, will somewhat change the position of Jupiter, as to that Common Center of Gravity of all those Bodies; which yet, because of their smallness, may chance to be so little, as that, at this distance, the change of this apparent place may not be discernable. And what is said of Jupiter, is in the like manner to be understood of Saturne and his Satelles, discovered by Hugenius: For all these Satellites are to their Principals, as so many Moons to the Earth. And I do very well remember, in the Letters forecited, Mr. Horrocks expresseth some such little inequalities in Saturnes motion, of which he could not imagine what account to give, as if (to use his Expression) this crabbed Old Saturn had despaired his Youth. Which, for ought I know, might well enough have been accounted for, if at that time the Satelles of Saturn had been discovered, and that Mr. Horrocks had thought of such a motion as the Common Center of Gravity of Saturn and his Companion, to be considerable, as to the guiding of his motion.

You have now, in obedience to your Commands, an Account of my thoughts, as to this matter, though yet immature and unpolished: What use you will please to make of them, I shall leave to your prudence, &c.

An APPENDIX, written by way of Letter to the Publisher; Being an Answer to some Objections, made by several Persons, to the precedent Discourse.

I Received yours; and am very well contented, that objections be made against my Hypothesis concerning Tydes: being
proposed: but as a conjecture to be examined; and, upon that Examination, rectified, if there be occasions or rejected, if it will not hold water.

1. To the first objection of those you mention: That it appears not how two Bodies, that have no eye, can have one common Center of Gravity: that is (for so I understand the intendment of the objection) can act or be acted in the same manner, as if they were connected: I shall one by answer, that it is harder to shew how they have, than that they have it. That the Loadstone and Iron have somewhat equivalent to a Tye; though we see it not, yet by the effects we know. And it would be easy to shew, that two Loadstones, at once applied, in different positions, to the same Needle, at some convenient distance, will draw it, not to point directly to either of them, but to some point between both, which point is, as to those two, the common Center of Attraction; and it is the same, as if some one Loadstone were in that point. Yet have these two Loadstones no connexion or tye, though a Common Center of Virtue according to which they jointly act. And as to the present case, how the Earth and Moon are connected; I will not now undertake to shew (nor is it necessary to my purpose;) but, That there is somewhat, that doth connect them, (as much as what connects the Loadstone, and the Iron, which it draws,) is past doubt to those, who allow them to be carried about by the Sun, as one Aggregate or Body, whose parts keep a respective position to one another: Like as Jupiter with his four Satellites, and Saturn with his one. Some Tye there is, that makes those Satellites attend their Lords, and move in a Body: though we do not see that Tye, nor hear the Words of Command, and so here.

2. To the second objection: That, at Chatham and in the Thames, the Annual Spring tydes, happen about the Equinoxes: not (as this Hypothetic doth suppose elsewhere to have been observed) about the beginning of February and November. If their meaning be, that Annual High Tydes, do then happen, and then only: If this prove true, it will ease me of half my work. For it is then easily answered, that it depends upon the Obliquity of the Zodiac; the parts of the Equinoctial answering to equal parts of the Zodiac.
Zodiack, being neer the Solstitial points greatest, and near the Equinoctial points least of all. But besides this Annual Vici-
situde of the Equinoxes, not to say of the 4. Cardinal Points
(which my Hypothesis doth allow and affert;) I believe it will
be found, that there is another Annual viciTitude answer-}
ing to the Sun's Apogæum and Perigæum. And that the greatest Tydes
of all, will be found to be upon a result of these two causes Co-
operating: which (as doth the Inequality of Natural dayes, de-
pending on these same causes:) will light nearer the times, I
mention. To what is said to be observed at Chatham and in the
Thames, contrary to that I allege as observed in Rumney marsh:
I must at present advert, and refer to a melius inquirendum. If
those who object this contrary observation, shall, after this no-
tice, find, upon new Observations heedfully taken, that the
Spring-tides in February and November, are not so high, as those
in March and September; I shall then think the objection very
considerable. But I do very well remember, that I have seen:
in November, very high Tydes at London, as well as in Rumney
Marsh. And, the time is not yet so far past, but that it may be
remembered (by your self or others then in London,) whether
in November last when the Tydes were so high at Dover, at Deal,
at Margate, and all along the Coast from thence to Rumney,
Marsh, as to do in some of those places much hurt; (and, in Hol-
lard, much more;) whether, Hay, there were not also at the
same time, at London; (upon the Thames) very high Tydes.
But a good Diary of the Height and time both of High-water,
and Low-water, for a year or two together, even at Chatham,
or Greenwich: but rather at some place in the open Sea; or at the
Lands end in Cornwall, or on the West parts of Ireland; or at St.
Helens, or the Bermudas, &c. would do more to the resolvings
of this point, than any verbal discourse without it.

3. To the third Objection, That supposing the Earth and Moon
to move about a Common center of gravity; if that the highast Tydes
be at the New-moon, when the moon being nearest to the Sun, the Earth
is farthest from it, and its compound motion at the swiftest; and that
the Tydes abate as the Earth approacheth nearer, till it comes into the
supposed Circle of her Annual motion: It may be demanded, why do
they not still abate as the Earth comes yet nearer to the Sun and the
Swifline of
(284)

Swiftness of its compound motion still slackens? And so, why have we not Spring tides at the New Moon (when the motion is swiftest) and Neap-tides at Full Moon (when the motion is slowest) but Spring tides at both? The answer (if observed) is already given in my Hypothesis it self. Because the effect is indifferently to follow, either upon a suddain Acceleration, or a suddain Retardation. (Like as a loose thing, lying on a moving body; if the body be thrust suddainly forward, that loose thing is cast back, or rather left behind, not having yet obtained an equal impetus with that of the body, on which it lyes; but if stopped, or notably retarded, that loose incumbent is thrown forward, by its formerly contracted impetus not yet qualified or accomodated to the slowness of the Body, on which it lyes.) Now both of these happening, the one at the New Moon, the other at the Full Moon, do cause high Tides at both.

4. To the fourth Objection, That the highest Tydes are not at all places, about the New Moon and Full Moon; and particularly, that, in some places of the East Indies, the Higheft Tydes are at the Quadratures: I must first anfwer in general; That as to the particular varieties of Tydes in several parts of the World, I cannot pretend to give a satisfactory account, for want of a competent History of Tydes, &c. Because (as is intimated in what I wrote in the general) the various positions of Channels, Bays, Promontories, Gulfs, Shallows, Currents, Trade-winds, &c. must needs make an innumerable variety of Accidents in particular places, of which no satisfactory account is to be given from the general Hypothesis (though never so true) without a due consideration of all those. Which is a task too great for me to undertake, being so ill furnished with materials for it. And then as to the particular instance of some places in the East Indies, where the highest Tydes are at the Quadratures: I suppose, it may be chiefly intended of those about Cambaia, and Pegu. At which places, beside that they are situate at the inmost parts of Vaft Bayes, or Gulfs (as they are called) they have also vast Indraughts of some hundred Miles within Land; which when the Tydes are out, do lye (in a manner) quite dry: And may therefore very well be supposed to participate the effect of the Menstrual Tydes many dayes after the cause
cause of them happens in the open Sea, upon a like ground as in Straights and narrow Channels the Diurnall Tydes happen some hours later than in the Ocean. And a like account must be given of particular accidents in other places, from the particular situation of those places, as Bays, Channels, Currents, &c.

5. To the 5. Objection, That the Spring-Tydes happen not, with us, just at the Full and Change, but two or three daies after. I should with the more confidence attempt an Answer, were I certain, whether it be so in the Open Seas, or onely in our Channels. For the Answers will not be the same in both cases. If onely in our Channels, where the Tydes find a large in-draught, but not in the Open Seas: we must seek the reason of it from the particular position of those places. But if it be so generally in the wide Open Seas: We must then seek a reason of it from the general Hypothesis. And, till I know the matter of Fact, I know not well, which to offer at; left whilst I attempt to salve one, I should fall foul of the other. I know that Mar- riners use to speak of Spring-Tydes at the New and Full of the Moon; though I have still had a suspicion that it might be some daies after, as well in the open Seas, as in our narrower Channels; (and therefore I have chosen to say, in my Papers, About the New and Full, rather than At the New and Full; and even when I do say At, I intend it in that laxer sense in which I suppose the Mariners are to be understood; for Neer that time:) Of which suspicion you will find some intimations even in my first Papers: But this though I can admit; yet, because I was not sure of it, I durst not build upon it. The truth is, the Flux and Reflux of water in a vessel, by reason of the jogging of it; though it follow thereupon; yet is, for the most part, discernable some time after. For there must, upon that jog, be some time for Motion, before the Accumulation can have made a Tyde. And so I do not know but that we must allow it in all the Periods. For as the menstrual High Tyde, is not (at least with us) till some Daies after the Full and Change; so is the Diurnall High water, about as many Hours after the Moons comming to South; (I mean, At Sea; for in Channels it varies to all Hours, according as they are neerer or further from the open Sea:) And the Annual High-Tydes of November and February; somewhat later than

(what
(what I conjecture to be from the same causes) the greatest inequalities of the natural Days, happening in January and October. But this though I can admit, yet (till I am sure of the matter of Fact) I do not build upon. And since it hath hitherto been the custom to speak with that laxness of expression; affigning the times of New-moon, Full-moon, and Quadratures, with the Moons comming to South, for, what is neer those times: I did not think my self obliged in my conjectural Hypothecis (while it is yet but a Candidate) to speak more nicely. If the Hypothecis for the maine of it be found Rational; the Niceties of it are to be adjusted, in time, from particular Observation.

Having thus given you some Answers to the Objections you signifie to have been made by several persons to my Hypothecis, and that in the same order your Paper presents them to me: I shall next give you some account of the two Books, which you advised me to consult; so far as seems necessary to this business: Which, upon your intimation, I have since perused, though before I had not.

And first, as to that of Isaac Vossius, De motu Marium & Ven- torum; Though I do not concur with him in his Hypothecis; That all the Great motions of the Seas, &c. should arife only from so small a warming of the water as to raile it (where most of all) not a Foot in perpendicular, (as in his 12th Chapter;) Or that there is no other connexion between the Moons motion, and the Tydes mensural period, than a casual Synchronism (which seems to be the doctrine of his 16th and 18th Chapters;) Besides many other things in his Philosophy, which I cannot allow: Yet I am well enough pleased with what is Historical in it, of the matter of Fact; Especially if I may be secure, that he is therein accurate and candid, not wresting the Phenomena to his own purpose. But I find nothing in it which doth induce me to vary from my Hypothecis. For, granting his Historicals to be all true; the account of the constant Current of the Sea Westward, and of the constant Eastern Blasts, &c. within the Tropicks, is much more plausibly, and (I suppose) truly rendered by Galileo long since, from the Earths Diurnal motion: (which, neare the Equator. describing a greater Circle, than nearer the Poles,
Poles, makes the Current to be there more conspicuous and swift, and, consequently, the Eddy, or recurrent motion, nearer the Poles, where this is, more remote Than can easily be rendered by so small a Tumor, as he supposed. Not to add, that his account of the Proweotive motion, which he supposeth to follow upon this Tumetaction, and by Acceleration to grow to so great a height near the Shoar (as in Chap. 13, and 14) is a Notion, which seems to me too extravagant to be salved by any laws of Staticks. And that of the Moons motion solely Synchronizing with the Tydes, casually, without any Physical connexion; I can very hardly assent to. For it can hardly be imagined, that any such constant Synchronism should be in Nature; but where, either the one is the cause of the other, or both depend upon some Common cause. And where we see so fair a foundation for a Physical connexion, I am not prone to ascribe it to an Independent Synchronism. In sum; His History doth well enough agree with my Hypothesis; and I think, the Phenomena are much better salved by mine, than his.

And then as to Gassendus, in his discourse de Estu Maris, I find him, after the relating of many other Opinions concerning the Cause of it, inclining to that of Galileo, ascribing it to the Acceleration & Retardation of the Earths motion compounded of the Annual and Diurnal; And moreover attempting to give an account of the Menstrual Periods from the Earths carrying the Moon about itself, as Jupiter doth his Satellites, which together with them is carried about by the Sun, as one Aggregate; (and that the Earth with its Moon is to be supposed in like manner to be carried about by the Sun, as one Aggregate, cannot be reasonably doubted, by those who entertain the Copernican Hypothesis, and do allow the same of Jupiter and his Satellites) But though he would thus have the Earth and Moon looked upon as two parts of the same moved Aggregate, yet he doth still suppose (as Galileo had done before him) that the line of the Mean Motion of this Aggregate (or, as he calls, motus equilibris et veluti medius) is described by the Center of the Earth (about which Center he supposeth both its own revolution to be made, and an Epicycle described by the Moons motion;) not by another Point, distinct from the Centers of both, about which as the
(288)

common Center of Gravity, as well that of the Earth, as that of the Moon, are to describe several Epicycles. And, for that Reason fails of giving any clear account of this Menstrual Period, (And in like manner, he proposeth the Consideration as well of the Earths Aphelium and Perihelium, as of the Æquinoctial and Solstitial Points, in order to the finding a Reason of the Annual Vicissitudes; but doth not fix upon any thing, in which himself can Acquiesce: And therefore leaves it in medio as he found it.)

It had been more agreeable to the Laws of Staticks, if he had, (as I do,) so considered the Earth and Moon as two parts of the same movable, (not so, as he doth, aliam in Centro et sequentem præcìse revolutionem axis, aliam remotius ac velut in circumferentia, but,) so, as to make neither of them the Center, but both out of it, describing Epicycles about it: Like as, when a long stick thrown in the Air, whose one end is heavier than the other, is whirled about, so as that the End, which did first fly foremost, becomes hindmost; the proper line of motion of this whole Body is not that, which is described by either End, but that, which is described by a middle point between them; about which point each end, in whirling, describes an Epicycle. And indeed, in the present case, it is not the Epicycle described by the Moon, but that, described by the Earth, which gives the Menstrual Vicissitudes of motion to the Water; which would, as to this, be the same, if the Earth so move, whether there were any Moon to move or not; nor would the Moons Motion, supposing the Earth to hold on its own course, any whit concern the motion of the Water.

But now, (after all our Physical or Statical Considerations) the clearest Evidence for this Hypothesis (if it can be had) will be from Celestial Observations. As for instance; (see Fig. 5.) Supposing the Sun at S; the Earths place in its Annual Orb at T; and Mars (in opposition to the Sun, or near it) at M: From whence Mars should appear in the Zodiac at γ, and will at Full moon be seen there to be; the Moon being at C and the Earth at c: (and the like at the New-moon.) But if the Moon be in the First quarter at A, and the Earth at a; Mars will be seen, not at γ, but at a; too slow: And when the Moon is at B, and the Earth at b, Mars will be seen at β; yet too slow: till at the Full-moon,
moon, the Moon at C, the Earth at c, Mars will be seen at y, its true place, as if the Earth were at T. But then, after the Full, the Moon at D, the Earth at d; Mars will be seen, not at y, but at p; too forward; and yet more, when the Moon (at the last Quarter) is at E, the Earth at e, and Mars seen at z. If therefore Mars (when in opposition to the Sun) be found (all other allowances being made) somewhat too backward before the Full moon, and somewhat too forward after the Full-moon, (and most of all, at the Quadratures: ) it will be the best confirmation of the Hypothesis. (The like may be fitted to Mars in other positions, mutatis mutandis; and so for the other Planets.)

But this proof, is of like nature as that of the Parallaxis of the Earth's Annual Orb to prove the Copernican Hypothesis. If it can be observed, it proves the Affirmative; but if it cannot be observed, it doth not convince the Negative, but only proves that the Semidiameter of the Earth's Epicycle is so small as not to make any discernable Parallax. And indeed, I doubt, that will be the issue. For the Semidiameter of this Epicycle, being little more than the Semidiameter of the Earth itself, or about 1 ½ thereof (as is conjectured, in the Hypothesis, from the Magnitudes and Distances of the Earth and Moon compared: ) and there having not as yet been observed any discernable Parallax of Mars, even in his nearest position to the Earth; it is very suspicious, that here it may prove so too. And whether any of the other Planets will be more favourable in this point, I cannot say.

ANIMADVERSIONS

Of Dr. Wallis, upon Mr. Hobbs's late Book, De Principiis & Ratiocinatione Geometrarum.

These were communicated by way of Letter, written in Oxford, July 24. 1666, to an Acquaintance of the Author, as follows:

Since I saw you last, I have read over Mr. Hobbs's Book Contra Geometras (or De Principiis & Ratiocinatione Geometrarum) which you then shewed me. A New Book of old matter: Containing but a Repetition of what he had before told us, more than once; and which hath been Answered long agoe.

In which, though there be Faults enough to offer ample mat-
ter for a large Confutation: yet I am scarce inclined to believe, that any will bestow so much pains upon it. For, if that be true, which (in his Preface) he saith of himself, *Aut solus insanio Ego, aut solus non insanio:* it would either be Needlefs, or to no Purpofe. For, by his own confeffion, *All others, if they be not mad themselves, ought to think Him so:* And therefore, as to Them, a Confutation would be needlefs; who, its like, are well enough satisfied already: at least out of danger of being fedu-
ced. And, as to himself, it would be to no purpofe. For, if He be the Mad man, it is not to be hoped that he will be convinced by Reafon: Or, if *All We be fo;* we are in no capacity to attempt it.

But there is yet another Reafon, why I think it not to need a Confutation. Because what is in it, hath been sufficiently con-
futed already; (and, to Effectually: as that he profefleth him-
selt not to Hope, that *This Age* is like to give sentence for him; what ever *Nondum imbuta Posteritas* may do.) Nor doth there appear any Reafon, why he should again Repeat it, unless he can hope, *That, what was at first False, may by oft Repeating, become True.*

I shall therefore, instead of a large Answer, onely give you a brief Account, *what is in it; & where it hath been already Answered.*

The chief of what he hath to say, in his first 10 Chapters, againft Euclids Definitions, amounts but to this, That he thinks, Euclide ought to have allowed his *Point* some *Bignefs;* his *Line,* some *Breadth;* and his *Surface,* some *Thickness.*

But where in his Dialogues, pag.151,152. he solemnly under-
takes to Demonstrate it; (for it is there, his 41th Proposition:) his Demonstration amounts to no more but this; *That, unless a Line be allowed some Latitude; it is not possible that his Quadratures can be True.* For finding himfelf reduced to these inconve-
niences: 1. That his *Geometrical Constructions* would not confift with *Arithmetical calculations,* nor with what *Archimedes and others have long fince demonftrated:* 2. That the *Arch* of a Circle must be allowed to be sometimes *Shorter* than its *Chord,* and sometimes *longer* than its *Tangent:* 3. That the fame *Straight Line* must be allowed, at one place onely to *Touch,* and at another place to *Cut* the fame *Circle:* (with others of like nature;) He finds it neceffary, that these things may not feem *Absurd,* to allow his *Lines* some *Breadth,* (that fo, as he speaks, *While a Straight Line with its Outside doth at one place Touch*
Touch the Circle, it may with its Inside at another place Cut it, &c.) But I should sooner take this to be a Confutation of His Quadratures, than a Demonstration of the Breadth of a (Mathematical) Line. Of which, see my Hobbis Heauton-timorumenus, from pag. 114. to p. 119.

And what he now Adds, being to this purpose; That though Euclid's, ζύμιον, which we translate, a Point; be not indeed No- men Quantii; yet cannot this be actually represent'd by any thing, but what will have some Magnitude; nor can a Painter, no not Apelles himself, draw a Line so small, but that it will have some Breadth; nor can Thread be spun so Fine, but that it will have some Bigness; (pag. 2, 3, 19, 21.) is nothing to the Business; For Euclide doth not speak either of such Points, or of such Lines.

He should rather have considered of his own Expedient, pag. 111. That, when one of his (broad) Lines, passing through one of his (great) Points, is supposed to cut another Line propo- sed, into two equal parts; we are to understand, the Middle of the breadth of that Line passing through the middle of that Point, to distinguish the Line given into two equal parts. And he should then have considered further, that Euclide, by a Line, means no more than what Mr. Hobs would call the middle of the breadth of his; and Euclide's Point, is but the Middle of Mr. Hobs's. And then, for the same reason, that Mr. Hobs's Middle must be said to have no Magnitude; (For else, not the whole Middle, but the Middle of the Middle, will be in the Middle: And, the Whole will not be equal to its Two Halves; but Bigger than Both; by so much as the Middle comes to;) Euclide's Lines must as well be said to have no Breadth; and his Points no Bigness.

In like manner, When Euclide and others do make the Term or End of a Line, a Point: If this Point have Parts or Greatness, then not the Point; but the Outer-Half of this Point ends the Line, (for, that the Inner-Half of that Point is not at the End, is manifest, because the Outer-Half is beyond it;) And again, if that Outer Half have Parts also; not this, but the Outer part of it; and again the Outer part of that Outer part, (and so in infinitum.) So that, as long as Any thing of Line remains, we are not yet at the End: And consequently, if we must have passed the whole Length, before we be at the End; then that End (or Punctum terminans) has nothing of Length; (for, when the whole Length is past, there is nothing of it left. And if Mr. Hobs tells us (as pag. 3.) that this
End is not \textit{Punctum}, but only \textit{Signum} (which he does allow \textit{non esse nomen Quantil}) even this will serve our turn well enough. \textit{Euclid's \textit{Συμβολικ\textit{η}}, which some Interpreters render by \textit{Signum}, others have thought fit (with Tully) to call \textit{Punctum}: But if Mr. Hobs like not that name; we will not contend about it. Let it be \textit{Punctum}, or let it be \textit{Signum} (or, if he please, he may call it \textit{Vexillum}.) But then he is to remember, that this is only a Controversie in \textit{Grammar}, not in \textit{Mathematicks}: And his Book should have been intituled \textit{Contra Grammaticos}, not \textit{Contra Geometras}. Nor is it \textit{Euclid}, but Cicero, that is concern'd, in rendring the Greek \textit{Συμβολικ\textit{η}}, by the Latine \textit{Punctum}, not by Mr. Hobs's \textit{Signum}. The Mathematician is equally content with either word.

What he saith here, \textit{Chap. 8. & 19.} (and in his fifth \textit{Dial. p. 105.} &c.) concerning the \textit{Angle of Contact}, amounts but to thus much, That, by the \textit{Angle of Contact}, he doth not mean either what \textit{Euclid} calls an \textit{Angle}, or any thing of that kind; (and therefore saies nothing to the purpose of what was in controversy between Clavins and Peletarius, when he saies, that An \textit{Angle of Contact} hath some magnitude.) But, that by the \textit{Angle of Contact}, he understands the \textit{Crookedness} of the \textit{Arch}; and in saying, the \textit{Angle of Contact} hath some magnitude, his meaning is, that the \textit{Arch} of a \textit{Circle} hath some \textit{crookedness}, or is a \textit{crooked line}: and that, of equal \textit{Arches}, That is the more \textit{crooked}, whose chord is shortest: which I think none will deny; (for who ever doubted, but that a \textit{circular Arch} is \textit{crooked} or, that, of such \textit{Arches}, equal in length, That is the more \textit{crooked}, whose ends by bowing are brought nearest together?) But, why the \textit{Crookedness} of an \textit{Arch}, should be called an \textit{Angle of Contact}; I know no other reason, but, because Mr. Hobs loves to call that \textit{Chalk}, which others call \textit{Cheese}. Of this see my \textit{Hobbinus Heauton-timorumenus}, from pag. 88. to p. 100.

What he saith here of \textit{Rations or Proportions}, and their \textit{Calculus}; for 8. Chapters together, (\textit{Chap. 11. &c.}) is but the same for 

\textit{substance}, what he had formerly saied in his 4th. Dialogue, and elsewhere. To which you may see a full Answer, in my \textit{Hobbinus Heauton-timorumenus}, from pag 49. to p. 88. which I need not here repeat.

Onely (as a \textit{Specimen} of Mr. Hobs's \textit{Candour} in \textit{Falsifications}) you may by the way observe, how he deals with a \textit{Demonstration} of Mr. \textit{Rook}'; in confutation of Mr. Hobs's \textit{Duplication of the Cube}: Which when he had repeated, \textit{pag. 43.} He doth then (that it might seem absurd) change those words, \textit{aquales quattuor}
quattror subis \textit{dv}; \ (pag. 43. line 33.) into these (p. 44. 1. 5.) equalia quattuor Linuis, nemoque quadruplus \textit{Reba dv}: And would thence perfwade you, that Mr. 
Rook had assigned a \textit{Solide}, equal to a \textit{Line}. But Mr. 
Rook's Demonstration was clear enough for Mr. Hobs' Comment. Nor do I 
know any \textit{Mathematician} (unles you take \textit{Mr. Hobs} to be one) who 
thinks that a \textit{Line} multiplied by a \textit{Number} will make a \textit{Square}; (what 
ever \textit{Mr. Hobs} is pleased to teach us.) But, That a \textit{Number} multiplied by a \textit{Number}, may make a \textit{Square Number}; and, That a \textit{Line} drawn into a \textit{Line} may make a \textit{Square Figure}, \textit{Mr. Hobs} (if he were, what he would be thought to be) might have 
known before now. Or, (if he had not before known it) he might have 
learned, (by what I shew him upon a like occasion, in my \textit{Heb. Heant}. pag. 
142. 143. 144.)\textit{How to understand that Language, without an Abridgment.}

Just in the same manner he doth, in the next page, deal with \textit{Clavius}. For 
having given us his words, \textit{pag. 45. 1. 3. 4. Dico hanc Lineam Perpendiculararem 
extra circulum cadere} (because neither \textit{intra Circulum}, nor \textit{in Peripheria};) He doth, when he would shew an errour, first make one, by 
falsifying his words, \textit{line 15}, where instead of \textit{Lineam Perpendiculararem}, he substi-
tutes \textit{Punctum A}. As if \textit{Euclid} or \textit{Clavius} had denied the \textit{Point A}. (the 
utmost point of the \textit{Radius},) to be in the \textit{Circumference}. Or, as if \textit{Mr. Hobs}, 
by proving the \textit{Point A}, to be in the \textit{Circumference}, had thereby proved, 
that the \textit{Perpendicular Tangent A E} had also lyen in the \textit{Circumference} of 
the \textit{Circle}. But this is a \textit{Trade}, which \textit{Mr. Hobs} doth drive so often, as if he 
were as well faulty in his \textit{Morals}, as in his \textit{Mathematics}.

The \textit{Quadrature of a Circle}, which here he gives us, \textit{Chap. 20. 21. 23.} is 
one of those \textit{Twelve} of his, which in my \textit{Hobbies Heant-timorumenus} (from 
pag. 104. to pag. 119) are already confuted: And is the \textit{Ninth} in order 
(as I there rank them) which is particularly considered, \textit{pag. 106. 107. 108}. I 
call it \textit{One}, because he takes it so to be; though it might as well be called 
\textit{Two}. For, as there, so here, it consisteth of \textit{Two branches}, which are Both 
\textit{False}; and each overthrow the other. For if the \textit{Arch of a Quadrant} be 
equal to the \textit{Aggregate of the Semidiameter} and of the \textit{Tangent} of 30. Degrees, 
(as he would \textit{Here} have it, in \textit{Chap. 20}. and \textit{There}, in the close of \textit{Prop. 27}.;) 
Then it is not equal to that Line, whose \textit{Square} is equal to \textit{Ten squares} of the 
\textit{Semiradius}; (as \textit{There}, he would have it, in \textit{Prop. 28.} and, \textit{Here}, in \textit{Chap. 
23}.;) And if it be equal to \textit{This}, then not to \textit{That}. For \textit{This}, and \textit{That}, are 
not equal: As I then demonstrated; and need not now repeat it.

The grand Fault of his Demonstration (\textit{Chap. 20.}) wherewith he would 
now New-vamp his old \textit{False} \textit{quadrature}; lies in those words \textit{Page 49. line 
30. 31. Quod impossibile est nisi ba tranfasit per c}, which is no impossibility at 
all. For though he first bid us \textit{draw the Line R e}, and afterwards the \textit{Line R d}: 
Yet, 
Because he hath no where proved (nor is it true) that \textit{these two are the 
same Line}; (that is, that the point \textit{d} lies in the \textit{Line R e}, or that \textit{R e} passeth 
through \textit{d}); His proving that \textit{R d} \textit{cuts off from} \textit{ab a Line equal}, to the \textit{line of} 
\textit{B c}, doth not prove, that \textit{a} \textit{passeth through} \textit{c}. For this it may well do, though 
\textit{a b} \textit{lye under} \textit{c}, \textit{vid. in case \textit{d} lye beyond the line \textit{R e}}, that is, further from \textit{A}:
or though it lye \textit{above} \textit{c}, \textit{vid. in case \textit{d} lye before the line \textit{R e}}, than \textit{R e}, to the point \textit{A}. And 
therefore, unless he first prove (which he cannot do) that \textit{A d} (a \textit{sixth} 
part of \textit{A B}) doth just reach to the line \textit{R e} and no further; he only proves 
that
that a sixth part of $ab$ is equal to the Line of $Bc$. But, whether it line above it, or below it; or (as Mr. Hobbs would have it) just upon it; this argument doth not conclude. (And therefore Hugoniou's assertion, which Mr. Hobs, Chap. 21. would here give way to this Demonstration, doth, notwithstanding this, remain safe enough.)

His demonstration of Chap 23. (where he would prove, that the aggregate of the Radius and of the Tangent of 30. Degrees is equal to a Line, whose square is equal to 10 Squares of the Semiradius;) is confused not only by me, (in the place recited, where this is proved to be impossible;) but by himself also, in this same Chap. pag. 59 (where he proves sufficiently and doth confess, that this demonstration, and the 47. Prop. of the first of Euclide, cannot be both true.) But, (which is worst of all;) whether Euclid's Proposition be Falfe or True, his demonstration must needs be Falfe. For he is in this Dilemma: If that Proposition be True, his demonstration is Falfe, for he grants that they cannot be both True, page 59 line 21. 22. And again, if that Proposition be False, his Demonstration is to 100; for This depends upon That, page 55. line 22 and therefore must fall with it.

But the Fault is obvious in His Demonstration (not in Euclid's Proposition:) The grand Fault of it (though there are more;) lies in those words, page 56. line 26. Erit ergo MO minus quam MR Where, instead of minus, he should have said major. And when he hath mended that Error, he will find, that the major in page 56. line penult. will very well agree with majorum in page 57. line 1 (where the Printer hath already mended the Fault to his hand) and then the Falsem ergo will vanish.

His Section of an Angle in ratione data; Chap. 22. hath no other foundation, than his supposed Quadrature of Chap 20. And therefore, that being False; this must fall with it. It is just the same with that of his 6. Dialogue. Prop 46. which (besides that it wants a foundation) how absurd it is, I have already shewed; in my Hobbinus Hæmaton timor. page 1 19. 120.

His Appendix, wherein he undertakes to shew a Method of finding any number of mean Proportionals, between two Lines given: Depends upon the supposed Truth of his 22. Chapter; about Dividing an Arch in any proportion given: (As himself confesseth: and as is evident by the Construction; which supposeth such a Section.) And therefore, that failing, this falls with it.

And yet this is otherwise faultily, though he should be supposed True. For, in the first Demonstration; page 67. line 12. Producta Lf incidit in I; is not proved; nor doth it follow from his Quoniam igitur.

In the second Demonstration; page 68 line 34. 35. Recta Lf incidit in x; is not proved; nor doth it follow from his Quare.

In his third Demonstration; page 71. line 7. Producta Y P transibic per M; is said gratis; nor is any proof offered for it. And to this whole Structure falls to the ground. And with ill. the Prop. 47. El 1 doth full stand fast (which he tells us, page 59. 61. 78. must have fallen, if his Demonstrations had stood;) And so, Geometry and Arithmeti (do still agree, which he tells us, page 78. line 10.) had otherwise been at odd.

And this (though much more might have been said,) is as much as need to be said against that Piece.

Printed with Licence for John Martyr, and James Allestry. Printers to the Royal Society.

Observations made in several places, Of the late Eclipse of the Sun, which hapned on the 22 of June, 1666.

The Observations that were made at London by Mr. Willughby, Dr. Pope, Mr. Hook, and Mr. Philips, are these:

The Eclipse began at 5h. 43'

<table>
<thead>
<tr>
<th>h.</th>
<th>diam.</th>
<th>at</th>
<th>5</th>
<th>dig.</th>
<th>at</th>
<th>7</th>
<th>06</th>
</tr>
</thead>
<tbody>
<tr>
<td>6</td>
<td>00</td>
<td>6</td>
<td>07</td>
<td>4</td>
<td>dig.</td>
<td>7</td>
<td>13</td>
</tr>
<tr>
<td>6</td>
<td>13</td>
<td>6</td>
<td>21</td>
<td>3</td>
<td>dig.</td>
<td>7</td>
<td>20</td>
</tr>
<tr>
<td>6</td>
<td>21</td>
<td>6</td>
<td>32</td>
<td>2</td>
<td>dig.</td>
<td>7</td>
<td>26</td>
</tr>
<tr>
<td>6</td>
<td>32</td>
<td>6</td>
<td>57</td>
<td>1</td>
<td>dig.</td>
<td>7</td>
<td>32</td>
</tr>
<tr>
<td>6</td>
<td>57</td>
<td>7</td>
<td>00</td>
<td>0</td>
<td>dig.</td>
<td>7</td>
<td>37</td>
</tr>
</tbody>
</table>

Its Duration hence appears to have been one hour and 54 m. Its greatest Obscurity somewhat more than 7. digits. About the middle, between the Perpendicular and Westward Horizontal Radius of the Sun, viewing it through Mr Boyle's 60. foot Telescope, there was perceived a little of the Limb of the Moon without the Diske of the Sun: which seemed to some of the Observers to come from some shining Atmosphere about the Body either of the Sun or Moon.

They affirm to have observ'd the Figure of this Eclipse, and measured the R Digits
Digits, by casting the Figure through a 5 foot Telescope, on an extended paper, fixt at a certain distance from the Eye-glafe, and having a round figure, all whose Diameters were divided, by 6 Concentrick Circles, into 12 Digits.

The Observations made at Madrid by a Noble Member of the Royal Society, His Excellence the Earl of Sandwich, as they were sent to the Right Honourable, the Lord Vice Count Brounker, are these;

The Eclipse began at Madrid about 5 of the Clock in the morning, at 5 h. 15', the Sun's Altitude was 6 deg. 55'.

The Middle of it was at 6 h. 2', the Sun's Altitude, 15 deg. 5'.

The End was exactly at 7 h. 5', the Sun's Altitude, 25 deg. 24'.

The Duration, 2 h. 4'.

37. Parts of the Sun's diameter remained light.

63. Parts of the same were darkened.

The Observations made at Paris by Monsieur Payen, aslifted by several Astronomers, as they were printed in French, and addressed to Monsieur de Montmor, are these;

The Eclipse began there, at 5 h. 44'. 52". mane. It ended at 7 h. 43'. 6". So that its whole Duration was 1 h. 58'. 14". The greatest Obscuration they assign to have been 7. dig. 50. m. but they adde, that it seem'd to have been greater by 3 minutes; which M. Payen imputes to a particular motion of Liberation of the Sun's Globe, which entertain'd that Luminary in the same Phasis for the space of 8. min. and some seconds, as if it had been stopped in the midst of its Course; rather than to a tremulous Motion of the Atmosphere, as Scheiner would have it.

They intimate that they took the time of each Phasis from half digit to half digit, as well by a Pendulum, as by the Altitudes of the Sun's Center above the Horizon, corrected by the Vertical Paralaxies and Refractions, by which they judged, that though the Time by the Pendulum may be sufficient for Mechanical Operations, yet 'tis not exact enough for establishing the Grounds of true Astronomy.

They further conceive that the apparent Diameters were almost equal; seeing that in the Phasis of 6. Digits, the Circumference of the Moon's disk passed through the Center of that of the Sun, so as that two Lines drawn through the two Horns of the Sun, made with the Common Semi-diameter two Equilateral Triangles.

Next, they affirm, That there was so great a Variation in the Paralaxies, by reason as well of the Refractions of the Air, which environ the Earth, as of the Alteration of the Air, which encompasses the Moon, that the Horns of the Sun, there formed by the Shadoow of the Moon, appeared in all kinds of Figures; Sometimes inclined to the Vertical, sometimes Perpendicular to the Horizon, and at last Parallel; the Convexe part respecting the Heaven, and the Concave, the Horizon. By the crossing (so they go on) of the Horns
Horns with the Angles of Inclination, it will be easy to those, that have exactly observed them, and that are skill'd in the higher Astronomical Calculations, to compute the true Place of the Moon in her Orbit, that so it may be compared with that of the Tables, and with that, which has been observed in other places, for the more precise determining of the Difference of Meridians (that being the way, esteem'd by Kepler the most certain) and for making a good Judgment of the defect or exactness of the Celestial Tables.

Then they observe, That the Beginning and the Middle of this Eclipse hapned to be in the North Eastern Hemisphere, and the End, in the South Eastern. The first Contact (as 'twere) of the two Disks was observ'd in the Superior Limb of the Sun's Disk in respect to the Vertical Line, and in the Inferior in respect to the Ecliptick: But the Middle, and the End were seen in the Superior Limb, in respect both to the Vertical and the Ecliptick: And (what to this Author seems extraordinary) both the Beginning and the End of this Eclipse hapned to be in the Oriental part of the Sun's Disk.

Lastly, they take notice, that by their Observations it appears, that there is but little exactness in all the Astronomical Tables, predicting the Quantity, Beginning and Duration of this Eclipse; Those of Lamberti importing, That the Obfuscation should be of 19. dig. 48'; those of Ricciolo, of 9. dig. 1'; and those of Kepler, of 7. dig. 30'. 1''. Again, that the Duration should be of 2 h. 2'. Lastly, The Beginning did anticipate the Ricciolan Tables by 5. minutes; the End by 23; and the Middle, almost by 11. In the mean time the Author notes, that the Rudolphin Tables come nearest to the Truth; and withal assures the Reader of the goodness of the Instruments employed in his Observations, and of the singular care, he, together with his skilful Assistants, took in making them.

Some Inquiries and Directions concerning Tides, proposed by Dr. Wallis, for the proving or disproving of his lately published Discourse concerning them.

The Inquisitive Dr. Wallis, having in his lately printed Hypothesis of Tides intimated, that he had reason to believe, that the Annual Spring-tides happen to be rather about the beginnings of Febr. and Nov. than the two Aquinoxes, doth in a late Letter to the Publisher, written from Oxford in Aug. last, desire, Sea, some understanding Persons at London, or Greenwich, but rather nearer the that or upon the Sea-shore, would make particular Observation of all the Spring-Tides (New-Moon and Full-Moon) between this and the End of November; and take account of the Hour, and of the Perpendicular height: that we may see, whether those in September, or those of November be highest: And it were not amiss, the Low waters were observed too. Which may be easily done by a mark made upon any standing Post in the Water, by any Water-
Water-man, or other understanding Person, who dwells by the Water-tide.

It would also deserve (thinks he) to be inquired into, whether, when the Tides be highest, the Ebbs be ever lowest, & contra; (which is generally affirmed, and almost put out of question) or rather (which fixes best with his Hypothesis) whether, when the Tides are highest, both in the Annual and Menstrual Periods, the Low waters be not also highest; and at Neap-Tides, the Ebbs also very low.

He adds, that he should expect, that the Spring-Tides now coming, and those at the beginning of September, should not be so high, as those at the middle of September, and then lower again at the beginning of October, and after that, higher at the middle of October, and higher yet about the beginning of November (at the usual times of Spring-tides after the New and Full.)

Considerations and Enquiries concerning Tides, by Sir Robert Moray; likewise for a further search into Dr. Wallis's newly publish'd Hypothesis.

In regard that the High and Low waters are observed to increase, and decrease regularly at several seasons, according to the Moons age, so as, about the New and Full Moon, or within two or three days after, in the Western parts of Europe, the Tides are at the highest, and about the Quarter-Moons, at the lowest, (the former call'd Spring-tides, the other Neap-tides;) and that according to the height and excels of the Tides, the Ebbes in opposition are answerable to them, the highest Tide having the lowest Ebb, and the lowest Ebb, the highest Tide; the Tides from the Quarter to the highest Spring-tide increasing in a certain proportion; and from the Spring-tide to the Quarter-tide decreasing in like proportion, as is supposed: And also the Ebbes rising and falling constantly after the same manner: It is wizt, that it may be inquired, in what proportion these Increases and Decreases, Rises and Falls happen to be in regard of one another?

And 'tis supposed, upon some Observations, made in fit places, by the above-mentioned Gentleman, though, (as himself acknowledges) not thoroughly and exactly performed, that the Increase of the Tides is made in the Proportion of Sines; the first Increase exceeding the lowest in a small proportion; the next in a greater; the third greater than that; and so on to the mid-most, whereof the excess is greatest, diminishing again from that, to the highest Spring-Tide; so as the proportions, before and after the Middle, do greatly answer one another, or seem to do so. And likewise, from the highest Spring-tide, to the lowest Neap-tide, the Decreases seem to keep the like proportions; the Ebbes rising and falling in like manner and in like proportions. All which is supposed to fall out, when no Wind or other Accident causes an alteration.
(299)

And whereas 'tis observed, that upon the main sea-shore the current of the Ebbings and Flowings is sometimes swifter, and sometimes slacker, than at others, so as in the beginning of the flood the tide moves faster but in a small degree, increasing its swiftness constantly till towards the middle of the flood; and then decreasing in velocity again from the middle till to the top of the high-water; it is supposed, that in equal spaces of time, the increase and decrease of velocity, and consequently the degrees of the risings and fallings of the same, in equal spaces of time, are performed according to the proportion of times.

But 'tis withall conceived, that the said proportion cannot hold exactly, and precisely, in regard of the inequalities, that fall out in the periods of the tides, which are commonly observed and believed to follow certain positions of the moon in regard of the equinox, which are known not to keep a constant course; so that, there not intervening equal portions of time between one new-moon and another, the moons return to the same meridian, cannot be always performed in the same time; and consequently there must be a like variation of the tides in the velocity, and in the risings and fallings of the tides, as to equal spaces of time. And the tides from new-moon to new-moon being not always the same in number, as sometimes but 57, sometimes 58, and sometimes 59, (without any certain order of succession) is another evidence of the difficulty of reducing this to any great exactness. Yet, because 'tis worth while, to learn as much of it, as may be, the proposer and many others do desire, that observations be constantly made of all these particulars for some months, and, if it may be, years together, and because such observations will be the more easily and exactly made, where the tides rise highest, it is presumed, that a fit apparatus being made for the purpose, they may be made about bristol or cheap-proof, best of any places in england, because the tides are said thereabout to rise to ten or twelve fathoms; as upon the coast of britannia in france, they do to thirteen and fourteen.

In order to which, this following apparatus is proposed to be made use of. In some convenient place upon a wall, rock, or bridge, &c. let there be an observatory standing, as near as may be to the brink of the sea, or upon some wall; and if it cannot be well placed just where the low water is, there may be a channel cut from the low water to the bottom of the wall, rock, &c. the observatory is to be raised above the high-water 18. or 20. foot; and a pump, of any reasonable dimension, placed perpendicularly by the wall, reaching above the high-water as high as conveniently may be. Upon the top of the pump a pulley is to be fastened, for letting down into the pump a piece of floating wood, which, as the water comes in, may rise and fall with it. And because the rising and falling of the water amounts to 60. or 70. foot, the counterpoise of the weight, that goes into the pump, is to hang upon as many pulleys, as may serve to make it rise & fall within the space, by which the height of the pump exceeds the height of the water. And because by this
this means the Counterpoise will rise and fall slower, and consequently by
less proportions, than the weight itself, the first Pulley may have upon it a
Wheele or two, to turn Indexes at any proportion required, so as to give the
minute parts of the motion, and degrees of risings and fallings. All which
is to be observed by Pendulam-watches, that have Minutes and Seconds, with
Cheeks, according to Mr. Hugen’s way.

And because if the Hole, by which the water is let into the Pump, be as
large as the Bore of the Pump it self, the weight that is raised by the water,
will rise and fall with an Undulation, according to the inequality of the
Sea’s Surface, ’twill therefore be fit, that the Hole, by which the water en-
ters, be less than half as big as the Bore of the Pump; any inconvenience
that may follow thereupon, as to the Periods and Stations of the Flood and
Ebb, not being considerable.

And to the end, that it may appear the better, what are the particular Ob-
servations, desired to be made, near Bristol or Cheap-Row bridg, it was
thought not amiss, to let them down distinctly by themselves.

1. The degrees of the Rising and Falling of the water every quarter of an
hour (or as often as conveniently may be) from the Periods of the Tides and
Ebbs; to be observed night and day, for 2 or 3 months.

2. The degrees of the velocity of the Motion of the Water every quarter
of an hour for some whole Tides together; to be observed by a second
Pendul-watch; and a logg fastened to a line of some 50 fathoms, wound
about a wheel.

3. The exact measures of the Heights of every utmost High-water and
Low-water, from one Spring-tide to another, for some Months or rather
Years.

4. The exact Heights of Spring-tides and Spring-Ebbs for some Years to-
gether.

5. The Position of the Wind at every observation of the Tides; and the
times of its Changes; and the degrees of its Strength.

6. The State of the Weather, as to Rain, Hail, Mist, Haziness, &c. and
the times of its Changes.

7. At the times of observation of the Tides, the height of the Thermom-
eter; the height of the Baroscope; the height of the Hygrooscope; the Age
of the Moon, and her Azimuths; and her place in all respects; And lastly
the Sun’s place; all these to minutes.

And it would be convenient, to keep Journal Tables, for all these Observ-
vations, each answering to its day of the Month.

For the Apparatus of all these observations, there will be particularly ne-
cessary.

A good Pendulam-watch.
A Vase showing Azimuths to minute parts.
An Instrument to measure the strength of the Winde.
A large and good needle showing Azimuths to degrees.
An Account
Of several Books lately published.

I. Johannis Hevelii DESCRIPTIO COMETÆ, Anno æra
Christiana MDCLXV. exordi. una cum MANTISSA Prodromi Co-
metici, Observationes omnes prioris COMETÆ MDCLIV, ex ilisque
genuinum motum accurate deductum, cum Notis & Animadversionibus, exhibens.

This Book (as the Title it self intimates) undertakes two things. First, To
give an Account of the Second of the two late Comets, which appeared, when
the other was scarce extinxt; Concerning which, the Author doth, from
the Observations made by himself with a Sextant of 6 foot, and divided into
minutes and seconds, assign both its true place (as well in respect of the E-
cliptick as the Equator) and its proper motion: Adding a fair Delineation
of its Course, together with the genuine Representations of its Head and
Train, in each day of its apparition; and subjoining a General Description
and Discourse of some of the more notable Phenomena thereof. It was first
seen at Dantzick by the Watchmen, the 5th of April a. n. 1665. and then
observed by the Author, from April 6. about 1½ of the Clock in the morning,
till April 20. at 3. in the morning. During which time, it went with a reasona-
able velocity; making 46 deg. in its Orb, according to the Order of the
Signs, moving from the Breast of Pegæsus, towards the Head of Andromeda,
and the Left Horn of Aries; having, as 'tis presumed, taken its rise from
above Sagittary, and run through the Breast of Antinous, under Aquila, and
the Dolphin, to the said Pegæsus; and so on, as is already expressed.

The Head of it is in the Book described of a Colour like that of Jupiter, all-
along much brighter than that of the former Comet, though of a somewhat
lesse magnitude; having in its middle onely one round, but very bright and big:
Kernel or Speck, resplendent like Gold; and encompassed with another more
dilate and seemingly uniform matter: its Tail being at first, about 17 deg.
and afterwards 20. and sometimes 25 deg. long, and divaricated towards
the End.

Next, it is observed, that though this Star did afterwards slacken its pace,
yet it retained the vividness of its Colour, both of the Head and Train; the
Head especially, keeping at the time as well of the last Observations, as of the
first, the brightness of its single kernel, though the environing more dilute matter were then almost all lost; it being, according to the Author, more and more attenuated, and grown narrow, the nearer the Star approached to the Sun.

Thirdly, 'tis noted, that this Comet did very much digress from the Hypothesis, delivered by M. Auzout, in regard that, whereas according to that Hypothesis, this Star should not arrive to the Ecliptick till after the space of 3 months, it arrived there the 28 of April. And then, that its first Conjunction with the Sun happen'd between the 19 and 20 of April, and the second, the last of April, not (as M. Auzout, would have it) the 15 of May. So that he concludes, that this Comet never came down to the Pleiads and the Eye of Taurus, as the Hypothesis of M. Auzout requires, but that from April 20, it did immediately take its course towards the Ecliptick, deflecting every day more and more from the Section of a Great Circle, to the Lucida of Aries, arriving at the Ecliptick the last of April, about the 8th or 10th deg. of Taurus, not in July about the 8th of Gemini, and the Eye of Taurus.

Fourthly, He intimates, that if this Comet had appeared some few weeks sooner, it would have confronted the former Comet, being yet in its vigour and of a conspicuous bigness, in the same place, where that was, viz. the Head of Aries.

Fifthly, He observes, that this Star in progress of time became Retrograde, whence it came to pass, that in the Months of June and July it did not appear again before the Rising of the Sun, though the Sun left it far behind: whereas, if it had proceeded toward the Eye of Taurus, it would have appeared again in the morning.

Sixthly, He maintains, that this Comet was not the same with the former; which he thinks may be demonstrated, onely by a due Delineation of both their Course upon the Globe, where he faith it to be evident, that the former could never come to the Head of Pegasus, as moving already in February in a direct Course about the Head of Aries: Besides, that the former went in the very beginning in a Retrograde motion; but this perpetually in a direct one: that, about the end, very slow, its Head lessening and growing dark; this swift enough, with its head conspicuous and bright. To which he adds, that the whole Course of the former was made under a quite different Angle of the Orbite and Ecliptick, and a different Motion of the Nodes from the latter: As also that their Faces differed very much from one another; the first exhibiting all along a matter, which as to its density and rarity, altered from day to day exceedingly, whereas the second retained (to the Authors admiration, who affirms, never to have observed the like) all the time he saw it, one and the same round, dense and bright Speck or Kernel.

All which he concludes 1, With an Intimation of his sense concerning two other Comets, pretended to have been lately seen, One at Rome, about the Girdle...
Girdle of Andromeda, in the Months of February and March, 1664: the other in Germany in Capricorne, about Saturne in the head of Sagittary, during the Months of September and October, 1665. 2ly. With an Advertisement of what he has done in that important Work for the Advancement of Astronomy, the due Restitution of the First Stars, vid. That he has almost finish'd it, himself alone, without trulling to any other man's labour, that was not directed by him.

The Second Part of this Book (the Mantissa to the Prodromus Cometicus) endeavours to justify the Authors' Observations touching the former Comet, excepted against by M. Auzout, in several particulars; as 1. That it had not pass'd to the First, but Second Star in Aries, and had mov'd in quite another Line, than He had described. 2. That its proper motion about the end of January and the beginning of February, 1665, had not been rightly assign'd. 3. That the Bignesse of its Diameter had not been truly deliver'd; Nor 4. The Faces of its Head in due manner represented.

To all which the Author endeavors to answer: 1. By delivering all his Observations of that Comet, thereby to shew, what care and diligence, he had us'd, particularly to make out, how great its Diurnal motion had been, in what proportion, and how far, it decreased, and where and in what degree it increased again: Which being, as he conceives, duly and exactly deduced, and demonstrated, he esteems it afterwards to be easie for every one, versed in these matters, certainly to collect and to judge, what way the Comet, after it became invisible to the naked Eye, and could be no longer observed with Sextants and Quadrants, had taken, and what Line it had described. 2. By subjecting all those Observations, with great diligence and labour, to a rigid Calculus, thereby to obtain, for every day, the Longitudes, Latitudes, Right Ascensions, Declinations, Proper motion, Angle of the Ecliptick and the Equator, and the Nodes of that Comet; for the construction of an Ephemerides of its whole Motion. From all which he pretends to prove, that he has not erred in his Observation of February 18, nor been preposset by any Hypothesis, nor deluded by any First Star, as M. Auzout thinketh, but that near the First Star of Aries there then appear'd a Phenomenon, most like to that Comet, that was seen some days before, if compared with the Observations made thereof Feb'r 12, 13, 14. Though he will not hitherto positively determine, whether that Phenomenon, which appear'd to him February 18, was indeed
indeed that very Comet, which he saw with his naked Eye, and observed with his Geometrical Instruments, the said 12, 13, and 14. days of February; or whether it was another, and whether he had lost that Comet, which moved towards the Second Star in Aries: but leaves it to the Learned World, and particularly to the Royal Society, after they shall have well examined and considered all his Observations, and the Calculus raised therefrom, to judge of this, and the other particulars in controversie.

II. Isaacus Vossius de NILI et ALIORUM FLUVIUM ORIGINE. It was Numb. 14. of these Transactions, that gave an account of the Cause of the Inundation of the Nile, as it was rendered by Monseur de la Chambre: This is to give you another, not only of the Inundation, but also of the Origine of that, and of other Rivers, as it is delivered by Monseur Isauc Vossius, who undertakes in this Book to shew;

1. That those Subterraneous Channels, through which several Philosophers teach, that the Sea discharges itself into the Rivers, are not only imaginary, but useless, in regard 'tis impossible for the water to rise from the Subterraneous places up to the Mountains, where commonly the Sources of Rivers are.

2. He explicates, why, if a Pipe be put into a Bason full of Water, the water is seen more raised in the Pipe, than in the Bason; and rises higher according as the Pipe is narrower; On the contrary, if the same Pipe be put into a Bason full of Quicksilver, the Quicksilver stays lower in the Pipe, than in the Bason. The reason, which he renders hereof, is, That as the Water sticks easily to all it touches, it is sustaine'd by the sides of the narrow Pipe wherein it is included: And indeed, if the Pipe be quite drawn out of the Water, the Water doth not all fall out, but so much of it remains, as the sides of the Pipe could sustain: Whence it is, that the Water which is kept up by the Walls of the Tube, weighing no longer upon that which is in the Bason, is thrust upwards, and keeps it self raised above its Levell; but the Quicksilver not adhering so easily, as Water, to Bodies it touches, is not sustained by the sides of the Tube, and so mounts not above its Levell, but rather descends below it, because the Pipe, which is straight, hinders the endeavor that is in the Mercury to rise to its Levell. He adds, that this Observation makes nothing for the Explication of the Origine of Rivers, because, though it be true, that the Water by
by this means rises above its Levell, yet it does never run out at the top of the Pipe. Having said this, he answers to the other Arguments, commonly alleged to maintain this Opinion.

3. He pretends, that all Rivers proceed from a Collection or Rendezvous of Rain-waters, and that, as the Water, that falls upon Hills, gathers more easily together, than that which falls in Plains, therefore it is, that Rivers ordinarily take their Source from Hills. Thence also comes it (saith he) that there are more Rivers, than Torrents, in the Temperate Zones; and, on the contrary, more Torrents, than Rivers, in the Torrid Zone: For, as in hot Climates the Mountains are far higher, the Water that descends from them with impetuosity, runs away in a little while, and forms such Collections of Water, as soon dry up; but in cold Climates, the Waters do not run away but slowly, and are renew'd and recruited by Rain, before they are quite dryed up; because the Hills are there lower, and so the Bed of Rivers hath lefse declivity.

Having thus discourse'd of Rivers in General, he treats of the Nile in particular; and there

1. Observes, That the Order of the Seasons of the Year is quite inverted under the Torrid Zone. For, whereas it should be then Summer, when the Sun is near; and Winter, when the Sun is farther off: Under the Torrid Zone 'tis never lefle hot, than when the Sun is nearest, nor more hot, than when the Sun is farthest off: So that to the people that live between the Equinoctial and the Tropicks, Summer begins about Christmas, and their Winter, about St. Johns day. The reason whereof is, (saith he) that when the Sun is directly over their Heads, it raises abundance of vapors, and draws them so high, that they are presently converted into Water by the coldness of the Air; whence it comes to passe, that then it rains continually, which does refresh the Air; but when the Sun is farther off, there falls no more rain, and so the Heat becomes insupportable.

2. He proves by many recent Relations, that the Sources of the Nile are on this side of the Equinoctial in Aethiopia, of which he gives a very accurate Mappe, correcting many faults which Geographers are wont to commit in the Description of the Kingdom of the Abyssins, which they believe to be much greater than indeed it is.
3. This supposed, he easily gives an account, why the Nile yearly overflows about the end of June: For, as at that time there falls much rain in Ethiopia, it must needs be, that the Nile, whose source is in that Country, should then overflow, when those rains begin, and subside, when they cease.

There are besides, in this Book, two other Tracts. In the first, Mr. Vossius endeavours to maintain the Doctrine, he had deliver'd in his Book De Lumine, and to shew, that the Soul of Animals is nothing but Fire, that there are no invisible Atoms; nor so much as any Pores, even in the Skin of man. Here he treats also of Refractions, and alludes the Examples of several persons, who have then seen the Sun by the means of Refraction, when really he was under the Horizon.

In the second, He discourses of some points of the Mechanicks; and relates, among other things, that the Arrows and battering Rams (Arises) of the Antients did as much execution, as our Muskets and Canons; and then, that the Vehemence of the percussion depends as much upon the Length of the percussive Body, as upon the velocity of the Motion. He adds, that the Length of a Canon ought not to exceed 13 foot, and that a greater length is not only useless, but hinders also the effect of the Gun, not because the Bullet is thrown out of the Gun, before all the powder is fired (as some believe;) but because the Bullet is then beaten back into the Gun by the Air, re-entering into it with impetuosity, when the flame is extinct.

III. LE DISCERNEMENT DU CORPS ET DE L'AME, par M. de Cardemoy.

This French Treatise (but very lately come to the Publisher's hands) examines the different Operations of the Soul and Body, and the Secret of their Union, pretending to discover to every one, what he is, and what is transacting within him. It consists of six Discourses.

1. In the first, the Author examines the Notions, we have in general of Bodies and Matter; of Quantity, of Qualities; of Place; of Rest; of Motion; of Vacuity; of Forms; to shew what is to be understood by these Terms, which cause all the perplexity that is in the ordinary Physick. He begins with taking notice, that hitherto Philosophers have had no distinct notions of Bodies and Matter; from the want whereof he conceives, that almost all the Errors in Common Physick have sprung.
sprung: To rectify which, he defines **Bodies** to be **Extended Substances**; and **Matter** an **Aggregate** of **Bodies**. Whence he infers, that **Bodies** are **Indivisible** and **Matter** **divisible**; a **Body** being nothing but one and the same **substance**, whose **different extremites** are inseparable, because they are the extremities of one and the same **extension**, and, in a word, of one and the same **substance**: but **Matter** being nothing but an **Association** or **Collection** of **Bodies**, it is evident, if he conceives, Nature cannot subsist, if a **Body** in the sense he takes it, be divisible; and that **Motion** and **Rest** cannot be explicated without it. As for **Quantity**, he makes that to be nothing but *More* or *Less* **Bodies**; not allowing, that each **Body** should be a **Quantity**, though it be a part of **Quantity**; no more than an **Unite** is a Number, though it make part of a Number: so that **Quantity** and **Extension** are two distinct things with him, the first belonging properly to **Matter**, the last to a **Body**. Touching **Vacuity**, he conceives, that the **Bodies**, which compose a mass, are not every where so near one another, as not to leave some interval in several places. Neither does he think it necessary, that those intervals should be fill'd up; nor unconceivable, that there should be no **Body** between two **Bodies**, which touch not one another. And when it is said, that those intervals cannot be conceived without **extension**, and that consequently there are **Bodies** that replenish them, he frankly pronounces that not to be true; and affirms, that though it may be said, that between two **Bodies**, which touch not one another, other **Bodies** may be placed of so or so many feet, &c. yet ought it not to be inferred, that therefore they are there, but only, that they are thus placed, that there **may** be put between them so many **Bodies**, as joy ned together would compose an **extension** of so many feet. So that one conceives only, that **Bodies** may be placed there, but not that they are there: and as we can have an **idea** of many **Bodies**, though none of them be in being, so we can conceive, that some **Bodies** may be put between others, where really there are none. And when it is alleged, that if all the **Bodies**, that fill a vessel full, were destroyed, the sides of the vessel would be closed together, he prorifes he understands not that ratiocination, nor can conceive, what one **Body** does to the subsistence of another, more than to sustain themselves mutually, when they are thrust by the neighbouring ones, and therefore sees not, why the sides of the vessel should close, if nothing did thrust them together; but understands clearly, that two **Bodies** may well subsist so far from one another, that one might place a great many **Bodies** between them, or none at all, and yet they neither approach to, nor recoil from one another.
(308)

2. In the Second, he examines the Changes, which he knows in Matter, and makes it his business to explicate all those that respect Quantity, Qualities and Forms, by Local Motion, esteeming their needs no other.

3. In the third, he explains the Motion of Artificial Engins, and that of Natural ones, by one and the same Cause; endeavouring among other things to shew, that the Body of an Animal is moved after the same manner with a Watch. That cause of motion he makes the Materia Subtilis; and the finer or subtler that is, the better and fitter he conceives it to be to preserve Motion.

4. In the Fourth, he teaches, that though Experience seems to evince, that the Soul moves the Body, and that one Body moves another; yet there is nothing, but God, that can produce any motion in the World, and all other Agents, which we believe to be the Cause of this or that Motion, are no more but the Occasion thereof. In doing this, he advances certain Axioms, and Conclusions, which are in short,

a. The Axioms: That no substance has that of itself, which it can loose, without ceasing to be, what it is; That every body may loose of its motion, till it have no more left, without ceasing to be a Body; That we cannot conceive but two sorts of substances, viz. a Spirit (or That which thinketh) and a Body, wherefore they must be considered as the Causes of all, that happens, and what cannot proceed from the one, must necessarily be ascribed to the other: That to Move, or to cause motion, is an Action: That an Action cannot be continued but by the Agent, who began it.

b. The Conclusions; That no Body hath Motion of itself: That the First Mover of Bodies is not a Body: That it cannot be but a Spirit, that is the First Mover: That it cannot be but the same Spirit, who has begun to move Bodies, that continues to move.

In the Fifth, He treats of the Union of the Body and Soul, and the manner, how they act one upon the other; and esteems it not more difficult to conceive the Action of Spirits upon Bodies, and of Bodies upon Spirits, than to conceive the Action of Bodies upon Bodies: the cause of the great difficulty in understanding the two former, arising (according to him) from thence, that we will conceive the one by the other, not considering, that every thing acting according to its own nature, we shall never know the action of one Agent, if we will examine it by the notions we have of another, that is of a quite differing nature. Here he notes, that the Action of Bodies upon Bodies is not
more known to us, than that of Spirits upon Bodies; or of Bodies upon Spirits; and yet most men admire nothing but this, believing to know the other: whereas he Judges, that all things being well examin'd, the Action of Bodies upon Bodies is no more conceivable, than that of Spirits upon Bodies. Mean while the opinion of the Author touching this subject, is, That the union of Soul and Body consists only in this, that certain motions of the Body are followed by certain Considerations of the Soul, and, on the contrary, that certain Thoughts of the Soul are follow'd by certain Motions of the Body. And, having suppos'd, that Bodies are said to act upon one another, when they cause some change suitable to Extension; and Spirits to act upon one another, when they cause some change suitable to a Thought; he infers, that when a Body acts upon a Spirit, that cannot he by causing any change of motion, of figure, or parts, as having none of all these; nor when a Spirit acts upon a Body, that cannot be by producing any change of Thought, as having none: But, when this Body, or its motion, or figure, or other thing, depending upon its nature, can be perceived by a Spirit, so as, upon that occasion, this Spirit has thoughts, it had not before, it may be said, that the Body has acted upon this Spirit, for as much as it has caused all the change in it, whereof it was capable according to its nature.

In the Sixth, After he hath shew'd, what is to be understood by what we call Soul, and by what we call Body, he labours to make it out, that we are much more assured of the Existence of the Soul, than of that of the Body, which he conceives he can prove from hence, that we cannot doubt, that we think, because even doubting is thinking; but one may doubt, whether one has a body, for several reasons, which he alleges, and thinks so cogent, that he concludes, it is not evident to him by the light of reason, that he has a Body. But supposing, there be Bodies, he examines, what are the Operations, that belong to the Soul, and what those, that belong to the Body; and lastly, what those, that result from the Union of both: And then explains, how all those operations are perform'd, and particularly, Sensation; where he shews, that the Nerves, holding at one end to the Brain, whereof they are but Allongations, and being at the other end extended to the extremities of the Body; when an Object comes to touch those exterior ends of the Nerves, the interior ones in the Brain are preftently shaken, and cause different sensations according to the diversitie of Nerves, and the differing manner, in which they are shaken. And, to shew, that 'tis this shaking, that causes Sensation, he notes, that if any thing shakes the interior parts of the Nerves, though the object be absent, the Soul has preftently the same sensations.
sations, as it would have, if it were present. As, if one should knock on’s head forcibly against a wall, the shaking, which the blow gives to the Brain, moving the interior extremities of the Nerve, which causes the sensation of Light, the Soul has the same sensation, which it would have, if it saw a thousand Candles: On the contrary, if the interior extremities of the nerves are not shaken, though the object be present, it causes no sensation, whence it comes, that if a strong Ligature be made upon the middle of the Arm, and the hand be then pricked, no pain is felt, because the shaking of the nerves that are pricked, being stopped by the Ligature, cannot reach to the extremities of the Nerves, that are within the Brain.

---

Advertisement.

The following Errata, left by the Press in Num.16, the Reader is desired thus to correct.

Page 169. lin. 27. read, motion of B. above the Center; G. is also, with a semi-colon after the word Center. p.274. l. 13. it to do so the. p.277. l.24. r. natural days. p.281. l.16. r. of his. ib.l.17. r. a motion. p.293. l.4. r. enough without. ib. l.43. r. to the Sin of. p.194. l.1. r. to the Sin of.

---

LONDON,

Printed for John Martin and James Alesty, Printers to the Royal Society. 1666.

"
The Contents.

Patterns of the Tables proposed to be made for Observing of Tides; promised in the next foregoing Transactions. Other Inquiries touching the Sea. Some Considerations touching the Parenchymous parts of the Body. Observables concerning Petrification. A Relation from Paris, of a kind of Worms, that eat out Stones. Some promiscuous Observations made in Somersetshire. A Problem for finding the Year of the Julian Period, by a new and very easy Method. An Account of some Books, not long since publish'd: which are, 1. Tentamina Physico-Theologica de Deo, autbore Samuele Parkero. 2. Honorati Fabri Traictatus duo; Prior, de Plantis et de Generatione Animalium; Posterior, de Homine. 3. Relation du Voyage de l'Evêque de Beryte, par la Turquie, la Perse, les Indes, &c. par Monsieur de Bourges.

Patterns

Of the Tables proposed to be made for Observing of Tides, promised in the next foregoing Transactions; by Sir Rob. Moray.

In performance of what was promised in the last of these Papers for Observing the Tides, here are subjoined Patterns of the Tables there mentioned: One, for making the precise Time of the High waters and Low waters during one Month; that is, between New and New Moon, or Full and Full Moon. The other, for marking the Degrees of the Rilings and Fallings of the Water in Equal spaces of Time, and the Velocity of its motion at several heights: The Degrees of Heat and Cold, &c.

The Times, assigned in the first, to the High waters and Lowest Ebb, are taken out of Mr. Wing's Almanack, for this present year 1666—as he calculates them for the Month of September for London Bridge. Only, whereas he takes notice but of one High-water for every day, Here are let down
down the Times of the other, and the two Ebbs intervening, by subdividing the Differences, he assignes between two Tides, equally amongst them. In all which, though there may be Errors, that is not to be considered, seeing the Differenc is to Correct and State the Times of the Tides exactly by Experiments, after this method. Mr. Wing states the High waters to fall out at London-Bridge constantly, when the Moon is 46° deg. 30. min. to the Westward of the Meridian. For the Times, he marks for them, are made up by adding every day 3. hours, 6 minutes, to those in his Table for knowing the Time of the Moons coming to the South.

The First Table consists of two Parts, and each part of four Columns. The first part marks the Tides and Ebbs from the day of the New Moon to its Full: The other, from the Full to the next New. The first Column in both parts hath the day of the Month and Week; M. standing every where for Morning, and A. for Afternoon. The third Column hath the Character of the day of the Week prefixed to the Hour and Minute of the High-water, and answering to the day of the Month. The last Column hath the same for the time of Low-water, varying the Character of the day, as often as the Low-water falls out more early than the High-water. In this Example between the said New Moons there falls out in all just 57. periods of the Tide or Flowing water, and 58. of the Ebb or Low-water; which numbers vary according to the Intervals of the Moons changes; but with what constancy and exactness, is to be inquired after: Which whoSoever undertakes to do, may keep such a Table, as is here proposed, in a Book by it self.

The other Table doth in 9 Columns comprehend the particular Observations of the Degrees of the Rising and falling of the Tides, and the other things specified at the Tops of them: The first Column marking the Hour and Minut common to all the severall Observations. Each hour is divided in 3. equal Parts, that number of Observations being only pitch'd upon by way of Example: The numbers may else be varied at pleasure, when other more frequent Observations are thought fit to be made, or when they prove too frequent and laborious; though the most frequent are most desirable, till competent information of all particulars be attained.

The Rising of the Tide from Low-water to the highest pitch of the Full Sea, is here supposed to be 60. foot: And the Degrees of its rising every 20. Minutes, to be in the Proportion of Signes. The whole time of Flowing supposed to be 6. hours. But this Example will serve for marking the Spaces of the Increasing or Rising, as well as of the falling of the water, in order to the Investigation of their Proportions to one another, when the Duration of the Tide exceeds 6. hours by any number of minutes, as well as for just 6. hours; seeing they may be easily collected from any Number of Observations; their precise Time and that of the Duration of the waters Rising and Falling (that is, the just interval between the High-water and Low-water) being known: This Calculation by Signes being only set down as a Conjecture, flowing from Observations of the Motion of the water in its Rising and Falling, which
which seems to observe this, or some such like Proportion; which is supposed
still to hold in all Tides, be the Duration what it will; the Increase still
continuing proportionably till the very midst of the Hight and Duration, and
Decreasing afterwards in the same manner: Which whether it be so indeed
or not, is that, which is desired to be known.

There is the like Proportion here supposed to be in the different degrees of
the Velocity of the Current of the Water after Equal spaces of Times, as in
its Rising and Falling: And so it is mark'd in the Third Column. But because the
ture Velocity of the Current of the Water, raised above the Levell
\( \frac{\text{a}}{\text{b}} \) of a-foot, is unknown, it is by way of Supposition set at Ten feet in
one Minute of an Hour, which being once stated, the rest distant from each
other by the space of 20 Minutes of an Hour, are set down according to the
same Proportion of Signs before suggested. It being supposed, that of the
Velocity of the Current of the Tide, after it hath flowed 20 minutes of
an hour, be such, as a Log of Wood placed in the Water will move 10 foot
in the space of one minute of time, at the middle of the Tide it will in the
like space of Time move 114 f. \( \frac{\text{a}}{\text{b}} \), and so proportionably at other times:
Which, howsoever these Proportions shall be found by Experiments to fall
out, may be not unworthy of the pains and charges requisite to acquire the
knowledge of it. For, besides the satisfaction it may afford upon other ac-
counts, it may possibly be of no small use to those, who need an exact rec-
koning of their Ships running, when the Velocity of the Current of the
Tide may be necessary to be known; left through the defect of the know-
ledge of that, especially when it is reckoned less than indeed it is, the Ship
be thrown in the night upon Shores, Rocks or Sands, when they reckon them-
selves to be far from them.

The Numbers in the 4, 5, 6, 7, and 8. Columns are set down at random,
only for Examples sake; there being no difficulty in the apprehension of it,
and imitating of it in setting down the true Hights and Variations of the
Thermometer, Baroscope, &c. The Use whereof is so vulgarly known, that
there needs no further Direction concerning them. But if any person who
would make these Experiments, do not know the fabrick or use of any of
the Instruments requisite for some of these Observations, nor where to have
them, he may address himself to Mr. Shortgrave, one of the Operators of the
Royal Society, lodged in Gresham Colledge, from whom he will receive full
satisfaction about these things.

But the labour employed in the Observations of the Heat, Cold, &c, required to be taken notice of in order to the Ends proposed in the former
Tract, and others that may be of no less delight than advantage, will be
much retrenched, when Dr. Christopher Wren puts in practice, what he some
years ago proposed to the Royal Society concerning an Engine with a Clock-
work, which may perform these Observations in the last enumerate Columns,
without being toucht or lookt after but once or twice a day.

The Tables themselves follow,
| Age | Time of High-water | Time of Low-water | 1666 | Age | Time of High-water | Time of Low-water | 1666 | Rising, and fall of Tides | Velocity of | Baro. | Hygro | Azimuth of | Weather |
|-----|--------------------|-------------------|------|-----|--------------------|-------------------|------|--------------------------| metre     | scope | Trench | force of | the Wind |              |
| 1   |                        |                   | 1666| Age |                      |                   | 1666| Rising, and fall of Tides | Velocity of | Baro. | Hygro | Azimuth of | of |              |
|     |                        |                   |     |     |                      |                   |     | Feet                      | metre     | scope | Trench | force of | the Wind |              |
|     |                        |                   |     |     |                      |                   |     |                          | Foot      | inch  |    | Wind.    | deg.     |              |
|     |                        |                   |     |     |                      |                   |     |                          |           |       |    |          |          |              |
|     |                        |                   |     |     |                      |                   |     |                          |           |       |    |          |          |              |

**Notes:**
- Age refers to the age of the moon in days since the new moon.
- Time of High-water and Time of Low-water are given in hours and minutes.
- 1666 likely refers to a year in a historical or astronomical context.
- Rising, and fall of Tides are measured in feet.
- Velocity of meteor is measured in feet per minute.
- Baro. scope and Hygro. trench are measurements used in meteorology.
- Azimuth force of the Wind is measured in degrees from the north.

**Weather:**
- Rain great, Rain small, Rain very small, Fair but cloudy, Fair and warm, Warm and cloudy, Sun shine, Cloudy (the Horizon), Hazy about, Misty, Cleaning up, Clear, Sun shine.
A Perpendicular Line divided into Signes, supposed to be the Periods of the Risings and Fallings of the Tides, as is in the other Table represented.

| a | The Low-water | 5, 049 |
| b | 4, 740 |
| c | 4, 284 |
| d | 3, 696 |
| e | 3, 000 |
| f | 2, 211 |
| g | 1, 353 |
| h | 0, 456 |
| i | 0, 456 |
| j | 1, 353 |
| k | 2, 211 |
| l | 3, 000 |
| m | 4, 284 |
| n | 3, 696 |
| o | 2, 211 |
| p | 3, 000 |
| q | 2, 211 |
| r | 1, 353 |
| s | 0, 456 |
| t | The High-water | 5, 049 |
| u | 4, 740 |
| v | 4, 284 |
| w | 3, 696 |
| x | 3, 000 |
| y | 2, 211 |
| z | 1, 353 |
| A | 0, 456 |
Other Inquiries Concerning the Sea.

The Publisher of these Tracts, knowing, that the Honorable Robert Boyle had not left unconsidered the Natural History of the Sea, of which Subject the late, and these present Papers, have entertained the Reader as to the Observables of its Flux and Reflux; He was on this occasion infrant, with that Gentleman to impart to him, for publication, these Heads of Inquiries, he had drawn up, touching that Subject: Which having obtained (though the Author desires, they may be lookt upon as unfinished) he thus subjoyns.

What is the Proportion of Salt, that is in the Water of differing Seas; And whether in the same Sea it be always the same? And if it be not, how much it differs?

What is the Gravity of Sea-waters in reference to Fresh Waters and to one another: Whether it vary not in Summer and Winter, and on other Scores? And whether in the same Season its Gravity proceed only from the greater or lesser Proportion of Salt, that is in it, and not sometimes from other Causes? And what are the differing Gravities of the Sea-water, according to the Climates.*

What are the Odors, Colours and Taffs, observable in Sea-water?

What is the depth of the Sea in several places, and the Order of its increase and Decrements. And whether the Bottom of the Sea does always rise towards the Shore, unless accidentally interrupted?

Of the Bottom of the Sea, and how it differs from the Surface of the Earth, in reference to the Soyl, and Evenness or Roughness of the Superficies; And the Stones, Minerals and Vegetables to be found there?

What the Figuration of the Seas from North to South, and from East to West, and in the several Hemispheres and Climates?

What communication there is of Seas by Streights and Subterraneal Conveyances?

Of the Motion of the Sea by Winds, and how far Storms reach downwards towards the Bottom of the Sea?

Of the grand Motions of the Bulk or Body of the Sea; especially of the Tides: Their History as to their Nature and Differences.

* This last Clause containing a difficult Quære and that may seem something odd, Mr. Boyle thinks fit to note. That having recommended this matter, among others, to a learned Physian, that was sailing into America, and furnished him with a small Hydrostaticall Instrument, to observe from time to time the Differences of Gravity he might meet with: This account was returned him, That he found by the Glass, the Sea-water to increase in weight, the nearer he came to the Line, till he arrived at a certain Degree of Latitude; as he remembers, it was about the 30th; after which, the Water seemed to retain the same specific gravity, till he came to the Barbadoes, or Jamaica.
What power the Sea hath to produce or
hasten Putrefaction in some Bodies, and to
preserve others; as Wood, Cables, and others
that are funk under it?

Of the Power ascribed to the Sea to eject
Dead Bodies, Succinum, Ambergris?

Of the shining of the Sea in the night?

What are the Medical vertues of the Sea, especially against Hydrophobia?

What is its vertue to Manure Land? And what are the Plants, that thrive
best with Sea-water.

Some Considerations
Concerning the Parenchymous parts of the Body.

These were communicated by the inquisitive M. Edmund King at the
Instance of the Publisher, as follows;

The Parenchymous parts of the Body, are by Anatomists generally suppo-
sed to be in very many places wholly void of Vessels; designed chiefly to fill
up Cavities and Interstices between the Vessels, and to boulter up the same,
and to convey them through the parts.

But having many years endeavoured to excarnate several parts of the Body,
 viz. the Liver, Lungs, Spleen, Kidneys, &c. (not to name the Placenta Vite-
ri, which seems to be Parenchymous too;) and being very desirous to make a
Scheme of the Vessels of any of these, what ever they were, I fixt upon;
I found, notwithstanding all my care to preserve the Vessels, when I was
freeing them, as heedfully as I could, from the supposed Parenchyma, that
in every breach, I made, either with my fingers or otherwise, all my en-
deavors were destructive to my purpose: and if, upon examination of those
bits, much of which is called Parenchyma, I met in them more Vessels,
than I had preserved in the parts whence they came: And though the Portion
were never so small, yet my bare eye could make this discovery, much more
could I, when asisset by a Microscope, perceive, I had destroyed more Vess-
els than preserved, in despite of the exactest care, I was capable to use.
And being not a little concern'd, that I should undertake to preserve the
Vessels by such a Caufe, as I saw plainly to be their destruction (were the
part never so big, or never so small,) I was both confounded and tired.
For I saw (and I must say, that will attempt this work) in my endeavour-
ing to preserve one Vessel of a traceable magnitude, I spoiled an infinite
number of others less discernible, which were as truly Vessels, as the other,
differing only in size and figure (as to appearance.) Then reviewing, what
mischief I had done in everyplace, quite through the whole Traft of my
Fingers, Knife, &c. I began to think with my self, That it was not impos-
sible for these parts to consist wholly of Vessels curiously wrought and inter-
woven (probably for more Uses, than is yet known;) And the considera-

\[316\]
tion, which came into my mind, of a piece of fine Cloth (which consists of so many several minute Hairs, call’d wool) was no discouragement to this opinion. Yet I durst not be presumptuous as to indulge my self too much in it; much less to venter presently to speak of a thing, which seem’d to contradict so many Learned Men’s belief. But being reflect, till I might receive more satisfaction in the thing, I iterated experiments over and over; some of which prov’d so successful to my apprehension, that I was encour-aged in the year 1663, and 1664. to discourse of it to several very worthy Persons, as Mr. Boyle, Sir William Petty, Dr. Williams, Dr. Lenthal, Dr. Jasper Needham, Dr. Samfon (who afterwards sent me a Letter from France, intimating the acquaintance he had made with the Learned Steno, who hath since publish’d something of the same Discovery) Mr. Daniel Cox, and Dr. Samuel Parker, &c, who doubtles cannot but remember, that then I related to them, I found much cause to believe, that that substance commonly call’d Parenchyma, was in most, if not in all its Parenchymous parts, full of Vessels; however it had been imagin’d by all, I could ever meet with, to consist in great part of a substance, in many places void of Vessels, desig-ned for such ules, as are above mentioned.

Against which I have now further to alledge, 1. That I observe in a piece of Muscularous Flesh (so call’d) either raw, rosted, or boiled, &c. that if I so far extend it, as to make it to be seen through, I can (as I suppose) perceive it full of Vessels placed as thick as is possible to be imagin’d, the fat is there any, being first removed) there appearing then nothing but Vessels, yet so as with a Microscope may be seen through, when they are extended, 2. That if any one, as he is at dinner, take a piece of flesh, and begin either at the head or tail of a Muscle, he may divide it in infinitum, all along from head to tail, without breaking any thing of that, called Flesh, only these transverse Fibres, that seem to stitch them together, and (as I am apt to think) pass through the very Bodies of the smallest of them, and quite through the whole Muscle up the Cutaneous porosities; so that there is not one of these small ducts, that run per longinudinem, but ’tis furnish’d with a sufficient number of outlets, when need requires, though too minute to suffer any alimentary juice to pass transversely (in a living Body) or any other liquor, when the Body is dead and cold. But to wave their use at present, and to return to what I was saying, Compress between the fingers this bit of flesh, and you shall find the juice, especially if the Meat be hot, to go before your fingers to-ward either end you please; but if you compress both ends, you shall see it swell into the middle; and again, if you press the middle, it will run out at both ends. But further, suppose a piece of flesh, called Parenchyma, as big, or as little as you please, in any part of the Body, and let me prick it with a Needle, where you shall appoint; if you feel it, I presume you will acknowledge, a Nerve, or a Fibrilla, related to it, is touch’d; if you feel it not, I am sure some liquor either sanguineous or other, will follow the Needle: And from whence can that come, but out of Vessels? unless accidentally
accidentally, as by a Contusion, &c., it be extravas'd; in which case my Argument will not be injured, because the part is depraved, whereas I speak of the parts, as they are in their natural State.

To confirm and illustrate all which, I desire, that the following familiar Observations may be considered:

1. If a Horse, fat and fair to look on, without a hollow to be seen between his Muscles, be rid extreme hard, and into a great sweat, and then kept one day without water or moist meat, you shall see him look so thin in many places as in the mufcular parts, that you will hardly believe it to be the same Horse, especially if he be (as the Phrase is among Horse-masters) a Nafs or Wash-Horse. The cause of which thinness will easily be granted to be only an exhaustion of Juice, expended out of the Blood, which did stuff out these Vessels. And whoever, that is used to ride hard, shall observe, how thick this foul Horse breaths, and at what a rate he will reek and sweat, will not much wonder at the alteration. But if the Horse be a hardy one, and used to be hard ridden, then you will see, that one days rest, and his belly full of good meat and drink, will in one day or two almost restore him to his former plight, the food being within that short space of time so distributed, that all the Vessels will be replenished again, as before. And the cleaner the Horse is, the sooner recruited, and the least sign of hard riding will appear. This seems to shew the facility, with which the Juice, called Blood, passeth; Which surely, if there were such a thing as a Parenchyma, might by several accidents (not difficult to mention) be so deprav'd in several parts of it, that it might lose its receptive faculty, than which it may be thought to have none of greater use, being supposed to be without Vessels.

