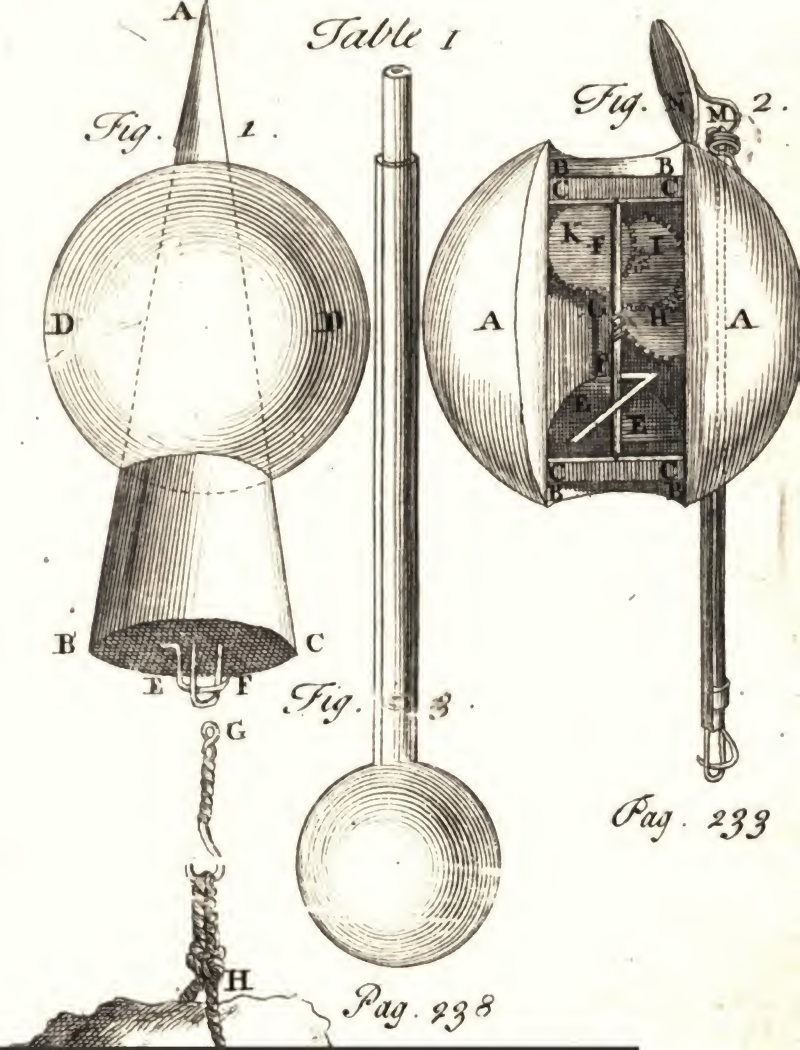

Philosophical experiments and observations of the late eminent Dr. ...

Robert Hooke

Philoſophical

EXPERIMENTS

AND

OBSERVATIONS

Of the late Eminent

Dr. *ROBERT HOOKE*,

S. R. S.

And Geom. Prof. *Greſh.*

AND

Other Eminent VIRTUOSO's in his Time.

With COPPER PLATES.

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MDCCXXVI.

TO

The Right Honourable

JULIANA,

COUNTESS-DOWAGER

OF

BURLINGTON,

This Collection of PAPERS,

As well for her Perſonal Virtues and Merits, as for her ſingular Favours to me, are, with greateſt Reſpect and Gratitude, humbly dedicated by

Her Ladyſhip's

Moſt obliged

Humble Servant,

W. DERHAM.

AMENDMENTS.

PAG. 153. Insert in the Margin at l. 14. *V. Post Works.* p. 564. P. 226. l. *ult.* after *loose*, add *in the same manner.* P. 227. l. 4. for *Tab.* III. read I. Ib. l. 27. read *Weight* K K. Ib. l. 29, 30. r. *Hook* E F. G *the Ring to be hung on the Hook* F. P. 228. l. 31. for *believed*, r. *received.* P. 230. *ult.* r. *Height.* P. 231. l. 25, 26. r. *Height.* P. 233. l. *antepen.* r. *Tab.* I. P. 234. l. 17. r. *these.* Ib. l. *penult.* after left add, *and by that Means.* P. 237. l. 3. after Stick add, *smaller, and tapering upwards towards great* D, *which is an hollow very light Ball of Wood.* Ib. l. 26. read *Tab.* II. P. 238. l. *penult.* r. *Tab.* I. P. 239. l. 12. r. *Tab.* I. Ib. l. 20. r. *proportion.* P. 240. l. 4. for *so*, r. *by.* Ib. l. 18. after or add *that.* P. 321. l. 28. r. *Condore.* P. 328. l. 16. r. *abiegno.* P. 336. l. 17. r. *Lignum-Aloes, Civet, Storax & Ladanum.* P. 245. l. 5. read *mea.*

TO THE

READER.

THE principal Author of theſe Papers being a Perſon of great Repute, I thought the Publication of them would be very acceptable to the Curious; and therefore was willing to undertake the Work, although I found it would be very laborious, by reaſon the Papers were very numerous, and in great Confuſion.

After Dr. Hook's *Death, both his Papers, and ſome of his Figures and Modules (but I fear not nearly all) fell into the Hands of my ingenious Friend* Richard Waller, *Eſq; out of which he ſelected thoſe that he publiſhed in* 1705; *and intended others for the Preſs: But dying before he had accompliſhed that Deſign, a Part of the Papers were entruſted*

truſted to me, by Mr. Waller's *Lady, and* Jonathan Blackwell, *Eſq; In which I expected great Matters from ſuch illuſtrious Names, as I found among them: But when I came to peruſe, and examine them, I found only here and there ſome, that anſwered my Expectation; which the Reader hath in the following Collection. In which he may probably expect ſome of the many* Lectures, *which the Doctor read in* Greſham College, *and thoſe of Sir* John Cutler's *Inſtitution. But the beſt of theſe Dr.* Hook *himſelf, or Mr.* Waller *publiſhed: So that what I have in my Hands, will be of little Uſe to the learned World, moſt of them ſeem to have been intended by the Doctor, for half an Hour's Amuſement to a ſmall Auditory, rather than for the Preſs.*

As for Order, or Method, little could be obſerved in ſuch a confuſed Variety of Subjects, as theſe Papers contain. And therefore the beſt I could do, was to rank them, as near as I could, according to the Order of the Time in which they were written, or communicated.

And as for other Papers interſperſed with Dr. Hook's, *they are, for the moſt part, of ſuch conſiderable Perſons, that the Reader will expect no Excuſe for my inſerting of them.*

But if any remarkable Obſcurities or Imperfections ſhould be met with, it is what I could not help, by reaſon ſome of the Papers were

were torn, ſome obliterated, ſome written in an Hand ſcarce legible, &c. and I was not minded to give my own Senſe, leſt it ſhould be thought that I had impoſed my own, inſtead of the ſeveral ingenious Authors Senſes.

But after all, many of thoſe Imperfections, and Obſcurities, are owing to the Miſcarriage of ſome of the Papers, which either never came to Mr. Waller's *Hands; or, if they did, were loſt, or miſlaid, before they came to mine, the Papers being put into different Hands, after Mr.* Waller's *Death. And whereas* Figures, *or* Modules, *would have explained divers of the Papers, that are publiſhed, and have enabled me to have imparted others, altogether as valuable; but finding few, or none, but what are here publiſhed, neither among the Papers themſelves, nor in the Repoſitory, nor Papers of the Royal Society, I was forced to be content.*

For a Concluſion of this Preface, I ſhall anſwer two Accuſations that have been, or may be charged upon me: One is, That I have long detained theſe Papers from the Publick: The other, that I have engaged myſelf in Matters lying out of my Way. To both which, one Anſwer may ſerve, namely, That I have made the collecting, and publiſhing theſe Papers, my Diverſion, at Leiſure Hours: By which Means, and by reaſon the Papers, out of which theſe were

were ſelected, were very numerous, and many of them came late to my Hands, their Publication hath been the longer delay'd. And as for the Diverſity of this from the Buſineſs of my Profeſſion: I confeſs it is not direct Divinity, but yet I think it, by no Means, unfit for a Clergy-man's Diverſion. For as it is neceſſary for a Clergy-man (as well as others) ſometimes to divert, and unbend his Mind, from his more ſerious Studies, ſo what Diverſion more innocent, or proper, than that which promotes Knowledge, and Experience, and is a Diſcovery (if never ſo ſmall) of any of the Works of the infinite Creator? To the promoting which End, the Publication of theſe Papers was, in ſome Meaſure, intended by

W. DERHAM.

CURIOUS PHILOSOPHICAL *Observations* and *Experiments* OF Dr. *ROBERT HOOK*, AND Other Eminent VIRTUOSO's in his Time.

Of the Invention of the BAROMETER, *in the Year* 1659.

IN one of Dr. *Hook's* Papers (not here publiſhed, becauſe imperfect) I find this Remark, *viz. The Inſtrument, for finding the different Preſſure of Air upon the Parts of the Earth ſubjacent, was firſt obſerved by the Honourable Mr.* Boyle, *who, upon the Suggeſtion of Sir* Chriſtopher Wren, *erecting a Tube of Glaſs ſo filled with* Mercury, *as is now*

uſually done in the common Barometer, in order to find out, whether the Preſſure of the Moon, according to the Carteſian *Hypotheſis, did affect the Air; inſtead of finding the Fluctuation which might cauſe the Phenomena of the Tides, diſcovered the Variation of its Preſſure to proceed from differing Cauſes, and at different Times, from what that Hypotheſis would have predicted. That Propriety of the Air (for ought appears) was never diſcovered till that Time, which is not yet thirty Years ſince,* &c.

To this I *W. D.* ſhall add another Remark I find in the Minutes of the *Royal Society*, *February* 20. 167½, *viz.* Upon a Diſcourſe of ſome Experiments to be made with the Barometer on the Monument, *it was queried, how this Experiment of the differing Preſſure of the Atmoſphere came at firſt to be thought of? And it was related, That it was firſt propounded by Sir* Chriſtopher Wren, *in order to examine Monſieur* des Cartes's *Hypotheſis, Whether the paſſing by of the Body of the Moon did preſs upon the Air, and conſequently alſo upon the Body of the Water. And that the firſt Trial thereof was made at Mr.* Boyle's *Chamber in* Oxford.

The Time, when theſe Obſervations were made, was about the Year 1658, or 59; at which Time Mr. *Boyle* having a Barometer fixed up, for the obſerving the Moon's Influence upon the Waters, happened to diſcover the uſe of it in relation to the Weather, and to aſſure himſelf, that it was the Gravitation of the Atmoſphere which kept up the Quickſilver to ſuch an Height, as the learned Abroad, particularly *Torricelli*, had ſuſpected before.

But although this Uſe of the Baroſcope is owing to Sir *Chriſtopher Wren*, and Mr. *Boyle*, yet, to do every Man Juſtice, I ſhall give

give the Hiſtory of this excellent Inſtrument, from the Extracts of a very ingenious Friend.

THE firſt Inventor of it was *Torricelli*, at *Florence*, in 1643. From whence Father *Merſenne* brought it into *France* the Year following, 1644. And Monſieur *Paſcal* being informed of it by Monſieur *Petit*, the Engineer, they both tried it in 1646, at *Rouen*, with the ſame Succeſs as it had been tried in *Italy*. Some Time after which, an Experiment was made with a Tube of forty ſix Feet, filled with Water, and alſo with Wine: Which Experiment Monſieur *Paſcal* gave an Account of in a Piece printed in 1647; in which Year he was informed of *Torricelli*'s Solution of the Phenomenon, by the Weight of the Air; and deviſed, for the examining it, the famous Experiment with two Tubes, one within the other; which he mentions in a Letter written in *November* 1647. And laſtly, in 1648 the ſame Monſieur *Paſcal* made his Experiments on the Tops and Bottoms of Hills, Buildings, &c. which laſt Experiments Monſieur *Des Cartes* laid Claim to; affirming, that he deſired Monſieur *Paſcal* to make them two Years before, and predicted their Succeſs, contrary to Monſieur *Paſcal*'s Sentiments.

Monſieur AZOUT alſo laid the ſame Claim, but it is the moſt probable that Monſieur *Paſcal* had the beſt Title.

THIS Experiment which *Torricelli* made with Quickſilver, *Galileo* had in effect tried with Water in long Tubes by Pumping; with which he found he could never get the Water to aſcend above thirty three Feet: But the Cauſe he could never hit of.

AFTER the *Torricellian Experiment* had been much celebrated in divers Places, at laſt *Otto de Guerrick*, Conſul of *Magdeburgh*, was informed

formed of it by Father *Valerian* at *Ratisbon*, who claimed it as his own Invention: But this was not till the Year 1654. After which *Guerich*'s Experiment (called the *Magdeburgh Experiment*) was much talked of.

From this short History of the Barometer, not only the Inventor and Improvers of it appear, but in some Measure also the excellent Uses of it: Particularly the Gravitation of the incumbent Atmosphere, (one of the noblest philosophical Discoveries) the Changes of the Weather, *&c.*

W. Derham.

The Lord Kingkardine's *Observations of the Pendulum Clocks at Sea, in* 1662.

The Lord *Kingkardine* did resolve to make some Trial what might be done, by carrying a *Pendulum* Clock to Sea; for which End, he contrived to make the Watch Part to be moved by a Spring instead of a Weight; and then making the Case of the Clock very heavy with Lead, he suspended it, underneath the Deck of the Ship, by a Ball and Socket of Brass, making the *Pendulum* but short; namely, to vibrate half Seconds, and that he might be the better inabled to judge of the Effect of it, he caused two of the same Kind of *Pendulum* Clocks to be made, and suspended them both pretty near the middle of the Vessel, underneath the Deck; thus done, having first adjusted them to go equal to one another, and pretty near to the true Time; he caused them first to move parallel to one another, that is, in the Plane of the Length of the Ship, and afterwards he turned one to move in a Plane at Right Angles

Angles with the former; and in both these Cases it was found by Trials made at Sea, at which I (*i.e.* Dr. *Hook*) was present, that they would vary from one another, though not very much, sometimes one gaining and sometimes the other, and both of them from the true Time, but yet not so much but that we judged they might be of very good Use at Sea, if some farther Contrivances about them were thought upon, and put in Practice. This first Trial was made in the Year 1662; whereupon, these being found to be able to continue their Motion without stopping, several other Clocks of this Nature were made and sent to Sea, by such as should make farther Experiment of their Use. And we have an Account which was given from Sir *R. Holmes*, who tried them in sailing from St. *Thomas* West-ward about 800 Leagues, and then tacking about steer'd about 300 Leagues N. N. E. towards the Coast of *Africa*, and by observing these Clocks only, he was able to judge much better than the Masters of the other Vessels that were in Company, who differed from his Account, some 80, some 100 Leagues, some more Leagues; and whereas several of them thought themselves near to *Barbadoes*, he judged by his Clocks that he was not far from *Fuego*, one of the Islands of *Cape Verde*, and the next Day by Noon reached that Island. But yet this was not so exact as was expected; however, it performed somewhat towards this Effect of finding Longitudes somewhat more than ordinary, and enough at least to give inquisitive Men Occasion to speculate, and make farther Trial. And though there hath been no very considerable Improvement of that Instrument, or Experiment since that Time by any, and tho' I fear it may at best be insufficient to perform what is necessary to this Matter, yet I question not but that there may be some other Way that

may perform it to a much greater Degree of Perfection, as I ſhall hereafter endeavour to prove.

Dr. HOOK's *Experiment of weighing* AIR. *Shewed to the* Royal Society, *Dec.* 3. 1662.

TWO ſmall Glaſs Balls, blown and ſealed with a Lamp, each of them about an Inch and half over, were ſuſpended at the End of a Beam, and counterpoiſed with a ſmall leaden Weight; and then a Grain being taken away from the Counterpoiſe, ſo that the Balls preponderated by a Grain, the Beam was hung into the Globe, and the Mouth of it clos'd, and the Forcer was wrought; whereupon, as the Air was condenſed in the Globe, the Balls by Degrees grew lighter and lighter, and the oppoſite Counterpoiſe at length did more preponderate the Globes, than they had before the Condenſation; but upon the letting out of the impriſon'd Air, the Balls again recovered their Prepollency, and remained as they were when firſt put in.

THE Experiment affords us a manifeſt Proof of the Weight and Spring of the Air, and after what Manner they work upon the Bodies incloſed in it. *1ſt.* That though the Air be a heavy Body, yet it not only preſſes downwards, as ſome have erroneouſly thought, and ſo have imagin'd it ſhould break People's Necks, and roul and preſs down the Graſs, and all kinds of weak Plants, as *Deuſingius* ſuppoſes; or ſhould preſs a Diſh of Butter, or ſome ſuch ſoft Body, quite flat, as Mr. *Hobbs* imagines. But *2dly*, it preſſes upwards and ſideways, as much as downwards; whence every Body, ſuſpended in it, does ſuffer, from this ambient Fluid, a greater

Preſſure

Preſſure againſt its under Side to thruſt it upwards, than againſt its upper Side, to force it downwards; and does in all Things of Staticks act according to the ſame Laws, and after the ſame Manner, that other heavy fluid Bodies work upon the Body they incompaſs. And this Experiment, in ſhort, is nothing elſe but a Variation of *Archimedes*'s Experiment of examining compounded Metals. For the two Bodies that weigh againſt each other, being of a very differing Bulk, though pretty near of the ſame Gravity when in the Air, when they are incompaſs'd with a more denſe and heavy Fluid, that which is more bulky muſt neceſſarily loſe more of its Weight or Power downwards than the other, ſince it is a known Law of the Staticks, that a Body, remov'd out of a lighter into a heavier Medium, loſes ſo much of its former Gravitation, as the Weight of a Part of the heavier Fluid, equal in Bulk to the incloſed Body, amounts to.

THE Uſes that may be made of this Experiment may, be many, and thoſe, I think, not the leaſt conſiderable.

Firſt, IT may ſerve as an Inſtance, to ſhew by what Means the Vapours and Exhalations are raiſed up into the higher Parts of the Air; for if by any Means the Vapours, or Waters rarify'd, obtain a greater Rarity, and conſequently a leſſer Gravitation than the ambient Air; the Preſſure of that muſt neceſſarily buoy and carry them up ſo far, till the Abatement of Preſſure on the Parts of the ambient Air, by reaſon of their ſublime Stations in the upper Regions, and till the Abatement of Heat, that kept the Vapours rarify'd, has reduc'd both to an Æquilibrium, where they are ſtay'd and ſuſpended; which affords us a ſecond Uſe, namely, to explain how the Clouds or Exhalations are ſuſpended and carried to and fro directly at ſuch a Height, and no lower nor higher. For ſince

it is found by Experiments made by *Torricellius*, that ſeveral others, whom I now forbear to name, and the Preſſure of the Air at the Top of Mountains is differing from what it is in the Valleys, therefore the Rings of Preſſure (if I may ſo call thoſe Parts of the incumbent preſſing Atmoſphere) ſeem not at all to be regulated by the Form of the Earth's Surface; that is, are not at all parallel to the Surface of the Earth, but they ſeem to be regulated rather by the Diſtance of the Parts of the Air from the Center of the Earth, or rather are parallel to the Surface (if there be any) of the Air, or to the Superficies of the Sea. And, indeed, I have very often obſerved, not without Wonder, that in cloudy Weather all the under Surfaces of the Clouds have been exactly terminated with a Spherical Concave Surface, no one being raiſed above or depreſs'd below ſuch a determinate Surface. And I have after obſerved the Vapours often riſe like Smoak upward, till they come to ſuch a Height, and then to ceaſe aſcending, and ſpread themſelves in Breadth almoſt like Oil upon the Water: The Reaſon of all which is, probably, nothing elſe but that at ſuch a Height the Air is reduc'd by the Decreaſe of Preſſure to ſuch a Degree of Rarity, that it is unable to raiſe the Vapours any higher, and below it is able to raiſe them. The Reaſons how the Vapours come to retain that Degree of Rarity, *&c.* is an Enquiry more proper for another Place.