2. Discoursing sometimes with Grafters in the Country, about the Pasture of Cattle, I have been informed by them, that, if they buy any Old Beasts, Oxen, or Cows, to feed, they choose rather those that are poor, as can be, so they be found; because that, if they are pretty well in flesh, what they then add to them by a good pasture, though it make them both look and feel well, yet it will not make them eat so well, their flesh proving hard and very tough: Which some may suppose to be the age of Parenchyma, and so it is of that so called. But if those Beasts be old and extremely poor, then they feed very kindly, and will be not only very fat, but spend well, like young ones, and eat very tender.

Of which I take the reason (excluding a Parenchyma now) to be this. When an Ox or a Cow is grown old, and in an indifferent plight as to his flesh (for so it is call'd) all those Vessels having been kept at that size for the most part, have contracted a teneness and firmnes, and their fibers less extensive, not so fitted for the reception of more unctuous particles to relaxe them: and that additional unctuous matter, which occasions firmnes, is forced to seek new quarter, any where (often remote from Muscles) where it can be with least difficulty received; sometimes to one place, sometimes to another.
ther, as may be seen in Shambles: Whereas, if there were such a thing as a Parenchyma, that certainly would, like a hungry Sponge, immediately swell up in several parts, (which without much difficulty might be discover’d in dissection) and more eminently, where it should find the pores most patent: And in the dissection of such Muscles it would be very strange, not to find some, if not many, pieces of them in various shapes, to the great inconvenience of the parts, in which they are seated: Which yet I confess I could never find in any Muscle, unless it were where there had been a Contraction, or an Impostume, or the like. But according to my opinion of the Parenchymous parts, the reason, why the Flesh of a very lean Oxe or Cow, that hath got new Flesh in a good pasture, eats tenderer, seems to be this: That in a very lean Beast the Vessels dessein’d for admitting and distributing the nourishing Juice, are so near contracted, and lye so close together, that, when once they are relax’d, by the fresh and unctuous nourishment, they extend every way in all extensive parts, until a short time the whole Creature is, as it were, created a new, having got new flesh upon old bones: And the necessity of extreme extension makes all those parts, that are, as has been said, for the admission of nourishment, so thin and fine, that it will make the lean Beast, put into a rich pasture, eat young and tender: Whereas one of the same Age, that never was very poor, fed in the same pasture, shall eat hard and tough.

3. It has been observed, that Corpulent Persons in some Diseases, that seize on them, do fall away to wonder, not only in the Waste, but in the Arms, Legs, and Thighs; and the very Calves of the Legs have been observed to flaccid and loose, that one might wrap the skin about the bones: The reason whereof, according to the opinion deliver’d, may be easily rendred to be, A great Consumption of the Stock of Liquors, that in Health kept the Vessels turgid: Which Vessels I suppose to make up those Muscles. But when the Pores are obstructed, that the nourishment is hindered (which then also ues to be but sparingly administered) and sweats, either spontaneous, or forced, are large, there must needs be a great expence of those Liquors, the supply being but inconsiderable: which cannot but contract all these ducts of all sorts nearer together, and make them much less in themselves, meekly from Exhaustion: Or, if there should be no sweats, the internal Heat spends the spirits, and dries up the Liquors; the consequence whereof may reasonably be presumed to be this Flaccidity of parts, and great and sudden Change, made in them; not that there is need of any Parenchyma to fill up those Muscles, considering what hath been said. Mean while, I humbly conceive, that if it be in any part of a Muscle, their Ingenuity, that plead for it, will put them upon some experiments, to bring it to Ocular Demonstration, either in a Living or Dead Muscle, any kind of flesh, raw, roasted, boy’d, or in what they can beft make it out. And when I shall be convinc’d of an Error in what I have discurr’d, I shall beg pardon for giving the Occasion of the trouble of that Experiment, which shall prove a
Parenchyma in any Muscle; and think my time well spent in receiving a full satisfaction of the ungroundedness of my opinion; and readily submit to the Author, with a grateful acknowledgement of my Obligation to any one, that shall rectifie me in my mistake, if it be one.

Observables

Touching Petrification.

Though much hath been already laid and written of Petrification, yet this conceived, that all that comes so far short of a competent fcock for the composing of a perfect History of Petrification, that the incompleteness thereof ought to awaken the more diligent attention of the Curious, and to call in their aid for Additions, thereby so to increase and to complete the Materials for that work, that it may the better serve to clear and make out the Cause of that Transmutation. And that the rather, because if it lay in the power of humane Skill (by the knowledge of Nature's works) to raise Petrification, or to allay, or prevent it, or to order and direct it (which perchance in time might be attained the said way) much use might be made of this Art; especially if it could be made applicable to hinder the Generation of the Stone and Gravel in humane Bodies, or to dissolve the Stone, where 'tis formed; besides other valuable Uses, that might be excogitated.

Upon this Consideration, care is, and further will be taken in these Papers, to record, among other Observables of Nature, what shall be communicated of this kind of Change.

In Numb. 1. 2. and 5. several Relations have been made belonging to this Argument. Much of it, together with considerable Reflections may be seen in Mr. Boyle's Essay of Firmness: In Helmont de Lithias, where, among other remarks, is recited the Testimony of Parasus, of a Petrified Child seen at Paris, and by the Owner used for a Wheestone: In Deufingins's Historia Infantis in Abdomine inventae, & in duritiem lapideam conversi: In Mr. Hook's Micrography, and in others. To omit now, what has been related (but perhaps not well enough attested) by Authors, concerning the stupendious Petrifications of whole Companies of Men, and Troops of Cattle; by Aventinus Lib. 7. Annal. Bojorum; by Purchas in his Pilgrimage p. 426. in fol. printed at London 1614. and, (of a Troop of Spanish Horsemen) by Jos. Acosta lib. 3. c. 9.

To all which, the Curious Dr. Beale now adds a Narrative of a Stone, not long since taken out of the Womb of a Woman of his neighbourhood near Trent in Somersetshire, by incision, and afterwards perfectly cured, though she had born the Stone with extreme torments for 8. or 9. years. The operation he relates to have been made in Easter last; after which time, he affirms to have seen the Stone, and weighed it in Gold-Scales, where it wanced somewhat of four Ounces, but had lost of the weight, it formerly had, being
being very light for a Stone of that Bulk. He further describes it to be of a
whitish colour, lighter than Ash-colour; perchance (faih he) not unlike
to that recited out of Scaliger by M. Boyle in his Eflay of Firmness pag. 238.
qui a staring contactu postea in gypseam tum speciem tum firmitatem convenit.
It had no deep asperities, and had somewhat of an Oval figure, but Iefs at
one end, than a Hen-Egge, and bigger and blunter at the other, than a
Goole-egge.

This Stone (to he concludes) is intended for the Royal Society, with the
Testimony of the Chirurgeon, that perform'd the Operation, and other Wit-
nesses of special credit; where allo will be annexed the manner of
Operation.

It appears by this laft clause (to add that on this occasion) that this Well-
wlsher to the Improvement of all wsefull knowledge, has taken notice of that
considerable Collection of Curiosities, lately presented to the lately nam'd
Society for their Repository, by that Publick-minded Gentleman Mr. Da-
niel Colwall, a very worthy and Useful Member of that Body: To which
Repository whatsoever is presented as rare and curious, will be with great
care, together with the Donors names and their Beneficence recorded, and the
things preferred for After-ages, (probably much better and safer, than in
their own private Cabinets;) and in progress of Time will be employed for
considerable Philosophical and Usefull purposes; of which perhaps more
largely in another place.

A Relation
Of a kind of Worms, that eat out Stones.

This is taken out of a Letter, written by one M. de la Voje to M. Anzert,
to be found in the 32. Journal des Scavan; as follows.

In a great and very ancient Wall of Free-Stone in the Benedictins Abby at
Caen in Normandy, facing Southward, there are to be found many Stones
to eaten by Worms, that one may run his hand into most of the Cavities;
which are variously fashion'd, like the Stones, which I have seen wrought
with so much Art in the Louvre: In these cavities there is abundance of live-
Worms, their excrement, and of that Stone-dust, they eat. Between many
of the Cavities there remain but leaves, as it were, of Stone, very thin,
which part them. I have taken some of these living Worms, which I found
in the eaten Stone, and put them into a Box with several bits of the Stone,
leaving them there together for the space of eight dayes; and then opening
the Box, the Stone seem'd to me eaten so venibly, that I could no longer
doubt of it. I lend you the Box and the Stones in it, together with the living
Worms; and to satisfy your Curiosity, I shall relate to you, what I have
observed of them, both with and without a Microscope.
These Worms are inclosed in a Shell, which is grayish, and of the bigness of a Barly-corn, sharper at one end, than the other. By the means of an excellent Microscope I have observ'd, that 'tis all overspread with little Stones, and little greenish Eggs; and that there is at the sharpest end a little hole, by which these Creatures cast out their excrement; and at the other end, a somewhat bigger whole, through which they put out their heads, and fasten themselves to the Stones, they gnaw. They are not so shut up, but that sometimes they come out, and walk abroad. They are all black, about two Lines of an Inch long, and three quarters of a Line large. Their Body is distinguish'd into several pleys, and near their head they have three feet on each side, which have but two Joints, resembling those of a Lowle. When they move, their Body is commonly upwards, with their mouth against the Stone. They have a big head, somewhat flat, and even, of the colour of a Tortoise-Shell, braunish, with some small white hair. Their mouth is also big; where may be seen four kinds of Jaw-bones, lying crosswise, which they move continually, opening and shutting them like a pair of Compasses with four branches. The Jaws on both sides of the mouth are all black; the nether Jaw hath a point like the Sting of a Bee, but uniform. They draw threads out of their mouth with their fore-feet, using that point to range them, and to form their Shells of them. They have Ten Eyes, very black and round, which appear to be bigger than a Pins-head. There are five of them on each side of the head, standing after this manner:

```
               o          o
          o        o
     o    o    o
```

But besides these Worms, I have found, that Mortar is eaten by an infinit number of small Creatures, of the bigness of Cheele-Mites. These have but two Eyes, and are blackish. They have four feet on each side pretty long. The point of their Muzzel is very sharp, as that of a Spider. I send you but one of them, though I had abundance, but they are dead and lost. It may be, you'll find some at Paris, seeing that in the old Mortar betwixt Stones, that is found in Walls made with rubbish, there is great store of them, together with great plenty of their little Eggs. I have not yet examined, whether these be those, that in the surfaces of all the Stones, where they are met, with, make little round holes, and small traces and impressions, which make them look like Worms-eaten Wood. But 'tis probable, they are such. It should be observed, whether these Worms do not take Wings, and all the other appearances of Cater-pillars; and whether they are not to be found in Plaiter, that is full of holes, in Bricks, in Greety Stones, and in Rocks.

You may observe more of them in Walls exposed to the South, than in others; and that the Worms, that eat the Stone, live longer, than those, that
that eat the Mortar, which keep not above eight dayes alive. I have observed all their parts with a very good Microscope, without which, and a great deal of attention, 'tis difficult to see them well.

I have seen other very old Walls altogether eaten, as those of the Temple at Paris, where I could find no Worms, but the Cavities were full of Shells of various kinds, diversely figurd and turn'd : all which I believe to be little Animals petrified.

_Some promiscuous Observations, made in Somerset-shire, and imparted by the above-mention'd Dr. Beale._

His words are these, in a Letter to the Publisher, of the 24. September, 1666. at Yeovil in Somersetshire;

I have two or three remarks, perhaps not unworthy to be recorded for further application in like cases of time and place.

1. In the Moores from hence towards Bridgewater, in the extreme drought, we have endured this Summer; some lengths of pasture grew much sooner withered and parched, than the other pasture. And this parched part seem'd to bear the length and shape (in gross) of Trees. They digg'd, and found, in the place, Oakes indeed, as black as Ebony. And hence they have been instructed to find and take up many hundreds of Oakes, as a neighbour of good credit assures me. This advertisement may be instructive for other parts, as Kent, Essex, Lincoln, &c.

2. My Colen Philips of Montague has in his pastures of Socke, about three miles off, a large Pool, to which Pigeons retort; but the Cattle will not drink of it, no not in the extremest want of water in this drought. To the taste it is not only brackish, but hath other loathsome tasts. In a Venetiglass it looked greenish and clear, just like the most greenish Cider as soon as it is perfectly clarified. I boy'd a Pint of it in a Pofnet of Bell-Mettall (commonly used to preserve Sweatmeats:) suddenly it yielded a thick froth, whence I scumm'd half a score Spoonfuls; of which the inclosed is a part.

3. From Lamport, towards Bridge-water, Eales are so cheap in the frosts of Winter, that they vend them for little. Their abundance is from hence, that as the people walk, in the frosty Mornings, on the banks of the river, they discern, towards the edges of the banks, some parts not hoare, as the rest, but green; where searching the holes of the banks, they find heaps of Eales.
A Problem for finding the Year of the Julian Period by a new and very easie Method.

This occurs in the Journal des Scavans n°. 36, as it had been proposed and communicated by the Learned Jesuit DE BILLY, viz.

Multiply the Solar Cycle by 4845, and the Lunar, by 4200, and that of the Indiction, by 5916. Then divide the Sum of the products by 7980, which is the Julian Period: The Remainder of the Division, without having regard to the Quotient, shall be the year enquired after.

E.g. Let the Cycle of the Sun be 3; of the Moon 4; and of the Indiction 5. Multiply 3, by 4845, and you have 14535; and 4, by 4200, comes 16800; and 5, by 6976, comes 34380. The Sum of the products is 65915, which being divided by 7980, gives 8, for the Quotient, and the number 2075, which remains, is the Year of the Julian Period.

Some learned Mathematicians of Paris, to whom the said T. de Billy, did propose this Problem, have found the Demonstration thereof; as the same Journal intimates.

An Account of some Books, not long since published.

I. TENTAMINA PHYSICO-THEOLOGICA DE DEO, Sive THEOLOGIA SCOLASTICA, ad Normam Nova & Reformata Philosophia concinnata, & dubius libris comprehensa. Quorum altero, de Dei existentia adversus Athaeos et Epicureos ex inpormmnet Principis disputatur; altero, de Eiusdem Essentia & Attributis; primo, secundum Theologiæ Ethicam, ubi explicatur, Quantum haecens Alii in Gentilium sententiis, de jummi Numinis Natura crucidis, hallucinari fuerint; deinde secundum Theologiæ Christianam: Et quid de Divina Essentia ac Attributis sit, destinatur. Quibus propter accedit specialis Dissertatio de Primo Numinis Attributo, AETERNITATE. Author: S. Samuele Parkero, A. M.

This Treatise, publish'd the last year, would sooner have been taken notice of in these Tracts, had it not escaped the Publishers view till late, when he, upon serious perusal, found it very worthy the recommending it to all sorts of persons, and particularly to those, who either please themselves with that fond opinion, That Philosophy is the Apprentiship of Atheisme; or hearken to the aspersions, that are generally laid upon the Reformation of Philosophy.

This excellent piece removes both these; and being joined and compared with the truly Noble Mr. Boyle's Considerations in his First Part of the Useful
Usefulness of Experimental-Natural Philosophy, will strongly evince, How Much that Philosophy, which searches out the real Productions of Nature (the true Works of God) does manifest the Divine Glory more, than the Notions of the Gentils.

This Author (now a Fellow of the Royal Society) delivers his Matter in two Books.

Lib. 1. Cap. 1. Atheists are disappointed of the Authority of Epicurus, and of other Antient Philosophers, for their gross Atheisme.

Cap. 2. The beautifull Frame of the World evinceth the Architectonical Author and Governor.

Cap. 3. The admirable Contrivance in the Structure of Mankind, and of Animals, does more conspicuously shew the Deity.

Cap. 4. The Atheist caught in his one Net, or convinced by the true force of his own Arguments.

Cap. 5. The Arguments devised against Atheists by Des Cartes, and drawn from the Idea's of our Mind, examin'd and found imperfect and invalid.

Lib. 2. Cap. 1. The Opinions of the Gentils concerning God, unduly applied to the Deity, which we worship; but properly to be understandd by them of the Sun, or of the Soul of the World.

Cap. 2. More expressly proved, that the Antient Philosophers conceived, the Soul of the World to be God.

Cap. 3. The Historical Theology of the Gentils, for the most part is unduly applied or accommodated to the Holy Scriptures.

Cap. 4. The Divine Substance, Immensity, Incomprehensibility, Invisibility, explicated, as far as our weak reason does reach.

Cap. 5. The Divine Perfections, and other Attributes and Affections, how far explicable.

Cap. 6. The Eternity of God, how apprehended.

These are in short the Heads of the Book, which is yet but in Latin. It were to be wish'd, the Author would make it speak his own lively English.

As the Matter of this Book is considerable, so is the order and dependance of all its parts excellent; in regard that all the Propositions are ranged according to a Geometrical method, and so well disposed, that the latter do always suppose the former, and seem to depend all of them upon certain evi dent principles, whence they flow by a natural consequence.

This Volume contains two Treaties.

The First is divided into 5. Books. In the four first, he treats of Plants, and distributes them into three Classes; some growing in the Earth, as Trees; others, growing upon Plants, as Mosses; and a third sort, growing upon Animals, as Hair, Horns, and Feathers. He examines and considers the Parts.
Parts of all these Plants and their Life, the manner, how they are produced, and nourished; and their different Qualities. He discourses also of Bread, Wine, Oyle, and the other Mixtes, that are made of Plants.

In the Fifth Book, he treats of the Generation of Animals, where he delivers many curious matters, explicating in a very easy and familiar way that Argument, which hath always been lookt upon, as one of the obscurest in Natural Philosophy.

The Second Treatise consists of 7 Books; wherein the Authors considers, what appertains to Man. He discourses first, of Digestion, of the Circulation of the Blood, and of the Life of the principal parts of the Human Body. Next, he treats of the Senses, External and Internal; of all he Motions of the Body, both Natural and Voluntary; of the Sensitive Appetite, and the Passions; Thence he proceeds to the Temperaments, Habits, Instinct, Sleep, Sickness, &c. Lastly, passing to the Rational Soul, he endeavours to demonstrate the Immortality thereof, and to explain also the Manner, how it worketh upon the Body, and is united with the Body; where he omits not to reason of all the Powers of the Soul, of Liberty, and of the Operations of the Understanding and Will.

In general, the Authour makes it his Study, for the explicating of the most perplexed Difficulties, to shew, that Nature works not but by very simple and easie wayes.

In particular, he interperses several curious remarks. E.g. He teacheth how to make Perspectives, that magnifie Objects, without Glasses; telling us, that when an Object is lookt upon through a small hole, it appears much greater than it is; and that therefore, if instead of Glasses one did cast before ones eyes two Plates having little holes in them, it would furnish us with a new kind of Perspectives, more commodious than those of Glasses, which spoil the Sight by reason of the refraction of the Rays, caused thereby. Again, He renders the cause of that common, but surprizing, effect of Painters, drawing certain Portrairtures, which seem to look directly upon all their Beholders, on what side soever they place themselves: Videli That in those Pictures, the Nose is a little turned to one side, and the eyes to the other. Whence it comes, that such Pictures seem to look to the right side, because the Eyes are indeed turned that way; but they appear also to look to the left, because the point of the Nose is turned that way, and the Table, whereon the Picture is drawn, being flat, the Looker on perceives not, that the Eyes are turned the other way; which he would do, if the Eyes of the Portrait were convexe: Whence it comes, that no Figure can be made embossed, which looks every way.

The art, which he teacheth of making Parsley shoot out of the ground in a few hours, is this. Infuse the seed of it in Vineagar; and, having sown it in good ground, cast on it a good quantity of the Ashes of Bean-Cods, and sprinkle it with Spirit of Wine, and then cover it with some linnen. He mentions also, that if you calcine Earth, and then water it well, it will produce
produce a great variety of different Herbs; and that the Ashes of Corn burnt, being sown, have sometimes produced other Corn.

To add that by the by, This Author is not so addicted to Aristotle, as to be on his side, when he thinks Truth is not. He hath emancipated himself considerably from the Scholastick way of Philosophing. He dares maintain, that the Vegetative and Sensitive Souls are not Substantial Forms; and that it is with Plants and Animals, as with Artificial things, the Form whereof results from the Union and Disposition of the parts. According to this Hypothesis, he explicates all the Operations of Plants and Animals, without having any recourse to the Soul. He avers also, that there are no Species Intentionales, and no Habitudes, and that the Animal Spirits, which Philosophers commonly believe to be necessary for all the Operations of Life, are useless.

It might also be observed out of this Author, what he discourses of the Generation of Animals by Putrefaction; of the Cause of Intermittent Fevers, and of the Animal instinct, and of many other particulars; were it not better to refer the Curious to the Book itself.

III. RELATION DU VOYAGE de l'Evêque de Berype, par la Turquie, la Perse, les Indes, &c. jusques au Royaume de Siam, & autres lieux; par M. de Bourges, Prétre &c.

This Author employing his Pen chiefly, according to his design, to give an Account of the Success, the Undertakers of this Voyage had, in propagating the Christian Faith in the remoter parts of the World, and relating on that occasion, What number of Churches they have Founded in Cochin-China, and the Kingdom of Tonquin (in which latter alone he affirms, that there are more then three hundred thousand Christians;) Being, I say principally intent upon that Subject, he seems not to have made many Philosophical observations in those places. Mean while he does good service to those, that have occasion to travel into the East-Indies mostly by Land, by describing the passage, they took thither; which was, That they embarked at Marseilles; in September, the most convenient and favourable season for that Voyage; whence Ships do ordinarily pass every Month from Syria, reckoning one Month for the time of Sayling to Alexandria. Thence to Aleppo, counting one month more for the Stay, to be made there to meet the Caravane for Babylon, and six weeks more for the March from Aleppo to Babylon; where a fortnight will pass, before an opportunity happen to embarque upon the Tyger for Balfora; which Journey will require a fortnight more. And about this time it will be near the end of January. Thence is always convenience to pass to Congo, 4. days Journey from Comoron or Gombroun; to which latter part there is also frequent occasion to pass by sea from Balfora, which will take up some 15 or 16. days Sail. There (vid. at Comoron) you will every year meet with English, Portuguese, Dutch, and Morishe Vessels, for Surate, from October till the end of April; for they are obliged to be at Surate, before the end of May, because all the ports of those

[327]

X

Indies
Indies are short the 4. ensuing months, by reason of the danger of that Sea.

But besides this Direction, the Book is not quite destitute of Natural Observations. It relates, 1. How Diamonds are found and separated in Golconda: They take off the Earth, held to be proper to form them, which is reddish, and distinguish'd with white veins, and full of flints and hard lumps. Then they put near the places, which they will digge, a close and even Earth; and to it they carry those Earths, they have digged out of them, Mine, and gently spread it abroad, and leave it expos'd to the Sun for two days. Then being dry'd enough, they beat it, and sifting this Earth, they find the Diamonds in ashes of Flints, in which Nature hath fet them. Heretofore he adds, that the King of that Country farms out these Diamond-Mines for 600000 Crowns per annum, referring to himself the right of all the Diamonds, that exceed ten Carats in weight: There are Diamonds that mount to 35. and 40 Carats. And this is the great Treasure of that Prince.

2. That the most esteemed fruit in those parts, the Durion (of the bigness and shape of an ordinary Melon) has a very unpleasing and even intolerable smell, like to that of a rotten Apple.

3. That Rice prospers most in waterish grounds; and that the fields, where it grows best, resembles rather to Marshes, than to any ploughed: Soyle: Yea, that that Grain has the force, though 6. or 7. foot, water stands over it, to shoot its Stalk above it; and that the Stem, which bears it, rises and grows proportionally to the height of the water, that drowns the field.

4. That the way of keeping ones self harmless from a wild Elephant, when he runs directly upon one, is, to hold something to him; as a Hat, a Coat, a piece of Linnen, which he seizes on with his Trunk, and playes with it, as if he were pleas'd with this apparent homage, done to him; and he passes on. If he be in a rage, that then the only remedy is, to turn incessantly behind him to the left side, in regard that naturally (saith this Author) he never turns himself that way, but to the right. And the time, there is to turn, because of the Beasts unwieldiness, affords leisure enough to climb up some high Tree, or to mount some steep ground: all which if it fail, by holding always his tail, and turning with him, the Animal will be tired, and give opportunity to escape.

LONDON,
Printed for John Crook in Duck-Lane near Little-Britain. 1666.
The Contents.

An Addition to the Instances of Petrification, formerly enumerated. Articles of Inquiries concerning Mines; as, to the neighbouring Country about them; the Soyl where they are; the Signs of them; the Structure and other particulars belonging to the Mines themselves; the Nature and Circumstances of Ore; and the Reduction of Ore into Metal. Promiscuous Inquiries formerly recommended to Monsieur Heuelius, particularly about Cold; together with his own, and his Correspondents. Answer to some of them. The success of the Experiment of Transfusing the Blood of one Animal into another.

An Addition to the Instances of Petrification, enumerated in the last of these Papers.

This Instance was some while since communicated to the Royal Society by that Ingenious Gentleman Mr. Philip Packer, a worthy Member of that Body; in these words:

At a Bank in a Close of Mr. Purefoy, neer his house, call'd Wadley, a mile from Farrington in Berks, there grows an Elme, which hath now lost the top, and is grown hollow, containing neer a Tun of Timber. From the But of the same Tree, one of the spreading Claws having been formerly cut off with an Axe; that part of the But, from whence the same was sever'd, being about 1 1/2 foot above ground, and inward within the trunk of...
of the Tree, hath contracted a petrified Crust, about the thickness of a shilling, all over the woody part within the Bark; the Marks of the Axe also remaining very conspicuous, with this petrified crust upon it. By what means it should thus happen, cannot well be conceived, in regard there is no water near it; the part, above the ground and out of the weather; the Tree yet growing: unless being cut at some season, when the sap was flowing, the owsing of the sap might become petrified by the Air, and the Tree grow rotten and hollow inward since that time; which how long since, is not known.

A piece of that part cut, was presented, together with this Account, to the said Society, for their Repository.

**Articles of Inquiries touching Mines.**

What the Honourable Robert Boyle gave the Reader cause to hope for, in *Numb. 11*. when he was pleased to impart those General Heads for a Natural History of a Country, there publish'd; he is not unmindful to perform, by enlarging them as occasion serves, with Particular and Subordinate Inquiries. Here he gratifies the Curious with a considerable Set of Inquiries about Mines: which though unfinished, yet the Publisher, was instant to obtain their present Publication, to the end, that he might the more conveniently recommend them to several Forreigners of his Acquaintance, now ready to return to their several Countryes, which he understands to abound in Mines; and from the Curious Inhabitants whereof, he expects to receive a good Accompt upon some at least of these Inquiries; which also by several of them have been earnestly desired, as Instructions, to direct them, what Particulars to inquire after upon this Subject.

These Queries are reduced by the Author to six Heads:

The first, The neighbouring Country about the Mines.

The second, The Soyl where the Mines are.

The third, The Signs of Mines.

The fourth, The Structure and other particulars belonging to the Mines themselves.


The sixth, the Reduction of the Ore into Metal.
1. Whether the Country be Mountainous, Plain, or disting-

guish'd with Vales? And in case it be mountainous, what kind

of Hills they are; whether high, or low, or indifferently

elevated? Whether almost equal or very un-equal in height?

Whether fruitful or barren; cold or temperate; rocky or not;
hollow or solid? Whether they run in ridges, or seem confu-

sedly placed; and, if the former, what way the ridges run,
North and South, &c. And whether they run any thing parallel
to one another?

2. Whether the Country be barren or fruitful? And, if any

way fruitful, what it produces, and what it most abounds

with?

3. What Cattle it nourishe, and whether they have any such

thing peculiar in point of bigness, colour, shape, longævity,

fitness or unfitness to make good meat, &c. as may be rather ad-
scribed to the peculiar nature of the place, than to the barren-
ness of the Soyl, or other manifest causes?

4. Whether the Natives, and other Inhabitants, live longer

or shorter than ordinary? Whether they live more or less healthy?

Whether they be subject to any Epidemical Diseafes, that

may very probably be imputed to the Mines; and what these
Diseafes are; and what Remedies are found successful?

5. Whether the Country be, or be not furnish'd with Rivers,
Brooks, Springs, and other Waters; and how these waters are
conditioned?

6. Whether the Air be dry or moist; hot or cold; clear or

foggy; thick or thin; heavy or light; and especially, whether
the Weather be more or less variable than ordinarily; or
whether it be subject to great and sudden changes, that may
probably be imputed to the Mineral and Subterraneous Steams;
and what they are?
About the second Title.

7. Whether the Soyle that is neer the Surface of the Earth, be Stony; and, if it be, what kind of Stones it abounds with? Whether it be Clayie, Marley, Chalkye, &c. And, if it be of several kinds, how many they are; and by what properties they are distinguish'd?

About the third Title.

8. By what signs they know or guess, that there is a Mine in such a place?

9. These Signs are either upon the Surface of the Earth, or beneath it.

To the former belong these quaries.

10. Whether the Ground be made barren by Metalline or Mineral Effluviums?

11. Whether it be observed, that Trees and other greater Plants seem to have their tops burnt, or other leaves or outside discoloured? or whether there be any Plants, that do affect to grow over such Mines; and whether it have been tryed, that other Plants, that would prosper in the adjacent places, will not be made to grow or thrive there?

12. Whether the Stones and Pebles, that are wash'd by the Brooks, Springs, or other Waters, have any colour'd substance left upon them; and if they have, of what colour, weight, &c. these adherences are?

13. Whether the Waters of the place proposed, do by their tast, smell, ponderousness, &c. disclose themselves to contain Minerals? And, if they do, what Minerals they or their residences, when they are evapourated away, do appear to abound with, or to participate of?

14. Whether Snow will not lye, or Frost continue so long, or Dew be generated or stay upon the ground in the place proposed, as on other neighbouring grounds?

15. Whether the Dew that falls on that ground, will discolour white Linnen or Woollen-Cloths, spread over night on the surface
surface of the ground, and employed to collect the Dew? And whether the Rain that falls there, and may be supposed to come thither from elsewhere, will discolour such Clothes, or afford any residence of a Mineral Nature?

16. Whether the Place be more than ordinarily subject to Thunder and Lightning, and to sudden Storms or Earthquakes; as likewise to Nocturnal Lights and fiery Meteors.

17. Whether Mists use to rise from Grounds stored with Minerals? What is observable in them, and what Minerals they signify, and may be supposed to be produced by?

18. Whether the Virgula Divinatoria be used to find out the Veins of proposed Mines; and, if it be, with what success?

19. What other Signs above ground afford probability of Mines, or Direction for following a Vein over Hills, Valleys, Lakes, Rivers, &c.

The second sort of Signs belonging to these Queries, are such as follow.

20. Whether there be any Clayes, Marles, or other Mineral Earths, yellow or liquid matters, that usually give notice of the Ore? And if there be more than one, how and at what depths they are wont to lye respectively? Of what thickness and consistence they are; and in what Order the Diggers meet with them?

21. Whether there be any Stones or Marchesites to be found neer, or not very far from the surface of the ground, by which one may have ground to expect a Mine? As is often observed in the Tin-Mines of Cornwall, over which such kind of Stones are divers times found lying above ground?

22. Whether all Stones of that kind do equally signify that Mine? And, if not, how the significant Stones are to be known, as by Colour, Bigness, Shape, Weight, Depth under ground, &c.

23. Whether there be any Earths of peculiar kinds, as to Colour, Consistence, &c. that indicate a Mine beneath or near them; and, if there be, what they are, and what is their conception, if they have any?

24. Whether Heat or Damps give any assurance or a probability of finding a Mine?

25. Whether
25. Whether Water of any kind, met with in Digging, especially at this or that depth, do betoken a Mine?
26. Whether there be any Signs of the neerness of the Mine, and what they are?
27. Whether there be any Signs of ones having miss'd the Mine, either by being past above, or beneath, or having left it on either hand; and what they are?
28. Whether there be any Signs not only of the distinct and determinate kind of Metals or Minerals, but of the Plenty and Goodness of the Vein; and what they are?
29. Whether there be any Signs of the depth of the Vein beneath the surface of the Earth; and what they are?
30. Whether there be any proper or peculiar Signs, that show it to be hopeless, or at least unlikely, to find a Vein in the place where it is digg'd for; and what those are?

**About the fourth Title.**

31. What is the depth of the Shaft or Groove (which though named in the singular Number, the Questions about it are generally applicable) till you come at the Vein or Ore?
32. Whether the Vein run or lye Horizontal, or dippe? And if it dippe, what inclination it hath, how deep the lowest part lies; and consequently how much deeper than the uppermost? As also, what it's Flexures, if it have any, are? And whether it runs directly North or South, East or West; or seem rather to have a Casual tendency, than any determinate one by Nature? and how far it reaches in all?
33. What is the Wideness of the Groove at the Top, and elsewhere? Whether the Groove be perpendicular or crooked; and if crooked, after what manner, and with what distance it winds?
34. How the Groove is supported? What are the kinds, length, bigness, and way of placing the Timber, Poles, &c. that are employed to support it? And how long the Wood will last, without being spoyled with the subterraneous fumes and waters? and what wood lasts longest?
What Air-shaft belongs to the Mine? Whether it be single, or more than One? Of what breadth the Air-shaft is at the Orifice? Whether it be convenient enough, or not? How near it is placed to the Groove; and in what position? And if there be several Air-shafts, what their Distances and Situation are in reference to the Groove, and to each other? Or how Air is supplied, if there be no Air-shafts?

Whether they meet with any Waters in the Mine? And, if they do, how copious they are; at what depths they occur; how they are qualified; and what way they Spring, &c.

Whether they are constant or temporary? whether they increase or diminish notably in Summer or Winter, or at any other time of the year; and if they do, at what season that is; how long it is wont to last; and the proportions of Increase and Decrease?

What Expedients and Engines are employed to free the Mines from Water? The materials, the parts, the bigness, the shapes, the coaptation; and, in short, the whole structure, number, and way of applying the Instruments, that are made use of, to free the Mines from Water?

What are the Conditions, Number, &c. of the Adits?

Whether the Mine be troubled with Damps, and of what kind they are? whether they come often or seldom at any set time, or altogether irregularly? what Signs fore-run them? what mischief they do? what remedies are the most successfully employed against them, as well in reference to the Clearing of the Mine, as to the Preservation and Recovery of the Workmen?

What Methods the Mine-men use in following the Vein, and tracing their passages under ground (which they call Plumbering and Djalling) according to the several exigencies? And whether they employ the Instruments, made with the help of the Load-stone, the same way that is usual; and if not, wherein they differ in the use of the same Instruments; or what Instruments they substitute in their place?

What ways they take to secure themselves from the uncertainty, incident to the guidance of Magnetick Needles from the Iron-Stone or Ore, that they may meet with under ground?
(of which yet perhaps there is not so great danger, as one may imagine; as far as I could find by a Trial, I purposely made in a Groove, where I was sure, there wanted not Iron-Ore.) And what other ways may be used to direct Miners without the help of a Load-stone?

43. How the Miners deal with the Rocks and Sparrs, they often meet with, before they come at the Ore? Whether they use Fire to soften, calcine, or crack them? How they employ it, and with what measure of success?

44. What ways and cautions they use, to free the Mine and secure the Work-men from the inconveniences and danger accruing from the use of much fire in it?

45. What Instruments they use to break the Rock &c? And how those Instruments are conducive; and how long they last?

46. How the Mine-men work; whether naked or cloathed? And what Lights they use to work by; what materials they are made of, what measure of light they give; how long they last; and by what ways they are kept burning in that thick and foggy air?

47. How Veins are follow'd, lost, and recover'd? And how several Miners work on the same Vein? And what is the best way of getting all the Ore in a Vein, and most conveniently?

48. How they convey out their Ore, and other things, that are to be carried out of the Mine? Whether they do it in Baskets drawn up by Ropes, or upon Mens backs; and if this last-named way; what kind of Vessels they use for matter, shape, and capacity? And whether the Work-men deliver them one to another; or the same Work-men carry them all the way? And whether the Diggers descend and ascend by Ladders of Wood, or of Ropes, &c.

About the Fifth Title.

49. Whether the Ore runs in a Vein; or lie dispers'd in scatter'd pieces; or be divided partly into a Vein, and partly into loose masses; or like a Wall between two Rocks, as it were in a Cleft; or be interspers'd in the firm Rock, like speckled Marble? Or be found in Grains like Sand or Gravel; as store
of excellent Tin is said to be found in some parts of Cornwall at the Sides and in the Channels of running Waters, which they call . . . . . . ; or whether the Ore be of a softer consistence, like Earth or Lome, as there is Lead-ore in Ireland holding store of Silver, and Iron-ore in the North parts of Scotland and elsewhere? And what is observably in it as to Weight, Colour, Mixture, &c?

50. Whether any part of the Metal be found in the Mine perfect and complete? (As I have had presented me good valuable Copper, and pieces of perfect Lead, that were taken up, the one at Jamaica, and the other by an acquaintance of mine, that took them out of the ground himself in New England.)

51. Whether the Mine affords any parcels of Metal, that seem to grow like Plants (as I have sometimes seen Silver growing, as it seemed, out of Stone, or Sparre almost like blades of Gras; as also great Grains of a Metal, which appear'd to me, and which those that tried some of it, affirmed to be Gold, abounding in a stony lump, that seem'd to consist chiefly of a peculiar kind of Sparre.)

52. Whether the Vein lie near, or much beneath the surface of the Earth, and at what depth?

53. Whether the Vein have or have not any particular Concomitants, or Coats (if I may so call them;) and, if any, what they are, and in what order they lie? (As the Veins of Lead-ore, with us, have frequently annexed to them a Substance call'd Sparre, and next to that another, call'd Caulk.

54. Whether (besides these Coats) the Vein have belonging to it any other Heterogeneous substance? (As in Tin-mines we often find that yellow substance, which they call Mundick.)

55. What are the principal Qualities of these Extraneous substances? (As that Sparre is white, but transparent, almost like coarse Crystal, heavy, brittle, easily divisible into flakes, &c. Caulk is of a different texture, white, opacous, and like a Stone, but much more ponderous. Mundick, I have had of a fine golden colour; but, though it be affirm'd to hold no Metal; yet I found it in weight, and otherwise, to differ from Marchasites; and the Mine-men think it of a poisonous nature.)
56. Whether the Vein be inclosed every way in its Coats; or whether it only lye between them?

57. Whether the Vein be every way of an uniform breadth, and thickness; and, if it be, what these Dimensions are; and, if not, in what places it varies; and in what measures? (The like Questions are to be made concerning the Sparre, Caulk, and other Teguments or mixtures of the Ore?)

58. Whether the Vein be un-interrupted, or in some places broken off; and whether it be abruptly, or not; and whether it be by Vales, Brooks, Gulletts, &c.

59. How wide the Interruptions are? what Signs, whereby to find the Vein again? whether the ulterior part or division of the Vein be of the same Nature, and hold on in the same Course, as to its tendency upwards or downwards, or Horizontally, Norward, Southward, &c. with the Vein, from which it is cut off?

60. Whether, in case the last end of the Vein be found, it terminate abruptly, or else end in some peculiar kind of Rock or Earth, which does, as it were, close or Seal it up, without leaving any crack or cranny, or otherwise? And whether the terminating part of the Vein tend upwards, downwards, or neither? And whether in the places where the Vein is interrupted, there be any peculiar Stone or Earth, that does, as it were, seal up the Extremity of it?

61. Whether it be observed, that the Ore in Tract of time may be brought to afford any Silver or Gold, which it doth not afford, or more than it would afford, if it were not so ripe? And whether it have been found, that the Metalline part of the Vein grows so, that some part of the Mine will afford Ore or Metal in tract of time, that did not so before? And whether this Maturation of the Mine, the being exposed to the free Air be necessary; or, whether at least it conduce to the Acceleration of it; or otherwise?

62. Whether all the Ore, contained in the Mine, be of the self-same nature and goodness; and, if not, what are the differing kinds; and how to be discriminated and estimated?

63. What is the fineness and goodness of the Ore, by which the Mine is wont to be estimated? And what are the marks and characters,
characters, that distinguish one sort from another?

64. What proportion of Metal it affords? (As in our Iron-mines 'tis observed, that about three Tuns of Iron-stone will afford one Tun of Metal: And I have had Lead-Ore, which an Ingenious man, to whom I recommended such Tryals, affirm'd to me to afford three parts in four of good Lead.)

65. Whether the Ore be pure in its kind from other Metals, and, if not, of what Metals it participates; and in what proportion? Which is especially to be inquired into, in case the Mine be of a base metal, that holds a noble metal. (As I have known it observ'd, that Lead-Ore, that is poor in its own metal, affords more Silver, than other; and I remember, that the Ore lately mention'd, being rich in Lead, scarce afforded us upon the Cuppel, an Atome of Silver. And Matthæus informs us, that a little Gold is not unfrequently found in Iron-Ore. And I have by me some Gold, that never endur'd the Fire, taken out of a Lump of Tin-Ore.)

About the sixth Title.

66. What are the mechanick and prævious Operations, as Beating, Grinding, VVashing, &c. that are used to separate the Ore from the Heterogeneous Bodies, and prepare it for the Fire? Or whether the Ore requires no such preparation? (as it often happens in Lead, and sometimes in Iron, &c.)

67. Whether Mercury be made use off, to extract the nobler from the baser metals? (as is their practice in Peru, and other parts of the West-Indies.)

68. Whether the leaving the Ore expos'd to the open Air and Rain for a good while, be used as a Præparative? (as I have seen done in Iron-stone.)

69. Whether the Burning and Beating of the Ore be used to prepare it for the Furnace? (as is practis'd in Iron, and almost always in Copper:) And, in case they use it more than once, how often they do it; (for, Copper-Ore is in some places washed 8. or 10. times, and in others, 12. or 14.) and with what circumstances; as, how long the Ignition lasts at a time, whether the Ore be suffer'd to cool of it self, or be quench'd? whether it be washed betwixt each Ignition?

70. What Flux-powders, and other ways they have to try
and examine the goodness of the Ore in small quantities?

71. Whether, when they work in great, they use to melt the Ore with any Flux or Additaments, or only by the force of the Fire, or in any way between both? (As throwing in of Charcoals when they melt Iron-omite does not only serve to feed the Fire, but perhaps by the Alchemy of its Ashes, to promote the fusior: to Lime-omite, &c.)

72. What kind of Furnaces they use, to melt the Ore in? Whether they be all of one sort and bigness, or of differing?

73. What are, the Situation, Materials, Dimensions, Shape, Bigness, and in short what is the whole structure and Convenance of the Furnace? If there be any thing peculiar and remarkable? What Tools are used in Smelting, their Figures, use, &c. And the whole manner of working?

74. What kinds of Fewel, and what quantities of it, are wont to be employed in the Furnace, within the compass of a day, or week? How much is put in at a time? How often it is renewed? And how much Ore in a determinate time, as a week or a day, is wont to be reduced to Metal?

75. In case an Additament be employed, what that is, and in what proportion it is added? Whether it be mingled with the Ore, before that be put into the Fire, or cast in afterwards; and, if so, at what time, &c.?

76. Whether the Ore be melted by the Wind, excited by the Fire itself, as in Wind-ovens? Ore by the course of Waters? Or acuated by the blast of Bellows; and, if so, whether these Bellows be mov'd by a Wheel, turn'd by Water running under it, or falling on it? And what are, the Dimensions, Situation, &c. of the Bellows?

77. What contrivance they have, to let or take out the Metal, that is in fusion; and cast it into Bars, Sows, Pigs. &c.?

78. What Clay, Sand, or Mould they let it run or pour it through? And after what manner they refrigerate it?

79. Whether or no they do, either to facilitate the fusion, or to obtain the more or better Metal, mingle differing sorts or degrees of Ore of the same metal? (As in some places 'tis usual, to mingle poor and rich Ore; and at Mendip they mix two or more of those differing kinds of Lead-ore, that they call Prim-ore, Steel-ore, Potern-ore, &c.)

80. Whe-
82. Whether or no, having once brought the Ore to fusion, they melt all the Metal it self, to have it the more pure? And, if they do, with what circumstances they make the fusion?

83. Whether they have any Signs, whereby to know whether the Fusion have been well or ill perform’d; and the Metal have obtain’d the perfection, to be expected from such Ore, melted in such a Furnace?

84. Whether they observe any great difference in the goodness of the Metal, that first melts, from that of the rest of the Metal which comes afterwards in the same or another operation? And whether the Rule holds constantly? (For, though they observe in Tin-Mines, the best Metal comes first, yet in the works of an Industrious friend of mine, he informs me, that the best Metal comes last.)

85. Whether the produced Metal be all of the same goodness? And if it be, how good it is in reference to the Metal of other Mines, or other parts of the same Mine or Vein? And if it be not, what differences are observ’d between the produced portions of Metal; and what disparity that amounts to in the price?

86. What are the Ways of distinguishing them, and estimating their goodness?

87. Whether they do any thing to the Metal, after it is once brought to Fusion, and, if need be, melt it over again, to give it a melioration? (As when Iron is refined, and turn’d into Steel;) And what distinct Furnaces, and peculiar Ways of ordering the Metals are employ’d to effect this improvement? With a full description of them and the Tools in all Circumstances, observ’d in the refining of Metals.

88. Whether in those places, where the Metal is melted, there be not elevated some Corpuscles, that stick to the upper parts of the Furnance, or Building? And, if there be, whether they be barely fuliginous and recrementitious exhalations, or, at least in part, Metallin Flowers? (As in the Cornish Tin-mines, after some years they usually destroy the tharch’d Houses, where the Ore hath been melted, to get the stuff, that adhears to the insides of the Roofs, out of which they melt store of excellent Tin.)

89. Whether the Metal, being brought to fusion, affords any
any Recrements? (As iron-stone affords store of a dark Glass or Slagg) And, if it do, what those Recrements are? How they are separated from the Metal; and to what Uses they are employed?

90. Whether, after the Metal has been once melted, the remaining part of the Ore being exposed to the Air, will in tract of time be impregnated, or ripen'd, so as to afford more Metal? (For, this is affirm'd to me of the Cornish Tin-Ore; and what remained after the fusion of iron-ore in the Forest of Dean, is so rich in Metal, that a Tenant of mine in Ireland, though he had on the Land, he held from me, an Iron-Mine, found it less profit to work it, than to send cross the Sea to the Forest of Dean for this already us'd Ore, which having lain for some ages, since it was thrown aside in great heaps expos'd to the Air, he affirm'd to yield as well great great store of Iron, as very good: though I somewhat doubt, whether this be totally to be ascribed to the Aire, and length of time; or to the leaving of Metal in the Slaggs in old times, before great Furnaces were in use.)

Promiscuous Inquiries about Mines, from the same Author.

1. Whether the Territorie, that bears the Mine, abounds with no other Kind of Mineral in some distinct part of it? (As in Kent near Tunbridge, one part of the Country which is Hilly, abounds all along with iron-Mines; the other, which is also Hilly, and divided from it but by a small Valley, abounds exceedingly (as the Diggers and Inhabitants told me upon the place) in Quarry's, which the Metallin-Country wants, but is quite destitute of iron-stone. And so at Mendip, in one part of the Hill, I saw store of lead-Mines, containing several Kinds of Ore of that Metal; another part of the Hill I found to be full of Cole-pits, which had some Marchastes, but no Metal; and in another place, iron-ore, and mixt with Ores, which yet they did not think fit to work.)

2. Whether the Air appear to be really cold in Summer, and
and hot in Winter at the bottom of the Mines, by surer proofs than the Testimony of our Touch?

3. Whether they ever meet with places and Stones actually very hot, as Matthesius relates? And whether that spring not from the quenching of Marchasites?

4. Whether they find in the Mines any Mineral Gelly, such as the German Naturalists call Ghur? And whether in process of time it will harden into a metal, or Mineral Concretion?

5. What are the Laws, Constitutions, and Customs, Economical, Political, Ethical, that are receiv'd and practis'd among the Mine-men?

6. Whether the Diggers do ever really meet with any subterraneous Demons; and if they do, in what shape and manner they appear; what they portend; and what they do, &c.?

7. Whether they observe in the Trees and other Plants, growing over or neer the Mine, not only, (as hath been already intimated) that the Leaves are any whit gilded or silver'd by the ascending Mineral Exhalations, but also, that the Trees or other Plants are more solid and ponderous? And if they have not also some discernable Metalline or Mineral Concretes, to be met with in the small Cavities and Pores of their Substance?

8. Whether there be not Springs, and also greater Streams of Water neer the Mine, that rise, and run their whole course under ground, without ever appearing above it?

9. Whether the Subterraneous Springs do rise with any wind or determinate change of weather?

10. How much heavier the Atmosphere is at the bottom of the Mine, than at the top? And whether Damps considerably increase the weight of it?

11. Whether they find any strange Substances in the Mines, as Vessels, Anchors, Fishes inclos'd in Sparr or Metal, &c.?
Promiscuous Inquiries, chiefly about Cold, formerly sent and recommended to Monsieur Heuelius; together with his Answer return'd to some of them.

A considerable piece of the grand Design of the Modern Experimental Philosophers being, to procure and accumulate Materials for a good Natural History, whence to raise in progress of time a solid Structure of Philosophy; all possible Endeavours are used in England, to send abroad and recommend to as many of Foreign parts, as there is opportunity, Directions for searching into the Operations of Nature, and for observing what occurs therein, as well as in Mechanical operations and practices.

Several Heads of that kind have been already publish'd for this purpose in several of the former Tracts; to which, as we have added, in this, the Queries about Mines, so we shall subjoin those, that were not long since committed to the care of that Excellent Promoter of Astronomy and Philosophy, Monsieur Heuelius, Consul of Dantzick; who demonstrates so much zeal for the advancement of real knowledge, that he not only improves and promotes it by his own Studies, but labours also to incite others to do the like; having already warmed many of the Northern Climate, particularly Poland, Prusse, Livonia, Sweden and Denmark, into a disposition to be studious and active in inquiring after such particulars concerning Philosophy, as are recommended from hence, and render the very willing to employ themselves in things of that nature.

The Inquiries sent to Dantzick, are these:

1. What Signior Burattini (an Italian Gentleman, Master of the Mint to the King of Poland, and reputed a great Master in the Mechanicks) hath perform'd in Dioptricks? Whether at present he employs himself, as is related, in grinding a Telescope of 120 foot long? And, if so, what way he means to make use of?
of, commodiously to handle a Tube of that length?

2. Whether the same have the Art (as has been written from Paris) to make such Glass, as is not at all inferior to Venice-glass, and exceeds any plate of Glass, hitherto made there, twice or thrice in bigness?

3. What is the way of making Pot-ash in Poland?

4. What is to be observed about Succinum or Amber? whether it be an Exsudation of the Sea? whether it be seen to float upon the surface of the Sea? whether it be soft, when 'tis first cast on shore? At what season of the year, and in what manner 'tis taken up, &c?

5. What is to be observ'd in the Digging of Sal Gemma in Poland? what is the Depth of the Mines, stored with this Salt? what their distance from the Sea, &c?

6. What truth there is in that Relation concerning Swallows being found in Winter under waters congealed, and reviving, if they be fish'd and held to the fire?

7. Whether there be in the Bodnich Bay a Whirl-pool, as is related to be in the Sea of Norway, which is commonly call'd the Maal-froom? And whether there be any Signs, that speak the communication of those Gulphs by subterraneous passages; as the Jesuit Kircher affirms in his Mundus Subterraneus T. I. p. 146?

8. To what depth the Cold in those parts pierces the Earth and Water?

9. Whether their Watches go slower by the intense cold?

10. Whether their Oyls in hard frosts are turn'd into true, that is, hard and brittle, Ice?

11. Whether they can freeze there a strong Brine of Bay-Salt; and a strong Decotion of Sal Gemma, or Soot; or a strong Solution of Salt of Tartar, or of Sugar of Lead?

12. Whether they can congeal meer Blood, all the serous part thereof being fever'd? Item, Canary Wine; the Lixiviums of Soap-boylers, and such as are prepared of other Salts; as also, the Spirits extracted out of Salts, as Spirit of Vitriol, Nitre, &c?

13. Whether an intense and lasting Frost makes any alteration in Quick-silver, exposed very shallow in a flat Vessel.

14. Whether the Purgative virtue of Catharticks be increased or lessened, or even totally destroy'd by a strong and continued Cold?
15. Whether Harts-horn thaw'd, and such like substances, using the same method of Distilling, yield the same quantity of Liquor, which they use to yield, when not frozen?
16. What Cold operates in the Fermentation of Liquors?
17. Whether Birds and Wilde Beasts grow white there in Winter, and recover their native colour in Summer?
18. Whether Colours may be concentrated by a sharp cold? E. g. A strong Decoction of cocheneel in a fit Glass?
19. Whether the Electrical virtue of Amber, and the Attractive and Directive force of the Magnet, be changed by a vehement Cold?
20. Whether pieces of Iron and Steel, even thick ones, be made brittle by intense frosts, and therefore Smiths are obliged for prevention, to give their Iron and Steel-tools a softer temper?
21. Whether accurate Observations evince, that all Fishes dye in frozen Waters, if the Ice be not broken? Where it is to be diligently inquired into, whether the Cold it self, or the want of changing or ventilating the water, or the privation of Air, be the cause of the death of Fishes?
22. Whether any Physicians or Anatomists have inquired, by freezing to death some Animals (as Rabits, Pullets, Dogs, Cats, &c.) after what manner it is, that Intense Cold kills men? whether they have found any Ice in the Inner parts; and if so, in which of them; Whether in the Ventricles of the Brain and Heart; and in the greater Vessels?

These were the Queries recommended about a Twelve-month ago. Monsieur Henelius in a late Letter of his, accompanied with several papers from others, returns this Accompt.

The Inquiries you proposed to me, I did impart to several of my Learned friends: But hitherto I have attained an Answer but to few particulars. Among the rest you'll find a Letter of the Learned Johannes Schefferus, Professor in the Swedish University at Upsall, wherein he discourses handsomely of several things, being ready to entertain a Literary Commerce with you about such matters. Touching Amber, I am almost of the same mind with him, that it is a kind of Fossil Pitch or Bitumen, seeing it is not only found on the Shore of the Bornflian Sea, but also digg'd up in subterraneous places, some German miles distant from the Sea.
...and that not only in Sandy, but also in other Hills of firmer Earth; of which I have seen myself pretty big pieces. Concerning Swallows, I have frequently heard Fisher-men affirm, that they have here often fifh*d them out of the Lakes, in the Winter; but I never have seen it myself. Whilst I am writing this, I receive Letters out of Denmark, advertising me, that those two Learned men, Thomas and Erasimus Bartholin, do intend shortly to answer the same Queries. Next Winter, if God vouchsafe me life and health, I purpose to make a Journey to Konigs-berg, where I hope to learn many things, especially about Amber.

Thus far in answer to those Inquiries for the present.

To this he subjoins other things, no less fit to be communicated to the Curious, in these words;

The Books you have sent me over sea, I have not yet received; I wish, they were all translated into Latin; for I have not English enough, to understand all particulars perfectly. For the rest, you have obliged me, by communicating the Observations of the last Eclipse of the Sun; as well those made in England, as those of Paris and Madrid. That I may require you in some measure, I send you my Observations both of that, and the Moons last Eclipse. In the Sun's Eclipse, this is chiefly observable; That the Semidiameter of the Moon from the very beginning, to about 5, or 6, digits of the increasing Phasis was much less than the Rudolphin Account imports. For it was then almost equal to the Semidiameter of the Sun: but, after the greatest Obscuration, when I again contemplated the Moons Semidiameter, I found it 8° or 9° bigger than that of the Sun; so that the Semidiameter of the Moon was not always, during this Eclipse, constant to itself. It will therefore be worth while, to be hereafter more diligent and curious in this particular, and accurately to observe in the Phasis of each Digit the Proportion of the Semidiameters of both Luminaries; to the end, that first it may be made manifest, whether in all the Eclipses of the Sun, or in some only, that variation happens; next, that the Causes of such a Phenomenon may be diligently inquired into. Of this Variation, the Excellent Ismael Bullialdus hath also observed something at Paris. For he has written to me, That in the same Eclipse the Semidiam of the Sun to the Semid. of the Moon was, as 16° 9". to 16° 22"; but that in another

\[ \frac{Z \times 2}{2} \]
Phasis of 6 digits, the Semidiameters appear'd equal. These my Observations, if you think them worthy, you may communicate to other Mathematicians. The last year 1665. July 27. (ft. n.) the Tables did also indicate an Eclipse of the Moon: but though the Sky here was very clear, yet the Moon was not at all obscured by the true shadow, but entred only a little into the Penumbra, wherein it continued 50'. The beginning of its touching the Penumbra did then almost happen, when Aquila was elevated 36° 18'; which is an Example worthy to be noted. I have many Observations of the Eclipses of former years by me, which I could not yet make publick, by reason of the multitude of my business, which do almost over-whelm me. The Eclipse of the Moon of this Year 1666. June 16. (ft. n.) was observed from a Hill near my Garden, to the end, that we might see both together the Sun's setting, and the Moon rising, But I was disappointed of my hopes: For very thick Exhalations, besieging the Horizon, where the Moon was to rise, unto 2° 30', hindred me from seeing the Moon rise, in the Article of the setting of the Sun. Wherefore the first Phasis of 1. dig. 45'. did not appear but in the Moons Altitude of 2° 30'; when the greatest Obscuration was already past. The End fell out hor. 9. 27', about 128° from the Zenith Westward.

I am very glad to understand, that you have so good Telescopes, as to make such considerable Observations in Jupiter and Mars, as you have lately done in England. I have no leisure now, by reason of the Observations of the Fixt Stars, which I now almost constantly am employ'd about, to do anything in the advancing of Telescopes. I am obliged to finish the Catalogue of the Fixt Stars; having mean while the contentment to find, that many excellent persons labour about the Improvement of Optick Glasses. If I could get a good one of those of 60. foot, you mention, at a reasonable rate, you would oblige me in sending me one; perhaps may I be so happy, as to make likewise some good discovery or other, by the help thereof. In the mean time, let me know, I pray, the Dimensions of those Glasses, and how they are to be managed. The ingenious Burattini has not yet finish'd his Telescope; as soon as
as he hath, I shall acquaint you with it. Before I conclude, I must give notice to the Lovers of Astronomy, that on the 24. of September (ft. n) of this year, I have observ'd that New Star in Pètære Cygni (which from the year 1662, until this time hath been almost altogether hid) not only with my naked Eye, like a Star of the sixth or seventh Magnitude, but also with a very great Sextant. It is still in the very same place of the Heavens, where it was formerly from A. 1601. to almost 1662. For, its Distance from Scheat Pegasi hath been by me found 35\(^\circ\) 51\(^\prime\) 20\(^\prime\) and from Marcab, 43\(^\circ\) 10\(^\prime\) 50\(^\prime\). which Distances (as I have found in my Journal) are altogether equal to those, which I observ'd A. 1658. the 1. of November. For the Distance from Scheat at that time was 35\(^\circ\) 51\(^\prime\) 20\(^\prime\). and from Marcab, 43\(^\circ\) 10\(^\prime\) 25\(^\prime\): where that former from Scheat exactly answers to the recent; and that from Marcab, 'tis true, differs in a very few Seconds, but that disparity is of no moment, since it only proceeded from thence, that this New Star is not yet so distinctly to be seen, as at that time, when it was of the third Magnitude. It is therefore certain, that it is the same Star, which Kepler did first see A. 1601. and continued until A. 1662. But whether in time it will grow bigger and bigger, or be lost again, time will shew. He that will observe this Star, must take care, left he mistake those three more Southern ones, of the Sixth Magnitude, and now in a manner somewhat brighter (though not extant on the Globe) than the New Star in Collo Cygni. The highest of those three, is distant from Scheat Pegasi 36\(^\circ\) 25\(^\prime\) 45\(^\prime\). the middlemost from the same, 37\(^\circ\) 25\(^\prime\) 20\(^\prime\). and the lowest, 38\(^\circ\) 4\(^\prime\) 30\(^\prime\). Farewell, and assure the Most Illustrious Royal Society of my humblest Services.

So far Monsieur Hovellius, whose accurate Calcul. of the Solar Eclipses Duration, Quantity, &c. is intended to be fully represented the next Month, since it could not be conveniently done this time. The annexed Papers follow.

One is from Monsieur Job. Schefferm, to this purpose.

1. That he is confident, the Royal Society of England will do much good for the advancement of useful Knowledge.

2.
2. That he conceives Amber to be a kind of Fossil Pitch, whose Veins lie at the bottom of the Sea; believing that it is hardened in tract of time, and by the motion of the Sea cast on shore: He adds, that hitherto it hath been believed, not to be found but in Bornfia; but he assures, that it is also found in Sweden, on the shores of the Ifle Biorkob, in the Lake Melero, whose water is sweet. Of this, he faith, he hath a fine piece by him, two inches large and thick, presented him by one, that himself with his own hands had gathered it and several other pieces, on the shore of the said Island; affirming withall from the mouth of a Shepherd of that place, that it is thrown out by a strong Wind, bearing upon the shore.

3. That it is most certain, that Swallows sink themselves towards Autumn into Lakes, no otherwise than Frogs, and that many have assured him of it, who had seen them drawn out with a Net together with Fishes, and put to the fire, and thereby revived.

4. That 'tis also very true, that many Animals there grow white in Winter, and recover their own Colour in Summer. That himself hath seen and had Hares, which about the beginning of Winter and Spring were half white, and half of their native colour: that in the midst of winter he never saw any but all white. That Foxes also are white in Winter; and Squirrels grayish, mixt of dark and white colour.

5. That 'tis known there generally, that Fishes are killed, by reason of the Ice not being broken: but first, in ponds only or narrow Lakes; next, in such Lakes only, where the Ice is pretty thick; for, where 'tis thin, they dye not so easily. Lastly, that those Fishes that lie in limy or clayie ground, dye not so soon as others. But, he adds, that even in great Lakes, when 'tis a very bitter Frost, Ice is wont to be broken, either by the force of the Waves, or of the Imprisoned Vapors, raised by the agitation of the Water, and then bursting out with an impetuosity; witness the noise made by the rupture of the Ice through the whole length of such Lakes, which he affirms to be not less terrible than if many guns went off together. Whereby it falls out, that Fishes are seldom found dead in great Lakes.

6. That neither Oyle, nor a strong Brine of Bay-Salt, is truly con-
congeal'd into Ice, in those parts, Viz. at Upsall in Sweden.

7. That the Frost pierces into the Earth, two Cubits or Swedish Ells; and what moisture is found in it, is white, like Ice: That Waters, if standing, freeze to a greater depth, even to three such Ells or more; but those that have a Current, less: That rapid Rivers freeze not at all; nor ever-bubbling Springs; and that these latter seem even to be warmer in Winter, than Summer.

So far this Observer, who likewise offers his Services in giving an answer to the remaining Queries, and in entertaining a commerce in such other Philosophical matters, as he is conversant in.

Another Paper written by Monsieur Fehre, chief Secretary to Prince Ratzlif, contains these particulars;

1. That the College of the Learned in Russia finds it not so easy to resolve all those Queries sent from England to M. Hene-neus: but yet that they will try what may be done upon it.

2. That as for himself, he can assure from his own Experience concerning the Effects of Cold; First, That in the War against the Muscovites and Cossacks, A. 1655, in January, in White Russia, at the Siege of Biebow, 30 Leagues from Smolensko, and three from Malihlo, near the River Borishtenes, when they had Quarter in a Village call'd Iskan, they were seized on with such a Frost, that all their Provisions of Spanish Wines or Petersimen, and Beer, were in one Night frozen upon the Sleds, notwithstanding they were cover'd with Straw; in so much, that when next morning they would have drawn of those Liquors, they found all dry, and were constrain'd to carry them into a Stove, to thaw them; which they could not do in two whole days; and were obliged to break the Vessels, and put pieces of the Icy Wine into Kettles to thaw them over the Fire, for Drink: That they asked not for a Draught, but a Morsel of Wine or Beer: That their Horses had no better cheer than themselves, as to matter of Drink; the Pool of the Village being so thoroughly frozen, that there was but very little Water left between the Ice and the bottom of the Pool; whereby the poor Beasts were forced to drink with great reverence, kneeling on their forefeet to thrust their heads into the holes, made for them in the Ice, and to suck thence some drops of Water; and that, if they had not had Snow to eat, there would have dyed a far greater number
number of them, than there did. Moreover, that he observed, that the Hungarian Wine, of which they had a Tun, resisted the Cold better, than the Peter Simon; for it was not so much frozen; unless it be, that the Butler had more care of that, than the rest, by transporting it sooner into the Stove, when he found the excess of Cold. Again, that one presenting him in the March with some Aqua-vite, the Scrue of the Flagon, put to his Mouth, stuck so close to his Lips, that he could not draw it off, without drawing blood.

In a third Paper, I find these particulars from the same M. Fevre.

1. That a considerable person, one Dr. Becker, a great Lover of Curious Inquiries, has given him hopes to entertain this Philosophical Commerce.

2. That he hath seen men dye in Poland and Lithuania both of Heat and Cold. And first, that A. 1653. in July, being with this present King of Poland in March from Leopoli to the Camp of Glignani, it was so furiously hot that day of their March, that it caused such an alteration in that Regiment of Foot, which was the Kings Guard, marching most of them bare-foot upon Sands, that more than an hundred of them fell down altogether disabled, whereof a dozen dyed out-right, without any other Sickness. Secondly, as to the Cold, that the frost was so bitter, that

3. Soldiers dyed of it, A. 1665. the 2. of January, in passing a long Ditch: besides, that divers persons lost some of their Lims.

The Success of the Experiment of Transfusing the Blood of one Animal into another.

His Experiment, hitherto look'd upon to be of an almost unsurmountable difficulty, hath been of late very successfully perform'd not only at Oxford, by the directions of that expert Anatomist Dr. Lower, but also in London, by order of the R. Society, at their publick meeting in Gresham Colledge: the Description of the particulars whereof, and the Method of Operation, is referred to the next Opportunity.

Errata to be corrected in Number 18.


London, Printed for John Crook near the Blew-Anchor in Duck-lane; and Mose Pits at the White-Hart in Little-Britain.
The Contents.

The Method observed in Transfusing the Blood out of one live Animal into another: And how this Experiment is like to be improved. Some Considerations concerning the same. An Accompt of some Sanative Waters in Herefordshire. A farther Accompt of the Vitriolate Water mention'd Numb. 18. together with some other particulars touching Waters. Inquiries for Turkey. An Observation about Optick Glasses made of Rock-Crystal, communicated from Italy. A Relation of the Use of the Grain of Kermes for Coloration, from France. An Accompt of some Books lately publish'd, vid. 1. PINAX Rerum Naturalium BRITANNICARUM, continens VEGETABILIA, ANIMALIA & Fossilia ANGLIÆ, inchoatus; Anth. Christophoro Merret, M. D. 2. PLACITA PHILOSOPHICA Guarini. 3. GUSTUS ORGANUM per Laurentium Bellini deprehensionem.

The Method observed in Transfusing the Blood out of one Animal into another.

This Method was promis'd in the last of these Papers. It was first practis'd by Doctor Lower in Oxford, and by him communicated to the Honourable Robert Boyle, who imparted it to the Royal Society, as follows.

First, Take up the Carotidal Artery of the Dog or other Animal, whose Blood is to be transfus'd into another of the same...
same or a different kind, and separate it from the Nerve of the
Eight pair, and lay it bare above an inch. Then make a strong
Ligature on the upper part of the Artery, not to be untied a-
again: but an inch below, videl, towards the Heart, make an-
other Ligature of a running knot, which may be loosen’d or fast-
ned as there shall be occasion. Having made these two knots,
 draw two threads under the Artery between the two Ligatures; and then open the Artery, and put in a Quill, and tie the Artery
upon the Quill very fast by those two threads, and stop the Quill
with a stick. After this, make bare the Jugular Vein in the o-
 ther Dog about an inch and a half long; and at each end make a
Ligature with a running knot, and in the space betwixt the two
running knots drawn under the Vein two threads, as in the o-
 ther: then make an Incision in the Vein, and put into it two
Quills, one into the descended part of the Vein, to receive the
bloud from the other Dog, and carry it to the Heart; and the
other Quill put into the other part of the Jugular Vein, which
comes from the Head (out of which, the second Dogs own
bloud must run into Dishes.) These two Quills being put in and
tyed fast, stop them with a stick, till there be occasion to open
them.

All things being thus prepar’d, tie the Dogs on their sides to-
wards one another so conveniently, that the Quill may go into
each other, (for the Dogs necks cannot be brought so near, but
that you must put two or three several Quills more into the first
two, to convey the bloud from one to another.) After that,
unstop the Quill that goes down into the first Dog’s Jugular
Vein, and the other Quill coming out of the other Dog’s Arte-
ry; and by the help of two or three other Quills, put into each
other, according as there shall be occasion, insert them into one
another. Then flip the running knots, and immediately the
bloud runs through the Quills, as through an Artery, very im-
petuously. And immediately, as the bloud runs into the Dog,
unstop the other Quill, coming out of the upper part of his Jug-
ular Vein (a Ligature being first made about his Neck, or else his
other Jugular Vein being compress’d by ones Finger;) and let
his own bloud run out at the same time into Dishes (yet not
costantly, but according as you perceive him able to bear it)
till the other Dog begin to cry, and faint, and fall into convulsions, and at last dye by his side.

Then take out both the Quills out of the Dogs Jugular Vein, and tye the running knot fast, and cut the Vein asunder, (which you may doe without any harm to the Dog, one Jugular Vein being sufficient to convey all the blood from the Head and upper parts, by reason of a large Anastomosis, whereby both the Jugular Veins meet about the Larynx.) This done, sow up the skin and dismiss him, and the Dog will leap from the Table and shake himself, and run away, as if nothing ailed him.

And this I have tryed several times, before several in the Universities, but never yet upon more than one Dog at a time, for want of leisure, and convenient supplies of several Dogs at once. But when I return, I doubt not but to give you a fuller account, not only by bleeding several Dogs into one, but several other creatures into one another, as you did propose to me, before you left Oxford; which will be very easy to perform; and will afford many pleasant and perhaps not unuseful Experiments.

But because there are many Circumstances necessary to be observ’d in the performing of this experiment, and that you may better direct any one to doe it, without any danger of killing the other Dog, that is to receive the others blood, I will mention two or three.

First, that you fasten the Dogs at such a convenient distance, that the Vein nor Artery be not stretched; for then, being contracted, they will not admit or convey so much blood.

Secondly, that you constantly observe the Pulse beyond the Quill in the Dogs Jugular Vein (which it acquires from the impulse of the Arterious blood:) For if that fails, then ’tis a sign the Quill is stop’d by some congealed blood, so that you must draw out the Arterial Quill from the other, and with a Probe open the passage again in both of them, that the blood may have its free course again. For, this must be expected, when the Dog, that bleeds into the other, hath loft much blood, his heart will beat very faintly, and then the impulse
of blood being weaker, it will be apt to congeal the sooner, so that at the latter end of the work you must draw out the Quill oftener, and clear the passage; if the Dog be faint-hearted, as many are, though some stout fierce Dogs will bleed freely and uninterruptedly, till they are convuls'd and dye. But to prevent this trouble, and make the experiment certain, you must bleed a great Dog into a little one, or a Mastiff into a Curr, as I once try'd, and the little Dog bled out at least double the quantity of his own blood, and left the Mastiff dead upon the Table, and after he was untied, he ran away and shak'd himself, as if he had been only thrown into water. Or else you may get three or four several Dogs prepared in the same manner; and when one begins to fail and leave off bleeding, administer another, and I am confident one Dog will receive all their blood, (and perhaps more) as long as it runs freely, till they are left almost dead by turns: provided that you let out the blood proportionably, as you let it goe into the Dog, that is to live.

Thirdly, I suppose the Dog that is to bleed out into dishes will endure it the better, if the Dogs that are to be administered to supply his blood, be of near an equal age, and fed alike the day before, that both their bloods may be of a near strength and temper.

There are many things I have observed upon bleeding Dogs to death, which I have seen since your departure from Oxford, whereof I shall give you a relation hereafter; in the meantime since you were pleased to mention it to the Royal Society, with a promise to give them an account of this experiment, I could not but take the first opportunity to clear you from that obligation, &c.

So far this Letter: the prescriptions whereof having been carefully observ'd by those who were employ'd to make the Experiment, have hitherto been attended with good success; and that not only upon Animals of the same Species (as two Dogs first, and then two Sheep) but also upon some of very differing Species (as a Sheep and a Dog; the former Emitting, the other Receiving.)

Note only, that in stead of a Quill, a small, crooked, thin Pipe.
Pipe of Silver or Brass, so slender that the one end may enter into a Quill, and having at the other end, that is to enter into the Vein and Artery, a small knob, for the better fastening them to it with a thread, will be much fitter than a strait Pipe or Quill; for this Operation: for so they are much more easy to be managed.

Tis intended, that these tryals shall be prosecuted to the utmost variety the subject will bear: As by exchanging the blood of Old and Young, Sick and Healthy, Hot and Cold, Fierce and Fearful, Lame and Wild Animals, &c. and that not only of the same, but also of differing kinds. For which end, and to improve this noble Experiment, either for knowledge, or use, or both, some Ingenious men have already proposed considerable tryals and inquiries, of which perhaps an account will be given hereafter. For the present we shall only subjoin some Considerations about this kind of Experiments.

1. It may be considered, that the blood of the Emitent Animal, may after a few minutes of time, by its circulation, mix and run out with that of the Receptor. Therefore to be attend'd in these Tryals, that all the blood of the Receptor is run out, and none left in him but the adventitious blood of the Emitent, two or three or more Animals (which was also hinted in the method above) may be prepared and administred, to bleed them all out into one.

2. It seems not irrational to guess a priori, that the exchange of blood will not alter the nature or disposition of the Animals, upon which it shall be practis'd; though it may be thought worth while for satisfaction and certainty, to determine that point by Experiments. The case of exchanging the blood of Animals seems not like that of Griffins, where the Cyns turns the Sap of the Stock, grafted upon into its nature; the Fibres of the Cyns so straining the juice, which passes from the stem to it, as thereby to change it into that of the Cyns, whereas in this transfusion there seems to be no such

Per-
Percolation of the blood of Animals, whereby that of the one should be changed into the nature of the other.

3. The most probable use of this Experiment may be conjectured to be, that one Animal may live with the blood of another, and consequently, that those Animals, that want blood, or have corrupt blood, may be supplied from others with a sufficient quantity, and of such as is good, provided the Transfusion be often repeated, by reason of the quick expence that is made of the blood.

Note.

In the last Transactions was also promised an Account by the next, of Monsieur Hevelius his accurate Calcul of the late Solar Eclips, Duration, Quantity, &c. But this being to be accompanied with a Scheme, the Graving whereof met with a disappointment, it must be still referred to another Opportunity.

An account of some Sanative Waters in Herefordshire.

This account was communicated by Dr. B. in these words.

There are two Springs in Herefordshire, whereof one is within a Bolt, or at least Bow-shot of the top of the near adjoining lofty Hill of Malvern, and at great distance from the Foot of the Hill, and hath had a long, and old fame for healing of eyes. When I was for some years molest'd with Teters on the back of one and sometimes of both my hands, notwithstanding all endeavours of my very friendly and skilful Physicians, I had speedy healing from a neighbouring Spring of far less fame. Yet this Spring healed very old and Ulcerous sores on the Legs of a poor Fellow, which had been poison'd by Irons in the Gaol, after other Chirurgery had been hopeless. And by many trials upon my hands, and the Teters, I was perswaded, that in long droughts, and lasting dry Frosts, those waters were more effectually and more speedily healing, than at other times. And not to omit this circumstance, I did hold this water in my mouth till it was warm, and perchance somewhat intermingled with fasting Spittle, and
and so dropping it upon the Teter, I there could see it immediately gather a very thin skin upon the raw flesh, not unlike that which is seen to gather upon Milk over a gentle fire. This skin would have small holes in it, through which a moisture did issue in small drops, which being wiped away, and the water continued to be dropp'd warm out of the mouth, the holes would diminish, and at last be all quite healed up.

For the Eye-waters, I conceived them more strongly tereine, and clearing the Eyes; and they had a rough smartness, as if they carried Sand or Gravel into the Eye.

I have known and try'd three or four healing Fountains of late discovery, or of no old fame that I could hear of.

I did once put rich pearl for some days in a vessel of water, to try whether the water would acquire a healing vertue; but my Experiments were interrupted. I had in my thoughts many other ways of Tryal, which I may resume hereafter.

A farther Accomp of the Vitriolate-water, mention'd Num. 18 p. 323. Together with some other Particulars touching Waters.

This comes from the same hand as follows:

I formerly mention'd to you, that, if that Pool of Mr. Philip's, which seems to be of Vitriolate-water, were on my ground, I would drain it, and search the head of the Spring, pursuing the source, till I could well discern, through what lay of Earth or Gravel it does pass. Now I shall tell you, that I have taken order for the further tryal of the said Water, by boiling a greater quantity in a Furnace &c. But just as we were in readiness for the tryal, a stream of Rain-water fell into the Pool, and so discourag'd us for the present. I have also taken a course to turn the falling Waters aside, and to drain the Pool, that we may see, what the Native Springs (whether one or more) may be. Of which more hereafter.

I wish (so he goes on) we had a full Accompt of our Salt- Springs at Droit-wych near Worcester, and at Nant-wych in Cheshire (what other Salt-Springs we have in England, I know not;)

It
It should be inquired, at what distance they are from the Seas, or from Salt-fluxes, from Hills, and how deep in the Vales? What the weight? Whether in droughts or long Frosts the proportion of Salt or weight increaseth? Whether the Earth near the Springs, or in their passage hath any peculiar ferment, or produceth a blackishness, if it rests, after it is well drained.

**Inquiries for Turky.**

Though many Relations and Descriptions of Turky be extant in Print, yet they leave in many a desire of fuller information in the following particulars, lately drawn up, for the most part by Mr. H. and recommended to an Ingenious Gentleman, bound for that Country; and desired also to be taken notice of by others, that may have occasion to visit the same.

1. In what part of Turky the *Rajma* is used in Turky to take away hair, and in what quantity? Whether the Turks employ it to any other Uses, besides that of the taking away of Hair? Whether there be differing kinds of it? How it is used to take off hair, and how to get store of it.

2. Whether the Turks do not only take Opium themselves for strength and courage, but also give it to their Horses, Camels and Dromedaries, for the same purpose; when they find them tired and faint in their travelling? What is the greatest Dose, any men are known to have taken of Opium? and how prepared?

3. What effects are observed from their use, not only of Opium (already mention'd) but also of Coffee, Bathing, having their Heads, using Rice; and why they prefer that which grows not unless water’d, before Wheat, &c?

4. How their Damasco-steel is made and temper’d?

5. What is their way of dressing and making Leather, which though thin and supple, will hold out water?

6. What method they observe in breeding those excellent Horses, they are so much famed for?

7. Whether they be so skilful in Poylsoning, as is said; and how their Poylsoms are curable?

8. How
8. How the Armenians keep Meat fresh and sweet so long, as 'tis said they do?

9. What Arts or Trades they have worth Learning?

10. Whether there be such a Tree about Damascum, call'd Monilac, which every year about the Month of December is cut down close by the root, and within four or five Months time shoots up again apace, bringing forth Leaves, Flowers, and Fruit also, and bearing but one Apple (an excellent Fruit) at once?

11. Whether about Reame in the Southern part of Arabia Felix, there be Grapes without any grains? And whether the people in that Country live, many of them, to a hundred and twenty years, in good health?

12. Whether in Canedia there be no poisonous Creatures; and whether those Serpents, that are there, are without poison?

13. Whether all Fruits, Herbs, Earth, Fountains, are naturally saltish in the Isle of Cyprus? And whether those parts of this Isle, which abound in Cyprus-trees, are more or less healthful, than others?

14. What store of Amiantibus there is in Cyprus, and how they work it?

15. Whether Mummies be found in the lands of Arabia, that are the dried flesh of men buried in those sandy Deserts intravelling? And how they differ in their vertue from the Embalmed ones?

16. Whether the parts about the City of Constantinople or Asia Minor, be as subject to Earth-quakes now, as they have been formerly? And whether the Eastern winds do not Plague the said City with Mist, and cause that inconstancy of Weather, it is said to be subject to?

17. Whether the Earth-quakes in Zant and Cephalonia be so frequent, as now and then to happen nine or ten times a Month? And whether these Isles be not very Cavernous?

18. What is the height of Mount Caucasus, its position, temper in its several parts, &c.?

19. With what declivity the Water runs out of the Euxine-Sea into the Propontis? With what depth? And if the many Tides and Eddies, so famous by the name of the Euripi, have any certain Period?

20. If
20. If in the Euxin-Sea there can be found any sign of the Caspian-Sea emptying it self into it by a passage under ground? If there be any different Colour, or Temper as to Heat or Cold; or any great Current or Motion in the Water, that may give light to it?

21. By what Inland-passage they go to China; there being now a passage for Caravans throughout those places, that would formerly admit of no Correspondence by reason of the Barbarism of the Inhabitants?

22. Whether in the Aqueducts, they make, they line the inside with as good Plaister, as the Ancients did? and how theirs is made?

23. To inquire after these excellent Works of Antiquity, of which that Country is full, and which by the ignorant are not thought worth notice or preservation? And particularly, what is the bigness and structure of the Aqueducts, made in several places about Constantinople by Solyman the Magnificent? &c.

An Observation of Optick Glasses, made of Rock-Crystal.

This is contained in a Letter, of Eustachio Divini, Printed in Italian at Rome, as the 39. Journal des Scavans extracts it: void.

Though it be commonly believed, that Rock-Crystal is not fit for Optick-Glasses, because there are many Veins in it; yet Eustachio Divini made one of it, which he faith proved an excellent one, though full of Veins.*

*It may be queried whether those were true Veins, or only Superficial Strictures, and slight Fur-ches.

An account of the Use of the Grain of Kermes for Coloration.

This was communicated by the Ingenious Dr. Croen, as he received it from one, Monsieur Vergy, a French Apothecary at Montpelier; who having described the Grain of Kermes, to be an excrescence, growing upon the Wood, and often upon the leaves.
leaves of a Shrub, plentiful in Languedock, and gather'd in the end of May, and the beginning of June, full of a red Juyce; subjoynes two Uses, which that Grain hath, the one for Medicine, the other for Dying of Wool. Waving the first, notice shall only be taken here of the latter, vid. That, for Dying, they take the Grain of Kermes, when ripe, and spread it upon Linnen: And at first, whilst it abounds most in moisture, 'tis turn'd twice or thrice a day, to prevent its Heating. And when there appears red powder amongst it, they separate it, passing it through a Searce; and then again spread abroad the Grain upon Linnen, untill there be perceived the same redness of the powder; and at the end, this red powder appears about and on the surface of the Grain, which is still to be pass'd through a Searce, till it render no more.

And in the beginning, when the small red Grains are seen to move (as they will do) they are sprinkled over with strong Vinegar, and rubb'd between on's hands: afterwards, little Balls are form'd thereof, which are expos'd to the Sun to dry.

If this red powder should be left alone, without pouring Vinegar or some other accid liquor upon it, out of every Grain thereof would be form'd a little Fly, which would skip and fly up and down for a day or two, and at last changing its colour, fall down quite dead, deprived of all the bitterness, the Grains, whence they are generated, had before.

The Grain being altogether empty'd of its pulp or red powder, 'tis wash'd in Wine, and then expos'd to the Sun. Being well dryed, 'tis rubb'd in a Sack to render it bright; and then 'tis put up in small Sacks, putting in the midst, according to the quantity, the Grain has afforded, 10. or 12. pounds (for a Quintal) of the dust, which is the red powder, that came out of it. And accordingly, as the Grain affords more or less of the said powder, Dyers buy more or less of it.

'Tis to be noted, That the first red powder, which appears, issues out of the Hole of the Grain, that is on the side, where the Grain adhered to the Plant. And that, which about the end appears sticking on the Grain, hath been alive in the husk, having pierced its cover; though the hole, whence it commonly issues, remains close as to the Eye.
An Account of some Books lately published.


The Learned and Inquisitive Author of this Book, hath by his laudable example of collecting together, what Natural things are to be found here in England, of all sorts (which he has done upon his own expences) given an invitation to the curious in all parts of the world to attempt the like, thereby to establish the much desired and highly useful commerce among Naturalists, and to contribute everywhere to the composing of a genuine and full History of Nature.

In the Preface he intimates, that his stock does still increase daily; and that therefore the Reader may expect an Appendix to this collection.

In the Body of the Book, he enumerates all the Species, Alphabetically: And, as to Vegetables, he reckons up about 410 sorts; and gives their Latine and English Names, and the Places and Times of their growth: reducing them afterwards to certain Classes, hitherto used by Botanick Writers in their Histories of Plants: Adding the Etymology of their Generick Names, and a compendious Register of the Time, when and how long the English Plants do shoot and flourish.

As to Animals, he finds of them about 340 kinds in England, whereof the four-footed are about 50. Birds 170, and Fishes 320. Insects are innumerable, which yet he endeavours to enumerate, and to reduce to certain Classes; into which he also brings the three former kinds.

Concerning Fossils, he first takes notice of the Metals found in English Mines; as Silver, Tin, Copper, Iron, Lead, Antimony, and some Gold extracted out of Tin. Next of the Stones, of which he finds about 70 sorts; & amongst them, Bristol-Diamonds, Agates, Hyacinths, Emerods, Lead-stones, Torz-stones (which last yet he affirms to be nothing but the grinding-teeth of the Fifth
Filth (lupus) Pearls, Corals, Marble, Alabaster, Emery: To which he adds the various kinds of Coals; as also Bitumens, Turfs, and Jets. And thirdly of the various kinds of Allum, Vitriol, Niter, Sea-salt, Pit-salt. But fourthly of the various Earths, of which he reckons up 15 peculiar sorts (besides those that serve for Husbandry, which are not easily numbered;) and amongst them Read-lead, Black-lead and Fullers-earth.

He concludes all with mentioning the several Meteors appearing in England; and the Hot Springs, and Medical Waters; as also, the Salin, Petrifying, and some more unusual Springs: Item, Subterraneous Trees, Subterraneous Rivers, Ebbings and Flowings of Wells, &c.

II. PLACITA PHILOSOPHICA Guarini. The chief subject of this Treatise is Natural Philosophy; upon many important questions whereof it largeth, as those of the Motion of the Celestial Bodies, of Light, of Meteors, and of the vital and animal functions; leaving sometimes the common opinions, and delighting in the defence of Paradoxes.

E.G. That the material substantial Form, is nothing but mera potentia, and subsists not by itself; by which means the Author judges, he can free himself from many great difficulties touching Generation and Corruption, which do perplex the other Philosophers.

He holds Epicycles to be impossible, and Excentrickes, not sufficient to explicate the motion of the Stars; but that all the irregularities of this motion may be salved by the means of certain Spiral Lines; largely proving this Hypothetis, and particularly explicating the motion of each Planet.

He denies the middle Region of the Air to be cold; and believes that cold is not necessary to condense the vapours into Water.

He admits not that received Axiome, That the generation of one Body is the corruption of another; maintaining that there are Generations, to which no corruption ever proceeded; and that it may happen, that one Animal without dying may be changed into another Animal.

He allidges several reasons to evince, that the Air breathed in, enters not only into the whole capacity of the Chest, but also into the lower belly.
He is of opinion, that the Air, which is commonly believed to corrupt easily, is incorruptible; alluding among other reasons, this for one, that experience shews, that if a Bottle be exactly stop'd, there is never any mixt Body form'd in it; wherefore, faith he, the Air is not corrupted there.

He maintains, that 'tis not the Magnet that draws the Iron, but rather the Iron that attracts the Magnet. To explain which he affirms, that the Load-stone spreads abroad out of it self many corpuscles, which the substance of the Iron imbibes; and that, as dry things attract those that are moist, by the same reason Iron draws the Load-stone.

He rejects the species Intentionales, Vital and Animal Spirits, and holds many other uncommon opinions, touching Light, the Iris, the Flux and Re-flux of the Sea, &c.

This Author propofing to himself to discover both the principal Organ of the Taste, and the nature of its object, begins with the latter, and examines first, what is Taste? He judges that it is caused by nothing but Salts, which being variously figured, affects the tongue variously: alluding this for his chief reason, that the Salt which is extracted by Chymists out of any mixt body whatever it be, carries away with it all its taste, and that the rest remains taste-less. He adds that the Teeth in grinding the Food, serve much to extract this Salt: And he notes by the by, that the Teeth are so necessary for preparing the aliment, that certain Animals which seem to have none, have them in their stomach; and that nature has put at the entry of the palat of those that are altogether destitute of them, certain moveable inequalities, which are to them instead of Teeth.

But then secondly, concerning the Organ of Taste, he esteems, that 'tis neither the Flesh, nor the Tongue, nor the Membrans, nor the Nerves found there, nor the Glanduls, called Amygdaline; but those little eminences, that are found upon the tongue of all Animals. To obtain which, he observes,

1. That from the middle of the Tongue to the root, as also towards the tip, there are found innumerable little Risings called...
led Papillares; but that from the tip of the Tongue unto the
string there is observed none at all.

2. He hath experimented, that if you put Sal Armoniack up-
on the places of the Tongue, where those Eminencies are not,
you shall find no Taste; but that you will find it presently, as soon
you put any such Salt, where they are, to be met with. Ergo,
faith he, those Eminencies are the principal Organ of Taste.

3. He assures, that with a Microscope may be seen in those
Risings many little holes, at the bottom whereof there are small
nerves, terminating there: But he directs, to observe this in
live and healthy, not in dead or sick Animals.

Having laid down these Observations, he concludes, that the
manner, after which Taste is perform'd, is this: That the parti-
cles of Salt passing through those pores, which pierce the Papil-
lar Eminences, and penetrating as far as to the nerves, that
meet them there, do by the means of their small points prick
them; which pricking is called the Taste.

In the mean time he acknowledges, that before him Signior
Malpighi, Professor at Messina, had made some of these disco-
veryes.

The notice of these two last Books we owe to the French
Journal.

Correct in Numb. 19.
Pag. 342. lin. 33. read mixt Orcs in stead of, mixt with Orcs.

Correct in this present Numb. 20.
Page 359. line 13. Read Marie for Perle.

London, Printed for Moses Pitt at the White-
Hart in Little-Britain.
Eclipsis Solaris GEDANI.
Anno 1666 Die 2 Iulii s.n. ante m. observata.
A Johanne Hovelen.

Eclipsis Lunae observata GEDANI.
Anno 1666. Die 16 Iunii. St.n. a Johanne Hovelen.

Phases
8 Dig. 29 8 D. 7 D. 6 D. 5 D. 4 D. 3 D. 2 D. 1 D.

Crescentes

A

Phases

DeCrescentes
Finis 1 D. 2 D. 3 D. 4 D. 5 D. 6 D. 7 D. 8 D.

A
The Contents.

An Account, formerly promised, of Monsieur Hevelius's Calculation of the late Solar Eclipse's Quantity, Duration, &c. The Figure of the Star in the Constellation of Cygnus, together with the New Star in it, discovered some years ago, and very lately seen again by the same Mr. Hevelius. An Extract of a Letter, written by Mr. Auzout, concerning a way of his, for taking the Diameters of the Planets, and for knowing the Parallax of the Moon: Giving also a Reason, why in the Solar Eclipse above-mentioned, the Diameter of the Moon did increase about the end. A Relation of the loss of the Way to prepare the Bononian Stone for shining. A Description of a Swedish Stone, affording Sulphur, Vitriol, Allum, and Mignon. A Relation of the Raining of Ashes. An Extract of a Letter from Rome, rectifying the Relation of Salamanders living in Fire. An Account of several Engagements for Observing of Tydes. Some Suggestions for Remedies against Cold. A Relation of an uncommon Accident in two Aged Persons. An Account of Two Books, I. ISAIAELIS BULLIALDI ad Astronomos Monita duo: Primum, de Stella Nova, in Collo Ceti ante aliquot annos visa. Alterum, de Nebulosa Stella in Andromedae Cinguli parte Borea, ante biennium iterum orta. II. ENTRETIENS sur les vies & sur les Ouvrages des plus excellens Peintres, antients & modernes, par M. FELIBIEN.

Monsieur Hevelius's Calculation of the late Solar Eclipse's Quantity, Duration, &c.

His Calculus was not long since communicated by Monsieur Hevelius in a Letter to the Publisher, as follows,

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>H.</td>
<td>H.</td>
<td>0.</td>
<td>H.</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>5.51.11</td>
<td>5.51.0</td>
<td>17.45</td>
<td>5.53.12</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>5.57.5</td>
<td>5.57.0</td>
<td>18.37</td>
<td>5.59.28</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>6.0.0</td>
<td>6.0.0</td>
<td>18.55</td>
<td>6.1.28</td>
<td></td>
</tr>
<tr>
<td>Initium</td>
<td>6.55.30</td>
<td>6.57.30</td>
<td>6.57.30</td>
<td></td>
<td></td>
<td>Initium circa 79 gr.; à puncto Zeniti osculorum versus contigit.</td>
</tr>
<tr>
<td>10</td>
<td>6.57.30</td>
<td>7.0.23</td>
<td>7.0.0</td>
<td>7.2.23</td>
<td>7.4.30</td>
<td></td>
</tr>
<tr>
<td>20</td>
<td>7.2.30</td>
<td>7.1.8 ferœ.</td>
<td>7.7.59.30</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>31</td>
<td>7.1.73</td>
<td>7.1.73.50</td>
<td>7.1.73.50</td>
<td>7.19.50</td>
<td></td>
<td></td>
</tr>
<tr>
<td>41</td>
<td>7.4.50</td>
<td>7.5.23 ferœ.</td>
<td>7.23.35</td>
<td></td>
<td>7.23.35</td>
<td></td>
</tr>
<tr>
<td>51</td>
<td>7.10.57</td>
<td>7.10.57</td>
<td>7.25.43</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>63</td>
<td>7.1.45</td>
<td>7.1.45.57</td>
<td>7.29.53</td>
<td></td>
<td>7.29.53</td>
<td></td>
</tr>
<tr>
<td>73</td>
<td>7.1.75</td>
<td>7.1.75.57</td>
<td>7.33.50</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>84</td>
<td>7.21.35</td>
<td>7.21.35</td>
<td>7.38.55</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>94</td>
<td>7.23.43</td>
<td>7.23.43</td>
<td>7.40.0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>105</td>
<td>7.27.53</td>
<td>7.27.53</td>
<td>7.41.45</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>116</td>
<td>7.31.50</td>
<td>7.31.50</td>
<td>7.44.30</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>126</td>
<td>7.36.55</td>
<td>7.36.55</td>
<td>7.46.6</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>136</td>
<td>7.38.5</td>
<td>7.38.5</td>
<td>7.48.0</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>147</td>
<td>7.39.45</td>
<td>7.39.45</td>
<td>7.50.21</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>157</td>
<td>7.42.30</td>
<td>7.42.30</td>
<td>7.53.15</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>167</td>
<td>7.44.6</td>
<td>7.44.6</td>
<td>7.55.37</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>177</td>
<td>7.46.0</td>
<td>7.46.0</td>
<td>7.57.45</td>
<td></td>
<td></td>
<td>Maximo obtortu exstitit 5.8.2.123.46.</td>
</tr>
<tr>
<td>187</td>
<td>7.48.2</td>
<td>7.48.2</td>
<td>7.59.8</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>198</td>
<td>7.51.15</td>
<td>7.51.15</td>
<td>8.1.5</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>208</td>
<td>8.0.63</td>
<td>8.0.63</td>
<td>8.8.3</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Quod Sciatorium cum correcto tempore non omnino convenit, non nisi Lines Meridianæ imputandum.

Hujusque Semidiameter Luni aqualis extitit Solari.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>247</td>
<td>3 2 1 4</td>
<td>H.</td>
<td>H.</td>
<td>0.</td>
<td>H.</td>
</tr>
<tr>
<td>257</td>
<td>2 1 4</td>
<td>8.11.25</td>
<td>8.12</td>
<td>8.13.25</td>
<td></td>
</tr>
<tr>
<td>267</td>
<td>3 2 1 4</td>
<td>8.17.30</td>
<td>8.18</td>
<td>8.19.30</td>
<td></td>
</tr>
<tr>
<td>275</td>
<td>2 2</td>
<td>8.28.8</td>
<td>8.28</td>
<td>8.30.8</td>
<td></td>
</tr>
<tr>
<td>285</td>
<td>1 2 1 4</td>
<td>8.3014</td>
<td>8.30</td>
<td>8.32.14</td>
<td></td>
</tr>
<tr>
<td>294</td>
<td>1 2 1 4</td>
<td>8.3625</td>
<td>8.36</td>
<td>8.38.25</td>
<td></td>
</tr>
<tr>
<td>303</td>
<td>1 2 1 4</td>
<td>8.4319</td>
<td>8.43</td>
<td>8.45.19</td>
<td></td>
</tr>
<tr>
<td>313</td>
<td>1 2 1 4</td>
<td>8.4612</td>
<td>8.46</td>
<td>8.48.12</td>
<td></td>
</tr>
<tr>
<td>323</td>
<td>1 2 1 4</td>
<td>8.47.32</td>
<td>8.47</td>
<td>8.49.32</td>
<td></td>
</tr>
<tr>
<td>332</td>
<td>1 2 1 4</td>
<td>8.50.57</td>
<td>8.50</td>
<td>8.52.57</td>
<td></td>
</tr>
<tr>
<td>342</td>
<td>1 2 1 4</td>
<td>8.54.15</td>
<td>8.54</td>
<td>8.56.15</td>
<td></td>
</tr>
<tr>
<td>351</td>
<td>1 2 1 4</td>
<td>8.58.24</td>
<td>8.58</td>
<td>8.58.24</td>
<td></td>
</tr>
<tr>
<td>361</td>
<td>1 2 1 4</td>
<td>8.59.35</td>
<td>8.59</td>
<td>9.0.35</td>
<td></td>
</tr>
<tr>
<td>370</td>
<td>1 2 1 4</td>
<td>9.1.38</td>
<td>9.1</td>
<td>9.1.38</td>
<td></td>
</tr>
<tr>
<td>380</td>
<td>1 2 1 4</td>
<td>9.3.20</td>
<td>9.3</td>
<td>9.5.20</td>
<td></td>
</tr>
<tr>
<td>390</td>
<td>Finis</td>
<td>9.6.53</td>
<td>9.6</td>
<td>9.8.53</td>
<td></td>
</tr>
</tbody>
</table>

This Observation is by the same Astronomer, represented also by the Figures AAAAAA; as that of the Horizontal Eclipse of the Moon, is, by the Figures BB.
The Figure of the Stars in the Constellation of Cygnus; together with the New Star in it, discover'd some years since, and very lately seen by M. Hevelius again.

The Relation concerning this New Star in the Breast of Cygnus, very lately discover'd again at Danzig, by M. Hevelius, was publish'd Numb. 19. p. 349. The Figure of that Constellation, with the New Star in it, was thus, hastily drawn, sent over by that Observer.

In ancone Ala Bor.

In Peílore

* Nova ante pétum.

* in Collo.

* In Postro.
An Extract

Of a Letter written Decemb. 28. 1666. by M. Auzout to the
Publisher, concerning away of his, for taking the Diameters of the Planets, and for knowing the Parallax of the Moon; as also the Reason, why in the Solar Eclipse above calculated, the Diameter of the Moon did increase about the end.

I did apply my self the last Summer to the taking of the Diameters of the Sun, Moon, and the other Planets, by a Method, which one M. Picard and my self have esteem'd by Us the best of all those, that have been practis'd hitherto, since we can take the Diameters to Second Minutes, being able to divide one foot into 24000, or 30000. parts, scarce failing as much as in one only part, so as we can in a manner be assure'd, not to deceive our selves in 3. or 4. seconds. I shall not now tell you my Observations, but I may very well assure you, that the Diameter of the Sun has not been much less in his Apogee, than 31. m. 37. or 40. sec. and certainly not lesse than 31. m. 35. sec. and that at present in his Perigee it passes not 32. m. 45. sec. and may be lesse by a second or two. That, which is at the present troublesome, is, that the Vertical Diameter, which is the most ease to take, is diminisht, even at Noon, by 8. or 9. sec. because of the Refractions, which are much greater in Winter than Summer at the same height; and that the Horizontal Diameter is difficult, because of the swift motion of the Heavens.

As for the Moon, I never yet found her Diameter less than 29. m. 44. or 45. sec. and I have not seen it pass 33. m. or if it hath, it was only by a few seconds. But I have not yet taken her in all the kinds of situations of the Apogees and Perigees which happen, with the Conjunctions and Quadratures. I do not mention all, what can be deduced from thence, but if you have Persons at London, that observe these Diameters, we may entertain our selves more about this Subject another time. I shall only tell you, that I have found a Way to know the Parallax of the Moon, by the means of her Diameter: Vid. If on a day, when she is to be in her Apogee or Perigee, and in the most Boreal Signes, you take her Diameter towards the Horizon, and then towards the South, with her Altitudes above
above the Horizon. For, if the Observation of the Diameters be exact; as in these Situations the Moon changes not considerably her Distance from the Earth in 6, or 7. hours, the Difference of the Diameters will shew the Proportion there is of her Distance, with the Semi-diameter of the Earth. I do not enlarge, because that as soon as one hath this Idee, the rest is easie. The same would yet be practis'd better in the places, where the Moon passes through the Zenith, than here; for the greater the difference is of the Heights, the greater is that of the Diameters. I do not note (for it easily appears) that, if one were under the same Meridian, or the same Azimuth in two very distant places, and took at the same time the Diameter of the Moon, one would do the same thing; though this Method goes not to preciseness.

From what has been said, may be collected the reason of the Observation, which M. Hevelius made in the last Eclipse of the Sun, touching the increase of the Moon's Diameter about the end. I am exceeding glad, that a person, who probably knew not the cause of it, has made the Experiment: but it is strange, that until now no Astronomer has foreseen, that that should happen, nor given any precepts for the Change of the Moons Diameter in the Eclipses of the Sun, according to the places, where they should happen, and according to the Hour and Height, the Moon should have. For, what happened in that Eclipse of Augmentation, would have fallen out contrarily, if it had been in the Evening; for, the Moon, which in that Eclipse, that began in the Morning, was higher about the end than at the beginning, was nearer us, and consequently was to appear bigger: But if the Eclipse should happen in the Evening, she would be lower at the end, and therefore more distant from us, and consequently appear less. So also in two different places, whereof one should have the Eclipse in the Morning, and the other at Noon, the Moon should appear bigger to him that hath it at Noon: And she must likewise appear bigger to those, who shall have a less Elevation of the Pole under the same Meridian, because the Moon will be nearer them.

I wish, I could satisfy you about the Optick Glasses of Signior Burattini in Poland, which he hath sent hither; but I have not yet seen their performances myself. I only saw once the Glasses, which
which are perfectly well wrought, and well polish'd. Those, that have tried them, find them very good, but they are only, the one of 10, the other of 8. foot. A good Astronomer told me, that they would bear a great Aperture in respect of their length.

I do not well know, what to say to yours concerning M. Hevelius. Mean while, the interest of truth, and the obliging manner, he has treated me with, engage me to answer him, in the matter of the Comets: I am perswaded, I shall convince him, but since he hath taken the Illustrious Royal Society for Judge, I accept that with all my heart.

A Relation
Of the loss of the Way to prepare the Bononian Stone for shining.

Though several Persons have pretended to know the Art of preparing and calcining the Bononian Stone, for keeping a while the Light once imbibed; yet there hath been indeed but one, who had the true secret of performing it. This was an Ecclesiastick, who is now dead, without having left that skill of his to any one, as Letters from Italy and France, some while since, did inform. There is no substance, in Nature, known to us, that hath the effect of this Stone; so that (to the shame of the present Age) this Phenomenon is not like to be found any where, but in Books, except some happy Genius light upon the same or the like skill.

*A Description
Of a Swedish Stone, which affords Sulphur, Vitriol, Allum, and Minium.

This was communicated to the R. Society by Sir Gilbert Talbot Knight, a Worthy Member of that Body, as he had received it in Denmark, being his Majesties Extraordinary Envoy there, as follows.

Here is a Stone in Sweden of a Yellow Colour, intermixed with streaks of white (as if composed of Gold and Silver) and heavy withal. It is found in firm Rocks, and runs in Veins.
upon which they lay Wood, and set it on fire. When the Stone is thus heated, they cast Water upon it, to make it rend, and then dig it up with Mattocks. This done, they break it into smaller pieces, and put it into Iron-pots, of the shape represented by Figure C, the mouth of the one going into the other. These they place, the one in the Oven upon an Iron fork sloping, so that, the Stone being melted, it may run into the other, which stands at the mouth of the Oven, supported upon an Iron. The first running of the Stone is Sulphur.

The remainder of the burned Stone is carry'd out, and laid upon a high Hill, where it lies exposed to the Sun and Air for the space of two years; and then taketh fire of itself, casting forth a thin blew flame, scarce discernible in the day time. This being consumed, leaveth a blew dust behind it, which the Workmen observe, and mark with wooden pins. This they dig up, and carry into the Work-house, and put it into great Tubs of Water, where it infuseth 24 hours or more. The Water they afterward boil in Kettles, as we do Saltpeter, and put it into cooling Tubs, wherein they place crofse Sticks, and on them the Vitriol fastens, as Sugar-candy doth.

The Water, that remains after the extraction of the Vitriol, they mix with an eight part of Urin and the Lees of Wood-ashes, which is again boiled very strong, and being set to cool in Tubbs, croffe Sticks are likewise placed, and thereon the Allum fastens.

In the Water, which remains after the Allum, is found a Sediment, which being separated from the Water, is put into an Oven, and Wood laid upon it and fired, till it become red, which makes the Minium, wherewith they paint their Houses, and make plaster.

So far this Description; Which gave occasion to a curious person to call to mind, That there was a kind of Stone in the North of England, yielding the same substances, except Minium.
A Relation
Of the Raining of Ashes, in the Archipelago, upon the Eruption of Mount Vesuvius, some years ago.

This came but lately to hand from that knowing person, Mr. Henry Robinson; and was thought fit to be now inserted here, that it might not be lost, though it hath hapned above 30 years ago. It was contained in a Letter, (subscribed by Capt. Will. Badly) in these words:

The 6th of December 1631, being in the Gulf of Volo, riding at Anchor, about ten of the Clock that Night, it began to rain Sand or Ashes, and continued till two of the Clock the next Morning. It was about two inches thick on the Deck, so that we cast it over board with Shovels, as we did Snow the day before: The quantity of a Bushel we brought home, and presented to several Friends*, especially to the Masters of Trinity House. There was in our Company, Capt. John Wilds Commander of the Dragon, and Capt. Anthony Watts, Commander of the Elisabeth and Dorcas. There was no Wind stirring, when these Ashes fell, it did not fall only in the places, where we were, but likewise in other parts, as Ships were coming from St. John D’Acre to our Port; they being at that time a hundred Leagues from us. We compared the Ashes together, and found them both one. If you desire to see the Ashes, let me know.

An Extract
Of a Letter not long since written from Rome, rectifying the Relation of Salamanders living in Fire.

This came from that Expert Anatomist M. Steno, to Dr. Croon; Videl. That a Knight called Corvini, had assured him, that, having cast a Salamander, brought him out of the Indies, into the Fire, the Animal thereupon swell’d presently, and then vomited store of thick flimy matter, which did put out the neighbouring Coals, to which the Salamander retired immediately, putting them out again in the same manner, as soon as they re-

D d d kindled,
kindled, and by this means saving himself from the force of the Fire, for the space of two hours; the Gentleman above-mentioned being then unwilling to hazard the Creature any further: That afterwards it lived nine Months: That he had kept it eleven Months without any other food, but what it took by licking the Earth, on which it moved, and on which it had been brought out of the Indies; which at first was covered with a thick moisture, but being dried afterwards, the Urin of the Animal served to moisten the same. After the eleven Months, the Owner having a mind to try, how the Animal would do upon Italian Earth, it died three days after it had changed the Earth.

An Account
Of several Engagements for observing of Tydes.

Since nothing is more important for discovering the Cause of that Grand Phenomenon of Nature, the Flux and Reflux of the Sea, than a true and full History of the Tydes, the Virtuosi of England have of late (especially since the Publication of Dr. Wallis, his Theory touching that Appearance) taken care, to direct and recommend in several parts of the World, and particularly in the most proper places of these Islands, such Observations, as may contribute to the elucidating of that Subject.

And as formerly they have sent their Inquiries of this Nature to the Isle of St. Helena, situated in the open Ocean beyond the Equinoctial, and already received some account thereupon; so they have since dispatched the like for the Bermudas, an Isle that hath no less convenience of situation for that purpose. And they intend (as will more amply appear, God permitting, in a short time) to lodge with such Masters of Ships and Pilots, as shall sail into remote parts, very particular directions of that kind, to be printed at the Royal Societies charges, and to be committed to the care of the Masters of Trinity House for disposing of them to that end.

And, as for the Observations, to be made in these Kingdoms; it is hoped, that the Masters in the Art of Navigation at Bristol (Mr. Standridge and Mr. Iff) will undertake that business with affection and care: the former of these two having already (as we are informed from a good hand) made a Collection of the Tydes, for...
for some years past, and found them differing from former Observations and Tables; the other promising future diligence in this matter; noting in the meantime, that some Tides of last Autumn were so far differing from former Observations, that neither he, nor any others there, could make anything of it.

We must not omit here to mention the readiness, expressed by these worthy Gentlemen, Mr. Rob. Boyle, Sir. Rob. Moray, and Mr. Henry Powle, for concurring in this Work; the first, having undertaken to recommend Observations of this nature, to be made, upon the Western Coast of Ireland*; the second, upon the West of Scotland; and the third, in the Isle of Lundy; to whom we must adde the inquisitive Mr. Sam. Colepresse, for Plymouth, and the Lands-end. Besides, we hope to engage the curious of France in the same undertaking, especially for procuring, besides what is known already concerning that place, a very particular and exact account of the Tides upon the coast of Brittany, where (especially about St. Malo) they are found to rise to admiration, even to 60, 70, and sometime 80, feet, at the New and Full Moon.

Some Suggestions
For Remedies against Cold.

As there have been Remedies found out against excessive Heat, and Means of cooling Meat and Drink; so it was lately, on the occasion of the sharp Season, suggested, That Remedies might be thought on against Cold; and that particularly it might be inquired into,

1. What things in Nature, or by Art, or Mechanical contrivance will retain a warming Heat longest, or a melting or scorching Heat?

2. What will continue or maintain Fire longest?

Some that observe common practices and vulgar Trades, take notice, That Foplers use Leaden-Pots for their Glue, alleging for a Reason, That Lead, being a close Mettal, retains the heat longer

* The Observations particularly recommended for these Coasts, are these:

1. At what hour it is High-water on the day of the New and Full Moon, upon every Cape and Bay of the Western Coast of Ireland.

2. How long after the New and Full Moon the highest Spring-tides fall out.

3. What are the perpendicular heights of the Floods, both at the ordinary, and the Spring-tides.
longer than other Mettals. Cary's Warming-stone promised a
warmth for six or eight hours; if it performed but for two or three
hours, it would be of great use. 'Tis found by sad experience, how
hurtful Bright Fires, and especially of Stone-coal are to the
Eyes.

To retain Fire long, certain Black Earths are useful, as we were
newly informed by the Inquisitive Dr. B. That a Gentleman in
Sommerisfetshire, called Mr. Speke, had bountifully obliged Ilminster,
and his Neighborhood, by a Black Fat-Earth lately found in
his Park. But thesame Correspondent adds, That he never saw
any parallel to a Sea-weed, which he and some of his Fellow-Stu-
dents had in Cambridge in the mouth of a Barrel of good Cysters.
It was smaller than Peafe-halm, yet cut, it lasted two very great
Fires of Sea-coal, burning bright in the midst of the Fire, and
by a streak of the Tongues, it fell into the Hearth, jingling like
Mettal.

A Relation

Of an uncommon Accident in two Aged Persons.

This was imparted by the above-mentioned Mr. Colepresse, who
assures in his Letter, containing this Account, That the matter of
fact was thorowly examined by himself, and that he was fully, and
in all respects, satisfied of the truth thereof.

The Relation of the one, is in these words.

Joseph Shute, Clerk, Parson of Mary (nigh Plymouth) in the
County of Devon, aged 81 years, being a temperate man, and of
an healthy constitution, having the in-most Grinder loose, and so
remaining, perceived, that his mouth, about three Months since,
was somewhat frighten'd, and upon inquiry into the cause of it,
found, That he had a new Tooth (the third Grinder) being the
innermost of the upper Jaw in the Right Cheek, which still remains
firm.

The Account of the other follows thus.

Maria Stert, of Benecliffe, in Plympton St. Mary (near Plymouth)
in Devon, aged about 75 years, an healthy person, having had
nine children, about the fortieth year of her age lost three of her
upper Incisores or Cutters, the other drawn out, and so remained Toothless, as to them, for about 25 years, when she perceived, that a new Tooth came forth (without any pain) next the Canini of the left Check: And about two years after, another Tooth grew out likewise without pain, close by the former. The first whereof, never came to above half the length of her former Cutters, the latter scarce breaking the skin: Both which yet proved serviceable, till about six weeks since, when she eating (no hard, crusty, or solid) Meat, that Tooth which came out first, fell down into her Mouth, without any looseness before hand perceived, or any pain; which had not a phang like other Cutters, but much less, and shorter. The other abides firm, and serviceable.

To the truth of these Relations, not only the said Joseph Shute and Maria Stert, have put the one his name, the other her Mark, the third and seventh of January, 1666, but also Sir William Strode, and Mr. Coleprese have subscribed the same, as believing the Relation to be true.

An Account of two Books.

I. ISMAELE BULLIALDI ad Astronomos Monita duo: Primum, De Stella Nova, que in Collo Ceti ante annos aliquot visa est. Alterum, De Nebulosa in Andromede Cinguli parte Boreae ante biennium iterum orta.

The chief end of the Author in publishing this Tract, seems to be, To excite Astronomers to a diligent observation, both of that New Star in the Neck of the Whale, to be seen in February and March next; and of that other, in the Northern part of Andromeda's Girdle, to be seen at this very present.

As to the former of these Stars, he affirms, that, as it hath appeared for many years in the said place, so it will in the beginning of March next appear equal to the Stars of the third Magnitude, or perhaps bigger, and that about the end of the same Month, if the Crepuscle do not hinder, the greatest Phasis of it will appear, if so be, that it keep the same Analogy of Motions and Periods, which it observed from An. 1638 to An. 1664. Where he takes notice of the Causes, why its two greatest Appearances could not be seen, An. 1664, 1665, 1666; and how he comes...
comes to know, that in the beginning of March next, it will equal, or even exceed the Stars of the Third Magnitude; noting, that from the Observations hitherto made of this Star, it is manifest, that the greatest Phases thereof do every year anticipate by 32. or 33. days; forasmuch as An. 1660. its greatest Appearance was about the end of October and the beginning of November; An. 1661. about the end of September, or the beginning of October; An. 1662. about the end of August, &c. so that this year it must be in March, if the former Analogy do hold.

He collects also from the Observations, That one Period from the greatest Phase to the next, consists of about 333. days: but that the interval of the time betwixt the times of its beginning to appear equal to the Stars of the Sixth Magnitude, and of its ending to do so, consists of about 120. days: And that its greatest Appearance lasts about 15. days: All which yet he would have understood with some latitude.

This done, he proceeds to the investigation of the Causes of the Vicissitudes in the Emerision and Disappearance of this Star, and having discoursed, That the apparent Increase and Decrement of every Lucid Body proceeds either from its changed distance from the Eye of the Observer, or from its various site and position in respect of him, whereby the angle of Vision is changed; or from the increase or diminution of the bulk of the lucid body itself: and having also demonstrated it impossible, that this Star should move in a Circle, or in an Ellipsis; and proved it improbable that it should move in a Strait Line; he concludes, that there can be no other genuine, or at least, no other more probable cause of its Emerision and Occulation, than this, That the bigger part of that round Body is obscure and inconspicuous to us, and its lesser part lucid, the whole Body turning about its own Center, and one Axe; whereby for one determinate space of time it exhibits its lucid part to the Earth, for another, subducts it: it not being likely, that fires should be kindled in the Body of that Star, and that the matter thereof should at certain times take fire and shine, at other times be extinguished upon the consumption of that matter.

So far of that Star. As to the other in the Girdle of Andromeda, seen about the beginning of An. 1665, she relates, that, when in the end of 1664. the World beheld the then appearing Comet, Astro-
Astronomers observed also this new Phænomenon, which was called by them Nebulosæ in Cingulo Andromedæ. Concerning which, he notes, that the same had been already seen many years before by Simon Marius, vid. An. 1612. when with a Telescope he search'd for the Satellites of Jupiter, and observed their motions; alledgeing for proof hereof, the said Authors own words, out of his own Book, De Mundo Foviali, publish't An. 1614. And farther shews, that it hath formerly appear'd (about 150. years ago) and been taken notice off by an expert, though Anonymous, Astronomer; whose words he cites out of a Manuscript, brought out of Holland by the Excellent Jacobi Augusti Thuanus, returning from his Embassy to Paris; wherein also was marked the Figure of that Phænomenon, represented in print by our Author: who from all this collects, that, whereas this Star hath been seen formerly, and that 150. years since, but yet neither observed by Hipparchus, nor any other of the Antients, that we can find; nor also in the former Age by Tycho Brahe, nor in our Age, by Bayerus; and appear'd also in the Month of November last (wherein he wrote this Tract) much lessened and obscure, after it had, two years ago, shone very bright, that therefore it must needs appear and dis-appear by turns, like those in the Necks of the Whale and Swan.

II. ENTRIENS sur les Vies et sur les Ouvrages Des plus excellens Peintres, Anciens et Modernes, par Monsieur FELIBIEN.

This Author, having first discoursed of that Royal Palace the Louvre, and the Designs of finishing it; passeth on to the Art of Picturing, and treats of the three principal things, wherein a good Master of the Art must excel, vid. the Composition, Designing, and Laying on of Colours, which done, he ravelles into the Origne, and deduces the Progress of Painting, and relates what is most remarkable in the Lives of the Antient Painters: And among many particulars, he observes in the Life of Andreas de Sarte, how difficult it is, to judge well of a Picture; relating, that a Duke of Mantua, having obtained of Clement VII. a Portrait of Leo X, which had been done by Raphael Urbin, and was at Florence, those of that Town being unwilling to lose so excellent a piece,
piece, caused a Copy thereof to be made by the said Andreas de Sarte, which they sent instead of the Original. This Copy was so perfect, that Giulio Romano, who had been bred and taught by Raphael, and was one of the best Painters of Italy, took it for an Original; and would never have been undeceived, if one Vasari had not assured him, that it was but a Copy, which himself had seen made, and had not shew'd him certain marks, that were there put to discriminate it from the Original.

In the Second Part, the Author has set down all that is requisite to judge and discourse well of Painting. But, to add Examples to Precepts, he discourses of the Modern Painters, and making a Description of their best Works, he takes occasion to observe, what is there found most excellent, and to shew, how they have put in practice the Rules of Art. He treats also of the declining of Painting, and affirms, that nothing considerable hath been done in it from the time of Constantine, till An. 1240, when one, Cima-bue, began to raise this Art again. After this, he gives a List of the Painters, that since have been famous for their Works, preferring before all others, Raphael Urbin. The last of all is the above-mention'd Andre de Sartes, who died, An. 1530, and whom the liberality of Francis I. had drawn into France.

The Printing of these Tracts is now return'd to the first Printer thereof, as being somewhat re-setted after the late sad Fire of London.

FINIS.
The Contents.

Trials proposed to be made for the Improvement of the Experiment of Transfusing Blood out of one live Animal into another. A Method for Observing the Eclipses of the Moon, free from the Common Inconveniences. An Account of some Celestial Observations lately made at Madrid. Extract of a Letter, lately written to the Publisher, containing some Observations about Insects and their Innoxiousness, &c. An Account of some Books; vid. I. TOME TROISIEME DES LETTRES DE M. DES CARTES. II. ASTRONOMIA REFORMATA P. RICCIOLI. III. ANATOMIE MEDULLÆ SPINALIS ET NERVORUM, inde provenientium, GERARDI BLASII, M.D. An Advertisement about the re-printing of M. Evelyns Sylva and Pomona. A Table of the Transactions, printed these two years.

Trials proposed by Mr. Boyle to Dr. Lower, to be made by him, for the Improvement of Transfusing Blood out of one live Animal into another; promised Numb. 20. p. 357.

The following Queries and Tryals were written long since, and read about a Moneth ago in the R Society, and do now come forth against the Authors intension, at the earnest desire of some Learned Persons, and particularly of the worthy Doctor, to whom they were addressed; who thinks, they may excite and assist others in a matter, which, to be well prosecuted, will require many hands. At the reading of them, the Author declared, that of divers of them he thought he could fore-see the Events, but yet
yet judged it fit, not to omit them, because the Importance of the Theories, they may give light to, may make the Tryals recompence the pains, whether the success favour the Affirmative or the Negative of the Question, by enabling us to determine the one or the other upon surer grounds, than we could otherwise do. And this Advertisement he desires may be applied to those other Papers of his, that consist of Quaries or proposed Tryals.

The Queries themselves follow.

1. Whether by this way of Transfusing Blood, the disposition of Individual Animals of the same kind, may not be much altered? (As whether a fierce Dog, by being often quite new flocked with the blood of a cowardly Dog, may not become more tame, &c.;

2. Whether immediately upon the unbinding of a Dog, replenisht with adventitious blood, he will know and fawn upon his Master, and do the like custumary things as before? And whether he will do such things better or worse at some time after the Operation?

3. Whether those Dogs, that have Peculiarities, will have them either abolished, or at least much impaired by transfusion of blood? (As whether the blood of a Mastiff, being frequently transfused into a Blood-bound, or a Spaniel, will not prejudice them in point of scent?)

4. Whether acquired Habits will be destroy’d or impair’d by this Experiment? (As whether a Dog, taught to fetch and carry, or to dive after Ducks, or to set, will after frequent and full recruits of the blood of Dogs unfit for those Exercises, be as good at them, as before?)

5. Whether any considerable change is to be observ’d in the Pulse, Urin, and other Excrements of the Recipient Animal, by this Operation, or the quantity of his insensible Transpiration?

6. Whether the Emittent Dog, being full fed at such a distance of time before the Operation, that the mass of blood may be suppos’d to abound with Chyle, the Recipient Dog, being before hunry, will lose his appetite, more than if the Emittent Dogs blood had not been so chylous? And how long, upon a Vein
Vein opened of a Dog, the admitted blood will be found to retain Chyle?

7. Whether a Dog may be kept alive without eating by the frequent Injection of the Chyle of another, taken freshly from the Receptacle, into the Veins of the Recipient Dog:

8. Whether a Dog, that is sick of some disease chiefly imputable to the mass of blood, may be cured by exchanging it for that of a sound Dog. And whether a sound Dog may receive such diseases from the blood of a sick one, as are not otherwise of an infectious nature?

9. What will be the Operation of frequently stockling (which is feasible enough) an old and feeble Dog with the blood of young ones, as to liveliness, dulness, drowsiness, squeamishness, &c. or vice versa?

10. Whether a small young Dog, by being often fresh stockt with the blood of a young Dog of a larger kind, will grow bigger, than the ordinary size of his own kind?

11. Whether any Medicated Liquors may be injected together with the blood into the Recipient Dog? And in case they may, whether there will be any considerable difference found between the separations made on this occasion, and those, which would be made, in case such Medicated Liquors had been injected with some other Vehicle, or alone, or taken in at the mouth?

12. Whether a Purging Medicine, being given to the Emitter Dog a while before the Operation, the Recipient Dog will be thereby purged, and how? (which Experiment may be hugely varied.)

13. Whether the Operation may be successfully practis'd, in case the injected blood be that of an Animal of another Species, as of a Calf into a Dog, &c. and of a Cold Animal, as of a Fish, or Frog, or Tortoise, into the Vessels of a Hot Animal, and vice versa?

14. Whether the Colour of the Hair or Feathers of the Recipient Animal, by the frequent repeating of this Operation, will be changed into that of the Emitter?

15. Whether by frequently transfusing into the same Dog, the blood of some Animal of another Species, something further, and more tending to some degrees of a change of Species, may be
be effected, at least in Animals near of Kin; (As Spaniels and Setting Dogs, Irish Grey-hounds and ordinary Grey-hounds, &c.)

16. Whether the Transfusion may be practised upon pregnant Bitches, at least at certain times of their gravidation? And what effect it will have upon the Whelps?

There were some other Queries proposed by the same Author, as, the weighing of the Emittent Animal before the Operation, that (making an abatement for the Exfluviins, and for the Excrements, if it voids any) it may appear, how much blood it really loses. To which were annexed divers others not so fit to be pursued but by Physitians, and therefore here omitted.

A Method
For Observing the Eclipses of the Moon, free from the Common Inconveniencies, as it was left by the Learned Mr. Rook, late Gresham-Professor of Geometry.

Eclipses of the Moon are observed for two principal ends; One Astronomical, that by comparing Observations with Calculations, the Theory of the Moon’s Motion may be perfected, and the Tables thereof reformed: the other, Geographical, that by comparing amongst themselves the Observations of the same Ecliptick Phases, made in divers places, the Difference of Meridians or Longitudes of those places may be discerned.

The Knowledge of the Eclipse’s Quantity and Duration, the Shadows, Curvity, and Inclination, &c. conduce only to the former of these ends. The exact time of the Beginning, Middle, and End of Eclipses, as also in Total ones, the Beginning and End of Total darkness, is useful for both of them,

But because in Observations made by the bare Eye, these times considerably differ from those with a Telescope, and because the Beginning of Eclipses, and the End of Total darkness, are scarce to be observed exactly, even with Glasses (none being able clearly to distinguish between the True Shadow and Penumbra) unless he hath seen, for some time before, the Line, separating them, pass along upon the Surface of the Moon,) and lastly, because in small
Partial Eclipses, the Beginning and End, and in Total ones of short continuance in the Shadow, the Beginning and End of Total darkness, are unfit for nice Observations, by reason of the flow change of Apparances, which the Oblique Motion of the Shadow then causeth. For these reasons I shall propound a Method peculiarly design'd for the Accomplishment of the Geographical end in Observing Lunar Eclipses, free (as far as is possible) from all the mentioned Inconveniences.

For, First, It shall not be practicable without a Telescope. Secondly, The Observer shall always have opportunity before his principal Observation, to note the Distinction between the True Shadow and the Penumbra. And, Thirdly, It shall be applicable to those Seasons of the Eclipse, when there is the suddenest Alteration in the Apparances.

To satisfy all which intents,

Let there be of the Eminentest Spots, dispersed over all Quarters of the Moons Surface, a select number generally agreed on, to be constantly made use of, to this purpose, in all parts of the World. As, for Example, those which M. Hevelius calleth,

\[\text{Sinai, Aethna, Insula, Besbicus, Palus, Meotis, Marisotis.} \]

Let in each Eclipse, not all, but (for instance) three of these Spots, which then lie nearest to the Ecliptick, be exactly observed, when they are first touch'd by the True Shadow, and again, when they are just compleatly entred into it, and (if you please) also in the Decrease of the Eclipse, when they are first fully clear from the True Shadow: For the accurate determinations of which moments of time (that being in this business of main importance) let there be taken Altitudes of remarkable Fixed Stars, on this side
side of the Line, of such, as lie between the Equator and Tropic of Cancer; but beyond the Line, of such, as are situate towards the other Tropic; and in all places, of such, as at the time of Observation, are about 4 hours distant from the Meridian.

An Account

Of some Observations, lately made in Spain, by His Excellency the Earl of Sandwich.

The Right Honourable the Earl of Sandwich, as he appears eminent in discharging the Trust, his Majesty hath repose in him, of Ambassador Extraordinary to the King of Spain; so he forgets not in the midst of that Employment, that he is a Member of the Royal Society; but does from time to time, when his weighty State-Negotiations do permit, employ himself in making considerable Observations of divers kinds, both Astronomical and Physiological; and communicateth the same to the said Society; as for instance, lately, what he has observ'd concerning the Solar Eclipse in June last, the Sun's height in the Solstice, and also the Latitude of Madrid, esteeming by the Sun's Altitude in the Solstice, and by other Meridian Altitudes, the Latitude of Madrid to be 40 deg. 10 min, which differs considerably from that assigned by others; the General Chart of Europe giving to it 41 deg. 30 min, the General Map of Spain, 40 deg. 27 min. A large Provincial Map of Castile, 40 deg. 38 min.

To these particulars, and others formerly imparted, his Excellency is making more of the same nature; and particularly those of the Immotion of the Satellites of Jupiter.

We must not omit mentioning here, what he hath observed of Halo's about the Moon; which he relates in these words:

Decemb. 25. Old Style, 1666. In the Evening, here (vid. at Madrid) was a great Halo about the Moon, the Semidiameter whereof was about 23 deg. 30 min. Aldebaran was just in the North-east part of the Circle, and the two Horns of Aries just enclosed by the South-west of the Circle, the Moon being in the Center. I note this the rather (faith he) because five or six years ago, vid. Novemb. 21. Old Style, 1661, an hour after Sun-set, I saw a great Halo about the Moon of the same Semidiameter,
at Tangier, the Moon being very near the same place, where she was now.

Extract

Of a Letter, lately written by Mr. Nathaniel Fairfax to the Publisher, containing Observations about some Insects, and their Innoxiousness, &c.

The Ingenious Author of this Letter, as he expresses an extraordinary desire to see the Store-house of Natural Philosophy, more richly fraughted (a Work begun by the single care and conduct of the Excellent Lord Verulam, and prosecuted by the Joynt-undertakings of the R. Society) so he very frankly offers his Service in contributing some of his Observations, and begins in this very Letter to perform his Offer. For, Having taken notice of what was published in Numbr. 9, p. 161, out of the Italian Philosopher Redi, vid. That Creatures, reputed Venomous, are indeed no Poysons, when swallow’d, though they may prove so, when put into Wounds: He, for confirmation thereof, alledges Examples of several Persons well known to him (himself also having been an Eye-witness to some such Experiments) who have frequently swallow’d Spiders, even of the rankest kind, without any more harm than happens to Hens, Robin-red-breasts, and other Birds, who make Spiders their daily Commons. And having made mention of some men, that eat even Toads, he adds, that though a Toad be not a Poyson to us in the whole; yet it may invenome outwardly, according to some parts so and so stirr’d; an instance whereof he alledges in a Boy, who stumbling on a Toad, and hurling stones at it, some Juyce from the bruised Toad chanced to light upon his Lips, whereupon they swelld, each to the thickness of about two Thumbs: And he neglecting to use, what might be proper to restore them, they have continued in that mishapen size ever since; the ugliness whereof, when the Relator saw, gave him occasion to inquire after the cause of it, which thereupon he understood to be, as has been recited.

On this occasion, the same Gentleman relates, that once seeing a Spider bruised into a small Glass of Water, and that it tinged
It somewhat of a Sky-colour, he was, upon owning his surprise thereat, informed, that a dozen of them being put in, they would dye it to almost a full Azure. Which is touch't here, that, the Experiment being so easy to make, it may be tried, when the season furnishes those Insects; mean time, it seems not more incredible, that this Creature should yield a Sky-colour, when put in water, than that Cochineel, which also is but an Insect, should afford a fine red, when steep'd in the same Liquor.

An Account
Of Some Books.

I. Le Tome troisième et dernier des Lettres de M. DES-CARTES.

As the two first Tomes of M. Des-Cartes his Letters, contain Questions, for the most part of a Moral and Physiological Nature, proposed to, and answer'd by him; so this consists of the Contests, he had upon several Subjects with divers Men eminent in his time.

To pass by that sharp Contest, he was engaged in by some Professors of Divinity at Utrecht, who endeavoured to discredit his Philosophy, as leading to Libertinisme and Atheisme; notwithstanding he made it so much his business, as to assert the Existence of a Deity, and the Immortality of a Soul: We shall take notice of what is more to our purpose, vid. the Differences, he had touching his Dioptricks and Geometry.

As for his Dioptricks, though a great part of the Learned World have much esteem'd that Treatise, as leaving little to be said after him upon that Subject; yet there have not been wanting Mathematicians, who have declared their disagreement from his Principles in that Doctrine. The first of them was the Jesuit Fourdin, Mathematick Professor in the Colledg of Clermont at Paris, but this difference was soon at an end. A second was Mr. Hobbs, upon whose account he wrote several Letters to Mersennus, containing many remarks conducing to the Knowledge of the Nature of Reflection and Refraction. But the Person, that did most learnedly and resolutely attack the said Dioptricks, was Monsieur Fermat, writing
writing first about it to Mersennius, who soon communicated his Objections to M. Des-Cartes, who failed not to return his Answer to them. But Fermat replied, and Des-Cartes likewise, and after many reciprocations, in which each party pretended to have the advantage, the matter rested; until M. Fermat taking occasion to write afresh of it to M. De la Chambre, several years after Des-Cartes's death, upon occasion of a Book, written by M. De la Chambre, Of Light; discoursed with this new Author after the same rate, as he had done before with Des-Cartes himself, and seemed to invite some-body of his friends, to re-assume the former contest. Whereupon M. Clerflier and M. Rohault took up the Gantlet, to assert the Doctrine of the deceased Philosopher, exchanging several Letters with M. Fermat, all inserted in this Tome, and serving fully to instruct the Reader of this Difference, and withal to elucidate many difficult points of the Subject of Refractions; especially of this particular, Whether the Motion of Light is more easily, and with more expedition, performed through dense Mediums, than rare.

Besides this, though one would think, Disputes had no place in Geometry, since all proofs there, are as many Demonstrations; yet M. Des-Cartes hath had several Scuffles touching that Science. As M. Fermat had assaulted his Diptricks, so he reciprocally examined his Treatise De Maximis & Minimis, pretending to have met with Paralogismes in it. But the Cause of M. Fermat was learnedly pleaded for, by some of his Friends, who took their turn to examine the Treatise of Des-Cartes's Geometry; whereupon many Letters were exchanged, to be found in this Book, and deserving to be considered, which doubtless the Curious would easily be induced to do, if Copies of this Book were to be obtain'd here in England, besides that one, which the Publisher received from his Parisian Correspondent, and which affords him the opportunity of giving this, though but Cursory, Account of it.

As to Physics, there occur chiefly two Questions, learnedly treated of in this Volume; though not without some heat between M. Des-Cartes and M. Roberval. The one is, touching the Vibrations of Bodies suspended in the Air, and their Center of Agitation: about which, there is also a Letter inserted of M. Des-Cartes.
M. Des-Cartes to that late Noble and Learned English Knight, Sir Charles Cavendish. The other is, whether Motion can be made without supposing a Vacuum: where 'tis represented, That, if one comprehend well the Nature, ascribed to the Materia Subtilis, and how Motions, called Circular, are made, which need not be just Ovals or true Circles, but are only called Circular, in regard that their Motion ends, where it had begun, whatever irregularity there be in the Middle; and also, that all the Inequalities, that may be in the Magnitude or Figure of the parts, may be compensated by other inequalities, met with in their Swiftness, and by the facility, with which the parts of the Subtle Matter, or of the first Cartesian Element, which are found everywhere, happen to be divided, or to accommodate their Figure to the Space, they are to fill up: If these things be well understood and considered, that then no difficulty can remain touching the Motion of the parts of Matter in pleno.

Besides all these particulars, treated of in this Tome, there occur many pretty Questions concerning Numbers, the Cycloid, the manner of Working Glasses for Telescopes, the way of Weighing Air, and many other Curiosities, Mathematical and Physical.

I. ASTRONOMIA REFORMATA, Auctore.
JOHANNE BAPT. RICCIOLI, Soc.
Fesu.

For the Notice of this Book, and the Account of the Chief Heads contained therein, we are obliged to the Journal des Scavans, which informs us,

First, That the Design of this Work is, that, because several Astronomers, having had their several Hypotheses, there is found so great a diversity of opinions, that it is difficult whence to conclude any thing certain; this Author judged it also necessary, to compare together all the best Observations, and upon examination of what they have most certain in them, to reform upon that measure the Principles of Astronomy.

Secondly, That this Volume is divided into two Parts; where-
of the First is composed of Ten Books; in which the Author confi-
considers the principal Observations, hitherto made of the Motion of the Planets and the Fixed Stars, of their Magnitude, Figure, and other Accidents, drawing thence several Conclusions, in which he establishes his Hypothesis. The Second contains his Astronomical Tables, made according to the Hypotheses of the First Part, together with Instructions teaching the manner of using them.

Thirdly, That Astronomers will find in this Book many very remarkable things, concerning the Apparent Diameter of the Sun and the other Stars, the Motion of the Libration of the Moon, the Eclipses, Parallaxes, and Refractions: And that this Author shews, that there is a great difference between Optical and Astronomical Refraction, which Tycho and many others have confounded, undertaking to prove, that, whereas these Astronomers have believed, that the remoter any Star is, the less is its Refraction, on the contrary the Refraction is the greater, the more a Star is distant. And among many other things, he ingeniously explicates the two contrary Motions of the Sun, from East to West, and vice versa, by one only Motion upon a Spiral, turning about a Cone.

Fourthly, That he represents, How uneasy it is to establish sure Principles of this Science, by reason of the difficulties of making exact Observations. So, for example, in the Observation of the Equinox, every one is mistaken by so many Hours, as he is of Minutes, in the Elevation of the Pole, or the Diameter of the Sun, or the Refraction, or in any other circumstance. In the Observation of the Solstice, the error of one only Second causeth a mistake of an Hour and an half: mean time ’tis almost impossible to avoid the error of a Second; and even the sharpest sight will not be able to perceive it, except it be assisted with an Instrument of a prodigious bigness. For to mark Seconds, though Lines were drawn as subtil as the single threads of a Silk-worms Clew, (which are the smallest spaces to be discerned by the sharpest Eye) by the Calculation made by this Author there would need an Instrument of 48 feet Radius, since Experience shews, that there needs no more at most, than 3600 threads of Silk to cover the space of an inch. But, suppose one could have a Quadrant of this bigness, who can assure himself, that dividing it into
324,000 parts (for so many Seconds there are in 90 Degrees) either in placing it, or in observing, he shall not mistake the thickness of a single thread of Silk. He adds, that Great Instruments have their defects, as the small ones: For in those, that are Movable, if the thread, on which the Lead hangs, is any thing big, it cannot exactly mark Seconds; if it be very fine, it breaks, because of its great length, and the weight of the Lead: And in the Fixed ones, the greater the Diameter is, the less the Shadow or the Light is terminated; so that it is painful enough, exactly to discern the extremities thereof. Yet 'tis certain, that the greater the Instruments are, the furer Astronomers may be: Whence it is, that some Astronomers have made use of Obelisks of a vast bigness, to take the Altitudes; and Signior Cassini, after the example of Egnatio Dante, caused a hole to be made on the highest part of a Wall of 95 feet in a Church at Bononia, through which the beams of the Sun falling on the Floor, mark as exactly as is possible, the height of that Luminary.

Fifthly, That the Author reasons for the Immobility of the Earth after this manner. He supposes for certain, that the swiftness of the Motion of heavy bodies doth still increase in their descent; to confirm which principle, he affirms to have experimented, That, if you let fall a Ball into one of the Scales of a Ballance, according to the proportion of the height, it falls from it raiseth different weights in the other Scale. For example, A Wooden Ball, of 1 1/2 ounce, falling from a height of 35 inches, raiseth a weight of 5 ounces, from the height of 140 inches, a weight of 20 ounces; from that of 315 inches, one of 45 ounces; and from another of 560 inches, one of 80 ounces; &c. From this principle he concludes the Earth to be at Rest; for, saith he, if it should have a Diurnal Motion upon its Center, Heavy Bodies being carried along with it by its motion, would in descending describe a Curve Line, and, as he shews by a Calculus, made by him, run equal spaces in equal times; whence it follows, that the Celerity of their Motion would not increase in descending, and that consequently their stroke would not be stronger, after they had fallen thro' a longer space.
III. ANATOME MEDULLAE SPINALIS, ET NERVORUM inde provenientium, GERARDI BLASII, M. D.

The Author shews in this little Tract a way of taking the entire Medulla Spinalis, or Marrow of the Back, out of its Theca or Bony Receptacle, without Laceration; which else happens frequently, both of the Nerves proceeding from it, and of the Coats investing it, not to name other parts of the same. This he affirms to have been put into practice by himself, by a fine Saw and Wedge; which are to be dexterously used: and he produceth accordingly in excellent Cuts, the Representations of the Structure of the said Medulla thus taken out, and the Nerves, thence proceeding; and that of several Animals, Dogs, Swine, Sheep.

He intermixes several Observations, touching the Singleness of this Medulla, against Lindanus and others; its Original, vid. Whether it be the Root of the Brain, or the Brain the Root of it: its difference of Softness and Hardness in several Animals; where he notes, that in Swine it is much softer than in Dogs, &c.

He exhibits also the Arteries, Nerves, and Veins, dispersed through this Medulla, and inquires, Whether the Nerves proceed from the Medulla itself, or its Meninx; and discourses also of the Principle and Distribution of the Nerves; referring for ampler information in this and the other particulars, to that Excellent Book of the Learned Dr. Willis, De Anatome Cerebri.
Advertisement.

It was thought fit to publish here the following Advertisement of John Evelyn Esquire, and that as himself proposed it. Viz.

Being much solicited by many worthy Persons, to publish a Second Edition of my Discourse and Directions concerning Timber, &c. which was printed at the Command and by the Encouragement of the R. Society, I do humbly request, that if any Person have any Material Additions or Reformations, which he thinks necessary either to the Part, which concerns the Improvement of Forrest-Trees, or that of Cider, he would be pleased to communicate his Notes and Directions to Mr. H. Oldenburgh, one of the Secretaries of the said Society, at his House in the Pahmal of St. James's Fields Westminster, with what speed they conveniently can, before our Lady-day next, to be inserted into this intended Edition.

Note.

What was observed, Numb. 20. p. 364. L. 18. of the Number of Vegetables, (vid. That they are about 410,) found in England, and catalogued by Dr. Merret in his Pinax, &c. is to be understood only of the different Kinds of Plants, not of the several sorts of several Plants; for, these being comprised, the Number will amount to about 1400.
THE PHILOSOPHICAL TRANSACTIONS OF Two Years, 1665 and 1666, beginning March 6, 1665, and ending with February 1666; abbreviated in an ALPHABETICAL TABLE:

And also afterwards Digested into a more NATURAL METHOD.

In the TABLE, the first Figure signifies the Number of the Tracts: the second, the Page, as it is remarked in the same:

A: Grieculture, Heads of Inquiries concerning it, num. 5. pag. 91.

Air. The weight of it in all changes, by wind, weather, or whatever other influence observable by a standing Mercurial Balance, call'd a Baroscope, hinted in reference to M. Hooks Micrography, n. 2. p. 31. applied to particulars by Dr. Beale, 9. 153. with additions, 10. 163. described with observables relating to an Earth-quake about Oxford by Dr. Wallis, 10. 167. Mr. Boyle's remarks on the same, 11. 181. The Wheel-Baroscope improved and delineated by M. Hook, 13. 218. Another Balance of the Air contriv'd by M. Boyle, and call'd Statical, by which the former may be exactly stated and examin'd for many particular applications, 14. 231.

Anatomy, see Flesh, Blood, Animals, Lungs, Petrifaction, Taste; item, Stego, Graeffi, Bellinus, Redi, in the List of Books.

Animals; one may live by the blood of another, the whole mass of his own blood being drawn out, and the blood of another infused in the mean time, 20. 353. See Bloods

Transfusion. The Generation and Functions of Animals deduced by Mechanical principles, without recourse to a substantial form, 18. 325. See Honor, Fabri. & n. 20. p. 365. See also Guarini.

Artificial Instruments or Engines. To weigh Air, see Baroscope, or rather Air. To discern drought or moistue of the Air, see Hygroscope. n. 2. p. 31. applicable in the observation of Tydes, 17. 300. Thermometers, to measure degrees of heat and cold, 2. 31. described, 10. 166. applied in the examination of Tydes, 17.300. An Instrument for graduating Thermometers, to make them Standards of heat and cold, 2. 31. A new Engine for grinding any Optick Glasses of a Spherical figure, 2. 31. To measure the Refractions of Liquors of all kinds; for establishing the Laws of Refraction, 2. 32. To break the hardest Rocks in Mines, 5. 82. To try for fresh waters at the bottom of the Seas, 9. 147. To find the greatest depths in the Sea, 9. 147. The Engin for fetching up fresh water defended by Explication, 13. 228. Huge Wheels, and other Engins for Mines, 2. 23. By the fall of water to blow wind, as with Bellows, 2. 25.
Astronomical Remarks of a New Star seen by Hevelius in Ptol. Cigini, which he supposed to be the same, which Kepler saw A. 1661, and continued until 1662, and was not seen again till 1662, and then almost always hiding itself till 24. Nov. 1666. That, seen by Kepler was of the third magnitude; this now, of the sixth or seventh. Q. Whether it changes place and magnitude, 19. 349. The Scheme, 21. 374. A New Star in Collo Ceti, observed from 1638, to 1664, 1665, 1666, with its vicissitudes and periods, and causes of change, observed by Bullialdas, who conceives the bigger part of that round body to be obscure, and the whole to turn about its own center, 21. 382. Another New Star called Nebulosa in Cingulo Andromedae, seen when the Comet appeared in 1665, observed by the said Bullialdas to appear and disappear by turns, ibid. 383. A method for observing the Eclipses of the Moon, free from the common Inconveniences, by Mr. Rooke, 22. 387.

B

Aroscope. See Air and Artificial Instruments.

Blood. The new Operation of Transfusing blood into the veins, out of one Animal into another, with considerations upon it, 20. 353. The first Rise of this Invention, 7. 208. The Success, 19. 352. Proposals and Queries, for the improvement of this Experiment, by Mr. Boyle, 22. 385, 386. Little Blood-letting in China, 14. 149. Blood found in some mens veins like Milk, or of the colour of Milk, 6. 100. again p. 117, 118, and again 8. 139. A Bottle in Hungary good as Bole Armenick, 1. 11.

The Bononian Stone, see Light or Stone, 21. 375.

Books abbreviated, or recited:


Mr. Boyle of Thermometers and History of Cold, abbrev. 1. 8. more 3. 46.

— His Hydrostatical Paradoxes abbrev. 8. 143., more largely 10. 173.
— His Origin of Forms and Qualities, 8. 145, abbreviated 11. 191.

Monseur de Bourges his Relation of the Bishop of Evesc his Voyages in Turky, Persia, India, abbrev. 18. 324.

Bullialdas Monita duo, abbrev. 21. 381. See supra Astronomy.

Des Cartes his Third Volume of Letters, n. 22.

De la Chambre’s Causes of the inundation of the Nile, abbrev. 14. 251.

Condemny of the difference of Bodies and Souls, or Spirits, and their operation upon one another, abbrev. 17. 306.

Euclidis Elementa Geometrica novo ordine demonstrata, 15. 261.

Hon. Fabri Soc. Jef. Trad. duo 1. de Plantis & Gener. Animalium, 2. de Homine; abbrev. 18. 325.

Felibien of the most excellent Paintings, 21. 383.

Catalogue of Fermats Writings, and his character, i. 15.

De Graeff, de succi Pancreatici natura & usu, abbrev. 10. 178.

Guanini Placita Philosophica, abbreviated, 20. 365.

Hevelius’s Prodromus Cometicus, abbrev. 6. 104. His Descriptio Cometica cum Manifes, abbrev. 17. 301.


Hook’s Micrographical and Telescopical Observations, Philosophical Instruments and Inventions, abbr. 2. 29.

Kircher’s Mundus Subterraneus, abbrev. 6. 109.

Lower’s Vindication of Dr. Willis de Febri- bus, 4. 77.

Mert’s Pinnax Rerum Naturalium Britannic- carum, continens Vegetabilia, Animalia & Fossilia, in hac insula reperta, inchoatums; abbrev. 20. 364.

Parker’s Tentamina Phylici Theologica, abbrev. 18. 324.

Redi an Italian Philosophers, of Vipers, abbrev. 9. 160.

Ricciolel’s Altronomia Reformata, Volumen quantum abbrev. n. 22.


Stennis
C.

In China very ancient Books found of the nature and vertues of Herbs, Trees and Stones, 14. 249.

The Root there called Genfeng, very restorative and cordial, recovering agonizing persons, fold there each pound for three pounds of silver, 14. 249.

China Dishes how made there, ibid.

A way found in Europe to make China-Dishes, 7. 227.

Chymists in China pretend to make Gold, and promise Immortality, 14. 249.

Cold, see M. Boyle's History, abbr. More Inquiries, and some answers touching Cold, 19. 344. How Cold may be produced in hottest Summers by Sal Armoniack, discovered by M. Boyle, 15. 255. Some suggestions for remedies against Cold, by D. Beale, 21. 379.

Comets. The motions of the Comet of December 1664. predicted, 1. 3. Cassini concurs; 2. 17. Aumont, who first predicted the motion, reflects upon Cassini, 2. 18. and predicts the motions of the second Comet of March, April 1669, n. 3. 36.

Controversies and Discourses, some at large, concerning Comets, n. 1. p. 32. n. 2. p. 175. 18. n. 3. p. 36. n. 6. p. 104. n. 9. p. 150.


D.

Directions for Seamen bound for far Voyages, by M. Rook, 8. 140. Mr. Boyles Inquiries, 18. 315.

Philosophical Directions or Inquiries for such as Travel into Turkey, 20. 360.

Directions or general Heads for a natural History of a Countrey, by M. Boyle, 11. 186.

Directions or Inquiries concerning Mines, by the fame, 19. 330.

Diamonds where, and how the fairest are discovered, 18. 327.

E.

The Earthquake about Oxford, Anno 1665. described by D. Wallis, 10. 181. by M. Boyle, 11. 179. noting the Concomitants thereof by Baroscope and Thermometer.

The Earth's Diurnal motion prov'd by the motion of the Comets, 1. 6. & 7. especially by the low motion of the second Comet, 3. 39. See M. Aupont, confirm'd by M. Hefulus, 6. 105. confirm'd also by the Tydes at Sea, 16. 265.


Elephants: How to elcape, or to combat with them, 18. 328.

Eeles discovered under Banks in Har-Frosts, by the Greens of the Banks approaching, 18. 383.

F.

The Flethy parts of the Body which are usually reputed, and do seem void of Vessels, are argued to be full of Vessels, by D. King, 18. 316.

Friction and sometimes Touch, how fatal, by several Examples, 12. 206.

Frictions much used by Physicians in China with good success, 14. 249.
G


I

Inquiries, see Directions, supra.
Ice and Snow how to be preserv'd in Chaffe, and how Snow-houses are made in Livorn, 8. 139.
Insects, in swarms pernicious in some Countries; the cause of them; and what Remedies, 8. 139. some Insects, commonly believed pylonous, not so, by M. Fairfax, n. 22.
To find the Julian period by a new and easie way, 18. 324.

K

Kerma, how gather'd and used for Colouration; describ'd with many considerables, 20. 362.

L

Light, to examine what figure or celerity of motion begetteth or increaseth Light or Flame in some Bodies, by D. Beale, 13. p. 226. Shining Worms found in Oysters, 12. 203. The Bononian Stone duly prepar'd continues light once imbibed above any other substance yet known amongst us, 21. 375. The loss of the way of preparing the same for shining, fear'd, ibid.
Longitudes at Sea, how to be ascertain'd by Pendulum-Watches, 1. 13.
Lungs and Windpipes in Sheep and Oxen strangely stopp with Hand-Balls of Glass, 6. 100.

M

Marbles, that a liquor may be made to colour them, piercing into them, 7. 325.

Mar's, by what steps and degrees of diligence discover'd to be turbinated, both in England and Italy. Compare n. 10. p. 158. and n. 14. 230. 242. see the Schemes there.
May-dew examin'd by various Experiments, by M. Henham, 3. 33.
Mechanical Principles in a Geometrical method, explicating the nature or operation of Plants, Animals, 8. 325.
Medecins in China conflict for the most part of Simples. Decoctions, Cauterias, Frictions, without the use of Blood-lettings, 14. 249.
The Physicists there, commended for speedy Cures, and easie, ibid.
Mediterranean Sea, whether it may be join'd with the Ocean, debated, 3. 41.
Micrography epitomiz'd, 2. 27. M. Aucoute's Objections to a part of it; vid. the new way of grinding Spherical Glasses by a Turn-lathe, 4. 57. M. Hook's answer thereto, 4. 64. both at large.
Mercury-Mines in Friuli, and the way of getting it out of the earth, 2. 21.
Mineral Inquiries, see Directions, Engines, Artificial Instruments. Mineral at Liege yielding Brimstone and Vitriol; and the way of extracting them, 3. 35. How Adits and Mines are wrought at Liege, 5. 79.
A Stone in Sweden yielding Sulphur, Vitriol, Alum and Minium, and how, 21. 375.
See Kircher's Mundus Subterraneus abbr. 6. 109.
Monsters, a Calf deform'd, and a great stone found in a Cows-womb, n. 1. 102; a Cote with a double eye in one place, 583.
Mulberry-Trees how to be cut low, and easie to be reach'd, for relief of Silk-worms, in China, 14. 249. in Virginia, 12. 202. see Silk.

N

Nile's Inundations, the cause attributed to Niter, by Dela Chambre's opposed by Voffius. See both in the Lift of Books, 14. 251, and 17. 304.
The North-Countries of Poland, Sweden, Denmark, &c. are warm'd by the influence of the Royal Society, 19. 344.
Ocean, what Seas may be joined with it, 3. 41.

Opticks, Campani's Glass do excell Divini's; this ease they to distinguish people at four Leagues distance, 2. 131, and 12. 209. What they discover in Jupiter and Saturn, 1. 1, and 2. The proportions of Apertures in Perspectives reduced to Table by M. Auzout, 4. 55. Animadverted upon by M. Hoek, 4. 69.

How to illuminate Objects to whatsoever proportion, proposed by M. Auzout, 4. 75.

Hevelius, Hugenius, and some in England, endeavour to improve Optick Glasses, 6. 98.

Seigneur Buratti.ii's advance in the same inquired after, 19. 348. Some answer to it from Parle, 21. 347.

Divini makes good Optick Glasses of Rock-cryftal, that bad veins (if he mistook not somewhat else for veins) 20. 362.

To measure the distances of Objects on earth by a Telescope, undertaken by M. Auzout, and others of the Royal Society, 7. 123.

How a Telescope of a few inches in Diameter may draw some hundreds of feet, 7. 127.

How a Glass of a small convex-sphere may be made to reflect the Rays of Light to a Focus at a far greater distance than is usual, 12. 202.

Parley, to make it shoot out of the ground in a few hours, see Hon. Fabri 18. 325.

Pictures, a curious way in Trace of making lively Pictures in Wax, and Maps in a low reliefe, 6. 99.

The cause why Pictures seem to look upon all Beholders, on which side forever they place themselves, 18. 326.

Ancient Paintings compar'd with the Modern, and a judgment of the Paintings in several Ages, their perfections, and defects, see M. Felibien, 21. 383.

Petrification, in the wombs of Women, 18. 320. in a Calf in the Cow's womb, 11. 10. Stones found in the heart of the Earl of Belcaris, 5. 86. Part of an Elm by incision, or otherwise, petrified a foot above the root and ground, 19. 329. Wool petrified in a sandy ground in England; and of a Stone like a Bone or Osteocolla, 6. 101.

A Stone of excellent vertues found in the head of a Serpent in the Indies, 6. 102. The causes of Petrification inquired, 16. 320.

Planets, see Jupiter, Mars, Saturn, Sun, Moon, which are tubininated, and which not, 8. 143. To find the true distances of the Sun and Moon from the earth, 9. 191.

Physicians of China commended, see Medicins.

Preservation, to preserve small Birds taken out of the shell, or other Beasts for discoveries, 12. 198.

Pulses of the Sick how diligently, and to what good purposes observ'd in China, 14. 249.

R

Rainbows strangely posited, 13. 219.

Raining of Ashes, and how, 21. 377.

Rice, prospers best in watery places, see Marshes, 18. 328.

S

Alamander, how it extinguishes fire, and feeds by licking Indian earth, 21. 377.

Salt by excessive use stiffens, and destroys the body, 8. 138.

Salt-Springs, see Springs.

Salt-Peeter how made in the Mogols Dominions, 6. 103.

The proportion of Salt in best Salt-Springs; and what grounds or signs of best Salt, 8. 136.

Sea-fluxes, the cause proposed by way of a new Theory; by Dr. Wallis, 16. 263. See Tydes.

Seas, whether they may be united, 3. 41.

Silk-Worms and Silk-Trade solicited, 5. 87.

Sndes, how they differ from Vipers, 8. 158.

Rattle--Snakes, how sometimes kill'd in Virginia, 3. 43.

Snow-boufes dreece, and how to preserve Ice and Snow in Chaff, 8. 139.

Springs, of peculiar note, n. 7. 127, n. 8. I 33, 133, and 136. n. 18. 323.
Thee, in China and what; and how exchanged there for dried leaves of Sage by the Dutch, 14. 249.

W.

W. Hale-fishing about Bermudas and New-England, how it is performed, n. 1. 11. n. 8. 132.

Winds, how to be raised by the fall of water, without any Bellows, 2. 25. shewed in a draught.

Worms, that eat holes in stones; feeding on stone, 18. 321.
The more

**NATURAL METHOD.**

1. A Natural History of all Countries and Places, is the foundation for Solid Philosophy. See Directions, Inquiries, and Instructions for a Natural History of a Country, p. 11, p. 186.


See the cause of Tides proposed by D. Wall, 16. 263.

See the further Examination by a severe History of Tides, Winds, and other Concomitants or Adherents, directed, n. 17, n. 18, n. 21.

See the Inquiries concerning the Seas, and Sea-waters, n. 18. 315.

See Directions for Seamen bound for far Voyages, 8. 140.


Mr. Boyle's Directions and Inquiries touching Mines, 19. 330.

Philosophical Directions and Inquiries for such Travel into Turkey, n. 20. 360.

The Relation of M. de Bourges, 18. 324.

M. Thevenots Relation of divers curious Voyages, &c. more particularly of China, 14. 248.

The causes of the inundation of the Nile disputed by Dela Chambre and Vossius. In the List of Books.

See Mr. Boyle's Mechanical Deductions, and Chemical Demonstrations of the Origin of Forms and Qualities, 11. 191.

See the Application of these Mechanical Principles more particularly to the Nature, Operation, and Generation of Plants and Animals, and to our humane Textures, in a Geometrical method, by Hon. Fabri, 18. 325.

See Mr. Boyle's History of Cold and Thermometers, n. 1. p. 8, n. 3. p. 46.

The History of Winds and Weather, and all changes of the Air (especially in relation to the weight) observable by the Baroscope, n. 9, n. 10; n. 11.

Light, some special search into the causes, and some peculiar Examples, See above in Light.

Petrification solicited, see Petrification, Stone.

The Earth's Diurnal Rotation, see Earth, supra.

Adventurous Essays in Natural Philosophy, see Gravini, 20. 365.

Earthquakes, and their Concomitants observed, n. 10; n. 11.


The raining of Ashes and Sand at great distance from the Mount Vesuvius, see Rain, 21. 377.

Springs, and Waters of peculiar Note, see Springs.

Insects in Swarms; how begotten & pernicious, and how destroyed, 8. 137.

Monsters, or Irregularities in Nature. The Calf, Calf, supra.

Four Suns at once, and two Strange Rainbows, 13. 219.

See the flatistical position and tendency or gravitation of Liquids, in Mr. Boyle's Hydrostatical Paradoxes, 8. 145.

See in M. Hook's Micrography, a History of minute Bodies, or rather of the minute and heretofore unseen parts of Bodies; it being a main part of Philosophy, by an artificial reduction of all gross parts of Nature to a closer inspection.

Medicinals, see Medicine. Physitians, Chiral Friction, Dr. Sydenham. Dr. Lower, Friction, supra. n. 4. 77. n. 12. 206.

Anatomy, see Steno de Musculus et Glandulis. How a juice in the stomach dissolves the shells of Grasthies, ibid.

Gracif de Succo Pancreatico; that flesh hath Vellies, n. 18. 316. Blood degenerated to resemble milk, n. 6. 117. The Translation of
Salt too much stiffens and destroys the Body, 8. 138.

II. Singularities of Nature severely examined.
The ordering of Kermes for Color, n. 20. 362.
How the Salamander quenches Fire, and lives by licking the Earth, n. 21. 377.
Whether the Hungarian Bolus like the Armonus? 1. 11.
Rattle-Snakes how kill'd in Virginia, 3. 43.
Snakes and Vipers how they differ, see Snakes above.
The Qualities and Productions of May-dew, 3. 71.
Damps in Mines how they kill, 3. 44.
Teeth growing in aged persons, 21. 380.
Steam and Expirations of the Body how stopp'd; and the stoppage dangerous or mortal, 8. 138.
Shining Worms in Oysters, 12. 203.

III. Aids, or Aids for the discovery or use of things Natural.
See Artificial Instruments in the Table.
Agriculture, see the Inquiries, 5. 91.
English Vineyards vindicated, see in the Catalogue of Books.
Geometry, see Euclid methodized for Facility, Fermat in the Catalogue of Books.
Astronomy, see Astronomical Remarks. Bully, Hevelius, Comets, Planets, Saturn, Jupiter, Mars, Sun, Moon, Eclipses.
Opticks, see that Head in the Table.

Picture, see that Head in P. and Felibien in the Catalogue of Books.
How to paint Marbles within, see the Head Marble.
Pendulums, Watches to ascertained Longitudes at Sea, 1. 13.
Whale-fishing about Bermuda, 1. 11. and 8. 132.
Silk-trade solicited in France, Virginia, see Silk in the Table.
Elephants enraged, how to escape or subdue, 18. 328.
Seas and vast waters, whether they may be united to the main Ocean, 3. 41.
To proportion the distance necessary to burn Bodies by the Sun, and shewing, why the Reflections from the Moon and other Planets do not burn, 4. 69.
The Art of making Salt-Peeter, as practised in the Mogols Dominions, 6. 103.
To make China-Dishes, 14. 249, expected from Seigneur Sepiallo to be made in Europe, 7. 127.
To convey blood of one Animal, or other Liquors, into the blood of another Animal, 20. 353.
To preserve Ice and Snow by Chaffes, 8. 138.
To preserve Ships from being Worm-eaten, 11. 190.
To preserve Birds taken out of the Eggs, or other small Creatures, for Anatomical, or other Discoveries, 12. 199.
To allay the heat in hottest Summers, for Diet or Delight, 15. 255.
Remedies against extreme Cold suggested, 21. 379.
Trees of Oak, as black as Ebony discovered, and taken up out of Moors and Marshes in draughty weather, 11. 323.

Note,

That though in this last Head there is repeated the Transfusion of Blood, because the Operation is an Art requiring diligence, and a practised hand to perform it for all advantageous Discoveries, and so to be distinguish'd from the Anatomical Account, yet that there is not affected noise and number, may well appear by reviewing and comparing the particulars of Artificial Instruments in the Table,
Table, where sometimes one Engin or Instrument may minister Aid to discover a large branch of Philosophy, as the Baroscope, an optick Glass, &c.

And very particularly M. Rook's directions for Seamen, which specifies Instruments, may hereunto belong.

And sometimes in one of the Discourses herein mention'd, and abbreviated, there are almost as many Artificial Inventions, as Experiments, as in Mr. Boyle's Hydrostatical Experiments: Besides all the Chymical Operations, recited in the Treatise of the Origine of Forms, &c.

ευρέω τῷ μεγάλῳ τῷ ἐν, καὶ τῷ ἐν τῷ μέγα.

ERRATA.

Pag. 392. Lin. 23. blot out, as ibid. Lin. 24. read of, the Soul.

FINIS.
In the SAVOY,
Printed by T. N. for John Martyn, and James Allestry, Printers to the Royal Society: And are to be sold at their Shop without Temple-Bar, and in Duck-lane, 1667.
PHILOSOPHICAL Transactions: giving some accompt of the present undertakings, studies, and labours of the ingenious in many considerable parts of the world.

Vol. II.
For Anno 1667.

In the Savoy.
Printed by T. N. for John Martyn at the Bell, a little without Temple-bar, Printer to the Royal Society.
Translators Account

According to the
Instructions
of Lord
Wolfe
To the Right Honourable

William Lord Viscount Brouncker,

Chancellor to Her Majesty, and

President to the Royal Society, &c.

MY LORD,

After I had dedicated the First Volume of these Philosophical Occurrences to the Royal Society, to whose service I have dedicated myself, I thought it my next duty to present the Second to your Lordship, who have for so many years with so high and universal an Applause presided in that Illustrious Assembly, and there given full proof both of the vast extent of your knowledge, and the incomparable solidity of your judgment in all the various Arguments and Matters there produced, observed, experimented and discoursed of. This, my Lord, though it deserves a far better Pen to be proclaimed to the world, than mine; yet did I think, I might be suffer'd in this crowd to cast in my voice, and to deliver the truth and my persuasion thereof in these plain expressions. To which I shall add no more but my humble acknowledgments for your Lordships particular favour and goodness, in condescending on all occasions, to encourage these (though rude and undigested) Communications, and thereby to fortifie (against the obloqies of some singular men) the endeavours of the Author for the improving and enlarging his Philosophical Commerce; which, being done, may perhaps be a means to render these Papers less inconsiderable for the future.

I am,

My Lord,

Your Lordships

LONDON,

March 2, 1667

Very humble, and very much obliged Servant,

Henry Oldenburg.

AN INDEX
FOR THE
PHILOSOPHICAL TRANSACTIONS.
OF
An. 1667, beginning with Number 23, and ending with Numb. 32.

The first Letter (n) signifies the Number, the second (p) the Page.

A.

Ir, being exhausted, Vegetables do not prosper or grow, p. 23. p. 24.

what effects Air exhausted, and re-admitted hath on Light, in the shining bodies of rotten Wood or Fish, n. 32. p. 581, &c. and what upon Burning Coals, n. 32. p. 605, &c.

Ale, how it may be transported from England to East- or West-Indies, n. 27. p. 495.

Amber, n. 23. p. 430, &c.

Ambergreefe, n. 23. p. 431, &c. n. 28.

Analysis; the subtle Art of Analysing: Geometrically opened. See infra in Books du Laurens.


Animals of strange kinds in China, Wild-
An Index for the Philosophical Transactions.

frether or falter) from the bottom of the Sea. See the whole numb. 24. To measure the Diameters of Planets most exactly, n. 25. p. 457. described, and in effigie, n. 27. p. 541.

Asbestus, a Fossil in China to be drawn and spun, n. 26. p. 486.

Aphi in an excellent Map. See Kircher in Books.