Thirdly, THIS may hint us a Solution of a late Obſervation made by an excellent Perſon, and a Member of this Society, that in Fogs with an Eaſterly Wind, the Preſſure of the Atmoſphere was obſerved to be very great. The Reaſon of which Phenomenon might, perhaps, be this, that the Cold and Preſſure of the Air being then very great, the Denſity and Gravity of it might thereby

by become ſo conſiderable, as to raiſe up many Bodies, even in the Form of Water, and keep them ſuſpended ſomewhat above the Surface of the Earth, though by reaſon of the Want of Heat to rarify thoſe ſmall Parts into aerial Vapours, it were not able to carry them to any conſiderable Height.

A Brief Account of the Experiments tried before the Royal Society, *with* Glaſs Balls, *November* 19. 1662. 1. *Of driving out the Air by bare Heat.* 2. *Of driving it out by Vapours of Water and Spirit of Wine.* 3. *Of their breaking of themſelves.* 4. *Of their breaking by a Knock.* 5. *Of the Quantity of Water they admitted.* 6. *Of the Weight of Air they admitted.* 7. *Of the ſhrinking and ſtretching of them.* 8. *Of their breaking outward.*

A SMALL Pipe of white Glaſs, melted over a Lamp, is blown into a pretty large Bubble, the ſmall Neck or Pipe of which being, whilſt the Ball is yet red-hot, ſuddenly and carefully ſealed up, I obſerved that thoſe Bubbles being left to cool, ſome of them that were either not very equally or over thin blown, would, in the cooling, break inward, with a very brisk and loud Noiſe, ſome ſooner whilſt yet hot, others later when even quite cold; but this latter yielded the loudeſt Report. Some, that were ſtrong and even blown, remained intire when quite cold: The Balls of which I obſerved to endure a much greater and more violent Blow, before they would break,

break, than others much of the ſame Make, which were left to cool without ſealing up. But, when by a pretty brisk Blow they were broken, they yielded, beſides the Noiſe of the broken Pieces, ſometimes a ſmart, ſometimes a more faint Noiſe. Some of theſe Bubbles whilſt thus hermetically ſeal'd, being pois'd in a pair of exact Scales, and then the little ſeal'd End nipp'd off, a *Sibilus* or hiſſing Noiſe might very ſenſibly be heard for a ſmall Space of about a Second; after which the ſame Scales and Counterpoiſe being left free, the Bubbles were always obſerved to preponderate, ſome a $\frac{1}{4}$ of a Grain, others $\frac{1}{3}$, others more. The End of ſome other of theſe being broken off under the Water, the Water was obſerved to aſcend with a very great Impetuoſity, and to look white, until ſuch Time as it had fill'd the Bubble or Ball, about $\frac{3}{5}$ or $\frac{3}{4}$ of the whole; ſome more, ſome leſs, according as they were more or leſs hot when ſeal'd up. Then holding the Bubble over the Flame of a Candle, till the Water was boil'd or exhal'd away, I immediately ſeal'd up the ſmall End again, and obſerved ſome of them to break with a much louder Crack than thoſe that had been ſealed up when red-hot. Breaking others under Water, I found a much greater Quantity of Water to enter, inſomuch as to fill almoſt the whole Ball, leaving a very little Bubble of Air at the Top: Others, that I weighed, I found to increaſe ſomewhat more in Weight, by the Admiſſion of the Air, than they had done before by the other ſealing. After this, having emptied out the Water, I put into ſeveral of them a ſmall Quantity of indifferently well rectify'd Spirit of Wine, and taking the ſmall Stem in my Hand, I held the Ball over the Flame of the Lamp, till the Spirit with great Impetuoſity was evaporated, and driven out through the ſmall Neck, in a Kind of miſty Steam;

Steam; which ceaſing, I immediately ſeal'd up the Neck, and letting the Bubbles cool, I found them to be much of the ſame Kind with thoſe that I had ſeal'd up with Water, both as to the Noiſe they yielded when broke, and to the admitting of Water, and for the weighing of Air; only in this theſe two laſt Ways differ'd from the firſt, that whereas the red-hot Glaſſes when cold were clear, theſe, though they appeared clear when hot, were, notwithſtanding, all tarniſhed over, with a Kind of Dew in the Inſides when cold; which Dew would quickly diſappear, if they were again heated pretty hot. There were ſeveral other Circumſtances, which, becauſe they will be more notable in other Experiments, I here omit.

THE Reaſons of which Phenomena I humbly conceive to be theſe. *Firſt*, That the elaſtical Power of the exceedingly heated Parts of the Air, that are within the Glaſs when red-hot, being very much intended, a very ſmall Parcel is able to preſs and keep out all the reſt of the ambient, contending Atmoſphere; and whilſt it has that Ability, the Paſſage being ſhut, the ambient Air is hinder'd from ruſhing in that Way, though the Air within growing colder, and ſo loſing its Elater, could not have been able to have hindered it. * Now the Preſſure of the included Air againſt the Sides decreaſing with its Elater, and that with the Heat, and the Preſſure of the ambient, remaining the ſame, that curious arched Vault of the Glaſs is forcibly preſs'd and cruſh'd together, and ſo the Particles are put into a cloſer Texture. And that they are ſo, I found by this Experiment. I fitted a pretty large Bubble with a ſlender Neck into a Bolt-Head, whoſe Neck was drawn very

* Query, *Whether the Bubbles ſhrink?*

ſmall

ſmall, and left only big enough to contain the Neck of the Bubble, and whoſe Bottom was cut off, that thereby I might include the Ball. Having ſo fitted the Ball and Bolt-Head, I ſhut up the Bottom again with Cement, and filling up the Space left in the Bolt-Head with Water, till it reach'd into the Small of the Neck, I nipp'd off the ſeal'd Top of the Bubble, whereupon the Water in the ſmall Neck roſe about a Barley Corn's Breadth, which could proceed from nothing elſe than its Return to its former Dimenſions, before it was ſealed up; which affords us a noble Inſtance of Compreſſion, where that ſo hard and well compacted Body of Glaſs is compreſs'd into leſſer Room, and that by no greater a Force than that of the Preſſure of the Air; whence we may conclude that the Parts of that Body are not ſo cloſe joined together, but that there may be Pores or Receſſes left between them, into which they may be protruded, and ſo be made to lie cloſer to each other, which whether Water and other fluid Bodies may not do the like, Trial will inform.

THE Experiments ſuggeſt theſe Queries.

WHAT may be the Cauſe of Noiſe or Sound?

BY what Means Heat rarifies and expands Bodies, and Cold condenſes?

WHETHER the Cauſes of the almoſt ſimilar Phenomena of the Glaſs Drops, may not be deduced from theſe Principles: Or what may be their Cauſes?

THE Strength of a Knock, or what may be the Force of falling Bodies?

WHAT is the true Weight of Air in Winter?

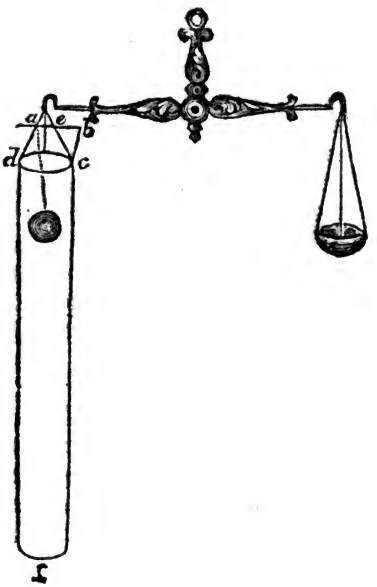
WHETHER Bodies, that will not melt, may be expanded by Heat?

THE Difference of the external and internal Preſſure increaſing by the Decreaſe of the included Air's Elater, if ſome Parts of this Arch (if I may

ſo call it) be weaker or irregular, the ambient Preſſure breaks it in: Even as in Architecture the ſame would happen in thoſe larger Arches, if in either of theſe Particulars they deviated from the Rules of that Art. But if ſufficiently ſtrong and equal, the ambient Preſſure makes the chryſtalline Vault the firmer, as in Arches of Stone is commonly obſerved. The Cauſe of the Noiſe I dare not yet determine, but I think it worth a further Enquiry, whether it proceed not from the *Impetus* wherewith the broken Pieces of Glaſs are daſhed againſt one another, though the Noiſe ſeem of another Kind; or from the ſudden ruſhing of all the Parts of the ambient Air towards the Middle of the Ball, whereby all the other Parts of the circumambient being likewiſe moved towards the ſame Middle, the Drum of the Ear may likewiſe be moved, and ſo a Sound heard: Or 3*dly*, Which I think the moſt plauſible, from the ſudden and violent ruſhing towards the Center, and (by there meeting each other, or at leaſt the broken Particles of Glaſs) there finding as ſudden and violent a Recoil or Repulſe, one of which two laſt (if not a third, namely, the ſudden flying out of the Air) ſeems to be the Reaſon of the Noiſe of a diſcharged Shot of Powder. The Alteration, as to Weight, does clearly enough proceed from the Admiſſion (which the Hiſſing plainly enough ſpeaks) of the heavy Particles of Air. A manifeſt Experiment that Air does gravitate in Air. The violent ruſhing in of the Water argues the forceable Preſſure of the external, as the Multitude of Bubbles do the languid Reſiſtance of the included Air.

An Account of ſome Trials for the finding how much, aſcending and deſcending Bodies preſs upon the Medium through which they paſs : Made before the Royal Society, *Dec.* 24. *and Dec.* 31. 1662.

A Glaſs Tube about fourteen Inches long, and an Inch and half over, being open above, but ſhut beneath, was hung by a Piece of Tape faſtened about the End of it, to the End of a Beam ; then being fill'd with Water, and a



round Glaſs Ball ſomewhat more than an Inch in Diameter (which was made heavier than Water, by Quickſilver included in it) being hung by a String

String of Silk ſo far within the Tube, that it was quite covered with Water. The other End of this String was tied to a Wire, that was faſtened to the End of the Tube. This Tube, I ſay, thus accoutred, being hung at the End of an exact Beam, was counterpois'd with ſomewhat more than 36 Ounces Troy. Then the Scales being in a very exact Equilibrium, the Silk String, by which the Ball hung, was ſuddenly cut aſunder with a ſharp Pair of Sciſſers. And the Beam, all the while the Ball was deſcending through the Water, and after it came to the Bottom, kept its former horizontal Paralleliſm. This was repeated a ſecond Time with the like Succeſs.

AT the ſame Time in the ſame Tube, as it hung in this Poſture, there was let down to the Bottom of it a ſmall Piece of Lead, which had a ſmall Loop of Wire, through which a Silk String being put, a round Glaſs Ball much lighter than Water, and about the former's Bigneſs, was, by that String, drawn down, and kept at the Bottom of the Water, and the other End of the String was faſtened about the former Wire. This done, the Scales were brought to an Equilibrium, and then, as before, the Thread was cut, and the Ball quickly aſcended to the Top; in which Time the Beam was obſerv'd to be very much turned from its Equilibrium, and upon Trial ſix Grains, detracted from the Counterpoiſe, was requiſite to bring them to an Equilibrium. This laſt Experiment was twice repeated, but in the latter Trial the Paralleliſm of the Scales was not at all diſturb'd, as in the former Experiment; which gave Occaſion for a Conjecture, that the former odd Phenomenon was cauſed by ſome extraordinary Accident.

IN Proſecution of this Enquiry, *Dec.* 31. Trial was made by a Variation of the former Experiment;

riment; for the Thread of Silk that the Ball hung by, was not tied to the former Wire, but to a *Suſtentaculum* above the Beam; then the Scales being brought to an Equilibrium, and the String cut as before, the deſcending Ball made that End of the Beam, to which the Tube hung, to be exceedingly depreſs'd, and being come to the Bottom it kept the Beam in that Poſture.

FURTHER, that it might be known how much heavier that End was than the other, whilſt the Ball lay at the Bottom, the Beam was brought to an Equilibrium; after which, ſix Grains were taken from the Counterpoiſe of Weights. Then the Ball being tied by a String as before, and the Scale wherein the Weights hung being kept up to a convenient Height, that the Beam might hang parallel to the Horizon, and the String cut as before, the deſcending Ball was obſerved manifeſtly to depreſs the Tube End. Trial was made a third Time by counterpoiſing and ordering all Things, as in this ſecond Trial, and detracting only three Grains, notwithſtanding which, the deſcending Ball manifeſtly depreſs'd the Tube End; which laſt Trials were a Confirmation of the firſt Experiment, when the Ball was hung to the Wire.

THESE Experiments ſeem to hint this Axiom, That every Body, whether aſcending or deſcending in a fluid Body, does add ſo much Weight or Preſſure to that fluid Body, as its own Weight amounts to, and not as much as the Weight of ſo much of the Fluid as is equal in Bulk to what the moved Bodies amounts to.

THIS I ſhould have put as an Axiom, did not ſome Difficulties ſuſpend my Aſſent.

Firſt, SINCE the ſwifter a Body is moved, the greater Reſiſtance it finds from the Medium through which it paſſes, and conſequently the ſtronger is its Preſſure againſt that Fluid; and ſince deſcend-

deſcending Bodies grow ſwifter in their Motion, the lower they deſcend, it ſeems rational to judge, that the deſcending Ball's Preſſure, on the Water, ſhould be increaſed with its Swiftneſs.

NEXT, ſince the Body that hinders its Motion is a Fluid, it ſeems ſomewhat difficult to conceive, how the Preſſure of a deſcending Body can be communicated to the Bottom, ſince the Parts of the Fluid are circulated. And no leſs difficult is it to ſay, on what Part of the Bottom the Preſſure reſts; whether on the whole, or only that Part immediately ſubjacent to the falling Ball; for which Way ſoever is taken, there are ſeveral Difficulties ſomewhat hard to be explicated.

Thirdly, IF the Weight of the deſcending Body be all the while ſuſtained by the Fluid, and conſequently by the Bottom, how comes the Body, when it touches the Bottom, to preſs with more Force than its own Weight; as is evident, in Bodies deſcending through the Air.

Fourthly, SINCE the Preſſure of a fluid Body, againſt the Bottom, is greater, or leſs, according to the Height of the Surface of the Fluid above it: It ſeems that an aſcending Body, in Water, does manifeſtly contradict this Axiom.

COROLLARIES, deducible from theſe Experiments, certainly made, may be ſuch as theſe:

Firſt, THAT Exhalations and Vapours preſs not leſs upon the Surface of the Terraqueous Globe, when they aſcend, than when they are falling; nay, than when they are fallen: The Certainty of which, I think, were worth examining.

NEXT, That the Preſſure of any contained fluid Body, againſt the Sides of the Veſſel, will be abated by opening an Hole at the Bottom; though the Height of the Water be continued the ſame. That is, that the Preſſure of a Perpendicular

Height of running Water, is not the ſame with that of ſtanding Water.

Thirdly, IT ſhould ſeem, that the Preſſure of a River, againſt the Pillars of a Bridge, is leſs whilſt the Water is running between them, than when that Paſſage is ſtopp'd, though the Height in both remaineth the ſame.

Dr. HOOK's *Enquiries for* Greenland. *Jan.* 14. 166$\frac{1}{2}$.

WHAT, and how much, was the Heat of the Sun in the midſt of Summer, compared with the Heat of it in *England*?

WHAT is the moſt conſtant Weather there, whether clear, cloudy, rainy, miſty, foggy, *&c.*? Or what moſt uſual at ſuch and ſuch Times of the Year? Next, what Conſtancy or Unconſtancy there is of the Winds to this or that Quarter of the Horizon, or this or that Part of the Year? What the Temperature of each particular Wind is obſerved to be; and particularly, whether the North be the coldeſt, if not, what Wind is? What Wind is obſerved to bring moſt Ice, and what to make a clear Water at Sea? What Currents there are, how faſt, and which Way they ſet? Whether thoſe Currents are not ſtronger at one Time of the Moon than another, whether always running one Way? What is obſervable about the Tides, Spring or Neap? Whether the Sea Ice be ſalt or freſh? What Rivers there are in the Summer? What Fowl are found to live there, and what Beaſts; how they are imagined to ſubſiſt in the Winter; how they breed and feed their young? What Vegetables grow there, and whether they yield any Fruits? How deep the Cold penetrates into the Earth? Whether there be any Wells, or deep Pits, or Mines, wherein the Water will remain unfrozen

unfrozen at the Bottom? How the Land trends? And whether the Parts, under or near the Pole, be there thought to be Sea or Land? Whether the Perſon made any Experiment, about the Loadſtone or magnetical Needle, or any mathematical Obſervations, about the Height of the Sun and Luminaries, or their apparent Diameters, or Refraction, or the like?

WHAT Fiſh moſt frequent thoſe Seas, and any thing about their fiſhing, with the uſual Bigneſs of Whales, *&c.* their Strength, the Anatomy of their Entrails? Whether any People do or have been known to ſtay there all the Winter, and how they do or have ſhifted? How near any has been known to approach the Pole? What Notice he has taken of the Moon, *&c.*

Dr. HOOK's *Enquiries for* Iceland.
Jan. 21. 166⅔.

HOW deep the Ground is frozen?

WHAT Wind is coldeſt?

WHAT Rivers and Springs they have?

THE Anatomy of Whales, or other very large Fiſhes.

ABOUT the Lungs of Whales and Contrivance of Reſpiration in other Fiſhes and Morſes?

CONCERNING the Fountain that is hot enough to ſcald a Fowl.

WHETHER the burning extraordinarily of *Hecla* portend foul Weather?

REFRACTION, whether the ſeven Stars are ſeen in the *Pleiades?* Whether *Mercury* can be oftener ſeen than in *England?* The differing Heat of Summer and Winter: How near the Moon may be ſeen to the Sun?

AN exact Obſervation of the Eclipſes that happen.

The Saltneſs of the Sea-water, by boiling, how much Salt it yields?

The Height of the Quickſilver in the Torricel Experiment.

What Wind blows moſt and oftenest?

The uſual Temperature of the ſeveral Winds there.

About Corruption and Preſervation of Bodies.

What Bodies will keep in the Snow, what not?

The burning of the Mountain, other Obſervations with the Needle in ſeveral Places about *Hecla*, or the other fiery Mountains, and in other Places of that Iſle.

The Figure of Snow, whether Hexangular, whether always larger than in theſe Parts?

The uſual Bigneſs of Hail-Stones and Figure.