B. Arks of Trees, being cut off, are made by Art to reunites, n. 25. p. 452.


A Bell in China weighing 1000 pounds; whereas the Bell of Erfurtd in Germany, supposd hitherto to be one of the biggest in the world, weighs but 25400 pounds, n. 26. p. 487.

Bermudas hath no Sand, Flint, Pebles, or Stones that are hard enough to sharpen Knives, or grinde Glasses: Wells diggd there above the Surface of the Sea, yield fresh Water; diggd lower, they yield Salt-water or brackish. See more, and of other matters, n. 30. p. 585.

Berggarten, See Champike in Books.


Books abridged.


Mr. Chappuzeau's History of the riches of the Orient and Occident: Of Diamonds, Rubies, Emeauds, Pearls, Coral, Bezoary, yellow Amber, Amber-gris, Indigo, &c. n. 23. p. 429.


Honor. Fabri synopsis optica, n. 32. p. 626.

Nouveaux Elements de Geometrie, n. 32. p. 625.


Kloebii Historia of Amber-gris, n. 28. p. 538.


Dr. Guat. Needham de Furo formato, n. 27. p. 509.


Th. Willisi Pathologicæ Cerebrî & Nervosi genera specimen, n. 31. p. 600.

Brain
Epiiepfy. See Kircher in Books.

Braia anatomised by Signeur Malpighi n. 27. p. 419. How the Brain and Nerves do caufe Convulsions, Cramps, Epilepsy, Hysterical diseases, Scorbut, feboratical Gouts, Consumptions, Dropfies, Feavers, and Epidemical diseases in England and Germany, and the proper Remedies. See Dr. Willis in Books.


C.

Alamba-wood in China, a kind of Lenticum, or rich Terebinth, worthy to be transplanted into Europe, &c. n. 26. p. 485.

Canes in China so big that a Barrel may be made at every knot, n. 26. p. 486.

Carmelians highly applauded, but defective in not expatiating the efficient power, with the Matter. See Cornelii in Books.

A Chafel in China very huge. See Kircher in Books.


Cider made excellent by the mixture of the Juice of Mulberries, n. 27. p. 503.

Cold drink may suddenly kill one that is accustomed to hot or warm drink, n. 29. p. 550. Shining Wood and Luminous Fishes rather Cold than warm as to us. See Mr. Boyles experiment by a curious Thermometer, n. 32. p. 611.

Coral where to be found. See Chapuzseau in Books.

Cormorants or Pelicans being put two hours under ground, lofe their Fiſh-like taste, n. 27. p. 501.


D.

Eaſt and dumb how they may learn speech. See Alphab. Nature in Books. Diamonds how to be found. See Chapuzseau in Books.

A Dog made to draw his breath like a wind-broken Horse, n. 29. p. 544.

E.


Eaths that are Cosmetick. See Kircher in Books.

Elephants eating Sugar-canies which take roots in their stomacks. Quarre in Kircher among Books.

Engines. See Artificial Instruments.

Emerauds. See Chapuzseau in Books.

F.

Ferments, their Caufe. See Travagini in Books.

Fig-trees bearing leaves big enough to wrap a Man in, n. 26. p. 486.

Fishes of strange kinds. Some that fly on land to seek their food in Summer, and in Autumn return to the waters. See Kircher in Books.

Flames or Flashes from the Sea; how great, and when, and where greatest, n. 27. p. 497.


Foms call'd subordinate, submitted to Mechanical Principles. See Mr. Boyle and Monsieur Steno, in Books.

Fossils of strange kinds in China. See Kircher in Books.

G.

Generation explicated. See Cornelii and Travagini in Books.

Geometry explicated in New Methods. See Dutaurens and Nouveaux Elements in Books.


Granaries of what kind in several places, n. 25. p. 464.

Gravitation consider'd, n. 32. p. 637.

Gunery how to be improv'd, or the Point-blank-force discover'd for all shapes, n. 26. p. 477.

Gun-powder invented in China long ago, according to Kircher. See Books.

H.


An Hermaphrodit curiously described in all changes of Nature, shape and affectious, from infancy to adult age, n. 32. p. 624.

Horses eyes apt to be defective by a spungy excelleſſe not hitherto obſerved, and the remedy hinted, n. 32. p. 613.
A Preface to the Third Year of these Transactions, which is begun with this. An Account of the Enlargements of Philosophical Correspondencies; together with an Invitation to contribute Inquiries and Directions. Inquiries for several parts of the World, as Suratte, and the East-Indies in general; for Persia, the West-Indies, and there particularly, for Virginia, Bermudas, Guaiana, Brasil. Those for other Countries referred to another opportunity. Of a considerable Load-stone, digged in Devonshire, weighing 60. pounds. Some Observables about Load-stones, and Sea-Compasses. Proposals to try the effects of the Rarifying Engine exhausted, on Plants, Seeds, Eggs of Silk-worms. An Experiment proposed of Grafting Pears upon Spina Cervina (Buckthorn.) Observations concerning Emmets, their Eggs, Production, Progress, Coming to Maturity, and Use. Account of a Book in French, intituled HISTOIRE DES JOY-AUX.

A PREFACE

To the Third Year of these Tracts.

Having, by Gods Assistance, finish'd my solicitations for the Philosophical Transactions of the two last years, I crave leave to reflect a little upon what hath past.

I think, I may safely assume, that in these Fragments, something hath been contributed to fowe such seeds, as may somewhat conduce to the illustration and improvement of Philosophy, and of all Laudable and Useful Arts and Practices. And
I hope, our Ingenious Correspondents have examin'd all circumstances of their communicated Relations, with all the care and diligence necessary to be used in such Collections; not taking up old Fame, or flying Reports, upon too easy trust; nor straining for other Kinds of Wonders, than the most wise Author of Nature hath allowed, but attending closely to the strict measures of Natural Truth, and to the useful Contrivances of Art. For some evidence whereof, I refer the Reader to the particulars, indicated in the Tables annexed to the Treat of February, lately past.

And because well advised Histories of Natural Productions, and of Artificial Contrivances, are necessary to beget sound knowledge, and to excite profitable Inventions, some have furnish'd us with accurate Instructions, under the modest Title of Inquiries and Propositions: And further Disquisitions are by the hands of many Accomplish'd Persons severally, and often with joint endeavours in a manner universally prosecuted.

Neither have we discouraged or refused the Effays of some famous Philosophers, learned Philologers and Antiquaries; whose Disquisitions, Readings, and Reasonings, have extended farther than their Experiences; since by such bold Excursions and Sallies many valuable Truths may be started out of their recesses. Architects do require some variety and store of Materials for the further satisfaction of their Judgment in the Choice: And the Sculptor must pare off somewhat of his richest Marbles, Onixes, Diamonds, &c., before he can perfect the Portraiture. Such liberty an exact Philosopher must claim in his Extracts from Men of much Learning.

In Medicinals we have now and then occasionally inquired after some rarities, medical applications and experiences; what the uses and performances are by Phlebotomy, Frictions, Simples or Compounds not ordinary, by Diet or Chymical Operations in some of the remotest parts of the World, particularly in the famous China. Neither have we altogether omitted to commemorate those obvious reliefs, which the Divine Bounty has offered freely and in common, for distressed Mortals, by Springs, Baths, Bolus's, Medicated Earths, &c. And we have had a
due care to erect a Phareus for a Caution against undiscern'd dangers.

By Anatomy we have sometimes enter'd into the Chambers and Cabinets of Animal Functions, to find many Meanders and changeable Varieties, and the immediate Organs and Conduits of Life and Sensation.

As for the Growth of Arts and Inventions, I think, it may justly be said. That these our Entries sometimes assist and promote their Improvements. And the same will hereafter remain faithful Records to shew, By what steps and degrees, and by what Effays, Emulations, and Encouragements these Noble Arts advanced to perfection. And a punctual information of these Gradual Processes, may be instructive to promote other Inventions. And the Wise will consider it, at what easier rates they obtained Monthly Advices of the Designs and Successes of Industrious and Eminent Persons, and by the same means came to know as much, as was purchased at their great charges and assiduous labour. Of which Arts as they are now improved, and still improving, I presume I need not spare to say, That they would have obliged an Alexander, or a Solomon, and I must avouch with confidence; That they would have raised Acclamations, Applauses, and Admiration of most, and have provoked them to refund full Rivers of Treasures in Just Rewards, and extraordinary Achievements.

Neither is it much amiss, that there are yet some, who do prefer the darkness of old Heathenism before this Noon-light. Otherwise, the next Age might hardly believe, that Men pretending to Wit, Prudence, and Learning, would ever make such strange Oppositions against their own great Emolument and Accommodations: And so the Vertuous might be deprived of a fair beam of the future Glory, due to their Memories for their unchangeable Resolutions, as unconcern'd in scoffing Discourses, and standing firm as Rocks against the dashes of foaming Disputants.

And truly, they do much oblige us, in that they are pleased by their frets, and eager contentions, and by their fruitless and obstreperous Verbosity, to make themselves a foil, to set off the Serene Lustre of the real and obliging performances of the Experimental Philosophers.
And yet (in short to convince and reclaim as many as are hopeful) I dare, without leave, but with sincere affectations, in behalf of the Learned Virtuosi, undertake to join issue with them; and to offer fair proof, That, whereas they pretend to Aristotle as their Grand Oracle, we have a true and higher esteem for his true worth, than these Pretenders do effectually manifest.

We say, his Logicks and Rhetoricks are very valuable. His Ethicks and Politicks, for the most part, found. His Metaphysics in many Notions acute. But all these are generally overwhelmed and degraded by the swarms of Insectile Systems and dilute Commentaries.

And as for the other more useful Volumes of Aristotle, his Treats of Animals (which did cost Great Alexander so many Talents for the furniture, and an ample Salary for encouragements) his Mathematical Discourses, and Mechanics, these they never salute. They weed out his onely defects and animosities, his Veditations with his Elders and Compeers about Atomes and darker Principles; a Matter, which is neque quid, neque quantum, neque quale, a Formal and Substantial enteleχeа (a word too hard for Cicero to translate) and Privation, a Principle as good as the rest; his Definitions of Causes and Affections; his Quaternion of grosse Elements and groffer Mixtures, and insipid Compositions and Qualities, lesse significant than the popular Air: All of them much fitter to beget Eternal Controversies, than to administer any satisfaction to a reasonable Understanding. These they gather up for the sweetest Posies and fairest Garlands, wherewith to adorn their Brows and Temples; and so they take their leave of Aristotle at the very Threshold.

Thus they reject the Harmony, and waste all their time in tuning the Instrument, and are best pleased, even ravish’d, with those strokes which glance below the Bridge, by which they sharpen and turn their Spirits habitually, and set the teeth of their disciples on edge; and then

Quo semel est imbuita recens, servabit, &c.

We take leave to ask, Whether Aristotle did not illustrate his beft Conceptions in his Works, with Mathematical Demonstrations? In this, Blan<ref>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...<br>...</ref>
of note, for any thing else but honest Moralties, did neglect the Mathemackts? What free-born Child, or yet what Slave, of any promising hopes, was not entered into these Disciplines, before they could number ten years of their Age. If these men would addict their palates to the pure fountains, and not wander after every polluted stream, then they would find more leisure for better things, to do some good for themselves and others: Then they would taste the pleasure, and reap the profit of their old Rule,

Dulcis ex ipse fonte, &c.

And withal they would have better understood their best friends.

Certainly; If Aristotle had been so happy, as to have enjoyed our Opticks, and other Instruments of Arts, and such Engins as we now employ, He would have been quite of another spirit than these are; and would have acknowledged a greater variety and more curious contexture, and more brisk Mechanicks in the Insects, which were in those days invisible, than in all the Animals, that were then known, or than are yet to be found in a far wider circumference; and would have confess'd the productions of our Pyrotechnical Furnaces to excell all, that could be reasonably expected from his own vast Fiery-Region.

We say heartily, Read Aristotle, read him in his own Stile, read him entirely and fully, not feeding only on his Ulcers and Excrencencies; nor taking up your rest in his Un-intelligible Heavens, at their Adamantine Gates, or about their Flaming Walls: Embrace his calm rays, and his dis-interested Reasonings: chuse his best Vertues, examine and weigh all his Mathematical Illustrations, descend to his particulars: And then hasten to our Christan Philosophers, and they will forth-with acquaint you with the true Works and wonderful Contrivances of the Supreme Author, and with the Discoveries, which by his indulgent Providence and his benigne Inspirations have been in former and later Ages afforded, for the benefit, and the sincerely grateful acknowledgements of humane race.

'Tis our main business, as well to retrieve all valuable Antiquities, as to supply fresh Discoveries: to recover good Old Helps, as well as to devise New. All our Artificers are designed, and appropriated, to unlock all the Repositories of Nature, To draw our her most concealed Operations and Rarities, To produce them with
with their best Advantages, and in their fairest Ornaments, for all good occasions: And whatever we find excellent in old Greece, or Rome, or in more ancient Monarchies, or in any one more happy part of the World, That in due season to communicate all over the World, to as many, as have the Ingenuity to give them a hearty Entertainment.

After thus much of Preface (which the Candid Reader will interpret with the same affection, it was written) I return to my task. Where I think it not amiss,

First of all, to take notice of the late Enlargements of our Philosophical Correspondencies in both remote and nearer parts of the World; concerning which we are singularly obliged to several of the Generous and Intelligent Citizens of this Famous Metropolis of England, especially the Eminent Governours of the East-India and Turkey Companies, beside those of the same City, that travelling into the West-Indies, have been very ready to receive, (and to promise good Accounts upon) such Philosophical Instructions, as were presented to them concerning many particulars, thought worthy to be further inquired into, in our American Colonies. To which we shall add, what we have procured, for this our purpose, by our Commerce with Spain, Portugal, and Barbary; as well as by our Navigations into Greenland and Iceland: And also what occasions we have before us, to enter into a conso- ciation with Germany, Bohemia, Hungary, Transylvania, Carin-thia, Tyroll, and with all the principal Cities of Italy: it appearing already by our former Papers, what interest we have, before now, establisht in France, and, by the assistance of the Eminent Heve-lius, in all the Countries upon the Baltic Sea, and in Poland; which is therefore related here, that we may thence take occasion to invite all Ingenious Men, and such as consider the importance of Cementing Philosophical Spirits, and of assembling together Ingenuities, Observations, Experiments and Inventions, scattered up and down in the World; that they would be pleased partly to recommend themselves, as they have occasion, these Kinds of Inquiries to their Ingenious Acquaintance, either living upon, or travelling into such places, as are concerned; partly to join their Symbola's, and to fend in their Proposals; and whatever shall occur to them worthy to desire information about, in those Countries
Inquiries for Suratte, and other parts of the East-Indies.

Though these Queries have been already dispatcht for India, and some of them even received an Answer, yet, because 'tis altogether necessary, to have confirmations of the truth of these things from several hands, before they be relied on, it was thought fit, rather to publish the Inquiries alone, for a more certain and full Information, than now to joyn such Answers thereunto.

The Inquiries are these; as the Relations published by Purchas, Linsehten, and others, concerning those parts, have given occasion to propose them.

1. Whether it be true, that Diamonds and other Precious Stones, do grow again after three or four years, in the same places where they have been digg'd out?

2. Whether the Quarries of Stone near Fettipore, not far from Agra, in the Mogol's Dominions, may be cleft like Loggs, and fawn like Planks, to ceel Chambers and cover Houses therewith. Likewise, Whether about Sadrapatan, on the Coast of Coromandel, there be a Stone of the like nature, so as, setting a Wedge upon it, one may cleave it with a Mallet as thick and as thin, as one pleaseth?

3. Whether upon the same Coast of Coromandel, about Tutucorin, and upon that of Ceylon, at Manar, and Jaffanapatam, they fish Pearls, as good as those about Ormus? Whether those Pearls are the better, the deeper they lie? What is the greatest depth, they are known to have been taken at? And whether it be true, that some of the Natives there, can stay under Water half an hour, without any Art?

4. Whether the Iron in Pegu and Japan, be far better than ours; and if so, what is to be observed in the melting, forging, and tempering of it?

5. Whether
5. Whether in Sumatra there be a fountain, running a very Sanative Oyl? And whether the ignivomous Mountain in the same Island, do burn continually, and cast out stones so eaten out by the fire, that they swim?

6. What is the Opinion of the more Inquisitive Men in those parts, of Amber-gris? And whether the greatest quantities and masses of it are found about the Isle Mauritius?

7. Whether it be Winter on the East-side of the Mountain Gates, which comes from the North to Cape Comorin, whilst it is Summer on the West-side, and so, vice versa?

8. Whether it be true, that upon the Coast of Coromandel, 16 deg. Northern Latitude, between Paleacote and Maselupatan, 50. Leagues in length (the hot Winds blowing from the Land-ward from 8. in the Morning, till 4. in the Afternoon, with such a suffocating heat, that the Inhabitants are not able to endure it, without extraordinary helps and refreshments;) every one daily for his provision of drink, hangs his Bottle, made of common pot-earth, and filled with Well-water, or other potable Liquor, upon some Post, Tree, or Wall, in places, where the Sun and Wind are most piercing; leaving it there all the day long in the scorching heat; and then taking it up about Evening at 4. of the Clock, the Drink is more cool, than any depth of Cellerage with us can make it? And whether, on the contrary, the Bottles being suffered to continue in the Air, as before, during the cool Sea-gales, which come in after the said hour, and continue all Night, till 8. in the Morning, to the refreshment of all Creatures, the Liquors grow hot and unfit for drink?

9. Whether the Tide near Mindanae, going from the Molucca’s to the Philippina’s, are so swift, that neither contrary Winds nor Anchors, can save a Ship from being carried away by it; and that it rises but about 3. or 4. feet? And whether the like be observed in the Bay of Cambaja, and in that between Martaban and Pegu? And particularly, Whether in the said Bayes, the Tides come in with that impetuosity and swiftness about the Quarters of the Moon, that the Watch-men from high Towers must with their Trumpets give warning to the people to retire; and that a Horse in his swiftest course, when such a Tide comes upon him, cannot out-run it: as Isaac Vossius observes, Lib. De Motu Marium.
And what other particulars are observable upon all those Coasts, concerning the Tydes?

10. Whether there be any Discoveries newer, than the newest printed Maps, of the parts of the World North-east of Japan? And whether Japan be truly an Island, or no?

11. What is the true way of making and colouring China-Dishes; and how in China and Japan, they make the Black-varnish?

12. With what Materials, and how they paint both upon Cloths, commonly call'd Pintado's, and likewise upon Canvas, &c?

13. Whether the Lignum Aloes be the Wood, or Root of a Tree? In what Country it is found: And how to know the best of the kind?

14. Whether the best Tea be that, which comes forth at the first of the Spring, and are the Top-leaves? In what manner 'tis dried; and whether the too hastily drying thereof hurts it?

15. Whether there grows a Wood in Java, that naturally smells like humane Excrement? And if so, what kind of ground it grows in?

16. Whether in the Molucque Islands there be a Red Wood, which burns, sparkles, and flames, without being consumed; yet may be reduced to powder, by rubbing between ones fingers?

17. Whether near the Fort of Ternate there be a Plant, call'd by the Inhabitants Catap; whence fall little Leaves, which are turned into Butter-flies?

18. Whether in Pegu, and other places of the East-Indies, they use a poyson, that kills by smelling; and yet the poisonous smell is hardly perceived?

19. Whether it be true, that the only Antidote hitherto known against the famous and fatal Macassar-poyson, is humane Ordure, taken inwardly? And what substance that poyson is made of?

20. Whether there be such a Vegetable in Java, call'd Mangas bravus, that is so poisonous, that it kills presently, and for which no remedy hath been yet found?

21. Where the best Calamba-wood, or Palo d' Aquila, grows? Whether the Palo d' Aquila be much inferior to Calamba, and how
how they are distinguished? Whether the later be the Pith of the former? Whence the best sort comes? Whether it be stored with a rich and cordial Balme, and that be the cause of its great rate, being much used in cases of decay of Spirits, and the lamen-
ess and impotency of Nerves?

22. Whether they draw an Oyl, resembling Oyl of Camphire, from the Roots of Cinamon-trees; and if so, how they draw it?

23. Whether the Camphire of Borneo be not the Exsudation or Gum of a Tree?

24. Whether the Indians can so prepare that stupifying Herb, call'd Dutroa or Datura, that they make it lie several Days, Moneths, and Years, according as they design it, in a Mans Body, without doing him any hurt, and at the end kill him, without missing an hours time?

25. Whether the Betele hath such a contrariety to the Durion, that a few leaves of that, put to a whole Shop-full of Durions, will make them all rot suddenly? And whether those, that have surfeited on Durions, and thereby over-heated themselves, do, by laying a leaf or two of Betele upon their Breasts or Stomachs, immediately cure the Inflammations, and recover?

26. Whether the Papayas, which bear fruit like a Melon, do not bear, unless Male and Female (as the Vulgar distinguishes them) stand together?

27. Whether there be two sorts of the Tree, call'd Arbor Tri-
ste, one, by the Name of Triste di Die, the other, Triste di Not-
te; whereof the former sheds his flowers at the Rising, the o-
ther, at the Setting of the Sun? And whether the distilled water thereof (call'd Aqua di Mogli by the Portugals) may not be trans-
ported into these parts?

28. Whether one of those Trees, call'd Arbre de Rays, pro-
pagates itself into a whole Forrest, by shooting up, and letting fall Roots from all its branches into the ground, that spring up again, and so on? And whether there be any single ones of these Trees, that are above 50 feet in Diameter, as some affirm?

29. What particulars are observable in any other Plants of those parts?
30. Whether those Shell-fishes, that are in these parts, plump and in season at the Full Moon, and lean and out of season at the New, are found to have contrary Constitutions in the East-Indies?

31. Whether the Animal, that yields the true Muske, be like a Deer, hornless, found in the High-Country between Pegu and China? And whether the Muske grows in Baggs, Blisters, or Swellings, which the Beast rubs off against Trees, it being affirmed to have been found in the Woods by the Scent? Whether true Muske, is discerned from false by its yellowness, when rubb'd upon ones hand, and by its keeping that Colour and the Scent?

32. Whether there be two sorts of Gum-lack, one produced by an Insect, a certain winged Ant; the other, the Exudation of a Tree?

33. To inquire after the Fish call'd Caballa, said to be very powerful in staunching of blood?

34. Whether about Java there be Oysters, or other Shell-fishes, of that vast bigness, as to weigh 300 pounds?

35. Whether in Malacca there grows sometimes a stone in the stomack of a kind of Porcupine, call'd Pedro Porco, esteemed for its Cordial Vertue above Bezoar?

36. Whether there be found in the head of a certain Snake, a Stone, which laid upon a wound of any Venemous Creature, sticks fast to it, and draws away all the Poyson; then being put in Milk, voids its poyson, and turns the Milk blew; and then applied again, draws out the rest of the poyson, that may be behind, till the wound be perfectly cleansed?

37. Whether the Rhinoceros have such an Antipathy against Elephants, as is commonly related?

38. Whether in the Island of St. Helena, the Tide be at the same time round in the several Coasts of it; and what is the hour of Full Sea, and what the age of the Moon at the time of Observation?
Inquiries for Persia.

1. What are chiefly the present Studies of the Persians; and what Kind of Learning they now excell in?

2. What other Trades or Practices, besides Silk-and Tapistry-making, they are skilled in?

3. Whether, there being already good Descriptions in Words of the Excellent Pictures and Basse Relieves, that are about Persepolis at Chimilnar, yet none very particular; some may not be found sufficiently skill'd, in those parts, that might be engaged to make a Draught of the Place, and the Stories there pictured and carved?

4. How they make that Plaister, wherewith in those parts and in India they line their Tanks or Cisterns, and which, when dry, shiness like Marble, and is much harder?

Other Inquiries, concerning the Air, Waters, Minerals, Vegetables, Animals, &c. peculiar to Persia, may be taken out of those General Heads of Inquiries, for a Natural History of a Country, printed in Numb. 11. and out of those Articles of Inquiries concerning Mines, published in Numb. 19. to which we refer the Reader.

As to the Inquiries proper for Turkey, they also are already publish'd. See Numb. 20.

Inquiries

For Virginia and the Bermudas.

1. Concerning the Varieties of Earths; 'tis said, there is one kind of a Gummy consistence, white and clear: Another, white, and so light, that it swims upon water: Another, red, call'd Waperg, like Terra Sigillata. Quare, what other considerable kinds are there? And to feed over a parcel of each.

2. What considerable Minerals, Stones, Bitumens, Tinctures, Drugs?

3. What hot Baths, and of what Medicinal use?

4. What is the Original of those large Navigable Rivers, which
which empty themselves into the Bay of Chesapeake? And whether on the other side of that ridge of Mountains, from which they are supposed to proceed, there be not other Rivers, that flow into the South-Sea?

5. How the Silk-grasse is prepared?

6. To give a full account of that Vulnerary Root, called Witch-caran: Of Pocone, a Root of a red juye, a good tincture: Of Musquashpen, a Root of a red tincture: Of the Plant Maricock, whose fruit is said to be fashion’d like a Lemon, exceeding pleasant to the taste; of a blossom most beautiful: Of the Chincomen-Tree, whose fruit is said to have a huske like a Chesnut, luscious and hearty meat, both raw and boiled.

7. Whether there be in the Bermudas a Poison-weed, like our Ivy, whose leaves do by the touch cause Blitters: And a Reed, whose juye or infusion causeth Vomit?

8. What kind of Trees those Barkes are taken from, that are used instead of Tile or Slate in the covering of their Houses, being cooler in Summer and warmer in Winter, than Stone?

9. To give a particular account of the Spider in the Bermudas, said to be large and beautiful for its colours, weaving a Web betwixt several Trees, which is affirmed to be for substance and colour like perfect raw Silk; so strong, that Birds, like Snites, are snared therein?

10. Whether Deer have there generally three or four Fawnes at a brood? And whether any of the Cattle transported from hence, becomes there more fruitful, than they were here?

11. Whether the Relation be true, of a Glue made of Harts-Horn, that will not dissolve in Water; and if so, how made?

12. Whether at the bottom of the Bay of Chesapeake Northward, the Natives be still of such a Gigantick Stature, as has been reported? And, whether there be another people, not far from these, Eastwardly, of a Dwarfish Stature?

13. Whether round about the Coast of the Bermudas, the Tides keep the same time; and at what a clock precisely 'tis High-water on the Days of Full and New Moon; and how high the Water rises then? And the like on the Coasts of Virginia and Florida?
For Guaiana and Brasil.

1. Whether about Urraba near Oronoque, some 8. degrees Northern Latitude, and about the Town Darien, Toads are presently produced, by throwing a kind of Moorifli Water found there, upon the Floors of their Houses: Linschoten.

2. Whether it be true, that the Locust of Brasil, call'd Caayara, changeth in the Spring-time of that Countrey into a Plant, and withers away, like a Plant? And whether in the same Countrey, that kind of Eruca, call'd by the Portugals Lagartas des Verias, turns into a Bird, admirable for Colour and swift flying; the change thereof being made so leisurely, that one may for a while see half of the Insect, and the other half of the Bird, which the Natives call Guainumbi, the Portugals Pegafrel. Pifo.

3. Whether upon the Leaves of that Brasilian Tree, call'd Cereiba, there is, in a Sun-shiny day, found a White Salt in that quantity, that one may gather as much from two or three Leaves, as will salt a good pot of Broth: Pifo.

4. Whether there be found about the mouth of the River of Amazons, a green Argilla, which, though very soft under water, yet, when exposed to the Air, grows almost as hard as a Diamond; in so much that the Natives make Hatchets of them, strong and sharp enough to cleave Wood, for which purpose also those Indians are said to have used it, before they got Iron-ones? And, whether this Argilla, become Stone, have a peculiar virtue against the Epilepsy, when carried by the Patient? Pelleprat in his Relation of the Islands and Terra-firma of the Southern America.

5. Whether the Black Bees in Guaiana, about the River Oronoque, make black Honey and Waxe? And whether they have no Stings, as the same Pelleprat affirmeth?
Of a considerable Load-stone digged out of the Ground in Devonshire.

This Stone was lately sent up out of the said County, and presented to the R. Society by the Reverend Arch-deacon, Doctor Edw. Cotton, with this description, That it weighs 60 pounds; and that, though it take up no great weight, yet it moves a Needle about nine Foot distant. Some part of it being broken off, he hath sent up also, because (faith be) being put in its proper place, it adds much strength to it, but without that addition it moves not much more than seven Foot.

Care will be had, that Tryals be made of the Virtue of this Stone, both of the two pieces closed together, and of each piece separately, and that uncapped as well as capped.

Some Observables about Load-stones, and Sea-Compasses.

A Noble Person did upon a late occasion, affirm, That a Needle of a Sea-Compass, put in a good Iron Mine (which, he said, yeilded 23 pounds of Metal, out of 120 pounds of Ore) was not sensibly moved thereby.

Another Honourable Person desired, it might be observed, whether touched Needles move otherwise, when the Veins of Iron do not lie North and South, then when they do so?

It being inquired by a Note from forreign parts, Whether the Sea-Compasses in England were brought to a greater perfection, than in other Countries? Answer was made by intelligent persons here, That all the perfection of our Sea-Compasses, as yet, consisted in this, That the Needles be touched by good Load-stones, and well librated, and that the Variation be truly placed: Though it was suggested withal, that for the greater perfection of such Sea-Compasses, a way was contriving, to shew the Variation to Minutes and Seconds.

It was also propos'd, That it might be inquired into,

1. Whether a Needle may be so toucht upon any Magnet, as not to point to the true North and South, to be tried in such places where there is no Variation known?

2. Whether
2. Whether different Load-Stones will give different Directions? And whether fainter or stronger touches upon one and the same Magnet, will cause any Variation in the Directions? For which purpose, as many Load-Stones should be procured, as could be had, and a good number of Needles exactly made, of the same Metal, bigness, and figure:

PROPOSALS
To try the Effects of the Pneumatick Engine exhausted, in Plants, Seeds, Eggs of Silkworms.

The Ingenious Dr. Beale did formerly suggest, as follows.

It would be, I think (faith be) very well worth the trial, to see what Effects would be produced on Plants, put into the Pneumatick (or Rarifying) Engine of Mr. Boyle, with the Earth about their Roots, and flourishing, whether they would not suddenly wither, if the Air were totally taken from them. And particularly to try in the Season, Cherry-Blossoms, when partly opened, partly not opened, upon a Branch; to wit, whether the Air may be so attenuated as to blast. But it may be noted, that the Blossoms do not forthwith discover the blast: An old experienced Country-man having once given me notice of a blasty Noon, (it being then a Sultry weather, and somewhat gloomy with the thickness of Exhalations, almost like a very thick Mist) and within a day or two shewing the proof upon the Cherry-Blossoms then flagging, but not much altering their Colour till two days more were past.

The Noble Mr. Boyle suggests as proper for the approaching Season; That it may be tried,

1. Whether Seeds (especially such as are of a hasty growth, vid. Orpin, Lettice, Garden-cress-seeds, &c.) will germinate and thrive in the exhausted Receiver of the said Engine?

2. Whether the Exclusion of Air from the Sensitive Plant, would be harmful to it?

3. Whether the Grafting of Pears upon Spina Cervina (the almost only Purgative Vegetable known in England) will produce the effect of communicating to the Fruit that purging quality, or not?

4. Whether Silkworms Eggs will be hatched in such an exhausted Receiver, in the Season proper for hatching?
To which may be added, the Trials of putting in a Vial full of water, some of those Herbs that will shoot and grow in water alone, including them in such a Receiver, and pumping out what Air you can, to see whether they will then shoot, or not.

And though some of these Proposals have been formerly begun to be Experimented, yet ought they to be diligently prosecuted, to see how far the Air is necessary to Vegetation; and whether Plants do indeed live as much upon the Air, as the Earth, and the Branches of them are rooted (as it were) in and quickned by the Air, as their Roots are planted and nourish’d in and by the Earth.

The Experiment heretofore made of this kind, was, That some Lettice-seed being sown upon some Earth in the open Air; and some of the same Seed at the same time upon other Earth in a Glass-Receiver of the above mention’d Engine, afterwards exhausted of Air; the Seed exposed to the Air was grown up an inch and a half high, within eight days; but that in the exhausted Receiver, not at all. And, Air being again admitted into the said emptied Receiver, to see whether any of the Seed would then come up; it was found, that in the space of one week it was grown up to the height of two or three Inches.

**Observations**

Concerning Emmets or Ants, their Eggs, Production, Progress, coming to Maturity, Use, &c.

This was communicated by Doctor Edmund King, Fellow of the R. Society, at the Instance of the Publisher, as followeth.

1. Here have occurred to my Observation but three sorts of Ants, commonly without Wings; viz. Very Black, Dark Brown, and the third sort of near the colour usually call’d Phillemort.

2. Each kind inhabit by themselves in their several Banks; two sorts seldom or never being found together; and if either of the other two sorts be put into the black Ants Bank, ’tis worth observing, what enmity there is betwixt these little Creatures, and with what violence the Black ones will seize on the Red, never leaving to pinch them on the head with their Forceps or Claws, till they have kill’d them upon the place: which done, they will carry...
ry them dead out of the Field, from their Bank. But if you put Black Ants into a Bank of the Red, the Black seem to be so sensible of the strangeness of the place they are in, that they will not meddle with the Red, but as if they were frightened, and concerned for nothing but self-preservation, run away.

3. Upon opening of these Banks, I observe first a white substance, which to the bare eye looks like the scatterings of fine white Sugar or Salt, but very soft and tender; and if you take a bit of it, as big perhaps as a Mustard-seed, and lay it on the Object-plate of a good Microscope, you may by opening it with the point of a Needle, discern many pure, white and clear appearances in distinct Membrans, all figur'd like the lesser fort of Birds Eggs, and as clear as a Fishes Bladder. This same substance as it hath been just now described, I finde in the Ants themselves, which I take to be the true Ants Eggs; it being obvious to observation, that where ever this is uncovered, they make it their business to carry it away in their mouths to secure it, and will after you have scatter'd it, lay it on a heap again with what speed they can.

4. I observe they lie in multitudes upon this (if I may so call it) Spawn of theirs; and after a little time, every one of these small adherances is turn'd into a little Vermicle, as small as a Mite, hardly discerned to stir; but after a few days more, you may perceive a feeble motion of flexion and extension, and they begin to look yellowish and hairy, shaped very like a small Maggot, and so, keeping that shape, grow almost as big as an Ant, and have every one a black spot on them.

5. Then they get a Film over them, whitish, and of an Oval shape, for which reason I suppose they are commonly call'd Ants Eggs, which yet (to speak properly) they are not.

6. I have, to prevent mistakes, opened many of these vulgarly call'd Ants Eggs, I mean, the lesser fort; (for there are some as big as a Wheat-corn, others less then a Rye-corn) and in some I finde only a Maggot, to appearance just such as was described before: In others, I finde a Maggot beginning to put on the shape of an Ant about the head, with two little yellowy specks where the Eyes are design'd: In others, a further progres, and furnifht with every thing to compleat the shape of an Ant, but wholly transparent, the Eyes onely excepted, which are then as black as black Bugles.

7. But
7. But when they have newly put on this shape, I could never discern the least motion in any one part of the little Creature, whereof the reason may perhaps be, the weakness of their Fibres; for after a little more time, when they begin to be brownish, they have strength to stir all their parts.

8. At last I met with some of these reputed Eggs, which being carefully open’d by me, I took out of several of them, every way perfect and compleat Ants, which did immediately creep about among the rest, no way differing from many other Ants, but by a more feeble motion of their Limbs. And this I took for a clear demonstration of what I designed, which was to know, That the Film does onely cover the Maggot, while she is transforming into an Ant, and fit to shift for herself.

9. The black Speck that is at one end of every such reputed Ants Egg, I suppose to be cast out of the Maggot in her transformation; since, after it puts on the shape of an Ant, the Speck is quite gone, and the whole body of the Ant pure clear; since also this Speck at the end of the said Egg, lies always close to the Anus of the included Ant.

10. As to their care for their Young (by which I mean all the forts and degrees aforesaid, from the Spawn to the vulgarly call’d Eggs, in every one of which you’ll finde a young Ant) it is observable; How upon the breaking up of their Banks, they make it their business immediately to carry their Young out of sight again, laying the several sorts of them in several places and heaps: the which if you mingle again or scatter, you shall, laying but some bits of Slate, or the like, in any place they may come to and get under, after a few hours see all the Vermicles, and vulgarly call’d Eggs, laid in their several and distinct parcels, under such pieces of Slate, &c. Provided the place be not so cold as to chill their Limbs; which if it be, by being brought to the fire they will soon recover their strength, and fall to their business again, of securing their little Ones.

11. I have observed in Summer, That in the Morning they bring up those of their Young (that are vulgarly call’d Ants Eggs) towards the top of the Bank, so that you may from Ten in the Morning, until Five or Six Afternoon, finde them near the top, especially about One, Two, or Three of the Clock, and later, if the Weather

Kk k 2 be
be hot, when for the most part they are found on the South-side
of the Bank: that towards Seven or Eight at Night, if it be cool,
or likely to rain, you may dig a foot deep before you can finde
them.

* They know all the sort of their Young to well, that you cannot
deceive them, though you may with fine Sugar, Salt, or the Crums
of very white stale Bread, scatter'd in the Mould, where their first
true Eggs are (as I call them) be mistaken your self, yet the
Ants will not, nor touch a bit of what is not their own Off-spring.

13. I cannot pass by the Use of Ants in feeding young Pheasants
and Partridges, they being the principal Food of these Birds, both
wilde and tamed, for several weeks, as is well known to all that are
versed in breeding them up. And a chief reason, why many finde
it so nice a thing to breed up the said Birds, is, that either they give
them too sparingly of this Food, or let them fast too long, not
knowing, that as soon 'tis day-light, they will seek it for their
Breakfast, and if they want it, will in a few hours be faint and weak,
and some grow so chill for want of that supply of Nourishment,
that it is no easie matter to recover them.

14. But (to add this by the by) Though these Insects be so
good a Food to these Birds, whilst very young; yet when by ill
ordering of those that should keep them sweet, and often shift their
water, or by ill Dyet, as musty Corn, &c. they grow sick; then
Ants will not always recover them, though you give them never
so many: And I have been forced to make use of other Insects to
cure them, to wit of Millepedes and Earwigs, either of which will
do good, but both together, better; given in a good quantity two
or three times, at least, a day; but then those other things must
be observed too, of keeping their Houfe clean, and giving them
sweet Corn, and shifting their water twice a day, keeping them
within, till the Dew be from the ground, letting them bask in
Sand, partly in the Sun, the place a little shaded, and putting them
up in a warm house before Sun-set.

Which particular I thought not amiss to add for those that de-
light in breeding up Pheasants and Partridges, my self having lost
many of both sorts, till I learned that Vertue of those Insects; af-
ter which, seldom any of them, by me intended to be bred up,
have died.
An Account
Of a small Book in French, entitled
HISTOIRE DES JOYAUX.

Des Principales Richesses de l'Orient et de l'Occident,
par le Sr. Chapuzeau.

His History treats of Diamonds, Rubies, Emeralds, Pearls, Coral, Bezoar, Yellow Amber, Amber-gris, Indigo, &c.

Of Diamonds, The Author shews:

1. The Places, whence they are taken; of which he finds but five in all the East-Indies; whereof two are Rivers, vid. Saccadan in Borneo, and Nage in the Kingdom of Bengal; at the bottom of both which, he faith, the Diamonds are found among the sand, after the waters, that fall as great Torrents from the Mountains, are run off, and the three others are Mines, in the Kingdoms of Decan, Cuncan, and Golconda. In this Relation he observes, that the Diamonds which are found at the bottom of those Rivers, have the best Water; but those, in Mines, have often Flaws (which he imputes to the violent knockings of the Rock) and Blebs, ascribed to the condition of the Earth or Sand they are found in, vid. when that is not pure, but fattiish or black. He takes also notice, that Diamonds are the heaviest of precious Stones, as Gold is of Mettals.

2. The Manner, how they are found and separated; which is the same in substance, with that, described Num 18. p. 328.

3. The Price of them, according to the proportion of their weight, for which he gives this Rule. Take, faith he, a Diamond of 10 Carats: this number is to be squared (which makes 100,) then, if the Stone be clean, each Carat, according to its perfection, may be worth 40 to 60 Crowns; if it have no good water, or have a Bleb or Flaw, the Carat will not be worth but from 10 to 30 Crowns. So multiplying the said 100 by the number, which each Carat of such or such a Stone may be worth, the product is the price of the Stone.
For Rubies he discourses also of the Places, where they are found; and of their Price. The Places are, the Kingdom of Pegu, and the Isle of Ceylon; whence very few are suffered to be carried away. The Price is, that a good Rubi of the weight of 1 Rati (which is 2 of a Carat) is esteemed at 20 old Pagodes in India, each Pagode being about 10 shillings English.

<table>
<thead>
<tr>
<th>Ratis</th>
<th>Pagodes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Of 2</td>
<td>is valued at 100.</td>
</tr>
<tr>
<td>Of 3</td>
<td>—— 250.</td>
</tr>
<tr>
<td>Of 4</td>
<td>—— 500.</td>
</tr>
<tr>
<td>Of 5</td>
<td>—— 900.</td>
</tr>
<tr>
<td>Of 6</td>
<td>—— 1500.</td>
</tr>
<tr>
<td>Of 7</td>
<td>—— 2300.</td>
</tr>
<tr>
<td>Of 12</td>
<td>—— 12000.</td>
</tr>
</tbody>
</table>

Concerning Turquois, they are found in Persia, in the Province of Chamaquay, North of Isfahan, in two Mines, called the Old and the New Rock. These of the New, are of an ill whitish Blew; but those of the old, are not suffered to be digged out, but by the King of Persia himself.

Emeraulds are affirm'd by him, never to be found in the East-Indies, but in Peru, whence they were carried by that Trading People to the Moluccas, even before America was discovered by the Europeans; and so they come from the Orient; of much les value, than they were formerly, by reason of their commonness. The Author notes, that Emeraulds grow in stones, just as Chrysfals, forming a Vein, in which they are by little and little refined and thickened; and that some of them are seen, half white and half green; others, all white; and others all green and perfect.

To Pearls he assigns in the Orient, four places, where they are fish'd: The Isle of Baharem in the Persian Gulf: The Coast of Arabia Felix, near the Town of Catif, over against Baharem: The Isle of Ceylon about Manar: The Isle of Japan. The best at Ceylon, but small; the biggest at Japan, but uneven. In the West-Indies they are fish'd in the North-Sea, in the Isles of Marguerite, Cubagua, St. Marthe; and at Comana, and Comanagote, near the Continent; and in the South-Sea, near Panama: which

American
American fort, though they are much inferior to the Oriental, in Lustre, yet they far excel them in bigness, amounting sometimes (faith this Author) to 42 Carats.

In this Relation 'tis mention'd, that sometimes 5. or 6. Pearls are found in one Oyster: That Pearl-fishers are fed with dry and roasted meat, to give them better breathing: That Pearl-bearing Oysters are not good to eat, being flat and hard of digestion, 

As to the Price of good Pearls, well fashion'd, he marketh it, as follows:

<table>
<thead>
<tr>
<th>Grain</th>
<th>Crowns</th>
<th>Carats</th>
<th>Crowns</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>4</td>
<td>289</td>
</tr>
<tr>
<td>2</td>
<td>4</td>
<td>4</td>
<td>324</td>
</tr>
<tr>
<td>3</td>
<td>9</td>
<td>4</td>
<td>361</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>400</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>441</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>484</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>529</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5</td>
<td>576</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6</td>
<td>625</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6</td>
<td>675</td>
</tr>
<tr>
<td></td>
<td></td>
<td>6</td>
<td>729</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7</td>
<td>784</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7</td>
<td>841</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7</td>
<td>900</td>
</tr>
<tr>
<td></td>
<td></td>
<td>7</td>
<td>960</td>
</tr>
<tr>
<td></td>
<td></td>
<td>8</td>
<td>1024</td>
</tr>
</tbody>
</table>

Of Corals, he taketh notice, where they are fish'd, and in what manner. The Places, he saith, to be Eight; Three upon the Coasts.
Coasts of Corsica and Sardinia, vid. at Argueil (where is the best) Baza, and near the Isle of St. Peter: One upon the Coast of Sicily, near Drepanum: Two upon the Coast of Africa, near the Bastion of France, and at Tabarca: One more, upon the Coast of Catalonia, at the Cape of Quiers: And the last, about Majorca. Observing, that red Coral is not found, but in the Mediterranean alone, where ’tis fished from the beginning of April, till the end of July, employing commonly about 200 Boats. The manner of fishing them, is with two big beams of wood, laid cross wise, with a good piece of Lead on the middle, to make it sink, casting about it course Hemp, carelessly twisted, and tying this Wood to two Ropes, whereof one hangs at the Sterne, the other at the fore-part of the Boat: And so letting this contrivance fall into the Current, along the Rocks, where the Hemp being turned about, and engaged in the Coral, there need sometimes many Boats to draw away the Instrument.

Bezoar he faith, is not onely found in Golconda, in the Province of Renquery, in the Maw of Goats, whereof some are at times furnished with a dozen a piece, but also at Macasar, in the Isle of Celebes, in the Body of Apes, bigger than those found in Golconda. He mentions, that the people in those parts, to find whether a Goat hath any of those Bezoar-stones in its body, do beat his belly with their hands, and rub it, till all the stones in the Animal come together, and then they feel and tell them, as you do stones in a Bag, &c.

FINIS.

In the SAVoy,
Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar, and James Allestry in Duck-Lane, Printers to the Royal Society, 1667.
The Contents.

Directions for Observations and Experiments to be made by Masters of Ships, Pilots, and other fit Persons in their Sea-Voyages: Printed with Enlargements and Explications of what was formerly published of this Kind, suggested partly by Sir R. Moray, partly by Mr. Hook; as, The several ways of Observing, both at Sea and Land, the Declinations and Variations of the Needle: Some ways of knowing the different Gravities of Sea-water: A Form of a Scheme, representing at one view, to the eye, Observations of the Weather for a whole Moneth, &c.

Directions
For Observations and Experiments to be made by Masters of Ships, Pilots, and other fit Persons in their Sea-Voyages.

Though the Art of Navigation, one of the most useful in the World, be of late vastly improved, yet remain there many things to be known and done, the knowledge and performance whereof, would tend to the accomplishment of it: As the making of exact Mapps of all Coasts, Ports, Harbors, Bayes, Promontories, Islands, with their several Prospects and Bearings; Describing of Tydes, Depths, Currents, and other things considerable in the Seas: Turnings, Passages, Creeks, Sands, Shelves, Rocks, and other dangers: Nice Observations of the Variations and
and Dippings of the Needle, in different places, and in the same place, at different times: The Winds, Weather, and Tempers of the Seasons everywhere: The great Depths, Ground, and Vegetables at the bottom of the Sea: The various Degrees of Saltiness of the Sea-water, in several places, and at several Depths at the same place. If besides Astronomical things, to be hereafter lookt into, the following Experiments be carefully made, and Directions observed by as many Ingenious Persons, as have opportunity, it may fairly be hoped, that from multitudes of Experiments and Observations, such Rules may be framed, as may be of inestimable use for Seamen. To which purpose, the Royal Society, having some years ago, ordered that Eminent Mathematician Master Rooke, one of their Fellows, and Geometry Professor of Gresham Colledge (since deceased, to the great detriment of the Common-wealth of Learning) to draw up some Directions for Seamen, the better to capacitate them for making such Observations abroad, as might be pertinent and suitable to the purposes above-mentioned; such Directions were drawn up accordingly, and soon after printed in Numb. 8, of these Transactions. But, further to encourage and facilitate the Work of those, that shall be engaged to put them into practice, it was thought fit, that what of this Kind was heretofore but barely proposed, should now be publish'd with ample and particular Explanations, and considerable Additions; which done, a good Number of such printed Copies is, by the Care, and at the Expences of the R. Society, to be lodged with the Master of Trinity-House, to be recommended to such, as are bound for far Sea-Voyages, and shall be judged fit for the performance: who are also to be desired, to keep an exact Diary of such Observations and Experiments, and deliver at their return a fair Copy thereof to the Lord High Admiral of England, his Royal Highness the Duke of York, and another to Trinity-House, to be perused by the said R. Society.
The Particulars themselves follow:

1. To observe the Declinations and Variations of the Compass or Needle from the Meridian exactly, in as many Places as they can, and in the same Places, every several Voyage.

At Land, where by the help of good fixed Dials, and other fit Instruments, the precise Meridian of the place may be known, it is easie to find the Variation of the Needle, divers ways: As, by applying of the Needle, &c. to the Shadow of a Thread hanging perpendicular, when the Sun is in the Meridian; or to the Meridian Line; or the Side of a fixed Horizontal Dial, &c.

But at Sea, in regard the Meridian is not so easie to be found to any tolerable exactnes, to know the Variation of the Needle, is much more laborious and difficult. The Height of the Pole; and the Sun's Declination being known, a large Ring-Dial, truly wrought, having a Box with a Compass or Needle fixed to its Meridian below, may go as near as any other Instrument, to shew the Variation required. For, when it is set to the just hour and minute of the day, the Meridian of it stands just in its due place; and so shews how far the Needle varies from it, as exactly as the largeness of the Card will permit.

But because these Dials are so rarely just, &c. though they may be used and taken notice of, yet are they not to be relied on. The thing therefore is to be performed, as followeth:

Find out the Sun's Azimuthal Distance from the Meridian, some hours before, or after Noon, and then its Magnetic Azimuth, or Distance from the Meridian pointed at by the Needle, and the Difference of these two Distances, is the Variation of the Needle.

To find the Sun's true Azimuth, or by how many Degrees, &c. of the Horizon it is distant from the Meridian; its Declination, its Altitude, and the Elevation of the Pole, must all three be known.
For finding whereof, every Expert Mariner is instructed, or may be so, from his Sea-Books, and so it needs not here to be set down. Nor how by the help of these, the Azimuth required may, to Degrees, if not nearer, be found out upon a good Globe or Planisphere, (whereof there is a design to have one, that is, the Analemma, contrived into a form of Instrument for the use of the publick, and that ere long, which will with great facility perform all that the Globe can do, with much more exactness and conveniency) that being sufficiently known.

But to do it accurately, you must constitute a Spherical Oblique-angled Triangle, of the three Complements, of the Sun’s Declination, its Altitude, and of the Height of the Pole; the measures of all the Sides whereof are known; one from the Zenith to the Pole, another from the Pole to the Point of the Sun’s Altitude, and the third, from that point to the Zenith. Now by those you are to find out the Angle at the Zenith, which being found, subtract it from 180, and the remainder is the Sun’s true Azimuth, or Distance from the Meridian of the Place.

This Angle is to be found divers ways, as by the Tables of Sines, Logarithmes, &c. the manners of doing whereof, are set down and demonstrated by John Newton in his Institutio Mathematica, Case 11, and in other Books of Trigonometry.

And the true Azimuth of the Sun being thus found, and the Magnetical Azimuth of it, according to your Needle, observed, subtract the lesser Number from the greater, and the Remainder is the Variation of the Needle. If the Magnetical Azimuth be less than the other, then the Variation is towards the same side of the Meridian, where the Sun is, if greater, on the other.

To observe the Sun’s Azimuth by the Needle, and the Needles Variation to Degrees, any Needle, long enough to afford upon a Card under it, a Circle divided into Degrees, put in a Square Box, after the ordinary manner of Clinatories, will serve turn; by placing the Box so, as the Sun may shine upon any two opposite sides of it, at the same times that the Sun’s Height, &c. are taken: For then the Needles Distance from the Diameter of the Circle on the Card, that is parallel to those sides, is the Magnetical Azimuth required.
The same may be done with an ordinary Sea Compass, so it have a Circle towards the Limb of the Card, divided into Degrees, by fastening a small Thread, Lute-string or Wire (not of Iron) so upon it, as to pass just over the Center of that Circle, and placing a small piece of Wood or Brass-wire perpendicular on the edge of the Box at the end of the Thread, and turning it to the Sun, till the Shadow of it fall just upon the Thread: then observe, what Degree of the Circle on the Card the Thread cuts, by looking plum upon it; and that is the Sun's Magnetic Azi-muth.

But to have the Variation to Degrees and Minutes (which is most desirable) then the Observation last mention'd must be made with a Quadrant, Sextant, or some such other Instrument, so large as to admit of the division of a Degree into Minutes; which will require the Radius to be about three foot; the larger the better. If a Quadrant, then, it being laid flat, and the Square Box with the Needle placed upon it, move the Quadrant to and again till that side of it, on which the Box is placed, lie parallel to the Needle, when at quiet: Then the Sight of the Quadrant being slid along the Rimb of it, till the Sun shine on both its sides at the same time, the Mid-Line, that divides equally the Sight, when the Sun shines upon it thorow the slit, will mark the Degree and Minute of the Sun's Magnetic Azimuth. All which is easie to be put in practice.

And if many such Observations be made by several persons at the same place, and by the same or other persons distant from one another, 1, 10, 20, or more Years; Not only will the Compass become more useful than formerly, even to be conducive possibly to the finding the Longitude at Sea, at least in some places: but the variation of the variation of the Needle being known in different places; all will be reduced to Rules, and so from hence, Philosophical or Natural Knowledge, will probably be enlarged by a happy discovery of the true cause of the Verticity, or Directive faculty of the Loadstone, one of the Noblest and most abstruse Phenomena, that falls under the cognizance of humane Reason.

To find this variation by the Stars, is so easie, as every Master can do it; seeing there is no more requisite, than to find out the true North, that is, the Meridian, and compare the Needles position
on with it. By this means, the variation may be had well enough to degrees, half degrees, and some smaller parts; and if carefully and curiously prosecuted, even to Minutes too. But it will not be amiss, to do it both by the Sun and Stars, that the greater certainty may be attained.

2. To carry Dipping-Needles with them.

The Dipping-Needle is to be used at least as frequently as the former Experiment is made, and in the same places, in order to the same purposes. All that needs be said of the Manner, is, that when the Dipping of the Needle is to be examined, the Circle, in which it moves, is to be hung perpendicular, and turned, till it be just in the Magnetical Meridian, where it dippeth most, and the degree of its depression under the Horizon is to be noted in a Table. See Figure 1.

3. To mark carefully the Flowings and Ebbings of the Sea, in as many places as may be.

The Particulars here to be regarded, are, 1. The precise times of the beginnings of the Flood and Ebb, in all Rivers, Bayes, at Promontories, Capes, and in all Roads, Harbours, &c. 2. Which way Currents run in all places, with their Times, Changes, &c. 3. What perpendicular Distance there is, between the highest reach of the Tide, and lowest of the Ebb, both of all Spring-Tides and Neap-Tides, with their irregularities, &c. 4. What day of the Moon's age, and what times of the Year the highest and lowest Tides fall out: and all other considerable Accidents observable in Tides, chiefly in and near all Sea-ports, Harbours, Roads, Islands, &c. as St. Helens Island, Bermudas. 5. The position of the Wind at every Observation of the Tides, &c.

4. To
4. To remark curiously the Scitation, Figures, &c. of all dangerous Rocks, Sands, Channels, Entries, and Courses of Rivers, and all difficult Passages and Courses in all places; to measure and describe the same Exactly, their distances, bearings, &c. As also the Prospects of remarkable Coasts, Promontories, Ports, Islands, &c. in the same manner; and make Draughts, Plots, and Maps of them, with their Longitudes, Latitudes, Scales, &c. and all Beacons, Buoys, Landmarks, Light-houses, &c. which serve for directing the Course of Ships through narrow Channels, over Bars and Banks, into Rivers, Ports, Bayes, &c. And to sound Depths near all Coasts, in all shallow Places, Roads, &c.

5. To sound the deepest Seas without a Line, by the help of an Instrument, represented by Figure 2.

To perform this, take a Globe of Firr, or Maple; or other light wood, as A, let it be well secured by Varnish, Pitch, or otherwise, from imbiring Water, then take a piece of Lead or Stone, D, considerably heavier, than will sink the Globe: Let there be a long
long Wire-staple B, in the Ball A, and a springing wire C, with a bended end F, and into the said Staple, press in with your fingers the springing Wire on the bended end: and on it hang the weight D, by its hook E, and so let Globe and all sink gently into the Water, in the posture represented in the said Figure, to the bottom, where the weight D touching first, is thereby frop; but the Ball, being by the Impetus it acquired in descending, carried downwards a little after the weight is frop, suffers the springing Wire to fly back, and thereby sets it itself at liberty to re-ascend. And by observing the time of the Ball’s stay under Water (which may be done by a Watch, having Minutes and Seconds; or by a good Minute-Glass; or best of all, by a Pendulum, vibrating Seconds; the which must be three foot, three inches, and one fifth of an inch long, viz. between the middle of the Bullet and the upper end of the Thred, where it is fastned, or held when it vibrates.) You may by this way, with the help of some Tables, come to know any depth of the Sea.

Note. That care must be had of proportioning the weight and shape of the Lead, to the bulk, weight, and figure of the Globe, after such a manner, as upon experience shall be found most convenient.

In some of the Trials already made with this Instrument, the Globe being of Maple-wood, well covered with Pitch, to hinder soaking in, was 5½ inches in Diameter, and weighed 2½ pounds; the Lead, of 4½ pounds weight, was of a Conical (but is now used of a Globous) Figure 11 inches long, with the sharper end downwards, 1½ at the bottom in Diameter. And in these Experiments made in the Thames, in the depth of 19 foot water, there passed between the Immersion and Emission of the Globe, 6 Seconds of an hour; and in the depth of 10 foot water, there passed 3½ Seconds, or thereabouts: From many of which kind of Experiments, it will likely not be hard to find out a method to calculate, what depth is to be concluded from any time of the like Globes stay under water: As for instance, if in the depth of 20 fathom, measured by the Line, the Globe stay under water 15 Seconds; then if the Ball stay 700 Seconds, the depth of the Sea is 933 fathom and 2 foot, if the Ball be found to move equal spaces in equal time.
In the same Trials made with this Instrument in the said River of Thames, it has been found, that there was no difference in time, between the submersions of the Ball at the greatest depth; when it rose two Wherry's length, from the place where it was let fall (being carried by the Current of the Tyde) and when it rose only a Yard, or so, from the same place, where it was let down: And that it must be so in great depths and stronger Currents, is as certain, as easy to be demonstrated.

And if it be alleged, that it must be known, when a Light Body ascends from the bottom of the Water to the Top, in what proportion of time it rises, it may be considered, that in this Experiment the times of the Descent and Ascent are both taken and computed together; so that, for this purpose, there needs not the nicety, which is alleged.

Of other Experiments of this way of sounding without a Line, made by the Noble Lord Viscount Browneker, Sir Robert Moray Knight, and Mr. Hook, in the Channel at Sheerness, the following Account was given, Vid.

**Weighed** | **Ounc. Grains.**
---|---
A Wooden Ball (A) | 32 1/6 00
Another Wooden Ball (B) | 30 22
A Lead (A) | 30 00
Another Lead (B) | 30 3/4 00

The Ball (B) and the Lead (B) were let down at 16 fathoms, and the Ball returned in 48 single strokes of a Pendulum, held in the hand, vibrating 58 single strokes in a Minute.

A second time repeated with the same success; therefore, the motion was 4 foot every second.

Again the Ball (A) and the Lead (B), whose Nail was bended into a sharper Angle; the Ball returned in 39 strokes. A second time repeated with the same success, at the same depth.

Ball (B) Lead (B) in which trial the Line, not being clear, stopped a little the motion; the Ball returned in 47 at the same depth.

M m m Ball
Ball (A) Lead (A) at 8 fathom and 1 foot, returned at 20; repeated at 8 fathom, returned at 19.

Tried the third time at 10 fathom and 4 foot, return'd at 28.

A fourth Trial, at the same depth, just the same.

A fifth, at 10 fathom, 5 foot, returned in 27.

A sixth Trial, just the same.

A seventh, at 12 fathom, 5 foot, in 37.

An eighth Trial, just the same.

Another Day, near the same place.

Note, That the Pendulum was this Day adjusted, and made a little shorter, there having been but 58 vibrations in a Minute, the other Day.

Ball (A) Lead (B) at 14 fathom, returned in 32½.

A second Trial, a little after in the same place, returned in 33.

In making of which Trial, the Vibrations were told aloud, and the Lead having been let down by a Line, was found to touch the bottom in just half the time, the Ball same under water. By a second Trial, the Ascending and Descending was found to be in equal times. And by a third Trial with another Lead, the very same was found, vid. 16½ descending, and 16½ ascending. This Lead and Ball let down without a Line, the Ball returned in 13 vibrations; a sign it went not to the bottom.

A Trial made with a Lead, whose Iron-Crook was fasten'd at the top of it (like that in the Figure 3.) succeeded very well, and the Ball returned in 34½. But by reason of the Current, the Experimenters could not perceive, when the Lead touched the bottom. This Lead being let down without a Line, the Ball returned in 32½. The depth of the water was now found by the Ships Lead, to be 14 fathom.

Another Trial was made with a Lead, bowing the point of the Lead (like that in the Fig. 4.) and the Ball return'd in 34. The same let down without a Line, the Ball return'd in 6 or 7 vibrations; a sign again, it went not to the bottom.

In a Trial with another Lead, the Ball return'd in 34.

Repeated again with the same success.
In a Trial with a Lead, whose Nail was set awry (like that of the Fig. 5.) the Ball returned in 34. After which Trial, the depth was found to be just 14 fathom.

The last Lead and Ball being let down without a Line, the Ball returned at 35.

In another Trial with a Lead that never failed, the Ball returned in 34, and the Lead toucht the bottom at 17.

By a Trial with another Lead, the same time was found exactly.

By a third Trial with this last, the very same.

These Trials were made near about High-water, at the depth of 14 fathom just by measure: And in them, the motions seem to be 5 foot every second.

In all these Trials, the greatest difficulty was, in the use of Conical Figures, with Iron Crooks, to bend the Iron, that it might be sure to carry down the Ball with it to the bottom, and when come thither, to let it go: for almost every one of these Leads failed in one of these requisites, till by several Trials they had been adjusted;

It is not to be omitted, That the last Trials being made near High-water, the Ball was found to rise (by the Boat, being permitted to drive) far off upon one side, out of the way, that any light thing, suffered to swim on the water, would be carried; which seemed to argue a motion of the under parts of the water, differing from that of the upper (a thing which is said to be at certain times of the Tydes, both at the mouth of the Sound, and of the Streights, which deserves to be further inquired into.) The Angle made by these different motions, seemed to be about 40 Degrees.
6. To keep a Register of all changes of Wind and Weather at all hours by Night and by Day, shewing the point, the Wind blows from, whether strong or weak: The Rains, Hail, Snow, and the like; the precise times of their beginnings and continuance: especially Hurricanes, and Spouts; but above all to take exact care to observe the Trade-Winds, about what degrees of Latitude and Longitude they first begin, where and when they cease or change, or grow stronger or weaker, and how much; as near and exact as may be.

The strength of the Winds is measured by an Instrument, such as is represented by Figure 6: which being exposed to the Wind, so as the flat side may be right against it, the number of Degrees upon the Limb $AB$, to which the Wind blows up, or raises, that flat side $CD$, shews the force or strength of the Wind, in proportion to the resistance of the flat side of the Instrument; and is to be recorded.
The Form of a Scheme,
Which at one view represents to the Eye Observations of the Weather, for a whole Moneth, may be such, as follows.

<table>
<thead>
<tr>
<th>Days of the Month and Place</th>
<th>Remarkable Hours of the Sun</th>
<th>Age and Sign of the Moon at Noon</th>
<th>The Quarters of the Wind, and its Strength</th>
<th>The Faces or visible appearances of the Sky</th>
<th>The Notablest Effects</th>
<th>General Deductions</th>
</tr>
</thead>
<tbody>
<tr>
<td>June</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>14</td>
<td>4</td>
<td>W - 2</td>
<td>Clear blue, but yellow in the NE</td>
<td>A great lowew,</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>8</td>
<td>-</td>
<td>Clouded toward the South</td>
<td>Thunderfar to the S</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>12</td>
<td>-</td>
<td>Checkered blue</td>
<td>A very great Tyde</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>24.6'</td>
<td>Perigeeum</td>
<td>WSW 1</td>
<td>Wind pretty constant between N. and W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>8</td>
<td>NW 3</td>
<td>A clear sky all day, Not by much but a little check-fo big a Tyde er'd about 4 P. as yesterday, M. At Sun-set A great Thun- red and hazy. Thunder-Showre from the N.</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>15</td>
<td>28</td>
<td>N W 3</td>
<td>Wind pretty constant between N. and W.</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>6</td>
<td>24.51'</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>12</td>
<td>-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>10 New Moon S</td>
<td>7.25 A. M.</td>
<td>Overcast and very lowing, &amp;c.</td>
<td>No Dew upon the ground, but very much upon Marble-stones, &amp;c.</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>16</td>
<td>10.8'</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>-</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
7. To observe and record all Extraordinary Meteors, Lightnings, Thunders, Ignes fatuos, Comets, &c. marking still the places and times of their appearing, continuance, &c.

8. To carry with them good Scales and Glass-Viols of a pint or so, with very narrow mouths, which are to be fill'd with Sea-water in different degrees of Latitude, and the weight of the Viol full of water taken exactly at every time, and recorded; marking withal the degrees of Latitude and Longitude of the Place, and the Day of the Moneth, and the Temperature of the Weather: And that as well of Water near the Top, as at a greater Depth.

The Viol is to be made with a very narrow Neck, and when it is almost full, water is to be dropt into it, drop by drop, till it can hold no more, drying well the Viol before it be weighed. The weight of the empty Viol is also to be recorded every time, weighing all to grains. And by evaporating gently the water, till the Salt be left dry on the bottom; they, who lift, may have the satisfaction to know, what proportion the Salt of each water holdeth to its weight.

There is, among some other ways of finding the different gravities of Water, a very pretty one, mentioned by some Authors, as Johannes Toldenus (a German Artist) Cabaus, and Kircher in his
his Mundus Subterraneus, and improved and first brought into use here, divers years ago, by the Noble R. Boyle, who also, as himself informed the Publisher, hath in some of his Writings, yet unpublish'd, set down a full Description thereof.

It is such a Glass-Tube as is represented by Fig. 7. blown at a Lamp, and poifed in good common Water by putting Quick-silver into it, until it sink to low, that nothing appear above the Superficies of the Water, but the Top, which done it is to be sealed up, and to be graduated on its side, into what parts you please; which may be done with a Diamond. And then, being put into any Water to be weighed, it will, by its more or less sinking into it, shew the differences of the Waters gravity.

9. To fetch up Water from any Depth of the Sea.

To perform this, let there be made a Square Woodden Bucket (such as C in Fig. 8.) whose bottoms E E are to be so contrived, that the weight A do sink the Iron B (to which the Bucket C is fastned by two Handles D D, on the ends of which are the moveable bottoms or Valves E E) and thereby draws down the Bucket, the resistance of the Water keeps up the Bucket in the posture C, whereby the Water hath a clear thorough-passage all the while it is descending: whereas, as soon as the Bucket is pulled upwards by the Line F, the resistance of the Water to that motion, beats the Bucket downward, and keeps it in the posture G, whereby the included Water is preserved from going out, and the Ambient Water kept from getting in.

By the advantage of which Vessel, or such like, you may come to know the Degrees of Saltiness of Sea-Water, according to its nearness to the Top or Bottom; or rather, the Constitution of the Sea-Water in several Depths of several Climates: Likewise, whether in some places of the Sea, there be any Sweet Water at the Bottom; the Affirmative whereof is to be met with in the East-Indian Voyages of Van Linschoten, who pag. 16, of that Work, as’tis Englished, records, that in the Persian Gulph, about the Isle of Babarem, they fetch up with certain Vessels (which he de-
describes not) Water out of the Sea, from under the Salt-Water, four or five fathoms deep, as sweet as any Fountain-water. And since'tis argued by some, that such Sweet-Water proceeds from certain Sweet Water-Springs, that were formerly on the Continent, at some distance from the Sea, and came afterwards to be covered by the Sea; it may be presumed, that in other places we may find the like. Besides, we know not, but that there may be in many parts, Eruptions of large Springs at the Bottom of the Sea, that were never taken in by any of its encroachments.

These Experiments are to be repeated every New Voyage, the multitude and frequency of them being necessary for finding out and confirming the truth of them; which as it will conduce exceedingly to the Enlargement of Natural Knowledge, so it may in time produce New and more accurate Sea-Maps and Cards; than hitherto have been published; and great helps and advantages to Navigation: especially those of the Variation, and Dipping of the Needle; the Depth and Saltiness of the Water; the Nature of the Ground at the Bottom of the Sea; and indeed almost every one of the rest; there being a Design to consider all, and to draw out of them such Rules and Directions, as may bring no less Honour, than Benefit to the English Nation.

The Instruments, described and represented in these Papers, may be had from Mr. Richard Shortgrave, Operator to the R. Society, to be found at Gresham Colledge; who also will be ready, if there be occasion, to give more particular Directions for the use of the same.

**ADVERTISEMENT.**

It is desired by Christopher Merret M. D. to inform the Publick, that within the space of four Moneths, he shall re-publish his Pinax Rerum Naturalium Britannicarum, with many Additions, and in his proposed New Method; and that he wholly disclaims the Second Edition of that Book, as being printed and published without his knowledge.

In the **SAVOT**,

Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar; and James Allestry in Duck-Lane; Printers to the Royal Society, 1667.
The Contents.

An Account of an Easier and Safer Way of Transfusling Blood, vid. by the Veins only. An Experiment of bleeding a Mangy, into a Sound Dog. An Extract of a Letter written by a French Philosopher, concerning the same Subject of Transfusion. Observations touching the Uniting of Barks of Trees cut, to the Tree itself. An Experiment of making Cherry-Trees, that have wither'd fruit, to bear good and full fruit. An Experiment on Aloe Americana Serratifolia weigh'd; seeming to infer a Circulation of the Sappe in Plants. An Extract of a Letter, about the Invention of Dividing a Foot into many thousand parts, for Mathematical purposes. More Wayes proposed, for the same purpose. Observations of the Star called Nebulofa, and of that in the Neck of the Whale. Extract of a Letter, concerning a New Discovery of the Communication of the Ductus Thoracicus with the Emulgent Vein. A Description of several sorts of Granaries, as those of London, Dantzick, Muscovy. Inquiries for Hungary, Transylvania, Egypt, Guiny.

An Account

Of an easier and safer Way of Transfusling Blood out of one Animal into another, viz. by the Veins, without opening any Artery of either.

This Way was first practised (for ought we know) by Doctor Edmond King, and the success thereof in two Experiments, communicated by him to the Royal Society, as follows.
1. I took a Calf and a Sheep, both of the larger sort, and having prepared a Jugular Vein in each, I planted my Pipes and Quills, as is usual, both in the Jugular Vein of the Calf (designed to be the Emittent) and in that of the Sheep (intended for the Recipient.) Then I took out of the Sheep 49 ounces (Haver de pois weight) of blood, before any other blood was let in; about which time, the company concluding the Sheep to be very faint, and finding the blood to run very slowly, I stopp'd the Vein of the Sheep, and unstopp'd the Pipe in the Calf, letting run out 10 ounces into a Porringer, which was done in about 40 seconds of a Minute. Then I convey'd Pipes from the Emittent Calves Vein, into the Recipient Sheeps Vein, and there ran a good free stream of blood for the space of 5 minutes (though perhaps less swift than the first 10 ounces.) And not to be deceived in the running, I did often strike with my finger the upper part of the emitting Vein, and thereby easily felt every stroke answered on the Recipient Vein, just like a Pulse. And now supposing that by this time (viz. the lapse of 5 minutes) the Sheep had received as much, if not more blood, than it had lost, we stopp'd the current of blood from the Calf, and closed also the Vein of the Sheep; and then having untied her, and set her down in the room, she went about and appeared to have as much strength, as she had before the loss of her own blood. Then resolving to bleed the Sheep to death, we bound her the second time, and open'd the emittent part of the Vein again; whereupon having bled about 60 ounces, she fell into Convulsions; and after the loss of about 5 ounces more, she died upon the place: and being dress'd by the Butcher, there did not, in all the usual places, appear above 3 ounces of blood; and the whole Sheep look'd of a lovely white; and the meat of it (to the taste of those, that eat of it) was very sweet.

The Sheep being dead, we resolv'd likewise to see the Calf bleed to death; but he having bled 10 ounces, and then for the space of 5 minutes more into the Sheep, and rested a good while, the blood by that time began to coagulate in the Vein; which made me open the Carotid Artery, letting thence run out about 25 ounces of blood, of a very lovely and vivid colour, vastly excelling
celling therein the blood of the Vein. The Calf, when dress'd, had, by the information of the Butcher, as little blood as the Sheep; and we saw him look whiter, than usually they do in the ordinary way of killing.

2. I took out 45 ounces and better, of blood, out of the Jugular Vein of a Sheep, of a lesser size than the former; by which time, the Spectators, as well as myself, found her exceeding faint, and some thought her past'd recovery, without a supply of blood. Then I convey'd blood from the Jugular Vein of a Calf into that of the Sheep, for the space of 7 minutes, when we did believe, by the continuance of a good stream from the Calf, that the Sheep had already received more blood, than she had lost. Whereupon we set her free, and she had no sooner got her liberty, but seeing a Dog near her (which was a Spaniel, that had formerly suffered the transmission of Sheeps-blood into him) she butted with great violence at him three or four times, not appearing at all concern'd at what she had endured in the Experiment. We keep this Sheep alive, she being sent to grass again, and seeming hitherto very strong and lusty.

The Calf was much larger than the Sheep. We bled the Calf to death, and received from him six Porringers full of blood, after the Sheep had been suppleid, each Porringer containing 11 1/2 ounces of water. The Sheep lost four of the same measures full of blood; which being supply'd by that of the Calf, we reckon, that the Calf lost 10 such measures in all.

An Account
Of another Experiment of Transfusion, viz.
of Bleeding a Mangy into a Sound Dog.

This was made by Mr. Thomas Coxe, and imparted likewise to the Royal Society in manner following.

I procured an old Mungrell Curr, all over-run with the Mainge, of a middle size, and having, some hours before, fed him plentifully with Cheese-parings and Milk, I prepared the Jugular Vein,

No 2
as we use to do the Carotid Artery of the Emittent Animal, not designing any thing further, than to determine by Experiment the Infection of the Recipient's blood. Then I made as strong a Ligature upon the Dogs Neck, as I durst, for fear of choaking him, to the end, that the Venal blood, which is much more fluggish in its motion and evacuation, than the Arterial, might be emitted with the greater advantage of Impetus.

Then I took a young Land-Spaniel, of about the same bigness, and prepared his Jugular Vein, as is usually done in the Recipient Animal; the heart-ward part of the Vein to receive the Maingy Dogs blood, and the head-ward part of it to discharge his own into a Dish.

Having thus prepared them both, and placed them in a convenient posture one to the other, I let slip the running knots, and by frequent compression of the Neck (besides the Ligature I had made) by reason of the tardy running of the Venal blood out of the Emittent, transfused about 14 or 16 ounces of the blood of the Infected, into the Veins of the Sound Dog, as near as I could guess by the quantity of blood, which ran into a Dish from the Recipient, supposing the Recipient Animal to lose near about the same proportion to what the Emittent supplies.

The effect of which Experiment was, no alteration at all, any way, to be observed in the Sound Dog. But for the Maingy Dog, he was in about 10 days or a fortnights space perfectly cured: which might with probability enough, I think, have been expected from the considerable evacuation, he made; (perhaps the quickest and surest remedy for the cure of that sort of disease, he was infected with, both in Man and Beasts.)
An Extract
Of a Letter of M. Denis Prof. of Philosophy and Mathematicks to M. * * * touching the Transfusion of Blood, of April 2, 1667.

This we English out of the 8th Journal des Scavans of 1667.

Viz.

Since the Experiments, of which I wrote to you the 9th of March, we have transfused the blood of three Calves into three Dogs, to assure ourselves, what the mixture of two such differing sorts of blood might produce. I shall hereafter acquaint you at large with the particulars; at present I shall only inform you, that the Animals, into whom the blood hath been transmitted, do all of them eat as well as before, and that one of these three Dogs, from whom the day before so much blood had been drawn, that he could hardly far any more, having been supplied the next morning with the blood of a Calf, recover'd instantly his strength, and shew'd a surprizing vigor.

We have found new ways of making this Transfusion with so much facility, that M. Emmerez undertakes to perform it without any Ligature, only by pricking, like that, which is used in Letting of blood.

Observations
Concerning the Uniting of Barks of Trees cut, to the Tree it self; made by Christopher Merret M.D. and read before the Royal Society January 9. 1666.

In the midst of March An. 1664. I made a Section of the Rinds of Ash, and of the Tree, falsely called Sycamor. The first Section of each of the Rinds was square, whereof three sides were
cut, the fourth uncut. The success was, that the whole Bark did unite, by binding it with pack-thred, leaving a scar in each of the sides cut.

Then I cut off and separated entirely from the Tree, several parts of the Bark, some shallower, leaving part of the Bark on; others to the very wood itself, both in the Trunk and Branches; from an inch square to less dimensions; and some of them I bound close with pack-thred: all which were separated, a new Rind succeeding in their place. Some I cover'd over, beyond the place of Incision, with Diachylon-plaister, and tied them fast with pack-thred. All which, thus bound and plaister'd, did within the space of three weeks, firmly unite to the Tree, not without some shriveling of the outward skin of the Bark, and also with some shrinking in each side, where the Incision was made; where also appear'd in each of the Interstices a scar.

The like Experiment I made, some years before, about the same time of the year, and succeeded as before related. But tying the same about Michaelmas, and in the Winter-season, at neither of these times any Union could be made of the Bark to the Tree. I suppose, it was, because the Sappe mounted not so vigorously and in such plenty, as in the Spring-season.

Some Branches of the fore-mentioned Trees were decorticated round, and where no Union was, there certainly follow'd a withering of the Branch beyond the place, where the Section was made.

I also separated a Twig from the Branch, by cutting off it sloping, for the better fastning of it to the Branch again. This Twig I exactly fitted to the Branch, from whence 'twas cut, in the same posture, it before grew in: I firmly bound it, and cover'd it with Diachylon-plaister. The success was, that in 3 dayes time, the Twig, that was cut off, withered.
An Experiment
Of making Cherry-Trees, that have withered fruit, to bear full and good fruit; and of Recovering the almost withered fruit.

Communicated by the same, as follows:

Anno 1665, I made the following Experiment with 3 May-Cherry-Trees (planted in a rich Mould) which lay to a South-wall, shaded 4 Winter-Months from the Sun by a high Building, so that the Sun came not on them, till the beginning of March, when being high, and shining somewhat fiercely upon them, the fruit constantly withered for some years before. Now this year, the season being very hot and dry, I bared the roots of one of them, by making a hole about it, and watered it every Morning and Evening with about a Gallon of water, for about a fortnight before the Cherries came to redness, and the fruit was full and good. The other two Trees, left without this ordering, had most of their fruit withered, having only skin and stones. Now to try this Experiment farther, I made a hole round about one of the other Trees, and fed it with water daily, as the former: In a Weeks time, those that were quite withered, fell off, and the rest, that were not so, grew and increased exceedingly, the other Tree, that was not used after this manner, had not any of its fruit come to perfection.

An Experiment
On Aloe Americana Serrati-folia weighed; seeming to import a Circulation of the Sappe in Plants, by the same Dr. Merret.

August 4, 1656. this Aloe weighed 21 Ounces, 6 Drams, 2 Grains. Its colour was of a pale-green, consisting of 11 Leaves; it was bound about with a red dry Cloth, and was hung up without Oil, as is usual, in the Kitchin.
So that in a whole year it lost 2 Ounces, 3 Drams, 24 Grains. The succeeding year being drier and hotter, it lost 3 Ounces, 2½ Scruples, and more than double in the 6 colder, than the 6 hotter Months. I kept it about 5 years, and it decreased much about the same proportion. And in the year 1660, hanging it in a colder Garret, it perished.

These Observables I had about it, that every Year two of the greater Leaves first changed Colour, and wither’d; and in the Spring-time, there grew out two very fresh and green ones, never amounting to the bigness of any of the precedent, insomuch that all this time I had the same number of Leaves. And then, these new Leaves were more fresh and green, and not serrated, and thicker also in proportion to their other Dimensions. Whence perhaps it may probably be inferred, vid. from the growth of these latter Leaves, that there is a Circulation in this Plant of the Success nutrient. For, how is it possible, that the Roots, continuing as firm and solid as at first, should supply so much nourishment, as to procreate new Leaves, unless it were from the return of the said Success, from the old and decaying Leaves, into the Root, and there protruded for the production of new ones? For, all Bulbous Roots, as Garlick, Onions, Tulips, and especially Squils, who protrude their Leaves, placed in a Shop or House, have their Roots lighter, and more spongy;
the leaves being formed out of the substance of the root, as a chick out of the albumen; in the mean while the whole decreasing in weight, as in the aforesaid aloe; as 'tis manifest by many experiments made by me.

An Extract
Of a Letter, written by Mr. Richard Townley to Dr. Croon, touching the Invention of Dividing a Foot into many thousand parts, for Mathematical purposes.

Finding in one * of the last Philosophical Transactions, how much M. Auzout esteems his Invention of dividing a Foot into near 30000 parts, and taking thereby Angles to a very great exactness; I am told, I shall be look't upon as a great Wronger of our Nation, should I not let the World know, that I have, out of some scatter'd Papers and Letters, that formerly came to my hands of a Gentleman of these Parts, one Mr. Gascoigne, found out, That before our late Civil Wars, he had not only devised an Instrument of as great a power, as M. Auzout's, but had also for some Years made use of it, not only for taking the Diameters of the Planets, and Distances upon Land; but had farther endeavour'd, out of its preciseness, to gather many Certainties in the Heavens; amongst which, I shall only mention one, viz. The finding of the Moons Distance, from two Observations, of her Horizontal and Meridional Diameters: Which I the rather mention, because the French Astronomer esteems himself the first that took any such Notice, as thereby to settle the Moons Parallax. For, our Country-man fully consider'd it before, and imparted it to an Acquaintance of his, who thereupon proposed to him the Difficulties that would arise in the Calculation, with considerations upon the strange Niceties, necessary to give him a certainty of what he desired. The very Instrument he first made I have now by me, and two others more perfected by him; which doubtless he would have infinitely mended, had he not been slain unfortunately in His late Majesties Service. He had a Treatise
life of Opticks ready for the Press; but though I have used my utmost endeavour to retrieve it, yet I have in that point been totally unsuccessful: But some loose Papers and Letters I have, particularly about this Instrument for taking of Angles, which was far from perfect. Nevertheless, I find it so much to exceed all others, that I have used my Endeavors to make it exact, and easily tractable; which above a Year since I effected to my own desire, by the help of an Ingenious and Exact Watchmaker in these Parts: Since which time, I have not altogether neglected it, but employed it particularly in taking the Distances (as occasion served) of the Circum-jovialists, towards a perfect setting their Motion. I shall only say of it, That it is small, not exceeding in weight, nor much in bigness, an ordinary Pocket-Watch, exactly marking above 40000 Divisions in a Foot, by the help of two Indexes; the one shewing hundreds of Divisions, the other, Divisions of the hundred; every last Division, in my small one, containing 12 of an Inch; and that so precisely, that, as I use it, there goes above 24 Divisions to a Second. Yet I have taken Land-Angles several times to one Division, though (for the Reason mention’d by M. Auzout) it be very hard to come to that Exactness in the Heavens, Viz. The swift motion of the Planets. Yet, to remedy that Fault, I have devised a Rest, in which I find no small advantage, and not a little pleasing those persons who have seen it, being so easy to be made, and by the Observer manag’d without the help of another: Which second Convenience, my yet nameless Instrument hath in great perfection, and is, by reason of its smallness and shape, easily applicable to any Telescope. Sir, If you think this Invention, thus improved, worthy to be taken notice of by the Curious, you may * command a more perfect Description of it, or any of the Observations, either from M. Gascoigne, or my self have made with it.
More Ways
For the same Purpose, Intimated by M. Hook.

I have by me (saith Mr. H.) two or three several ways of measuring the Diameters of the Planets, whether Horizontal, Perpendicular, or Inclined, to the exactness of a Second, by the help of a Telescope. As also, of taking the Position and Distance of the Small Fixt Stars one from another, or from any of the less bright Planets, if the Distance be not above two or three Degrees.

The Particulars hereof, the Author refers to the next Opportunity.

Observations
Of the Star, called Nebulosa, in the Girdle of Andromeda; and of the Wondrous Star in the Neck of the Whale: made and communicated by Monsieur Bullialdus.

Anno 1667. in January, when the Cloudy and Misty weather, which had continued for a good while, did permit us to observe the Star Nebulosa, in the Girdle of Andromeda (which may well enough be seen by the bare Eye) appeared much obscurer than the Year before. In the Months of February and March I did not see it.

Anno 1667, January 20. at Night, h. 6. 30'. the Sky being pretty serene, the Star in the Neck of the Whale, did approach to the bigness of a Star of the Sixth Magnitude, and grew bigger afterwards.

February 12, h. 6. 30'. I saw the same again, equalling now a Star of the Fourth Magnitude at least.

February 24, h. 7. This Star was equal to those of the Third Magnitude, shining very bright.

O 0 0 2 February
February 26, The fame appeared yet to increase; as also February 27. But after this time I could not see it, by reason of the ill weather.

The fame Astronomer did subjoin the following Extract of a Letter he received from Monsieur Hevelius, March 15. 1667. concerning such another Star; viz.

I have watched the New Star in the Neck of the Whale, as often as the weather would give me leave, which it hath done but seldom this Winter. In January, the 3, 4, 5, 7, and 13 days, it did not yet appear. From this time, the Sky was continually overcast, till January 23. on which day, I found a little Star of the Sixth or Seventh Magnitude, about the same place where the said New Star uses to appear. But it then seemed to me not the genuine New Star, but another, to wit, preceding the New; whose Longitude hath been defined by me in Mercurius in Sole visus in Aries, gr. 25. 43. 3". and the Latitude gr. 14. 41. 32". South, Anno 1660. Then from January 23, to February 2, it was Cloudy weather again; but this second of February, it appear'd very bright, and that, when the Moon shone, of the bigness of that in the Mouth of the Whale, or Nodo Lini: from which time I always observed it to grow bigger. March 13. I did still find it extremely bright, but could not by my naked Eye, because of the vivid Crepuscle, and the low site of the Star, accurately determine its Magnitude.

I have received (faith be further) your two Monita ad Astronomos; and the Discourse hath much pleased me, you having not much deviated from the Truth, in respect of the Appearance. Heretofore I had of this, and other New Stars, another Hypothesis; but I cannot thereby so accurately divine the Appearances, as you will read more largely in its due place.
An Extract

Of a Letter of M. Pecquet to M. Carcavi, concerning a New Discovery of the Communication of the Ductus Thoracicus with the Emulgent Vein: Taken out of the Journal des Scavans, N. VII. 1667.

I cannot forbear longer to inform you of the Experiments, which M. Perrault, M. Gayant, and I, made last Night upon the Corps of a Woman, that died some few days after she was brought to bed.

Our Design was to continue the Discovery of the Vessels, that carry the Chyle to the Breast, of which I have indicated the Way, pag. 134. of the Second Edition of my Anatom. Experiments, printed 1654. But the Body being not fit for that, we referred the search thereof to another time; and we have had the good fortune to make another Discovery, which may prove not less useful to Physick; it is the Communication of the Milky Channel, now call'd the Ductus Thoracicus, with the Emulgent Vein. The Experiments were these:

1. M. Gayant having discovered the Ductus Thoracicus upon the 7th and 8th of the Vertebra's descending from the Back, inserted a Quill into the said Ductus, and having tied it upon the Quill, he did blow into it; whereupon the Ductus was fill'd with wind from the Quill unto the Subclavial Vein. This wind issued at the Ascending Cava, which had been cut, when he, whose the Corps was, had lifted up the heart to make the demonstration of it, M. Gayant would tie this Cava, but it was cut so short, that the Ligature could not hinder the wind to issue out of it; which was the cause, that it could not be thrust as far as the Breasts. I would supply this defect, by compressing with my finger that place of the Vein, at which the wind came out (which was at about the third Vertebra, descending from the Back) and M. Gayant having blown
blown afresh into it, I compressed with my fingers the Vena Cava and the Ductus Thoracicus together; but the wind, that was thrust into this Channel, shewed us, that it had another way to escape. And indeed we saw as often as we did blow, that the Emulgent Vein was on the left side filled with wind, and that thereupon the body of the Vena Cava also filled itself from the Emulgent unto the Nisisces. This wind seem'd to us to come from the Left Kidney, and to infinuate itself into the Emulgent Vein, and thence into the Cava. The Right Kidney had been removed, so that we could say nothing of its communication with the said Ductus: That shall be for another time.

The Question was made, Whether the wind, that seem'd to enter into the Emulgent, and the Cava, did there enter indeed; or, whether it did not slide, betwixt the proper coat of this Vein, and that common one, which comes to it from the Peritoneum? This Question did oblige us to flit the Cava at the place of the Emulgent; and then blowing into the Ductus Thoracicus, we saw, that the wind, which had swelled the Emulgent, did escape at the opening, just now made in the Cava.

This Experiment made us judge, there was a communication of the Ductus Thoracicus with the Left Kidney, or at least with the Emulgent Vein, in the Body of this Woman. And to clear it the more, we made the following Experiment.

2. We lifted with the hand the Lungs, that filled the left Cavity of the Thorax, and having cleaned this Cavity with a Spunge, M. Gayant did blow into the Ductus Thoracicus, whilst I compressed the Vein and the Ductus with my fingers upon the third Vertebra, descending from the Back: And we saw the wind infinuate it self under the Pleura, by a trace, which raised it suddenly as often as we did blow. This trace appeared from the 4th Vertebra descending unto the Diaphragme, and made us conclude, that there was under the Pleura a Channel of Commerce coming from the Ductus Thoracicus, and passing to the Emulgent Vein by this Cavity of the Thorax. We could not doubt, but that this Channel, which passed under the Pleura, went as far as to the Kidney, because we saw, that the wind did get in on the side of the Kidney into the Emulgent Vein, and came out at the hole of the
the Cava, that had been made in the first Experiment. We perceived, that this Channel of Communication came from the Ductus Thoracicus, at the place of the fourth Vertebra of the Back. But to be the surer of it, we made the following Experiment.

3. I compressed with my fingers the Ductus upon the fifth descending Vertebra of the Back; and M. Gayant having blown into the Quill, which was upon the seventh, the wind passed not to the Kidney, nor to the Emulgent Vein. Which made us conclude, that the Communication was not beneath the fifth Vertebra. Then I compressed with my fingers the Ductus Thoracicus and the Vena Cava upon the third descending Vertebra; and the Emulgent swelled, when M. Gayant blew into the Quill; Which gave us more strongly to believe, That the place of the Ductus Thoracicus, whence goes the Channel of Commerce with the Emulgent, was between the third and fifth Vertebra of the Back; as the wind had informed us in the second Experiment.

To be yet more assured thereof, M. Gayant split the Ductus Thoracicus upon the third Vertebra of the Back, and having blown into it at the Quill, the wind came out at the Axillary Vein, and the Ascending Cava; but the Emulgent swelled not at all.

We made a fourth Experiment, which seemed very curious to us, and will not be mis to relate here, viz.

4. M. Gayant having blown into the Aorta, whereof all the branches, that had been cut, were tied up, it swelled immediately, and the Emulgent Artery grew tumid at the same time: but the wind, that was protruded through the Emulgent Artery into the Left Kidney, returned not into the Emulgent Vein; which taught us, that the Blood often passeth, where the Air does not.

We have an evident proof of it in the Kidney, since that the Blood of the Emulgent Artery, which goes to the Kidney, returns through the Emulgent Vein into the Vena Cava, pursuant to the Rules of the Circulation of the Blood; and that the Air propelled through the Emulgent Artery into the Kidney, comes not back through the Emulgent Vein into the Vena Cava.
We have yet another proof thereof in the Lungs, from the Experiment, we made of it in the Assembly upon the Corps of a Woman, that was there dissected in the beginning of February last; where we saw, that the Air, which was propelled thorow a Quill into the Vena Arteriosa (which is the Artery of the Lungs) returned not thorow the Arteria Venosa (which is the Vein thereof) into the Left Ventricle of the Heart, though, by the Circulation, the Blood pass there with ease, and even Milk, which having been let in by this Vena Arteriosa, returned easily thorow the Arteria Venosa, into the Left Ventricle of the Heart.

I draw no consequence from these Trials, as to the Channel of Communication, that passes from the Ductus Thoracicus into the Emulgent Vein; because one ought to infer nothing from one only Body. When we shall be certain, that this Channel of Commerce is found in Men, as well, as we have found it in this Woman, we shall then judge better of it. We are therefore going to make frequent Operations upon divers Animals, to see whether we shall there meet with any thing like it, to the end we may impart it to the Publick.

A Description
Of several Kinds of Granaries, as those of London, of Dantzick, and in Muscovy.

Concerning the Granaries of London, the Inquisitive Dr. Merret, (who indeed occasion'd the Inquiry into the rest, as a thing, which many were desirous to be informed about, for the better Preservation of Grain, in times of its Plenty) gives this Account of them.

All the Twelve Companies of London, and some other Companies and Private Persons, have their Granaries at the Bridge-House in Southwark (where are a Justice of the Peace, a Steward, and two Masters.) These Granaries are built on two sides of an Oblong; one whereof stands North and South, and is near 100 yards long, whose Lattice-windows respect North-East, the other side may be about 50 yards long; the Windows look to the North,
North, and the opposite sides have no Apertures. All the Windows are about a yard high, without any Shutters, and run on in a continued Series, with very small partitions, sufficient only to nail the Lettices to. Each of them is three or four Stories high. The Garret-windows are Jetty-wife, with a yards distance one from another, glazed out of the Tiles, The Ground or lowermost Story, 12 foot from the ground, is used only for a Warehouse, &c. To settle the first Story upon strong Pillars, fortified with Spikes of Iron, that no Vermin might get up, would make that Story firter for drying of Corn, and more perATTLE, especially where there is no use of the lower Rooms. The other Stories, made for Granaries, are in breadth some 6 yards, and in height 6 foot or somewhat more. The uppermost or Garret-Granary to the Top or Angle, made by the raising pieces, much more. They have each in the midst from the sides at 8 or 9 foot distance, a strong Post; and all the Timbers made very strong, to support and bear the great weight of the Grain. The Boards best made of round Oak, two inches thick, and close joynted. In some places they put, in all the inside of their Rooms, Iron-wire, of so narrow Metches, that neither Rats nor Mice can get thorow them, two or three foot deep. Others erect, on all the sides, Boards of Timber, andatten others to the top of the Perpendicular, one lying either parallel to the Horizon, or so that they make an acute Angle with the former, to the same purpose. For, besides the devouring of the Grain, the Excrements and Urin of that Vermine, moistning the Wheat or Rye, make them apt to corrupt and breed Weevils.

The two main Considerables in building these Granaries, are, To make them strong, and, To expose them to the most drying Winds.

The Ordering of their Corn is this, In Kent, to separate the dust and other impurities in it, when it's thrashed, they throw it in Shovels from one side to the other, which the longer it is, the better: by which means, all such impurities remain in the middle betwixt the two heaps of Corn; which they skreen, to part the Corn, that is good, from the said impurities: then, when they first bring the Grain into the Granaries, they lay it about half a foot thick, and turn it twice a week, and once in that time skreen it;
It, and this for two Moneths space. After that, they lay it a foot thick for two Months more, turning it once or twice a Week, and skreen it proportionably, according as the drying season is, seldom or oftener. After 5 or 6 Moneths, they raise it to two foot in height, and turn it once a Fortnight, and skreen it once a Moneth, as occasion is. After a Year, they lay it two and a half or three foot deep, and turn it once in three Weeks or a Moneth, and skreen it proportionably.

When it hath lain two Years or more, they turn it once in two Moneths, and skreen it once a Quarter, and so on, as they find it in brightness, hardness and dries. The oftner these two things are done, the better the Grain proves.

They leave an empty space about a yard wide on all sides of the Room, and at six foot distance, thorow the whole Area; empty of Corn, into which empty spaces they turn the Corn as often as 'tis needful.

In Kent they make two square holes in both the ends of the Floor, and one round in the middle; by which they throw the Corn from the upper into the lower Rooms, & contra, to air and dry it the better.

The Skreens are made with two partitions, to separate the dust from the Corn, which falls into a Bag, and when sufficiently full, is cast away, the good Corn remaining behind.

Corn has been kept in London-Granaries, 32 Years; and the longer 'tis kept, the more flower it yields, in proportion to the quantity of the Corn; and makes the purer and whiter Bread, the superfluous humidity onely evaporating.

Dr. Pell mention'd at a Meeting of the R. Society, that they keep Corn at Zurich in Helvetia, 80 Years.

So far the Doctor.

As for the Granaries of Dantzick and Moscovia, some observing Merchants and Travellers give this short Account of them.

First, That those of Dantzick are generally Seven Stories high, some, Nine Stories, having each of them a Funnel, to let the Corn run down from one Floor to another; thereby chiefly saving the labour and charges of carrying it down. And then, that they
in that Town, are built altogether surrounded with water, whereby the Ships have the conveniency of lying close to them, to take in their Lading. No Houses suffered to be built near them, to be thereby secured from the casualties of Fire.

Secondly, That those of Muscovy are made under Ground, by digging a deep Pit, of almost the Figure of a Sugar-loaf, broad below, and narrow at the top; the sides well-plaister'd round about, and the top very close cover'd with Stone. The people of that Countrey are so very careful, to have the Corn well dried, before they put it into those Subterraneous Granaries, that, when the weather of that Northern Climat serves not to dry it sufficiently, they heat their Barns, by the means of great Ovens, and thereby very well drying their Corn, supply the deficiency of their short Summer.

Inquiries

For Hungary and Transylvania.

In prosecution of the Engagement, published Numb. 23. p. 414, 422. we now subjoin some other Inquiries, and first these, that were very lately recommended to a studious and inquisitive Transylvanian, who from London returned to his Countrey, and promised to procure good Answers to the following particulars; Viz.

1. What is observable in Hungary, Transylvania, and the Neighbouring parts, as to Minerals, Springs, Warm Baths, Earths, Quarries, Mettals, &c. (Reference was here given to the Inquiries concerning Mines, printed Num. 19.)

2. Particularly, To inquire into the several sorts of Antimony, or Antimony-core, to be found in Hungary; and to inform us of the several places, whence they are digged, to the end, that they may be sent for;
3. To inquire, where the best Hungarian Vitriol is to be found, and the Cinabarum naturae?

4. To give us a true account of the right Gold- and Silver-Earth- or ore, said to be found at Cranach in Hungary, whence the Gold is called Cranach-Gold: first lighted upon by the care of the Emperor Rudolphus, and chymically wrought by his order and inspection?

5. To inquire after, and send over some of that kind of Vitriol, which by credible persons is affirmed to be found: crystallized in Transylvania. As also, after the Vitriol of Tyrol, said to yield Gold.

6. To inform us of the Salt-pits in Transylvania, said to yield two sorts of perfect Salt, the one being a Sal Gemmæ, the other, a common Table-Salt. To observe how deep these Salt-mines lie from the surface of the Ground? How deep they are digg’d hither-to; and what Damps are met with in them, &c?

7. To inquire after the Veins of Gold and Quick-silver at Cremenitz in Hungary, and the Vein of Silver at Schemnitz, in the same Kingdom: And to send over some of the best Ores of them?

8. To inquire, Whether the Waters of the Therma, that pass by Schemnitz, depose a certain sediment, which in time turns into a yellow Stone?

9. Whether in the Mines of Gold, Silver, Copper, Iron, Lead, in Hungary, there be generally found Quick-silver and Sulphur?

10. Whether it be true, that in the Copper-Mines of the place called Herren-ground in Hungary, there be found no Quick-silver at all?

11. Whether it be true, that in some parts of the Upper Hungary, the Ores of Copper, Iron, and Lead, be sometimes so commixed, that there is often found in the upper part of the Concrete, matter of Iron; in the midst, matter of Copper, and in the lowermost, Lead. And that in other places of the same Country, Copperish fluor are mixed with Leaden ones?

12. Whe-
12. Whether it be true, what Athan. Kircher writes from Relation, That the Ductus's of Mettals do sometimes run North and South, sometimes Cross-ways?

13. Whether there be in Hungary such a River, as is mentioned in Busbekius, whose water is so hot, and which is yet so ful of Fish, that he faith, one would expect, that all the Fish drawn thence, would come out boyled?

14. Whether there be Springs about Buda or Alba Regalis, that rise at the bottom of the River so hot, that those, who go to bathe, dare not put their feet so low as the Sand, for fear of having them parboyled?

15. Whether there be in Hungary an Avernus, that exhaleth al-
most always such poyfonous Steams, that Birds flying over it, do often-times fall down, either stupified, or quite dead?

16. Whether the Iron, that is said to be turned into Copper, by the Vitriolate Springs at Cremnitz or Smolnitz in Hungary, do after that Transmutation or Precipitation, contain a pretty deal of Gold?

17. Whether the Depth of the Gold-Mines of Hungary be 2400 feet?

18. Whether they find Trees, or any other Bodies in the solid Salt of their Salt-Mines.

19. Whether there be a great Lake in Moravia, whence of late years all the Waters were by accident drawn away, though formerly carrying Boats, and full of Fish?

20. Whether it be true, what is affirmed by Authors, That in some parts of Hungary near the Gold-Mines, the Leaves of the Trees have their lower superficies, if not their upper also, gilded over with yellowish Exhalations?

21. What is the way, said to be used in Austria and Hungary, of extracting the perfect Mettals out of their Minera’s without Lead; performed by casting a Powder upon the Minera, which makes a quick and advantageous Separation; Sulphur being supposed to be an Ingredient of it?

Inquiries.
Inquiries

For Ægypt, by Thomas Henshaw Esq.

1. Whether it rain there at any time, and if so, what time of the Year? Whether Rain makes the Air wholesome, or pestilential, or otherwise unhealthful?

2. To consider the Niter that is commonly sold there, and what affinity there is betwixt that, and our common Saltpeeter: to try by dissolving it in warm water, filtering it well, then boiling almost half away, and putting the remainder in an earthen Pan, and setting it in a cool place for two days, to see whether it will shoot into Crystals of Peeter. To send some quantity of it into Europe.

3. Whether the Earth of Ægypt, adjoining to the River Nilus, prefered and weigh'd daily, keeps the same weight, till the 17th of June, and then grows daily heavier with the increase of the River?

4. Whether, if the Plague be never so great before, yet on the first day of the Nile's increase, it not only increaseth, but absolutely ceaseth, not one dying of it after?

5. To inquire particularly into the manner of Hatching Eggs in Ægypt; How the Camel-dung is prepar'd, wherein they are laid: how often the Eggs are turn'd: how cover'd: Whether they hatch in Twenty one dayes, as they do with us under a Hen? Whether the Chickens be as perfect as ours? If imperfect, Whether that may not happen to them by rough handling, while they are remov'd, being very tender, out of the place where they are hatch'd: To take the design of the manner, how by the Pipes the heat is convey'd into several Rooms. How they treat them betwixt the time of their hatching, and taking away by the Owners? Whether they do not also use to hatch Eggs under Hens?

6. To
6. To inquire, Whether the great quantity of Yellow Amber, which is sold at Cairo, be by reason that it is the Gum of a certain Tree growing in Egypt, or Ethiopia, as Bellonius after Diodorus Siculus affirms? And whether, besides several Animals that are found inclosed in that Amber, there is very frequently some part of the Bark of a Tree found sticking to it?

7. To inquire of a certain Tree, growing not far from Cairo, which bears a Fruit stuffed with Wool, that is finer than Silk, of which the Arabs make Linnen that is softer than Silk, and whiter than Cotton?

8. Whether Crocodiles, that are found to be sometimes Thirty foot long, are hatched of an Egge, no bigger than a Turkeys?

9. Whether the Ichneumon, or Egyptian Water-Rat can kill a Crocodile, by skipping into his Mouth, and gnawing his way out, as Old Writers affirm?

10. Whether it be true, That the Arabs can Charm the Crocodiles, or, Whether there be on the Nile's side any Talismans, or Constellated Figures, beyond which the Crocodiles cannot pass, as some would make us believe?

11. To inquire at Cairo for several Drogues, which are common there, and much in use, yet not brought into Europe, as Acacia, Calamus Odoratus, Amomum, Costume, Ben Album, and divers such others.

12. Whether the Female Palm-Tree be not fruitful, unless she grow by the Male, as some would bear us in hand?

13. To inquire, Whether the Appearance of the Leggs and Arms of Men, related to stand out of the ground, to a great number, at a place five Miles from Cairo, on Good-Friday, do still continue? And how that Impositure is performed

14. Whether Children born there in the Eighth Month, do usually
usually live, contrary to what is believed to happen in other Countries?

15. To take an Account of the Wooden Locks there, which are said to be made with as great Art, as those of Iron, with us.

16. To observe the Course of the Waters, both of the Mediterranean, and the Red-Sea.

Inquiries
For Guiny, by Abraham Hill Esq.

1. Whether the River Niger overflows the Countrey yearly, like Nilus?

2. Whether the Rain, when it falls, be often very hot, roasting the Cloaths, if not presently dried, and breeding Worms in them?

3. Whether the Gold there, be of very different fineness, and that which is uppermost in the Mine be the finest?

4. Whether the Palm affords them Wine, Oyl, Vinegar, Soap, Bread; and out of the Leaves they pick Threads, making thereof very curious Works?

5. Whether they have, besides their Palm-Wine, a Drink made of Grain, like our Ale? What Grain that is, and how prepar'd?

6. Whether their Arrows, they make, be poysenous? By what Tree, and how prepar'd?

7. Whether some People on the River Gambra, be only Tawny, as others very Black?

8. Whether the Negroes have such sharp fights, that they discover a Ship at Sea much farther off, than the Europeans can?

9. What Reason there is to conclude, That the Common People being accustomed to drink Water, is the cause, that they are troubled with Worms in their Bodies, very painful to them; and difficult to get out?

FINISH.

In the SAVOY,
Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar, Printer to the Royal Society, 1667.
Fig. I. A Bridge in China.

Fig. II. China Wall.
PHILOSOPHICAL
TRANSACTIONS.

Munday, June 3. 1667.

The Contents.

Experiments for Improving the Art of Gunnery; To find out the Point-blank distance; the Quantity of Powder, for the just Charge of any Piece; and what Gun shoots farthest. An Answer to some Magnetical Inquiries, formerly published in these Transactions. Extract of a Letter from Paris, containing an Account of some Effects of Blood Transfused, and of two Monstrous Births, &c. A Relation of two other Monsters, not long since produced in Devonshire. Some Observations made in Mines, and at Sea, occasioning a Conjecture about the Origine of Wind. An Account of a great number of Stones, found in one Bladder. The Description of a Well and Ground, in Lancashire, taking Fire by a Candle applied to it. An Account of Athanasii Kircheri CHINA ILLUSTRATA.

Experiments for Improving the Art of Gunnery.

The better to determine the three Grand Desiderata, in the Art of Gunnery viz. 1. The Point-blank distance. 2. The Quantity of Powder for the just Charge of any Piece. 3. What Gun (for Size, Bore, Weight, Metal, &c.) shoots farthest: The following Experiments are proposed and directed by Sir Robert Moray; to give occasion to such as are Curious in this Art, to improve the same, as they shall have opportunity. Who we cannot but suppose will be so generous, as to impart the Successes and the Events of their Trials of this kind to the Publisher of these Transactions; for further Improvement and Use.

O Know, how Far a Gun Shoots Point-blank (as they call it) that is, so near the Level of the Cylinder of the Piece, that the difference is either not discernable, or not considerable: On a fit plat-form, place and point the Gun at a Mark, as large as the Bullet, some 50, 60, or more Yards distant, so as the under-side of the Mark may be in the same Level or
Line with the under-side of the Cylinder of the Peece. Then, between the Gun and the Mark at several places, place pieces of Canvas, Sheets of Paper pasted together, or the like, upon Stakes fixt in the ground, so as the under-side, being level with the Horizon, may just touch the Visual line, that passeth from the Eye to the upper-side of the Mark; when the Eye is placed in the Line, that passeth from it to the upper-side of the Cylinder of the Gun; the Canvas being so broad and long, that, if the Bullet pass through it two or three foot higher than the Level of the Mark, or of either hand, the hole it makes, may make it known, how much it flieth higher than the Level of that place. Such piece of Canvas, &c. may be placed; one, at half the distance between the Gun and the Mark; another, half-way between the first, and the Mark, &c. And if it be found, that even at so small a distance the Bullet falls lower than the Mark, if it touch not the Canvas, the Gun may be next time raised a little, and so on, till the Bullet hit the Mark, or as high as it: And if at first it fall as high as the Mark and cut the Canvas, the Mark and Canvas may be brought nearer the Gun, till it needs be done no more: Afterwards the Mark may be removed to greater and greater distances, till, to hit the Mark, it fly higher, than some or all the interpofed Canvasles: And thus the Experiment is to be repeated and varied at pleasure.

II. To know, what Quantity of Powder, is the just Charge of any Peece, so as it maketh the farthest Shot, and fires totally.

1. Raise the Gun to a mean Random, as of 20 or 25 degrees, and Shoot with the ordinary Charge of Powder, in some convenient ground, where the fall of the Bullet may be easily seen, and having made a Shot, measure the distance with a Chain, between the hole made by the Bullet, and the Muzzle of the Gun.

2. Then, instead of a full Charge of Powder used in the first Shot, take \( \frac{1}{2} \) part lefre, or some such proportion, for the next tryal, doing all things else as before.

3. For a third, fourth, or more tryals, diminish still the Quantity of Powder by \( \frac{1}{16} \) at a time, till the Shot be considerably shorter, than at first.

4. Then take \( \frac{1}{16} \) more than the first Charge, and do all things else as before; and so continue more tryals, increasing still the Quantity of Powder in the same proportion, every new tryal, till you find the increase of the Charge does not make the Peece Shoot.
Shoot further: Only over-Charge not so far, as to endanger
the Gun.

5. The right Charge found, the best Random is to be sought
by trying all Randoms, by degrees at a time.

To know, what Gun shoots farthest:

1. A Gun, to be prepared of Culverin-Bore (as being held
the best for Shooting far,) but much longer (double the Ordi-
nary length may do well;) is to be placed as in the former
Experiments, and charged with the Ordinary Charge of a Cul-
verin, or rather with that Quantity, which by the former Ex-
periments shall be found the best; and being Shot, the fall
of the Bullet is to be marked, and distance measured, as hath been
suggested.

2. Then try less, and more Powder in her, as before.

3. Then cut off two inches of the Muzzle with a Saw, and
try as before, doing every thing in the same manner: And so cut
off still for new tryals, till the Shot begin to fall shorter than
before.

4. The same may be done with Guns of different Bores.

Advertisements.

1. The way to accommodate the Canvas, &c. proposed for I.
finding out the point-blank-distance; is first to pitch two stakes
of the just height of the upper-side of the Cylinder of the Piece, 
some 6 or 8 foot asunder, in the straight line between it and the
upper-side of the Mark, by a long Ruler, having one end in the
Piece, after the Piece is duly point at the Mark; and then, by
the Eye looking over the Stakes to the upper-side of the Mark,
or rather by a Telescope, the Paper or Canvas interposed may be
let down, or placed just so, as the undermost side may seem to
touch the upper side of the Mark, to one that looks at it from
the top of the first Stake.

2. If this way of Experiment be made for further distances
and raisings of the Piece, as high as conveniently may be above
the Level, and the distances measured as hath been intimated;
and then all Randoms above these likewise tryed and measured,
the distance of an Object, to be Shot at, being known, and oth-
er necessary cautions, beneath to be mentioned, carefully ob-
serv’d, good Gunners may with great confidence undertake to
hit the Mark, be the distance what it will, so it exceed not the
reach of the Gun.

A a a 2

1. The
II. 1. The Experiments here proposed, are to be made in Guns of all Sizes, Bores, Weights, Metals, &c.
   2. Three or more Shot to be made with every different Charge, and at every several trial, that the certainty may the better appear.
   3. The first Shot being Measured and marked, the rest may all be Measured from it, or from one another, to save labour.
   4. The Gun is to be pointed, placed, and ordered every time in one and the same place and position, aiming still at the same Mark, or pointing still in the very same Line or Azimuth; that so all the Shot may fall in the same Line, as near as is possible.
   5. The Powder must be exactly weigh'd, every time the Piece is charged, left it having been weigh'd long before, the weight may be alter'd; though Experiment may be made with Cartridges and without.
   6. The Powder and Bullet is to be ramm'd home equally at every Shot; though the looser the Powder lye, it fire the better.
   7. When the right Charge of a Piece is found, that makes the farthest Shot in the ordinary and plain way of Charging, Monsieur de Sons contrivance of a Wedge may be try'd, to make it Shoot farther; which is a piece of Board, so long, as being thrust home to the Breech of the Piece at one end, the other may reach farther out than the outside of the Bullet, being ramm'd up to its place; broad about an Inch, and thin so far as the Wadd before the Bullet reaches on the outside; there it is to have a Shoulder, from which forward to the end, it is to be cut a flop like a Wedge, being of such thickness, as that at the place, where the Center of the Bullet is to be, it may make it stick so fast, that the Powder finding more resistance may at length drive it out with the greater violence.

8. Another of this nature is a Wooden Tampion, like a piece of a Cylinder, big enough to fill the hollow Cylinder of the Gun, the length somewhat more than the Diameter of it and hollow'd towards the Bullet, so as to fit it; and either flat, or (which is better) hollow likewise towards the Powder, and serving instead of a Wadd. These and such others will probably render the effect of the Powder greater, than otherwise it would be.

9. The Strength of the Powder must be examin'd by a Powder-Tryer, that raiseth a Weight, such as one as has been contriv'd by Mr. Hook, and is made by Mr. Shortgrave, Operator to the Royal Society.

10. The
10. The Powder used in a Set of these Experiments, ought to be all of the same goodness.

11. The same Bullet is to be made use of, if it can be had, till the Figure of it be marred; otherwise another as near of the same Size, Shape, and Weight, as is possible.

12. The Strength of the Wind, is to be observed at every time of Shooting; which may be done by an Engine, made by the lately nam’d Operator.

13. Observe also the Position of the Wind, with a Fane and Compass at every Shot.

14. Note also, at what Azimuth the Mark stands from the Gun.

15. Take precise notice, what effect the Wind hath every time upon the Bullet, in carrying further, in hindering, or turning it aside.

16. Note the Figure, dimensions, and Weight of the Gun, Carriage and Wheels.

17. The platform to be very Level.

18. The Wheels to be at every Shot placed in the very same place and position, to avoid inequalities.

19. Every thing to be exactly recorded in a Book, as also every Accident and Observation.

20. After all other Experiments are made, every Piece may be tried with the right Charge of Powder, laying every time more and more Weight upon the Carriage; and at last fixing the Gun so, as it may not recoyle at all, observing every time how far the Bullet goes, and how much less Powder than the full Charge will serve to Shoot the Bullet, when the Piece is fixt, as far as the whole Charge does, when it recoys freely.

21. Care is to be had, that the Experiments with the Wedge, Tampion, and the like, made for increasing the force of the Powder, and the fixing of the Piece, do not endanger it.

1. The Long Guns are to be made without any Ring about the Muzzle.

2. The pieces cut off from the Muzzle, to be always laid on the Carriage, when new shots are made, or their weight of Lead in a convenient Figure, that the recoyl may still be the same.

3. The Quantity of Powder, that Shoots furthest in an Ordinary Culverin being known, there needs no Variation of it in the Long one.
Answer to some Magnetical Inquiries, proposed Numb. 23: of these Transactions, Pag. 423, 424:

The Queries were these.

1. Whether a Needle may be so toucht upon any Magnet, as not to point to the true North and South? &c.

2. Whether different Load-Stones, will give different directions? or, whether fainter or stronger touches, upon one and the same Magnet, will cause any Variation in the directions? &c.

To these the Industrious Mr. Sellers returns this Answer:

To the First: That he had often made trial with many Needles touching them in each Hemisphere of the Stone, with all variety of ways he could imagine, to find, if it were possible by that means to cause any of these Needles to vary in its direction but, that he always found the contrary; all of them conforming to the Magnetical Meridian, and standing North and South, as other Needles, that were toucht upon the very Pole of the Stone. He adds, that some of these Experiments he tryed in London, when there was no Variation known.

To the Second: 1. That, upon frequent tryals of touching Needles upon different Load-Stones, of several bignesses, as also of different vertue; the several Needles, touched upon these different Stones, gave all of them the same directions. This he thinks is confirm’d by all the Needles and Sea-Compasses, made in several parts of the World, and consequently toucht upon several Stones of several Countries, yet all agreeing in this Magnetical Harmony, that they all give the same directions. 2. That having sometimes drawn a Needle, only over the Pole of the Stone, within the Sphere of its vertue, without at all touching the Stone; it hath received the same directive quality from the Stone, as if it had been really toucht upon the Stone itself, though not altogether so strong, as if it had toucht the Stone. Again, that having toucht Needles upon the Stone, with faint strokes, and other Needles with stronger; all these Needles received the same effect from the Stone, both for strength and direction; he conceiving, that ’tis not the fainter or stronger touches upon the Stone, nor the multiplicity of Strokes, that varies the Needles strength or direction; but that the Nature of the Steel, whereof the Needle is made, and the Temper that is given thereunto,
thereunto, causeth different effects, as to the strength it receiveth from the Stone; himself as having tryed all sorts of steel, that he could possibly procure, and all the different tempers he could imagine, for the most powerful receiving and retaining the vertue from the Load Stone; wherein he affirms to have fully satisfied himself, so that he can infuse such vertue into a piece of steel, that it shall take up a piece of iron of two ounces weight or more; and give also to a needle, the vertue of conforming to the Magnetical Meridian, without the help of a Load Stone, or any thing else, that hath received vertue therefrom.

So far this Answerer; whom as we cannot but much commend for his diligence in searching, and frankness of communicating; so we give these particulars to the publick, that further tryals may be made by others, for more discovery; hoping withall, that the same Inquisitive person, that hath made these returns, will not scruple to add to them the wayes, he uses for infusing that Magnetical vertue into steel and needles, without the help of a Load Stone, spoken of in the end of this his Answer.

Extract of a Letter, Written from Paris, containing an Account of some Effects of the Transfusio of Blood; and of two Monstrous Births, &c.

I was present, when M. Gayant shew'd the transfusion of the blood, putting that of a young dog into the veins of an old, who, two hours after, did leap and frisk; whereas he was almost blind with age, and could hardly stirr before.

In the House of M. Bourdelots was shew'd a Monster in form of an Ape, having all over its shoulders, almost to his middle, a mass of flesh, that came from the hinder part of its head, and hung down in form of a little cloak. The report is, that the Woman that brought it forth, had seen on a Stage an Ape so clothed: The most remarkable thing was, that the said mass of flesh was divided in four parts, correspondent to the coat, the Ape did carry. The Woman, upon inquiry, was found to have gone five months with Child, before she had met with the accident of that unhappy sight. Many questions were on this occasion agitated: viz., about the Power of Imagination, and whether...
whether this Creature was endow'd with a humane Soul; and if not, what became of the Soul of the Embryo, that was five months old.

A little after, another Monster was produced, which was an Infant come to maturity, having instead of a Head and Brains, a Mass of flesh like any Liver; and was found to move. And this Fetus occasioned a Question for the Cartesians, how the motion could be performed, and yet the Glandula pinealis, or Conarium be wanting; nor any Nerves visible, which come from the Brain? The marrow in the Spine was of the same substance. It liv'd four days, and then dyed: It was anatomized by M. Emmerez in presence of the Assembly.

There came a Letter from Florence, Written by M. Steno, which has also somewhat perplexed the followers of Des Cartes. A Tortoise had its head cut off, and yet was found to move its foot three days after. Here was no Communication with the Conarium: As this seems to have given a sore blow to the Cartesian Doctrine, so the Disciples thereof are here endeavouring to heal the Wound.

An Account of two Monstrous Births, not long since produced in Devonshire; communicated by M. Colepresse.

1. One Robert Cloak, a Joyner (whom I know) of Clamick in the Parish of Beer-Ferris in Devonshire, had on Febr. 24. last, a Monstrous black Ram-Lamb fallen with one Head, but two distinct Bodies, with eight Legs; which Bodies were joyned in the Neck: It had two Eyes, and as many Ears, in the usual places; and one extraordinary Eye in the Niddock, with one single Ear, about an inch distant from the Eye backwards: Its Dame, which was White, usually brought forth two Lambs every year, as she did this year also a White one, which with the Ewe remains alive. But whether this Monster was produc'd dead or alive, is not known, it being found dead by the hedge, and soon after put into the Earth. There were ten White Ews accompanied with three White Rams.

2. One John Cauce, Servant to Mr. William Knighton of Lockridge, likewise in Beer Ferris; had among his Sheep, on the seventeenth of Febr. last, a White Lamb fallen on a Common in the said Parish, with two distinct heads and Necks, joyned at the Shoulders.
shoulders, but one only Body, and that well form'd, yet having double entrails in all respects. The Ewe remains well. The Monster dyed, and is now in my Custody, after it hath been dried in an Oven, and by the Sun.

Some Observations made in Mines, and at Sea, occasioning a Conjecture about the Origine of Wind: Imparted by the same Mr. C.

One John Gill, a Man well experienced in Mineral affairs, discoursing with me about the Wind and its Origine; declared to me his thoughts concerning the same, as a result of twenty years Experience and Observations of his own.

First, He affirmed, that if in digging deep under ground, the Work-men meet with Water, they never want Air or Wind; but if they miss Water (as sometimes it happens, even at 12 or 16 Fathoms depth) they are destitute of convenient Air, either to breath in, or to make their Candles burn.

Next, When (as usual) there happens to be a great quantity of a Winters standing water, in a deep Mine, they commonly bring, or drive up an Adit for drawing away such water: But as soon as that part of the Level is made, that any of the standing Water begins to run away, the Men must secure themselves, as well as they can, from danger of being dash'd in pieces against the sides of the Adit: For the included Air or Wind in the standing Water, breaks forth with such a terrible noise, as that of a Piece of Ordnance, and with that violence, as to carry all before it, loosening the very Rocks, though at some distance in the Work or Adit.

Thirdly, he hath observed on several occasions going to and fro, between London and Plymouth, by Sea, that being in a Calm, that way, which the Sea began to Loom or move, the next day the Wind was sure to blow from that point of the Compass, towards which the Sea did Loom the day before.

An Account of Hail-Stones of an unusual bigness, Communicated by D. Nath. Fairfax, with his Reflections on them.

This Account came but very lately to hand, though the thing hapned a while ago; the Ingenious Author thereof, having but newly entred into a Philosophical Correspondence with the Publisher.

July, 17. 1666. About 10 in the Forenoon, there fell a violent storm
Storm of Hail about the Coast-Towns of Suffolk, tracing along Seckford-Hall, Wood-bridge, Snape-bridge, Aldborough, &c. more to the North-ward. The Hail was small near Yarmouth; but at Seckford-Hall, one Hail-stone was found by measure to be 9 Inches about. One of this Town (viz. Wood-bridge) found one at Melton, 8 Inches about. At Snape-bridge a man affirm’d, that he lighted on one about 12 Inches about. A Lady of Friston-Hall, putting one of them into a Ballance, found it weigh 12 s. 6 d. Several persons of good credit in Aldborough affirm’d, some Hailstones to have been full as bigg as Turkeys-Eggs; (an ordinary Hens-Egg weighs about 9 s.) J. Baker of Rumborough, driving a Cart on the Heath by Aldborough, had his head broken by the knocks of them through a stiff Country-felt. In some places his head bled; in others, bunnyes arose: The Horses were so pelted, that they hurried away his Cart beyond all command. They seem’d all white, smooth without, shining within. ’Tis somewhat strange, methinks, that their pillar of Air should keep them aloft, if they were not clapt together in the falling; especially at such a time of the year, when the Air is less thickened and its Spring weaker.

Account of a great number of Stones, found in one Bladder, by the same.

Mr. Goodrick Chirurgeon of Bury St. Edmunds affirm’d to me, that himself Cutting a Lad of the Stone (for which he hath a great name) took out thence, at one time, 96 small Stones, all of them of unlike shape, Size, Corners, Sides; some of which were so beflow’d as to slide upon others, and had thereby worn their flats to a wonderfull slininess. He affirm’d me also, that in the same place, another, when dead, had a Stone taken from him, almost as big as a new-born Childs head, and much of that Shape.

The Description of a Well, and Earth in Lanchashire, taking Fire by a Candle approached to it.

This was imparted by that Ingenious and Worthy Gentleman, Thomas Shirley Esq; an Eye-witness of the thing, now to be related in his own words: viz.

About the later end of February 1659, returning from a Journey to my house in Wigan, I was entertained with the relation of an odd Spring, situated in one Mr. Hawkley’s Ground (if I mistake.
(483)

(thake not) about a mile from the Town, in that Road which leads to Warrington and Chester.

The people of this Town did confidently affirm, that the Water of this Spring did burn like Oyle; into which Error they suffered themselves to fall for want of a due examination of the following particulars.

For when we came to the said Spring (being five or six in company together) and applied a lighted Candle to the surface of the Water; 'tis true, there was suddenly a large flame produced, which burnt vigorously; at the sight of which they all began to laugh at me for denying, what they had positively asserted: But I, who did not think my self confuted by a laughter grounded upon inadvertency, began to examine what I saw; and observing, that this Spring had its eruption at the foot of a Tree, growing on the top of a neighbouring Bank, the Water of which Spring fill'd a Ditch that was there, and covered the burning place lately mention'd; I then applied the lighted Candle to divers parts of the Water, contained in the said Ditch, and found as I expected, that upon the touch of the Candle and the Water, the Flame was extinct.

Again, having taken up a dishfull of Water at the flaming place, and held the lighted Candle to it, it went out. Yet I observed that the Water at the burning place did boyle, and heave like Water in a Pot upon the Fire, though my hand put into it perceived it not so much as warm.

This boyling I conceived to proceed from the Eruption of some bituminous or sulphureous Fumes; considering, this place was not above 30 or 40 yards distant from the mouth of a Coal-pit there. And indeed Wigan, Ashton, and the whole Country, for many miles compass, is underlaid with Coal. Then applying my hand to the surface of the Burning place of the Water, I found a strong breath, as it were a Wind, to bear against my hand.

Then I caused a Dam to be made, and thereby hindering the recourse of fresh water to the Burning place; I caus'd that, which was already there, to be drained away; and then applying the burning Candle to the surface of the dry Earth at the same point, where the Water burned before; the Fumes took fire, and burn'd very bright and vigorous. The Cone of the Flame ascend'd a foot and a half from the Superficies of the Earth. The Basis of it was
was of the Compass of a Mans hat about the brims. I then caused
a Bucket-full of Water to be poured on the fire, by which it was
presently quenched, as well as my companions laughter was stop-
ped, who then began to think, the Water did not burn.

I did not perceive the Flame to be discolour'd, like that of sul-
phureous Bodies, nor to have any manifest scent with it. The Fumes,
when they broke out of the Earth, and prest against my hand,
were not, to my best remembrance, at all hot.

Account of Athanasii Kircheri CHINA ILLUSTRATA.

The Author by publishing this Volume, discharges the Promise,
he had made some years ago, that he would do so. He acknowled-
ges himself much obliged to Martinus, and his Atlas Sinicus;
as also to Michael Boim, a Polonian; Philippo Marino, a Jefuit of Ge-
noa; and two other of the fame Society, viz. Henry Roth of Aus-
burg, and John Gruber, an Austrian; whereof the latter went A.
1656. over Land from Rome, through Anatolia, Armenia, Persia,
Ormus, Cambaja, and India, to Macao, the famous Port of China, and
thence to Pekin, the Court of that Empire; whence two years
after, he came back to Rome, accompanied for a part of the way,
by the Jefuit Albert Dorville; traversing by Land in a manner the
whole breadth of China, and a great part of the confining Tartary,
and so further, through the Moguls Dominions, to Agra, where the
said Dorville dying, the above mentioned Henry Roth supplied his
place in accomplishing this Voyage.

The Book it self, a large Folio, is divided into 6. Parts.
The three first, and the last, being besides the design of these
Tracts, we shall but glance at, taking only notice; First, That they
pretend to perswade the Reader, that Christianity was spread over
all Asia by St. Thomas the Apostle, and his Successors; and hath
been there continued, though not without great Eclipses, to these
very times. And here the Chino-Chaldean Monument, said to have
been erected several hundred years since in China, and found out
A. 1625. is with great labour asserted and interpreted. Next,
That the Rife of the Idolatry, in those remote parts, and their
different Ceremonies in Worship, is confronted with those An-
cient ones of Egypt. Lastly, that a large Account is given of the
Chinese Letters, their Figure, Power, &c.

But we haften to the Fourth Book, as belonging to our Sphere.
That undertakes to describe the Curiosities and Productions of Nature and Art, in China. Here, the Author having premised something of the advantageous Situation of China, and its Political Government; Calculated also both the Number of its Inhabitants, (which according to him, amount to 200 Millions of Men, besides Women, Children, Officers, and Eunuchs;) and the Annual Revenue of the Emperor (which he makes to be 150 Millions of Gold-Crowns;) he relateth many considerable productions and works of Nature in that Country. As

1. Mountains very odd for shape, burning, and raising of Tempests.

2. Isles, to the number of 99, all turned into one, under the same extent of space they had, when they were divided by water.

3. Lakes, some changing Copper into Iron, and causing Storms, when any thing is cast into them; and others, sprung up by Earth-quakes.

4. Rivers, whereof one is said to be of a Blew colour in Autumn, and for the rest of the year Limpid; Another, to be cold at the top, and very hot beneath.

5. Fiery Wells, serving to boil meat over: Perhaps of the same Nature with that here in England, we described above.

6. Plants, as 1. some Roses, changing their Colour twice a day: Whence the Author takes occasion to speak of that Plant, which grows at Rome, in the Garden of one Signior Corvino, call'd Vioia Nocturna, changing its colour sensibly, according to the degrees of the rising and declining of the Sun; destitute of all smell in the day-time, but having a very fragrant one in the night. 2. A Farinaceous or Mealy Tree, serving to make Bread of it. 3. Leaves of certain Trees, standing on the side of a Lake, which falling into the water, become like black Birds: which he ascribes eth to the Seminal parts of some Eggs, broken on those Trees, fill'd with Birds' nests. 4. The, and its wholesomeness, as to the suppressing of Vapours, and preventing the Stone. 5. A kind of Wicker-Tree, which, as if it were a Rope twisted by Nature, about an inch thick, creeps along upon the Earth, sometimes the length of 120 paces, much embarrassing the way; but serving for Cables to Ships, Seats, Hurdles, Beds, Matts; enduring no Vermin; and being cool and refreshing in hot Seasons. 6. The Calambo-wood; that it is esteem'd by some to be a kind of Lentiscum.
tisicum,' by others, a sort of Terebinth, but of a nobler rank, by virtue of that Climat: which makes the Author suggest, that care should be taken to have it brought into Europe, and carefully cultivated there. 7. Rhubarb; of which he observes, that, because the virtue of its Roots, if they be exposed to dry hastily, soon evaporats; therefore the skilfull, lay them upon a Table within doors, and turn them several times a day, to incorporate and fix the Juyce the better, and then string them and expose them to the Wind, in a shade, altogether free from the Sunbeams. 8. Pine Trees; of which he faith some are so big, that eight Men can hardly Fathom them. 9. Canes, so big, that they can make as many Barrels of them, as they have internodes or Joynts. 10. Trees, sweating a Gum, call'd Cie, like the dropings of Turpentine; which Gum, as long as tis not dryed, emits a very wholsome and dangerous Steam. To passe by the Polonie Tree, producing fruit without any blossoms, immediately out of its Trunk, as big as one man can well carry; and that kind of Fig-Tree, that bears Leaves as big as to wrap up a man in, &c.

7. Animals, Here he discurseth of the Musk Dear, and the several Compositions of Musk: the Sea-Horse, and Wild Men: Of some Birds, no where seen but in China (as he thinks) and among them a Wool-bearing Hen: Of Fishes, in Summer flying out of the Sea, seeking their food, like Birds, and in Autumn returning to the Sea: particularly of a Fish of a very exquisite taste, called Hoancio-yu, or the Croceous Fish: Further, of Sea-Cows, going often ashore, and fighting with the Land-Cows: Of Bats, of a vast bigness, eaten by the Chinese as a delicious meat: Of the Serpent, that breeds the Antidotal Stone; whereof he relates many experiments, to verifie the relations of its virtue: Which may invite the Curator of the Royal Society, to make the like trial, there being such a stone in their Repository, sent them from the East Indies. Again, of Silk Worms, spinning twice a year, and yielding a double Crop.

8. Fossils, where occurs the Relation, 1. Of an odd Specular Stone, representing the figure of the Moon in all her Appearances, when exposed to Her. 2. Of an Earth called Quei, very Cosmetick, and absterfive of all blemishes of the face. 3. A Mineral cerussè, blended of Lead and Antimony. 4. Of Asbestos, that can be drawn and spun; the way of which he affirms to have described L. 12. Mundis sui subterranei. 5. The Matter that makes Porcelain, which he affirms to be nothing else but a transparent Sand,
which they soak in water, and then reduce to a Maffe or Dough, and so bake it. Not a word of the way of giving it the colour, which, it seems, they keep as a great secret. They have Gold and Silver Mines, but dig them not, pretending the danger and trouble in the work, and contenting themselves with the Filings and Dust of Gold, which they gather out of the Mud and Sand of Rivers and Fountains.

The Fifth Book contains an account of their Works of Architecture, and other ingenious Mechanick Arts. Where he speaks, 1. Of their stupendous Bridges, one of 360 Perches long, and 1 ½ Perch broad, without any Arch, standing upon 300 Pillars, with acute Angles on both opposite sides, all the stone being of an equal size and shape. Another, built from Mountain to Mountain by one only Arch, 400 cubits long, 500 cubits high (whence tis called Pens volans) from the surface of the Saffrany River, running under it. This is represented, for the satisfaction of the Curious, by Figure I.

2. Vaft Towns, but whose Houses are generally but one Story high, and good reason therefore, the Towns should be very big. They are, for the most part, built of Timber.

3. Turrets very artificial, whereof one is all of Porcellan.

4. The China Wall, 500 German Leagues long; 30 Cubits high, 12 (in some places 15) Cubits broad, so that 12 Horses can very conveniently go in front on it; built 215 years before Christ, by the Emperour Pius, a brave and most Warlike Prince, and dispatch in the space of five years; commonly it is defended by a Million of men. A Pattern of this also was thought fit to exhibit here, by Figure II.

5. The Channel, that passeth from one extremity of China to the other, having some 24 sluices, to retain water, when tis necessary; a work of incredible industry and extraordinary advantage.

6. Vaft Bells, one whereof, at Pekin, weighs 120,000 pounds; whereas that of Erfurdt in Germany, hitherto esteemed to be one of the biggest in the World, weighs but 25,400 pounds.

As for their ingenious Inventions, this Author mentions chiefly,

1. Their Vernice, of which he sets down some Receipts both for the Red and Black, together with the way of their Use and Application, as he received them both from an Augustinian Friar; affirming, that it differs not at all from that of China.

2. Their way of Printing, invented long before that in Europe, giving a large description of the same.
3. Gunpowder, which he also faith, they had before the Europeans.

4. Bell-founding.

These are the principal Subjects treated of in this Book. We passe by severall Stories, which seem much to require confirmation. E.g. That of Sugar-Canes, eaten by an Elephant, and taking root in his stomach; that of Boys eating Serpents with as much greediness, as others eat Eels, or any good meat, &c.

Among the Cuts of this Volume, there is a Map of Asia, not un-instructive; delineating the way, the two Jesuits took in their Land-voyage from Pekin to Goa; as also that, which the Muscovian Ambassadors, not many years since, took in travelling from their Country, through the vast Tract of the Northern Tartary, to China, arriving on the North side of the China Wall at Pekin: Item, The Land-passage, heretofore made by B. Goes (described by Rigaultius) from Persia, by Labor in the Mogols Empire, through the Kingdoms of Cabul, Zancut, &c. to China; or (which is all one to this Author, as it is to several others) the Province of Pekin in China. Item, The passage of Paulus Venetus over Land, out of Europe into the same China; and lastly, That pretended one of St. Thomas, out of Palestina, through Syria, Mesopotamia, Persia, the Mogols Empire, the Pen-inful between the Bays of Cambaya and Bengal, to Maltapur, on the Coast of Coromandel, where the Name of the Christians of St. Thomas is still in request.

L O N D O N,
Printed by T. R. for John Martyn, Printer to the Royall Society, and are to be sold at the Bell a little without Temple-Barr, 1667.
A LETTER

Concerning a new way of curing sundry diseases by Transfusion of Blood, Written to Monsieur de MONTMOR, Counsellor to the French King, and Master of Requests.

By J: DENIS Professor of Philosophy, and the Mathematicks.

Munday July 22. 1667.

SIR,

The project of causing the Blood of a healthy animal to pass into the veins of one diseased, having been conceived about ten years ago, in the illustrious Society of Virtuosi which assembles at your house; and your goodness having received M. Emmeriz, & my self, very favorably at such times as we have presum'd to entertain you either with discourse concerning it, or the sight of some not inconsiderable effects of it; You will not think it strange that I now take the liberty of troubling you with this Letter, and design to inform you fully of what pursuances and successes we have made in this Operation; wherein you are justly intituled to a greater share than any other, considering that it was first spoken of in your Academy, & that the Publick is beholding to you for this as well as for many other discoveries, for the benefits & advantages it shall reap from the same.

But that I may give you the reasons of our procedure & con-
vince you that we are not culpable of the rashness wherewith we may possibly be charged: be pleased to permit me to relate to you in few words the reasons alleged by some persons, to oppose our design, and to compare the same with those which engag'd us to execute, and by certain degrees carry it on to that perfection wherein it is at present.

You have heard of the tryal we made about four moneths agoe upon Dogs, to transmit the Blood of the Crural Artery of one into the Jugular Vein of another. And being this operation even at the first attempt succeeded as happily as we could wish, according as it is more at large set forth in the Journal des Seavans, of March last; we were encouraged to repeat the same several times both in publick and private, and we added so many circumstances to the manner of performing it, that its easiness seemed to invite us not to neglect it, but to make abundance of Observations which might be of some benefit in Practice.

Accordingly, We afterwards made the Transfusion several ways, sometimes from an Artery into a Vein, sometimes from one Vein into another, both in those of the Neck, and those of the Leg; in Dogs both weak and strong, great and small; as also in such as had already either received or communicated blood in former transfusions. And not finding one of nineteen to die, but on the contrary always observing some surprising effect in all such as had received new blood, we were strongly persuaded that the transfusion would have no such dangerous consequences, as some people endeavor'd to preface.

Wherefore we resolv'd to drive the business yet a little further, and not contented to have seen it happen well in subjects of one and the same species; we thought good to try it in some of a different species, and accordingly we took a Calfe and a Dog; because we conceiv'd the blood of these Creatures altogether dissimilar. In your presence we transmitted a Calf's blood, into a Dogs Veins, on the 28. of March; since which time we have also done the same upon other occasions, always adding somewhat new towards the facility of the Operation.
ration. Yet in all these Experiments we never could observe any indisposition in the Dogs which receiv'd the Blood; whence we became confirm'd in the Opinion, that there was more ground to hope effects rather advantageous than hurtful to mankind, from this discovery of Transfusion of Blood.

Nevertheless, that we might vent nothing unadvisedly, we publish these Experiments, and were well pleased to understand the judgment of the ablest Philosophers and Physitians, upon the matter we examin'd, whether any of them had reasons weighty enough to dissuade us from carrying on our trials even in Man; and I beseech you permit me to sum up the principal here which are come to my knowledge, and judge your self whether they ought to have stopped us, and whether we deserve praise or blame for having proceeded further. Some have attack'd this design by decrying it as Chymerical & impossible. 1. Because the diversity of Complexions (which is founded in the Blood) supposeth so great a diversity in the several Bloods of different Animals, that 'tis impossible but one must be Poison and Venome in respect of another. 2. Because Blood entrajafated or remov'd out of its natural place, must necessarily corrupt, according to the dictate of Hippocrates. 3. Because the Blood issuing out of its proper Vessels, and being to pass through inanimated conveyances, such as are the Pipes or Tubes employ'd in the Transfusion; it must infallibly coagulate, and so coagulated descending to the heart must cause there a Palpitation, whereof death will be the speedy consequent.

These Reasons have not appear'd to us of great importance. For as to the first, we acknowledge that there are as many different complexions and various qualities in the blood, as there are Individuals in every Species; But we believe it not conclusible from thence, that the Blood of one must be Poison in respect of others; no more than it follows, that all the Meats wherewith we are nourish't, and whereof our Blood is only an Elixir or Quintessence, must corrupt and poison the same, because they are endu'd with qualities differing from those
those of the Blood. On the contrary, as 'tis certain that cooling Meats or Medicaments serve to temper the heat of the Blood when it boiles in the Veines, and hot food or Physick excite new vigour in such blood, as old age or a Disease hath render'd torpid and coagulated in the Vessels; so 'tis as reasonable to infer, that great advantages will follow upon the mixture of different bloods, provided the same be directed and manag'd by the ordinary prudence of able Physitians; and that if a hot Blood can reinfuse new strength into that which languisheth with coldness, such blood as hath colder qualities, may also check the Ebullitions and Tumults of that which is overmuch chafed.

As for the authority of Hippocrates, who faith that extravasated blood corrupteth, It is no wise repugnant to our pretensions. For to salve the honor of that great Person, his meaning must be rightly expounded, which is no more but this; that the Blood necessarily corrupteth when it is out of the Vessels, which preserve its heat, and which permit it a natural motion and free circulation, whereby it purifieth itself continually. For example, being extracted into a dish, wherein all its parts are stoppt and coagulated, it must in a little time alter its constitution, as experience witnesseth. Nor doth this hinder but that it is also corrupted sometimes in its own place, contrary to that Aphorisme, Quicquid corrumpit, in loco alieno corrumpitur: For if its motion be interrupted in the Veines either by some obstruction form'd therein, or some division occasion'd by a Wound or Contusion, it corrupteth in a short space and degenerateth into purulent matter. But in this Experiment of Transfusio, the Blood is communicated in such manner, and passeth out of one animal into another, that 'tis impossible its natural motion should be interrupted. 'Tis true it passeth through unnatural Conduits, but these being once warmed and admitting no air into their cavities, they cause no more alteration in the Blood than the Arteries and Veines themselves do.

Moreover, such as conceive that the blood Transfused, must
must needs coagulate by the way and afterwards cause some mortal palpitation; they foresee an accident which never happened to us, and against which I desire no other security than the experience we have had of all the animals hitherto employed by us, which are still living.

Others who have either been witnesses of some of our Transfusions, or have understood the same from credible relations, dispute not the possibility of the thing; but yet not to authorize appears new, they say, That whatsoever care and caution be used in the Transfusion, it can never be practis'd upon Man with success; and these are their principal Reasons.

First, The blood of a sound, and the blood of a diseased body having qualities very different, the one being pure, the other impure, a perfect mixture thereof cannot be effected; they are two contraries, which will be at perpetual feud, the issue whereof can be no other but the ruine and destruction of the subject on whom the experiment is attempted: I wish those that discourse thus, first understood but what they endeavour to perswade others of; and that they would explain to us what artifice they fancy in the Veins and Arteries, to give passage to one sort of blood & exclude another at the same time. For my part I confess I cannot comprehend why the continual circulation and rarefaction made in the heart by the heat of its Ventricles, are not more than sufficient to make a perfect mixture there of these two sorts of blood, & the difficulty seems the greater in regard experience appears to flatter me into a contrary opinion. For having a few dayes agoe syring'd about a quarter of a pinte of Milk into the veins of an Animal, and having opened the same some time after, we found the Milk so perfectly mixt with the whole substance of the blood, that there was not any place wherein appear'd the least footstep of the whiteness of Milk, and all the Blood was generally more liquid and less apt to coagulate.

The second Objection of the same Author is, That should the pure Blood mingle with the impure, yet it would not long
long preserve its Purity and temperament; in regard that upon its Arrival at the Liver (which is the grand Organ of Sanguification wherein the blood receives all its qualities) it would infallibly lose all its goodness, and in an instant become like the whole Mass, which we suppose corrupted; and consequently can produce no benefit therein by its mixture.

But this Objection is ill enough grounded, since it supposeth the blood to be made in the Liver, and is contrary to experience which demonstrateth most evidently, both the existence of blood in Animals before the formation of the Liver, and also the Arrival of the Chyle, and at the heart, and conversion of the same into blood there, before it entreteth into the Veines which carry it to the Liver: wherefore I dismis this reason and proceed to another, which seemeth stronger and is offer'd by persons more judicious.

'Tis not contested in this Objection in what place sanguification is perform'd, because 'tis held to be done in several parts of the body, namely by reiterated circulations in the heart, Veines and Arteries: But 'tis likewise maintain'd that all these parts come either by sickness or age, to a certain degree of intemperature and malignity, from which there is no possibility to recover them; and that in this state they have power to communicate their evil qualities to whatsoever approacheth them; whence consequently in a little time they corrupt the laudable blood, wherewith they are newly irrigated. Proof of this is afforded both by example and experience. The example is taken from a Hoghead of Vinegar; which being once throughly imbued with that Liquor, is no longer fit to contain any other, but every thing pour'd thereinto is immediately infected with fowerness by contagion of the Caske. The experience consists in that which was lately publishd by an English Doctor, who transfusing the blood of a Mangie Dog into a sound one, to try whether the Mange would be communicated with the blood, found the Mangie Dog cur'd, and the other who had receiv'd his blood not to become Mangie.
Now to answer to all this in order, I say in the first place, that such a great intemperature, from whence 'tis said the blood can never recover, is either rare or very common. If very rare, it requires not much consideration, and if the mixture of laudable blood suffice not to restore it to its due temperament, I would gladly learn other means to do it. But if such intemperature be ordinary in all diseased and in all declining old Ages, I demand to what end serve the aliments which Physick allows, or the Medicaments it prescribes in these occasions? Can these things enter into the body without mingling with the blood within an hour? And if by so mingling they contract the ill temperament of the blood, and impart not to the same any of their good qualities, is it not loss of time to study Physick, and to reduce into practice what we are taught touching the choice of all those things? For my part, I am so far from having such a thought, that I doubt not but the strength & goodness of the meats & drinks we take, is able to correct the ill temperament of the blood and render it better: If there be any impediment of this effect, I should not so much look for it in the blood itself, or in the parts which elaborate the same, as in those through which the aliments pass before they arrive at the heart. For 'tis sufficiently manifest that if there be any irregularity in the stomach, if the digestion be not well made, if the juices of the Gall and Pancreas (which serve for Dissolvers in the Intestines) be not well tempered, if there be any Obstruction or Abscission in the glands of the Mesentery which strain the purer portion of the Chyle; the best and most excellent things become so transform'd before their mixture with the blood, that no good nourishment or refreshment to the sick can with reason be expected from them.

Now in this way of Transfusion all those inconveniences are avoided; the blood communicated is not made to pass through those many turnings and windings, where the least corruption of a part destroys in an instant whatever good the soundness of the rest can produce. The good blood enters...
immediately into the Veins of the receiving Animal, and there mixing and circulating with the rest, carries good nourishment to the principal parts, which are not ordinarily indisposed in sickness and old age, but by the vitiosity and faultiness of the blood which humecteth them.

This is an Opinion which I see many Physicians embrace, and were any thing capable to make me renounce it, the Barrel of Vinegar, above mention'd must not be it. For if fowerness be the last quality which Wine assumes, and (as I may say) its Death, since it is never recoverable from the same; I do not believe that a like quality is to be found in the blood; that is to say, which corrupts the blood in such manner that it can never be purifi'd, unless perhaps in some Diseases accounted incurable, which are not pertinent to the Question. But if comparisons be currant, let us rather consider Wine with all sorts of other qualities but fowerness, and then our comparisons will in my opinion, fall more just. For as the roughest Wine may be sweetned, the fowleſt clarified, the weakeſt become stronger, the Oylie rid it self of its fatness; in a word, that which is decay'd may be amended by mixture of certain liquors, known to those that have the secrets of them, and practise the same every day: In like manner, 'tis reasonable to conceive that blood too thick may be refined and subtilized, that too subtle be fixed and incrassated, that too hot be temper'd, that too cold be heated; and all this by the mixture of other sorts of blood, the particular qualities whereof are known to the Physitian who prescribes the Transfusion.

As for the Experiment of the Dog, which is said to have received the corrupted blood of another Mangie one without contracting his disease; many things of sufficient importance may be said to it. 1. Is there any assurance that the blood of the Mangie Dog was putrefied, and corrupted in his Veins? On the contrary might it not have been purified before, by discharging its impurities through the pores of the flesh, where the same was converted into Mange upon the Skin. That which confirms me the more in this particular, is, that very
very frequently Scabs divert a greater malady; since if those impurities which cause the same, issued not out at the surface of the body, they would remain mixt with the substance of the blood, and falling upon some noble Part, produce very dangerous effects therein. 2. Supposing the blood of the Mangie Dog wholly corrupted in his Veins, is it necessary it should produce the Mange in him that receives it? Is it not possible that vicious blood may be purified when it becomes mixt with better, and that the great heat which caused the extravasation in the one in order to production of the Mange, may be allayed by the coolness of the other wherewith it is mixt, and consequently not produce the same effects. Lastly, Is there any assurance that the Mangy Dog's blood hath not produced some evil effects in the body of him who receiv'd it? He should have been open'd some days after, and perhaps the corruption which render'd not it self apparent outwards, might have been visible within.

Hitherto therefore I find not either Reasons or Experiments sufficient from the opinion I have conceived of the benefit of Transfusion; and in the Discord I observe of Physitians, whereof some approve the Invention, and maintain that it may with prudence be prescrib'd for the care of divers Diseases, others respect it as a very useless Novelty; and Lastly, others keep themselves undetermin'd, expecting what success the Experiment will produce; I willingly joyn with the first, and resign my self to the ensuing reasons, until some body shall produce others to the contrary of greater weight.

1. The Transfusion of the blood of one Animal into another is sufficiently taught us by Nature it self, and it must be grant-ed that if we ever practise the same, we shall do no more but imitate her; since whilst she cannot yet administer nourishment to the Fetus by the mouth, and his stomack is not fit for digestion, she makes a continual Transfusion of the Maternal blood into the Umbilical Vein of the Infant, therewith to nourish, vivifie and encrease all the parts of the same. Nor is it to be answered here, that the Mother and Child are in this state to be consider'd, but as one and the same Body and Substance. For it happens frequently enough, that the Paternal Seed
Seed predominating above that of the Mother, the Constitution of the Foetus is very different from her's that bears it, although nourisht by the transfusion of her blood.

2. The Transfusion of blood is but a very compendious way of the continual transfusion of our Aliments, which is also taught us by Nature after our birth. For, whereas the Meats which we eat for the reparation of our strength, and refreshment of the heated parts, have very many impurities mixt with their good juices, which must be concocted by the heat, and digested in the acid Liquor of the Stomach; which done, the purer portion of the same digested juices is converted into Chyle, that Chyle impell'd into the Veins to be mingled with the Blood, convey'd therewith to the heart, and there receive its utmost perfection; The Transfusion of blood is made more speedily, and with much more effect, since thereby, in a very small time, a most elaborated Liquor is immitt'd immediately into the Veins, where it instantly redresseth such defects as it findeth in the internal parts, when they are distemper'd.

3. Physitians cannot deny, That the greatest part of our Diseases are but Results of the Distemper and Corruption of the blood; since the speediest and commonest remedy they have in Practice, is, to evacuate the same by Phlebotomy, or else refresh and cool it by Juleps. But they must also confess, That whilst they endeavour to draw out the Corruption, they at the same time diminish the strength and vigor; and that great Fevers by this means are oftentimes follow'd by Faintnesses and Dropsties: On which consideration, some Physitians spare bleeding as much as possibly they can. Moreover, it must be acknowledged, That Drinks passing through the Stomach and Intestines, before it arrive at the Veins and Arteries, to be mingled there with the blood, they may be alter'd a thousand ways by the Ferments occurring in the passage, or else they may cause some alteration in those parts by Ferments accompanying them, and so leave weakness and dangerous Crudities behind them. But in Transfusion all these things are avoided, and all Physitians brought to agreement within a short time. They who are for blood-letting, find
and the same practis'd in this Operation, the old and corrupt
being first evacuated, to make room for new and pure. And
those also that decry bleeding, as that which too much weak-
en the sick, will have no reason to complain, since the new
blood conduceth to strengthen them more.

Lastly, Every one knows that many persons dye through
loss of blood, and Hæmorrhagies not to be stopt, many are
emacerated by them, and others precipitated into untimely
old Age, by defect of blood and vital heat. Now who
doubts but the Transfusion of a mild and laudable blood may
preserve the one, and prolong the life of the others. A man
may foresee some benefits and advantages from this Opera-
tion in Pleurisies, the small Pox, Leprosies, Cancers, Ulcers, St.
Anthonies fire, Madness, Dotage, and other Maladies arising
from the Malignity of the blood: But the success is rather
to be expected in Experiments, which will be made within a
little time. In the interim, give me leave to acquaint you
with my Judgment concerning the Election of the Animal
that is to communicate its blood.

Many have conceiv'd, That if ever the Transfusion of blood
should come to be practis'd upon men, it ought to be done
with blood of the same Species; and consequently, That it
would be a very barbarous Operation, to prolong the life of
some, by abridging that of others. But for my part, I am
far from that Opinion, and I am persuaded that it will be much
more expedient to make use of the blood of other Animals,
than of that of men, for many reasons, the chief whereof are
these:

1. Tis easy enough to judge, That the blood of Animals
is less full of impurities, than that of men; because debauch-
edness and irregularity in eating and drinking, are so ordina-
ry to them, as to us. Sadness, Envy, Anger, Melancholy, Dif-
quiet, and generally all the Passions, are as so many causes
which trouble the life of man, and corrupt the whole sub-
stance of the blood: Whereas the life of Brutes is much more
regular, and less subject to all those miseries, which we ought
to consider as sad consequences of the prevarication of our
first Parents. And indeed, Experience sufficiently shews us,
That if 'tis a rare chance to find its blood in the Veins of Beasts; 'tis almost impossible not to find some corruption in that of Men, how healthful soever they seem to be. Yea, even in Children yet fucking, it is not wholly unblamable; because, having been nourisht with the blood and milk of their Mothers, they have sucked corruption together with their nourishment.

2. If Physicians so successfully employ the milk of certain Animals, to which they oblige certain Patients for some whole years together: If we are very well nourisht with the flesh of some, during all our lives; and if their juices be so advantageous to us for repairing our natural strength; Why may we not hope greater advantages from the mixture of their blood with ours.

3. The Operation may be made more boldly, and with greater success by employing Brutes. For, being we design to take the best blood that is to be had, and least subject to coagulation, 'tis certain that the Arterial blood upon account of its heat and tenuity, possesseth both those qualities in much more perfection than the Venal. And the dangers and inconveniencies which would occur in opening the Arteries of a man are not considerable, when 'tis a beast on whom the Operation is made.

4. Beasts may be better fitted and prepar'd for this use than men. For I should advise those, that would employ this Experiment to the best advantage, to feed their Beasts for some daies before-hand with more care and exquisiteness than ordinary; since, if by the taste we can discern the flesh of Calves fed for some time, with Milk, and Yolks of Eggs, to be much more pleasant than that of others; reason ought to perswade us that their blood is also meliorated according to their feeding.

All these Reasons joyn'd to the Experiments we have made, could not but induce us to give assurance to the publick of the little danger to be fear'd, and the considerable effects to be expected from this Operation. Yea, we imagin'd 'twere no rashness to make tryal of it upon men, and indeed divers persons of much gravity and prudence solicited us to beg some condemned Criminal, on whom to make the first Essay. But having
ving consider'd that a man in that condition, being already much disorder'd by the apprehension of death, might be further intimidated, by looking upon this transfusition as a new kind of death; and thus, conceit might possibly cast him into faintings and other accidents, which would undoubtedly be ascribed to the Experiment by such as decry it: We thought not fit to expose our selves to that danger, nor to importune his Majesty without any necessity; being persuaded that there would not be so much reason to fear the like events in persons whom we knew perfectly well, and who had some confidence in our words, we chose rather to wait till a favourable occasion offer'd us such a person as we wisht, than to hazard the loss of all by too much precipitation. This Resolution being taken we neglected nothing that prudence obliged us unto; and at length after some attendance we lighted upon a subject suitable to our wishes. The particularities of our proceedings I here subjoyn in few words, they being as many authentick confirmations of all that I have hitherto written.

On the 15 of this Moneth, we hapned upon a Youth aged between 15 and 16 years, who had for above two moneths been tormented with a contumacious and violent fever, which obliged his Physitians to bleed him 20 times, in order to affwage the excessive heat.

Before this disease, he was not observed to be of a lumpish dull spirit, his memory was happy enough, and he seem'd cheerful and nimble enough in body; but since the violence of this fever, his wit seem'd wholly sunk, his memory perfectly lost, and his body so heavy and drowsie that he was not fit for any thing. I beheld him fall asleep as he fate at dinner, as he was eating his Breakfast, and in all occurrences where men seem most unlikely to sleep. If he went to bed at nine of the clock in the Evening, he needed to be wakened several times before he could be got to rise by nine the next morning, and he pass'd the rest of the day in an incredible stupidity.

I attributed all these changes to the great evacuations of blood, the Physitians had been oblig'd to make for saving his life, and I perswaded my self that the little they had left him was
was extreamly incrusted by the ardour of the fever (which
usually dissipates only the more tenuous part) and so flag-
tating in his vessels, he wanted the motion and heat necessary to
volatilize the same, and to diffuse a sufficient activity into the
Nerves and Muscles. Accordingly my conjecture was confir-
med by our opening one of his Veins, for we beheld a blood
so black and thick issue forth, that it could hardly form it self
into a thread to fall into the porringer. We took about three
ounces at five of the Clock in the morning, and at the same
time we brought a Lamb, whose Carotis Artery we had prepar’d,
out of which we immitted into the young mans Vein, about
three times as much of its Arterial blood as he had emitted in-
to the Difh, and then having stop’d the orifice of the Vein with
a little bolster, as is usual in other phlebotomies, we caus’d him
to lie down on his Bed, expecting the event; and as I askt
him now and then, how he found himself, he told me that du-
dring the operation he had felt a very great heat along his Arm,
and since perceiv’d himself much eafe’d of a pain in his fide,
which he had gotten the evening before by falling down a pair
of ftaires of ten steps, about ten of the clock he was minded to
rise, and being I observed him cheerful enough, I did not op-
pose it: and for the rest of the day, he spent it with much more
liveliness than ordinary; eat his Meals very well, and shewed
a clear and smiling countenance. He bled only 3 or 4 drops
at the nofe, about 4 a clock of the evening, and after he had
up’t very well, I caus’d him to go to bed about 9, and falling
asleep at 10, he awakned at 2 after mid-night, and finding that
he could not fall asleep again, he arose at 4 in the morning. All
this day we observ’d his humor much more lively, and the agi-
licity of his body much greater than ordinary. The next day
he slept a little more, and from that time he easily got the vi-
cotory over his drowsinesfs, which before he had often attempt-
ed without success: for now he never fails to rife very early
without needing to be wakened. He executes nimbly what-
ever is appointed him, and he hath no longer that flowness of
spirit nor heaviness of body, which before render’d him unfit
for any thing. He grows fat visibly, and in brief, is a subject
of amazement to all those that know him, and dwell with him.
Now
Now who sees not that all these admirable effects undoubted
edly proceed from that little Arterial blood of the Lamb, which
having been mixt with the mass of his thick blood, was like a
ferment to it, to rarifie and attenuate it more than ordinary,
whence follow'd the production of greater plenty of spirits,
and a more nimble performance of the actions of the body.
This first Tryal thus succeeding engag'd us to make a second
upon a stronger Man, aged 45 years. Now this Man having no
considerable indisposition, we intended to make a larger
Transfusion upon him, than on the former. But finding his
vessels very low & not well fill'd with blood, we took from him
only about 10 ounces, and afterwards immitt'd into him twice
as much from the Carotis of a Lamb purposely prov'd
for it, as well because the same is bigger and easier to come
at than the Carotis, as for that we would put some difference
between this second Experiment, and the first. The man a-
bated nothing of his Jovial humor during all the time of the
Operation, and amongst other reflections which he made con-
cerning the placing of the Lamb near his Arme, he said merrily
that there were strange waies in Physick to preserve life, that
he knew not who had invented this of bleeding, but that he
felt a very great heat from the Orifice of his Vein up to his
arm-pit; which proceeded from the course of the new Arteri-
al blood, pass'd up that way towards the heart.
When the Operation was ended, we advis'd him to lie down
to rest; but being he found no indisposition in himself, 'twas
impossible to prevail with him, and we could not keep him
from falling to work with the poor Lamb, cutting his throat,
and fleaving him, in which he is very dextrous, having exercis'd
the same profession from his youth. Afterwards he declared
his intention to return home, and promised us that he would
take a mess of some comforting broth there, and lay himself
to rest for the remainder of the day; but as soon as he went
forth, he betook himself to find out his Comrades, and carrie-
ed them to the Tavern to drink part of the money given him
for his daily business: at noon finding himself more hearty,
(whether by the new blood he had received six hours before,
or by the quantity of wine he had drank) he fell upon a sort of
work.
work so laborious to his whole body that it might almost tire a horse; thus he spent all the afternoon, and so kept us from making such observations upon him as we had intended. I met him the next day in the streets, and understanding from himself this behaviour, I was surprised at it, and blam'd him of imprudence. But he told me in excuse of himself, that he could not be at rest when he was in health, that he had felt no pain either during or after the operation, that he had eaten, drank and slept very well, that he had more strength than before, that if we were minded to repeat the same experiment at any time, he desired we would choose no other person for it but him, &c that another time he would lie down to rest, and punctually behave himself as we should command him.

This is an account of our Experiments, which indeed have not yet proceeded very far; nevertheless I could not longer conceal them from your curiosity, knowing well that from these few observations you will foresee consequences and advantages enough. I have not described the manner of our making this Experiment upon man, which is very different from that which we use upon beasts: but the particular relation would be but tedious and useless to you, since you will behold it more plainly in an example when ever you shall find a convenience to command us to make the same before you. And I assure myself, you will therein admire the dexterity and sagacity of Mr. Emmenez, and confess that his industry renders him as happy in this operation as in many others of Chirurgery which are much more difficult; for indeed 'tis done with as much speed as an ordinary phlebotomy, and he that receives new blood complains not of any pain that he feels.

But I perceive that I abuse your patience, and that the length of my letter hath passed the bounds wherein the respect I have for the person ought to confine me. I beseech you pardon me this liberty, and be not displeas'd that I take this occasion to confecrate to you my most humble service, and to assure you that I am,

Paris, June 25, 1667.

Sir, Your most humble and obedient servant,

J. DENIS.
PHILOSOPHICAL
TRANSACTIONS.

For the Months of July, August, and September.
Munday, Septem. 1667.

The Contents.

An Advertisement concerning the Invention of the Transfusion of Blood.

An Account of some Experiments of Infusing Liquors into the Veins of Animals; As also of some new discoveries pretended to be made in the Brain and the Tongue. An Experiment upon Blood grown cold. Some Observations of Quicksilver found at the roots of Plants; and of Shells found upon in-land mountains; Other Observations made by a curious person in his Voyage from England to the Caribes, concerning the Rusting of Iron by the Sea-air; the Changes of Thames-water carried by sea; The Variety of the Colours of the Sea; The Burning of the same; the Night-winds in the Indies; The Relations of the Seasons of the year rectified; Observables about Tortoises; The condition of English bodies first coming to Jamaica; A way of preserving Ale as far as to the same Island. An Extract of a Letter concerning some Magnetical Experiments; and an Excellent Liquor made of Cyder-Apples and Mulberries. An Account of two or three Books; One, The HISTORY of the ROYAL SOCIETY: The other, DISQUISITIO DE FÆTU FORMATO, The third, MUSCULI DESCRIPTIO GEOMETRICA.

An Advertisement concerning the Invention of the Transfusion of Blood.

The Author of these Papers returning now to his former Exercises, which by an extraordinary Accident he was necessitated to interrupt for some months last past, thought fit to comprise the Transactions of all the Months omitted in one Tract: In the very beginning of which he must inform the Reader, that if himself had published that Letter, which came abroad in July last, Concerning a new way of curing sundry diseases by Transfusion of Blood, written to Monsieur de Monsmor, &c. by F. Denis Prof. of Philosophy, &c, he should then have taken notice, as he doth now, of what
is affirmed in that Letter about the time and place of the Conception of that
Transfusing design; and intimated to the Curious, that how long soever
that Experiment may have been conceived in other parts (which is needless to contest) it is notorious, that it had its birth first of all in England;
some Ingenious persons of the Royal Society having first started it there,
several years ago, (as appears by their Journal) and that dextrous Anato-
mist, Dr. Lower, reduced it into practice, both by contriving a method
for the Operation, and by successfully executing the same: wherein he
was soon overtaken by several happy Trials of the skilful hand of D. Ed-
mund King, and others, encouraged thereunto by the said Society; which
being notified to the world Numb. 19. and 20. of these Transactions, Print-
Novem. 19. and Decemb. 17. 1666; the Experiment was, soon after that
time, heard of to have been tried in foreign parts, without hearing any
thing then of its having been conceived ten years ago.

An account of some Experiments of injecting Liquors into the Veins of Ani-
imals, lately made in Italy by Signior Fracassati Professor of Anatomy at
Pila.

1. Having infused into the Jugular and Crural Vein of a Dog some Aqua
fortis diluted, the Animal died presently; and being opened, all the bloud
in the Vessels was fixed, but that in the guts not so well. It was also ob-
served, that the great vessels were burst: perhaps by an effort of Nature;
even as in the greatest part of those that die of an Apoplexy, the vessels of
the Lungs are found broken. Upon which Experiment the Author maketh
these Reflections: First, That an Apoplexy being often caused by a like
Coagulation of the bloud (as hath been observed by the opening, made of
sundry persons, who died of that distemper) it might be cured by a time-
ly infusing some Dissolvent into the veins. Secondly, That it is likely,
that that useful secret, by which Monsieur de Bills dissectioned Animals with-
out any effusion of bloud, consists in some such Infusion.

2. There was afterwards infused into another Dog some Spirit of Vi-
atriol, which had not so present an effect; for the Animal complained a
great while, and foam'd like Epilepticks, and had its respiration very
thick; and observing the beating of his breast, one might easily judge,
the Dog suffered much: who dying at last, his bloud was found fixed
in the veins, and grumous, resembling froth.