What is obſervable about Meteors, as *Ignis Fatuus*, Star-ſhooting, Thunder, and Lightning.

What Kind of Subſtances are caſt out of the burning Mountain.

About Haloes and Rainbows, any thing extraordinary.

What kind of Ores, Stones, Clays, Minerals, *&c.* it yields.

Whether there be any of the *Selenitis*, or *Muſcovy* Glaſs to be found there.

The Declination, Inclination, and Variation of the Magnet in ſeveral Parts of the Iſle, with the Diſtances and Latitudes of thoſe Places, as near as may be.

Whether the ſame Point of a Magnet, that is a Pole of that Stone here in *England*, will be ſo there.

Whether the ſame Part of a *Terrella*, that, put upon Quickſilver, will lie toward the Earth here in *England*, will do ſo there likewiſe.

Whe-

WHETHER the attractive Virtue of the Magnet increaſe or diminiſh there, in reſpect of what it is found here.

WHICH Pole is there ſtrongeſt.

WHETHER Iron be more or leſs apt to ruſt there than here.

WHAT living Creatures, tame and wild, live and thrive there.

ANY thing of that Kind ſtrange or remarkable among the Beaſts, Birds, Inſects, or Fiſhes; as about their Generation, living in the Winter; for what they are or may be made ſerviceable; either for Burthen, Swiftneſs, Furrs, Feathers, Meat, *&c.*

WHAT Kind of Vegetables thrive beſt in that Iſland, as Trees, Shrubs, or Plants, and what Kind of Grounds they thrive beſt in; what Kinds of Vegetables the Sea yields, differing from our *Engliſh*. In what their Husbandry differs from ours, and whatſoever of that Kind is remarkable.

WHAT Woods it yields good for Building, Shipping, or other neceſſary Uſes.

WHAT notable Virtues are attributed to this or t'other Plant; whether for Divination, Phyſick, Dying, Smell or Taſte, *&c.*

THE Seeds of as many as may be gotten together, with their Names.

HOW ſeveral Creatures ſubſiſt in the Winter.

WHAT are the predominant Colours of Animals.

WHAT general Change is made on the Shipmen, that does not ſeem immediately to proceed from Cold, as what Diſeaſes they are moſt ſubject to.

THE Nature, Diſpoſition, Manners, and Cuſtoms of the Natives.

THEIR Apparel for Warmth, Houſing, Victuals, Firing, Bedding, Cookery, and other Obſervables, either Actions or Utenſils, *&c.*

ANY notable Effects produced by Cold, *&c.*

THE Height of the Iſlands of Ice, their Depth; whether it be freſh Water; whether it ſeem to be made up of Snow, and ſeem to lie in Plates one above another.

WHETHER Spirits appear; in what Shapes; what they ſay or do; any thing of that Kind very remarkable and of good Credit.

How much the Celeſtial Bodies are elevated by Refraction above their true Place.

WHAT Currents there are, the Time of the Tides in ſeveral Ports; their great riſing and falling in ſeveral Places; any thing notable concerning them.

WHAT Condition the Body is in that is preſerved by Snow, whether ſhrunk or ſwell'd, or chang'd in Colour or Taſte, *&c.*

WHETHER Quickſilver will congeal.

A BLADDER full of *Engliſh* Air carried thither, and one of that Iſland Air brought back.

Dr.

Dr. HOOK's *Propoſals, for finding out the Reſiſtance of the Air, to Bodies mov'd through it.*

TRYAL ſhould be made with Pendulums of all Sorts, whoſe Weights ſhould be made of ſeveral Sorts of Materials; as of Metal, Stone, Wood, Feathers, Wool, *&c.* and thoſe faſhioned into ſeveral Shapes, as round, elliptical, ſquare, oblong, flat, to move flat-ways and edge-ways, and the like; then to have one common Standard, or Pendulum, by which the Celerity and Duration of all the other are to be meaſured.

TRYALS ſhould be made with ſeveral of theſe Pendulums, in the exhauſted Receiver, where there is a much leſs Quantity of Air; and likewiſe in the Receiver, where the Air is very much condenſed; and the Differences meaſured, as before, and recorded, then compar'd with one another, and then with thoſe in the free Air.

TRYALS ſhould be made with Bodies of ſeveral Subſtances, and each of thoſe of ſeveral Shapes, which ſhould be let fall from ſeveral Heights; and the Times of each of their Deſcents to be exactly meaſured by a Pendulum, and recorded.

TRYAL likewiſe ſhould be made by ſhooting, Horizontally, ſeveral Kinds of Bodies, with a Croſs-Bow, or the like, from the Top of ſome high Place, and ſo obſerving the Time before they touch the Ground. And the

TRYALS ſhould be made by ſhooting Bodies perpendicularly upwards, and ſo obſerving both the Time of their Aſcent and Deſcent.

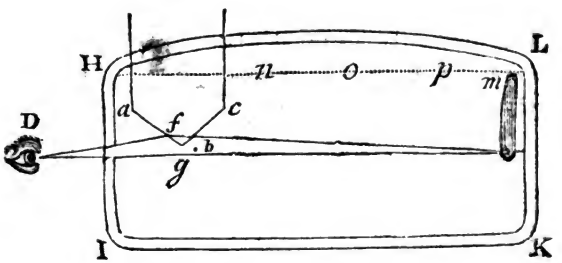
TRYALS likewiſe ſhould be made by ſhooting Bullets, or other Bodies, Horizontally; and ſo to obſerve with what Force they hit a Body, according as the Body is nearer, or further, from

the Inſtrument that ſhoots. And theſe Tryals to be made with Inſtruments of ſeveral Strengths.

Dr. Hook's *Experiment before the* Royal Society, *February* 11. 166⅔. *about the Refraction of* Ice *and* Cryſtal.

Having obſerved it to be almoſt a general Rule in Nature, that of pellucid Bodies, thoſe are found to have greateſt Refraction towards the Perpendicular, which are moſt maſſy and heavy in Bulk, I choſe a very pure and pellucid Fragment of Ice, about an Inch thick, which had very few, if any, perceptible Blebbs or Bubbles in it. Then I took a large cylindrical Cryſtal-Glaſs, about ſix Inches over; and filling it with very fair Water, I put into it this clear Piece of Ice, which did manifeſtly ſwim, with ſeveral of its Parts, above the Water; and though I ſeveral Times depreſs'd it with my Finger, yet would it incontinently riſe, as ſoon as I had remov'd my Finger. Then I took it out, and with a very ſharp edg'd Knife, I ſhaved one End of it, (which is very eaſy to do) into the Form of a very blunt Wedge, ſo that the two Sides of the Edge compos'd an Angle of about ninety Degrees; then ſmoothing thoſe ſhaved Sides, by rubbing them a little with the Palm of my Hand, I put it into the Water with the Edge downwards, and holding it pretty near that Side of the Glaſs, which was next my Eye, I cou'd plainly perceive, by looking through that Edge, that an Object, placed againſt the oppoſite Side, was manifeſtly refracted. For faſtening a ſmall Piece of Lead, ſo that the lower End of it reach'd about an Inch under Water, I could very plainly ſee that lower End, a little below

low the Bottom, when, looking through the Ice, the Bottom of it appear'd above the Edge of the Ice; that is, I ſaw the ſame Object in two Places. Now becauſe the Refraction of the Ice made it appear higher than really it was, it ſhews that the Refraction in the Ice was leſs than Water; which will more plainly appear by the Figure: Where H I K L repreſents the cylindrical Glaſs, that



held the Water; *m e*, a Piece of Lead hung againſt the Side of the Glaſs; *a b c*, the blunt Edge of the Piece of Ice; D, the Eye; *n o p*, the Surface of the Water; *f e*, the refracted Line, in which the Point *e* appeared to the Eye; *g e*, the unrefracted. This I ſeveral Times have repeated, and always found the ſame.

The Uſe of this Experiment may be, 1*ſt*, For to make an Exception from that general Rule of M. *Des Cartes*, in the ninth Section of the ſecond Chapter of his *Diopticks*; where he ſays, *Quanto firmiores & ſolidiores exiguæ partes corporis alicujus pellucidi ſunt, tanto facilius lumini tranſitum permittunt.* For, it ſeems, by this Experiment, not to be the greater or leſs Fluidity, or Firmneſs of Body, that cauſes a Difference in Refraction, but a more rarify'd or condens'd Texture.

Next,

Next, It affords us two Arguments againſt their Opinion, who affirm Cryſtal to be generated of Ice. For, *Firſt*, As to its Weight, this is found to ſwim upon Water; whereas the other ſinks. *Next*, The Refraction of Cryſtal is obſerv'd to be greater than that of Glaſs; whereas this of Ice I find to be leſs than Water.

Thirdly, This leſs Refraction of Ice, I take to be a good Argument, that the Lightneſs of Ice, which cauſes it to be born up of the Water, is not cauſed only by ſmall Blebbs or Bubbles, but from the uniform Conſtitution, or general Texture, of the whole Maſs.

Dr. Hook's *Method of making Experiments.*

The Reaſon of making Experiments is, for the Diſcovery of the Method of Nature, in its Progreſs and Operations.

Whosoever therefore doth rightly make Experiments, doth deſign to enquire into ſome of theſe Operations; and, in order thereunto, doth conſider what Circumſtances and Effects, in that Experiment, will be material and inſtructive in that Enquiry, whether for the confirming or deſtroying of any preconceived Notion, or for the Limitation and Bounding thereof, either to this or that Part of the Hypotheſis, by allowing a greater Latitude and Extent to one Part, and by diminiſhing or reſtraining another Part within narrower Bounds than were at firſt imagin'd, or hypothetically ſuppoſed.

The Method therefore of making Experiments by the *Royal Society*, I conceive, ſhould be this.

Firſt,

Firſt, To propound the Deſign and Aim of the Curator in his preſent Enquiry.

Secondly, To make the Experiment, or Experiments, leiſurely, and with Care and Exactneſs.

Thirdly, To be diligent, accurate, and curious, in taking Notice of, and ſhewing to the Aſſembly of Spectators, ſuch Circumſtances and Effects therein occurring, as are material, or at leaſt, as he conceives ſuch, in order to his Theory.

Fourthly, After finiſhing the Experiment, to diſcourſe, argue, defend, and further explain, ſuch Circumſtances and Effects in the preceding Experiments, as may ſeem dubious or difficult: And to propound what new Difficulties and Queries do occur, that require other Trials and Experiments to be made, in order to their clearing and anſwering: And farther, to raiſe ſuch Axioms and Propoſitions, as are thereby plainly demonſtrated and proved.

Fifthly, To regiſter the whole Proceſs of the Propoſal, Deſign, Experiment, Succeſs, or Failure; the Objections and Objectors, the Explanation and Explainers, the Propoſals and Propounders of new and farther Trials; the Theories and Axioms, and their Authors; and, in a Word, the Hiſtory of every Thing and Perſon, that is material and circumſtantial in the whole Entertainment of the ſaid Society; which ſhall be prepared and made ready, fairly written in a bound Book, to be read at the Beginning of the Sitting of the ſaid Society: The next Day of their Meeting, then to be read over, and further diſcourſed, augmented or diminiſhed, as the Matter ſhall require, and then to be ſign'd by a certain Number of the Perſons preſent, who have been preſent, and Witneſſes of all the ſaid Proceedings, who, by Subſcribing

ſcribing their Names, will prove undoubted Teſtimony to Poſterity of the whole Hiſtory.

Mr. OLDENBURGH's *Letter to Dr.* HOOK, *Aug.* 23. 1665. *Concerning the Plague then, and Graſs in Sheep's and Oxen's Lungs.*

SIR,

I CANNOT but commend you for being ſo careful of yourſelf in this dangerous Time, as not to venture to come amongſt us, eſpecially when you find yourſelf any ways out of Temper. The Sickneſs grows ſtill hotter here, though I find by all my own, and other Men's Obſervations, that very few of thoſe Houſes whoſe Inhabitants live orderly and comfortably, and have by Nature healthy Conſtitutions, (you muſt take all theſe together) are infected; and I can ſay, (God be praiſed for it) that as yet not one of my Acquaintance, except an under Poſt-Maſter, who lived cloſely and naſtily, and had all Sorts of People coming to his Houſe with Letters, is dead: So that, generally, they are Bodies corrupted, and Perſons wanting Neceſſaries and comfortable Relief, that ſuffer moſt by this Contagion.

THAT Obſervation, you mention of Mr. *Boyle's*, is this, that one of thoſe two Phyſicians, Dr. *Clerk*, and Dr. *Lower*, had aſſured him, that he had ſeveral Times found, in the Lungs of Sheep, a conſiderable Quantity of Graſs, in the very Branches of the *Aſpera Arteria*; and the other had related to him, that a few Weeks ſince, he, and a couple of Phyſicians more, 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 opened, 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 enquiring, both how ſuch a Quantity of Graſs ſhould get in there, and how being there, ſuch an Animal could live with it ſo long.

Extract of a Letter from Ballaſore, *Jan.* 6. 166⅞. *From Mr.* Henry Powell, *to his Father Mr.* William Daniell, *upon* London-Bridge: *Giving an Account of an Earthquake*, &c. *after the Appearance of the Comet then.*

THE ſame Star appeared in our Horizon, about the ſame Time 'twas ſeen with you: The Effects, in Part, have already been here, by unſeaſonable Weather, great Mortalities amongſt the Natives, *Engliſh*, and others. We have had ſeveral Earthquakes unuſual here, which, with hideous Noiſes, have, in ſeveral Places, ſwallowed up Houſes and Towns; but about ſeven Days Journey from *Ducca*, where were at that Time three or four *Dutch*, they, and the Natives, relate this Story. That in that Place the Earth trembled about 32 Days and Nights, without Intermiſſion; at the latter End, in the Market-Place, the Ground turn'd round as Duſt in a Whirl-wind, and ſo continued ſeveral Days and Nights, and ſwallowed up ſeveral Men, who were Spectators, who ſunk and turn'd round with the Earth, as in a Quagmire;

Quagmire; at laſt the Earth worked up, and caſt up a great Fiſh, bigger than hath been ſeen in this Country, which the People caught; but the Concluſion of all was, that the Earth ſunk with 300 Houſes, and all the Men, where now appears a large Lake, ſome Fathoms deep: About a Mile from this Town was a great Lake full of Fiſh, which, in theſe 32 Days of the Earthquake, caſt up all her Fiſh on dry Land, where might have been gathered many, which had run out of the Water upon dry Land, and there died; but when the other great Lake appeared, this former dried up, and is now firm Land.

Extract of another Letter from the ſame Mr. Powell, *to the Perſon abovementioned, from* Caſſumb, *Sept.* 27. 1666.

MINE, laſt Year, adviſed of the unknown Earthquakes which afflicted moſt of theſe Parts, in ſome to the deſtroying of whole Towns, *viz. June 1ſt*, in *Agra*, the King's Seat, at three in the Afternoon, ſuch a Darkneſs poſſeſs'd the Country, that none could ſee his Fellow in the Streets, nor his Hand, though never ſo near his Eyes, which continued half an Hour, and then diſſolved in Rain. It has pleas'd God to ſend this Year ſuch Rains and Overflowings of the Rivers, that in many Places whole Towns, with Cattle and Men, have been carried away, to the Deſtruction of many Thouſands. About the latter End of *Auguſt*, there was ſuch a Storm about *Pattava*, that it roll'd, as it were, that great City, their Houſes, in Heaps, deſtroyed many People, and continued three Days and Nights, in which we have loſt a Salt-petre Boat of Value, and the *Dutch*

Dutch another; alſo both ours and the *Dutch* Houſes, in all thoſe Parts, are blown down: We expect the ſame, it being uſual with us about the Middle of *October* yearly, but ſuch Inundations and Storms were never before heard of.

An Account of a petrified Bone. An oddly-coated Stone-Bottle: And a double Gooſe-Egg. Produced before the Society, *by Dr.* Brown *of* Norwich, *Feb.* 27. 166$\frac{7}{8}$.

THIS Bone was found laſt Year, 1666, on the Sea-Shore, not far from *Winterton* in *Norfolk*.

IT was found near the Cliff, after two great Floods, ſome thouſand Loads of Earth being broken down by the Rage of the Sea, as it often happeneth upon this Coaſt, where the Cliffs conſiſt not of Rock, but of Earth.

THAT it came not out of the Sea, may be conjectured, becauſe it was found near the Cliff; and from the Colour, for, if out of the Sea, it would have been whiter.

UPON the ſame Coaſt, but as I take it, nearer *Hasborough*, divers great Bones are ſaid to have been found; and I have ſeen a lower Jaw containing Teeth of a prodigious Bigneſs, and ſomewhat petrified. All, that are found on this Coaſt, have been found after the falling of ſome Cliffs; where the outward Cruſt is fallen off, it clearly reſembleth the Bones of Whales, and great cetaceous Animals, comparing it with the Skull and and Bones of a Whale, which was caſt up on the Coaſt near *Wells*, and which I have by me.

THE Weight thereof is fifty five Pounds.

THIS

THIS Bottle was filled with a green *Malaga*, above ſeven Years ago, and ſet up in a Nictrio of a Wine-Cellar-Wall in *Norwich*, where it contracted this *Mucor*: It was full at firſt, and is not yet empty.

A GOOSE-EGG, with another in it, or at leaſt over it; the outward Egg containing nothing but the White. The like I have obſerved in Hen's and Turky's Eggs. I would not omit to ſend it, becauſe though it ſometimes happeneth, yet few have the Advantage to ſee it, eſpecially in a Gooſe-Egg.