3. Then there was injected into a Dog some Oyl of Sulphur: But he
died not of it, though this Infusion was several times tried upon him. And
the wound being closed, and the Dog let go, he went into all the corners of the Room searching for meat, and having found some bones, he fell a gnawing of them with a strange avidity, as if this Liquor had caused in him a great appetite.

4. Another Dog, into whose veins some Oyl of Tartar was injected, did not escape so well: For he complained much, and was altogether swoln, and then died. Being opened, the Spectators were surprized to find his bloud not curdled, but on the contrary more thin and florid than ordinary; which seems to hint, that a too great fluidity of the bloud, as well as its Coagulation, may cause death.

An Account of some Discoveries concerning the Brain, and the Tongue, made by Signior Malpighi, Professor of Physick in Sicily.

1. He pretends to have discovered, that the Exterior and softer part of the Brain, doth not cover only the Corpus callosum, as hath been believed hitherto, but is also inserted into it in many places. He hath also observed, that the Corpus callosum is nothing but a Contexture of small Fibres, issuing from the Medulla Spinalis, and terminating in the said Exterior part of the Brain. And these Fibres, he faith, are so manifest in the Ventricles of Fishes brains, that when they are looked through they represent the figure of an Ivory Comb.

2. The Use, which he ascribes to the Brain, is much different, he faith, from what hath been assigned to it hitherto. He pretends, that as half, or at least, a third of the bloud of an Animal is conveyed into the Brain, where yet it cannot be consumed, the finest Serum of this bloud is filtrated through the exterior part, and then entering into the Fibres of the brain, is thence conveyed into the Nerves: which he affirms to be the reason, that the Head is so often found full of water, when the Brain hath received a wound, or an alteration by some distemper.

3. He hath taken a particular care of examining the Optique Nerve in divers Animals, it being one of the most admirable productions in the Brain. Having therefore among other Fishes dissected the head of a Xi-phis or Sword-fish, who hath a very big eye, he hath not observed any considerable cavity in the Optique Nerve, nor any Nervous Fibres; but found, that the middle of this Nerve is nothing else, but a large Membrane folded according to its length in many doubles almost like a Fan, and invested by the Dura Maser. Eustachio a famous Anatomist, had written something of this before, but obscurely, and without mentioning the Animal, wherein he had made this observation.

4. The
4. The same Malpighi thought he should have met with the same thing in Terrestrial Animals, but he found, that Fishes alone have such a structure of the Optique Nerve: For that of an Ox, Pig, and other such Animals, is nothing but a heap of many small Fibres of the same substance with the Brain, wrapped about with the Dura Mater, and accompanied with many little vessels with blood. Hence he draws the decision of that great question among Anatomists; Whether the Optique Nerve be hollow or not? For, saith he, it cannot be otherwise, but there must be many cavities in this Nerve; forasmuch as the small filaments, of which it is composed, cannot be so closely joined, that there should not be some void space betwixt them.

5. Concerning the Tongue, the same Author hath discovered in it many little Eminences, which he calls Papillary, and believes to be the principal Organ of Taste. * But here is not to be omitted the Observation of Fracassati, importing, that as the Tongue hath towards its point many Eminences, by the means whereof it goes, as it were, to meet objects of Taste; so on the contrary, it hath many cavities towards its root, wherein it receives them. All which cavities terminate in nerves, and seem to serve for Funnels to convey the aliment into them. Which maketh the Author think it very probable, that the finest part of the aliment paffeth immediately from the Tongue into the Nerves, whence it comes to pass, that Wine, being only taken into the mouth, restores her vigour presently.

*See Num. 20. of the Transactions, pag. 366, where a large account is given of this discovery from the Treatise of Laur. Bellini, de Organo Gustis.

An Experiment of Signior Fracassati upon Blood grown cold.

When any blood is become cold in a dish, that part which is beneath the superficies appears much blacker, than that on the top; and 'tis vulgarly said, that this black part of the blood is Melancholy blood, and men are wont to make use of this example to shew that the Melancholy humor as 'tis called, enters with the 3 others into the composition of the blood. But Signior Fracassati maintains, that this blackish colour comes from hence, that the blood, which is underneath, is not expos'd to the Air, and not from a mixture of Melancholy: to prove which he assures, that upon its being expos'd to the Air it changes colour, and becomes of a florid red.

An Experiment as easy to try, as 'tis curious.
Some Observations
Communicated by Signior Manfredus Septalius from Milan, concerning Quicksilver found at the roots of Plants, and Shells found upon In-land Mountains.

This Italian Virtuoso, famous for his knowledge and curiosity, as well as for his Hospitality to ingenious strangers, did in a late Letter of his to the Publisher, impart the following Particulars.

1. In the Valley of Lancy, which runs between the Mountains of Turin, grows a Plant like the Doronicum, (so also called by the Inhabitants and Botanists;) near the roots whereof you may find pure Quicksilver, running in small grains like Pearls; the juice of which Plant being expressed, and exposed to the Air of a clear night, there will be found as much Mercury, as there is lost of Juice. * This may be compared with those Relations, which acquaint us, that in Moravia, Hungary, Peru, and other parts; Mineral Juices concreted are found to stick to the roots of Herbs and Trees, some of those Juices ting'ing also the Leaves of Vegetables.

2. In a Voyage he made a few years since to Genoa, when he was to pass some mountains, he met with some Peasants, who digging on the sides of an Hill, had found and gathered very many Cockle-shells of divers kinds; which he wondering at, stopped his intended Journey, and went to the very place, where he was satisfied of the truth of the relation, finding great store of different shells, as the Turbinets, Echinii, and some Pearl-shells, whereof one had a fair Pearl in it, which, he faith, he put into his Repository.

Observations Made by a Curious and Learned Person, sailing from England, to the Caribbe-Islands.

These Observations shall be set down in the Authors own words, as they were obtained from him by Sir R. Moray; viz:

I took notice at Deal, whence I set sail for Jamaica, of the great difference in the rusting of Iron, in such houses, as front the Sea, in comparison of that effect in the Street immediately placed behind
that other, in which I made this observation. They told me that it rusted more at High-floods, than at Neap-tides; the height of the Beach hindring the Saline exhalations. This remark put me in mind of the vanity of the Argument of M. Ligons and others, viz. That the Air of the West-Indies was hot and moist, because of the Rusting of Iron; whereas it indeed arises from some other principle in the Air; for at the point of Cagua, where it scarce raineth 40 showers in a year, Iron rusts as much or more than anywhere; yet are there other parts of the Island, in which of 9 months not one passes without great Rains: besides, in Jamaica it rusts least in rainy Weather.

The Steams of the Sea are found of such a nature, that our sweet-meats rotted; Sugar of Roses, and other Lozenges grew moist; notwithstanding that there was no reason to attribute it to any rainy weather. And those Pies and Gammons of Bacon, which had kept well before, after they had been once exposed to the open Air, decayed more in a day or two, than in six weeks before.

On the point Cagua, the Iron Guns at the Fort were so corroded, that some were near become useless, being perforated almost like Honey-Combs: And I could at any time with 2 or 3 strokes of a Hammer break off some pounds of Rusty Iron, which served for prepared Steel, and in Salves. But the Guns which lay in the Salt-water, were not much endamaged by Rust; as we found, upon taking up some.

Many things receive damage by the Air: Not only Iron rusts, but even Linnen rots, and Silks once exposed to the Air do rot without losing their colour. If a Lance be once exposed to the air it will rust, though you presently put it up again; but if it be never exposed to the Air, it will hardly rust.

At Deal a certain Ale-feller will warrant, that the Ale, as he orders it, shall be carried good to the West or East-Indies. His way to prepare it is this, as he told me himself, he twice mathes it with Fresh-Malt, and twice boils it well; yet all this kept it not from fowring; as I observ’d during my stay there. We bought of it to carry to Jamaica, and then he directed us thus. To every Rundlet of 5 Gallons, after it is placed in the ship, not to be stirred any more, put in two new-laid Eggs whole, and let them lie in it; he said, that in a fortnight or little more, the whole Egg-shells would be dissolv-ed, and the Eggs become like Wind-Eggs, inclosed only in a thin skin.
skin; after this, the whole White would be prey’d on, but the Yolk would not be touch’d or corrupted. By this means we did preserve the Ale to Jamaica, and it was much better, than at Deal. I was told since by some others that the Experiment is usual with them, to keep Ale in England a quarter of a year: And if Eggs be thus put into March-beer, they preserve it from growing ever harsh. They must be put in, after the Liquor has done working.

Concerning the Thames-water, it is not only observable, that in eight months time it acquires a Spirituous quality, so as to burn like Spirit of Wine; and some East-India Ships, I am informed, have run the hazard of firing by holding a Candle near the Bung-hole at the first opening of the Cask;) but also that the stinking of it is no corruption, nor perhaps wholesome; for we drank it all the way, so as to hold our Noses, yet had no sickness, but we had proportion of Brandy each week, which perhaps might correct it. If you take off the Bung from any Cask that stinks, and let the Air come to it, it will in 24 hours become sweet again. And if you take a Broom-stick, and stir it about well, it will become sweet in 4 or 5 hours casting a black Lee to the bottom, which remixes with it, and so occasions a third or fourth fermentation, and stench; after which it stinks no more. But, though Thames-water upon stench do not putrifie, yet other Waters (as far as hath been hitherto observed) do become irrecoverable upon stinking, and dangerous to drink.

I observ’d at Sea, that though Glauber say, the water, as it grows Salt, becomes Greener, yet that is false. For, after we were out of the Narrow, the Sea grew darkish, and after perfect Azure, yet was it much more Salt, the farther we went: as I tried by a Water-poise of Glass, with Quick-silver at the one end, it rose about half an inch above the Sea-water in the Downs; and at 24 degrees more, 2 inches. But after that, I never observed any difference unto Jamaica, the Sea being probably so impregnated with Salt, as not to imbibe more; which crosses another observation, that the nearer the Tropiques and the Line, the Saltier the Sea.

As to the Colour of the Sea, I conceive there is as great variety in it and its steams, as in Grounds at Land; which may occasion the sickness in some places more than in others: For the Sea smells differently in the Narrow and Main. And as to colour, it is of a Sea-green (and more sickly) in the Downs, than at Torbay, and on Plymouth coast more, than past the Lands-end; and in the Bay of Biscay, than
in the Long-reach. Something perhaps may be imputed to the difference of the waves, which are short, and make a Copling Sea in the Bay of Biscay (yet we came not within 80 Leagues of Cape Finis Terra: ) in the Long-reach it is a long rolling wave, but never breaks. About Florida, Virginia, and New-England it is a great rolling wave, but breaks. And as the Sea coloureth from green to darkish, and so to blue; so in our return it colour'd from blue to dark, and so to green.

When we were in the Latitude of Barbadoes, and had failed so for two daies, and apprehended our selves to be within 70 or 80 Leagues, I observed the Sea was black and thick, not transparently blue, as before, and the foam against the Ship-sides was turbid, and of another consistence, than before. I had never seen the like before, yet was I willing to think the Sun not high enough, to give the water its due colour. I attended the Suns progress, but behold, it turn'd Green; whereupon I asked the Master, who told me, we were within 60 leagues of Barbadoes, and that the Sea was there soundable, whereas before it was not so. But at Barbadoes in the anchoring places, it was Blue; and as we row'd ashore, in the shallow it was Whitish: And so at Jamaica near the shore it is transparently White, but within three yards more, transparently Blue.

As to the Burning of the Sea, I could never observe so great a Light, as to perceive Fishes in the Sea of the Stern, though I frequently looked, as well as M. Ligon; yet was the Light great, and at sometimes more than other. I suppose several Subject Earths, Currents, and Winds do vary it. I observ'd, it burned more at Deal the night before we set sail, than ever in the Voyage: all the water ran off our Oars, almost like liquid fire; the wind was then South-East, and the Sea-men told me, that at East and South-winds it burned most. And it did never burn so much during our stay at Deal, as then, the wind having been alwaies Westerly. But in the Harbour of Jamaica I observ'd, that it did not burn equally there. As you pass the Current (which thwart the middle of the Harbour with a motion, different from the water on both sides) the water scarce seems white at the stroak of an Oar.

I shall not trouble you with an account, how two contrary Winds poise each other, and make a Calm in the midst, ships at a distance failing with contrary gales at the same time.

It is observable, that in the Indies such places, as have any high Mountains, have also every night a Wind, that blows from the Land, Maugre
For the Sea smells differently in the Narrow and Main. And as to colour, it is of a Sea-green (and more sickly) in the Downs, than at Torbay: and on Plymouth-coast more, than past the Lands-end; and in the Bay of Biscay, than in the Long-reach. Something perhaps may be imputed to the difference of the waves, which are short, and make a Copling Sea in the Bay of Biscay (yet we came not within 80 Leagues of Cape Finis Terrae;) in the Long-reach it is a long rolling wave, but never breaks. About Florida, Virginia, & New-Engl. it is a great rolling wave, but breaks. And as the Sea coloureth from green to darkish, and so to blue; so in our return it colour'd from blue to dark, and so to green. When we were in the Latitude of Barbadoes, and had failed so for two daies, and apprehended our selves to be within 70 or 80 Leagues, I observed, the Sea was black and thick, not transparently blue, as before, and the foam against the ships sides was turbid, and of another consistence, than before. I had never seen the like before, yet was I willing to think the Sun not high enough, to give the water its due colour. I attended the Suns progress, but behold, it turn'd Green's whereupon I asked the Master, who told me, we were within 60 leagues of Barbadoes, and that the Sea was there foundable, whereas before it was not so. But at Barbadoes, in the anko-ring places, it was Blue; and as we row'd ashore, in the shallow it was Whitish: And so at Jamaica near the shore it is transparently White, but within three yards more, transparently Blue.

As to the Burning of the Sea, I could never obverse so great a Light, as to perceive Fishes in the Sea of the Sterne, though I frequently looked, as well as M. Ligon; yet was the light great, and at sometimes more than other. I suppose, several subject Earths, Currents and Winds do vary it. I observ'd, it burned more at Deal the night before we set saile, than ever in the Voyage: all the water ran off our Oar's, almost like liquid fire; the wind was then South-East, and the Sea-men told me, that at East and South-winds it burned moist. And it did never burn so much during our stay at Deal, as then, the wind having been alwaies Westerly. But in the Harbour of Jamaica I observ'd, that it did not burn equally there. As you passe the Current (which thwarts the middle of the Harbour with a moti-
on, different from the water on both sides) the water scarce seems white at the streak of an Oar.

I shall not trouble you with an account, how two contrary Winds poise each other, and make a Calm in the midst, ships at a distance sailing with contrary gales at the same time.

It is observable, that in the Indies such places, as have any high mountains, have also every night a Wind, that blows from the Land; Maugre the Levantine Wind, which blows at Sea (but with a flacker gale all night; which seems to shew, it depends not only on the motion of the Earth, but Sun.) Wherefore this Wind should come, may be considered; there is none at Barbadoes or Saona, but at all the other Islands. And in Jamaica every night it blows off the Island every way at once, so that no ship can any where come in by night, nor go out but early in the morning, before the Sea-brise come in. I have often thought on it, and could imagine no other reason, but that those Exhalations, which the Sun hath raised in the day, make haste (after his strength no longer supports them) to those Mountains by a motion of Similar Attraction, * and there gather in Clouds, and break thence by their own force and weight, and occasion a Wind every way. For, as the Sun declines, the Clouds gather, and shape according to the Mountains, so that old Seamen will tell you each Island in the afternoon towards Evening by the shape of the Cloud over it. And this Attraction appears further, not only from the Rain that gathers on the Tree in the Island of Ferro, spoken of by J. Hawkins in his Observations, and If. Vossius upon Pomponius Mela, as also Magnenus de Manna; but also from the Rains in the Indies, there being certain Trees which attract the Rain, though Observations have not been made of the kinds; so as that if you destroy the woods, you abate or destroy the Rains. So Barbadoes hath not now half the Rains, it had, when more wooded. In Jamaica likewise at Guanaboa they have diminisht the Rains, as they extended their Plantations.

* Possibly it may be more plain, to say, That those Exhalations, condensed by the Cool of the night, and impelled downwards, fall by their weight, and then first of all meeting with the higher parts of the Earth, must needs gather and settle about the same, in clouds.
tions. **But (to return to *Jamaica*) that this night-wind depends much upon the Mountain, appears by this, that its force extends to an equal distance from the Mountain, so that at *Portmorant*, which is the Easter-most part of the Island, there is little of Land-brise, because the Mountain is remote from thence, and spends its force along the Land thither. I shall further illustrate this kind of Attraction. In the harbour of *Jamaica* there grow many Rocks, shap'd like Bucks—and Staggs-horns: there grow also several Sea-plants, whose roots are stony. Of these stone-trees (if I may term them so) some are insipid, but others perfectly Nitrous. Upon those other Plants with petrified roots there gathers a Lime-stone, which fixes not upon other Sea-fans, growing by them. It is observable also, that a *Monchinel-apple*, falling into the Sea, and lying in the water, will contract a *Lanugo* of Salt-peter: which is confirmed by the Author of the History of the *Antiles*. To conclude this particular, the Captain of our Ship ventured to give me a reason for these winds, which I will not conceal from you, since it may put you upon an Experiment, which he said he had often made: **Viz.** That the Sun did heat the Air, and exhale the Vapours, which after did settle on those hills, and as they grew cold, took up more room than before, and so made a wind by their pressure; as water, put hot into a Cask and closed, would, he said, as it cooled, break the Cask.

It is commonly affirmed, That the Seaons of the Year between the Tropicks are divided by the Rains and Fair weather, and six Months are attributed to each Seaon. But this observation holds not generally true: For at the *Point in Jamaica* scarce fall (as was, on another occasion, hinted above,) forty showers in a year, beginning in *August* to *October* inclusively. From the *Point* you may look towards *Portmorant*, and so along to *Ligonee*, six miles from the *Point*, and you'll scarce see, for eight or nine months, beginning from *April*, an afternoon, in which it rains not. At the *Spanish Town* it rains but three Months in the Year, and then not much. And at the same time, it rains at *Melis*, it rains not at the *Barbadoes*. And at *Cignateo* (otherwise called *Eleutheria*) in the Gulph of *Baha-ma* it rains not sometimes in two or three years, so that that
Island hath been twice deserted for want of rain to plant in.

At the Point of Jamaica, where ever you dig five or six foot, water will appear, which ebbs and flows, as the Tyde. It is not salt, but brackish; unwholsome for men, but wholsome for Hogs. At the Caymans there is no water, but what is brackish also; yet is that wholsome for men, insomuch that many are recovered there, by feeding on Tortoises, and yet drink no other water.

The Blood of Tortoises is colder, than any water, I ever felt there; yet is the beating of their Heart as vigorous, as that of any Animal (as far as I have observed.) And their Arteries are as firm as any Creatures I know: Which seems to shew, It is not heat that hardens the coats of the Arteries, or gives motion to the Heart, Their Lungs lye in their belly below the Diaphragme, extending to the end of their Shell. Their Spleen is Triangular, and of a firm flesh (no Parenchyma) and floridly red. Their Liver is of a dark green, inclining to black, and Parenchymatous. In the Oesophagus are a sort of Teeth, with which they chew the grass, they eat in the Meadows, which there grow at the bottom of the Sea.

All the Tortoises, from the Caribes to the Bay of Mexico and Honduras, repair in Summer to the Cayman Islands, to lay their Eggs and to hatch there. They coot for fourteen daies together, then lay in one night some three hundred Eggs, with white and yolk, but no shells: then they coot again, and lay in the sand, and so thrice. Then the Male is reduced to a kind of gelly within, and blind, and is so carried home by the Female. Their fat is green, but not offensive to the stomach, though you eat it as broath, stew'd. Your Urine looks of a yellowish green, and oily, after eating it.

There is no manner of Earth, but Sand, at the Point; yet I have eaten admirable Melons, Musk- and Water-melons, that have grown there. A great many trees also grow there, especially Mangranes, and Prickle-pears. In other parts it is ordinary to ride through woods, that are full of very large Timber, and yet have nothing of earth, only firm Rock, to grow in.

In some ground, that is full of Salt-peter, your Tobacco, that grows wild, flaseth as it is smoaked.
The fruit of Trees there of the same kind ripen not at one time: There is a Hedge of Plum-trees of two miles long, as you go to the Spanish-Town; on it I have many times remarked some Trees in Flower, others with Ripe, others with Green fruit, and others to have done bearing, at the same time. The like I have observed in other Trees. *Jasminus* I have seen to blow before their leaves, and also after their Leaves are fallen, again.

The *Sower-sop*, a pleasant fruit there, hath a flower with three leaves; when these open, they give so great a crack, that I have more than once run from under the Tree, thinking it all to be tumbling down.

There is a Bird, called a *Pelican*, but a kind of *Cormorant*, that is of taste Fishy, but if it lie buried in the ground but two hours, it will lose that taste, as I have been told for certain.

I tried some *Analysis* of Bodies by letting *Ants* eat them; and I found that they would eat *Brown Sugar White*, and at last reduce it to an *Inspid powder*. So they reduced a pound of *Sal-let-oyl* to two drams of powder.

At our first coming there, we sweat continually in great drops for three quarters of a year, and then it ceaseth: During that space I could not perceive my self or others more dry, more costive, or to make less Urine, than in England. Neither does all that sweat make us faintish. If one be dry, it is a thirst generally arising from the heat of the Lungs, and affecting the mouth; which is best cooled by a little *Brandy*.

Most Creatures drink little or nothing there, as Hogs; nay, Horses in *Guanaboa* never drink; nor Cows in some places of the Island for six months; Goats drink but once perhaps in a week. Parrots never drink, nor Parroquets; nor Civet-Cats, but once a month.

The hottest time of the day to us, is Eight in the morning, when there is no Breeze. I set a weather-glass in the window, to observe the weather, and I found it not to rise considerably at that time, but by two of the clock it rose two inches:

*Venice Treacle* did so dry in a Gally-pot, as to be friable; and then it produced a *Fly*, called a *Weavil*, and a sort of white-worm. So did the *Pilula de Tribus* produce a *Weavil*.

I shall
I shall conclude with an observation of a strange Quality of a piece of Land: There is in the midst of the Island a Plain, called Magotti Savanna, in which whensoever it rains (and the rain passes along the Island, before it falls there) the rain, as it settles upon the seams of any garment, turns in half an hour to Magots; yet is that plain healthful to dwell in; and an hundred years, that have seen the thing, assured me of it.

Infinite might the observations be, if I had alwayes enjoyed my health, for the speculative Philosophers almost every thing there being new, and Nature being luxuriant in her Productions in those parts: But I shall not trouble you with imperfect memorials. So far this curious Observer whose laudable Example may both quicken and direct other Travellers in the Particulars, to be taken notice of in their Voyages.

Extract of a Letter, written by Mr. Sam. Colepress to the Publisher, containing an Account of some Magnetical Experiments; as also of an excellent Liquor made of Cyder apples and Mulberries.

Presuming, what e're tends to the farther discovery of the Magnetick vertue, will not be unwelcome to you, encouraged by a hint, given in pag. 423. of your Phil. Transact. I shall not scruple to relate to you two or three Experiments of mine own, performed in the presence of Sir William Strode.

1. I took a Loadstone unpolish'd, which attracted but meanly, and I heated a Lath-nail glowing hot, nimbly applying the North-pole of the said Magnet to it, which quickly took it up, and held it suspend'd a great while, till I put down both the Magnet and Nail.

2. I took the same stone, and cast it into the Fire, letting it remain there, till it was thorow hot, altering its colour from black to red, and being red-hot, I applied the North-pole to another Lath-nail cold, and untoucht before, which it took up, but faintly, yet held it suspend'd for some time.

3. Two or three days after, I took the same Loadstone, and found, that it attracted then as strongly, as before it was cast
into the Fire. Whence I infer'd, that the Fire somewhat lessen'd its Attractive faculty, but did not deprive the Stone of it.

Cyder-season approaching, I know not how to conceal from the delicate and curious Cyder-drinker (though I myself find the pleasures of all liquors in one, even that of fountain water*) the notice of a liquor as commendable, as yet rare. It is a composition of theJuices of good Cyder-apples and Mulberries, producing the best tasted and most curiously coloured liquor, that many ever saw, or tasted. Of which the experiment may be easily made by those that are furnish'd with Mulberry-trees, without any considerable cost.

An Account of some Books.

I. The HISTORY of the ROYAL SOCIETY of LONDON, for the Advancement of EXPERIMENTAL Philosophy, by THO. SPRAT.

It was indeed highly suitable, that the History of the Royal Experimenting Society should be dedicated, as the Candid Author of it hath done, to that King, who is the first of all the Kings of Europe, that confirmed this Noble Design of Experiments, both by His own Example, and by a publick Establishment.

The Discourse itself, which is modest and elegant, is divided by the Author into these three general Heads:

The First gives a short view of the Ancient and Modern Philosophy; and of the most Famous Attempts, that have been made for its Advancement, by the Chaldeans, Egyptians, Grecians, Arabians, Romans, of old; and then, by several New naies of Philosophy.
Philosophy, in the compass of our memories, and the Age before us, representing what hath been attempted by the Modern Dogmatists, the Revivers of Ancient Sects, the late Experimenters, the Chymists, and the Writers of Particular Subjects: All which he deduceth, to the end, that by observing, wherein others have excelled, and wherein they have been thought to fail, he might the better fly, what is to be expected from these new Undertakers; and what moved them to enter upon a way of Inquiry, different from that, on which the former have proceeded.

The Second, consists of the Narrative itself; in which the Historian, out of the Registers and Journals of the Royal Society (which he hath been permitted to peruse) relateth the first Occasions of their Meetings, the Encouragement and Patronage they have received; their Patent, their Statutes, the whole Order and Scheme of their Design; the Qualifications of their Members; the Largeness of their Number; Their weekly Assemblies; the manner of their Inquiry; their way of Registering; and their Universal Correspondency; together with a Particular Enumeration of the Principal Subjects, about which they have been employed, since they were made a Royal Corporation, and this, to silence that importunate demand, What they have done all this while? And here the Historian hopes, that all reasonable men will find satisfaction, when they shall consider, First, That, besides that this Society hath passed through the first difficulties of their Charter, and Model, and overcome all oppositions, which use to arise against the beginnings of great things; their Aim, and the Nature of their Design, and the Extent of their task do admit of no violent and hasty dispatch. Next, That, though their work hath not been exposed to open view, yet their Registers are stored with a good number of Particulars they have taken pains about; As,

1. Queries, and Directions, they have given abroad.
2. Proposals, and Recommendations, they have made.
3. Relations, they have received.
4. Experiments, they have tried.
5. Observations, they have taken.
6. Instruments, they have invented, or advanced.
7. Theories,
7. Theories, that have been proposed.
8. Discourses, they have written, or published.
9. Histories of Nature, and Arts, and Works, they have collected.

The Particulars upon which Heads are more numerous, and of greater moment and variety, than perhaps Detractors and Cavillers imagine or expect: they exceed indeed the number of 700, of which the Experiments and Observations both together amount to above 350; the Relations, to about 150; the Queries, Directions, Recommendations, and Proposals, to above 80; the Instruments, to about 60; the Histories of Nature and Art, to above 50; and the Theories and Discourses, to as many.

To these he adds an Account of the Library and Repository, they have obtain'd by the bounty of two of their Members; and gives withall some Examples of their Experiments; Histories both of Nature and Art; Queries answered; Proposals recommended, &c. Which done, he concludes, That if any shall yet think, they have not usefully employed their time, he shall be apt to suspect, that they understand not, what is meant by a diligent and profitable labouring about Nature; and that such men seem not capable of being satisfied, unless the Gentlemen of this Society immediately profess to have found out the Squaring of the Circle, or the Philosophers Stone, or some other such mighty Nothings; which only argues the extravagance of the Expectations of such men. Mean time, the Author esteems, that, since the Society promises no Miracles nor endeavours after them, and since their Progress ought to be equal and firm, by Natural degrees, and thorough small things, as well as great, going on leisurely and warily, it is therefore fit, that they alone, and not others, who refuse to consider the nature of their work, and to partake of their burthen, should be Judges by what steps and what pace they ought to proceed.

The Third Part, is asserting both the Advantage and Innocence of this Design, in respect of all Professions, and particularly of Religion: and how proper, above others, it is for the present Temper of the Age, wherein we live: And
this is done, to free it from the Cavil of the Idle, and Malicious; and from the Jealousies of Private Interests; all which the Author shews to have nothing but Humour, or Envy, Prejudice, or Mistake, to bear themselves upon.

The promoting of Experiments, according to the Model of the Royal Society, will be, so far, from injuring Education, or from being dangerous to the Universities, that it will both introduce many things of greater concernment and benefit, to supply the place of what may be laid aside; and be mainly conducive to recover that Divine Dignity of Humane Nature, which consists in the Knowledge of Truth, and the Doing of Good.

The First years of Men being secured by this new Experimental way; it is made out to all Professions and Practical lives, that they can receive no ill Impressions from it, but that it will be the most beneficial and proper study for their Preparation and Direction. Whereas other Learning is charged to consist in Arguing and Disputing; and to be apt to make our Minds lofty and Romantick; presumptuous and obstinate; averse from a practical Course, and unable to bear the difficulties of Action; Propens to things, which are no where in use in the world; and careless of their own present times, by dotting on the past: This Experimental Philosophy will turn men to Tryals and Works; cure their minds of Romantick (swelling, by shewing all things familiarly to them, just as large as they are; free them from perversity, by not permitting them to be too peremptory in their Conclusions: accustom their hands to things, which have a near resemblance to the business of life: and draw away the shadows, which either enlarge or darken humane affairs: And of the Crafty, the Formal, and the Prudent (the usual Titles, by which men of business are wont to be distinguished:) Our Author resembles the Crafty, to the Emperick in Philosophy; the Formal, to the meer Speculative Philosopher; but the Prudent man, to him, who proceeds on a constant and solid course of Experiments: the one in Civil life, rejecting neither the wisdom of Ancient, nor that of Modern times; the other in Philosophy, having the same reverence for former Ages, and regard for the present; both raising their Observations
Observations unto Use, not suffering them to lye idle, but employing them to direct the actions, and supply the wants of humane life.

And as this Experimental way will afford much help to our Publick duties, and Civil actions, so it is proved to be very useful for the Cure of mens Minds, and the management of their private motions and passions, by keeping them from idleness with full and earnest employments, and by possessing them with innocent, various, lasting, and even sensible delights.

From hence our Author proceeds to make a defence of the Royal Society, and this new Experimental Learning, in respect of the Christian Faith; fully evincing, that as it is not at all dangerous to Religion in general, so it is not to the Doctrine of the Gospel, nor that of the Primitive Church, or of the Church of England.

This done, he declares, on what account the Study of Experiments is the most reasonable study for the present Temper of the English Nation, and then goes on to manifest the probable Effects of Experiments, in respect of all the Manual Trades, which have been heretofore found out, and adorned. This Argument he dispatches in a clear Resolution of these Four Questions:

1. Whether the Mechanick Arts are still improvable by humane Industry?
2. If they be, whether they may be advanced by any others, besides the Mechanick Artists themselves?
3. Whether there be any ground of hope from Experiments towards this Work?
4. Whether, if such Arts shall hereby happen to multiply, they are likely to prejudice those Trades, that are already settled?

In these Particulars, our Author doth so answer his Readers doubts, that it will easily be granted him, that it is not a vain or impossible Design, to endeavour the increase of Mechanick contrivances; that the enterprise is proper for a Mixt Assembly of Experienced Naturalists and Mathematicians; that the Course which the Royal Society observes towards it, will be effectual; and the Increase of such Operations, inoffensive to
others of the same kind, that have been formerly discovered.

Hence he proceeds to shew, That these Experiments are a proper study for the Gentlemen of this Nation, in which he finds them already well engaged: As also, that they will be beneficial to our Wits and Writers, who, if truly worthy men, will find in the works of Nature an inexhaustible Treasure for Fancy and Invention, which will be disclosed proportionably to the increase of their knowledge: Further, that they are advantageous to the Interest of the Nation, by enlarging the Trade and Power thereof.

Upon which and several other accounts (not possible to be contracted here) our Historian concludes his Discourse, with giving us a Catalogue of those, which at this present compose the Royal Society, amounting near to two hundred, whereof the King's Majesty is Founder and Patron. Among the Fellows are three of the Greatest Princes of Europe, his Royal Highness the Duke of York; his Highness Prince Rupert, Count Palatine of the Rhine; and his Highness Ferdinand Albert, Duke of Brunswick and Lunenburg: Then, the two Archbishops of England, and four Bishops; of Dukes, Marquess's, Earls, Viscounts, and Barons, English and Scotch, twenty nine; of Knights, thirty five; of Doctors and Bachelors of Divinity, fourteen; of Doctors and Candidates of Physick, twenty one; of Esquires, and other Gentlemen, and Merchants, sixty four; of Strangers, sixteen.

After the Enumeration of which, he recommends this Undertaking to the English Nation; to the bravest people, the most generous Design, which at once regards the discovering of New secrets, and the Purifying and Repairing all the profitable things of Antiquity: and here he represents, that if now this Enterprise should chance to fail for want of Patronage and Revenue, the World would not only be frustrated of their present Expectations, but have just ground to despair of any future Labours, towards the increase of Practical and Useful knowledge. But he hopes and presages, that the English Nation will lay hold on this opportunity, to deserve the Applause of Mankind, for having encouraged and supported a Work, which, instead of barren
I. \textit{DISQUITIO ANATOMICA DE FORMATO FÆTU}: Authore Gualtero Needham M.D. Londini, \textit{in 8°}.

This Disquisition consists of seven Chapters, full of the Learned and Ingenious Author, who was lately elected a Fellow of the \textit{Royal Society}, his own Experiments and Observations.

In the first he inquires into the Passages, by which the Nourishing Juice is conveyed to the Womb of the Animal: where he examines the Assertion of Everhard, importing, that some of the Lacteous Vessels carry the said Juice to the Uterus; which vessels are pretended to have been seen by himself in the dissection of Rabbets. Which engaged our Author to take up again the Anatomical knife, and to dissect with all possible accurateness both some of the bigger Animals, as Cows and Mares, and some of the smaller kind, as Rabbets, which are instanced by Everhard.

But having spent all his labour and care herein in vain, and besides, evinced by Ligatures, that the pretended Vessels are neither those that are described by Bartholin under the name of Lymphatick, nor others, presumed to be known by Everhard alone, as immediately carrying the Chyle out of its Receptacle to the Womb and Breasts; he imputes the cause of this mistake to the Trunk of the Lymphatics, running over the Vena cava into the Receptacle near the Emulgents, which Duftus he affirms to have often found filled with Chyle: from the 

\textit{Intestinum Rectum}, or the \textit{Ileum}, or \textit{Cæcum} (a Dog having no Colon;) but maintains with all, that by Ligatures it is manifest, that that Duftus goes to the Receptacle, and there depostes its liquor; which he proves to be alike true of all the Milky vessels, so that they carry nothing back, and consequently are unfit to convey any thing to the Womb. This he illustrates by a Noble
Noble Experiment of that Learned and Expert Anatomist, Dr. Lower, using to open sometimes the right side of the Thorax, and with his fingers to break the Receptacle; and sometimes on the left side, the Ductus Thoracicis, a little under the Subelaviar; whereby it hath come to pass, that Dogs, well fed all the while, have thrown out all the Chyle into the opened part of the Thorax, and, though plentifully fed, were starved within three daies: there appearing mean time in the Veins opened a craft blood, destitute of Serum, but not any mixture of transmitted Chyle.

Having rejected the Lacteous and Lymphatic vessels from this office, he declareth, that we must rest in the Ancient Doctrine, which layeth the task of conveying the Succus nutritius, to the Breast and Womb, upon the Arteries; unless the Nerves be call'd in for aid, for conveying some of the Spirituous Juyce, to be mixed with the Nutritious, to give life and vigour; And having proved this, he takes notice of the multitude of Anaftomoses, remarkable in the womb of pregnant Creatures; and subjoyns a discussion of the way how the Alimental Juyce is in the womb sever'd from the mals of the blood: whether by mere Percolation, or by some Ferment, working upon the Blood, and thence precipitating what is proper for the use of that part.

In the Second Chapter he treats of the Placentas and Glandules, and shews, How many waies the Juyce is derived from the Womb to the Fetus: First, simply from the Membrane of the Uterus to the Membrane of the Fetus; as in all Oviparous Creatures; and among Viviparous, in a Sow, all the time of her bearing; in a Mare, for half the time; and in a Woman, the first month only. Secondly, By a Mass of flesh, filtrig the Juyce; as in all Cake-bearing (called by the Latines, Placentifera) and in all Kernel-bearing (called Glandulifera) or Ruminating Animals. Where he giveth a particular account of the double Placenta or Cake, to be found in Rabbets, Hares, Mice, Moles, &c. and examines the learned Dr. Whartons Doctrine, assigning a double Placenta to at least all Viviparous Animals, so as one half of it belongs to the Uterus, the other, to the Chorion; shewing how far this is true, and declaring
declaring the variety of these Phenomena, together with a very ingenious assignation of the Cause of that variety. Where do occur many uncommon Observations concerning the difference of Milk in ruminating and other Animals; the various degrees of thickness of the Uterin liquor in Oviparous and Viviparous creatures; the property of the humour, turning into Eggs, with a hint of the cause of their being excluded, and not quickned and formed within; as also, of the cause of Moles in the womb, and of many kernelly and fleshly substances in other parts of the body: where he takes notice of a concretion seen by himself grown to the Cone of the Heart, of nine ounces weight in an healthy Body, that died of a violent death, and of the like adhering to the Spleen, Kidneys, Liver, without any perceived trouble to the Animal; yea, of some found within the heart itself.

He adds the Number, Shape, and Use of these Placenta's; and first observes, that those that are Kernel-bearing Animals, or chewing the Cud, have many; and those that are Cake-bearing, have, for the most part, one Cake for each Fetus; but a woman commonly but one, though she happen to have many Embrio's.

He annexes a particular description of the Placenta of a Woman, as the most considerable, and teaches, how it may be most conveniently severed from the Vessels, to render them conspicuous, which are a numerous off-spring of Arteries, Veins, and Fibres; of the last whereof he inquires, whether they be the capillary's of the Arteries, and Veins, or nervous.

The Shape of that in a Woman is Orbicular, about a foot large, and two inches thick; one of its Superficies's, convex, but uneven, the other concave, and every where sticking close to the Chorion.

The Use of the Placenta's is known to be, to serve for conveying the aliment to the Fetus. The difficulty is only about the manner. Here are examined three opinions, of Courvey, Everhard, and Harvey. The two former, do hold, that the Fetus is nourished only from the Amnion by the Mouth; yet with this difference, that Courvey will have it fed by the Mouth when it is perfect, but, whilst it is yet imperfect, by filtration only through
through the pores of the body, and by a kind of juxta-position: but *Everhard*, supposing a simultaneous formation of all the instruments of nutrition together at first, and esteeming the Mass of blood by reason of its asperity and eagerness unfit for nutrition, and rather apt to prey upon, than feed the parts, maintains, that the liquor is sucked out of the Amnion by the mouth, concocted in the stomach, and thence passed into the Milkie vessels, even from the beginning. Mean time they both agree in this, that the Embrio doth breath, but not feed, through the Umbilical vessels.

This our *Author* undertakes to disprove; and having asserted the mildness of, at least, many parts of the blood, and consequently their fitness for nutrition, he defends the *Harveyan* doctrine, of the Colliquation of the Nourishing Juice by the Arteries, and its conveyance to the fetus by the veins.

In the third Chapter, the Membranes and Humours of Embryo's are considered. The Membranes are in some, three, in others, four, in an Egg, six. All Placentiferous Animals (if I may assume this word) he affirms to have three Membranes, and Sows, Mares, and Women also; but only two Humours. Again, Bitches, Cats, and Conies, four Membranes, and three Humours; so that the Number of the Membranes hath been hitherto observed always to exceed that of the Humours.

Giving the History of both, he begins from Sheep, Cows, and other Ruminating Animals, describing first the Chorion, assigning its Use, and comparing it with that in Dear, Sows, Mares, Women, Rabbits, Bitches, and Cats, when with young. Then he proceeds to the description of the Allantoides (the Membrane immediately encompassing that skin, wherein the fetus is wrapped) and thence to that of the Amnion, wherein the Embryo it self lyeth, swimming in its alimental liquor. And lastly to that which is observed to be in Bitches, Cats, and Rabbits, and contains a very good and nourishing Juice; which how it comes thither, is a difficult inquiry, as well as that other, how the liquor gets into the Amnion. To resolve both which our *Author*, having disproved the Filtration of the liquor, held by Courvey and *Everhard* out of the Chorion into the Amnion, and evinced, that the liquor in the Allantoides, interjected between these
those two, is Urinous, he concludes, that the alimentary Juyce passes through the Umbilical vessels, by a proper Artery, depositing it in those Membranes we speak of, and reserving it there for the use of the Fetus.

Concerning the Humours, he affirms, that all of them in all Animals are Nutritive, except that in the Allantoides. He observes also, that most of Oviparous Fishes have Eggs,or Spawn, as to sense of one only colour, and but one humour; yet that the Spawn of a Skate hath a White and a Yolk. Birds have mostly three nutritious substances, that are visible, *viz.* a Yolk and a double White: to which upon incubation, comes a fourth, colliquated out of the former; the tender Embryo feeding upon the two Whites, till, they being consumed, the Yolk of the Chick, now to be hatcht, is shut up in the Abdomen, and thence by a peculiar Duitus conveyed into the guts; and so serves the young bird for breasts, it is fed by, until the twentieth day.

In Viviparous Creatures are found sometimes two, sometimes three humours, and in Bitches, Cats, and Rabbits, four: which perplexeth the Author, as to the giving a reason for it. These Humours, he faith, he hath examined, by concreting, distilling, and coagulating them; where he furnishes the Reader with no vulgar Observations. He concludes this Chapter by observing, that there is also Air in the said Membranes, which besides other Arguments, he proves from the crying of Infants in the Womb (of which he alledges a memorable and well attested example in a Child of an English Lady in Cheshire, the Child being yet alive and in good health;) and from Chickens, often heard to peep in the Egg, both before the breaking of the shells, and after, the Membranes being yet entire; adcribing the production of this Air to the spirituous liquor in the Membrane, apt to ferment, and thereby causing store of exhalations.

The fourth Chapter discourses of the Umbilical Vessels; and observes first, that they differ in different Animals, and hold proportion to the Membranes and Liquors, so as those that have two Liquors, have four Membranes, and three Liquors have six: the Oviparous also being furnished with a Duitus, passing to
the Guts, because they want breasts, and their yolk is shut up in the belly.

The Umbilical Arteries, belonging to the Placenta, and commonly said to be derived from the Crurals, are by him affirmed to proceed from the end of the Aorta. They are here described, and their several portions distributed for the Chorion and Amnion. Then an account is given of the Hepatic Vein, corresponding to the Arteries. It is in Viviparous Animals inserted into the Vena portae, passing again with the remaining blood through the Canalis Venosus into the Cava, without percolation made in the Liver. In Birds it enters not into the Liver, but passes over its convexity into the Cava. A description also is made of the Urachus, found in all Viviparous Creatures, though by many Writers denied to be in Man, who notwithstanding hath need, as well as other such Animals, somewhere to lodge his Urine. The Oviparous want this Umbilical funiculus, but yet are furnished with fit sanguineous vessels, which here also are explained; especially the Ductus Intestinalis, said to be omitted by Dr. Harvey, and to have been known to the Author long before Mr. Steno claimed the discovery of it: for which he appeals to the testimony of Mr. Boyle, and three worthy Physicians, Willis, Millington, and Lower, as also to that of two ingenious Frenchmen, Guifon, and Fiard, to whom our Author affirms to have shewed Anno 1659, when they were going over into Holland, not only this Ductus, but also the Ductus Salivales, and the Passages of the Nostrils, published afterwards by the said Steno.

The use of this Ductus Intestinalis is esteemed to be, the conveying of the Yolk into the Guts for a second coction, there made by the Pancreatic Juyce, acknowledged to be excellently handled by the Learned Sylvius, and his ingenious Schollar, De Graeff, from the former of whom our Author yet dissent, about the mixture of the Gall with the said juyce in the Heart, refuting it by several Experiments.

The fifth, explains the Communion of Vessels in Embryo's: In whom, he faith, three Anastomoses are usually observed, which, as soon as the Fetus is born, are closed. They are called; Foramen Ovale, Canalis Arteriosus, and Venosus. The two former to
(515)
to be met with about the *Heart*; the last in the *Liver*. All
three here described by the *Author*, who also compares, as
*Harvey* does, the *Fatus* yet in the Womb with the man-
er of operation of those Animals, that are provided but with
one cavity in the *Heart*, and with no Lungs; the bloud of the
*Fruit*, as long it is unborn, passing neither through the *Paren-
chyma* of the Lungs, nor that of the *Liver*. *Lastly*, The ne-
cessity of *Respiration* is explicated, and how the defect of
Lungs, and of one of the Ventrices of the *Heart*, is supplied
in *Fishes, viz.* by comminuting and mixing the Bloud in the
*Gills*. To which is annexed the manner of *Respiration* in *Am-
phibia’s*, which are furnish’d with Lungs and two Ventrices of
the *Heart*, and yet, if *Bartholin* misinformes us not, keep the
*Foramen Ovale* all their life time open; which yet our *Author*
calls in question, alledging, to have seen no *Diving* Animals,
which had not the said *Foramen* closed after their being
born.

The *Sixth* makes a digression, to discourse of the *Biolychnium*
and the *Ingress* of the *Air* into the Bloud, for the *Generation*
of Spirits, and the pretended kindling of a *vital Flame*. But
our *Author* can see nothing that may prove either the *existence*,
or the *necessity* of such a Flame: On the contrary, he finds the
Bloud unfit for taking Fire; and judgeth it very difficult to af-
sign either the place or the manner of this ascension; which is
not made in the Lungs, nor in the *Heart*, which he holds to be
destitute of all ferment. To which he adds, *first*, that the *Heat*
of the Bloud is not sufficient to cause such an inflammation, seeing
how much even good *Spirit of Wine* must be heated, before it
will flame, which it doth not without the actual application of
fire. *Next*, That *Examples* are very rare of Liquors kindled by
ventilation. *Further*, That *Fishes* and *Frogs*, which yet have life,
motion, and sense, are not thought to have this flame, as being
actually cold. *Besides*, That the *Animal* Spirits are not found in
the form of flame; which he endeavours to prove from the
*Willishan* doctrine of the manner; in which they are in the
*Brain* fevered from the Bloud. *Lastly*, That it is doubted by
*some*, whether any Air at all is received into the mass of bloud,
which yet is not questioned by our *Author*, who only doubt-
eth,
eth, whether through the Lungs there be a high way for the Air to the Blood.

After this, our Author gives his thoughts both of the true Use of the Lungs, and of Sanguification.

The Lungs, he saith, serve chiefly, by their constant agitation to comminate the blood, and so to render it fit for a due circulation; which office he thinks to be performed in Fishes by the continual motion of their Gills, a Succedaneum to Lungs.

Sanguification, according to him, is chiefly performed and perfected by the frequent pulsions of the Heart, and the repeated contractions of its left Ventricle at the passing of the Sanguineous liquor from thence into the Aorta.

The Seventh and last Chapter contains a Direction for the younger Anatomists, of what is to be observed in the dissection of divers Animals with young: and first, of what is common to all the Viviparous; then, what is peculiar to several of them, as a Cow, Mare, Cor, Ewe, She-Goat, Doe, Rabbet, Bitch, and a Woman: Lastly, What is observable in an Egg, Skate, Salmon, Frog, &c.

All is illustrated by divers accurate Schemes.

III. ELEMENTORUM MYOLOGIÆ Specimen; seu MUSCULI descriptio Geometrica, Authore NICOLAO STENONE.

This Book is not yet come into England; only the Excellent septalio having in his Letter above-mentioned given us notice of its being published, and dedicated to the great Duke of Tuscany, we thought it not amiss to inform the Curious of it.

Errata.


LONDON, Printed by T. R. for John Martin, Printer to the Royal Society, and are to be sold at the sign of the Bell, a little without Temple-Bar. 1667.
The Contents.

An Account of more Tryals of Transfusioon, accompanied with some Considerations thereon, chiefly in reference to its Cautious Practice on Man; together with a farther Vindication of this Invention from Usurpers. The Method of Transfusioon into the Veines of Men. Answers to some of the Inquiries formerly publish'd concerning Mines. An Extract of a Letter sent from Paris about the Load-stone; where chiefly the suggestion of Gilbert touching the Circumvolution of a Round Magnet, and the Variation of the Variation is examined. An Account of some Books and small Tracts: I. FREE CONSIDERATIONS about SUBORDINATE FORMS, by ROBERT BOYLE Esquire. II. JOH. SWAMMER-DAM, M.D. de RESPIRATIONE & USU PULMONUM. III. OBSERVATIONS faites sur un RENARD MARIN, & un LYON, à Paris. IV. HISTORIA AMBRAE GRISEAE, Auth. JUSTO KLOBIO, D.

An Account

of more Tryals of Transfusioon, accompanied with some Considerations thereon, chiefly in reference to its circumspect Practice on Man; together with a farther Vindication of this Invention from Usurpers.

This Experiment, as it hath raised Disputes among the Curious, both here and abroad; so it hath put some of them upon considering such ways, and giving such cautions, as may ren-
der the use of it safe and beneficial. Of the number of these seems to be that French Virtuoso, Gasper de Gurye de Montpoly, who in a late Letter of his to Monsieur Bourdelot, declares to the World, that this is a very Ingenious Invention, and such an one, as may prove very useful; but with all, that, in his opinion, it is to be used with much caution, as not being like to be practised innociously, if imprudent men do manage it, and the concourse of two differing sorts of Blood requiring many trial, and a careful observation of many circumstances, to give assurance. He supposes, that the Blood of every Animal is endowed with its peculiar Temper, and contains in the Aggregate of its parts, different natures, principles, figures, and even a different Centre. Whence he concludes, that two Substances thus differing, and containing plenty of Spirits, are not reducible to one and the same Centre, nor to one and the same Body without Fermentation; and that this Operation may prove of danger to him, that shall have admitted into his Veins a strange Blood (wont to be free in its native vessels) without passing through those degrees, that must give it Impressions suitable to the temper and Functions of the Vitals of the Recipient. And taking for granted, that no considerat man will hazard a Total Transfusion, he acknowledges that a Partial one may be in some cases and sicknesses very useful, provided, it be practised circumspectly, upon a Body yet strong enough, and in a moderate quantity, so as the Spirits and Blood of the Recipient may be able to dissolve and master the transfused strange Blood, and convert the same into its own nature by a gentle Ebullition, to obtain by such a commixture a principle of motion, that may cause a better habit of Body. And he believes, that this Ebullition must always happen in Bloods of differing parts and qualities; and that very hardly two Animals, of differing species's, ages and tempers, will be met with, that have Blood so like one another, as not to need Fermentation, to make a requisite mixture. He doubts not, that if a substance could be found so resembling that of our Spirits, as that it would immediately unite it self with them, not needing any alteration, the Transfusion of such a Substance would be capable to produce effects little les than miraculous, by relieving the prostrated forces
forces of Nature, and by fortifying in us the Spring of motion and life: In a word, by exciting that Principle of continual Motion, which, whilst it has strength enough, still subdues and gathers to itself whatever is proper to entertain it, and rejects what is not so. But such an Invention as this he fees cause to esteem very difficult, in regard that different Moulds cannot but Characterize things differently. Hence he proceeds to the Examples, wherein Transfusion hath been experimented, even upon Men; alledged in that known ingenious Letter of Monsieur Denys. And here he intimates, how much he was pleased to learn, that, according to his Conjecture, a Moderate Intromission of Blood had well succeeded, and the Fermentation, which he foresaw would be caused by the commixture of two Bloods, was made with advantage to the Patient: Which he judges did manifestly appear by his Bleeding at the Nose, (a signe of an Ebulition made in the Blood:) confirmed to him by this, that an expert Acquaintance of his, transfusing a great quantity of Blood into several Doggs, observed always, that the Receiving Doggs pilled Blood.

And as to the other successful Experiment, made upon a healthy and robust man, he notes, that he being a lusty Fellow, stored with blood, and taking the Air, and working hard on the same day that the tryal was made upon him, his vigorous Blood, Spirits, and Constitution, and the strong motion of his Heart, were able to convert into the substance of his own Blood that of the Lamb received, and to impart thereto its own nature, and to mould it into Figures fitable to the pores where it was to pass, and proper to the functions it was to performe.

But to these Reflections he subjoyns two other Instances, of an unlike success; whereof the one is afforded by a Man, the other by a Dogg. As to the Man, it ought to be related beforehand, to prevent wonder or misconstruccion, that his Intestins, when he was opened after death, were found to be gangren’d, and consequently, that then he appeared to have been a subject altogether unfit for this Experiment, seeing it was naturally impossible for him to live with such a putrefaction. But to come to the tryal itself; this Author saith, that Baron Bond, Son to the first Minister of State to the King of Sweden, undergoing the
Operation twice, appeared the first time to find new strength by it; but expired soon after the second Operation: * The Ebullition, it seems, of the corrupt Blood having mastered and enervated all the Blood he had in his Body: which, when open’d, no Blood at all was found in his Heart: probably, as the Author conjectureth, upon this account, that there being not left in the Patient Blood enough of his own, nor strength sufficient to turn a strange Blood into a substance homogeneous to that; the Heart was not capable to admit the Blood of the Emittent, as consisting of parts disproportionate to his own. But, as has been already observed, his Entrails were altogether vitiated by a Gangrene, and he therefore out of the reach of being relieved by this Experiment.

Concerning the other Instance, viz. of the Dogg, the Letter affirms, that that Tryal was made by Monsieur Gayen with great exactness, after this manner: He drew three great dishes of Blood from the Dog that was to receive, and weighed the other Dog that was to furnish; and, the operation being performed, he weighed him again, and found him weigh less than he did by two pounds; of which, having abated an ounce more or less, for the Urine, made by the Dog, and an ounce or two more for the Blood spilt in the Operation, there remained at least one pound and a half of Blood, that was transfused. But, the Recipient, though well dress’d, and well fed, died five days after; the Emittent being yet alive. Whence it seems evident to this Writer, that the too large Intromission of new Blood was predominant over the Native, and, as ’twere, overwhelm’d it. Whence he again inculcates the dangerousness of infusing too much Blood at once, in regard that such Blood being now separated from that Principle of life it had in the Emittent, and as yet destitute of the stamp necessary to live the life of the Recipient, it could not be moved and assimilated by the live Blood, which remained in the Recipient; and the Fermentation, that was made, passed rather to an Eagerness or Sowerness, than to such an one as precedes Digestion. And this kind of eager acidity he intimates was seen by the Spectators, and felt by the Receiving Animal, which
which was wounded, and remained as dead for half a quarter of an hour: And when some alleged, that the Dog died, because he was wounded in the neck, where he could not lick himself, which rendered his wound incurable, answer was given, that Experiments had been made, wherein not only a Vein was opened, but also an Artery, yea, even the *Aspera arteria* cut of a Dog, that could not lick himself, and yet survived.

This whole Account is concluded with an Admonition, that all those, who have convenience, would make frequent and exact trials of this Experiment on Brutes, and carefully observe *Weight* and *Measure*, and all other circumstances, before any thing be hazarded, that may damnify the publick, and depreciate the Invention.

*Abundans cantela non nocet,* is a Maxime very fit to be minded here; though several successful Experiments have been made in London, of very plentiful Transfusions; and among others (to mention a signal one) that upon a *Bitch,* which lost in the operation near 30 ounces of blood, and was recruited accordingly. This animal does not only survive to this very day, but had another more severe Experiment soon after tried upon her, by which her *Spleen* was cut out, without tying up the vessels, whence that *viscus* was separated: Since which time (even before the wound was healed up) she took dog, was with puppy, and brought forth whelps, and remains well and jocond, being kept for a piece of remarkable Curiosity in the House of a Noble-man, that is as severe in Examining matters of fact, as he is able in judging of their consequences.

So that it is not too hastily to be concluded, that *large* Transfusions are dangerous; but rather frequent Experiments should be made, before any thing be therein determined, with great as well as smaller quantities, both upon sound and sickly Beasts, carefully observing, how either is endured in either, and what are the Effects following the eon.

Before we dismiss this Subject, something is to be said of the Cause, why the Curious in England make a demur in practising this Experiment upon *Men.* The above-mentioned ingenuous Monsieur *Denys* has acquainted the World, how this degree was ventured upon at *Paris,* and what good success it there met.
with: And the *Journal des Scavans* glorieth, that the French have advanced this Invention so far, as to try it upon *Men*, before any *English* did it, and that with good success.

We readily grant, *They* were the first, we know off, that actually thus improved the Experiment; but then they must give us leave to inform them of this Truth, that the Philosophers in *England* had practised it long agoe, if they had not been so tender in hazarding the Life of *Man*, (which they take so much pains for to preserve and relieve) nor so scrupulous to incur the Penalties of the Law, which in *England*, is more strict and nice in cases of this concernment, than those of many other Nations are.

The *Publisher* can assert *bona fide*, that several Moneths agoe he saw himself the *Instruments* ready, and heard the *Method* agreed on, thought proper to execute this Operation upon *Man*. And, for further proof thereof, he shall here insert the whole way, peculiarly contrived here for this purpose, by the Ingenious Dr. *Edmund King*, and by him communicated in a Letter; *Monseur Denys* not having thought fit to describe the manner they used in *France* for *Men*; nor any body else, come to our knowledge.

The Letter is as follows,

**SIR,**

The *Method of Transfusing Blond you have seen pra-
etised, with facility enough, from Beast to Beast; and we have things in a readiness to transfuse Blond from the Artery of a Lamb, Kid, or what other Animal may be thought proper, into the Vein of a Man. We have been ready for this Experiment these six Months, and wait for nothing but good opportunities, and the removal of some considerations of a Moral nature. I gave you a view, you may remember, a good while agoe, of the *Instruments*, I think very proper for the Experiment, which are only a *Silver Tube*, with a *Silver Stopper* somewhat blunted at one end, and flatted at the other for convenience of handling, used already upon Beasts with good success. The way
way is in short this. After the Artery is prepar'd in the Lamb, Kid, &c. let a Ligature be made upon the Arm, &c. of a man (hard enough to render the Vein turgid) in the place, you intend to insert the lesser end of the Silver pipe, which is so fitted, that the Silver Stopper, thrust into the Tube, reaches somewhat, by its blunt end, beyond one of the ends of that Tube. This done, divide the skin of the part in the same manner, that is us'd in cutting an ileue, just over the vein, to be open'd. Then with a fine Lance open the vein; or, if you please, in case the Vein lye fair and high (especially if the skin be fine) you may open both together, according to the usual way of letting blood. Which done, let an Assistant clap his finger, or a little boulster, prepared beforehand, or the like, upon the Vein, a little below the Orifice, to hinder the blood from ascending. Keeping that position, insert the blunt-ended Tube upwards into the Vein; when 'tis in, hold it and the skin close together between your finger and thumb. Then pull out of the Tube the Stopper, and insert the Pipe, by which the Arterial blood is to be infused from the Emit-tent Animal; managing the remainder according to the known Method of this Experiment.

So far this Letter; which maketh the practicableness of this Method look so fair and easie, that nothing seems wanting to encourage the Trial, but the Direction and Assistance of discreet and skilful men, taking care, not to experiment it upon Subjects, that have their internal parts vitiated; for as much as it seems not reasonable to expect, that this Transfusion should cure Cacochymies, or restore a depraved constitution of the viscera.

We would have said no more of this Argument at this time, were we not obliged to remove a mistake found in one of the late French Journals, affirming with confidence, that 'tis certain, the
French have given the English the first thought or notion of this Experiment. And why because (say they) there are witnesses, that a Benedictine Fryer, one Don Robert de Gabets, discoursed of it at Monfieur de Montmors, ten years agoe. Surely, all ingenious men will acknowledge, that the certain way of deciding such Controversies as these, is a Publick Record, either written or printed, declaring the time and place of an Invention first proposed, the contrivance of the Method, to practise it, and the instances of the success in the Execution. All this appears in the field for England.

Numb. 7. of these Transactions (printed An. 1665, in Decemb.) acquaints the World, how many years since Dr. Christopher Wren proposed the Experiment of Infusion into Veins. And this was hint enough for the R. Society, some while after to advance Infusion to Transfusion; for the trial of which latter, they gave order at their Publick Meeting of May 17, 1665, as may be seen in their Journal, where it was registered by the care of their Secretaries, obliged by Oath to fidelity: The trials, proving then lame, for want of a fit apparatus, and a well contrived Method of operation, the Leained Physician and Expert Anatomist, Dr. Lower, since found out such a Method, which is not only registered in the same Book, but also published in Print Numb. 20. of these Tracts, before which time it had been already practised by the said Doctor in Oxford, who was followed by several ingenious men at London, that successfully practised it by the Publick Order of the aforesaid Society.

It seems strange, that so surprizing an Invention should have been conceived in France, as they will have it, ten years ago, and lain there so long in the womb, till the way of Midwiving it into the world was sent thither from London. To say nothing of the disagreement, there seems to be about the French Parent of this fatus, Monfeur de Gurye in the Letter above mentioned, fathering it upon the Abbot Bourdelot, but the Author of the French Journals, upon a Benedictine Fryer.

But whoever this Parent be, that is not so material, as that all that lay claim to this Child, should join together their endeavors and cares to breed it up for the service and relief of humane life, if it be capable of it; And this is the main thing aimed at and solicited in this Discourse; not written to offend or injure any,
any, but to give every one his due, as near as can be discerned by the Publisher.

Answers
To some of the Inquiries formerly publish'd concerning Mines.

That the Queries, scattered up and down in these Tracts, may not seem lost, or left un-regarded, the Publisher intends to impart at convenient times such of the Answers, shall be sent in by observing men, as may be thought acceptable to the Reader.

He begins now with an Account, communicated to him by the Learned and Inquisitive Mr. Joseph Glanvil, who premises in a Letter, that he procured the following Answers from a Person living near the Mendip-Mines, and upon whose relations we may securely depend: Adding, that he does not by these few suggestions think himself absolved of his Task, but shall pursue the matter farther, as soon as he has an opportunity of going into these Parts, whence he expects to be farther inform'd.

The Reader will be pleased to look back to the said several Queries, as they are extant in the Number 19; the following Answers respecting thither, and being accommodated to the Mines of Mendip in Somersetshire, where the following Observations were made, viz.

To the 1, 2, 3 Queries. That all Mendip is Mountainous, yet the Hills not equal in height. That it is barren and cold, and rocky in some places. That the Ridges thereof run confusedly, but most East and West, and not in any Parallel one with another. That upon the Surface thereof it is Heathy, Ferny and Furzy; and the Cattle, it feeds, for the most part are Sheep, which go there all the year; and young Beasts, Horses and Colts at Spring and Fall. That the Sheep are not faire, but big-bellyed, and will grow to no bigness, after they have been there fed; but will grow fat, if they are removed into better soyle, and so their Beasts and Horses.

To the 4, 5, 6, 7 Queries. That the Natives and Inhabitants live...
live neither longer or shorter, than ordinary, but live healthy, savi
ing such, as are employed about melting of the Lead at the Mines; who, if they work in the Smoak, are subject to a Disease, that will kill them, and the Cattel likewise that feed thereabout. The Smoak, that rests upon the ground, will bane them. And therefore the Inhabitants have keepers to keep them from it, for fear of the Infection. That the Country is not furnish'd with many Rivers, and Waters, that rise upon the Hills: But from the bottom of the hills there are many Springs round about; both to the North, South and West; and those Waters are very wholesome, and produce Rivers, after they have run to some distance from thence. That the Air is moist, cold, foggy, thick, and heavy. That it is observed often covered with mists and fogs; and if any Rain be in the Country thereabout, it is surely there; and 'tis probable, it may arise from the Mineral and Subterraneous Steams. That the Soyle near the surface of the Earth is red and stony; and the Stones that are drawn out thence, are either of the nature of Fire-stones, or Lime-stones, but no way Clays, Marly or Chalky.

To the 10, 11, 12, 13, 14, 15 Queries. That the Trees, growing thereon, have their tops burnt, and their leaves and out-sides discoloured, and scorched with the Wind, and grow to no bigness or stature. That the Stones and Pebbles, that are wash'd with the Brooks and Springs, are of a reddish colour, and ponderous. That Snow, Frost and Dew stay upon Mendip longer, then upon any of the neighbouring grounds; but whether the Dew, falling upon the ground, will discolour Linnen, I have not observ'd.

To the 16, 17, 18, 19, Queries. That Mendip is more than ordinary subject to Thunder and Lightning, Storms, Nocturnal Lights and fiery Meteors. That the Mists arise out of the Vales; but whether they signify, where the Minerals are, I cannot say. That the Virgula Divinatoria hath not been known to have been seen used in these parts. That there are no certain signs above ground, that afford any probability of a Mine, to my knowledge.

To the Query in the fifth Title, I can say little, save only, That the Ore upon Mendip lies in Veines as a Wall, in some places.
places deeper, in some shallower; in some places narrower, in some broader, but lies altogether, and is perfect Lead, only in the Outside, covered with reddish Earth.

To the Queries in the sixth Title, I cannot say much; it must be resolv'd by them, that melt the Lead-Ore, with which I have not been much acquainted, save only, that they beat the Ore small; then wash it clean in a running stream; then sift it in Iron-Rudders; then they make of Clay or Fire-stone a Hearth or Furnace, which they set in the ground, and upon it build their Fire, which is lighted with Char-coal, and continued with young Oaken-gadds, blown with Bellows by Mens treading on them: And after the Fire is lighted, and the fire-place hot, they throw their Lead-Ore upon the Wood, which melts down into the Furnace; and then with an Iron-Ladle they take it out, and upon sand cast it into what forme they please.

So far this Account, which is hoped will be made in time more compleat, and succeed with the like Answers from other places.

An Extract

Of a Letter, sent from Paris, about the Load-stone; where chiefly the suggestion of Gilbert touching the Circumvolution of a Globous Magnet, called Terrella; and the Variation of the Variation, is examined.

This Letter was written by the Intelligent and Experienced Monsieur Petit, Intendant of the Fortifications of his Most Christian Majesty, to the Publisher, as follows;

I have received yours, wherein you desire to know my sentiment about the present Variation of the Needle, intimating withal, that an Artift in London affirms, that whereas heretofore the Declination was East-ward, 'tis now about one degree and a half to the West.

Nothing can be more welcome to me, than to have occasion given me to discourse of this Subject, especially to the Philosophers of England, whence the Philosophy of the Magnet had

H h h 2
had its rise, and whence also the Principal Observations of
the Change of its Declination are come to us: so that 'tis just
that the Observations, made elsewhere concerning the same,
should return thither, as to its source.

I shall therefore let you know, that having always been
Curious in the Doctrine of the Load-Stone, after I had made
the Experiments, that are in Gilbertus and others, I made
that of the Needles Declination on three different Meridian-
Lines, which I traced An. 1630, in several places of Paris,
and found, that the Needle declined 4° degr. North east:
which having published, and made known here to the Curious
and to Artificers, some of whom counted 9 or 10 degrees accord-
ing to the Tradition and Writings of Orontius Fineus, and
Castelfranc; others, 11° degrees, following Sennertus and Of-
fusius: all at first rejected my Observation, and as commonly
New things meet with obstacles and contradictions, before
they are established, those that could not contradict what they saw,
pretended, that this Variety did perhaps proceed from the
greater or less vigour in the Loadstones, employed to touch
with; or from thence, that the Needles had been touch't near-
er to or farther from their Poles, which might make them
decline more or less from the Meridian, so as a Needle, be-
ing precisely toucht by the Pole of a good Magnet, might
perhaps have no Declination at all.

All which conjectures were not without their probabili-
ty; which was the greater, in regard that all the Load-
stones I had seen, being rude and like Flints, with irregu-
lar surfaces, in bunches and cavities, their Poles were al-
ways ill posited, and often within some of the Cavities,
so that one could not be sure to strike the Needle thro-
row the Pole of the Stone. To remove which difficulty,
and at the same time to find another quality (one of the ex-
cellentest of the World, if true;) viz., that which Gilbert
had assigned to Terrella's; I resolv'd, to make the Experi-
ment of it. And because I have not yet written of it, nor any
man, I know, (Men having contented themselves with re-
futing this Error by Discourse only,) you will perhaps not be
displeased to be inform'd of the success thereof.

You
You know, that Gilbert, though the first, that has writ rationally of the Magnet, and began to say no follies of it, writes about the end of his Book (yet without being positive) that if a Magnet altogether round were placed on a Meridian, and its Poles so poited, as to answer to the Poles of the World, and consequently its Axis to the Axis of the World, the Stone would continually of its self turn round in 24 hours. Whence he infers, that the whole Earth, as a great Magnet, turns also round about its Axis in the same space of time.

To explore the truth of this Proposition (which I wish were true; since then we should have a perpetual motion without wheels, and a Watch yet juister than Pendulums) I found the means of causing two Magnets to be turn’d with the powder of Emery; the one whereof having been made Spherical with all possible exactness, became very solid, plain, and without any visible pores, or diversity of matter, being 1½ inch in diameter: the other, bigger, of 3½ inches diameter, but of less vigour, porous also and uneven; which made me lay it aside as useless for this Experiment, because, though it had been perfectly Spherical, as the lesser, I could not be assured, that its Center of Magnitude was the same with those of its Gravity, and Strength, which was requisite to make good Gilbert’s Proposition.

But for the other smaller Magnet, that had no defect, and its three Centers were the same, with so much juftness, that after I had exactly found the two Poles of this Stone, I caused two small holes to be made therein, to support it by two points of Needles, as by two pivots: which having put in a Meridian of Brass, and suspended the Ball between them like a little Globe, it was so easily moveable, that I made it turn every way with a blast only of my mouth, and it stopp’d indifferently, now in one, then in another place, not any side of it prevailing by its gravity, nor descending, as it would have done, if any of them had been heavier than another.

This Stone thus prepared without any defect in virtue or figure, uniforme, homogeneous, equilibrated, being adjusted on its Meridian and a Horizon, so placed on its Meridian-line, that the Poles thereof answer’d to the Poles of the Heavens (as hath been said already;) the success was, that it had not any Motion, and
and a small white mark, I had made upon this Stone, remained still in the same place, where I had put it, without turning at all, whence I thought the Proposition of Gilbert sufficiently refuted.

This Stone, having serv'd me for this Experiment, did, together with the greater Stone, (whereof the Poles were also well marked) serve me also to find out, whether the Needles, touch'd in different places, nearer to, or further from the Poles, had different Declinations. Which having try'd frequently with these, and with other Stones, I found no difference at all in the Declination of the Needles.

And now to return to the main subject of the Letter, I then observ'd, that all these Needles declined then from the Meridian 4° degrees from the North Eastward. And, as I did not suspect, that this declination would have changed, having found it to be the same in many places, from Brest in Brittany to the Valto-line among the Alpes, I believed, the Antients had ill observ'd, and that the want of their exactness, in respect either of the Meridian-line, or the fabrick of their Needles, or the division of their Circles, was the cause of this defect. But I was soon undeceived of my own Error, when I learned a little while after, by Letters from England, that Mr. Burrows, Anno 1580, had near London observ'd the declination of the Needle to be 11° degr. 11° min. as well as Offusius and Sennertus: And that Anno 1612, Mr. Gunther, Professor of the Mathematicks, had in the same place found that Declination much diminished, having then found but 6 degrees: And lastly, that Anno 1633, Mr. Gellibrand had found it but 4° degrees North-east, conformable to my Observations. Which did assure me, that those Declinations were not constant, but had varied.

And that I might be convinced by my self, I made from time to time Experiments in divers places, and found still more and more diminution; so that Anno 1660, in June, after I had very exactly traced a Meridian by many Azimuths, before and after noon, with a Brass-Quadrant of 6, foot diameter, and applied good Needles upon it, the one of 7, the other of 10, inches long, I found that they declined but one degree, or thereabout: And the
the last year I found no more but 10 minutes on the same Meridian. Upon which having lately applied, since the receipt of the Letter, the same two Needles, me thinks, the Declination is yet less, than the last year. But this I can assure you, that the Declination is yet some minutes towards the East, at least at Paris. So that you may, upon my word, doubt of the Observation of your friend, whom perhaps the Meridian, or the Needle, or the Construction and Division of his Compass may have deceived, to a degree and a half North-west, which he at the present assigns to the Declination. But I doubt not, but in 12 or 15 years it will be found true what he affirms, as I have prognosticated by my Hypothesis, which makes the Declination to vary a degree every seven or eight years.

This is, what I had to return to the Letter, which I wish might deserve to be presented to your Illustrious Society, and contribute something to the discovery of so many admirable virtues lodged in this Stone, and principally to the finding out of the Cause of this Variation; for which I have already made some attempt, and proposed my thoughts in a Dissertation de Latitudine Paresiensis & Magnetiæ Declinatione, which M. du Hamel caused to be printed Anno 1660, with his Astronomia Physica. I shall be very glad, to learn the sentiment of your Learned Philosophers thereupon, and what cause they suspect there is of so singular an effect. I could discourse to you of other particulars touching the Proprieties of the Load-stone, and especially of a remarkable one, I have discover'd, and which, if I am not deceived, subverts that Theory, which undertakes to explicate all these effects by the Particula striata; but I reserve that for another occasion.
An Account of some Books.

I. FREE CONSIDERATIONS about SUBORDINATE FORMS; by the Honourable ROBERT BOYLE

This Tract is an Appendix to the Noble Author's Examen of Substantial Forms, published last year, and reprinted this. There hath been already given an Account of the principal Part, as appears by Numbr. ii. 'Tis very fit the like should be done now of this considerable Appendix.

First then it clears up and states the Doctrine about Subordinate Forms, as it is maintain'd by divers learned Moderns, especially Sennertus, who teacheth, that beside the Specific Form (so called by him) there may reside in Animals and Plants, certain other Forms, so subject to the predominant Mistress-Form, that they deserve the Title but of Subordinate Forms, and during the Reign of the Specific, are subservient to it; yet when that is depofed or abolifht, these Inferior Forms may come to set up for themselves, viz.

This done, the Author tries, Whether the Phenomena and Effects of these pretended Subordinate Forms may not be as well as the principal ones, intelligibly explicated by the Mechanical Principles, viz. Matter and Motion, and the thence resulting Shape and Texture. Which that it may be done, is so happily made out in this Tract, that a Rational, Unprejudiced and Attentive Reader cannot but embrace the Author's Doctrine, and, according to it, be satisfied, that the portions of Matter, that are endowed with these pretended Subordinate Forms, cannot pay the presumed Superintendent Form any other obedience, then some such kind of one, as the parts of a Clock or Engine may be said to yield to one another. So that the whole matter may be well conceived to be nothing but this; That, when divers bodies of differing natures or Schematismes come to be associated so as to compose a Body of one denomination, though each of them be supposed to act according to its own peculiar nature, yet by reason of the coaptation of those parts, and the contrivement of the compounded Body, it will many times happen, that the action...
action or effect produced, will be of a mixed nature, and differing from that, which several of the parts consider'd as distinct Bodies or Agents, tended to, or would have perform'd; As when in a Ballance, by putting in a weight into one of the Scales, the oppo-
site Scale, though as a heavy body, it will naturally tend down-
wards, yet by virtue of the fabric of the Instrument is made to mount upwards. So that those Actions, which Scholasticall men attribute to the conspiring of subordinate Forms to assist the Spe-
cific, are but the resultant actions of several Bodies, which being associated together, are thereby reduced in many cases to act jointly, and mutually modify each others actions; and that which they ascribe to the dominion of the Presiding Form, is to be impu-
ted to the structure and connexion of the parts of the compound-
ed Body.

This the Author confirms and illustrates by many very instru-
tive Examples and Comparisons, taken from manual Arts and Practises, Physicks, Chymistry, &c. And applying his doctrine a-

1. The word Form is of an indeterminate signification.

2. 'Tis not easy, to decide the Nobleness of Forms.

3. In divers Bodies the Form is attributed upon the account of some eminent Property or Use; which if it be present and continue, though many other things supervene, or chance to be wanting, the matter is nevertheless lookt upon, as retaining its Form, and is wont to be allow'd its usual denomination.

4. By reason of the Conjunction or Connexion of the parts, that make up a whole (or, at least an Aggregat of Bodies, that for their connexion are lookt upon as such) it will often happen, that several things will be perform'd by the joint or concurrent Action of these united or coherent parts.

5. We may yet in a sound sense admit, that in some Bodies there may be subordinate Forms.

6. The supervenying of a new Form is often but accidental to the Pre-existent Form, and (then) does not at all destroy its na-
ture, but modify its operations.

7. Besides the Specifick actions of a Body, that harbours sub-
ordinate Forms, there may be divers others, wherein some of the

Parts
Parts or Ingredients may act according to their particular and pristine nature.

8. In divers Bodies, that which is call'd or look'd upon as the Specifick Form, is often not so much as the Presiding, but only the most eminent.

9. The Forms discoursed of, seem to be rather concurrent, than subordinate.

To each of these Propositions are annexed short Comments, full of very pertinent and teaching Instances, Relations, Comparisons, &c. for which the Reader is referred to the Book itself.

II. Joh. SWAMMERDAM, M. D. Amsterodamensis de RESPIRATIONE & VSV PVLMONVM.

This Author is of opinion, that all those Philosophers, who have hitherto inquired into the Nature and Use of Respiration, have only caught the shadow of it, nothing of the substance. And of this he gives this for the chief reason, because they have been too negligent in considering the first manifest motion of the Breast and Lungs in a Fetus; which particular being understood, he thinks it very easy to judge of the Respiration of born Animals.

He scruples not to reprehend the immortal Doctor Harvey, for having excluded from the office of the Lungs the Use of Refrigeration; which he pretends to have asserted himself by most evident Experiments, and uncontrovertible Reasons.

To represent distinctly what he undertakes to make out in this Tract, we may take notice of these particulars:

1. He takes pains to refute the Doctrine of Attraction, and to substitute in its place the Doctrine of Pulsion or Intrusion of Air into the Lungs.

2. He endeavours to assert, that the Lungs do not fall down, but are by the Breast contracted.

3. He affirms, to have clearly shew'd, what is the proper function and work of the Diaphragme, and other Muscles serving for Respiration.

4. He pretends, to have experimentally evinc'd the Genuine Use of Respiration, and the Benefit thence resulting to the Animal Life.
In short, He makes Respiration to be a Motion of the Thorax and Lungs, whereby the Air is sometimes impelled by the Nose, Mouth and Wind-pipe into the Lungs; and thence again expelled, farther to elaborate the Blood, by Refrigerating it, and by Separating its fuliginous Steam, and so raise it to its ultimate and highest perfection, for the Conservation of the Life of Animals.

Notice may be taken here by the by, that this Author in his Preface promises the publishing of a Treatise about Insects; in which he engages to shew many wonderful things in those little and seemingly contemptible Creatures, and in particular to demonstrate to the Eye the very method and manner how a Caterpillar is transmuted into a Chrysalis or Aurelia: By performing of which, he hopeth, he shall make the Curious bear more easily the loss of Dr. Harvey's Treatise on that Subject.


This Great Fish, dissected by the Parisian Philosophers, was a Vulpecula Marina (a Sea-fox:) in which they observed:

First, The length of his Tail, equaling very near the whole length of the rest of his body, (the whole Fish being 8\footlong; feet long) and fashioned after the manner of a Sithe, bowed and turned up toward the belly.

Secondly, His Mouth was armed with two sorts of Teeth; one sort in the upper Jaw, being pointed, hard and firm, and of one only bone, in the manner of a Saw: the other sort, found in the rest of the upper, and in the whole under-Jaw, were moveable, and fastened by fleshy membranes.

Thirdly, His Tongue did altogether adhere to the lower Jaw, and its skin was hard and covered with little shining points, which rendered it very rough and scabrous one way. The points viewed with a Microscope, appeared transparent like Chrysal.

Fourthly, His Throat was very large, and the Oesophagus, as large as his Maw, concerning which Authors say, that he hath the dexterity of disengaging himself from the swallowed hook, by casting it up together with his Maw, the inside of it turned out. They
found in his **Man** the Sea-herb, **Varec**, 5. inches long, and a **Fish** of the like length without head, scales, skin and guts, all being wasted but the musculus flesh, which remained entire.

**Fifthly,** The superior part of his great **Gut** had this peculiar, that instead of the usual circumvolutions of Guts, the cavity of this was divided transversely by many partitions, consisting of the membranes of the **Gut** turned inwards, and in the figure of a **Vice**, like Snail-thels, or winding Stairs.

**Sixthly,** His **Spleen** was double; his **Liver** divided into two Lobes; the **Gall** found to have more of bitter than fower: the **Heart**, without a **Pericardium**, as big as a Hens egg; the **Head** almost nothing but a mass of flesh, very little Brains in it, and that which was there, having very few meanders or windings: the **Eyes**, bigger than those of an **Oxe**, only half-spherical, flat before; the **Sclerotica** formed like a Cup, very thin, but very hard; the **Cornea** very tender and soft; the **Chrysalin** perfectly spherical; the **Vucle** grayish; the **Choroides** of the same colour, and pierced, for the production of the **Retina**, by a very large hole: the bottom of this **Choroides** had that lustre of Mother of Pearl, which is found in Terrestrial Animals, but with less vivid colours: and the **Retina** was also streaked with very apparent sanguineous Vessels.

The Observables in the **Lyon** were,

**In general,** that for outward shape, and the constitution of many parts, as the **Claws**, **Teeth**, **Eyes**, **Tongue**, (besides the likeness of the Viscera) a Lyon resembles very much a **Cat**.

**In particular,** an admirable structure of his **Claws**; a peculiar shape and position of his **Teeth**; a very stiffe **Neck**; a mighty rough and sharp **Tongue**, having points like claws both for hardnens and shape. **Eyes** very clear and bright, even after death, which without closing the Eye-lids, Lyons can cover with a thick and blackish membrane, placed towards the great Angle, which by raising it self and reaching towards the small Angle, can extend it self over the whole **Cornea**, as tis in **Birds**, but especially in **Cats**: The reverse of the anterior **Vucle**, where it lyes over the **Chrysalin** is altogether black: the **Chrysalin** very flat, and its greatest convexity, which is not usual, in its anterior part, as tis in **Cats**: the **Aqueous** humour very plentifull, equalling almost the sixt part of the **Vitreous**, which plenty was judged to be the cause of the brightness, that remains in the eyes after death.
His **Throat** was not above an Inch and a half large: the **Stomach**, 6 inches large, and 18 inches long: all the **Guts** 25 foot long: the **Liver**, divided into 7 **Lobes**, as in Cats; its cavity under the **Bladder of Gall** was full of Gall, shed abroad in the substance of the **Liver**, and of the neighbouring parts; which was suspected by the Physicians, administering this operation, to have been the cause of this Lyon's death: the **Bladder of Gall** was 7 inches long, and 1½ inch large, of a peculiar structure: the **Spleen**, a foot long, 2 inches large, and 1 inch thick: the **Kidney** weighed somewhat above 7 ounces: the **Genitals** of a peculiar conformation, causing this Animal to cast his Urine backwards, and to couple like **Camels** and **Hares**.

His **Lungs** had 6 **Lobes** on the right side, and 3 on the left: the **Wind-pipe** had its annular Cartilages entire, excepting two or three; it was above four inches in compass, being very firm, and by this bigness and firmness enabling a Lyon, strongly to thrust Air enough through it, for his dreadful roaring.