Mr. Charles Towneley's *Relation, with Obſervations of the late Eruption of Water out of* Pendle-Hill. *Communicated by* Richard Towneley, *Eſq*;

AUGUST 18. 1669, betwixt 9 and 10 o' the Clock in the Morning, there iſſued, out of the North-Weſt Side of *Pendle-Hill*, a great Quantity of Water: The Particulars of which Eruption, as I received them from a Gentleman living hard by, are theſe. The Water continued running for about two Hours; it came in that Quantity, and ſo ſuddenly, that it made a Breaſt of a Yard high, not unlike (as the Gentleman expreſs'd it) to the *Eager* at *Roan* in *Normandy*, or *Ouſe* in *Yorkſhire*; it grew unfordable in ſo ſhort a Space, that two going to Church on Horſeback, the one having paſſed the Place where it took its Courſe, the other being a little behind, could not paſs this ſudden Torrent. It endanger'd breaking down of a Mill-Dam, came into ſeveral Houſes in *Worſion*, (a Village at the Foot of the Hill) ſo that ſeveral things ſwam in them. It iſſued out

out at ſome five or ſix ſeveral Places, one of which was conſiderably bigger than the reſt, and brought with it nothing elſe but Stone, Gravel, and Earth. He moreover told, that the greateſt of theſe ſix Places cloſed up again, and that the Water was black, like unto that of Moſs-Pits; and laſtly, that ſome fifty or ſixty Years ago, there happened an Eruption much greater than this, ſo that it much endamaged the adjacent Country, and made two Cloughs or Dingles, which, to this Day, are called *Oburſt* (or, in our *Lancaſhire* Dialect, *Braſt*) *Cloughs*. Thus far this Gentleman related; what follows take from my ſelf: Going, ſince this, to ſee what I could of this Accident, I found nothing that did contradict the aboveſaid Relation. What I obſerved more concerning this and other Eruptions, is, that paſſing under the North-Eaſt End, commonly call'd the *Butt End* of *Pendle*, I ſaw ſeveral Breaches in the Side thereof, at ſeveral Diſtances from the Top; from theſe, Stones, mix'd with Earth, had been tumbled down, and lay in ſuch a confuſed Order, as if they had been brought thither by ſuch a like Eruption as this laſt; and enquiring of a Country Fellow, who was our Guide, he confirmed the Conjecture, and told us, theſe Breakings out of Water were very frequent, ſo that he wonder'd we took ſo much Pains to go and ſee this late one. I went to look amongſt the Rubbiſh of Stone and Earth, of one of theſe Breaches, to ſee if I could find any thing like Ore, but could find nothing. Having paſs'd the End of the Hill, and coming to the other Side, we, after a ſhort Time, diſcovered the mentioned ſix Breaches, of which two ſeemed to be very near the Top of the Hill, and in the ſame horizontal Line; the others at ſeveral Diſtances from the Top. I went only to the biggeſt of theſe Breaches, in which I obſerved theſe Particulars:

The Water had taken away the Soil, (which was but about two Foot deep) and bared the Rock, betwixt ſome twenty and thirty Yards in Breadth, and downwards a conſiderable deal more: It appeared evidently, that the Water came from betwixt the Swarth and the Rock, for, at the Top of the Breach, we ſaw ſeveral Holes, whereat the Water had iſſued forth, others were cloſed up with the Fall of the Earth; whereſoever the Water had taken away ſome two Foot deep of Earth, the Rock appeared: Amongſt the Rubbiſh I found nothing that could be ſuppoſed to come out of the Bowels of the Hill, but only ſuch Stones as might lie looſe on the Rock, amongſt the Earth that covered it. This is what I obſerved in the Breach, which, for Bigneſs, was moſt remarkable, and preſume, I ſhould have found nothing worth Notice in the leſſer ones. Though the Noiſe of this Eruption was ſo great, that I thought it worth my Pains to enquire further into it; yet, in all theſe Particulars, I find nothing worthy of Wonder, or what may not be eaſily accounted for. The Colour of the Water, its coming down to the Place where it breaks forth, between the Rock and Earth, with that other Particular of its bringing nothing along but Stones and Earth, are evident Signs that it hath not its Origin from the very Bowels of the Mountain, but that it is only Rain-Water, coloured firſt in the Moſs-Pits, of which the Top of the Hill (being a great and conſiderable Plain) is full, ſhrunk down into ſome Receptacle fit to contain it, until at laſt, by its Weight, or ſome other Cauſe, it finds a Paſſage to the Side of the Hill, and then a Way betwixt the Rock and Swarth, until it break the latter, and violently ruſh out. The great Eruption, mentioned to have happened ſo many Years ago, perhaps, is that taken Notice of by *Cambden* in his *Brittannia*,

Brittannia, pag. 613. *Verum hic mons damno quod ſubjecto agro jam pridem intulit maximam aquarum vim eructans, & certiſſimo pluviæ indicio, quoties eius vertex nebulâ veſtitur, maximè inſignis eſt.* I know not whether it may not be worth Notice, that going to the Top of the Hill, and obſerving a conſiderable Part thereof, eſpecially towards the Skirts, where Turfs had been gotten, I found that the Rock reach'd within a Yard or two of the higheſt Part; conſidering this, with what I obſerved at the mention'd Breach, and ſeveral other Places, I think it is very probable, that the whole Mountain, as great as it is, is one continued Rock; and it may be a Queſtion, Whether all other Hills be ſo or no? But this I leave to further Enquiry.

Extract of a Letter from the Preſident Cornelis Frans, *and the Council in* Ternata, *to the Heer* William Maatſuiker, *and the Council in* Banda, *dated the* 12th *of* Auguſt, 1673. *Concerning Earthquakes there.*

WE hereby acquaint you with two Wonders, the like not before heard of. The firſt, that on the 20*th* of *May*, being *Saturday* Evening, that great and high Hill *Gammaknotra*, about thirteen Miles from hence, is, for the moſt Part, flown up in the Air, which cauſed the next Day, being *Whitſunday*, ſo great a Darkneſs, that we could hardly ſee one another; and this was accompanied with a great Earthquake, and all the Land, both here, at *Manado*, *Chianco*, *Jaſangy*, and *Mindanao*, a hundred Miles from hence, and God knoweth how much further, was covered

with Aſhes a Foot thick, and ſo much was fallen in the Sea, that a ſmall fluit Ship, in going and coming from *Manado*, was ſeveral Times hinder'd in her ſailing, through the great Quantity of Aſhes driving, and ſome Houſes and Negeries, at the Foot of the Hill, were quaſh'd with the Weight of the Aſhes fallen on them.

The ſecond Wonder is, that on the 12*th* preſent, in the Night, between 11 and 12 o' the Clock, a ſudden Earthquake ſurprized us, with ſuch terrible Shakings, as poſſibly the like was never known, which encreaſed ſo violently, that the Hill of *Ternata*, on the South Side, was rent from Top to Bottom; the King's *Mandarſahas* Stone-Houſes were caſt down; Parts of Hills ſunk; all the tiled Coverings, with ſeveral Walls, caſt down; and the Sea was in that Manner diſturbed, that the Ships, here in the Road, expected all to have been caſt away; and Quantity of Fiſh was flung on the Shore, with many other ſtrange Paſſages. And that which is worſe, the ſaid Earthquake continueth to this preſent Time; and here is nothing to be ſeen but bad Spectacles of Ruin. By a further Letter from the ſaid Preſident of *Ternata*, of the firſt of *September*, the before-going Relation is confirmed, and that the Earthquake yet continued, ſo that the Night before, the Houſes were thereby terribly ſhaked; all which is more at large expreſs'd in a Relation printed at *Batavia*.

To whiten Bees-Wax, April 3. 1674.

IN *March* or *April* melt yellow Wax without boiling; then having ſeveral Pewter Diſhes ready, dip the Outſide Bottom of each Diſh in fair Water; then dip them into the Wax, and take up a very thin Plate of Wax, the thinner the better: Take them off, and expoſe them upon the Graſs, to the Sun, Air, and Dews, 'till they be milk white, turning them often. Try ſome of them by ſprinkling Water on them with a Cloth. *Query*, Whether white Lead may not this Way be made with very thin Plates.

Dr. John Carte's *Letters to Dr.* Grew, *of the* Belland, *cauſed by the Fumes of Lead, and other curious Obſervations.*

I THOUGHT it might be worth while to give you a ſhort Account of a Diſtemper in *Derbyſhire*, very common among thoſe, who are employed in the Smelting-Mills, *i. e.* the Houſes where they melt the Lead down from the Ore; it is by the Country People called the *Belland*, but for what Reaſon I cannot learn; it is hard to give a conciſe Definition of it, becauſe it ſeldom appears but under the Diſguiſe of another Diſeaſe.

THIS *Belland* frequently imitates the *Tormina Ventris Scorbutica*, but in a moſt exquiſite Manner, which is uſually accompanied with extreme Coſtiveneſs, and a continued Suppreſſion of Urine: Sometimes appears like an *Aſthma Convulſivum*, ſometimes a continued and obſtinate *Dyſpnœa*, and often ſeizes the *Genus Nervoſum*, either

in a paralytick Reſolution of the Parts, or in Spaſms.

IT has a different Effect upon Men, according to their Age; if they come not to the Work of the Mills, till they are full grown, or of a middle Age, they ſuffer moſtly the aforementioned Pains of the Belly, or difficult Breathing. But if taken in while young, and growing, they are ſubject to the Palſy; their Limbs (eſpecially their Fingers) being often irrecoverably reſolved: Or ſometimes have their Fingers ſo contracted, as to render them (perhaps for ever) incapable of working. Both which I have ſeen.

I COULD not be informed of any Specificks, they had for this Diſeaſe; but that a Decoction of *Coloquintida*, in Ale, was very common among them. I remember once, an old Man complained to me of the *Belland*, it oppreſſed him in the Nature of an *Aſthma*; I adviſed him to ſulphurate Medicines, which did relieve him. The Contraction of the Fingers I have known cured, by often putting the Arms into hot Grains after Brewing.

I HAVE not obſerved, whether any of thoſe, that are paralytick by the *Belland*, die Hectick, as Dr. *Pope* relates of them, at the *Mercurial* Mines in *Firmly*, but it ſeems not improbable that they may.

THIS Diſtemper is not only incident to Men, but other Creatures, as Horſes, Cows, Dogs, Cats, Hens, Geeſe, *&c.* but, eſpecially, Cats are ſubject to it: Indeed few Creatures, that are young, will live near theſe Mills without the *Belland*.

DOGS do in their Fits howl and tumble up and down, foaming like *Epilepticks*; this the People impute to the Pain of their Bellies.

I KNOW a ſmall Rivulet, on which ſome of theſe Mills ſtand, wherein Trouts have been caught, which have been ſuppoſed affected with the

the *Belland*, by the Irregularity of their Growth, their Heads being great and miſhapen, their Backs crooked, their Tails very ſmall, which, I am apt to think, might proceed from their feeding on the *Smitham* or *Duſt* that is waſhed down at a Flood: For not only the Fumes, but alſo the Waſhings of Lead Ore, and the *Waſte* (as they call it) *i. e.* the Duſt that remains, after the Ore is melted, is very noxious to moſt Sort of Creatures, and for this Reaſon, they, that live near the Mills, dare not water their Horſes at the River, upon a Flood.

THESE poiſonous Fumes are not only hurtful to Animals, but alſo injurious to Vegetables; for if the Smoak be driven much upon any one Place, it deſtroys all the Graſs of it.

NOW that the *Belland* in Men, or other Creatures, proceeds moſtly from the Smoak, will be eaſily granted; but what theſe Fumes is impregnated with, is the Queſtion: Some fancy them to be Antimonial, but then, methinks, they ſhould have the ſame Effect with the Flowers of that Mineral, and I never heard that any of them were inclined to Vomit. I am much more apt to think, that the *Mercury* in the Ore is the Cauſe, both becauſe they, that work in the *Mercurial* Mines, are ſubject to the like Symptoms, eſpecially the Palſy; and alſo I am told, that this *Belland* often begins with a Swelling of the Glands about the Throat, which, perhaps, if not prevented, might terminate in Salivation. But why *Mercury* ſhould operate ſo variouſly upon Bodies, differing in Age, is a Queſtion will hardly be ſolved, till it appear more plainly, whether it be nearer a-kin to Alcalies or Acids: Its Effect is eaſily foretold in Bodies that abound with Acids, whether Scorbutick or Venereal; but in younger Perſons whoſe Humours are more inſipid, and their Blood freer from both fix'd Salts and Acids, it may, perhaps, fix

itſelf upon the Nerves, as the cooleſt Parts, and impede the Motion of the Spirits; but I had rather hear others Reaſons about the Cauſe of theſe Things, than trouble you with my own.

SOME other Things I have been informed of by the Work-men, as that a little Spar mix'd with the Lead Ore, promotes its Fuſion, I ſuppoſe, as the yellow Marchaſite, that's found with Silver, makes that Metal flow the ſooner: That if there be any Holly-Wood in the Fire, it hinders the fluxing of the Ore, which is certainly cauſed by the glutinous Sap of that Wood.

THAT the Smoak is obſerved to follow the Water very much: I ſuppoſe the Coldneſs of the Water does condenſe the Fumes, as is ſeen in reviving *Mercury* from *Cinnabar*. A blue Film is obſerved on the Surface of thoſe Waters, where the Smoak falls.

THAT a Man may by wetting his Finger in his Mouth, or common Water, draw it through melted Lead or Iron, without any Prejudice.

Sir, THESE Obſervations will ſeem barren, yet as good as I could make among theſe booriſh People of the *Peak*, few of which can give a rational Account of either what they do, or ſuffer, in ſuch Matters.

I am,

Mancheſter, Octob. 27. 1678.

Sir,

Yours, &c.

Part of a Second Letter to Dr. Grew, *Dec.* 6. 1678.

SIR,

SINCE I writ to you about the *Belland*, I have been in *Derbyſhire*; all, that I could learn farther of it, was, that they are leſs ſubject to that Diſtemper in thoſe Smelting-Mills, that ſtand in an open and moveable Air, or that have large Chimnies, and are not built cloſe: I met with a Gentleman who told me, a Servant or two of his had it very ſeverely in their Bellies, and were cured by taking the Salt that comes from the Sulphur-Well at *Knaresborough*; this Remedy is, I think, one of the likelieſt I have heard of.

Dr. Hook's *Deſcription of his Weather-Wiſer*; *about* Dec. 5. 1678.

THE Weather-Clock conſiſts of two Parts; *Firſt*, that which meaſures the Time, which is a ſtrong and large Pendulum-Clock, which moves a Week, with once winding up, and is ſufficent to turn a Cylinder (upon which the Paper is rolled) twice round in a Day, and alſo to lift a Hammer for ſtriking the Punches, once every Quarter of an Hour.

Secondly, OF ſeveral Inſtruments for meaſuring the Degrees of Alteration, in the ſeveral Things, to be obſerved. The firſt is, the Barometer, which moves the firſt Punch, an Inch and Half, ſerving to ſhew the Difference between the greateſt and leaſt Preſſure of the Air. The ſecond is, the Thermometer, which moves the Punch that ſhews

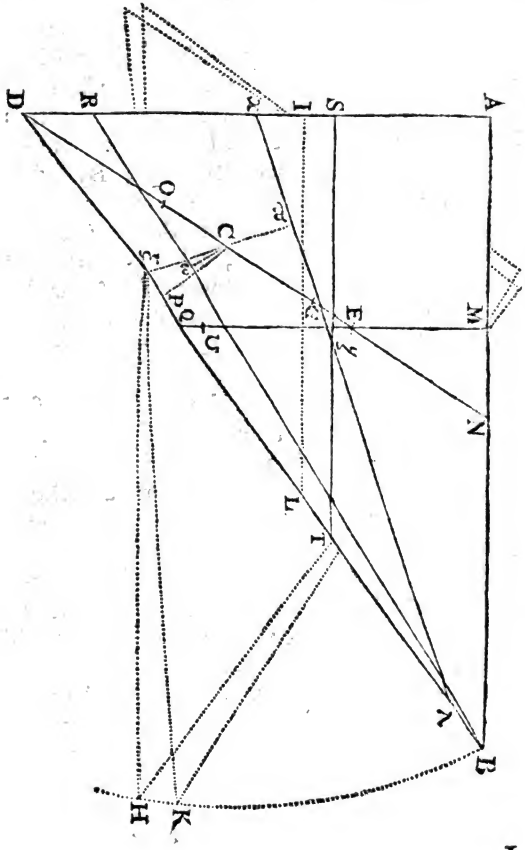
the Differences between the greateſt Heat in Summer, and the leaſt in Winter. The third is, the Hygroſcope, moving the Punch, which ſhews the Differences between the moiſteſt and drieſt Airs. The fourth is, the Rain-Bucket, ſerving to ſhew the Quantity of Rain that falls; this hath two Parts or Punches; the firſt, to ſhew what Part of the Bucket is fill'd, when there falls not enough to make it empty itſelf; the ſecond, to ſhew how many full Buckets have been emptied. The fifth is, the Wind Vane; this hath alſo two Parts; the firſt to ſhew the Strength of the Wind, which is obſerved by the Number of Revolutions in the Vane-Mill, and marked by three Punches; the firſt marks every 10000 Revolutions, the ſecond every 1000, and the third every 100: The ſecond, to ſhew the Quarters of the Wind, this hath four Punches; the firſt with one Point, marking the North Quarters, *viz.* N: N. by E: N. by W: N. N. E: N N W. N E by N. and N W by N. N E. and N W. The ſecond hath two Points, marking the Eaſt and its Quarters. The third hath three Points, marking the South and its Quarters. The fourth hath four Points, marking the Weſt and its Quarters. Some of theſe Punches give one Mark, every 100 Revolutions of the Vane-Mill.

THE Stations or Places of the firſt four Punches are marked on a Scrowl of Paper, by the Clock-Hammer, falling every Quarter of an Hour. The Punches, belonging to the fifth, are marked on the ſaid Scrowl, by the Revolutions of the Vane, which are accounted by a ſmall Numerator, ſtanding at the Top of the Clock-Caſe, which is moved by the Vane-Mill.

Dr. HOOK's *Contrivance of a Veſſel, to meaſure the Quantities of Rain falling: Being a Part of his* Weather-Wiſer *in the preceding Paper.*

PROBLEM.

To make a Veſſel, which, when it hath received a certain Quantity of Water, ſhall empty itſelf.



LET

LET the Veſſel be a Triangular Priſm, as z poiz'd like a Balance upon a Foot, ſo that the leſſer End may only deſcend, and not the greater, by means of the Stop D. And let one of the Sides be ABD. From N, the Half of AB, draw the Line DN; and from M, of AB, draw MQ parallel to AB; therefore E ſhall be the Center of Gravity of the Triangle ABD. And becauſe AB is an open Side of the Veſſel, ſome Point between E and D, as G, ſhall be the Center of Gravity of the whole Veſſel; taking a Point at P near Q, towards D, erect PC, and let C be one of the Centers of Motion, upon which, and the like oppoſite Point in the other Side of the Veſſel, it ſhall turn as a Balance. *Secondly*, By adding Weight in O oppoſite to G, equiponderate the whole Veſſel upon the Center of Motion C; therefore DCN will be a Balance, whoſe Center is C, and the Weights of equal Moment are G and O. *Thirdly*, Draw the Line ST parallel to AB, ſo that C may be the Center of Gravity of the Triangle DST.

Firſt, I SAY, if the Veſſel be fill'd ſhort of ST, the Side D ſhall preponderate; if higher, the Side B; becauſe C is the Center of the Balance DCN, and the Centers of Gravity of all the like Triangles, leſs then DST (as DIL) are upon the Arm DC, and the Centers of all the greater upon the Arm CN. Hence it follows, that becauſe it is ſtopp'd from deſcending at D, the Veſſel ſhall reſt till the Water riſe above ST, when the Side, towards B, ſhall preponderate.

2dly, I SAY, if the Veſſel be inclined towards B, the Part B ſhall ſtill preponderate; let ABD be inclined, (C the Center as before;) ſo that the Water, that lay before at ST, lies now as κζλ, and let πC be a perpendicular Line, becauſe the Triangles

Triangles DST, D $\chi\lambda$ are equal, but $\zeta\chi$S, the Triangle nearer the Perpendicular, is taken away, and $\zeta\lambda\tau$, being farther off, is added on the Side towards B; therefore that Side preponderates, and the more the lower it deſcends, becauſe the Center of Gravity, of the Triangle $\zeta\lambda$T, runs farther and farther from the Perpendicular, till it runs over at B.

3*dly*, I SAY, that when a Part given of the Water is poured out, the Reſidue ſtill preponderates, while it remains inclin'd. Let the Water be repreſented by the Triangle DRB in the Motion of pouring out, Part being run over; the Center of Gravity of the Water, is υ in the Line MQ: and C ω at right Angles to BR, will be the Perpendicular, as CP will be the Perpendicular when B is deſcended ſo low, that DB becomes horizontal, (that is, when all the Water muſt be poured out) therefore CP is between CO and υ, but by Conſtruction the neareſt Point of MQ is without CP towards B, therefore υ preponderates; therefore the Veſſel ſtill inclines, till all be poured out. Therefore that, which was required, is perform'd.

SCHOLIUM.

IF it be requir'd that the Veſſel, after it is empty, ſhould return again to its former Poſition, there muſt be added to the Point O yet more Weight at K, enough to reſtore the emptied Veſſel, in which Caſe a Triangle may be drawn as DBR, whoſe Weight upon its Center υ ſhall equiponderate to K in O; it ſeems therefore, that the Veſſel ſhould deſcend no lower than till BR be horizontal. But becauſe nothing that moves towards an Equilibrium reſts there, but is carried further by the impreſs'd Force which it gains in deſcending

ſcending to this Equilibrium, as it appears in all Manner of pendulous Motions. And becauſe K may be leſs than any Magnitude aſſigned, therefore, notwithſtanding the Counterpoiſe of K, it will deſcend ſo low, as to pour out all; that is, having gain'd an impreſs'd Force in its Deſcent from B to K, there is no Reaſon but it ſhould continue it beyond the Equilibrium to H and further.

Beſides this, I find two other Contrivances of Dr. *Hook*'s, among the Minutes of the Royal Society of *April* 1670. for meaſuring the Rain that falls, in theſe Words: Mr. *Hook* ſhew'd an Experiment in Mechanicks, which was a Way how to take notice of all the Rain that falleth, and was deſigned as a Part of the Weather-Clock. The Contrivance is the ſuſpending the Bucket that was to receive the Quantity of Rain, that fell at any time (whether more or leſs) ſo that according to the Quantity therein contain'd, the Place thereof ſhould either be higher or lower, but certainly be determin'd. This was perform'd by a Counterpoiſe to the ſaid Bucket. The Counterpoiſe was contriv'd two Ways; either by a String of leaden Bullets, ſo order'd, that when the Bucket was quite empty, all the Bullets reſted upon a Table; but when there fell as much Water into the Bucket, as equall'd the Weight of one of the leaden Bullets, then the Bucket deſcended one Space, and one Bullet was lifted up; when twice as much, two Bullets; and when three times as much, three Bullets were lifted up; and ſo forward, till all the Bullets were lifted up, and the Bucket had deſcended to its Place of Emptineſs; whereupon the Chain of Bullets preſently deſcended, and lifted up the Bucket into its empty Place.

But

But becauſe this Motion proceeded by Jumps, and was not equable, therefore a ſecond Contrivance was alſo ſhewn, which was this,

The Counterpoiſe to the Bucket, when empty, was a Cylinder immerſed into Water, *Mercury*, or any other Fluid. Which Cylindrical Counterpoiſe, according as the Bucket receiv'd more and more Water, was continually lifted higher and higher out of the Water, by Spaces always proportioned to the Quantity of Water that was contained in the Bucket. And when the Bucket was fill'd to its deſigned Fulneſs, it immediately emptied itſelf of the Water, and the Cylinder plung'd itſelf into the Water, and raiſed the Bucket to the Place where it was, again to begin its Deſcent.

This Contrivance, here made uſe of, was declar'd to be very uſeful for making a new and uſeful Beam, for examining the Weight of Bodies, with out any Trouble of adjuſting, the Riſing of the Cylinder immediately ſhewing the determinate Weight of any Body, put into the Scale, without any farther Trouble.

Mr.

Mr. TOINARD*'s Observation of the Difference of Longitude between* Paris *and* Breſt, *with Obſervations of* Jupiter*'s Satellite Eclipſes, in* 1679.

		H.	'	"
10 *Dec.* 79.	A Paris à	12	50	08
	A Breſt a	12	22	37
Breſt ſelon le grande Carte de France de Samſon del' an 1650 eſt plus occidentale que Paris de degr. -		08	10	00
Qui valent le temps de - -		00	32	40
Mais l'obſervation faite 10 Dec. donne de difference - -		00	27	31
Par conſequence la Carte qui eloigne Paris de Breſt de - -		00	32	40
Dont il faut ôter la veritable difference		00	27	31
Se trompe de - - -		00	04	22
Qui valent plus d'un degre & un cart.				
3 *Dec.* 79.	A Paris l'immerſion du Grand			
19	Satellite à - -	10	53	23
	A Paris l'immerſion du Premier a - -	09	16	03

Son gros camarade environ demie heure & demie caſt apres.

Mon

Monſieur TOINARD's *Obſervations of the Eclipſes of* Jupiter's *firſt Satellite in* 1680.

Satellitis *Jovis* primi ſeu proximi immerſiones in umbram *Jovis* Pariſiis, 1680. Stilo novo.

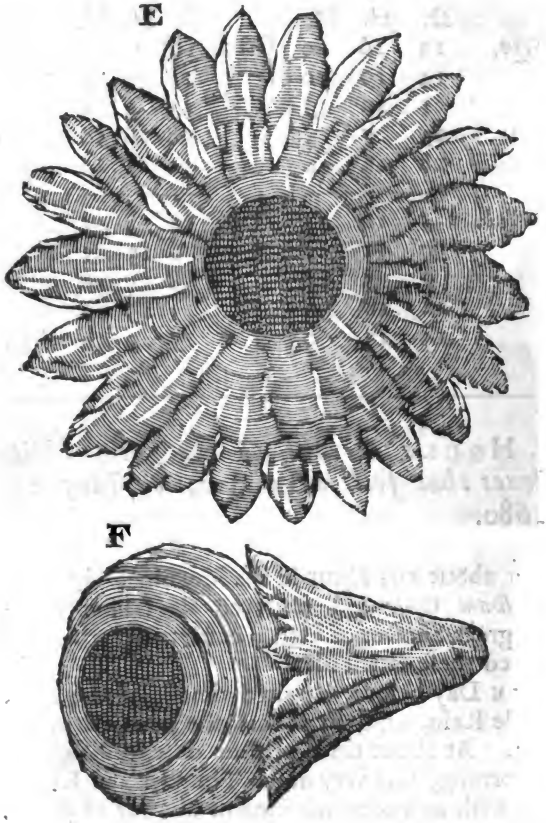
	D.	H.	M.		D.	H.	M.	
Sept.	12	16	20	*Octob.*	16	7	30	
	14	10	50		21	14	55	
	21	12	45		23	9	25	
	23	7	15		28	16	50	
	28	14	40		30	11	20	
Octob.	5	16	35	*Nov.*	4	18	45	
	7	11	5		6	13	15	
	9	5	35		8	7	40	
	12	18	30		13	15	10	
	14	13	0		15	9	40	vel 35

Dr. HOOK's *Account of the great Hail-ſtones that fell in* London, *on* May 18. 1680.

AT about 10¼ Hour in the Morning, in *Greſham College*, I obſerved the falling of a great Shower of Hail; concerning which, I obſerved theſe Particulars.

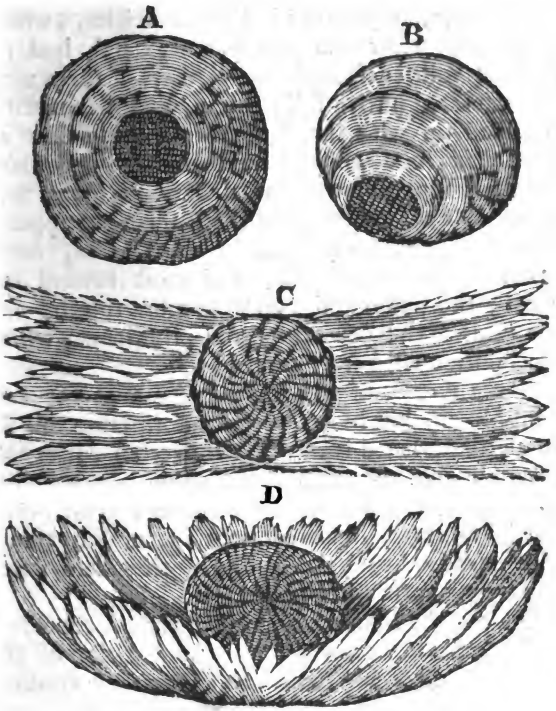
THE Day before, it rain'd almoſt, all the Day, a gentle Rain, and, by turns, the fore-part of the Night. At about three or four o' the Clock in the Morning, was very much Thunder and Lightning, with an exceeding violent Shower of Rain; whether any Hail then fell, I know not, being in

Bed; but, by ſome Circumſtances, I believe there did, for there were found, in the Morning, ſeveral great Spots of Wet, which, 'tis probable, proceeded from Hail-ſtones that fell down the Chimney. It continued to rain, and now and then to thunder much, till about Nine; then it clear'd up, and the Sun ſhone very clear, and



there

there was ſcarce a Cloud to be ſeen; about ten it began to thicken, and I heard the Thunder to the South Eaſt; at about half an Hour after ten, it grew very dark, and thunder'd very near; and ſoon after there began to fall a good Quantity of Hail-ſtones, ſome of the Bigneſs of Piſtol Bullets, others as big as Pullets Egs, and ſome above 2½ Inches, and near three Inches over the broad Way; the ſmaller were pretty round, and white, like Chalk, or Sugar Plums; the other of other Shapes: Some of the moſt remarkable were theſe.



BREAKING many of them, I found them to be made up of Orbs of Ice, one encompaſſing another; ſome of them tranſparent, and ſome white, and opaque; ſome of theſe were to the Bigneſs of near an Inch in Diameter, and were orbicular every Way. Some of them had the white Spot in the Middle, as A; others towards one Side, as B; and the Variety of white and tranſparent Spots very differing; thoſe, which exceeded theſe in Bigneſs, were made by an additional Accretion of tranſparent Icicles, radiating every Way from the Surface of the White Ball, like the Shooting of Niter, or toothed Sparre. Theſe in ſome ſtood, as it were, ſeparate in diſtinct Icicles, which were very clear and tranſparent, and had no Blebs or Whiteneſs in them. Others of them were all concreted into a ſolid Lump, and the Interſtices filled up with Ice, which was not ſo clear as the *Stiriæ*, but whiter; and thereby one Side, which, I ſuppoſe, was the undermoſt, was flat, almoſt like a Turnep; and the Radiations appeared to proceed from the Ball in the Middle, more towards the upper Side, and moſt toward the Sides; the Edges and Top were more rough, and the Ends of the *Stiriæ* appeared prominent; which the Figures will better expreſs.

THE Extent of this Shower I cannot yet certainly learn, but have, by the Information of ſeveral, underſtood it was ſeen above ten Miles off. I was alſo told by ſeveral Perſons, that, a little before the Hail fell, there were was heard a great Noiſe out of the Sky, like the Shooting, or Emptying, of a Cart-load of Pebbles, as if they had fallen one upon another in the Air.

FROM the Manner of their Figure, I conceive, their Accretion was made by a Congelation of the Water, as they fell; that the ſmall white Globule in the Middle, about the Bigneſs of a Pea, was the

the firſt Drop that concreted into Hail; this, in falling through the Clouds beneath, congealed the Water thereof into ſeveral Coats or Orbs, till ſome of them came to the Bigneſs of Pigeons Eggs, ſome white, ſome tranſparent, according to the ſeveral Degrees of Coldneſs it paſſed through, whilſt they congealed; that the laſt Accretion was made by a more violent and ſudden Cold, in the lower Part of the Cloud, where they paſſed through almoſt a continued Body of Water. Other Varieties of their Forms, which were very many, I conceive, muſt be made by their meeting with one another in their Paſſage.

NOTWITHSTANDING *Mr.* Waller *hath publiſhed the Subſtance of this Paper, in Dr.* Hook's *Life*, p. 22. *yet the Original may not be unacceptable to the Reader, by reaſon of the Figures, which the Doctor hath given of thoſe monſtrous Hail-ſtones; which I, my ſelf, ſaw falling, in great Numbers, in* Great Lincoln's-Inn-Fields, *and notic'd to have fallen on* May 19. 1680. *one of which a Servant brought me in his Hand, as large as a Turnep, and of the ſame Shape, which I inſtantly meaſured with a String, and found the Compaſs of the wideſt Part to be above thirteen Inches; which, I confeſs, ſeems ſomewhat incredible; but, I think, I did it with great Care, and was not miſtaken.*

W. DERHAM.

The Reverend Mr. PASCHALL's *Letter to Dr.* HOOK, *of an Earthquake in* Somerſetſhire, *dated* Jan. 4. 1680. *from* Chedſey *in that County.*

Worthy Sir,

YESTERDAY about ſeven in the Morning, I, being about to riſe, took Notice of what ſeemed a ſmart Guſt of Wind, which was followed with a Jog of our Houſe, and that immediately with a very ſenſible Shaking of the Houſe, and particularly the Bed in which I lay. I doubted the Fall of ſome large Piece of Timber, or Stone-Work, and cauſed the Servants to make diligent Search all about for the Cauſe of it; though not without Suſpicion that it might be an Earthquake. Before Night I became fully ſatisfied that it was ſo, for my Neighbours, many, obſerved the like in their Houſes, though no Hurt was done. This Day, I hear, that it was in other Pariſhes, one within a Mile of me, lying in the ſame Level; another above four Miles from me, lying on the further Side of an Hill, and which is a firm Rock. This Afternoon comes a Letter from an Acquaintance of mine in *Bridgwater*, (two Miles from me, and on the other Side of their navigable River) which ſays thus, "I ſuppoſe you heard of "the Earthquake, which happened with us this "Morning about ſeven a Clock: It ſhook our "ſtrong Stone-Houſe ſo, that I began to look "whether the Walls were ſcattered or craſed, with "a Noiſe, as if ſome very great Thing had fallen "upon the Ground. One or two in *Eaſtover* (a "Part of that Town on our Side of the River) "were ready to leap out of Bed upon it, *&c.*" The Air was very calm, as being a froſty Morning, upon the Snow lying, which fell the Day

before. It laſted but a very ſhort Time. I do not remember, for theſe eighteen Years of my Abode here, to have known any ſuch thing; but I call to mind the Obſervation of *Acoſta*, and others, that they do moſt commonly happen in Places near the Sea, and ſuch is our Country; of which I meet many Arguments which perſuade, that it was, in theſe Parts of it, formerly gained from the Sea. If you ſee my Lord of *S*——, I preſume, it would not be unacceptable to his Lordſhip, to have an Account hereof, ſeeing, 'tis likely, it will be a Matter of publick Diſcourſe.

An Extract of Mr. Leuwenhoek's *Letter from* Delf, *Jan.* 6. 1680. *Concerning the Minuteneſs of ſome* Animalcules *in the Waters.*

——SINCE I perceive you are pleaſed with ſome of my Speculations, I have, herewithall, tranſmitted alſo a Copy of that haſty Calculation, which, at the Deſire of the Honourable *Conſtantine Hygens van Zutichem*, I drew up for him in Writing; which was this which follows.

SIR,

I HAVE been often conſidering of the exceeding Smallneſs of thoſe Veſſels, of which the Sinews and Muſcles of theſe ſmall Creatures muſt be furniſhed with: That which put me upon this Speculation was, the Query put to me, Whether I could, by my Microſcope, diſcern the Particles of which Water doth conſiſt? To which I frequently gave this for an Anſwer, That there are in the Water living Creatures, many Millions of

Times ſmaller in Bulk than a ſmall viſible Sand. Further, That each of theſe Creatures, though I have not, as yet, been able to diſcover their Paws, Finns, or the like Inſtruments, by which they move, muſt, nevertheleſs, be furniſhed with ſome Kind or other of Organs, fit to produce that Motion. And that theſe Organs muſt be made up of Veins, Arteries, or Veſſels, to convey Nouriſhment to them, and in Sinews or Strings, to ſtir and move by, *&c.* If ſo, then by theſe Veſſels the Water muſt find its Paſſage, and conſequently the Particles of Water muſt be conſiderably ſmaller than theſe Veſſels, otherwiſe it could not freely paſs them; now the whole Animal itſelf, being but ſcarcely viſible, we muſt conclude, theſe their Veſſels muſt be wholly inviſible, and how much more inviſible muſt be the Parts of Water that move in them; inſomuch that I am very confident, that no Man will ever be able to attain, by the Help of Microſcopes, to diſcover and diſtinguiſh the Particles of which Water doth conſiſt.

Now that there are ſuch Creatures, that are ſo many Times ſmaller in Bulk, than a ſmall viſible Sand, I prove by theſe following Obſervations and Calculations. I do generally ſuppoſe (becauſe, as far as my Sight was able to help me, in taking the proportionate Bigneſs, I ſo judged it) that about 3 or 400 of the ſmalleſt of theſe Creatures, laid one by another in a Line, may make the Length of the Diameter of a middle-ſiz'd Grain of Sand. I here ſhall make uſe only of the leſſer Number, *viz.* 300; which multiplying cubically, I find the Product to be 27000000; whence it follows that there will go the Quantity of 27 Millions of theſe Animals, to make the Bulk of one ſmall Grain, *&c.* If we then ſuppoſe that eighty of theſe Sands, laid one by another, will make but

but one Inch in Length, then there will lie in the Space of a Cubical Inch no leſs than 512000 of theſe Sands, each of which being ſuppoſed to be as big as 27000000 of theſe Creatures, the Inch Cubical will contain no leſs than 13824000000000, almoſt fourteen Millions of Millions.

I HAVE conſidered alſo of the ſmall Veſſels, that ſerve to compoſe the Parts of our Bodies, and conceive them to be Pipes a thouſand Times ſmaller than an Hair of a Man's Head; and by a Braſs-Rule, curiouſly divided into Inches, and each Inch into thirty Parts, endeavouring to find, how many of theſe Hairs Breadths would make an Inch, I found that twenty Hairs would lie one by another in the thirtieth Part of an Inch, and therefore 600 in an Inch; and meaſuring my Body, I found that one Part, with another, equall'd a Cylinder of eight Inches Diameter; ſo that theſe Proportions conſider'd, I find, that one of theſe Veſſels muſt be 360000000 ſmaller than a Pipe of an Inch Diameter, and, conſequently one Part of the Body being equal to a Cylinder of eight Inches Diameter, which is 64 Times as big as one of an Inch, the Cylinder of the Body is bigger than the Cylinder, of one of theſe Veſſels, no leſs than 23040000000. Now if the Veſſels of the Bodies of theſe ſmall Creatures, in Pepper Water, ſhould hold the ſame Proportion to their Bodies, how can we conceive the Parts of the Water ſhould be diſcovered, that ſhould move in thoſe Veſſels.

Reiſelius *his Letter to Dr.* Grew, *concerning a Man's periodical Loſs of his Speech, from* Stutgard, March 6. 1680.