His **Heart** was dry, and without water in the **Pericard**, much greater in proportion, than of any other Animal, being six inches long, and four inches large towards the **basis**, and terminating in a sharp point. It had very little flesh, and was all hollow; the **Ventricles** very large; the **Auricles** very small: the proportion of the branches, which the ascending **Aorta** casts out, was such, that the **Carotids** were as big, as the left **Subclavial** branch, and as the rest of the right **Subclavial**, whence they issue; Which is considerable, seeing the **Brain** is so small: For the **Brain** was but two inches big, of any dimension; the rest of the head being very fleshy, and consisting of very firm **Bones**. By comparing the little quantity of the Lyons Brain with the plenty of that of a Calf, it was Judged, that the having but little Brain is rather a mark and a cause of a fierce and cruel temper, than want of wit. Which conjecture was strengthened by the observation formerly made in the **Sea-Fox**, in whom almost no Brains was found, though it be thought, that his craft and address hath occasioned men to give him that Name.

His Author reckons up 18 Opinions concerning Amber-grisse, and having examined every one of them, he embraces that, which holds, That it is the Dung of a Bird, (called in the Madagascar Tongue, Aschibobuch:) of which he gives the description out of Odoardus Barbosa and others; who affirm it to be of the bigness of a Goose, curiously feather'd, with a big head, well tufted. These Birds being found in great numbers in Madagascar, the Maldivies, and other parts of the East Indies, are affirmed by Authors to flock together in great numbers, as Cranes; and frequenting high Cliffs near the Sea-side, and there voiding their Excrement, the Sea washes it thence, if it fall not of its self into it.

There is another opinion among the said 18, for which the Author hath a good inclination, but yet dares not embrace it; viz. that 'tis the Excrement of a certain kind of Whales. If this Amber were but in those other places, where there is good store of such Whales, it seems that would make the Author relinquish the former Opinion.

This puts us in mind of a Relation, to be met with in Purchas, which, giving an Account of a certain Commission for a Gentleman to go Factor into Greenland for the killing of Whales and Mor-thes, takes notice, among other Particulars, of a sort of Whales, called Trompa, having but one Trunk on his head, whereas the Sarda, another kind of Whales, hath two. This Trompa, (faith the Author) hath teeth of a span long, and as thick as a man's Wrist, but no Finns. In his Head is the Sperma Ceti, faith he farther, and in his Entrails, the Amber-greese, being in shape and colour like Cowes-dung. Express order was given in the said Commission, that the person deputed should himself be present at the opening of this sort of Whale, and cause the residue of the said Entrails to be put in small Casks, and bring them along with him into England.

This will give occasion to increase our Inquiries for Greenland, which perhaps may be inserted in the Book of the next Month.
An Account

Of an Experiment made by M. Hook, of Preserving Animals alive by Blowing through their Lungs with Bellows.

This Noble Experiment came not to the Publisher's hands, till all the preceding Particulars were already sent to the Press, and almost all Printed off, (for which cause also it could not be mentioned among the Contents:) And it might have been reserved for the next opportunity, had not the considerableness thereof been a motive to hasten its Publication. It shall be here annexed in the Ingenious Author his own words, as he presented it to the Royal Society, Octob. 24, 1667. the Experiment itself having been both repeated (after a former successful trial of it, made by the same hand a good while ago) and improved the week before, at their publick Assembly. The Relation it self follows:

I Did heretofore give this Illustrious Society an account of an Experiment I formerly tried of keeping a Dog alive after his Thorax was all display'd by the cutting away of the Ribbs and Diaphragm, and after the Pericardium of the Heart also was taken off. But divers persons seeming to doubt of the certainty of the Experiment (by reason that some Tryals of this matter, made by some other hands, failed of success) I caus'd at the last Meeting the same Experiment to be shewn in the presence of this Noble Company, and that with the same success, as it had been made by me at first; the Dog being kept alive by the Reciprocall blowing up of his Lungs with Bellows, and they suffered to subside, for the space of an hour or more, after his Thorax had been so display'd, and his Asperra arteria cut off just below the Epiglottis, and bound on upon the nose of the Bellows.

And because some Eminent Physicians had affirm'd, that the Motion of the Lungs was necessary to Life upon the account of promoting the Circulation of the Blood, and that it was conceiv'd, the Animal would immediately be suffocated as soon as the Lungs should cease to be moved, I did (the better to fortifie my own Hypothesis of this matter, and to be the better able to judge of several others) make the following additional Experiment; viz.

The Dog having been kept alive, (as I have now mentioned) for above an houre, in which time the Tryal had been often repeated, in suffering the Dog to fall into Convulsive motions by ceasing to blow the Bellows, and permitting the Lungs to subside and lyestill, and of suddenly reviving him again by renewing the blast, and consequently the motion of the Lungs: This, I say, having been done, and the Judicious Spectators fully satisfied of the reality of the former Experiment, I caus'd another pair of Bellowes to be immediately joyn'd to the first, by a contrivance, I had prepar'd, and pricking all the outer coat of the Lungs with the slender point of a very sharp pen-knife, this second pair:
pair of Bellows was mov'd very quick, whereby the first pair was always kept full and always blowing into the Lungs; by which means the Lungs also were always kept very full, and without any motion; there being a continual blast of Air forc'd into the Lungs by the first pair of Bellows, supplying it as fast, as it could find its way quite through the Coat of the Lungs by the small holes pricked in it, as was said before. This being continued for a pretty while, the Dog, as I expected, lay still, as before, his eyes being all the time very quick, and his Heart beating very regularly: But, upon ceasing this blast, and suffering the Lungs to fall and lye still, the Dog would immediately fall into Dying convulsive fits; but be as soon reviv'd again by the renewing the fullness of his Lungs with the constant blast of fresh Air.

Towards the latter end of this Experiment a piece of the Lungs was cut quite off; where 'twas observable, that the Blood did freely circulate, and pass through the Lungs, not only when the Lungs were kept thus constantly extended, but also when they were suffer'd to subside and lye still, which seem to be Arguments, that as the bare Motion of the Lungs without fresh Air contributes nothing to the life of the Animal, he being found to survive as well, when they were not mov'd, as when they were; so it was not the subsiding or movelessness of the Lungs, that was the immediate cause of Death, or the stopping the Circulation of the Blood through the Lungs, but the want of a sufficient supply of fresh Air.

I shall shortly further try, whether the suffering the Blood to circulate through a vessel, so as it may be openly expos'd to the fresh Air, will not suffice for the life of an Animal, and make some other Experiments, which, I hope, will thoroughly discover the Genuine use of Respiration; and afterwards consider of what benefit this may be to Mankind.

FINIS.

In the SAVOY,
Printed by T.N. for John Martyn, at the Bell a little without Temple-Bar, and Nathaniel Brooks at the Angel in Gresham-Colledge, 1667.
PHILOSOPHICAL
TRANSACTIONS.

Monday, Novemb. 11. 1667.

The Contents.

A description of an Instrument for dividing a Foot into many thousand parts, and thereby measuring the Diameters of Planets to great exactness, &c. as it was formerly promised. An account of making a Dog draw his breath just like a Wind-broken-Horse. Divers Anatomical Observations on Humane Bodies. Several Instances of Peculiarities of Nature, both in Men and Brutes. A Confirmation of the Experiments, mentioned in Numb. 27. to have been made in Italy, by Injecting Acid Liquors into Blood. An Observation about the double Membrane call'd Epiploon, which covers the Entrails of Animals, and is fill'd with Fatt. Some Horatian Communications, about the curious Engrafting of Oranges and Lemons or Citrons upon one another's Trees, and of one Individual Fruit, half Orange and half Lemon, growing on such Trees. An imitation of away of preserving, in the more Northern Climates, Orange-Trees all winter long, without any Fire. Inquiries for Greenland. An Account of the Synopsis NOVAE PHILOSOPHIAE & MEDICINÆ Francisci Travagini, Medici Veneti.
some other impediments intervened, (after it was come to hand,) First on the Publisher's, then on the Engraver's side, the following Particulars concerning the same, promised some Months ago, had been imparted to the Publick a good while before this time. For the draught of the Figures, representing the New Instrument itself, and the Description of the same, we are obliged to the ingenuity of Mr. Hook.

The 1, 2 and 3 Figures, do represent the several parts of this Instrument; the 4th Figure, part of the Telescope with the Instrument applied to it, and the 5th, the Rest, on which the whole reposeth.

The 1. Figure represents the Brass-boxe with the whole Instrument, (excepting onely the Moveable Cover.) and the Screws, by which it is fixt to the Telescope. In this Figure (a a a a) is a small oblong Brass-box, serving both to contain the Screws, and its Sockets or Femal Screws, and also to make all the several moveable parts of the Instrument to move very true, smooth, and in a simple direct motion. To one end hereof is screwed on a Round plate of Bræs (bbbb) about 3 inches over; the extream Limb of whose outside is divided into a 100 equal parts, and numbered by 10, 20, 30, &c. Through the middle of this Plate, and the middle of the Box (a a a) is placed a very curiously wrought Screw of about the bigness of a Goose-quill, and of the length of the Box, the head of which is by a fixed Ring or Shoulder, on the Inside, and a small springing Plate, (d d) on the Outside, so adapted to the Plate, that it is not in the least subject to shake. The other end of this Screw is by another little Screw (whose small point fills the Center or hole made in the end of the longer Screw, for this purpose) rended so fixt and steady in the Boxe, that there appears not the least danger of shaking. Upon the Head of this Screw without the Springing-Plate, is put on a small Index (c c) and above that a Handle (m m) to turn the Screw round, as often as there shall be occasion, without at all endangering the displacing of the Index; it being put on very stiff upon a Cylindrical part of the Head, and the Handle upon a Square. The Screw hath that Third of it, which is next the Plate, bigger than the
other two-thirds of it, by at least as much as the depth of the small screw, made on it: the thread of the screw of the bigger third is as small again, as that of the screw of the other two thirds. to the greater screw is adapted a socket (f) fastned to a long barr or bolt (gg,) upon which is fastned the moveable sight (h,) so that every turn of the screw promotes the sight (h) either a thread nearer, or a thread farther off from the fixt sight (i,) The barr (gg) is made exactly equal and fitted into two small staples (kk,) which will not admit of any shaking. There are 60 of these threads, and, answerable thereto, are made 60 divisions on the edge of the bolt or Ruler (gg,) and a small index (l,) fixt to the boxe (aaa,) denotes, how many threads the edges of the two sights (h) and (i,) are distant; and the index (e e,) shews on the circular plate, what part of a revolution there is more; every revolution, as was said before, being divided into 100 parts. At the same time that the moveable sight (h,) is moved forwards or backwards, or more threads of the courser screw, is the plate (pp. in fig. 2.) by the means of the socket (q,) to which it is screw’d, moved forward or backward, or more threads of the finer screw: so that this plate, being fixt to the telescope by the screws (rr. in fig. 2,) so that the middle betwixt the sights may lye in the axis of the glass, however the screw be turn’d, the midst betwixt the sights will always be in the axis, and the sights will equally either open from it, or shut towards it.

Figure 2. represents the moveable cover containing the screws, to be by the bookseller cut off, by the pricked line (xxx,) from the paper, and to be firly placed on figure 1., according to the pricked line (yyy,) answering thereto; that by the taking off, as it were, or folding up of this cover, the inward contrivance of the screws and sights may appear.

And because it is conceived by some ingenious men, that it will be more convenient, instead of the edges of the two sights (h and i,) to employ two sights fitted with hairs, therefore is added figure 3., representing the two sights (r and s,) so fitted with threads (t and u,) than they may be conveniently us’d in the place of the solid edges of the sights (h and i.)

The 4th figure represents, how the screws are to be put on.
The Tube A D is divided into 3 lengths, of which (as in ordinary ones) B C is to lengthen or contract, as the Object requires. But A B is here added, that at A, you may put such Eye-glasses as shall be thought most convenient, and to set them still at the distance, most proper for them, Indexes or Pointers, which here are supposed to be at B. which length alters also in respect of divers persons' eyes, E, is a Screw, by which the Great Tube can be fixed so, as by the help of the figures, any smaller part of it can immediately be found, measuring only, or knowing the divisions on B C, the distance of the Object-glass from the Pointers. F is the Angular piece of wood, that lies on the upper Screw of the Rest. This Rest is represented by Figure 5.

As for a Description of the Uses of this ingeniously contrived and very curious Engine, the Reader is desir'd to look back to the first noted before alleged Numb. 25.

An Account

Of making a Dogg draw his Breath exactly like a Wind-broken Horse, as it was devised and experimented by Dr. Richard Lower;

with some of his Instructive Observations thereon.

This Experiment was made before the R Soc. Octob. 17. 1667. after it had been tried by the Author in private, some while before. The Account of it in his own Words and follows.

After I had often consider'd the manner and way of Respiration, and by many Observations been induced to believe, that the Diaphragm is the chief Organ thereof, I thought, there could be no way more probable to try it, then by breaking the Nerves, by which its Motion is perform'd: Which may be easily (as it was actually) done after the following manner;

First, pierce the side of the Animal between the 6. and 7 Rib in the middle of the Thorax, just over against the region of the Heart, with a small Incision-knife, passing the knife but just into the Cavity of the Breast (which you may justly know by finding no resistance to the point of it;) then take it out, and put in a Director, or a small Quill made like it, and thrust it in about an Inch, directing the end of it toward the Sternum, close to the inside of the Breast. Then cut upon it about an Inch on the Intercostal Muscles; by which you may be secure'd from touching the Lungs.
Lungs with the point or edge of your knife. This done, put in your finger, and with your nail separate the Nerve, which passes along the side of the Pericardium toward the Diaphragme. Then put in a Probe, a little inverted at the end like a hook, and apprehend the Nerve, and pull it to the Orifice of the Breast, and cut it off, and sow the hole up very close. Do the same on the other side, and presently let the Dog lose, and you will plainly see him draw his breath exactly like a Wind-broken Horse: Which yet you will see plainer, if you run him a little in a string after he is cut. But that any one may perform this Experiment the easier, let him first take notice, how the Nerves of the Diaphragme pass along on each side of the Pericardium in a dead Animal, before the trial be attempted in a Living one.

The most obvious Observations from this Experiment, are:

1. That the whole manner of Respiration is quite alter'd. For, as in a sound Animal, in Inspiration the Belly swells by the lifting up the Bowels by the Contraction of the Diaphragme, and in Expiration the Belly falls by the Relaxing of the same: In a wind-broken Dog or Horse 'tis quite contrary. For in them it is to be seen plainly, that when they draw their breath, their Belly is drawn in very lank and small, and when they breath again, their Belly is relaxed and swells again.

2. It being certain, that the Lungs do not move of themselves at all, but wholly depend upon the Expansion of the Thorax by the Intercostal Muscles, and the Diaphragme; by this Experiment it doth appear, how much the single motion of either of them doth particularly contribute to Respiration. For, all Inspiration being made by the Dilatation of the Thorax, and that Dilatation being caused partly by the Intercostal Muscles drawing up the Ribs, and partly at the same time the Diaphragme by its Contraction drawing downward the lower small Ribs, to which 'tis joyned, and also lifting up the Viscera of the lower Belly, by which they do jointly make all the space, they can, for the Air to come in and distend the Lungs: It must hence necessarily follow, that the Intercostal Muscles and the Diaphragme being constituted for two distant Employments (though both to the same end) and neither being able to perform the others Office, where one ceaseth from it's work, the other for the exigence of Nature must take more pains to supply.
happly the others defect. Which is very evident to be seen: for, the Diaphragme being made useless by loosing its Nerves, the Intercostal Muscles do dilate the Ribs much more than formerly, even to the utmost distance they can, when there is need for it; as, when you make the Dog run a little after he is cut, or when you gallop a Wind-broken Horse, doth manifestly appear.

3. The manner of Respiration being the same in a Dog, whose Diaphragme-nerves are cut, and in a Wind-broken Horse, 'tis more than probable, that the Cause may be as nearly the same, as the Signes are; and that, though there may be other faults found in the Lungs of such Creatures, yet 'tis very likely, they may be induced from the weakness of Respiration, but that they had their Occasion from the Relaxation or Rupture of the Nerves of the Diaphragme at first: which will seem more credible, if we re-

member, that by the streining of the Midriff too much (by which the Nerves may be quite broken or strecht beyond their proper tone) most commonly that accident happens.

Anatomical

Observations on a Humane Body, dead of odd Diseases; as they were communicated by Dr. Nathanael Fairfax.

A Young Maid of Rumborough in Suffolk, when she was about thirteen years of age, took Chalybeats for the Green-sick-

ness, and found some relief by it, but was after much pent in her wind. From 16 to 22, she much afflicted her self for the Death of her Father and Mother, and the misbehaviour of a Brother; during which time, she had every year an acute disease or two. At 18, she was very weakly, clogg'd in her Chest, and melancholy. If she went out in a windy day, she was fain to make haft in; for the wind, she said, was ready to choak her. She was a very slow Walker, going up-hill or up-stairs with much difficulty. She was now obserfed to be very thirsty, usually drinking at Bed-
time, and in the night too, sometimes; else, she said, she should be choak't with drought. Between 21, and 22, of her age, going down stairs, she heard a frightful Jolking in her Breast; which she then made known to the rest of the house, who when she shew'd them the manner of it by shaking her Body, joyn'd all with her in the wonder, concluding (as most would have done by the noise) that her Breast was almost full of water. She took fe-

veral
veral things of Dr. Browne and others at Norwich for about six moneths time, without finding relief. Half a year after, toward Michaelmas, upon taking a slight cold, she was so stop't up, that she could only whisper; nor could she lie flat, but rear'd up with pillows. I being sent for, caused presently a Vein to be open'd, as an Expedient only to make way for a freer Circulation, and room for Nature to disburthen her self. Within less than an hour she got breath, and soon after grew as well as she was before. She affirm'd, she never swet in her life, nor could it be procur'd by ordinary Sudorificks. Being desirous to adde an Empirical remedy, I gave her three of Matthews Pills, which did sweat her lightly, but beyond what ever she remembred. Several daily doses of Lockiers Pills, per dose, remov'd the Julking, as she said, lower to the Mid-riff: when she, fearing an Hyper-catarrhus, laid them by for two or three daies, and then taking them up again, could find no further alteration by them. She could never lie on her left side. In the 23. year of her age, in Winter, she had a dangerous Fever, with a Diarrhea, but came off. In her 24. in Winter again, she got cold, was quite stop't up, after five or six daies fell into Convulsion, and was bled, through want of care in those about her. By late bleeding she had present ease, and chear'd up in the Evening, but died the next Morning.

I had leave from her self, whilst living, and from her Relations, when dead, to open her Body; which I did accordingly.

First therefore I cleav'd aunder the Breast-bone from the Car-tilage, called EnSet-formis, to the neck; when, laying open the hollow of the Thorax, there steam'd out at first a very offensive smell, notwithstanding the sharp frost, there was at that time, it being about Christmas. Then making way to lay open either side of the Sternum, I was surpris'd to see (as I thought) almost the whole Cavity of the Thorax empty above, (as the Body lay supine) and fill'd with nothing but thick Milk beneath. But search-ing further, I found there was only all the right side of the Chest, and about a third part of the left, in that condition. It took up, in the part to the neck-ward a hand-breath, and ran three fi-ngers thickness to the left of the MediaStinum. The Liquor was like Cream, or rather like a size of Spanish White, having a cast of yellow, like Beefings. For, putting a spoon into it, from the bottom
bottom I took up a thick clammy matter, just like that Spanish White, that sinks to the bottom of its size. In quantity it might be about three pints, contain'd in a Bag, which was capable to hold as much more and better. The bag ran along from the left shoulder to the utmost of the right side of the Mid-riFF: not straight along nor stiffly stretcht; but about a hand-breadth from its rife it went directly down to the Midriff, with which it closed all along. Its skin or coat was thicker than that of the stomack, as well as its capacity larger, in as much as the Flexures of the Ribs joyn'd with it, and made up above half the compass. Where it adher'd to the Mid-riff, 'twas near a finger thick: And in one place, where I endeavor'd to separate it from the Mid-riff, I hit upon a thinner bagg, whence issu'd out 2 or 3 spoonfuls of shier water. How it got in, I found not. The Mediafinum was either wholly wafted, or else woven into the thicknes of the Bagg, as was also the Pleura, as far as the Bagg reach'd. It lay loofe and flapping from the left Axillar to the Chefe, having been before fill'd and distended either with fene or the Liquor. All the hollow was bedabed with the wallowings of the liquor about, as is the Oufe by the Ebbings and Flowings of the Tide in a Channel. That Lobe of the Lungs, which should have been on the right, was gone, and that on the left, wafted to near a third part. In the Lower Belly all was well.

Dr. Brownfaith, he hath met with the like in an Italian Author. His opinion was to salivate her. I had thoughts of a Paracenthesis, or Tapping between the Ribs. For by the noife of the Liquor, and by her not enduring to lye on the left, I concluded it must be in a Cystis on the right. But if that had been done, the Bagg being too thick might have mortified. The Jolking was exactly like that of Water or Milk. This Woman was as flat-breasted as a Man. Whether the Liquor proceeded from the falling down of the Chyle from the Axillars, is a quare, but seems to carry in it somewhat of probability. But I must not reflect.

Two other Anatomical Observations, impart'd, by the same hand.

1. A certain Serving-man about 27 years of age; dyed Hydrofical, which Disease he was molested with, 4 years before his death. He was
was ever a littleless, dull and melancholy fellow, never cheerful nor smiling, especially for ten years before he died. His words came from him as if forced, and speaking but a little, he would end with a sigh. When open’d, he was found to have the left Lobe of the Lungs almost quite wafted; but no Ulcer, nor ought preternatural appearing in the remaining part, except its wafting. The heads of the Vessels and branches of the Wind-pipe as big, as in the other Lobe. That Lobe of the Liver, which buts on the Mid-riffe, was black outwardly for about a hand-breadth and about a thumbs-breadth within the Parenchyma. Other parts found.

2. The other day I took notice in the Corps of a Felon, that, whereas ordinarily the Preparing Vessels arise, on the right side, out of the Cava, as on the left, out of the Emulgent, his right Vas preparans sprang cleerly from the right Emulgent.

Divers Instances

Of Peculiarities of Nature, both in Men, and Brutes; Communicated by the same.

1. One Mr. Morley of Bury St. Edmunds in an Asthmatick distemper, was advised by some to take down a spoonfull of good English Honey, which being done, the Patient fell into an Universal swelling, as if he had swallow’d the worst of Poysons. Mr. Goodrich being hastily call’d in, to save life, prescribed him a common Sudorifick, which in competent time relieved him. They then made inquiry at the Apothecary’s, Whether nothing were amiss in the Honey; and they protested, it was altogether right. But to be assur’d of it by Experiment, they afterwards got the like quantity at another place, which was given with the very same frightful event, and the Party was cured by the same Chirurgion (who is my Author) with the same kind of sweat.*

2. Mr. Twisse, a Minister of Metigham in Suffolk, about forty years of age, having been accustomed for some time to drink warm or rather hot Beer, which was then unknown to the Chirurgion, as was to the Patient the mixture; ) the place affected did soon after rankle, and grow so bad, that the Lady was constrained to send for him that had applied it, who being examined about the Ingredients, and declaring one of them to be Honey, the Lady soon acquainted him with her Antipathy to that substance: whereupon that Application was immediately removed, and another more proper for the Patient put in the place, with good success.

* The like Example hath been more than once related to the Publisher by a very credible person, of a Noble Lady in Ireland, who having received a small hurt on her Leg, and the chirurgion mingling in the Application, he made to it a little Honey (from which she hath an utter Aversion, which was then unknown to the Chirurgion, as was to the Patient the mixture; ) the place affected did soon after rankle, and grow so bad, that the Lady was constrained to send for him that had applied it, who being examined about the Ingredients, and declaring one of them to be Honey, the Lady soon acquainted him with her Antipathy to that substance: whereupon that Application was immediately removed, and another more proper for the Patient put in the place, with good success.
and coming from his House about Mid-summer to a house near Rumburgh-Church, where he was offered a Cup of cold Beer, out of modesty, or a humor to prevent the being wondered at, took it off thus cold, after he had taken a Pipe of Tobacco. Which done, he presently took horse, and rode with other Company towards Framlingham. Coming at Halewith, he found himself sick, his stomach much out of order. He lighted once or twice by the way and vomited, but coming at his Journeys end, his vomiting grew worse, and he was constrain'd to betake himself to his bed. Next day he grew yet worse, could find no help by Phisick, but died the very next morning.

It may be worth noting (adds the Author) that one, who is wont to drink cold Beer, is not, for ought we know, endanger'd by a draught of hot Beer: But I cannot tell, whether it may be thence interr'd, that hot things are more agreeable to the natural Tone of the Stomach, then cold. That it was not barely the coldness of particles, sensible to the Touch, appears, because the same Party could drink cold Wine, as I was inform'd from my own Father.

3. Madam Mary Brook of Yoxford hath such an Aversion to Wasps, that whilst their season of swarming about in Houses lasteth, she is forc'd to confine her self to a little close Chamber, and dares not then come out to Table, leaft their coming there should put her into such distempers, as Cheese doth those, who have an utter Antipathy against it.

4. Mrs. Raymund of Stow-market, when ever she hears Thunder, even a farr off, begins to have a bodily distemper seize on her. She grows faint, sick in her stomach, and ready to vomit. At the very coming over of it, she falls into a right down Cholera, and continues under a Vomiting and Looseless, as long as the Tempest holds, and that in a more violent way, than is commonly procured by such Medicaments as are usually exhibited for those very purposes. And thus it hath been with this Gentlewoman from a Girle.

5. I know a Woman in Stow-market, who, during her Green-sicknes, was invited by her Pica or longing, to suck the Wind out of Bellows, which as often as she could she took into her Body with open mouth, forcing it in by blowing with her own hands, the
the Bellowes inverted. I know another that was for crackling of
Cinders under her feet. From which kind of Instances I am in-
clined to doubt, whether that Distemper begins at the Deprava-
tion of the Acid liquor in the Stomach, and not rather at the Ute-
rus, which next infects the Brain, such kind of things gratifying
the Fancy somewayes mislaid, more than the Appetite natural
any wayes deprav'd.

6. Somewhat, like to this, is to be found in Brutes. In May
last a Greyhound Bitch at Brightwell-Hall, about five or six dayes
before she cast her Whelps, had such a wild kind of Hunger
(though she was fed sufficiently every day with usual food) that,
finding another Bitch's Whelps, she devour'd them all (4 or 5, as
I remember) and fell next upon the Bitch her self, who made a
shift to get from her as well as she could, being help'd. From
this, and from Sows devouring whole Litters of Pigs, I am prone
to think otherwise of the Longings of Teeming Women, than is the
common opinion.

A Confirmation
Of the Experiments, mention'd in Numb. 27, to have been made
by Signor Fracassati in Italy, by Injecting Acid Liquors into
Blood.

The Honourable Robert Boyle, having seen the particulars in-
serted in Numb. 27, concerning some Experiments made
by Signor Fracassati, and recollecting, what himself had experi-
mented of that nature, several years ago, was pleased to give to
the Publisher the following Information about it, by the favour of

Sir,

Hinted to you in my last something about the Original of the Ex-
periments, made in Italy, by Injecting Acid Liquors into Blood:
To explain which, I shall now tell you, that
about this time three years * I mention-
ed at Gresham Colledge to the Royal
Society an odd Experiment, I had for-
merly made (not by Chance, but De-

* The Journals of the Royal
Society being looked into by
the Publisher (who, by the ho-

nour of his Relation to that Il-
lustrous Body, hath the advan-
tage of perusing them, as he by
his Office hath the Care of seeing them faithfully managed) do fully agree with the Affirma-
tion of this Noble Person, as well in the Circumstance of the Time, as the Substance of
the Matter in question; it being in the Month of December of An. 1664, when, what is
now alleged in this Letter, was publickly related by its Author.
... upon Blood yet warm, as it came from the Animal, viz. That by putting into it a little Aqua fortis, or Oyl of Vitriol, or Spirit of Salt, (these being the most usual Acid Menstruums,) the Blood not only would presently loose its pure colour and become of a Dirty one, but in a trice be also coagulated; whereas if some fine Vinous Spirit, abounding in Volatil Salt, such as the Spirit of Sal Armomique, were mingled with the warm Blood, it would not only not curdle it, or imbaze its Colour, but make it look rather more florid than before, and both keep it fluid, and preserve it from Putrefaction for a long time.

This experiment I devis'd, among other things to shew the Amicableleness of Volatil Spirit to the Blood. And I remember it was so much taken notice of, that some very Inquisitive Members of the Society came presently to me, and desired me to acquaint them more particularly with it, which I readily did, though afterwards I made some further Observations about the same Experiment, that I had no occasion to relate.

This having been so publickly done, though I shall not say, that Signor Fracablati may not have hit, as well as I, upon the Experiments published in his Name, yet there is so little difference between the warm Blood of an Animal out of his Veins and in them, that 'tis not very improbable, that he may have had some imperfect Rumor of our Experiment without knowing whence it came, and so may, without any disingenuousness, have thence taken a hint to make and publish what now is English'd in the Transactions. If it be thought fit, that any mention be made of what I related so long since, I think, I can send you some other Circumstances belonging to it. For I remember, I tried it with other Liquors (as Spirit of Wine, Oyle of Tartar, Oyle of Turpentine,) and I think also, I can send you some remarks upon the Colour of the upper part of the Blood. And I shall on this occasion add in reference to Anatomical matters in general, that after I saw, how favourably the Usefulness of Experimental Philosophy was receiv'd, I was invited to inlarge it in another Edition; and for that, I provided divers Anatomical as well, as other Experiments, and design'd many more, so that I have by me divers things, that would not perhaps be unwelcome to Anatomists, &c.

An Observation

About the Epiploon, or the Double Membrane, which covers the Entrails of Animals, and is fill'd with Fat.

This Observation should have been added to those that were publish'd in Number
The Epiploon, being look’d upon by a good Microscope, is like a great Sack, full of abundance of other small Sacks, which do inclose Gatherings of Grease or Fat. There are many Vessels, which may be call’d Adipous or Fatty, which issue out of this Membrane, and spreading themselves all over the Body, convey Fat to it, just as the Arteries carry the Blood all over the same. Wherever is Fat or Grease, there is found store of these little Sacks, wherein that is inclosed, whence it is, that in lean and emaciated Bodies, in stead of Fat, you find nothing but skins.

The structure of these small Sacks and of the Adipous vessels sufficiently sheweth, that the Fat is not form’d accidentally out of the thick Vapours of the Blood, as is the common belief. Nor is its chief Use, to foment the Natural heat; but it seems rather to conduce to the allaying of the Acrimony of the Salts, that are in the Blood and the Serosities. And indeed (saith this Author) Lean persons, and those, whose Epiploon hath been cut, are more subject than others to Rhumatismes, Liereties; and the like Diseases that are caused by the Sharpness of the Humours. And those that are fatt, are not so easily seiz’d on by them, in regard the Acrimony of the Serosities is corrected by the Mixture of the Fatt, just as the sharpest Lixium will loose its force, if Oyl be mingled therewith.

Some Hortulan Communications about the curious Engrafting of Oranges and Lemons or Citrons upon another’s Trees; and of one Individual Fruit, half Orange and half Lemon, growing on such Trees, &c.

We have here Orange-trees, (saith the Intelligence from Florence) that bear a fruit, which is Citron on one side, and Orange on the other. They have not been brought hither out of other Countreys: and they are now much propagated by Engrafting.

2. This was lately confirmed to us by a very Ingenious English Gentleman, who asserted, that himself not only had seen, but bought of them An. 1660, in Paris, whither they had been sent by Genoa-Merchants; and that on some Trees he had found an Orange.
on one branch, and a Lemon on another branch; as also, (consonantly to the Florentine information) one and the same Fruit half Orenge and half Lemon; and sometimes three quarters of one kind, and one quarter of the other.

3. A Provencal at Paris pretends to keep Orenge-trees in that Town all the winter long without any Fire, though they remain in the Earth, and not be put in Gaisses or Boxes. This is thought to be effected by a peculiar sort of Dung, used for that purpose, and wrought deep into the Ground.

Q. Why should not the Experiment of some such thing be made about London, whose Latitude is but so little more North-ward than that of Paris?

Inquiries for Greenland.

To discharge our Promise made in the last Transactions, we shall subjoin the following Queries, which we also purpose to recommend in due season, to some of those English Masters of Ships and other fit Persons that shall Sail into Greenland for the Whale-fishing: Intreating withal, as many as have convenience, to assist us in these recommendations.

The Inquiries are

1. What, and how much is the heat of the Sun there in the midst of the Summer, compar'd with the heat of it in England? to be observed with a seal'd Thermometer.

2. What is the most constant weather there in Summer, whether Clear, Cloudy, Rainy, Foggy &c.

3. What weather is most usual at such and such times of the year?

4. What constancy or unconstancy there is of the Wind to this or that quarter of the Horizon, or to this or that part of the year?

5. What the Temperature of each particular Wind is observ'd to be? And particularly, whether the North-wind be the coldest? If not, what wind is? whether is the colder, the East or West, &c.

6. What wind is observed to bring most Ice, and what to make a clear water at Sea?

7. What Currents there are? How fast, and which way they set? Whether those Currents are not stronger at one time of the Moon than at another? Whether they always run one way?

8. What is Observable about the Tydes, Spring or Neap?
How high the High-water mark is above the Low-water? Which way it floweth? which way it ebbeth? what time of the Moon the Spring-tides fall out?

9. Whether the Ice that floats in the Sea be of Salt-water or Fresh:

10. What Rivers there are in the Summer, and what fresh water can be had:

11. What Fowl are found to live there, and what Beasts? How they are thought to subsist in Winter? How they breed and feed their young:

12. What Vegetables grow there, and whether they yield any Flowers or Fruits, &c.:

13. Whether there have been any Thunder or Lightning observed in those parts:

14. How deep the Cold penetrates into the Earth? whether there be any Wells, Pitts or Mines so deep, that the Cold does not touch the bottom thereof:

15. How the Land trends? and whither the Parts under or near the Pole be by those, that have gone furthest that way, thought to be Sea or Land: How near any hath been known to approach the Pole; & whether the Cold increaseth with the increase of Latitude?

16. To make, if possible, some Experiments and Observations about the Magnet or Needle; and particularly, How much the Declination is there? and whether they doe exactly observe the Degrees of Declination in their course? Likewise to make Observations about the Height of the Sun and other Celestial Bodies, and their Diameter, Refractions? &c.

17. What is their opinion concerning the North-East passage?

18. What Fish do most frequent those Seas, besides Whales? Any thing observable in their Fishing; as the Usual or Unusual bigness, strength, and the several sorts of Whales; and particularly to observe whether that kind of Whales they call Trompa, have in their Heads the Spermaceti, and in their Entrals the Ambergris, looking like Cow's-dung, as was alleged out of Purchas in Numb. 28. pag. 538?

19. To give in an exact Relation of the Whale fishing, throwing the Harp-irons, following the Fish, &c.

20. To describe the whole manner of making the Oyl of Whales.

An Account of the

SYNOPSIS NOVAE PHILOSOPHIAE & MEDICINÆ Francisci Travagini Medici Veneti.

Some months since there were two Letters sent hither from Venice, from Signior Francisco Travagino, giving notice of a Treatise of his, ready for the Press, under the Title of NOVAE PHILOSOPHIAE & MEDICINÆ.
MEDICINA. Those Letters came accompanied with a Synopsis in Print, giving a brief Account of the Contents of the said Treatise, to this effect, viz:

That this Author hath compos'd a System of Natural Philosophy by Observations and Experiments, accomodated to the benefit of Humane Life, and subservient to Physick and other subalternate Arts; which Philosophy he pretends to have rais'd on Principles, that are certain Bodies drawn out of Mixts; which, though in themselves invisible and incoagulable, yet become, according to him, visible by their Contrariety and mutual Operation upon one another, and so do constitute the Temperaments of Concretes, and cause not only their Dissolution, but also their Redintegration.

These Principles he undertakes to prove to be Two Salts, call'd by him Acidum and Sallum; which, as they work more or less on one another, when blended, so they lose more or less of their Volatility, and the degrees of their contrariety: And from their various Complication (in which he places the whole business and moment of Philosophy) he holds, that that great Multiplicity of Concretes, which is in the Universe, does result.

In particular he deduceth from the said Principles the cause of Ferments and their Variety, the nature of Generations, Concretions, Putrefactions, Precipitations, &c. and the wheath, how those Principles run through all Minerals, Vegetables and Animals, by their manifold combinations, and various ways of acting on one another.

He explains also the Mixtures of Alkaly's, Vitriols, Armoniaks, Sulphurs, Mercuries, and explicateth the Properties of Dissolvents, as also Tafts, Odors, Colors, &c. all from the same principles.

And having rais'd this Structure of his, as far as he judgeth it sufficient for Subordinate Arts, he proceeds to adapt it to the Art of Physick. And applying it to Animal Bodies, he thence draws the Diversity of Humors and Tempers, the Beginning and Duration of Vital Heat, the Motion of the Limbs, the Faculties of Entrals, the Origin, Vitality and Properties of the Blood, and the various Fermentations therein; shewing the Distempers of the Ferments and Juices in Animals, the nature of Coagulations, Dissolutions, Feavers and other Symptoms; as also the Original of Poylons in Animal Bodies; concluding with an Indication of the proper remedies (as he conceiveth) of many Diseases.

Whether this Philosophy be New, is ease to judge.

A Note to be insert'd above, pag. 544. after lin. 12.

This Refl (by Mr. Hooke's suggestion) may be render'd more convenient, if, instead of placing the Screw Horizontal, it be so contriv'd that it may be laid parallel to the Equinocials, or to the Diurnal motion of the Earth. For by that means the same thing may be perform'd by the single motion of one Screw, which in the other way cannot be done, but by the turning of both Screws: As will easily appear to thole that shall consider it.

In the S A V O Y:

Printed by T. N. for John Martyn, Printer to the Royal Society, and are to be sold at the Bell a little without Temple-bar. 1667.
An Account of the Experiment of Transfusion, practised upon a Man in London, A Narrative of some Trials of Transfusion, lately made in France. Some New Experiments of Injecting medicated Liquors into humane Veins, together with some considerable Cures perform'd thereby. An Extract of a Letter written from the Bermudas, giving an Account of the Course of the Tides there; of Wells both salt and sweet, digg'd near the Sea; of the Whale-fishing there practis'd anew, and of such Whales, as have the Sperma Ceti in them. A Method for finding the Number of the Julian Period, for any year assign'd, the Number of the Cycle of the Sun, the Cycle of the Moon, and of the Inductions, for the same year, being given. An Account of some Books. I. PETRI LAMBEII LIB. PRIMUS PRODROMI HISTORIÆ LITERARIAE. II. THOMÆ CORNELII PROGYMNASMATA PHYSICA. III. LES ESSAYS de PHYSIQUE du Sieur de LAUNAY. IV. FRANCISCI DU LAURENS SPECTMINA MATHEMATICa, duobus Libris comprehensa.

An Account of the Experiment of Transfusion, practis'd upon a Man in London.

This was perform'd, Novemb. 23. 1667. upon one Mr. Arthur Coga, at Arundel-House, in the presence of many considerable and intelligent persons, by the management of these two Learned Physicians and dextrous Anatomists Dr. Richard Lower, and Dr. Edmund King, the latter of whom communicated the Relation of it, as followeth.

The Experiment of Transfusion of Blood into an humane Veine was made by Us in this manner. Having prepared the
Carotid Artery in a young Sheep, we inserted a Silver-Pipe into the Quills to let the Blood run through it into a Porringer, and in the space of almost a minute, about 12 ounces of the Sheep's blood ran through the Pipe into the Porringer; which was somewhat to direct us in the quantity of Blood now to be Transfus'd into the Man. Which done, when we came to prepare the Veine in the Man's Arme, the Veine seem'd too small for that Pipe, which we intended to insert into it; so that we employed another, about one third part less, at the little end. Then we made an incision in the Veine, after the Method, formerly publish'd Numb. 28; which Method we observ'd without any other alteration, but in the shape of one of our Pipes, which we found more convenient for our purpose. And, having open'd the Veine in the Man's Arme, with as much ease as in the common way of Venæ-secti- on, we let thence run out 6 or 7 ounces of Blood. Then we planted our silver-pipe into the said Incision, and inserted Quills between the two Pipes already advanced in the two subjects, to convey the Arteriall blood from the Sheep into the Veine of the Man. But this Blood was near a minute, before it had past through the Pipes and Quills into the Arme; and then it ran freely into the Mans veine for the space of 2 minutes at least; so that we could feel a Pulse in the said veine just beyond the end of the Silver-pipe; though the Patient said, he did not feel the blood hot (as was reported of the subject in the French Experiment) which may very well be imputed to the length of the Pipes, through which the blood passed, losing thereby so much of its Heat, as to come in a temper very agreeable to Venal Blood. And as to the quantity of Blood receiv'd into the Man's Veine, we Judge, there was about 9 or 10 ounces: For, allowing this pipe 12 ounces, than that, through which 12 ounces pass'd in one minute before, we may very well suppose, it might in two minutes convey as much blood into the Veine, as the other did into the Porringer, in one minute; granting withall, that the Blood did not run so vigorously the second minute, as it did the first, nor the third, as the second, &c. But, that the Blood did run all the time of those two minutes, we conclude from thence; First, because we felt a Pulse during that time. Secondly, because when upon the Man's saying,
He thought, he had enough, we drew the pipe out of his Veine, the Sheeps-bloud ran through it with a full stream, which it had not done, if there had been any stop before, in the space of those two minutes, the bloud being so very apt to coagulate in the Pipes upon the least stop, especially the Pipes being so long as three Quills.

The Man after this operation, as well as in it, found himself very well, and hath given in his own Narrative under his own hand, enlarging more upon the benefit, he thinks, he hath received by it, than we think fit to own as yet. He urged us to have the Experiment repeated upon him within three or four dayes after this; but it was thought advisable, to put it off somewhat longer. And the next time, we hope to be more exact, especially in weighing the Emittent Animal before and after the Operation, to have a more Just account of the quantity of Blood, it shall have lost.

A Relation
Of some Trials of the same Operation, lately made in France.

1. M. Denys, Professor of the Mathematicks and Natural Philosophy at Paris, in a Letter of his to the Publisher relateth, That they had lately transmitted the Blood of four Weathers into a Horse of 26. years old, and that this Horse had thence received much Strength, and more than an ordinary Stomach.

2. The same person was pleased to send to the same hand a Printed Letter, written to the Abbot Bourdelot by M. Gadroys, being an Answer to a Paper of one M. Lamy, and confirming the Transfusion of Blood by New Experiments. In this Answer the Author is vindicating the Transfusion from Objections; where first he takes notice, That, whereas the Objector undertakes to refute the Experiments made, by simple Ratiocinations, it ought to be considered, that the Quodlibeticall Learning of the Schools is capable enough to find Arguments for and against all sorts of Opinions, but that there is nothing, but Experience, that is able to
give the *Verdict* and the last *Decision*, especially in matters of *Natural Philosophy* and *Physick*. That a hundred years agoe, there were no Arguments wanting to prove, that *Antimony* or the *Vinum Emeticum* was poiyon; the use of it being then forbidden by a *Decree* of the *Faculty* of *Physitians*; and that at this day there are no arguments wanting, to prove the contrary, and to affer, That it is a *Purgative* of great importance, follow'd with wonderfull effects; the same *Faculty* having Publish'd a *Decree* the last year, by which it permits, and even *ordains* the use thereof. So that it ought to be said, that *Sole Experience* hath determin'd this matter, and that the *Recovery* of many persons, and amongst them, of the *Most Christian King* himself, hath more conduced to convince Men of its usefulness, than all the bare *Ratiocinations*, that could be employed to defend it. And so it is with all *Remedies*, there being not one, that is not approved by some *Physitian* or other, who thinks to have reason on his side, and disapprov'd at the same time by others of that Profession, who conceive to have it on theirs: Whereas *He* certainly is to be esteemed the most *Rational*, that in these matters is guided by good *Experience*. And since the *Transfusion of Blood* is a *New* thing, (unknown for ought we know) to all former Ages, Ingenious Men, and Lovers of the Increase of the Stock, serving for the relief and conveniencies of Human Life, do no more, in this particular, than propose and recommend it to *Generous* and *Un-prejudicate Physitians*, to Judge of its agreeableness to *Human Bodies*, and to make trials of it accordingly; themselves esteeming, that since it concerns the Health and Life of Man, it cannot be examined too severely; though at the same time they conceive, that 'tis unequal, to stand herein to the *verdict* of such Arrogant Men, who from a self-conceit of knowing all things already, are very impatient at any thing discover'd, which they have not thought on themselves: Those Men being the best advised and the most to be relied on, who do not precipitate their Judgment, but stay for many *Experiments*, carefully made, to conclude themselves by. For which purpose, the *Author* wishes, that Persons in power would cause a good number of *Experiments* of this *Invention* to be made, and examine them either themselves, or give order to prudent and free-spirited *Physitians* and *Chyrurgians* to do so.
Among the objections, (which the Author finds to be generally grounded upon inconsiderations, mistakes, and a supposition, as if peremptory Affirmations touching the effects of this Transfusion were obtruded, whereas all is left to the success of Experiments faithfully made) there is one, directed against the effects of that operation, which appeared in the young Man, who (by Mr. Denys’s Relation in his Printed Letter to Monſieur de Montmor) after he had received the Arteriall Blood of a Lamb, was cured of an extraordinary Lethargy, consequent to a violent Fever, wherein he had been let blood 20 times. And the objection is, That the lively apprehension, the said young man had of a remedy so unusual, and whereof the success could not but appear very dubious to him, and so render him exceedingly anxious, did so rouse his spirits, and put them into such motion, as to disengage them from that embarraiment, which hindered their diffusion, upon which dis-entanglement follow’d all the other good effects, that are imputed to the Transfusion.

To this Conceit the Answerer replyes, That, if the apprehension could have cured this young Man, the cure would doubtless have been effected 24 hours before the Transfusion, because he then happened to have a very great one, by falling down stairs; as was also observed in Mr. Denys’s Relation of this Experiment. Besides, that this Patient was noted to be so far from apprehending or fearing this Operation, that he did not so much as know what the Transfusion was; but thought, the Lamb was onely applied to his Arm, to suck from him his ill Bloud, as he was made to believe, after an ancient and usuall way.

To that Objection, wherein some put weight, viz. That there is a great difference between the Fleſh, we eat for food, and the Blood, that is transmitted immediately into the Veines; the former undergoing a great Alteration, which the latter does not: Our Author replyes, That of the three principall Digestions of the Aliment, that have been always distinguished by Authors, the First, which is made in the Stomach, is not considerable in comparison of the two others, which are made of the Chyle and the Bloud, in the Heart, the Liver, and generally in all the parts, that receive nutrition. Which he illustrates by this, that as the Concoction, which is made of the Juyces of the Earth in the Root
and Heart of the Trunk of a Tree, does not so much serve to the production of this or that Fruit, as the last Filtration, that is made of those Juices in the small Fibres of the Gifts; so also all those Digestions, which are supposed to be made in the Stomack and the Heart or the Liver of Animals, do not so much serve to give the particles of the aliment those Figures, which they require to be converted into the substance of Man, as the diversity of pores, that straine them last of all, and differ in the Bones, Flesh, Cartilages, and other parts, in which the Ancients for this reason did admit as many different Assimilating facultys. Now, saith he, though the New Blood, which is given in the Transfusion, undergoes not the first Concoction, made in the Stomack, yet it suffers the two others, in making many Circulations together with the native blood; and that therefore nothing hinders, but it may be fit to be changed into the substance of Man, without inconvenience.

The rest of the Objections, here allledged, seeming to be of no moment, though allvver'd by our Author, we shall here pass by, and, for a general answer to all, employ Experience, and the several successfull Transfusions, he relates, as those of Lambs-blood into Dogs, which, after the space of several moneths from the time of the Operation, do not onely live, but are very well, and some of them grown fatter, than they were before, and of Kids-blood into a little Spaniel-bitch, of 12 years of age, which, a little while after the Operation, grew vigorous and active, and even proud in less than eight dayes. To which he adds a considerable Experiment, lately made upon a person, that had been for three weeks afflicted with the complicated distempers of an Hepatick Flux, a Lientery, and a bilious Diarrhaa, accompanied with a very violent Feaver, and had been attended by four Physicians, who having bloouded, purged, and clystered him, as much as they thought fit, he grew at last so weak, that he was unable to stir, lost his speech and senses, and vomited all he took whereupon they altogether despairing of, and abandoning the Patient, and declaring, that they did fo, in the presence of divers persons of honor, consented to have the Experiment of Transfusion made upon the Patient, which his Relations had proposed, as the last Refuge, very unwilling; to omit any thing, that might seem probable to rescue a dying Man.
man. M. Denys and M. Emmerey were besought to employ this last succours. But they, seeing the deplored state of the Sick, absolutely refused to make the tryal, alleging, that the Transfusion was not a mean to restore either the solid parts, or to cure a Gangrene, which was apparently in his Intestines; they should have used it sooner, and at the very time, when the great evacuations of blood were made in the Patient. But, notwithstanding all this, they were exceedingly preff'd, to comply with their desires, and not to let their friend dye without trying all means possible. They, being overcome by this importunity, and having secured their honor and safety, by the declaration above mentioned of the Phystians, and by their consent to the tryal of this Experiment, transfused into his veins a small quantity of Calf's-blood in a morning; whereupon, though this Patient was already in a Lethargy, and convulsive, and had a very low and creeping pulse, yet, behold, an unexpected change hapned to him. His pulse grew higher in an instant, and became more vigorous; his Convulsions ceased, he look'd fixedly on the By-standers, spoke pertinently, and in divers languages to those, that spoke to him, and fell into a very quiet sleep. Awakening three quarters of an hour after, he took several broths for the rest of the day, not vomiting at all, nor having any stool, although for three days before he could take nothing at the mouth, nor had had any intermission of his loosestnees since the very beginning of his sickness. Having thus remined for 24 hours, his forces began to diminish again; and his pulse to grow low, and the loosestnees to return. His friends then urged a second Transfusion, which being at last performed the next morning, the Patient indeed recover'd some vigour again, but that was of a short duration. For though then also he took his broath well, without vomiting, yet he voided still by stool, and at noon he began to decline, and about 5, at night he dyed, without the appearance of any convulsions. His Body being opened before the Phystians, the Inlet was found return'd into itself from the top to the bottom, and below that knot unto the anus the Baw-els were all divid, gangren'd, and of an unsupportable stench. His Pancreas was extraordinarilily hard, and so obstructed, that the Pancreatick Juice had no liberty to diffuse itself into the Guts.
His Spleen was very thick, and his Liver big, and in some places livid. The Heart very dry, and, as were, burnt. And having found the Vein, by which the Transfusion had been made, there was, from the place of the opening of the Arm, to the Heart, almost no blood found in it, no more than in the other Veins, nor in the Ventricles of the Heart, for as much as that little, he received, had been imbibed by his hot and drie Flesh. All which, this Author assures, can be attested both by a douzen persons of great veracity, who were present at this dissection, and confirmed by the Certificates given by the Physician themselves, to be sent to the Parents of the deceased Stranger, who is the very same with him, of whom a less punctual account was given Numb. 28. p. 519.

Some New Experiments
Of Injecting Medicated Liquors into Veins, together with the considerable Cures, performed thereby.

This was lately communicated in a Letter from Dantzick written by Dr. Fabritius, Physician in Ordinary to that City, which out of the Latin we thus English.

Eorasmuch as we had a great desire to experiment, what would be the effects of the Chyrurgerie of Injecting Liquors into Humane Veins, three fit Subjects presenting themselves in our Hospital; we thought good to make the Trial upon them. But seeing little ground to hope for a manifest operation from only Altering Medicines, we esteemed the Experiment would be more convenient and conspicuous from Laxatives; which made us inject by a Syphon about two Drachmes of such a kind of Physick into the Median Vein of the right Arm. The Patients were these. One was a lusty robust Soldier dangerously infected with the Venereal Disease, and suffering grievous Protuberations of the bones in his Arms. He, when the purgative liquor was infused into him, complained of great pains in his Elbows; and the little valves of his Arm did swell so visibly, that it was necessary by a gentle compression of one's fingers to stroke up that swelling towards the Patients Shoulders. Some 4 hours after, it began to work, not very troublesome; and so it did the next day, to such a degree, that the Man had five good stools after it. Without any other;
other remedies those protuberances were gone, nor are there any footsteps left of the abovementioned Disease.

The two other Trials were made upon the other Sex. A married woman of 35, and a serving Maid of 20 years of age, had been both of them from their Birth very grievously afflicted with Epileptick fits, so that there was little hopes left to cure them. They both underwent this operation, and there was injected into their Veins a Laxative Rosin, dissolved in an Anti-Epilepticall Spirit. The first of these, had gentle stools, some hours after the Injection, and the next day the fits recurring now and then, but much milder, are since altogether vanished. As for the other, viz. the Maid, she went the same day to stool 4 times, and several times the next; but by going into the air, and taking cold, and not observing any diet, cast her self away.

Tis remarkable, that it was common to all three, to vomit soon after the injection and that extremely and frequently; the reason whereof we leave to intelligent Physicians to assigne.

An Extract

Of a Letter, written from the Bermudas, giving an account of the Course of the Tides there, of Wells both Salt and Sweet, digg'd near the Sea; of the Whale-fishing there practis'd anew, and of such Whales, as have the Sperma Ceti in them.

This Letter was written June 18, 1667. by that Intelligent Gentleman, Mr. Richard Norwood, living upon the place, and relating as follows:

SIR,

I received your Letter of October 24, 1666, but, whereas you mention another formerly sent, that never came to my hands: Neither had I, before the receipt of yours, the least intelligence of the Institution of the R. Society, founded by the King; but am very glad, that God hath put into the heart of his Majesty, to advance such a Noble designe, and should rejoice, I were able to add my Mite for the furtherance of it. As to the particulars, you recommend to me, I shall answer to them, as I can, in the order, I find them.

First, touching the Conjunction of Mercury with the Sun, which
which you say you gave me notice of in your first, not received, and which happened Octob. 25. 1664. I had also notice of it from Mr. Street, and had provided in some measure to observe it; but the sky was so overcast, that the Sun could scarce be discerned all that day.

Next, concerning the Tides, I have only taken a general notice of them; as, that it is high water about 7. of the clock on the Change-day (in some Creeks an hour or two later.) The water riseth but little, as about 4. foot at a high water; but at the Spring-tides, it may be a foot more. The Tides without are very various in their setting. Sometimes the Tide of Floud sets to the East-ward, sometimes to the West-ward; but in fair, calm and settled weather they said Tide sets from the South-east, toward the North-west, as they say.

We digg Wells of fresh water sometimes within 20 yards of the Sea or less, which rise and fall upon the Floud, and ebb as the Sea doth; and so do most of the Wells in the Country, though further up (as I am inform'd,) Wheresoever they digg Wells here, they digg till they come almost to a Level with the Superficies of the Sea, and then they find either fresh-water or salt. If it be fresh, yet if they digg 2. or 3. foot deeper, or often less, they come to salt-water. If it be a sandy ground, or a sandy crumbling Stone, that the water soaks gently through, they find usually fresh-water; but if they be hard Lyme-stone-rocks, which the water cannot soak through, but passeth in chinks or clefts between them, the water is salt or brackish. Yet (to mention that by the by,) I never saw any sand in the Country such as will grind glasse, or whet knives, &c. as in England, but a substance like sand, though much softer, neither have we any Reble-stones or Flint.

For the killing of Whales, it hath been formerly attempted in vain, but within these 2. or 3. years, in the Spring-time and fair weather, they take sometimes one, or two, or three in a day. They are less, I hear, than those in Greenland, but more quick and lively, so that if they be struck in deep water, they presently make into the deep with such violence, that the Boat is in danger to be haled down after them, if they cut not the rope in time. Therefore they usually strike them in shoal-water. They have
have very good Boats for that purpose, mann'd with fix oars, such as they can row forwards or backwards, as occasion requireth. They row up gently to the Whale, and so he will scarcely follow them; and when the Harpineer, standing ready fitted, sees his opportunity, he strikes his Harping-Iron into the Whale, about or before the Fins rather than toward the Tayl. Now the Harping-Irons are like those, which are usual in England in striking Porpoises, but singular good metal, that will not break, but wind, as they say, about a man's hand. To the Harping-Iron is made fast a strong line the rope, and into the Socket of that Iron is put a Staffe, which, when the Whale is struck, comes out of the Socket; and so when the Whale is something quieter, they haul up to him by the rope, and, it may be, strike into him another Harping-Iron, or lance him with Lances in staves, till they have kill'd him. This I write by relation, for I have not seen any kill'd myself. I hear not, that they have found any Sperma Ceti in any of these Whales; but I have heard from credible persons, that there is a kind of such as have the Sperma at Eleutheria, and others of the Bahama-Islands (where also they find often quantities of Amber-grease) and that those have great teeth (which ours have not) and are very finewy. One of this place (John Perinchief) found one there dead, driven upon an Island, and, though I think ignorant in the business, yet got a great quantity of Sperma Ceti out of it. It seems, they have not much Oyl, as ours, but this Oyl, I hear, is at first like Sperma Ceti; but they clarifie it, I think, by the fire. When I speak with him (whom I could not meet with at present, and now the Ship is ready to set sail) I shall endeavour to be further informed; but at present with the tender of my humble service to the Royal Society, and commending your Noble Designe to the blessing of the Almighty, I take my leave, &c.
A Method

For finding the Number of the Julian Period for any year assigned, the Number of the Cycle of the Sun, the Cycle of the Moon, and of the Indictions, for the same year, being given: together with the Demonstration of that Method.

In these Transactions, N. 18. p. 324, is a Theorem for finding the Year of the Julian Period, by a new and very easy Method, which was taken out of the Journal des Scavans N. 36, as it had been proposed and communicated by the Learned Jesuite De Bill.

Multiply the \( \frac{\text{Solar Cycle}}{\text{Lunar Cycle}} \) by \( \frac{4845}{4200} \). Then divide the sum of the products by 7980 (the Julian Period) the Remainder of the Division, without having regard to the Quotient, shall be the Year inquired after.

Some Learned Mathematicians of Paris, to whom the said P. de Billy did propose this Probleme, have found the Demonstration thereof, as the same Journal intimates.

There being no further Elucidation of the said Theorem since published, Mr. John Collins, now a Member of the R. Society, communicated what follows, viz.

That the Julian Period is a Basis, whereon to found Chronology not liable to Controversies, as the Age of the World is: And 'tis the Number above said, to wit 7980, which is the Product of 28 the Solar Cycle.

19 the Lunar.

15 the Indiction.

Concerning this Julian Period, the late Arch-bishop of Armagh, Usher, in the Preface to his learned Annals, advertiseth, that Robert Lotharing, Bishop of Hereford, first observed the Conveniences thereof: 500 years after whom it was fitted for Chronological uses by Joseph Scaliger, and is now embraced by the Learned, as such a limit to Chronology, that within the space of 7980 years, the Number of the Sun's Cycle, the Prime, and the Year of the Roman Indiction (which relates to their ancient Laws and:
and Records) can never happen alike. And these remarques being given, the year of the Julian Period is by the former Rule infallibly found.

This Period is used by the said Arch-bishop in his Annals, and is by him accounted to exceed the Age of the World 709 years. Those, that desire further satisfaction about Aris's, Epocha's, and Periods, may repaire to many Authors, and among them to Gregories Posthuna; in English, Helvici Chronologia, Aegidii Stranchii Breviarium Chronologicum, who is one of the latest Authors.

Now as to the Probleme it self, it may be thus proposed.

Any Number of Divisors, together with their Remainders after Division, being proposed, to find the Dividend.

This thus generally proposed is no new Probleme, and was resolved long since, by John Geysius, by the help of particular Multipliers, such as those above-mentioned, and publickly by Alstedius in his Encyclopaedia in An. 1639, and by Van-Schooten in his Miscellanies.

We shall clear up, what Authors have omitted concerning the Definition and Demonstration of such fixed Multipliers, &c. And therefore say, that each Multiplier is relative to the Divisor, to which it belongs, and thus define it;

It is such a Number, as Divided by the rest of the Divisors, or their Product, the Remainder is 0, but Divided by its own Divisor, the Remainder is an Unit.

We require the Divisors proposed to be Primitive each to other, i.e. that no two or more of them can be reduced to lesser terms by any common Divisor. For, if so, the Question may be possible in it self, but not resolvable by help of such Multipliers, such being impossible to be found. The reason is, because the Product of an Odd and an Even Number is always Even, and that divided by an Even Number, leaves either Nothing, or an Even Number.

\[
\begin{array}{c|c|c}
\text{Divisors} & 19 & 5 \\
\text{there to are} & \text{15} & \text{56916} \\
\text{The Multipliers relative} & 4845 & 4200 \\
\end{array}
\]

The Definition affords light enough for the discovery of these Numbers. To instance in the first: The Product of 19 and 15 is
is 285, which multiply by all numbers successively, and divide by 28, till you find the Remainder required. Thus twice 285 is 570, which divided by 28, the remainder is 10: Also, thrice 285 is 855, which divided by 28, the remainder is 15. Thus if you try on successively, you'll find, that 17 times 285, which is 4845, is the Number required, the which divided by 28, the Remainder is an Unit. Hence then we shall find, that 4845

\[
\begin{array}{c}
4200 \\
6916
\end{array}
\]

is equal to the Solid or Product of 12 11 125.

\[
\begin{array}{c}
\frac{19}{15}, 17, 19, 13
\end{array}
\]

More easie wayes of performing this postulatum, are to be found in Van.Schooten’s Miscellaries, and Jacquet’s Arithmetick, which perchance are not so obvious to every understanding.

For Illustration of the Rule proposed, take this Example.

<table>
<thead>
<tr>
<th>In the year 1668</th>
<th>Cyclus Solis 25?</th>
<th>The Multipliers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Indictio 6</td>
<td>4845</td>
<td>121125</td>
</tr>
<tr>
<td></td>
<td>4200</td>
<td>67200</td>
</tr>
<tr>
<td></td>
<td>6916</td>
<td>41496</td>
</tr>
</tbody>
</table>

The Sum of the Products 229821, the which divided by 7980, the remainder is 6381, for the Year of the Julian Period; from which subtracting 709, there remains 5672, for the Age of the World, according to Arch-Bishop Usher.

For DEMONSTRATION of this Rule we thus argue:

1. Each Multiplier Multiplied by its Remainder, is measured or divided by its own Divisor, leaving such a Remainder, as is proposed.

For before, each Multiplier was defined to be a Multiplex of its own Divisor, plus an Unit. Wherefore Multiplying it by any Remainder, it doth onely render it a greater Multiplex in the said Divisor, plus an Unit, Multiplied by the Remainder, which is no other, than the Remainder itself; but if 0 remaine, that Product is destroyed.

2. The Sum of the Products, divided by each respective Divisor, gives the Remainder assigned.

For concerning the first Product, it is by the first Section measured
fund by its own divisor, leaving the remainder proposed, and if we add the rest of the products thereto, we only add a multiplex of its own divisor, which in division enlargeth the quote, but not the remainder.

Particularly the second multiplier is $28 \times 15 \times 10 \times$ Remainder, all which is but a multiplex of 28.

And so, the third product is $28 \times 19 \times 13 \times$ Remainder.

And what hath been said concerning the sum of the products, being divided by the first divisor, and leaving the remainder theretofore assign'd, may be said of each respectively.

3. The sum of the products divided by the solid of the three divisors, leaves a remainder so qualified as the said sum.

For concerning the said sum, 'tis evident by the second hereof, that it is no other, than the first product increased by adding a just multiplex of the first divisor, that thereby we did only enlarge the quote, not alter the remainder. By the like reason, the substracting a just multiplex thereof, doth only alter the quote, not the remainder, but the solid of all three divisors, multiplied hereby the quote, as there by the remainder, is no other than a just multiplex of the first divisor. Wherefore the remainder, after this division is perform'd, is of the same quality, as the sum of the products, and divided by the first divisor, leaves the remainder proper thereto: And the like may be said concerning each divisor.

As in the method hitherto deliver'd, we required the divisors be primitive to each other; so, if we take the probleme as generally proposed, in the preface to Helvicius his chronologia, we are told, common arithmetick failes in the solution thereof, and tacquet denies it to be performable by the regular falsa, and being unlimited, we must do it by tryals.

Wherefore,

When any two divisors with their remainders are proposed, try the multiplices of one of them, increased by its remainder, and divide by the other. If you find such remainders, as are not for the purpose, and that they are repeated, the probleme is impossible.

Example, Divisors Remainders

<table>
<thead>
<tr>
<th>Divisors</th>
<th>Remainders</th>
</tr>
</thead>
<tbody>
<tr>
<td>8</td>
<td>3</td>
</tr>
</tbody>
</table>
The Multiples of 8, increased by 5, are 
Those divided by 6, the Remainders are 

Here you see 24, and 45, for the purpose, and take the Progression, adding the common difference 24 (which is the least Dividend measured by 6, and 8) and you have 21, 45, 69, 93, 117, 141.

Admit, the Question had concerned these three Divisors.

Then dividing the former Progression by 9, the Remainders are 3, 6, 9, 6, 3, 6.

Wherefore I conclude, that the third and sixth of these Numbers are those sought, to wit 69, or 141, and so on progressively: Whereas, if you had propounded the Remainder of 9, to have been any other Number, than 3, 6, 9, the Probleme, as concerning all these, had not been possible.

Some cases of the Probleme are these:

When the Remainder of some Divisor is 0, and of each of the rest of the Divisors, an Unit, or lesse by an Unit, then the Divisor.

In which Cases you are to find such a Multiplex of the Product or least Dividend measurable by those Divisors, that have Remainders, which, increased or diminished by an Unit, may be a just Multiplex of that Divisor, that hath no Remainder. These Cases are handled by Tacquet, and Bachel in his Problemes plaisans & délectables.

PROBLEME.

To find the Year of the Julian Period for any Year of our Lord proposed.

It is necessary to be furnish'd with the Sun's Cycle, the Prime Number, and the Number of the Roman Indiction, which the industrious Mr. Street thus performs:

When
When 1, 9, 3, to the Year hath added been,  
Divide by 19, 28, fifteen.

The Remainders are the Numbers sought. And hereby we found them for the year 1668, in the former Example.

The use of the Prime is, to find the Epact, and thereby the Moons Age, Time of High Water, &c.

A farther use of the Sun's Cycle is, to attain the Dominical Letter, and thereby to know the Day of the Week, on which any Day of any Month happens. But this is more easily and with less caution obtained, by finding, on what Day of the Week the first of March happens for ever, according to such Rules and Verses, as I have elsewhere published.

In brief thus:

To the Number ———— 2.
Add the Year of our Lord, suppose 1669:
An its Even fourth part, neglecting what remains, if any ———— 3 417.

The Sum ———— 2088, Divide by 7, noting the Remainder, which shewes the Number of the Day of the Week, accounting Sunday first. If 0, remain, the first of March falls on a Saturday. In this Example there remains 2, shewing the first of March to fall on Monday.

If it were required, to performe this for years preceding our Saviour's Nativity, then take this Rule:

To the Year adde its even fourth part, the Sum divide by 7, the Remainder shewes the Day of the Week, accounting Sunday first, Saturday second, and so backward.

P R O B L E M E.
To find what day of the Month in the first Week of each Month, happens to be on the same day of the Week, as the first of March.

Use the (plain) following Verses, in which the 12 Words relate to the 12 Months of the Year, accounting March the first;

Ask endless Comfort, God enough bestows,
From Divine Axioms Faith confirmed grows.
The Alphabeticall Number of the first Letter of the word proper to the Month proposed, is the Answer;

**Example.**

If the Month were *April*, the word proper thereto is, *Endless*, and *E* is the fifth Letter in the Alphabet. Wherefore conclude, that the first of *March*, and fifth of *April* do for ever happen on the same day of the week; which for the year 1669, will be on Monday.

**Problem.**

To find, on what day of the week the first day of each Month happeneth.

Supposing the first of *March* known, it might be reckoned from the former Probleme, but the following Verse, beginning with *March*, as the former, is more ready for the purpose:

*A Dreadful Fire, Beholders daily Gaze,*
*Chastized England. Ah cruel fatal Blaze!*

**Explication.**

In the Year 1669, the first of *March* is Monday: I would know, on what day of the week the first of *October* happens. The word proper to the Month is *England*; then count Alphabetically to *E*, *viz.* A. Monday, B. Tuesday, C. Wednesday, D. Thursday, E. Friday, which is the day fought. Whence conclude, that the 1st, 8th, 15th, 22th, 29th days of *October* are all Fridays. Thence it is easie to reckon, on what day of the VVweek, any day of that Month happened, and so for all other Months.

**Problem.**

To find, on what Day of the Month the Sun enters into any Sign of the Zodiac.

For this, *ex super abundanti*, we give the following Verse:

*Charles brought Content, divers effects ensue,*
*Envy, Fear, Dolour, Danger, bids adieu.*

Here again the 12 VWords relate to the 12 Months, *March* being the first.
To the Number of the Letter of the Alphabet, the word begins with, add 7.

Example, Feare is the word for October, and F, the sixth Letter: Wherefore the Sun enters into the 8. Signe, to wit, Scorpio, on the 13. of October.

An Account of some Books.

I. PETRI LAMBECHII LIB. PRIMUS PRODROMI HISTORIÆ LITERARIIÆ, &c.

The Author of this Book is now the Historiographer and Librarian to the Emperour. He publisheth this Volume some few years agoe at Hamburg, the place of his Birth, (whence an Exemplar was but lately sent to the Publisher.) He was excited to this Work by the complaint made by the Illustrious Lord Verulam, (Lib. 2. cap. 4. de Angnis Scientiarum) of the want of a compleat History of Learning, that might give a satisfactory Account of the Rise, Progress, Trans-migrations, Interruptions, Declinations, and Restorations of all kind of Learning, Sciences, Arts, and Inventions; together with the Occasion of inventions through all Arts; the Method of teaching, and the Manner of improving and advancing them: Adding the various Sects, and the most famous Controversies among the Learned; the Encouragements, they received; the chief Writings, they composed; their Schooles, Academies, Societies, Colledges, Successions, Orders; and whatever belongs to the State of Learning.

This Grand Desideratum our Author undertakes to supply the World with; and in order thereunto hath given us the First Book of the Prodromus of this History, and with it the Four first Chapters of the Second Book, together with an Appendix, containing a Summary of the Chief Persons and Things, he intends more fully, and accurately to treat of in the remaining 32. Chapters, designed for the same Second Book: To which he subjoyns two Tables of Universal Chronography, in the first whereof he exhibits the succession of all Ages from the Creation of the World to the Beginning of the Common Christian Account; in the other, a Continuation of them from the Beginning of the said Account unto this present Age; In which Tables he gives a general Idea of the Connexion of all Ages, as they are computed in respect of the Vulgar
Vulgar Christian Account, either by Ascending to the Creation of the World, or by Descending to our Age: He also for the sake of this Work acquaints the Reader that he betook himself to the Explication and Castigation of the Bibliotheca Chronologica Classicorum Authorum JOHANNIS JACOBI ERISI Tiguri, substituting, as he affirms, a true Calculation in the place of a false one; reducing the Authors, there enumerated, to the true time of their Age, distinguishing what is supposititious from genuine, and adding many things, that were unhappily omitted. Which done, he saith, he proceeded from this Account of the Succession of Illustrious Writers, to the History of the Origin, Increase, Nature and Constitution of all Professions, Sciences and Arts, chusing the Eight Books of POLYDORÉ VIRGIL de rerum Inventoribus, and DIÓGENES LAERTIVS, De Vitis, & Dogmatibus veteris Graecie Philosophorum; as also, the Eight Books of JOHANNES MIDDEN-DORP IVS De Celebribus Universi Orbis ACAD-EMIIS.

He excuseth himself for having made no further progress in this Desirable Work, alledging the difficulty and trouble of the Undertaking, the unavoidable interruptions he hath met with, and the narrowness of a private Mans fortune to carry on so Chargeable an Attempt, requiring a Royall encouragement and Assistance.

II. THOMÆ CORNELII Consentini PROGYMNASMA-

TA PHYSICA.

This Author, a Friend to the Cartesian Philosophy, entertains the Curious in this Book with seven Exercitations, viz.

1. De Ratione Philosophandi: Where in the genuine Students of Naturall Philosophy he first requires the study of Mathematicks, to accustome their Minds to a fixed Attention, and to strict Reasoning; and next directs them to study Nature itself, and to labour after a true History of Nature: recommending lastly and particularly the Use of Chymistry, as an excellent key to open her Treasures, and the study of Mechanicall Principles, as nearly al-

lyed to those of Nature.

2. De
2. De Rerum Naturalium Initius: where he mentions the several Hypotheses and Principles of Philosophers, and approves of the Cartesian, esteeming, that none ever looked so like truth, as those, though he thinks them defective in this, that, how well soever they shew the production of things out of Matter variously modified, yet they seem not to have sufficiently accounted for the efficient power thereof.

3. De Universitate: where he seems to be in a Maze, and thinks, That the Structure of the Universe hath not been understood hitherto, nor will easily be hereafter.

4. De Sole: which Luminary he is inclin’d to believe to be a kind of Flaming Fire, appearing in a Telescope, like a Caldron full of boyling Mettal: where also he discourses of the nature of Light, Heat and Flame; and affirms Light (as other sensible Qualities) to be not in the Object, but the Sentient, as Pain is not in the Sword, but in the Animal wounded by the Sword.

5. De Generatione Hominis: where, distinguishing between Geniture and Semen, and making the former to be that substance, which either Sex furnishes to the Foetus, and the latter, the Concrete of both Parents, He is of opinion, that that which he calls Geniture, consists of two things. Vid. a Crasse liquor, manifest to sense; and of a very subtile and refined substance, containing all the virtue of Generation, and lodged in the former as its receptacle. Which having establisht, he affirms, that groffer part of the Geniture not to be Bloud elaborated, but a Juice, seperated from the Bloud, and being strained through the Corpus vari·cosum or plexus pampiniformis (wherein the feminal arteries are by innumerable anastomoses so combined and interwoven with veins, that very hardly any naked eye can discern a Vein from an Artery) it paffeth into peculiar fit vessels, and is of a colour like that of the White of an Egg. As to the Formation of the Foetus, he esteems That, before the appearance of any Bloud, or the framing of any member, there are form’d all the lineaments of the Animal to come, though indiscernibly; which he endeavours to make out very particularly, interweaving some Animadversions on Authors of differing sentiments, and mentioning several not un-philosophical Hints.

6. De Nutricatione: Here the Author observes some things in the
the structure of the stomach, which he thinks highly considerable for the understanding of the action and use of this viscus, and hitherto not taken notice of by others, that he knows. Then he teacheth, that the food is not digested in the stomach by heat, nor by acid dissolving juice, only, but that, many causes concurring to that digestion, the aliment is there fermented both by the warmth of the stomach itself, and of the neighbouring parts, but especially by the acrimonious steam, that passes through the gasterick and spleenick arteries into the stomach, which advances also its concoction by its compressing and relaxing motions, and is assisted by an apt liquor, bedewing, dissolving, and diluting the meat, and so converting it into a pulse or cremelike substance. Next, he teacheth, that the chyle passeth not through the milky veins (so called by asellius) to the liver, nor all of it through the channel of pecquet to the heart, but a great part of it, through the common veins of the stomach and the mesentery, to the liver. Nor will he admit, that the sanguification is performed in any one part of the animal, as the peculiar shop or elaboratory of it, whether liver, heart, spleen, &c. Nor that the parts are increased and nourished by the red part of the blood: but that, as to the former action, it is done by the means of a liquor, and by hot streams, giving the red colour to the chyle, as chymists use to change white juices into red, by the effusion of oyl of sulph, or the like liquors; that redness being much advanced by the motion and agitation of the blood in the veins and arteries. But as to the latter, we say, the nutrition, it is performed by that whitish juice, which is mixed with the blood, and separated from it by the straining glanduls of the body.

To these particulars he adds several not unconsiderable remarks touching the gall, spleen, lymphatick vessels, &c. Obseving also, that the whole kind of birds is destitute of milky vessels; and occasionally taking notice, that worms are bred in almost all the parts of animal-bodies, of which he alledges very odd observations and histories.

7. De vita: This he affirms to consist in the continued motion of the blood, depending from that of the heart; yet so that this latter proceeds not from the heat of the blood (as Des-Cartes
Cartes would have it) but the moist steams and exspirations of the Heart.

As for Respiration, he thinks it a vain opinion, that thereby the Heat of the Bloud is tempered and allay'd; but affirms, that it is therefore necessary, because that the Bloud, which out of the right Ventricle of the Heart is propelled into the Lungs, in such Animals, as are furnish'd with them, cannot pass into the left, unless the Air, breathed in, do swell and distend the small branches of the Wind-pipe; it being from thence, that the ramifications of the Arterial Vein, through which the bloud must pass, are compress'd, and the bloud, therein inclosed, is protruded into the branches of the venal Artery; For the proof of which, he alleges divers Observations: Adding, with all, that, since Animals, whilst they are in the Womb, respire not; there being peculiar ductus's, by which the bloud passeth into the Aorta, without passing through the Lungs, as it always doth in Animals destinat of Lungs; he doubts not, but that with art and care those channels may be preserved un-abolish'd, and made to grow and to be perfected with the other parts of the Animal; so that grown men may be brought to live the life of Amphibios Creatures. Nor doth he think this very difficult, in regard, that if their mouths and noses were from their very infancy often stop'd every day, and their breath so long intercepted, whilst the bloud passeth through those ductus's into the left ventricle of the Heart and the great Artery, the said passages would never be dry'd up: To confirm the possibility whereof, he alleges Examples of Divers, who from their childhood being given to swimming and diving, and so to the holding of their breath, did thereby preserve those channels from being dry'd up, that upon occasion they could stay a great while under water, as Amphibions use to do.

LES ESSAYS PHYSIQUES du Sieur DE LAV.-

NAY, Liv. premier.

This Learned Man having proposed to himself to go through the whole Body of Natural Philosophy, by the way of Essays, divides that Systeme into three Parts, whereof

The First being General, is to treat of what is common to all Bodies.
Bodies, both Superior and Inferior: and is divided again into six Books; whereof the first considers the Universe in general: the second is to discourse of Place, Vacuum, and Time, things as general as the World: the third, Of the material Principles of all Bodies: the fourth, of their Efficient Cause: the fifth, of their natural Qualities: and the sixth, of Motion, Generation, and Corruption of Bodies Inanimat and Animat.

The second part, is to examine the Celestial Bodies. The Third, shall treat of the Terrestrial, viz. the Elements, Meteors, Minerals, Plants, Brutes, Men.

Of this Work is now printed the first Book of the first Part, consisting of 5. Dissertations.

The first is about the Preliminary Questions of Physiology. The second inquiereth whether the Universe is compounded of many Worlds. The third is of the Systyme of the World, its Magnitude and Figure. The fourth examines, Whether the World be animated? The fifth, Whether it hath been or could be from Eternity? The sixth, is concerning the End of the World.

IV. FRANCISCI DV LAVRENS SPECIMINA MATHEMATICA, duobus Libris comprehensa.

Horum Prior, SYNTHETICUS, agit de Genuinis Matheseos Principiis in genere; in specie autem de Veris Geometriæ Elementis hucusque nondum traditis.

Posterior, ANALYTICUS, de Methodo Compositionis atque Resolutionis fusæ differit, & multa nova complectitur, quæ subtilissimam Analyseos Artem mirum in modum promovet.

ERRATA, forgot to be corrected sooner.

In No 28. Pag. 521. lin. 22,23. r. she took dog (even before the wound was heal'd up) was with puppy. p. 525. l. 8. r. Answers that shall. ibid. l. 20. r. Mineral Queries. p. 532. l. 18. dele, viæ. p. 535. l. 2. r. impelled at the Nose, ibid. l. 15. r. Grand poison.

In No 29. p. 541. l. 18. r. An intimation. p. 544. 1. 5. r. from the Indexes. ibid. l. 22. dele, and as. p. 545. l. 21. r. breath out. p. 548. l. 18. r. with wind or,

In the SAVOT,

Printed by T. N. for John Martyn, Printer to the Royal Society, and are to be sold at the Bell a little without Temple-Bar, 1667.
PHILOSOPHICAL
TRANSACTIONS.

Monday, January 6, 1668.

The Contents.

NEW Experiments, to the number of 16, concerning the Relation between Light and Air (in Shining Wood and Fish;) shewing, That the withdrawing of the Air from those and the like Bodies, extinguish'd their Light, and the Re-admission of Air restores it. An Account of two Books: I. PATHOLOGIAE CEREBRI & NERVOSI GENERIS SPECIMEN: in quo agitur de MORBIS CONVULSIVIS & SCORBUTO, studio THOMÆ WILLIS M. D. II. ALPHABETVM NATVRÆ. Author F. M. B. V. HELMONT.

New Experiments

Concerning the Relation between Light and Air (in Shining Wood and Fish;) made by the Honourable ROBERT BOYLE, and by Him addressed from Oxford to the Publisher, and so communicated to the ROYAL SOCIETY.

SIR,

To perform now the promise I made you the other day, I must acquaint you with what will perhaps somewhat surprise you, by giving you an Account of what I tried on Tuesday night last (Octob. 29, 1667,) and the two or three following nights, about the Relation between Air and Light, as this is to be found in some Bodies.

The Occasion of these Trials was this. Having, as you know, long since made some Notes, chiefly Historical, upon particular Qualities, and finding Light to be (how justly, I now dispute not) reckon'd by the generality of Philosophers among Qualities, I hudled together what Observations I had either made myself, or received from some Ingenious Travellers (to whom I recommended my Inquiries) about Shining Bodies; And had also prepared several Trials about them, to be made when I should have opportunity and requisite Instruments to put them in practice,
practise, which, as to some of those designed Experiments, have been long denied me. But having at length got higher one of my little Engines, and having also procured, after much enquiry, a few small pieces of Shining Wood, I began on the day aforesaid to try with them an Experiment, I found in my List. And though the main Experiment be but one, I intended to set down what occurred to me about it but as several Phenomena of it; yet finding it requisite, to acquaint you with some Trials, that are not so properly Parts of it, I shall for distinction sake, propose them as several Experiments; the Narratives whereof are taken, for the most part, verbatim out of the Notes I set down for my own use, when the things to be registred were freshly done. Which advertisement I give you, both to excuse the carelessness of the Style, and to induce you not to distrust a Narrative, that was made only to serve my Memory, not as Hypothesis.

Experiment I.

To try, Whether or no a piece of Shining Wood, being put into a Receiver of our Pneumatick Engin, would, upon the withdrawing and re-admitting of the Air, suffer such changes, as I have often observed a Live Coal, placed there, to doe; having at length procured a piece of such Wood, about the bigness of a Groat or less, that gave a vivid light (for rotten Wood) we put it into a middle-sized Receiver, so as it was kept from touching the Cement; and the Pump being set at work, we observed not, during the five or six first Exsuctions of the Air, that the splendour of the included Wood was manifestly lessened (though it never was at all increased;) but about the seventh suck, it seemed to grow a little more dim, and afterwards answered our expectation, by losing of its Light more and more, as the Air was still further pumped out; till at length about the tenth Exsuction (though by the removal of the Candles out of the room, and by black Cloaths and Hats we made the place as dark as we could, yet) we could not perceive any Light at all to proceed from the Wood.

Experiment II.