AT enim, ne ſine ſymbolo coram altari veſtro ſanctiſſimo appaream, appono hic Caſum quendam merè naturalem quidem, ut mihi videtur, propter multos ſimiles affectus periodicos Cephaleos pictum, Convulſiones, Colicas, ut de Febribus nihil dicam, ſed rarum tamen, ob tam conſtantem tamq; multis annis durantem periodum, uti obſervatus fuit a Collega meo examinante in preſentia Principis noſtri aliorumque magnatum *Novembri* menſe præteriti anni. Cujus cauſam, cum neque mihi neque aliis detur aſſequi, ab Societatis Regiæ Judicio diſcere gaudeo & rogare audeo. Quomodo a fermentatione quadam ut in Febribus aliiſq; morbis deducenda ſit hæc affectio, hæreo. An a cœli meridiano vigore trahendum malum ſeu bonum, dubito, cum olim aliis horis & inordinate notata ſit hæc affectio. Symptomata tamen, quæ quondam antegreſſa, morboſum quid innuunt. Hic talis eſt. *Georgius Algaier, Georgii Algaieri* Cauponis *Jeſingæ* propè *Kircſemium* in Ducatu *Wirtenbergico*, filius temperamenti Cholerici, annorum 25, jam ante annos quindecim feſto S. *Stephani* ſtatim poſt cœnam, adeo male toto in corpore ſe habuit, ut nullibi ſe continere potuerit. Anxietas cordis erat tanta, ut, niſi per Vomitum ingentem levatus fuiſſet, ſuffocari ſibi videretur. Hora poſt vomitum unica præterlapſa melius agebat, at per totum trium menſium decurſum valde triſtis & melancholicus, interdum etiam quaſi terrore percuſſus evadebat. Elapſo hoc tempore, primò ſaltim per unum fere momentum vocem & loquelam, quam prius accuratè

ratè callebat, amiſit, ut ne verbulum quidem, neque ullam vocem emittere poſſet. Quotieſcunq; verò loquela amittebatur, toties tum, (quod tamen ultra dimidium annum non duravit) è ventriculo, aliquid ſurſum, fauces verſus reperi ſentiebat. Atq; uti primùm vocis & linguæ ſuppreſſio ſaltem momentanea, ſic eadem poſt indies indieſq; creſcere incipiebat, ita ut a momento ad ſemihorium totum, duas, tres, & ultimò ad 23 Horas, inordinatè tamen, duraverit. Tandem typum adeo conſtantem habebat locutionis reſtauratio, ut jam per 14 annos, non niſi ſingulis diebus ab hora 12 meridiana, per horæ integræ ſpatium, ad primum ſcil. Pomeridianæ punctum uſque loqui poſſit. Nec falli poteſt hominis Horologicus ſenſus horarum tranſpoſitione, cum vel nullis campanis ſonantibus terminum horæ duodecimæ uſque ad primam ſemper & quam accuratiſſimè obſervet. Notandum etiam quod dum loqui poteſt patiens, aliquantiſper balbutiat, quin & tum extra tum intra locutionis tempus linguam ipſam non ſatis volubiliter queat movere. Præter amiſſionem vocis & loquelæ nulla de actione queritur, ſenſus tum interni tum externi ſunt integri. Accuratiſſimè ſemper audit, unde vel geſtibus vel literis (ſcribere enim ſcit ſatis intelligenter, ut ipſum hoc imitatione expreſſum propria manuſcriptum teſtatur nomen *Georg Ulgryer Jeſingus*) ad interrogata cuivis quantum poſſibile eſt reſpondet. Vixit aliàs hactenus omnimodò ſanus, nec ullum morbum, excepta Febri, qua ante tres menſes vexabatur, quotidiana; cujus paroxyſmum inordinatè jam mane, veſperi, jam etiam noctu ſuſtinuit, paſſus eſt; neque typus amiſſæ loquelæ ex Febri minimùm mutatus. Vivit etiamnum poſt febrem ſanus omninò & incolumis. Hucuſq; Caſus Muti periodicè loquentis.

Mr.

Mr. PIGOT *in his Letter to Dr.* HOOK, *from* Oxford, *Nov.* 26. 1681. *ſaith,*

MR. Caſwel, *in his Travels with Mr.* Adams, *obſerved* Lidford-Bridge, *in or about* Dartmore *in* Devonſhire, *whoſe Plane is level with the Ground, yet* 59 *Feet above the Water, that runs ſwiftly under it.*

At Droitwych *in* Worceſterſhire, *he viſited the Salt-Springs, which he found, upon Taſte, to be far ſalter than the Sea. They have three in the Town, cloſe by a freſh River Side, and could have more, but that the Merchants will not permit any more, to keep up the Trade. He tells me, the poor dirty Women, that work at the Salt-Houſes, are never troubled with Lice, Fleas, or Flies.*

Mr.

Mr. Leewenhoeck's *Letter in* Nov. 1681, *of the Structure of Hair; of the Excrements,* &c.

SIR,

I HAVE ſhewed that the *Cortex* of the Hair of an Elard Hart, *&c.* was compos'd of Globules. I found the ſame of my own Hair. I have ſince found it like the Bark of a Tree of Globules, but irregular from the ſqueezing of the Hair. The Subſtance of the Hair is made of Threads; ſome judge the Hair hollow, others to have Marrow; but viewing a Hog's Hair, I found the Hollowneſs of thoſe Hairs from Cleft. Hair grows by Protruſion, not as Plants, being thruſt continually forward, from within the Skin outward; what was within moiſt, expos'd to the Air, dries and ſhrinks, and the outward Skin hardening, the inward Threads, upon ſhrinking, cleave into one or more Clefts, which ſeems like Marrow. In a Piece of Hog's Hair the Threads appear plain, even in a common Microſcope, but bigger in Proportion to the Circumference for Eaſe; the Threads were but few, from the Roughneſs of the Razor. By theſe may appear their Miſtake, who aſſert Hairs round; 'tis rather true, they have all differing Figures. A Friend viſiting me after a Fit of Sickneſs, whereby he had loſt all his Hair, complain'd of a great Itching all over his Skin, yet his Stomach was good, which the Doctor attributed to a Sharpneſs of Blood; which I rather aſcribed to the filling of the Body, and from the new growing of the Hair in the Pores, whence it had fallen by his Diſeaſe, the Pores of which being cloſed, the new Hairs, thruſting againſt the *Cuticula*, cauſed the Itching. I myſelf have been ſo troubled in the Spring, which,

which, I conceive, to proceed from the ſame Cauſe, being my ſelf hairy, and ſhedding them yearly, as, I conceive, moſt Men do; this I obſerved in two Parts of my Body, alſo in three Places of my Hand, where I have ſhorn off the Hair, and found, that ſome Hairs grew, others not; ſome fell out, and I could pull them out without Pain; alſo, that theſe, which fall out, have thin ſharp Roots; thoſe which ſtay, thick; alſo ſuch, as have no Hairs on their Body, have Pores, and an iſſuing Matter, not ſo fit for Hair. This appears like black Specks, and are ſuppoſed Worms; and ſome Doctors of *Aken*, did preſcribe this Man to ſtand with his Back to a Fire made of Oak, and anoint his Body with Honey, that by Means of the Sweetneſs and Warmth, the Worms may come out, and ſo be cut off with a Razor, as the Gentleman Patient himſelf told me; hereupon I try'd to preſs, both out of my own, and out of another Man's Noſe, theſe ſuppoſed Worms, which ſeem'd, from their Shape, much to favour the Opinion, ſeeming to have a Head which proceeded from that Part of the Hair, which was next the Air, it being browner than that within the Skin, but no two like one another. I obſerv'd all its Parts, but found nothing like an Animal; but in ſeveral I found ſmall Pieces of Hair, ſome 25, others 100 Times thinner than a common Hair. Hence I concluded, the ſuppoſed Animals are only the Places of thoſe Hairs fill'd with the uſual Food of Hairs; my Opinion is confirm'd by new-born Children, over-grown with Hairs, which, I ſuppoſe, from too much Nouriſhment, grow hairy, but when they want that Supply, the Hairs fall out, and grow not again.

In a Looſeneſs I view'd my own Excrements, and took notice of what I eat and drank; it conſiſted of clear, yellow, roundiſh, irregular Particles, alſo of vaſt

Quantities

Quantities of Globules, like thoſe of Blood, ſix together equal to $\frac{1}{3}$ of the whole; others but $\frac{1}{12}$ of a Blood Globule: Theſe I found in a tranſparent Liquor, in which were many Animals, as big as a Globule of Blood, their Bodies oblong and flat, with many Feet underneath, with which they moved quick; like a Piſs-a-bed againſt a Wall, tho' they moved their Paws quick, yet they went but ſlow. Once I found but one in the Bigneſs of a Sand, at other Times, 4, 5, 6, 7, or 8. I have ſeen other ſhap'd Animals, (but of the ſame Bigneſs) like River Eels; theſe were very numerous, and ſo ſmall, that 5 or 600, extended in Length, would not reach the Length of a River Eel; theſe wriggled like a Snake, very quick, like a Pike ſhooting through the Water. At another Time, I ſaw Multitudes, 200 Times leſs than a Blood Globule, the *Axes* being about one to ſix, and I am confident, I have ſeen above 1000 living Animals, in the Bigneſs of a Sand, ſwiftly moving, and of three or four ſeveral Sorts. Some have thought, theſe Animals might paſs into the Blood; but, I conceive, the Paſſages of the Blood are ſo ſmall, that though the Animals were 1000 Times leſs, they could not paſs. My ordinary Excrements, mixed with a clear Liquor, had no Animals; but when thinner than ordinary, it had. I found alſo Parts of the Food I had eaten, undigeſted, as the Pipes of Aſparagus, the ſofter Parts being digeſted.

This Summer, in our Meadows, I have obſerved the Dung of Cows, Horſes, *&c.* freſh, but found no Animals. It conſiſted of Multitudes of Globules, ſome $\frac{1}{8}$, others $\frac{1}{12}$ of a Blood Globule, in a clear Liquor. In *May* laſt, riding my Mare hard, I obſerved the laſt thick Part of her Urine, and found, the thick Aſh Colour of it was cauſed by a great Variety of differing Globules, ſome as big

big as those of Blood, and these composed of six. The first of these were like a close-grown Bunch of Grapes, and though not perfect round, yet I call them Globules.

Dr. HOOK's *Letter to Dr.* TRAPHAM, *of Enquiries for* Jamaica, *Feb.* 18. 168½.

SIR,

IT will be a great Obligation to the *Royal Society*, if Dr. *Trapham*, or any other ingenious and knowing Person in *Jamaica*, will please to communicate any curious Observations they shall make, concerning any Part of Nature; as concerning the Temperature and Qualities of the Air, the Seasons, Winds, Storms, Hurricanes, Rains, Hails, Dews, Mists, Fogs, *&c.* the Heats, Colds, *&c.* of the Seasons; the Qualities of Springs, Rivers, Lakes, *&c.* the Description of any of the Animals, Birds, Beasts, Fishes, Serpents, Insects, or of any of their Qualities or Uses, for Food, Physick, Pleasure, *&c.* The Description of their Vegetables; as of their Herbs and Shrubs, whether of the Land or Sea; of the Trees; their Use in Food, Physick, Building, Dying, Perfuming, Firing, Joinery, Turning, Bows, *&c.* The Description of any of their peculiar Stones, Minerals, Ores, Metals, Clays, Earths, Sands, *&c.* of what Nature, what Use made of them, *&c.* Also to inform them concerning any accurate Observations, that have been made of any Eclipses of the Moon, and particularly that of the 11*th* of this Instant *February*; of the Variation of the magnetick Needle, from the Meridian, or North Point; of the Times of the Tides, both Spring and Neap, and of the Height

Height it riſes; of the Currents, what, when, which Way; of the Depths and Soundings of the Seas thereabouts, and whatever of this Kind ſhall be communicated; or if any curious Jewels, Shells, *Seeds*, *&c.* ſhall be ſent, the Society will not only pay the Charge of Freight, but any other way gratefully acknowledge the Favour that the Communicator ſhall deſire, either by recording it in their Regiſters, or publiſhing it in their Hiſtories.

Mr. Lewenhoek's *Letter to Mr.* Oldenburg, *receiv'd from Dr.* Crowe, *Aug.* 14. 1682. *Of the Fibres of the* Muſcles, Dura Mater, Brain, *and* Moxa.

Excellentiſſime ac Eruditiſſime Vir.

GRATISSIMAS, præteritæ menſis decima prima ad me datas, literas accepi, in quibus humanitatem Nobilitatis Veſtræ, dominorumque philoſophorum viſam, grato animo agnoſco.

IN literis 22da Februarii ſcriptis, nobilitas veſtra inquit, amicorum quoſdam optare, ut ſumma cum exactitudine obſervarem fibras muſculorum carneas, ut & corticem, medullamque cerebri.

IN literis meis, *Anno* 1674. prima Junii datis, dixi: Fibras muſculorum carneas ex valdè parvis conſiſtere globulis: Sed quo nobilitati veſtræ reliquiſq; amicis magis ſatisfaciam, omnes præcedentes meas obſervationes rejeci, firmiterque propoſui, de novo, clarè ac perſpicuè eas, oculis meis mihi perſpiciendas, ſumere.

INTER alias, *carnem vaccinam* accepi, quam, acutiſſimo cultello, in fruſta conciſam, per microſcopium â membranulis ſuis ſeparavi, quo perracto, tum primum mihi nudè ac dilucidè apparuit, tenuiſſima illa membranula, cui fibræ carneæ quaſi involutæ, aut intextæ jacent, cujus etiam *Anno* 1674. prima *Junii* in literis meis memini, dicens: Membranulas illas ex tot ſtriis ac fibris conſiſtere, quaſi nudo oculo omentum alicujus beſtiæ aſpiceremus. Eaſdem membranulas jam propius obſervans, totas illas ſolummodo conſiſtere ex fibris tranſverſim inter ſe mixtis, comperi, quarum quædam, in oculo meo, decies, vicies, & tenuiſſimæ quinquagies tenuiores pilo. Cogitabam, num quædam craſſioris generis, quæ in ramos ſe diſpergebant, non eſſent vaſa lymphatica.

SUBLATIS, à prædictis fibris carneis, prænominatis membranulis, eas nudè ac perſpicuè vidi, quæ in hac carne erant ad craſſitiem communis capilli. Ubi ſpiſſæ ac denſæ, rubicundæ erant; ubi tenues ac diſperſæ jacebant, magis apparebant pellucidæ.

VARIA obſervandi methodo uſus ſum, particulas carnearum harum fibrarum videndi, perpetuoq; inveni, eas ex talibus compoſitas partibus, quibus aliam quam globulorum figuram appropriare nequeo. Imo & minima fibrarum carnearum fruſtula, grano arenæ aliquoties minora, coram viſu meo in plurimas diviſi partes. Præterea etiam obſervavi, carne adhuc recenti & humidâ, quod, tum compreſſis vel fricatis carnis globulis, illi globuli reſolvantur & conjungantur, quaſi oleaginoſam, vel aliquo modo concretam, videremus, materiam aquoſam.

HI globuli, ex quibus fibras carneas conſiſtere dixi, adeo exigui ſunt, ut (juxta oculum, meumq; viſum, judicium ferens) dicam 1000000 non confecturos

fecturos unici arenæ grani, aliquo modo grandioris, quantitatem.

Et quamvis in mentem veniat, me antea Nobilitati Veſtræ ſcripſiſſe, particulas, ex quibus caro, adeps, oſſa, capilli, &c. conſiſtunt (quæ a me globuli vocantur) non eſſe veros & propriè ſic dictos *globulos*, ſed figurâ globulis proximos, eadem tamen hic repetam: Ex. gr. Imaginetur quis ſibi, ſe magnam veſicarum ovinarum, vel aliarum, aquâ repletarum habere quantitatem: Hæ veſicæ quamdiu, ab omni parte, ab aere circumdantur, rotundæ erunt: Sed imaginemur nobis, eas promiſcuè & indiſcriminatim in vas aliquod injici: Quo facto, veſicæ, globoſam, quam in aëre habuerant, rotunditatem, non ſervabunt, ſed a ſe invicem compreſſæ, nullum vacuum (ſic loquendo) locu relinquent: Et ſic quævis veſica aliam, ob flexibilem ſuam mollitiem, accipiet figuram. Sed quæ in vaſe ſupremæ jacebunt, in quantum ab aëre amplectuntur, globoſam retinebunt rotunditatem: Idem de globulis carnis, propter eorum mollitiem, ſit judicium.

Piam Matrem obſervavi, comperique membranam hanc, variis ſanguinis vaſibus intertextam, præter ea quæ nudo oculo, cerebro injacere, cernere poſſumus, (præcipue ſeparatione Piæ Matris & cerebri facta,) & inter ea venæ admirandæ & incredibilis tenuitatis: Et quantum dijudicare poſſum, membrana illa ex admodum exilibus conſiſtit fibris. Ulterius vidi, prædictas multiplices venas, per membranam hanc diſperſas, ramos ſuos per cerebri ſubſtantiam quoque diſpergere: Eo modo, ac ſi nobis imaginaremur diverſas & ſuperficiei terræ palmitibus ſuis injacentes vites (quas venis Piæ Matris comparo) eaſque ubique ex palmitibus ſuis radices, in plurimos diſperſas ramos, alte in terra egiſſe. Terram hic mihi imaginor ſub-

ſtantiam cerebri, & radices, venas per cerebri ſubſtantiam diſperſas.

Accedens jam ad partes ipſius cerebri, adhuc affirmarem, id, præcipue ubi paululum compreſſum ac compactum, non niſi ex globulis, & non ex aliis conſiſtere partibus: Sed ubi rarum ac tenue, cultro conciſum aut ſeparatum, ſeſe oſtendebat dilucidiſſima materia, quaſi oleum fuiſſet, quam videns imaginabar mihi cultro id cauſatum, globuloſq; cerebri diſruptos aut fractos. Verum enimvero perſeverans in obſervando, non tantum beſtiarum, ſed & piſcium, & præcipue quidem Aſelli majoris cerebrum, clare perſpexi materiam illam oleaginoſam, non fuiſſe cultro ex diſruptione globulorum cauſatam, ſed reverà eſſe materiam ſeparatam, cui prædicti cerebri globuli quaſi injacebant. Ulterius vidi, ſed clariſſimè in cerebro Aſelli majoris, prædictam oleaginoſam ſubſtantiam, reapſe etiam ex globulis, ſed multo minoribus, quam ipſius cerebri, conſiſtere.

Primo nominati globuli cerebri, meo judicio, circumcirca, globulis ſanguini ruborem afferentibus (ex quibus ſanguinem conſiſtere dixi) magnitudine æquales ſunt. Hi majores globuli, ex maxima parte cerebrum conſtituentes, reſpectu globulorum ſanguinis, valde irregulares vel inequales exiſtunt. Hujus rationem exiſtimo vel globulorum firmam inter ſe, aut cum vaſibus conjunctionem, vel eorum mollitiem, adeo ut ſe ſeparari non ſinant, quin (ſic loquendo) a ſe invicem diſcerpantur, ubi e contra globuli ſanguinis in fluidiori materia moventur, & propterea etiam, globoſam ſuam rotunditatem, quando in latiori ſpatio exiſtunt, retinent.