Wherefore we let in the outward Air by degrees, and had the pleasure to see the seemingly extinguish'd Light revive, so fast and perfectly, that it looked to us all, almost like a little flash of Lightning, and the splendour of the Wood seemed rather
rather greater, than at all less, than before it was put into the Receiver. But partly for greater certainty, and partly to enjoy so delightful a spectacle, we repeated the Experiment with the like success as at first. Wherefore being desirous to see how soon these changes might be produced, we included the Wood in a very small Receiver of clear glass, and found, That in this the Light would begin to grow faint at the second, or at least at the third Exhalation of the Air, and at the sixth or seventh would quite disappear. And we found by a Minute-watch, that the sending the Candles out of the room, the pumping out the Air till the Wood would shine no more, the re-admitting of the Air (upon which it would in a trice recover its Light) and the sending in for the Candles to consult the Watch, did in all take up but six Minutes.

**Experiment III.**

The forementioned Experiment, without taking notice how long it lasted, being reiterated twice in this new Receiver, we had a desire to see, whether this Luminousness of our Wood would more resemble a Coal, or the Life of a perfect Animal, in being totally and finally extinguished, in case the Air were kept from it a few minutes, or else the Life of Insects, which in our exhausted Receiver I had observed to lose all appearance of its continuing, and that for a much longer time than a few minutes, and yet afterwards, upon the restitution of Air, to recover presently, and shew manifest signs of Life: Wherefore having exhausted the Receiver, till the Wood quite dis-appeared, we stayed somewhat above a quarter of an hour in the dark, without perceiving, that the Wood had regained any thing of Light, though about the end of this time we made the place about it as dark as we could; and then it being too late at night to protract the Experiment, we let in the Air, upon whose admission the Wood presently recovered Light enough to be conspicuous at a distance; though it seemed to me somewhat less vivid than before: which yet may be either a weakness in my sight, or an effect of the steam of the Cement, unfriendly perhaps to the Luminousness of the Wood.

Thus far we proceeded yester-night, to which we this night added these Observations.

We
We put in a piece of Wood bigger than the former (this being above an inch long) and that hone very vigorously. And having by a few sucks quite deprived it of Light, we left it in the exhausted Receiver for full half an hour; and then coming into the dark room again, we found all had not continued to stand, but that some small portion of Air had insinuated itself into the Receiver. This we concluded to be but a small portion of Air, because the Wood was but visible to an attentive Eye. And yet, that it was really some Air, which was got in, that caused the little glimmering light, which we perceived, may appear by this, that it did presently (as we expected) vanish at the first or second suck; and then the Air being let into the dark Receiver, the included Wood presently shone again as before; though I suspected, I discerned some little diminution of its brightness; which yet, till further Trials of the like kind, and for a longer time, have been made, I dare not affirm. Before the Receiver was sufficiently emptied at the beginning of the Experiment made with this greater piece of Wood, a small leak accidentally sprung, which, letting in a little Air, did sooner than we intended, recall the almost dis-appearing Light.

Experiment IV.

Here is an Experiment of affinity with the former, which we thought it not altogether impertinent to try. For having observed on another occasion, That sometimes the Operation, which the withdrawing the Air hath upon a Body included in the Receiver, proves more considerable some minutes after we have ceased pumping, than immediately after the exercise is left off, I imagined, that even in such cases, where the Light is not made wholly to disappear (though it be made almost quite to do so) by the emptying of the Pneumatical glass, the suffering the Body to remain a while there, though without any pumping (unless now and then a very little to remove the Air, that might have stolen in in the mean time) the remaining Light of the Body might probably be further impaired, if not reduced quite to vanish. To examine this conjecture we put in a Body, that was not Wood, which had some parts much more luminous than the rest.
rest; and having drawn out the Air, all the others dis-appeared, and even the formerly brighter ones shone but faintly, when the Pneumatical glass seemed to be exhausted. But keeping the included Body a while in that unfriendly place, we perceived the parts, that had retained light, to grow more and more dim, some of them dis appearing, and that, which was formerly the most conspicuous, being now but just visible to an attentive Eye, and that scarce without dispute. For, if we had not known beforehand, that a shining matter had been included in the Receiver, perhaps we should not have found it out. And he that had the youngest Eyes in the company could not at all discern it: (the Air being let in, the Body began to shine again.) But this being a single Trial, which the lateness of the night hindred us from reiterating, is to be further prosecuted, and in differing substances, before much be built upon it.

Experiment V.

The Rarefaction or Expansion of the Air having so notable an operation upon our shining Wood, I thought it would not be amiss to try, what the Compression of the Air would do to it. For which purpose we included a piece of it in such a little Instrument to compress, which you may remember to have been devised and proposed by M. Hook. But though we impell'd the Air forcibly enough into the Glafs, yet, by reason of the thickness requisite in such Glasses, and the opacity thence arising, we were not able then, to determine whether or no any change was made in the luminousness of the Wood.

Which I thought the less strange, because by some Experiments purposely devised (at one of which I remember you were present) I had long since observed, That even a great pressure from a fluid Body, which presseth more uniformly against all the Parts, it toucheth of the consistant Body, does work a far less manifest change even on soft or tender substances, than one would expect from the force wherewith it compresseth.

And were it not, that one contrary oftentimes minds us of another, I might have forgot, that I had divers thoughts about finding some good ways of Trying, whether any such change of Texture might be discovered to be made in the shining Wood by the
the absence and return of the Ambient Air, as might with any probability have the loss or recovery of the Woods splendour attributed to it. For I had formerly (if I were not mistaken) found by several circumstances, which I shall not now stay to name, That a slight (so it be an appropriated) variation of the Texture of this Wood, and which may seem mainly to respect the Pores (which perhaps ought to be of a determinate shape and size, and filled with a determinate matter) will have a great operation upon its splendour. And I formerly found by other trials, that even consistient Bodies, if soft ones, may have their Pores enlarged and vitiated, and their bulk, and consequently their texture (at least as to their pores) manifestly enough altered by having the Air withdrawn from about them (whereby the Aerial particles within them were enabled to expand themselves) and let in again, whereby, as to sense, they seemed pretty well restored to their former state. But the success of my endeavours either with Microscopes (through which a vivid piece of Wood will shine by its own light) or otherwise, was not considerable enough to deserve a particular account; especially in this Paper, where I am not to venture at matter of Theory.

Experiment VI.

Thinking fit to try, Whether a small quantity of Air, without being ventilated or renewed, might not suffice to maintain this Cold fire, though it will not that of a Live Coal, or a piece of March, we caused a piece of shining Wood to be Hermetically sealed up in a pipe of clear and thin glass: but though, carrying it into the dark, we found it had quite lost its light, yet imagining that that might proceed from its having been over-heated (being sealed up in a Pipe not long enough to afford it a due distance from the flame of the Lamp we employed to seal it,) we caused two or three pieces of fresh Wood, amounting all of them to the length of about two inches, to be sealed up in a slender pipe between four or five inches in length; which being warily done, the Wood retained its light very well, when the Operation was over: And afterwards laying it by my bed-side, when the Candles were carried away out of the room, I considered
dered it a while before I went to sleep, and found it to shine vividly.

The next morning when I awaked, though the Sun was risen, yet forbearing to draw open the Curtains of my bed, till I had looked upon the sealed Glass, which I had fenced with a piece of Cloth, held between it and the window, my Eyes having not yet been exposed to the day-light since the darkness they had been accustomed to, during the night, made me think the Wood shined brighter than ever. And this night after ten of the Clock, looking on it in a dark place, it appeared luminous all its length, though not so much so as in the morning.

The next morning, and the night after that, the same Wood did likewise manifestly, though not vigorously shine, especially one piece, whose light was much more vivid than the rest. And, for ought I know, I might have observed them to shine longer, if one of the sealed ends of the Glass had not been accidentally broken.

Experiment VII.

While the former Trials were making, I was wishing for a good Bolonian Stone, to try what effect the withdrawing of the Air would have upon it. For though I knew it might be objected, that the Experiments of Light performable in our Engine must be made in the night, whereas the Bolonian Stone gains its light by being exposed to the Sun-beams, yet that objection did not hinder my wish, since the better sort of Bolonian Stones may be induced with a luminousness by the flame of fire, or of large Candles.

I also wished for such a shining Diamond, as is now in the hands that best deserve such a Rarity, our Royal Founder's. For you may remember, that in the Observations I made of that Stone, and annexed to the Conclusion of the Book of Colours, I shew how it may several ways be brought to shine, so that by one or other of those ways, especially that of external Heat, I thought it very likely, I should be able to make the light continue four or five minutes, which would be long enough to try in a very small Receiver, exhaustible at a Suck or two, whether the withdrawing and restoring the Air would have any visible Operation on it.
I also wish for some of the Glow worms, with which I formerly made other Trials. For though I forgot not, what operation the withdrawing of the Air, by our Engine is wont to have upon living creatures, yet that made me not forbear my wish; not only because of the different effect I have found the Engine to have on Insects in respect of other Animals, but because I am not of the opinion of those modern Writers, who will have the Light of Glow worms depend altogether upon their Life, and end with it. But being not likely by my wishes to procure any new subject to make trials on, I thought fit at least to do what was in my power, and accordingly (to gratifie them, who, I presumed, would, if present, propose such a Trial) caused a piece of Iron to be forged, whose top was of the bigness of a Nutmeg; the rest being a stem, of an inch, or an inch and a half long, for which we provided a little Candlestick of Tobacco-pipe-clay, which would not yield any smoke to fill and darken the Receiver. Then having heated the Iron red-hot, and placed it in this Clay, so that the round part was clearly protuberant, we conveyed it into a Receiver of white Glass, which was so placed as to keep the sides at as good a distance, as we could, from the Iron, lest the excessive heat should (as we much feared it would) break the Glass. Then sending away the Candles, and making the Room dark, we hastily pumped out the Air, but could not perceive the withdrawing of it had any operation on the glowing Iron. And though it continued shining long enough to give us opportunity to pump out and let in the Air three several times, yet we could not observe, that the Air had any manifest operation one way or other. For though upon the withdrawing of the Air the Iron grew dimmer and dimmer, yet that I attributed to the cooling of it: and the rather, because, having (to examine the conjecture) let in two or three times the Air, when the Receiver had been exhausted, there appeared no manifest increase of Light upon the sudden admisson of it.

Experiment VIII.

Having formerly in our Physico-Mechanical Experiments about the Spring of the Air observed, That the Air is thus far a Vehicle of Sound, that a Body but faintly sounding, being placed
placed in our Receiver, gave a yet weaker sight, when the Air was withdrawn from about it, then when the Receiver was full of Air: I presumed, some curious persons would, if they had been present, desire to have a trial made, whether or no a small piece of Shining Wood being so included in the Receiver, as that the Pumping out of the Air should have no injurious operation upon the body of it, its Light would upon the withdrawing of the Air be manifestly diminisht. And this I was the less backward to try, because (not to mention the Relation, which the former Experiments shew there may be in some cases between Light and Air) it did not readily occur to my memory, that by any manifest Experiment (for I know, there are probable Reasons to prove it) it appeared, that a Body more thin than Air will or can transmit Light, as well as other diaphanous medium's. And those modern Atomists, that think, there is in our exhausted Receiver very many times more Vacuum than Body, would, I presumed, be glad to be supplied with an Argument against the Peripatetics, to show, That the Motion of Bodies, viz. the Corpuscles of Light, may be freely made in Vacuo, and proceed without the assistance of a Vehicle.

Wherefore having Hermetically sealed up a small piece of Shining Wood in a slender Pipe, and placed it in a small Receiver, that was likewise made of clear Glass, we exhausted it of Air, and afterwards let in again that, which we had excluded. But by neither of the Operations could we perceive any sensible decrement or increase of the Light of the Wood, though by that very Observation it appeared, that the Glass had been well sealed, since otherwise the included Air would have got out of the Pipe into the Receiver, and have left the Wood without Light.

Experiment IX.

I had also a mind to try, both what degree of Rarifaction of the Air would deprive the Wood of its splendour in such and such measures, and whether or no the self-same Air, which, when rarified, would not suffer the Wood to shine, would, when reduced to its former density, allow it to shine as much as before.

This I proposed to do by putting some Shining Wood into a clear...
clear and conveniently shaped Glass, that the long Stem or Pipe being so far filled with *QuickSilver*, as that there might be about half a spoonful of Air left at the closed End, where the Wood was placed, it might be inverted into a little Glass of *Stagnant QuickSilver*, and therewith conveyed into a slender Receiver, out of which as the *Air* should come to be pumped, *that* included in the Glass, which held the Wood, might be rarified, and afterwards upon the admission of the outward Air (which must impell up the *QuickSilver* to its former height) might be restored to its former state. But when we came to make trial of this, we had no Receiver conveniently shaped, that was so clear and thin, as that we could see the Wood shine through both the Glasses. And though we would for an Expedient have substituted a fine thin Bladder, wherein the Wood was to be put, and a convenient quantity of Air strongly tied up with it, yet for want of a Bladder fine enough for our turn, that Expedient also proved useless to us. But being desirous to make what trial we could by the least unfit means, we had in our power, we got an old, but thin Glass, sealed at one end, whose shape was pretty *Cylindrical*, and whose bore was about the bigness of a man's little finger, and whose length was about a foot or more. Into this Pipe near the sealed end we put a piece of Shining Wood, wedged in with a piece of Cork to keep it from falling; and having inverted the nose of it into another slender Glass, but not *Cylindrical*, wherein was pretty store of *QuickSilver*, we put them both into a long Receiver, shaped almost like a Glass *Churn*, and having pumped a while, that the Air included in the Pipe, expanding it self, might depress the *QuickSilver*, and so make escapes into the Receiver, as long as we thought fit; we then let in the outward Air, that the *Stagnant QuickSilver* might be impelled into the cavity of the Pipe now freed from much of the Air, to the height requisite for our purpose.

This done, we plied the Pump again, and observed, That, as the Air in the Pipe did by its own *Spring* expand it self more and more, and grow thinner and thinner, the *Shining Wood* grew dimmer and dimmer, till at length it ceased to shine, the internal Air being then got a good way lower, than the surface of the external *QuickSilver*: whereupon opening the commerce between the cavity of the Receiver, and the *Atmosphere*, the *QuickSilver*
Silver was driven up again, and consequently, the air above it was restored to its former density; upon which the rotten wood also recovered its light. What the greatest expansion of this air was, we could not certainly determine, because the expansion raised the external quicksilver so high, as to hinder us to see and measure it. But we guessed, that the air reached to about a foot or more from the top of the pipe to the surface of the quicksilver near the bottom of it. But, when that rarified air was impelled into its former dimensions, we measured it, and found, that the upper part of the tube, unpossessed by the quicksilver, was about three inches; and the wood being about an inch long, there remained two inches or somewhat better for the air. But this experiment is to be repeated, when exacter instruments can be procured.

Experiment X.

Thinking it fit to try as well, whether stinking fish, that shines, be of the same nature as to luminousness with rotten wood, that shines too; as, whether the withdrawing of the air will extinguish or eclipse the light of a considerable bulk of luminous matter, as in the experiments hitherto made, we found it would do to a small one: We took a fish, that we had kept, and cauled to be watched, till it was almost all over luminous though much more in the belly and some parts of the head, than elsewhere: And having suspended him in a conveniently shaped receiver, we found him to give so great a light, that we suspected beforehand, that the withdrawing of the air would hardly have its full operation upon a body, whose bulk was considerable, as well as its light very vivid, and which had many luminous parts retired to a pretty distance from the air. Accordingly having exhausted the receiver as much as we were wont, it appeared indeed, especially towards the latter end of the operation, that the absence of the air did considerably lessen, and in some places eclipse the light of those parts, that shone least strongly. But the belly appeared not much less luminous than before: Wherefore supposing, that upon the turning of the stop cock the air coming in much more hastily than it could be drawn out, we should have the best advantage to discern, what interest it had in the luminousness.
nousness of the Fish, we re-admitted it, and upon its rushing in, perceived the Light to be as it were revived, and increased, those parts of the Fish, that were scarce visible before, or shone but dimly, receiving presently their former splendour.

And not to leave un-prosecuted the remaining part of the Experiment, which was to try, Whether it was the Kind of the Luminous Body, or only the Greatness of the bulk, and the Vividness of Light, and, if I may so speak, the Tenacity of the substance it resided in, that made the difference between the Fish and the Wood. We put part of the Fish of another kind, that shone much more faintly, than that, hitherto spoken off, and but in some places; and by the withdrawing the Air we made some of the luminous parts disappear, and the others so dim, as scarce to be discerned; and yet both the one and the other regained their former light upon the return of the Air.

And to pursue the Experiment a little further, we put in such a piece of the first Fish, as though it were bright, was yet but thin and not considerably great, and upon pumping out the Air, we found it, according to our Expectation, quite eclipsed, though it recovered its Light upon the Air's re-entry.

These, Sir, are the Experiments, I have lately made about Shining Bodies in our Engine. More I would have tried, notwithstanding the trouble we found in managing the Engine in the dark, if rotten Wood had not failed us, and if I were not in a place, where Glass-mens Shops are not near so well furnish'd as the Stationers.

I scarce doubt, but these Experiments will occasion among the Virtuosi several Queries and Conjectures, according to the differing Hypotheses and Inquiries, to which men are inclined. And particularly 'tis probable, that some will make use of this Discourse to counterbalance their Opinion, That notwithstanding the Coldness (at least as to sense) of Fires and other Animals, there may be in the Heart and Blood a Vital kind of Fire, which needs Air, as well as those Fires that are sensibly hot: Which may lessen the wonder, that Animals should not be able to live when robbed of Air. And if I had now time, I could possibly furnish you with some other Trials, that seem much to favour the Comparison, though, as to the Opinion it self of a Vital flame I shall not now tell you my thoughts about it. And though not only the Cartesians will perhaps draw an argument from the past Phenomena in favour of their Theory of Light, but divers others will discourse upon them, and propose further Questions and perhaps Inquiries suitable to their several Hypotheses; yet I shall content myself at present to have faithfully delivered the Historical part of these Appearances, without making
making, at least at this time, any Reflections on them. And the rather indeed, because I enjoyed so little health, when I was making the Experiments, that 'twas not fit for me to engage in Speculations, that would much exercise my thoughts, which, I doubt, have been more gratified, than my health hath been by the bare Trials, which are most seasonably made at hours unseasonable for one, that is not well.

POSTSCRIPT.

Sent by the same Noble Author from the same place, December 6. 1667.

My condition in point of health being not much improved since I write to you in October last, when I shall have added, that I have not these five or six weeks been able to procure any Shining Wood (except one single piece, which though large, was so ill conditioned, that it afforded me but one Trial) you will not, I hope, expect that I should add much to the Experiments I formerly sent you about the Relation 'twixt Light and Air. But however, since the subject is New and Noble, and since your curiosity about other matters has been so welcome and useful to the Virtuosi, I shall not decline even on this occasion to comply with it, and the rather, because I half promised you some Additionals a good while since, and because too, that though, what I shall acquaint you with, may seem to be but a Confirmation of two or three of the former Experiments, yet, besides that 'tis of them, which most needed a Confirmation, these Trials will also afford some Circumstances, that will not, I think, be unwelcom.

Experiment XI.

To examine then the Conjecture, mentioned in the last Experiment, That the durableness of the Light in the Shining Fish, in spight of the withdrawing the Air, might proceed in great part from the Vividness of it, and the beauty of the matter it resided in, rather than from the Extent of the Luminous Body in comparison of the small pieces of Shining Wood, I hitherto had made my Trials with; I put in, the above-mentioned piece of Wood, whose luminous superficies might be perhaps ten or twelve times as great, as that, which the Eye saw at once of the
The surface of such fragments of Shining Wood, as I was wont to employ: And though some parts of this large Superficies Shined vividly (for the light was usually enough, for rotten Wood, inferior to that of our Fish) yet this great piece, being put into a convenient Receiver, was, upon the withdrawing of the Air deprived of Light, as the smaller ones had been formerly; the returning Air restoring its Light to the one, as it had done to the other.

Experiment XII.

But this is not the chief thing I intended to acquaint you with, that being the success of some Trials, which we made in prosecution of these two neighbouring Experiments.

In the first of these I told you, I had been able to try but for half an hour or a little more, that a Shining piece of Wood, deprived in our Engine of Light, would yet retain a disposition to be as it were re-kindled upon the fresh access of the Air. Wherefore, though I could have wished to have made a further Trial with the same kind of Bodies, yet being able to procure none, I substituted in their room small pieces of Rotten Fish, that some of them more faintly, and some of them more vividly, in reference to one another, but none as strongly, as some, that I could have employed: and having in a very small and clear Receiver so far drawn off the Air, as to make the included Body disappear, we so ordered the matter, that we kept out the Air for about 24 hours; and then allowing the Air to re-enter in a dark place and late at night, upon its first admittance the Fish regained its Light.

Experiment XIII.

This, compared with some of my former Observations about Putrefaction, put me upon a Trial, which though it miscarried, I shall here make mention of; that in case you, who are better furnish'd with Glasses, think it worth while, you may get reiterated by the Society's Operator. Considering then, how great an interest Putrefaction hath in the Shining of Fishes, and Air in the Phenomena of Putrefaction, I thought, it might be somewhat to the purpose, to take a Fish, that was, according to the com-
mon course I had observed in Animals, not far from the state, as which it would begin to shine: and having cut out a piece of it, I caused the rest to be hung up again in a Cellar, and the expected piece to be put into a small and transparent receiver, that we might observe, if a day or two, or more, after the Fish in the Cellar should begin to shine, that in the exhausted receiver would either also shine, or (because that seemed not likely) would, notwithstanding the check, which the absence of the Air might be presumed to give the putrefaction, be found to shine too, either immediately upon the admission of the Air, or not long after it.

But this Experiment, as I lately intimated, was only designed and attempted, not completed; the receiver being so thin, that upon the exhaustion of the internal Air, the weight of the external broke it; and we could ill spare another of that kind from trials, we were more concerned to make: notwithstanding which we made one trial more, which succeeded no better than the former, but miscarried upon a quite differing account, viz., because neither the included piece of fish, nor the remaining, though it were of the same sort with the fishes I usually employed, would shine at all, though kept a pretty while beyond the usual time, at which such fishes were wont to grow luminous.

If this Experiment had succeeded, I had some others to try in prosecution of it, which I shall not now trouble you with the mention of. But that this paragraph may not be useless to you, I'll take this occasion to give you a couple of advertisements, that may relate not only to this Experiment, but also more generally to those, whether precedent or subsequent, where shining fish are employed.

Advertisement I.

In the first place then, I will not undertake, that all the experiments you shall make with rotten fish, shall have just the same success with these I have related. For as I elsewhere observed (in a discourse written purposely on that subject) that the event of divers other experiments is not always certain, so I have had occasion to observe the like about shining of fishes. And besides what I lately took notice of, at the close of the tenth experiment, I remember, that having
once designed to make observations about the Light of rotten Fishes; and having in order thereunto caused a competent number of them to be bought, not one of them all would shine, though they were bought by the same person I was was wont to employ, and hung up in the same place where I use to have them put; and kept not only till they began to putrifie, but beyond the time that others used to continue to shine, although a parcel of the same kind of Fishes, bought the week before, and another of the same kind, bought not many days after, shined according to expectation. What the reason of this disappointment was, I could not determine, only I remember, that at the time it happened, the weather was variable, and not without some days of Frost and Snow. Nor is this the oddest observation, I could relate to you about the uncertain shining of Fishes, if I thought it necessary to add it in this place.

Advertisement II.

Notice must also be taken in making Experiments with Shining Fish, that their luminousness is not wont to continue very many days. Which Advertisement may be therefore useful, because without it we may be apt sometimes to make Trials, that cannot be soon enough brought to an issue; and so we may mistake the loss of Light in the Fish to be a deprivation of it caused by the Experiment, which indeed is but a cessation according to the usual course of Nature.

Experiment XIV.

I know not whether you will think it worth while to be told of a Trial, that we made to save those Criticks a labour, that else might perhaps demand, Why 'twas not made. We put therefore a piece of Shining Fish into a wide-mouthed Glass, about half filled with fair Water, and having placed this Glass in a Receiver, we exhausted the Air for a good while, to observe, Whether, when the pressure of the Air was removed, and yet (by reason of the Water that did before keep the Air from immediately touching the Fish) the Exhaustion of the Receiver did not deprive the Fish of that contact of Air, which it had lost before; Whether, I say, in this case the absence of the Air would have the same influence on the shining Body, as in the former Experiments.
here, as far as the numerous bubbles excited in the Water would give us leave to discern it (for they did, though not unexpectedly, somewhat disturb the Experiment, which inconvenience we might have prevented, if we had thought it worth while) we could not perceive, that either the absence or return of the Air had any great operation upon the Light of the immersed Body: which yet did not keep me from intending to make a somewhat like Trial with Shining Wood (when I can get any) fastened to the lower part of a clear Glass, and covered over, but not very deep, with Quicksilver. Of which practice I shall not now stay to give you the Reasons, having elsewhere fully enough expressed them.

And that this Section may acquaint you with something besides the (seemingly) insignificant Experiment related in it, I shall here inform you (since I perceive, I did not in the first Papers I sent you) that though, when I formerly put together some Notes about Luminous Bodies, I confined not my Observations to one or two sorts of Fishes, yet the Experiments, sent you since October last, were all of them (except a Collateral one or two) made with Whittings, which among the Fishes, I have had occasion to take notice of, is (except one sort, that I cannot procure) the fittest for such Trials, and consequently fit to be named to you, to facilitate their future ones, in case you think it requisite to make any upon such subjects.

Experiment XV.

The other of the two neighbouring Experiments, I lately mentioned (viz. the ninth) I told you, when I sent it you, needed a reiteration to confirm it, since we had but once tried it (and that without all the conveniency we desired) that a Shining Body, which upon the first withdrawing the Air looeth much, but not all its Light, may be deprived of the rest by continuing in that unfriendly place, though the Air be no further exhausted. To prosecute therefore both the Experiments in one Trial, we took somewhat late at night a piece of rotten Fish, which we judged to shine too strongly, to be quickly deprived of all its Light, and having put it into a small and clear Receiver, we found (as we had foreseen) that the Light was much impaired, but nothing near

Rrr suppressed
pressed by the withdrawing of the Air. Wherefore having removed the Receiver into a convenient place, I caused it to be brought to me about midnight (after I was a bed) and having by close drawing the Curtains, and other means, made the place pretty dark, I perceived the included Body to continue to shine more vividly, than one would have expected, (and, if I mistake not, I saw it shining in the morning, whilst it was dark;) but the night after, coming to look upon it again, its light appeared no more: notwithstanding which, I made a shift to keep out the Air about 24 hours longer, and so after 48 hours in all, we opened the Receiver in a dark place, and presently upon the ingress of the Air were pleasingly saluted with so vivid an Apparition of Light, that the included Body continued to shine, when carried into a room, where there was both Fire and Candle, if it were but by a Hat screen’d from their beams.

Being encouraged, as well as pleased with this success, we forthwith exhausted the Air once more out of the same Receiver, and having kept it about 4 hours longer, we looked upon it again in a dark place, and finding no appearance of Light, let the Air in upon it, whereby it was made to shine again, and that vigorously enough, so that I caused the Receiver to be exhausted once more; but that it being Sunday night, I was unwilling to scandalize any, by putting my Servants upon a laborious, and not necessary work.

The suddenness, with which the included Body appeared to be, as ’twere, re-kindled upon the first contact of the Air, re- vived in me some suspicions I have had about the possible causes of these short-lived apparitions of Light (for I speak not now of real Lamps, found in Tombs, for a reason to be told you another time,) which disclosing themselves upon mens coming in, and consequently letting in fresh Air into Vaults, that had been very long close, did soon after vanish. These thoughts, as I was saying, occurr’d to me upon what I had been relating, by reason of the sudden operation of the fresh Air upon a Body, that but a minute before disclosed no light. For, though the Lights reported to have been seen in Caves, quickly disappeared, which that of our Fish did not; yet that difference might possibly proceed from the Tenacity, or some other Disposition of the matter, wherein
wherein the Luminousness of the Fish resides. For I remembred, that I had more than once observed a certain glimmering & small Light to be produced in a sort of Bodies upon putting them out of their former Rest, and taking them into the Air, which sparks would vanish themselves somtimes within one minute, sometimes within a few minutes. But as these thoughts were but transient conjectures, so I shall not entertain you any longer about them, but rather contenting my self with the hint already given, take notice of what may be more certainly deduced from our Experiment, which is, That the Air may have a much greater interest in divers odde Phenomena of Nature, than we are hitherto aware of.

And for Confirmation of our Experiment I shall adde, that, having in another Receiver eclipsed a piece of Fish, that shone when twas put in more languidly than divers others that we had tried, I kept it about three days and three nights in a Receiver, which (Receiver) being somewhat like another, at first suggested to me, when I came to take it, some scruple, but afterwards, upon further examination, concluded it to be the same; wherefore I opened it in the dark, and upon letting in the Air on this Body, that shined but faintly at first, it immediately recovered its so long suppressed Light: and having included another piece that was yet more faint than this, when it was put into the Receiver, I thought fit to try at once the Experiment hitherto confirmed, and the Converse of it. And therefore having kept this piece also three days and three nights in the exhausted Glass, I let in the Air upon it, and notwithstanding the darkness of the place nothing of life was thereupon revived. But this being little other than I expected from a Body, that shined so faintly when twas put into the Receiver, and had been kept there so long, I resolved to exercise my patience a while as well as my curiosity, and try, Whether the Appulse and Contact of the Air would have that operation after some time, that it had not at first; and accordingly, after having waited a while, I observed the Fish to disclose a Light, which though but dim, was manifest enough; but having considered it for some time, I had not leisure to watch, whether twould increase, or how long twould continue.

I know not, Sir, whether you are weary with reading, but I am sure I am quite tired with making so many Experiments upon one
Subject; and therefore I shall here conclude this Paper, as soon as I have added this Confirmation, as well of what I last related, as of something that I observed before. That having included in small Receivers two pieces of rotten Whitings, whereof the one, before it was put in, scarce shone so vividly, as did the other after the Receiver was exhausted; and having ordered the matter so, that we were able to keep out the Air for some days, at the end of about 48 hours we found, that the more strongly shining Body retained yet a deal of Light. But afterwards looking upon them both in a dark place, we could not perceive in either any show of Light. Wherefore having let in the Air into that Receiver, whereinto the Body that at first shined the faintlier had been put, there did not ensue any glimmering of Light for a pretty while: nay, upon the rushing in of the Air into the other Glafs (then also made accessible to the Atmosphere) the body that at first shone so strongly, and that continued to shine so long, shewed no glimmering of Light. But being resolved to expect the issue a while longer, our patience was rewarded within less than a quarter of an hour with the sight of a manifest Light in the Body last named, and a while after the other also became visible, but by a light very dimm. The more luminous of these Bodies I observed to retain some Light 24 hours after: and the hitherto recited Experiment had this peculiar Circumstance in it, That the two Receivers were un-interruptedly kept exhausted no less than 4 days, and as many nights.*

An Account of two Books.

I. PATHOLOGÆ CEREBRI & NERVOSI GENERIS SPECIMEN: in quo agitur de MORBIS CONVULSI-VIS & SCORBUTO, studio THOMÆ WILLIS, M. D.

What this excellent Author formerly promised of the whole Pathology of Brain and Nerves, he gives in this Book a very considerable Specimen of. The knowledge of the Diseases which use to affect these parts, is esteemed very difficult and intricate, and particularly the true Causes of Convulsions are of a very deep research. For the clearing them up, this Author Philosopহadeth
Iosophist after this manner. He teacheth, that there are indeed Animal Spirits, that they constitute the Being of the Corporeal Soul, and are also the next and immediate Instruments of all Animal motions, producing them by a kind of Explosion or Shooting, upon which Elastick, or Explosive power he establisheth his whole Doctrine of Convulsions. To which he annexes a Disquisition of the Scurvy, as being near of kin to the same Doctrine, and grounded upon the same Hypothesis.

The First part consists of XII Chapters.

1. Of Convulsive Motions in general.
2. Of the Epilepsy or Falling-Sickness.
3. Of the differences of the Epilepsy, and the Causes of some of the Symptoms thereof; together with a Draught of the method of Curing this Disease.
4. Of the other kinds of Convulsions, and particularly of those in Children.
5. Of the Convulsive fits in grown and aged persons, proceeding chiefly from the vitiated Origin of the Nerves.
6. Of those Convulsive Motions, whose Cause lies about the Extremities of the Nerves, or within the Plexus nervi.
7. Of those that are caused by Liquors, bedewing the nervous Bodies, and irritating the whole processes of them into Convulsions: where are considered the Cramps that arise from Poisons.
8. Of the Universal Convulsions, that are wont to be caused in Malign, or ill Judged, and some anomalous Feavers: where are described the Epidemical and Maligne Convulsive Distempers, formerly reigning in some parts of Germany, as also that Epidemical Feaver, which raged An. 1661 in England, and did principally afflict the Brain and Nerves.
9. Of those that proceed from the Scorbutick Disposition of the Nervous Fuyce.
10. Of Hysterick Passions, where he maketh the Womb plead Not guilty.
11. Of those Passions, that are vulgarly called Hypochondriacal; shewing, that they are chiefly Convulsive; where by the by he treateth of Chalybeat Remedies.
12. Of Convulsive Coughs and Asthma's, and their Cure.
The Second part contains XI Chapters.

1. Of a Description of the \textit{Scurvy}, its internal and next Cause to be principally in the Blood, and sometimes in the depraved Juyce of the Nerves.

2. Of the remotè Causes of the \textit{Scurvy}, and whence the Mass of the Blood, and consequently the Nervous Liquor, receive its depravation.

3. Of the Differences of the \textit{Scurvy}, its Signs and Symptoms, and chiefly such, as arise from the Blood being tainted.

4. Of those Accidents, which happen in the \textit{Scurvy} by reason of the Brain and the Nervous Stock being vitiated.

5. Of the Symptoms that arise from the conjoin'd Distempers of the Blood and the Nervous Juyce.

6. Of the Prognosticks of the \textit{Scurvy}; where he takes notice, that a prevailing Scurvy at last uttereth in a Dropse or a Consumption.

7. Of the Cure of the \textit{Scurvy}; where, among other Remedies, he mentions several \textit{Specificks} for this Disease.

8. Of such Medicaments as respect the \textit{Scurvy}, caused in a Hot Constitution, and in a Sulphury-sain Distemper of the Blood.

9. Of the Cure of the various and urging Symptoms of the \textit{Scurvy}, as, difficult Respiration, Scorbitical Collick, Fluxes, Giddines, Hemorrhages, loose and rotten Gums and Teeth; Pains in all the Limbs; besides the Scorbucket Goats, Convulsions; Palsies, Atrophy, Fever; as also Rheumatisms, Dropses, and the Noise in the Bones, which some are troubled with in this Disease.

10. Of the Vital Indication, declaring, by what method and Medicines either the fainting Patient may be supported, or his decayed Forces restored: Where he discourse of Cardiacal Op- piat Medicines, and of the Diet that is fit for Scorbitical persons.

11. Some rare and very considerable Relations and Cases of Scorbucket Men and Women.

II. ALPHABETUM NATURÆ, 1667. Author: F. M. B. V. HELMONT.

His small \textit{Tract} (the Subject whereof seems to be New, not treated of hitherto by any we know of in Print) is publish'd both in the Latin and German Tongue, but came to our hands in the latter only; a Latin Copy, designed
designed for us before, having miscarried at Sea, which we have not been able hitherto to get supply'd; because of the scarcity of the Copies of that Edition, as it was signified to us from Amsterdam.

It is divided into three parts: The First, in Five Dialogues, treateth

1. Of the Motions and Configurations of the Mouth of Man; and how a Man, born deaf, and consequently dumb, may come to understand both them, and by them the Mind of him that forms them: Where 'tis observed, that a Man born deaf is not altogether destitute of all Motion of his Tongue, and that he may be taught to understand others by the Motions of their Mouth and Tongue, much after the manner as 'others are taught to read; but that this is much more easily practicable in those wide-mouthed Languages, which do remarkably expose to the Eye the Motions of the Tongue, Lips, Throat, &c., than in those that are narrow-mouthed, and require but very slight Motions of the Lips and the other Organs of Speech. To which is annexed a Method suited to that principle, of teaching deaf and dumb Men to speak, together with an Example of a Musician, who being altogether deaf, and weak-sighted withall, was by the Author brought so far in the space of three weeks, that he was able to answer to all that was spoken to him, provided it were done slowly, and with a well-opened Mouth; who also afterwards by himself, as soon as he had by this very way learned to know the Letters, and to Read, did, by confronting only the German and Hebrew Bibles, learn in a short time the Hebrew Tongue so well, that now he understands the whole Hebrew Bible.

2. Of the Proprieties of the Hebrew Tongue. How the Letters thereof are mere representations of the Motions of the Mouth? How the Hebrew Alphabet may be described, (as 'tis done here) by representing the Letters thereof in the Mouths of so many pictured Men's heads? How this Alphabet is to be compared with other ways of Writing? To which is added a Disquisition concerning the true ancient manner of Writing, and the original of other ways, and how the true way may be found out, and how much the Names of the Letters conduce thereunto.

3. Of the Nature and Properties of the Tongue of Man. Where first is considered the Speech of young Children, how and in what order that is made: then, the requisiteness of the Change of Tone to Speech; and how by the various Openings of the Mouth and the Teeth, that change is caused. After which the Author proceeds to the description of the Tongue, wind-pipe, Larynx, Epiglottis; where he not only shews, how by a peculiar use of the Epiglottis one may come to speak inwardly, as do the Vemriloqui, by attracting the Breath, and without opening the Mouth; but annexeth also divers Rules concerning the Motions of the Tongue, observing especially the perpetual concord of the Mouth and Tongue in their Motions, and the Representation of the Tongue's Motion, requisite for every Letter, in the Image and Figure of such Letter.
4. Of the Breath of Man, its quality, and its necessity for the forming of the Voyce. Where he shews, How by inspiring, the Breath is distributed through the Lungs (the nature of which he also describes;) and how the same circulates through the whole Body.

5. Of the Animation of the Voyce of Man by his Masculine and Generative power. Where he discourses of the difficulty, found in little Children, to pronounce certain Letters; and also of the Weakness of the Voyce in Children and Eunuchs. Some of the first part.

The second part, in one Dialogue, gives an account, How the Motions and Configurations of the Mouth, and the Framing of the Voyce are represented in the Hebrew Alphabet (for, to this Tongue the Author confines himself;) but with what reason the Intelligent Reader of the Book it self may judge:) explaining the Sound of each Letter, the Motion of the Tongue accompanying the Sound, and the Figure of the Letter, resulting from thence; together with each Letters name, signification, and aptitude to form the next following Letter. Which done, he proceeds to shew, how the Vowels are formed by the different Openings of the Mouth and Teeth; so that to every Vowel belongs a peculiar dimension of Wideness in the Mouth: And according to the shape of the outward Mouth, the Tongue and Breath are conformably moved within. To which are added the Names of the Vowels, and their Figures, their absence in most ancient Writings, and the several places in the Mouth for their formation. All which is concluded partly with a deduction of the Cause, why the Hebrew Tongue is written from the Right hand to the Left? partly, with an Explanation of the method used to compose out of such Letters and Language a Grammar, viz. by comparing the Consonants with one another, and the Vowels as well with one another as with the Consonants: forasmuch as the nature of one being well considered with respect to the other, 'tis not difficult to judge, how they agree or disagree together: Whence Rules may be formed, how and why this or that Letter or Vowel is to be changed into another, &c.

The third part treateth of the perfection of the Hebrew Tongue; wherein it consists, and how highly 'tis to be valued. To which is subjoined first a Disquisition of reason, why the Radical Words in the Hebrew Tongue can consist of no more but three Consonants? Secondly, a Discourse touching the Hebrew Accents, shewing, that 'tis a New Invention, and that the Ancient Art of Musick being lost, and consequent to us unknown, the Modern Hebrew Accents cannot reasonably be said to have been the Musical Notes of the Ancients; since especially all sorts of Hebrew Books, and even their Chapters are marked therewith: though the Author acknowledges, that their newness deprives them not of the advantage of being useful.

In the 5 A V O T,
Printed by T. N. for John Martyn, Printer to the Royal Society, and are to be sold at the Bell a little without Temple-Bar, 1667.
The Contents.


Observations and Tryals
About the Resemblances and Differences between a Burning Coal and Shining Wood.

These particulars were already in our hands, when we publish the Experiments made on Shining Wood and Fish, in the last Papers, imparted then by the same Noble Author (Mr. Boyle) that those were; but wanted then room enough to contain these, which now follow; as they were sent in a Letter from Oxford, Viz.

And
And now, Sir, seeing the want of Shining Wood hath kept me ever since I sent you the former Experiments from making any New ones on that subject, I shall, by way of amends, subjoin some of the Observations, that I heretofore intimated to you, I had made of the Resemblances and Differences between a Live Coal and a Piece of Shining Wood; in perusing of which you will easily discern, that to those Particulars, which my Memory and the former Observations, I had noted down about Light and Luminous Bodies, had suggested to me, I have added some, that have been afforded me by those late Tryals, made in my Engine, whereof I shall you an account.

Resemblances.

The things, wherein I observed a piece of Wood and a Burning Coal to agree or resemble each other, are principally these Five.

1. Both of them are Luminaries, that is, give Light, as having it (if I may so speak) residing in them, and not like Looking Glasses or White Bodies, which are conspicuous only by the incident beams of the Sun, or some other Luminous Body, which they reflect.

This is evident, because both Shining Wood and a Burning Coal, shine the more vividly, by how much the place, wherein they are put, is made the darker by the careful exclusion of the adventitious light. 'Tis true, that the Moon and Venus appear brightest at, or about Midnight, and yet have but borrowed light; but the difference between those Planets and the Bodies we treat off, in reference to the difficulty we are considering, is obvious enough. For, though the Beholders eye that looks upon those Stars, be advantag'd by being in the dark, which enlarges the Pupill of the Eye, yet the Object itself is freely exposed to the beams of the Sun, which if they were intercepted, those Planets would quickly be darkned, as experience manifests in Eclipses.

2. Both Shining Wood and a Burning Coal, need the presence of the Air, and are too of such a density, to make them continue shining.

This has been prov'd as to a Coal, by what I long since published in my Physico Mechanical Experiments, where I relate, How quickly
quickly a Coal would be extinguish'd upon the with-drawing the Air from about it: And as to Shining Wood, the Experiments I lately sent you, make it needless for me to add any other proof of the requifitenefs not only of Air, but of Air of such a thick-ness, to make its light continue. How far this is applicable to Flame, it is not neceffary here to determine; though, when I have the fatisfaction of feeing you again, I may tell you fomething about that Question, which perhaps you do not expect.

3. Both Shining Wood and a Burning Coal, having been deprived for a time of their light, by the with-drawing of the contiguous Air, may presently recover it by letting in fresh Air upon them.

The former part of this, particular Tryals have often fhown you to be true, when kindled Coals, that seem to be extinguish'd in our exhausted Receivers, were presently reviv'd, when the Air was restored to them: And the latter part is abundant-ly manifest by the Experiments, to which this Paper is an Ap-pendance.

4. Both a quick Coal and Shining Wood will be easily quench'd by Water and many other liquors. The truth of this, as to Coals, is too obvious to need a proof; and therefore I shall confirn it only as to Wood. For which pur-pofe you may be pleas'd to take the following Transcript of some of my Notes about Light.

I took a piece of Shining Wood, and having wetted it with a little Common Water in a clear Glass, it presently loft all its light.

The like Experiment I tried with strong Spirit of Salt, and alfo with weak Spirit of Sal Ammoniack; but in both the light did, upon the Woods inbibing of the liquor presently disappear.

And leaft you fhould think, that in the Words, Many other liquors, I intended not to comprife any, that confift of soft and unctuous parts, or that are highly inflammable, I shall sub-join a couple of Notes, that I find next to thofe just now tran-fcribed.

* From hence you will easily ga her the reafon; why, when I lately told you of the Trial, I made with a piece of Shining Fish under Water in the un-exhausted Receiver, I did not propofe to have the like Trial made with Shining Wood and Water; but for this Liquor substituted Mercury.
I made the like Tryal with rectified Oyl of Turpentine, with a
not unlike success. The same Experiment I tried more than
once with high rectified Spirit of Wine, which did immediately
destroy all the light of the Wood, that was immersed in it;
and having put a little of that liquor with my finger upon a part
of the whole piece of Wood, that shine very vigorously, it quickly
did, as it were, quench the Coal as far as the liquor reach’d;
nor did it in a pretty while regain its luminousness: (which whe-
ther it recovered at all, I know not; for this Trial being made
upon my Bed, I fell asleep, before I had waited long enough to
finish the Observation.)

5. As a quick Coal is not to be extinguisht by the Coldness of the
Air, when that is greater than ordinary, so neither is a piece of
Shining Wood to be deprived of its light by the same quality of
Air.

As much of this Observation, as concerns the Coal, will be
readily granted, and for proof of the other part of it, I could
relate to you more Trials, than one, but that I suppose, one may
suffice, circumstances like that, which I shall now relate.

I took a small piece of Shining Wood, and put it into a slen-
der Glass-pipe, sealed at one end, and open at the other, and
placed this Pipe in a Glass Vessel, where I caus’d to be put a
strongly frigorifick mixture of Ice and Salt; and having kept it
there full as long, as I thought would be requisite to freeze an
Aqueous Body, I afterwards took it out, and perceived not any
sensible Diminution of its light. But to be sure, the frigorifick
mixture should not deceive me, I had placed by this Pipe an-
other, almost filled with Water, which I found to be turn’d into
Ice; and though I suffered the Wood to remain, a pretty while
after, expos’d to so intense a Cold, yet when I took it out, it
continued shining, and, if I much mistake not, it ceased not to
do so, when I look’d on it, 24. hours after. But though the
light of Shining Fish be usually (as far as I have observed) more
vigorous and durable, than that of Shining Wood; yet I cannot
say, that it will hold out against Cold so well, as the other.
For, having ordered one of my Servants to cut off a good large
piece of the luminous Whiting, and bury it in Ice and Salt, when
I call’d for it in less than half an hour after, I found it much stiff’n’d
by
by the Cold, and to have no light, that I could discern in a
place dark enough. And for fear, that this effect may have
proceeded not barely from the operation of the Cold, but
also from that of the Salt (for which suspicion you would
see reason enough, if I could shew you my Trials about
Shining Fish) I caused another time a piece of Whiting to be
put in a Pipe of Glass seal'd at one end, and having seen it
shine there, I lookt upon it again, after it had stay'd but a
quarter of an hour, by my estimate, in a frigorifick mix-
ture, which the Glass kept from touching the Fish; and yet
neither I, nor a Youth, that I employed to look on it, could
perceive in a dark place, that it retain'd any light; which
whether the Cold had deprived it of by that great change of
Texture, that the Congelation of the Aqueous Juice of the
Fish (which I have several times observed to be luminous) may
be supposed to have made in the Body invaded by it; or whe-
ther the effect depend more principally on some other cause, I
shall not now examine.

Differences.

1. The first difference I observed betwixt a Live Coal and
Shining Wood, is, That, whereas the light of the former is
readily extinguishtable by Compression (as is obvious in the pra-
ctice of suddenly extinguishing a piece of Coal by treading upon
it) I could not find that such a Compression, as I could conve-
niently give, without losing sight of its operation, would put out
or much injure the light ev'n of small fragments of Shining
Wood: One of my Trials about which I find thus set down a-
mong my Notes about Light.

I took a piece of Shining Wood, and having perch it be-
tween two pieces of clear Glass (whereof the one was pret-
ty flat, and the other convex) so that I could clearly see
the Wood through the Glass, I could not perceive, that
the compression, though it sometimes broke the Wood in-
to several fragments, did either destroy or considerably alter
the Light.

This Experiment I repeated, with the same success. But,
what a stronger or more lasting Compression may do in this Case, I had not opportunity to try.

2. The next unlikeness to be taken notice of betwixt Rotten Wood and a Kindled Coal, is, That the latter, will in very few minutes be totally extinguished by the withdrawing of the Air, whereas a piece of Shining Wood, being eclipsed by the absence of the Air, and kept so for a time, will immediately recover its Light, if the Air be let in upon it again within half an hour after it was first withdrawn.

The former part of this Observation is easily proved by the Experiments, that have been often made upon Quick Coals in the Pneumatical Engine; and the truth of the latter part appears by an Experiment about Shining Wood made by us in October last. Neither is it unprobable, that if I had had convenience to try it, I should have found, that a piece of Shining Wood deprived of its light by the removal of the ambient Air, would retain a disposition to recover it upon the return of the Air, not only for half an hour (which is all that I lately asserted) but for half a day, and perhaps a longer time.

3. The next difference to be mention'd is, that a Live Coal being put into a small-close-Glass, will not continue to burn for very many minutes; but a piece of Shining Wood will continue to shine for some whole dayes.

The first part of the Assertion I know you will readily grant, and the rather, because it contains matter of fact, without at all determining, whether the Coals not continuing to burn, proceeds from its being, as it were, stifled by its own smoke and exhalations (which can have no vent in a small-close-Glass) or from the want of fresh Air, or from any particular cause, which I must not here debate; though I have sometimes made Experiments somewhat odd to facilitate that enquiry. The other part of our Observation may be easily made out by what I tried upon Shining Wood, sealed up hermetically in very small Glasses, where the Wood did for several dayes (though I remember not precisely how many) retain its Light.

4. A
4. A fourth Difference may be this; That, whereas a Coal, as it burns, sends forth store of smoke or exhalations; Luminous Wood does not so.

5. A fifth, flowing from the former, is, That, whereas a Coal in shining wastes it self at a great rate; Shining Wood does not.

These two unlikenesses I mention together, not onely because of their affinity; but because what concerns the Coal in both, will need no proof; and as for what concerns Rotten Wood, it may be verified by an Observation, that I find by my Notes. I made in a piece of it Hermetically sealed up in a small clear Glass; where after it had continued luminous some dayes, I lookt on it in the day time to perceive, if any store of spirits or other steams had, during all that while, exhaled from the Wood, but could not find any on the inside of the Glass, save that in one place there appeared a kind of Dew, but consisting of such very small drops (if at least their Size were not below that name) that a multitude of them would go to the making up of one ordinary drop. But in pieces of Shining Wood I found the case much otherwise, as was to be expected.

6. The last Difference, I shall take notice of betwixt the Bodies hitherto compared, is, That a Quick Coal is actually and vehemently hot; whereas I have not observed Shining Wood to be so much as sensibly lake warm.

What is said of the Coals heat, being as manifest as its light, I shall need only to make out, what relates to the Shining Wood. To assist me wherein, I meet among my Notes, that, whose transcript I shall subjoyn, when I have premis'd, that (if my memory do not deceive me) the piece of Wood to be mentioned was one, that shone so vividly, that waking in the Night some hours before I tryed it, and perceiving, as it lay near me on the Bed, how luminous it was, I was invited to reach out to a place near the Beds-head, where there stood several Books, and laying the Wood on that, which came to hand, I could discern by the light of it, that the Book was an Hebrew Bible, and that of the Page, I lighted on, the wrong end was turn'd upwards: To which intima-
tion having added, that the little Glass-Instrument, mention'd in the Note, is such an one, as you may find described in my Preliminaries to the History of Cold, save that part of this was a little bending inward at the Basis, that it may sometimes stand by itself, and sometimes receive a small body into the dimple at its Basis: Having, I say, premised this, and, that as Shining Wood did not feel at all warm to me, so I also found Shining Fish palpably cold, I shall conclude your trouble with the premised Note, which speaks thus:

[ I put upon a large piece of Wood, which was partly shining, and, as near as I could, upon one of the most luminous parts of it, one of those Thermoscopes, that I make, with a pendulous drop of Water. But as I had formerly try'd, that by laying the tip of my Nose or Finger upon it, when it shone vividly enough, to enable me to discern both the one and the other, at the time of contact I could not perceive the least of heat, but rather an actual coldness; so by this Trial I could not satisfy myself, that it did visibly raise the pendulous drop, though the Instrument were so tender, that by approaching one Finger near it, yet without actually touching of it, it would manifestly be impelled up, and upon the removal of my Finger, would presently descend again. ]

And I remember, that having put such an Instrument upon a Shining Fish, that was pretty large, I could not thereby perceive, that it had any degree of heat, but rather the contrary. For, having divers times taken off the Glass, to apply it with the more advantage to several parts of the Luminous Fish, I divers times (for I remember not, whether 'twere always) took notice, that upon the removal of the Glass into the Air, the pendulous drop would manifestly rise a little, and subside again when the Glass was applied to the Fish. But whether this part of the Experiment will hold in all temperatures of the Air, I had not opportunity to try.
An Observation

Concerning a Blemish in an Horses Eye, not hitherto discover'd by any Author; which may be of great use in the Choice of an Horse to those who are Curious; made by Dr. Richard Lower at the Royal Society, January 23. 1667.

Among the many defects and distempers in the Eyes, the Eyes of Horses are peculiarly affected with one, which no Animal besides is troubled withall (as far as I have observ'd) neither do I remember any Author hitherto to have taken notice of it; and that is a Spungy Excrecence (commonly of a dark muske-colour) which grows out of the edge of that Coat of the Eye call'd the Uvea; which Spunge if it grow large or increase in number (as it frequently happens) it depraves the sight very much, or totally intercepts it. But that you may more easily conceive the manner, how it is done, you may remember, that the Uvea is a muscular part, the use of it being chiefly to contract and dilate it self for the admission of Objects with as much light, as the Eye can conveniently bear; so that, the brighter and more resulgent the light is, to which the Eye is expos'd, that Membrane contracts its self into a narrower compasse; and the more dark the place is, it delates it self the more, as you may see in a Cats eye more readily perform'd, than in any other Animal, I have yet observ'd: So that, if that Spungy substance, which growes out of the edge of the Uvea, be so great, or the number of them such, as that they grow in several places about the pupill of the Eye, where it contracts its self, the pupill or sight is very much (if not totally) obstructed, and consequently the Horse sees very little or nothing at all: As I have many times taken exact notice in some Horses, which being brought into the Sun-shine, could not see at all, but suffer'd me to touch the sight of their Eye with my finger without the least winking; which Horses being led back into the Stable, the Uvea in that obscure place dilating it self, they could see very well again, and would not suffer me to shew my finger near to the Eye without frequent closing their Eye-lids and tossing their heads. The same Horses I understood by the Tt r Owners
Owners were very apt to stumble in the day-time, if it were bright and Sun-shine, but travel'd very well and securely in the evening and in dark cloudy Weather.

What the cause may be of that fungous Excrecence, or why Horses are peculiarly obnoxious to it, or what kind of Horses most, I have not considered. But, I cannot think, it comes from straining in great draughts, and races, or from hard travel, because I have seen very large spunges (as I may call them) in young Horses eyes of 2 and 4 years old, before they were backed; which, after they have been taken up from grass, and kept with dry meat, have very much abated, and afterwards being turn'd to grass, in the Spring to cleanse and cool their bodies, have increased again to the wonted bigness. But whether it were from their moist feeding, or holding down their heads to eat (whereby there might be a greater defluxe of humors to that part) I cannot determine. But for as much as there are few Horses quite free from this evil, and many rendered very inconsiderable by it, I will recount the most remarkable Cases, which make Horses most useless and suspected;

1. The more and greater those Excreences are, the more the pupill of the Eye or the Sight is in danger of being quite obstructed; which you may farther examine by turning the Horse's Eye to the light, and observing, how much of the pupill they doe obstruct.

2. These spunges on the upper edge of the Uvea are apt to grow the largest, and hinder the Sight most.

3. That which grows on the middle of the Uvea, does more hinder the sight by distracting the Object, than that, which grows in either corner or angle of it.

As for the Cure, I suppose there can be none expected, but from a drying kind of dyet; though perhaps outwardly something may be devised to shadow the Eyes, and keep them from being nakedly exposed to the Sun, whereby the pupill will not be so closely contracted, and consequently the Sight not so much obstructed.
An Extract

Of a Letter written by Signor Caffini Professor of Astronomy in Bônonia, to Monsieur Petit at Paris, and Englished out of the Journal Des Scavans; concerning several Spots, lately discover'd there in the Planet Venus.

To give you some Account of my present Studies, I shall acquaint you, that having been a good while very assiduous and careful in making Observations of Venus, to see whether that Planet did not turn about its Axis, by a motion like to that of Jupiter and Mars; I met at first with many difficulties; but at last considering, that I should succeed better in my Observations at a time, when Venus is at a good distance from the Earth, than when she is near thereto, I attentively observ'd, when she was risen somewhat high above the Horizon, and shined brighter, whether I could not discern in her some part remarkable either by its brightness or obscurity, among the rest, especially about the middle of her Disc. And this I did not in vaine; for, I discover'd at last towards the middle of her Body a part clearer than the rest, by which one might judge of the Motion or the Rest of this Planet.

The first time I saw it, was Octob. 14, 1666. h. S. 45 [p. m.] and then this bright part was very near the Center, on the North-side. And at the same time I observ'd Westward two obscure spots, somewhat oblong. But I could not then see that reple- dent part long enough, to conclude any thing from thence; nor was I able to see any thing well of those parts till April 28, 1667; on which day, a quarter of an hour before Sun-rising I saw again a bright part, situated near the Section, and distant from the Southern Horn a little more than 1/4 of its Diameter. And near the Eastern Ring I saw a dark and somewhat oblong spot, which was nearer to the Northern than the Southern Horn. At the rising of the Sun I perceived, that this bright part was then no more so near the Southern Horn, but distant from it 1/4 of its Diameter. This gave me great satisfaction.
I was surprised at the same time to find, that the same Motion, which was made from South to North in the inferior part of the Disk, was on the contrary made from North to South in the superior part; whence the determination of the Motion may be better taken. For we have no example of the like motion, except it be in that of the Libration of the Moon.

The next day, at the rising of the Sun, the said bright part was not far from the Section, and distant from the Southern Horn of the Diameter. When the Sun was 4. deg. high, the same was situated near the Section, and remote from the Southern Horn of the Diameter. The Sun being high 6.d. it seem'd to have been past the Center, and that the Section of the Disk did cut the same. The Sun being 7. deg. high, it appeared yet more advanced North-ward, together with two Obscure Spots, seated between the Section and the Circumference, and equally distant from one another, and from each Horn on both sides. And the Sky being very clear, I observ'd the motion of the bright part for 1 hour, which then seem'd to be exactly made from South to North, without any sensible Inclination East- or West-ward. Mean-time I perceiv'd in the motion of the dark Spots so great a Variation, that it cannot be ascribed to any reason in Opticks.

May 10. and 13. before Sun-rising, I saw still the bright part near the Center North-ward.

Lastly, June 5. and 6. before the rising of the Sun, I saw the same between the Northern-Horn and the Center of this Planet, and I noted the same irregular Variation in the obscure Spots. But when Venus began to be further removed from the Earth, it was more difficult to observe these Phænomena.

I shall not presume to declare my sentiment touching these Apparances so boldly, as I did concerning the Spots formerly discover'd in Jupiter and Mars. For those spots I could very well observe for a whole night together, during the opposition of those Planets to the Sun; I could consider their Motion for the space of several hours; and at last, seeing them return regularly to the same place, I could judge, whether they were the same spots or not, and in how much time they
they absolv'd their Relation. But it was not so here with the Apparances in Venus. For one sees them but for so small a time, that it is far more difficult, certainly to know, when they return to the same place.

Yet this I can say, (supposing that this bright part of Venus, which I have observ'd, especially this year 1667, hath always been the same) that in less than one day it absolves its motion, whether of Revolution or Libration, so as in near 23 hours it returns about the same hour to the same situation in this Planet; which yet happens not without some irregularity. Now to affirm, (supposing it to be always the same bright part) whether this Motion is made by an entire Revolution, or by a Libration, I dare not yet doe, in regard I could not see the Continuity of the Motion through a great part of the Arch, as I did in the other Planets. And for this very reason, that will always be difficult to determine.

An Extract.

Of a Letter, written by J. DENIS, Doctor of Physick, and Professor of Philosophy and the Mathematicks at Paris, touching a late Cure of an Inveterate Phrensy by the Transfusion of Blood.

This Letter was lately sent by the Author himself to the Publisher, as it was printed at Paris in French; the substance whereof is in English, as follows.

It is now almost a twelve-month that I declared myself publickly in this matter of Transfusion, and after I had grounded my Conjectures upon divers reasons, and a number of Experiments which I made jointly with M. Emmerez, I resolv'd to expect in the sequel a further confirmation, by carefully observing all that should happen in the several Trials, I intended to practice.

In this resolution we have since let slip no occasion, to improve this Operation, which hath been follow'd with good success, and I could here alledge some particular Relations, the circumstances of which would appear curious enough, if I did not
not rather choose to refer them to a Collection, which possibly
I may send you within some time, to have the more room to
enlarge in this Letter on the circumstances of a Story, whereof
you will be very glad to learn the Event.

You have doubtless heard of a Mad man, that hath been late-
ly cured, and restored to his wits by the means of the Transfu-
sion. Some spread a rumour that he died soon after the operati-
on; others bore the people in hand, that he was relapsed into
a greater madness, than that before; and in short, it hath been
fo diversely discoursed of up and down, and with such differing
reflexions thereon, that I thought myself obliged, for the clear-
ing up of what false rumours had darkned, to give you a faith-
full and exact account of the condition, to which this poor
Man was reduced before the Transfusion; of what passed during
that Operation; and the surprising effects, that have followed
upon it hitherto.

The Patient is about 34 years of age. His Phrensy began
first of all to appear 7 or 8 years agoe, and as far as can be judg-
ed, it was occasion'd by a disgrace he received a little before,
in some Amours, where he hoped to find a very considerable
fortune. This first fit of extravagance was very violent, and la-
sted 10 months without any good intervall: but returning after-
ward by little and little to his wits, and having given all the pos-
sible marks of a sound understanding, he was married to a young
Gentlewoman, who was persuaded, that this madness of his
was the relick of a Sickness he had before; and that there was
no appearance he would ever relapse into it. But this was far from
proving so, as was imagined, and even the very first year of his
marriage ended not without his returning to his former Extra-
vagancies.

Thus then he relapsed, and was several times restored these
7 or 8 years last past. But what is here chiefly to be observed,
is, that the fit never lasted with him less than 8 or 10 months,
without any repit, notwithstanding all the care and means used
to relieve him. For it is also fit to take notice, that a person
of quality, having once taken a purpose to attempt his Cure by
all manner of wayes, caused him to be bled in his feet, armes,
and head, even 18 times, and made him bath himself 40 times,
not to mention innumerable applications to his fore-head, and potions. But instead of amendment, the distemper seem'd to be provoked by those remedies, and this poor creature fell into that rage, that there was a necessity to bind him up from doing mischief. His Madness hath been always *periodicall*, and never abated but by little and little, and that abatement hath be-falne him rather at such times, when nothing was done to him, than when he was tormented with medicines.

The last time, that he relapsed into his Extravagancy, was about 4 moneths since, in a place 12 leagues distant from *Paris*. And his Wife hearing of it, went immediately to him to relieue him. She soon shut him vp, and was even constrained to tye him for some time, because he was in such an extraordinary rage as to beat her. But for all her care, one time he got loofe stark naked, and ran away streight for *Paris*, no body knowing, how he could find his way in the dark night. His Wife had him searched for in all the neighbouring Villages, whilst he ran here in *Paris* up and down the streets, without finding any place to retire to, in regard that those, who had the charity of receiving him into their houses the first dayes, knew very well the danger they were in, of having their houses burnt over their heads.

He was not less outrageous in this last fit, than in the former. He hath spent 3 or 4 moneths without sleep, and his greatest diversion during that time was, to tear the Cloaths, that were given him, to run naked abroad, and to burn in the houses where he was, whatever he could meet with. He moved to compassion all good people, that saw him, and especially those in the *Marais du Temple*, where he was known to most, and where he had been wont to be seen before this distemper as well cloathed and fa-thon'd, as any one of his condition could be.

Monseur de *Montmor* among others was the person most touched with it, and resoluted to employ his interest to procure him a place in one of the Hospitals. But first he thought on the Transfusion, and believed, there would be no danger in trying it upon this Man, being so perswaded by many Experiments, we had already made in his presence. He therefore had been taken up for that end, and having sent for Me and M. Emmerez to ask
our opinion of the fitness of trying the Transfusion upon this man, we answer'd, that we could indeed give good assurance for his Life, and that this Operation was in itself not capable to cause the death of any one, if discreetly managed; but as to the Cure of such an Extravagance, as that appeared to us, we had not yet experience enough to dare to promise him that, and that our Conjectures went no farther, than to think, that the Blood of a Calf by its mildness and freshness might possibly allay the heat and ebullition of his Blood, being mixed therewith. The matter having been sufficiently examined, we resolv'd to carry this Man into a private house; and there we appointed for his Gardian that Porter, on whom we had already practised the Transfusion, 8 months ago, both that the Thing might not appear so new to him, as it might do to others, that never had seen the Experiment before, and that he might serve us the more to assure our Patient, and others, who should be present at the Operation, that there was no danger in it at all.

Decemb. 19. we used what art we could to dispose the Fancy of our Patient to suffer the Transfusion, which we resolv'd should be tryed upon him that night about 6 a clock. Many persons of quality were present, together with several Physicians, and Chirurgions, too intelligent to suspect them of being capable of the least surprize. Mr. Emmerez open'd the Crural Artery of a Calf, and did all the necessary preparations in their presence; and after he had drawn from the Patient about 10 ounces of Blood out of a Vein of the right Arm, we could give him no more again than about 5 or 6 ounces of that of the Calf, by reason that his constrained posture, and the crowd of the Spectators interrupted very much this Operation.

Mean time he found himself, as he said, very hot all along his Arm, and under the Armpits; and perceiving, that he was falling into a Swoon, we presently stopp'd the Blood running in, and closed up the wound. Yet he supped two hours after, and notwithstanding some dulness and sleepiness, he was in now and then, he yet pass'd that night with singing, whistling, and other extravagancies usual with him.

But yet next morning we found him somewhat less exorbitant, both in his actions and words; and that induced us to believe, that
that by reiterating the Transfusion once or twice, we might find a more remarkable change in him. We therefore prepar'd our selves to repeat it upon him the next Wednesday at fix a Clock in the evening again, in the presence also of several very able Physicians, Bourdelot, Lallier, Dodar, de Bourges, and Vaillant. But in regard that this man appear'd very thin, and that it was not at all probable, that his blood was peccant in the quantity after three or four months continual watching, and after the hunger and cold he had suffered in running naked on the streets without finding harbour at nights, we took but two or three ounces of blood from him, and having put him in a more convenient posture, we made this second Transfusion into his left arm more plentiful then the first. For considering the blood remaining in the Calf after the operation, the Patient must have received more then one whole pound.

As this second Transfusion was larger, so were the effects of it quicker and more considerable. As soon as the blood began to enter into his veins, he felt the like heat along his Arm and under his Arm-pits which he had felt before. His pulse rose presently, and soon after we observed a plentiful sweat over all his face. His pulse varied extreamly at this instant, and he complain'd of great pains in his Kidneys, and that he was not well in his stomack, and that he was ready to choak unless they gave him his liberty.

Presently the Pipe was taken out that conveyed the blood into his veins, and whilst we were closing the wound, he vomitted flore of Bacon and Fat he had eaten half an hour before. He found himself urg'd to Urine, and asked to go to stool. He was soon made to lie down, and after two good hours strainings to void divers liquors, which disturbed his stomack, he fell asleep about 10 a Clock, and slept all that night without awakening till next morning, was Thursday, about 8 a Clock. When he awaken'd, he shew'd a surprizing calmnes, and a great presence of mind, in expressling all the pains, and a general lafitude he felt in all his limbs. He made a great glass full of Urine, of a colour as black, as if it had been mixed with the foot of Chimneys.

Hearing of some of the company that we were in a time of Jubily, he asked for a Confessor, to dispose himself to be made participant of it. And he confess'd himself accordingly to M. de Veuau with that exactness, that the Confessor gave him the publick testimony of a sound understanding, and even judged him capable to receive the Sacrament, if he continued in that state and devotion.

He remained sleepy all the rest of that day, spoke little, and prayed those that came to importune him with interrogatories, to give him rest. And he went on to sleep well also the whole night following. Friday morning he fill'd another Urinal with his water, almost as black as that of the day before.
before. He bled at the Nose very plentifully, and therefore we thought it proper to take 2 or 3 small Porringers of blood from him.

Saturday morning, the last day before Christmas, he desired again to go to confefs, and so to dispose himself for the Communion. Then one Mr. Brouet examined him in hearing him confefs, and after he had found him to have all the reason necessary to receive the Sacrament, he presently gave him the Communion. That same day his Urine clear’d up, and after that time it resumed by little and little its natural colour.

His Wife mean time, that had sought him from town to town, came to Paris, and having found him out, when he saw her, he soon expressed much joy to see her, and related to her with great preface of mind the several Accidents that had befallen him, running up and down streets; how the Watch had seized on him one night, and how Calfs-blood had been transfused into his veins.

This Woman confirmed yet more to us the good effects of the Transfusion, by assuring us, that at the season we were now in, her Husband should be outrageous, and very mad against her self, and that instead of the kindness he shewed to her at this Full of the Moon, he used to do nothing but swear, and beat her.

’Tis true, that comparing his calm condition, wherein he now was, with that, wherein every body had seen him before the Transfusion, no man scrupled to say that he was perfectly recover’d. Yet to speak the plain truth, I was not so well satisfied as others seem’d to be, and I could not persuade myself that he was in so good a temper as to stop there, but I was inclin’d to believe by some things I saw, that a third Transfusion might be requisite to accomplish what the two former had begun.

Yet in delaying the execution of these thoughts from day to day, we observed so great an amendment in his carriage, and his mind so clear’d up by little and little, that his wife and all his friends having assured us that he was restored to the same state he used to be in before his Phrenzy, we entirely quitted that resolution. I have seen him almost every day since; he hath expressed to me all manner of acknowledgment, and been also with M. de Montmor, thanking him very civilly for his goodness in recovering him out of that miserable condition he was in by a remedy which he should remember as long as he lived.

He is at present of a very calm spirit, performs all his functions very well, and sleeps all night long without interruption, though he faith he hath now and then troublesome dreams. He hath carried himself so discreetly in some visits he made this week, that divers Physicians, and other persons worthy of credit, that have seen him, can render an authentic testimony to all the circumstances here advanced by me, who shall not employ against cavils and contradictions any other arguments than the experiment it self.
The last year I published my Conjectures and my Reasons. Of all those that have undertaken to combat them, there is not one that hath so much as touched the state of the Question. And this hath made me silent to them all. I have confined my self to the experiments alone: this, whereof I now send you the particulars, will perhaps open the eyes to some that are opinionable. I would not relate the story but very plainly, without embarrassing it by any ratiocinations; not but that there was good matter to discourse upon, both during the time of the Transfusion, and after it. Some believed, that the vomiting came from the eating much Bacon, half an hour before the operation: Others, considering the pain in his Kidneys during the Transfusion, and the swelling of his Stomack, which was presently followed by evacuations above and below, do believe, that the new blood, entering in great quantity, caused a plenitude, and a fermentation in the great vessels, which could not but be followed by all those effects. To which may be added, that almost all those, into whose veins other liquors then blood have been injected, have found the same Accidents.

It is also not yet agreed on, what was the cause of the laftitude, that obliged this man to keep his bed for some whole dayes: Some say, it proceeded from the disorder consequent to the entry of new blood into his veins. Others have look'd upon it as the effect of a kind of Rhumatisme he had got, lying stark naked in the streets; and have thought that he became not sensible of those pains, but after the recovery of his reason, just as those that have a hot Feaver do never complain of weariness, but after the abatement of the Fitt.

There hath also been very differing Discourse of that black colour in his Urine some dayes after the Transfusion; some alledging, that it was caused by some veins, which having been opened from too great a fulness, discharged themselves into the Kidneys and Bladder: others, believing that it was a black choler, discharging it self by the veins, and which being retained before, sent up vapours to the Brain capable to trouble the functions thereof.

I shall here suspend my judgment, resolved not to declare my thoughts, till I have made many experiments more. For I know very well, that in 50, we have made upon Brutes, we have found it but twice that the Recipient animal pissed blood after it, and as far as I can judge, I believe I have infallible ways of preventing all such disorders; and I have proposed to my self a manner of preparation and treatment, which the Patient may be made to observe both before and during the operation, to render it more beneficial. For 'tis not to be doubted, but measures are to be taken to dispose the body for Transfusion, as well as for all other operations to be undertaken with success. But we need not repent for not having observed them in this case; it appeared much better what the Transfusion all alone could
could do, and no cause hath been given to prejudiced men to impute this
cure to the *Preparation* rather, then to the *Operation* it self.

We hear of many other sick persons, who possibly may find relief from
this Experiment. I shall not fail to let you know the success of it in good
time.

**An Exact Narrative**

*of an Hermaphrodite now in London.*

This was communicated by the ingenious Dr. *Tho. Atton* (now a Fellow
of the R. Society) to a friend of his in a *Latin* Letter, in which as it was
imparted to the said Society, so it was thought fit to publish it here for the
view of the Learned; *viz.*

*Inter varias insolentias; Natura lusus, dicam? an errores, quos apud eos,
qui de Androgynis orerunt (quorum scripta sedulo deditaq; opera perla-
stravi) in lucem productos adhuc videre mihi contigisset, vix alium quenquaum
notatus dignorem memini occurrere hoc ipsò, quem tibi, Erudite Vir, imprefen-
tiarum exhibeo. Neq; enim hunc, quem jam descriptum es, Hermaphrodi-
tum, aut specifismis illis feminis, quae apud Graecos vel aldes audium, apud
Egyptios vero frequentissime reperiebantur, annumerandum, aut cum descrip-
tione quacum; haltemus quod sciam evulgata, ullatenus quadrare existimo.
Unde nec prorsus indignus mihi videtur, qui nativis depictis coloribus, absque
omni verborum succo, Illustrissime Leitifimaeque Regiae Societatis & tuis oc-
ulis usurpandus veniat.*

*Nomen ipse est Anna Wilde; natus vero est (condonandus enim Hermaphro-
dito solycismus) Monse Februario, ipso Purificationis fefto, Anno salutis,
1647. in pago non ignobili Agri Hamptoniensis, vulgo Ringwood. Sexto
atatis anno inter salutandum collectandum; cum puere virtuosis (quos omnes
viribus facile superabat) extuberationes dux, Herniarum Bucovinaculorum dicta-
rum, primum emiscere; quibus in ordinem redigendis (id enim illis animierat)
Chirurgi diu operam fuperunt. Testiculi enim erant, qui jam praegrandes facti,
scrips cutaneae, corrigatis pilisq; obstis inclus; non alio discrimine a Virilibus
naturaliter se habebimus distinguuntur, quam quod singulis testes suo proprio
divisq; ab invicem hic scrito gaudeant, ita tamen elongato, ut ex utrinq; pro-
ductione confingantur labia Vulva.*

*In fine Muliebri (ut jam a Mercurio ad Venerem transeamus) Nymphae
& Caruncula Myrtiformes, integra fatis se produnt: Quin & membranula
quadam, a Perineo sursum tendente, media pars Vulvae regitur. Clitoris non
apparet. Uterus ejusq; cervix a communi seseioris sexus legere ne minimum
quidem recedunt. Ufo, ad tertium supra decimum atatis annum pro famella
habitum, & feminæ vestitum indatur, numera illi sexum destinata inter feminas
affidue obibat. Cum forte vero patri subigendo strenuum navaret operam, en
derepente*
An Account of some Books.

I. NOUVEAUX ELEMENS DE GEOMETRIE:

Or a Mathematical Treatise, entituled, New Elements of Geometry, printed at Paris in quarto, Anno 1667.

Divided into 15 Books or Sections, containing

A New Method and Order, and new Demonstrations of the most common Propositions in Geometry,

New ways to discover what Lines are incommensurable,

New meaures of Angles not hitherto considered.

New ways of finding out, and demonstrating the Proportion of Lines.

Wherein we observe, that the Author delivers by a new Method and Order of his own, grounded upon Algebraical Elements, divers new Demonstrations
Demonstrations of the more common Propositions, contained chiefly in the first six Books of Euclid's Elements, and without recourse to Euclid, or any other Geometrical Writer for proof of any thing asserted in those new Elements.

Wherefore is added the solution of an Arithmetical Problem, which the Author calls Magick Squares, viz.

A square of Cells being given, even or odd, filled with Numbers, either in an Arithmetical or Geometrical Progression; so to dispose all those Numbers into another like square of Cells, that all the Numbers of each hand, whether to the right or to the left, upwards or downwards, or diagonally, the Numbers given being in an Arithmetical Progression; added together, do always make the same Summe, and those in a Geometrical Progression, multiplied into one another, do always make the same Product.


This Author pretends to have comprised in this Treatise, containing 58 Propositions, besides many Corollaries, all what hath been hitherto discover'd in Opticks, and to have added thereto many curious and useful remarks, not mentioned in other Authors.

He begins with that part, which is the most simple, and considers the Straight Ray, call'd by the general name of Opticks: where he shews what is the cause of those surprizing effects of the Perspective, which so pleasingly deceive the eye; examining there many curious Experiments.

In the second part (the Catoptricks, that have for their Object Rays Reflected) he gives an Account of all the Apparences in Looking-glasses, Convex, Concave, Cylindrical, &c.

In the third (the Dioptricks, that consider Rays Refracted) he treats largely of Telescopes of all sorts, Spherical, Elliptical, Hyperbolical; as also of Microscopes, and the effects of all of them. Where among many other particulars, he delivers and commends, as an invention of Eustachio Divini, the way of furnishing a Telescope with two Eye-Glasses, outwardly flat, and inwardly convex, so as that they touch one another in the center of their convex superficies.

In this part he explicates the Doctrine of Refractions and Parallaxes; annexing several Particulars concerning Comets, the Ring of Saturn, &c. and concluding all with an Appendix, wherein having refuted the Spiral Hypothesis, devi'd to support he Ptolemaick syste'me of the world, he advanceh a new one, judged by him very suitable to render an account of the Motion of the Celestial Bodies in the same syste'me that supposeth the Earths immobility, which he seems unwilling to desert.

III. DE VI PERCUSSIONIS, JOH. ALPHONS. BORELLI. Bonon-nia in 4. 1667.
Whereas in the doctrine of Percussion several things are to be accurately distinguished, as the Force percussive, the Motion, or the Velocity of the Percussion, and the Resistance of the Body percussed; and then an Estimate to be made of the Proportion of those three to one another: This Author pretends to have both assigned that difference, and demonstrated the Proportion; Adding, that though Galileo's Law and acknowledged (vid. at the End of his fourth Dialogue De Motu Projectorum) That the Force of Percussion was Infinite, or (rather) unlimited, yet he there referred discoursing upon that Argument to another opportunity; which not having been performed by him (for ought could be found by any of his writings, either Printed or Manuscript, which latter were purposely searched after his death to find such a discourse) our Author pretends, that that Proposition concerning the Infiniteness of the force of Percussion, not having been yet demonstrated by any, he hath in this Book resumed the whole matter concerning Percussion, and clearly demonstrated the true and genuine Nature of it, its Cause, Properties and Effects. In the doing of which, he taketh occasion to discourse also of Gravity, Magnetisme, Tremor of Bodies, Pendulums, &c. All which, whilst the Reader is considering, the Author tells him, that he is making ready his other Books concerning the Motions of Animals.

IV. NIC. STÉNONIS MUSCULI DESCRIPTIO GEOMETRICA, Florentiae in 40. An. 1667.

The Author of this Book declareth, that his design in composing it was, to shew, that in a Muscles neither the Parts of it can be distinctly named, nor its Motion duly considered, unless the Doctrine thereof become a part of the Mathematicks. And he is of opinion, that there is no other cause of the many Errors, which spoil the History concerning the Humane Body, than that Anatomy hath hitherto disdain'd the Laws of the Mathematicks. And therefore inviteth those that are studious in that part of Philosophy, to consider, that our Body is an Engine made up of a thousand subordinate Engines, whose true knowledge whoever thinks that it can be invetsigated without Mathematical assistance, must also think, that there is matter without Extension, and Body without Figure.

Hereupon he shews, that the very Fabric of the Muscles imposeth a kind of necessity upon considering Writers to explicate them Mathematically: In conformity whereunto he pretends to have found, that in every Muscle there is One Parallelepiped of Flesh, and Two Tetragonal Prisms of Tendons; defining a Muscle to be a Body composed of divers series of ranks of Fibres equal, like, and parallel among themselves, and immediately placed upon one another, that whole ranks are congruous to whole ranks. Here he explains the Dimensions of a Muscle, its Contraction, and Strength, and adds that the Use of this new discovery of the Structure of the Muscles, is, to demonstrate, That they may swell in their Contraction without the Accession of new matter.

He subjoins a Letter to Monsieur Thevenot, in which, among other things, he alludes several Experiments, to shew, that the Motion of the Heart is like the Motion of Muscles; and answers those, who pretend that the true Fabrick of the Heart hath already been
been observed heretofore; and those likewise, who think that these new Observations of the Muscles are uncertain; concluding this Subject with an Enumeration of the particulars, yet remaining to be search'd into, in the History of the Muscles.

To all these things he adds Two Narratives; One of a disjunct Head of a Shark, which he calls Canis Carpathia; where he delivers many curious Observations of the Skin, Eye, optic Nerves, Ocular Muscles, exceeding lomaldis of the Brain, as also of the Mouth, and strange Teeth of this Fish; examining withal, whether the Gloio-

peta be the Teeth of this Creature, or Stones produced by the Earth; in which controversy he takes their part, who maintain that those and divers other substances, found in the Earth, are parts of the Bodies of Animals, and endeavours to prove, that such sorts of Earth may be the sediments of Water, and such Bodies, the parts of Animals carried down together with those sediments, and in progress of time reduced to a stony hardness. *

This Subject Mr. Hook hath also discoursed of at large in several of his publick Lectures, founded by Sir John Cutler; which Lectures he read about Two years since in Gresham College, in the presence of many Learned and Curious persons; which also had been long since made publick, had not other indisensible affairs hindered him from taking care of the Press: where he hath not only shewn the Origin of these Gloiopectra, but of all other curiously figured Stones and Minerals, together with that of Mountains, Lakes, Islands, &c. though from a somewhat differing Hypothesis, of which the curious may shortly receive a further Account.

The other Narrative is of a Female Dog-Fish, disjunct also by himself, where do occur no less remarkable observations, than in the former, both of the parts in the Head, and of those in the Body; as touching the small weight of the Brain of this Fish, compared to the weight of its Body; several little Fishes found in the Stomach, untouched by any Teeth; the Ureters, the Ovarium, and Oviductus, where he digresses, to shew, Multi-

lum testes esse Ovaria analogos, and refers, for further proof of this to his intended Treatise, which is to give an account de partibus Genitalium Analogia.

An Advertisement.

The Publisher hereof gives notice, that a Brief Index for the Transactions of this last year, beginning at Numb. 23. in March 1667, shall be Print-
ed a part, for the use of such as desire to have all those Numbers to-
gether.

ERRATA.

What the Printer for want of room, did omit hitherto in the giving notice of an Error committed by him in Numb. 29. the Reader is now de
dev to observe here, viz. That in the said Numb. for want of Marks proper to express Multiplication, there was used pag. 571. l. 5.7. the mark of plus or addition; which yet is thought could hardly occasion any mistake in the Intel-
digent Readers, who might easily see the meaning of the Author by the lines 8. 9. 10. of the next precedent page 570.

In the S A V O R,

Printed by T. N. for John Martyn, Printer to the Royal Society, and are to be sold at the Bell a little without Temple-Bar, 1667.