In animum ſubit, me antea temporis obſervaſſe cerebrum *Anatis*, & tum judicaſſe, cerebrum ex parte conſiſtere ex filis, aut admodum exilibus vaſibus

vaſibus. His filis vel vaſibus poſtea mihi ſæpius occurrentibus, tum temporis & idem judicabam, ea tantum produci per firmiſſimam globulorum (ex quibus cerebrum ſolummodo conſiſtere putabam) inter ſe unionem, & qui minima extenſione ſic in fila mutarentur. Sed obſervationes meas per integrum menſem continuans, clare admodum vidi, multiplices valde, & ſupra modum exiguas venas (de quibus antea certus eſſe non poteram) eas in beſtiarum cerebro exiſtere, & revera venas eſſe, licet cognitu admodum difficiles. Verum obſervante, exactiuſque inſpiciente me Aſelli majoris cerebrum, multiplicia illa minima vaſa, aut venulas, quæ ſupra modum pellucidæ, clare mihi oſtendi: & multas, licet in ramos diſperſas, & quindecies vel vigeſies filo bombycis exiliores, tamen cognoſcere potui: Horum dictorum vaſorum vel venarum maximam multitudinem, in quantitate cerebri ad magnitudinem arenæ, vidi: Præterea & vaſa ſanguine repleta, vel quæ rubicunda apparebant, ut etiam vaſa ad craſſitiem unici fili bombycis, & inſuper pellucida vidi.

Hasce meas obſervationes circa beſtiarum cerebrum perſequens, vaſa ante nominata, admodum perſpicue quoque oſtendere potui, eaque ſumma cum admiratione vidi, partim ob ingentem multitudinem, partim ob ſupra modum ſummam eorum exilitatem. Si enim juxta oculum meum judicium feram, dicere teneor, quod, ſi globulus, ſanguini ruborem afferens, in octo eſſet diviſus partes, & unaquæque octava pars eſſet firma & ſolida, ne una quidem harum partium hæc vaſa tranſire poſſet. Et quamvis diverſis vicibus prædicta cerebri vaſcula mihi perſpicue ob oculos poſueram, in obſervationibus meis circa illa tamen continuavi, & quo penitius ac ſæpius obſervarem, eo exactius admodum multiplicia illa vaſcula, cum ipſorum ramis (qui adeo infirmi ac debiles, ut minima contrectatione diſrumperentur) dignoſcere potui.

INTER dictos globulos, ex quibus cerebrum ex parte conſiſtit, globulos ſanguinis jacentes vidi, qui, ob perfectam rotunditatem, clare a globulis cerebri diſtingui ac dignoſci poterant: hos ſanguinis globulos opinabar e ſanguinis vaſibus per cerebrum diſperſis, & cultro conciſis, effluxiſſe.

INTER corticem & medullam cerebri, aut parvam, aut nullam fere, differentiam, obſervare queo: præſertim cum paululum rariorem, & tenuiorem, eam mihi videndam ſumo: tantum dicam, venas, aut vaſa corticem cerebri permeantia, aliquo modo ſubfuſci vel ſubnigri eſſe coloris, ubi e contra vaſa medullæ cerebri erant dilucida ac pellucidiora.

IN cerebro, ſed plerumque in cortice, tam exiles ac rubicundas, ex majoribus procedentes, venulas vidi, ut capere nequeam, quomodo globuli ſanguinis eas permeare poſſint: & ultra, quo pacto globuli paulo rariores, & ſeparatim obſervati, ferme nullius ſaltem admodum modici eſſent coloris, ubi e contra ſanguis in hiſce vaſibus ruberet. Imo & per ipſas venas, in ſubſtantiam cerebri proximam, color ille rubicundus penetrarat, eamque infecerat. Sed animo revolvens, me in obſervandis Pediculis ſæpe vidiſſe, quando Pediculum eſurire feceram, ipſique prope fame confecto, jam ſanguinem ſugendum darem, ipſum non potuiſſe conſumere ſanguinem, aut etiam ejicere; quo evenit, ut globuli ſanguinis rubicundi liquefierent, & in materiam fluidiorem reſolverentur, & ſic per totum Pediculi corpus, imo per ipſas ungulas & cornua diſpergerentur, omnibuſque partibus ruborem afferrent. Cauſam non conſumpti ſanguinis opinabar, inteſtini aut parvarum in Pediculo venarum exſiccationem, defectu alimenti cauſatam: quo debitus ac ordinarius ſanguinis motus fuit impeditus, nec juſto modo per totum corpus vehi potuit. Sed memini, hanc ſanguinis mutationem, in ſanguine, in vitro per aliquod tempus, ſervato, aliquando etiam a me obſervatam.

Et

Et idem in parvis cerebri venis accidere poſſe opinor (quamvis adeo exiguæ ſint, ut globulus, rotunditatem ſervans, penetrare nequeat) ut reſolutis globulis, & venæ rubræ appareant, & cerebrum adjacens rubore tingatur.

MEDULLAM ſpinalem Vituli, Ovis, Gallinæ, ac *Aſelli* majoris etiam obſervavi, quam ex iiſdem cum cerebro partibus conſiſtere comperi, cum hac ſolummodo differentia, quod præter globulos, quos cum cerebro ſpina medulla communes habet, in hac ingens globulorum oleaginoſorum & pellucidorum numerus, ac diverſæ magnitudinis jaceret. Quidam enim quinquagies majores reliquis, ac præterea admodum molles, ac fluidi. Cæterum medullæ ſpinales multis ac ſupra modum tenuibus inſtructi erant venis aut vaſibus. Præterea hic per medullam ſpinalem diſperſæ erant fibræ coloris ſubfuſci, & ad craſſitiem capilli, quædam vero tenuiores: quibus viſis imaginabar mihi in initio, num quævis fibra forſan non eſſet vena: ſed ſumma cum exactitudine penitius inſpiciens atque obſervans, comperi, quamvis fibram non eſſe vas, ſed ſingulas earum conſiſtere ex aliis valde exiguis fibris aut vaſibus ſibi invicem adjacentibus, inter quas fibras pellucidiſſima videre erant vaſa ad craſſitiem fili bombycis. Hic tum opinabar, an hæc vaſa non eſſent ea, quæ ſpiritibus animalibus per medullam ſpinalem vehendis inſerviunt.

HÆC ſunt, clariſſime ac nobiliſſime vir, quæ poſt ultimos, indefeſſos, & exactiſſimos labores, hucdum in cerebro, *&c.* detegere valui.

UTI dixi antea, quo pacto multæ venæ ſibi invicem adjacent conjunctæ quaſi una tantum eſſent vena, ſic illud mihi non tantum occurrit in medulla ſpinali, & interdum quoque in cortice cerebri: Verum etiam in fructibus, & ſeminibus, præſertim in Caſtanearum venis. Ut & in cortice & putamine

tamine Amygdali: in ſecunda nigri piperis membrana; In putamine Avellanæ nucis duro, & membrana quæ intus in concavo ei adhæret, & in molli cortice cui nucleus injacet involutus: ubi quidem 15 aut 20 tenuiſſima vaſa ſibi invicem adjacentia vidi. Etiam in membrana nucleum Juglandis immediate amplectens. Omnia hæc vaſa ex continuata tortuoſitate compoſita ſunt, eo modo ac ſi nobis imaginaremur tenuiſſimumaliquod filum æreum aut ferreum craſſiori preſſim circumvolutum (in formam qua fuſtis vel baculus fiſſus iterum fune colligatur) poſtea extracto craſſiori filo, tenuiſſimum illud quod ei circumvolutum fuerat, omnes gyros ac circumvolutiones retinebat. Eodem modo (ut dixi) tenuiſſima in prænominatis ſeminibus & fructibus vaſa contorta vidi. Præterea in Malo & Piro tenuiſſima ſibi invicem adjacentia vaſa obſervavi.

Annus jam præteriit, cum in ædibus ſuis, nobilis dominus Conſtantinus Hugenius a Zulichem, mihi monſtraret *Moxam*, addens, quo pacto inuſtione iſtius herbæ podagra ſanaretur: Aliquantulum hujus ſic dictæ herbæ Moxæ mecum domum retuli, carpoque manus impoſitum juxta præſcriptum urendi modum, combuſſi (ex curioſitate nimia, nam podagra non divexor) quo extraordinariam hujus combuſtionis effectum detegerem, obſervavi autem cuti, in loco uſtionis, injacere materiam flavam ac oleaginoſam, quam principio judicabam pec combuſtionem cutis cauſatam. Verum hanc cutis inuſtionem intermittere coactus fui, non ob dolorem, ſed ſanationis difficultatem: ſi enim tam facile ſanare poſſem, ac vulnus ex inciſione cultri, (quod colligatum ac conſutum ſanatum æſtimo) ſæpius hanc inuſtionem iterarem. Per microſcopium Moxam examinavi, firmiterque ſentio Moxam non eſſe herbam ex optimæ terræ pharmacis artificioſe paratam, ut autumat dominus Buſſchoff in tractatu de Moxa p. 52 ſed ſolummodo vaporem

porem aliquem ejectitium alicujus fructus, ſicuti in malis Perſicis, Cydoniis *&c.* lanoſam videmus ſubſtantiam cortici adhærentem. Cogitaram etiam me de fructibus quibuſdam collecturum herbas moxæ quodam modo ſimiles, ſed hucuſque efficere non potui.

Moxa, quoad figuram, goſſypio reſpondet: ſicuti enim inter pilos, capilloſve, & lanam, nulla, niſi quoad craſſitiem & longitudinem, differentia, utpote ex globulis conſiſtentes,& ad rotunditatem inclinantes: æque parva inter moxam & goſſypium differentia, & illa & hoc enim duobus planis gaudent lateribus. Eandem figuram, lanoſum illud quod interne rubri corticis caſtaneæ convexo adglutinatum, oſtendit: in hoc tantum differens, quod moxa multo ſubtilior ſit goſſypio, hoc caſtaneæ lanoſitate. Moxam, cum inuſtio manus non placeret, juxta & goſſypium, forfice parumper diſſectum, quo facilius ignem perciperet, chartæ anguſtæ impoſui, & hæc moxæ & goſſypii combuſtio ſibi invicem exacte reſpondebant, adeo ut mecum ſtatuam, ſi inuſtio quendam, circa ſanationem podagræ, producat effectum, illud non evenire per aliquam moxæ propriam qualitatem, ſed tantum per inuſtionem ipſam, & ſi goſſypio inuſtionem faceremus, nos tantum effecturos quantum moxa.

Ulterius moxæ, goſſypii & lanoſitatis caſtaneæ æqualem ſumſi quantitatem, quam juxta ſe invicem poſita combuſſi, comperique quodvis horum trium poſt ſe reliquiſſe materiam aliquam oleaginoſam, ſed moxa plurimam; cauſam imaginabar, quod, quamvis quantitas moxæ quoad oculum non major, revera plus materiæ eſſet in moxa, utpote quæ ſubtilior molliorque, goſſypio arctius conjunctas haberet partes, & propterea majorem olei quantitatem poſt combuſtionem reliquerat. Adeo ut credam dominum Biſſchoff a Chinenſibus moxæ qualitates, præparationemque extollentibus, eſſe ſeductum ac deceptum. Etiam

ETIAM animo recolens commune chirurgorum dictum, goſſypium (ut Holl. dicitur) eſſe ignitum, hoc eſt, inflammationem cauſare, & noxam afferre vulneribus, quando iis colligandis applicatur. Malignitatem, goſſypio adſcriptam, in hoc conſiſtere judico, *viz.* quod, ut antea dictum, duo plana, & per conſequens quævis particula, duo acuta habeat latera. Hæc acuta latera tenuiora, ſubtiliora & duriora globulis fibrarum carnis, propterea (cum goſſypium vulneribus applicatur) non tantum caro adhuc ſana, ſed materia incarnationi novæ inſerviens, & molliores carne ſana globulos habens, vulneratur ac læditur imo conciditur & reſolvitur. Sed contrarium cum linteo evenit, utpote cujus partes rotundæ & arcte ſibi invicem junctæ, majus corpus efficiunt, ideoque globulos carnis & materiæ incarnationi inſervientis tam facile non, aut in totum non lædunt.

HÆC ſunt, nobilis vir, quæ excellentiæ veſtræ dominiſque philoſophis hac vice per literas nunciare volui: Submiſſe & ſubnixe rogans, nobilitas veſtra velit dominis philoſophis multam meo dicere nomine ſalutem, dataque occaſione, reſcripto, has bene perlatas, & quo pacto hæ meæ obſervationes aut conveniant cum antecedentibus, aut in quantum (ſi) ab illis diſcrepent, ſignificare. Nunquam occaſioni deero, qua demonſtrare potero

Excellentiſſime Clariſſime Vir,

Quod Sim Nobilitatis Veſtræ

Addictiſſimus Cultor,

Subſignaverat

ANTHONIUS LEWENHOECK.

Dr. John Carte's *Letter to Dr.* Hook, *of Worms like* Millepedes, *in the Stomach*, &c.

SIR,

I SEND you the following Caſe, which, in ſome of its Circumſtances, is not very common: A Girl about eight Years old, who has never been very healthful, but of late hath looked more pale than ordinary, and troubled with Pain at her Stomach, yeſterday, upon taking a purging Powder, vomited a Sort of Inſects, to the Number of about a Hundred, very much reſembling little *Millepedes*; I ſaw ſome of them, and three, that were living, I put in a Box, and a little Duſt to them, but they followed the Fate of the reſt, and died preſently; I have ſent you ſix of them. The Child had taken Worm-Seed over Night, but had a very troubleſome Night, could ſcarce be held a Bed, complaining both of the Pain and Soreneſs of her Belly, fancying the Worms had eaten it thin in one Place, and would eat a Hole in it. The Length of one of the biggeſt, (though there was but little Difference) was $\frac{1}{4}$ of an Inch: I view'd them through a ſmall Microſcope, which did not repreſent them ſo clearly, as to diſtinguiſh them from the common Wood-Lice, only their Bellies were more tranſparent, and their Heads of a more confuſed Figure, which laſt I thought afterwards might be cauſed by the rowing up of the *Antennæ* or Horns, which I obſerved ſome of the common *Millepedes* to do, when they die. That among them which was black, was accidentally ſo, by dropping a little Ink upon it.

THE Child, after her vomiting, had a Stool, in which were ſeveral very ſmall white Worms, about an Inch long, which are not uncommon, but ſhews

ſhews that the *primæ viæ* abounded with ſuch putrid Humours, as are uſually productive of a verminous Brood: She is now very hearty, and eats her Meat well, and free from all the former Symptoms.

I HAVE heard ſome Stories of the like Nature, but am not forwards to relate them, becauſe they totally depend on the Credit of others: One Man I know, who, many Years ago, was reduced to a thin conſumptive Habit, and, upon taking *Mercurius Dulcis*, voided by Stool an incredible Number, or rather Quantity, of ſmall Animals, which (according to the Deſcription I had of them) were leſs than theſe, and of a rounder Figure.

IF theſe were bred in a *Folliculus* of their own, that Part muſt apoſtemate, and ſo a purulent Matter be evacuated with them; but I rather think, they muſt be generated in the common Paſſage, and I remember I have often ſeen Abundance of Animals bred in humane Excrements, but was not ſo curious to obſerve their Figure.

IT is hard to imagine, how Worms ſhould live in the Stomach, amidſt that acid Humour, which, whether it be the Cauſe or Effect of Digeſtion, has the Force of a *Menſtruum*; but it muſt be ſuppoſed, that in ſuch Bodies, the Ferment is alter'd, if not deſtroy'd: You obſerve lately, that Birds are very induſtrious to kill Inſects before they eat them; I am apt to think, if they paſs'd immediately into the Gizard, there was no Need of killing them firſt; but the *Ingluvies* ſupplying the Want of Teeth, and only macerating what other Creatures chew, has no Acidity that would offend them.

SIR,

SIR, I write this Account haſtily, becauſe I would have you ſee them as ſoon as might be.

Mancheſter,
Auguſt 25,
1682.

I am,
SIR,
Your humble Servant,
J. CARTE.

N. B. *The Child had not taken any* Millepedes, *nor uſes to eat Earth or Dirt, which I have known ſome diſtemper'd Children do.*

A Letter from Mr. J. YONGE, *to Dr.* HOOK, *of divers curious Matters obſerv'd by him.*

A WOMAN, about 36 Years old, had from her Childhood been ſickly, more eſpecially tormented in her Belly with a Pain, accompanied at firſt, every three Months, and afterward every three Weeks, with a round Swelling like her Fiſt, in her left *Hypocondria*, ſenſibly moving to and fro', and plainly to be felt: Horrid Pain would then deprive her of Senſes, twelve or twenty-four Hours; and then ſhe would recover again, be without Pain, and the Tumour vaniſh, without being followed by any Evacuation, of either Wind, Water, Excrement, *&c.*

THOSE Paroxyſms, for many Years, kept a due Courſe of three Weeks; ſhe was generally coſtive, found that Milk irritated her Pain, that Fleſh and all ſalt Meats diſagreed with her.

NOT-

NOTWITHSTANDING this, she married about twelve Years since, and had one Child. During her Breeding, her Pains observ'd the Course, and abated nothing of their Vehemence, which equall'd, if not exceeded that of Child-birth.

UNDER this Plague she liv'd, till about *February*, 1680, the Pain seem'd fix'd on the left Side, on the Region of the Spleen, and seem'd as if proceeded from the Lodging of some heavy Thing, and begot such Pain, as she could not lie down in her Bed. Thus she continued in a miserable Condition, using Purges, Clysters, *&c.* which were advised by charitable People, she being very poor. The 15th of *November*, 1681, she became quit of all the Pain in her Side, and then felt somewhat to burthen, and, as it were, stop the *Intestinum Rectum*, causing frequent Motions to Stool, but no Evacuation, but a little Slime like a *Tenesmus*. The Suppression of her Evacuations that Way, for six Days, so press'd on some of the urinary Channels, that her Urine also stopp'd. In this doleful Condition, she sent for me, when, giving me the abovesaid History, I guess'd somewhat extraordinary must be in the *Rectum*. Accordingly, examining by a Probe, I felt a hard Substance like a Stone, which, with a strong Pair of *Forceps*, I extracted, and then cleansed out the Bowels with a Clyster; she remained void of any Pain, and is so to this Day.

THE Thing extracted, was of a round Figure, somewhat oblong, with some Depressions, such as a Man's Fingers make on Pitch, Plaister, or Wax. In Weight, was one Ounce and a Quarter; was five Inches round, swam on Water, though seem'd a Stone. Its Outside was black as Jet, smooth as Varnish, but no thicker than a Man's Skin; next to it, it was stony, or gritty, like Brick, the Thickness of half a Crown. After some Months, I cut

cut it in two with a Hatchet, and found that next to the gritty Shell, it was full of a woolly, hard Subſtance, like rotten Rags, or Sponge, or chew'd brown Paper, within which, lay a Lump of the Bigneſs and Form of a ſmall Prune. Cutting that in two alſo, I found it a Prune, or Plum indeed, the Pulp of which was dry, and hard as Paſteboard, as was the Kernel in the Shell, that lay in the Middle of it.

Whence it's manifeſt, that all theſe Accidents, that had ſo long moleſted this poor Woman, proceeded from this Plum, or Prune, ſwallowed above thirty Years before; which, probably, ſtuck in ſome folding of a Gut, or a Cavern, or Cell of the *Colon*, increaſing its Dimenſions by the Adheſion of new Matter, till (no one knoweth how) it tumbled down to the *Rectum*, and I drew it forth. But how the Surface became petrify'd, and ſo uneven, and varniſh'd over with a black ſmooth Matter, is to me a Wonder.

Before I broke it, I thought it might be a Gall-Stone, (tho' ſhe never had the Jaundice) having lately ſeen a Gentlewoman, almoſt dead in that Diſeaſe relieved by the Evacuation of one, almoſt as big as a Pullet's Egg, and another from a Man, as big as a Nutmeg. Both followed (tho' coſtive before) with a Lask, diſcharging prodigious Quantities of Choler. The Authors are innumerable, that mention this latter Sort, though I meet none ſo great, *Vide La. Riverius Obſ. ab Henrico Ruffeo com. obſ.* 4. *Tho. Bartholin Acta Med. A.* 71, 72. *obſ.* 100. *J. Fernelius lib.* 6. *de part. Morb. & Sympt. J. Skenckius, Obſ. Med. Sennertus, &c.* But few ſpeak of any, that appear generated in the Guts, *Vide Miſcel. Curioſa vol* 6. *obſ.* 20.

There

THERE lately died, in *Cornwall*, a Woman of about 154 Years of Age; I have employ'd a Friend to give me a particular Account of her Manner of living, *&c.* which I will not fail to tranſmit to your Hands.

HERE was lately, alſo, an Ewe kill'd, that had a full grown Lamb lapp'd up in the *Omentum*, among the Guts, without the Womb; queſtionleſs it was a Conception *in tuba Fallopiana*, which, when growing big, broke forth into the Bowels. But that the *Pedunculus* ſhould hold, and where the *Placenta* was faſtened, is ſtrange: In the *fundus uteri*, it cou'd not be, and if any where elſe, how was the nutritious Juices, *&c.* conveyed to it. It was ſeparated from the *Uterus*, and the Bowels thrown away before I knew it, ſo that I could not make that Examination: This Accident is not ſo new, but that Inſtances of the like are given by Monſ. *Bayle*, Mr. *Blegny*, *de Graeff*, *Elſchotius*, *Riolanus*, *Rheynhuſe*, &c.

A CHILD was lately heard, by ſeveral People, to cry in its Mother's Womb, ſome Days before the Birth; do not Children then breathe by the Lungs, before they are born?

I find ſuch another Relation (if not the ſame) of a Lamb in the Omentum, *told by Mr.* Younge, *in the* Phil. Tranſ. *Numb.* 323.

WILLIAM DERHAM.

Obſer-

Obſervata quædam Anatomica in Veſpertilione *diſſecto 22 die Sept.* 1682. *Per* T. Molyneux, *M. D.* Dublinij.

EXTERNAM hujus animalis figuram verbis deſcribere, ſupervacaneum fore exiſtimavi, utpote cum in hiſce noſtris regionibus adeo frequens occurrat Veſpertilio, ut cuique volenti, eum vivum intueri, facillime obtigit; vel ſaltem omnibus conceditur, ut illius vivam aſpiciant delineationem, cum apud tot varios autores de animalibus ſcribentes, hoc accurate depictum invenire liceat. Iis igitur omnibus omiſſis, quæ alii de Quadrupede hoc volanti jamdudum tradiderunt, ſolummodo hic notabimus quædam hactenus neglecta & inobſervata quæ in illius diſſectione nobis videre contigit.

Et primo *Penis* in conſpectum venit, inſignis quidem magnitudinis, habito reſpectu ad exiguum animalis corpuſculum; in eo Officulum hujus figuræ (I.) æmulum delituit, officulum in Murino penè contentum longitudine duplo ſuperans.

Teſticulos habuit ſatis amplos, extra abdominis cavitatem prominentes.

Veſiculæ Seminales, ex utroque latere *Veſicæ Urinariæ* ſitæ, ſemine mirum in modum turgidæ conſpiciuntur, Phaſeoli magnitudinem æquantes.

LONGITUDO omnium *Inteſtinorum*, ſcilicet a Pyloro uſque ad anum vixdum 6. pollices æquabat; at in Mure diſſecto (cujus ſimilitudinem ex omni animalium genere maxime præ ſe fert.) Inteſtinorum circuitus 21 pollices ſuperavit, nullâ habita ratione illius appendicis inteſtinum *Cæcum* dictæ, quo omnino caret Veſpertilio; cujus Inteſtinorum brevitatem, notabilem levitatis gratiâ, a Naturâ conſtitutam eſſe opinor, quæ ab hoc Quadrupede, per aerem volitare deſtinato, quicquid eſſet oneri provide deſumpſit.

Ventriculus, *Lien* & *Renes* iiſdem partibus in Mure omnino perſimiles ſunt; at Hepar & Pulmones in duos duntaxat lobos dividuntur.

PENITUS mortuo animale *Cor* motum ſuum, *viz.* Syſtolen & Diaſtolen, amplius horæ ſpatio peragebat.

Oculi inſigni convexitate donantur; eos autem in hunc finem ita fabricatos ſuſpicor, ſcilicet ut in tenebris videant; quippe per ſolam noctem & opaca crepuſcula prædam ſuam (Muſcas ſcil.) animal hoc infectatur, quas inter volandum Hirundinis ad inſtar captat.

Blaſius in ſua Anatome diverſorum animalium, ubi de Veſpertilione loquitur, hanc controverſiam inter quoſdam Medicos natam, meminit; ſcil. an *Caudam* habeat; ſed de re ipſis ſenſibus adeo evidenti ortam eſſe contentionem magnopere admiror, quippe æque bene diſputaſſent, an Mus Caudam habeat, cum in eo non magis manifeſtam *oſſis Coccygis productionem* (quæ in omni animale *Cauda* nominatur) quam in Veſpertilione aſpicere liceat.

ANIMAL eſt *Viviparum*, & nihil commune ullæ Volucrum ſpeciei poſſidet, exceptis *alis* & robore *Muſculorum pectoralium* alas moventium; quippe nec Bipes eſt, nec Pennatum, nec Roſtratum, *&c.* ſicut omnia volantium genera: quamobrem a *Clariſſimo Willughbeo*, in ſuo pereleganti libro de Avibus, inter Aves nequaquam numeratur, licet alii ſcriptores Veſpertilionem inter eos collocare haud dubitaverint.

DUM in vivis eſſet animal, in Pixide lignea incarceratum per ſpatium quatuordecem dierum aſſervabam, quo tempore Muſcos ex omni genere & Araneas avide comedebat: * *Corporis* autem *Situs*

* *I have ſeen him in this Poſture aſleep, above forty ſeveral Times.*

tus (quem ſemper eligebat quoties ſomnum caperet) ſingulare quid & inſolitum videtur; quippe ſpreto molli gramine in fundo ſuæ Caveæ ſubſtrato, pixidis lateri adhæreret, & poſterioribus ſuis partibus directe elevatis, anterioribus autem & Capite perpendiculariter deorſum poſitis, ſuſpenſus ſemper quieſceret: in hac autem inſolita poſitura corpus ſuum ſuſtentaret *poſteriorum pedum* beneficio, quorum uterque quinque digitis, acutiſſimis unguibus armatis, inſtructus eſt, & ab his ligneæ pixidis lateri infixis, pondus totius Corporis tuto dependebat; *anteriores* autem *pedes*, unico tantum digito inſtructi, ad illius ſuſtentationem in hoc ſitu nequaquam contulerunt.

Si quis hujus animalis *Oſtiologiam* cupiat, conſulat Cap. 26. Partis Secundæ Anatomiæ Blaſianæ variorum Animalium, & Tabulam 41. ubi Veſpertilionis *Sceleton* & *Effigiem* videre licet.

The Rule of False Position, in Dec. 1682.

MULTIPLY the Position by the alternate Errors, and if the Errors be of the same Kind, divide the Difference of the Products, by the Difference of the Errors; but if they be of divers Kinds, the Sum, by the Sum: And the Quotient, shall give the Number sought.

FOR Demonstration, what Number is that, which being multiply'd by B 3 will produce the Plane B A 30.

Positions.	Positions.
Let it be $A-C=6$	Let it be $A-D=8$
into $B=BA-BC=18$	into $B=BA-BD=24$
	$A=10.$
	$B=3.$
	$C=4.$
	$D=2.$
	$BA=30.$

The Errors therefore are.

First Error. BA plane $-BA+BC=12.$

Second Error. BA plane $-BA+BD=6.$

The Less substracted out of the Greater, there remains,

$\overset{12}{BC}-\overset{6}{BD}=6$ the Difference of the Errors.

Which being multiply'd into their altern Errors, the Products will be as follows.

BC Defect	BD Defect
$A-D$	$A-C$
$BCA-BCD=96$	$BDA-BCD=36$

And Subtraction again being made $\Big\}A=\dfrac{BCA-BDA}{BC-BD}=\dfrac{60}{6}=10$

Again. Data $\left\{\begin{matrix}B\\BA\end{matrix}\right.$ quæritur A.

Sit $A-C$	Sit $A-D$
in $B=BA-BC.$	in $B=BA-BD$
BA pl. $BA+BC$ minus.	BA pl. $-BA+BD$

Ergo Errores $+BC$ & $+BD$

$$\frac{A-D}{BCA-BCD}\quad\text{min.}\quad\frac{A-C}{BDA+BDC}$$

$$A=-\frac{BCA-BDA}{BC-BD}$$

Pos.

$$\begin{array}{lll} \text{Pos. } A+B & \text{Pos.} & A+C \\ D & \text{Errors} & E \end{array}$$

$$B.C :: D.E.$$
$$CD = BC.$$
$$AD+CD = AE+BE$$
$$AD-AE = AD-AE$$
$$\frac{AD-AE}{D-E} = A$$

As the Difference of the Errors to the first Error, so is the Difference of the Positions to a Number, which, contrary to the Sign of the first Error, being added or subtracted to or from the first Position, gives the true Position.

When the Errors have different Signs, their Sum is their Difference.

The Reason of the Proportion betwixt the two Errors of Position, is, because the Numbers added or subtracted, and apply'd to the one Term of Proportion, are proportionate to the Numbers added or subtracted, and apply'd to the other Term, because two Numbers, apply'd or divided by the same Number, continue the same Proportion. Likewise, if you add or subtract like proportional Parts, the Sums or Differences will be in the same Proportion.

As the Error of the first Position to the Error of the second Position, so is the Error of the first Operation, to the Error of the second Operation. But the Rectangle of the Means, is equal to the Rectangle of the Extreams. Subtract the one, from a Number containing the other, and you leave the true; only in greater Products contain'd so many Times, as the Difference of the lesser Error of Operation is to the greater Error of Operation, because the lesser Error could not take

away ſo many Truths as the greater Error had made in the greater Product.

From BA	From BA
Take $BA-BC$	Take $BA-BD$
Remains $BA-BA+BC$	Remains $BA-BA+BD$
The Difference	$BC-DB$
	$A-C$
$A-D$	BD
BC	$BDA-BCD$
$BCA-BCD$	
Subſtract $BDA-BCD$	
Remains the Diff. $BCA-BDA$	
Divide it by $BC-BD$ = Is the true Poſition ſought.	

Dr.

Dr. HOOK *of Earths, Salts, &c.*

MARCH, 14, 168$\frac{1}{1}$.

THE Nature of Clays, Stones, Limes, *&c.* being diſcourſed, I mention'd the Sorts of Stone which were here call'd Freeſtone, *viz.* ſuch as could be ſaw'd with a tooth'd Saw, ſuch as *Cone*, *Rigate*, *Burford*, *Ketten*, *&c.* That Stones were of two Natures, one bituminous, or ſulphureous, the other ſaline and watery; the ſulphureous would calcine into Lime, the ſaline make Glaſs, vitrify or diſſolve, and moulder with the Rain, Air, and Froſt. That both theſe Sorts are often found in the ſame *Portland*-Stone one Part whereof will moulder, the other harden with the Air. That Loam is a Mixture of various Sorts of Clays and Sands, and may be ſeparated by waſhing. That ſuch a Material is uſually choſen for Brick-Earth, as being moſt eaſily ſoftened and tamper'd for moulding, and moſt eaſily and ſpeedily dry'd for burning, and moſt eaſily burnt; to make it yet more eaſy for burning, 'tis uſually dry, and expoſed to the Winter Rains and Froſts, for mellowing againſt the Spring. That the fineſt Clay would make the beſt Bricks, were it not for the more than ordinary Labour and Charge in waſhing, working, moulding, drying, baking, as is evident in Pottery, and Tiles, and eſpecially in the *Roman* Bricks, which are ſome of them of ſo fine an Earth, ſo well moulded, and ſo thoroughly burnt, as to laſt even to this Day, as intire and perfect as when firſt made, in all probalility. That hungry Clay was hardeſt and beſt to endure the Fire without melting, but ſaline, and fine Clays, were moſt apt to vitrify: And thence the throwing in of three or four Shovels of Salt into

a Pot Furnace when hot, made all the Pots in the Furnace to be glaz'd. That *China* was ſuch an Earth, as was very difficult to be vitrify'd.

CONCERNING Salts, and other volatile and fix'd Bodies, I mention'd, that there were two Sorts, one that was homogenous to the Air, and would be diſſolved into it. This was call'd Volatile; the other heterogeneous, and would not at all be ſo diſſolved and mixed with it; and theſe were call'd Fixed. Of the Volatile, there are various Sorts, which will be diſſolv'd into the Air, by differing Degrees of Heat. Spirit of Wine, or ſuch other fermented Spirits, Camphire, the odorous Gums of Flowers, and Herbs, will be diſſolv'd into the Air with a ſmall Degree of Heat; other Bodies more difficultly, and require a ſtronger and ſtronger Heat, as they are more and more fixed; ſo ſome Salts and Gums, &c. will not riſe at all: And theſe are call'd fixed Bodies, or Alcaly Salts. Of theſe which are diſſolv'd into the Air, ſome are taſted as it were, by the Noſe, others not in the ſame Manner as in Tinctures made in Waters; ſome, whereof the Tongue does taſte, others not.

CONCERNING the *Oxford* Trial by blue Starch, which they affirm'd would turn red, with Acids, I ſaid 'twas impoſſible, Smalt being Glaſs, but it muſt be *Litmus*, or *Indico:* But moſt likely *Litmus*; being a clear, blue Tincture; but *Indico*, a thick Precipitation.

THE Experiment was very conſiderable, though plain, giving a further Explanation of Gravity, by making a large Glaſs vibrate, with a Viol Bow: By which Vibration, a certain Undulation is plainly ſeen to dart out from all ſuch Places where the Glaſs vibrates. And it was very plainly viſible, that the Water, and Bodies in it, did move towards every ſuch vibrating Part, and from every other Part that was at reſt.

Dr.

Dr. HOOK's *Experiments of the floating of Lead, &c.* July 4, 1683.

WEDNESDAY, *June* 27, 1683, I ſhew'd two Experiments to the Society, which ſucceeded; of which I gave an Account, *Wedneſday, July* 4, 1683, as follows.

Of the floating of unmelted Metal, upon the ſame melted, with the Cauſe.

I. THERE was melted, in a Crucible, about a Pound and half of Sheet Lead, and whilſt it remain'd melted, ſeveral ſmall Pieces of the ſame Lead were gently one by one, by the Help of a *Forceps*, laid upon the clear and bright Surface thereof (the Scum and Litharge being firſt removed) and it was found that they all ſwam upon it, and did not ſink to the Bottom; but if they were all cover'd or plung'd under the Surface, they would not riſe again, but ſink to the Bottom, and ſoon be melted.

THE Occaſion of the Experiment, was a Suggeſtion, that Lead, when it concreted, did (as Water when it congeals to Ice) ſettle itſelf into a more rarify'd Texture, than when fluid; by which Means, it became lighter than the melted Lead, and ſo ſwam at the Top of it. But though the Effect were anſwerable to the Aſſertion, yet the Cauſe, aſſign'd, was falſe; for it was very evident, that the Reaſon of its ſwimming, was much the ſame with that of the ſwimming of a Needle, or of Water-Spiders, and many other Inſects upon the Surface of the Water; namely, a Coherence of the Air to the Surface of the ſwimming Body; which Coherence of the Air does depreſs and remove a greater Part of the Fluid, Lead, or Water, than the

the meer Bulk of the Body itſelf would do; which, in both theſe Caſes, is very evident; and was, in theſe Trials, very remarkable; for the Surface of the Lead did plainly bend and ſink below its Level, with a Roundneſs where the Piece of Lead lay; which bending of the Surface, was made the greater by a thin Plate, or Skin of Litharge: which the Air does preſently make upon melted Lead, ſo ſoon as ever a former is remov'd or ſcummed off.

Of the Condenſation of Air by Water.

II. THERE was ſtuck into the Side of a Piece of wooden Pipe, for conveying Water, a ſmall cylindrical Pipe of Glaſs, about a Foot long, and ſomewhat better than half an Inch in Diameter; one End of which Pipe was hermetically ſeal'd, but the other End was open, and communicated with the Cavity of the wooden Pipe, by means of a ſmall Hole bor'd in the Side of that wooden Pipe, into which the open End of the Glaſs Pipe was thruſt hard, having a little Linnen Rag wrapped about it, as is uſual for Taps put into the End of a Barrel, or other Veſſel. Then (there being about a Foot of Air left in the Glaſs Pipe) Water was forc'd into the wooden Pipe by a ſmall Force-Pump; and it was plainly to be ſeen, that as the Water was more and more ſtrongly forc'd into the wooden Pipe, the Air left in the Glaſs Pipe, by the Water that enter'd into it by the aforeſaid Hole, was condenſed into a leſſer and leſſer Room; ſo that hereby, the true Degree of the Preſſure of the Water could be eaſily found and meaſured; which was conceiv'd to be an Experiment, or Inſtrument of great Uſe for Water-Works, becauſe by means hereof, the Force of Water, in any Pipe, might preſently be known; namely, both from what

what Height it deſcended, and to what Height it would there again riſe. The Rule of doing which, was the next Day, to be brought in.

Dr. Hook's *two Experiments, ſhewing the Preſſure of Water in Pipes, and how to meaſure it. Alſo the Expanſion of melted Metals, made before the* Royal Society, July 4, 1683.

July the 4th, 1683. I read the Accounts of the two Experiments made *June* 27; and likewiſe further explain'd the Uſes of them, by Diſcourſes in other Particulars, namely, that the ſecond Experiment was of great Uſe for the trying the Strength of Pipes, for Conveyance of Water. By which Means, I have examined ſeveral Sorts of earthen and other Pipes and Cements, and have found that earthen Pipes, made of a Material only, as hard as Houſe-Tiles, would endure the Preſſure of 100 Foot of Water; that the Uſe of the other Experiment, was chiefly luciferous, namely, to ſhew the Nature of Fluids and Congruity, of which I ſhould ſhortly have Occaſion to diſcourſe more at large.

Then I produced and read the Rule, according to which the Preſſure of the Water, in any Pipe, might, by means of a Trial with the former Inſtrument, be calculated and reduced to certain Meaſure in Feet and Inches. The Means of performing, I ſhew'd, were principally two, firſt Arithmetically, and ſecondly, Mechanically.

The Arithmetical Rule was this; that the Length of the Cylinder of the Air in the Pipe, before it was preſs'd upon by the Water in the Pipe, ſhould be compared to the Length of the Cylinder

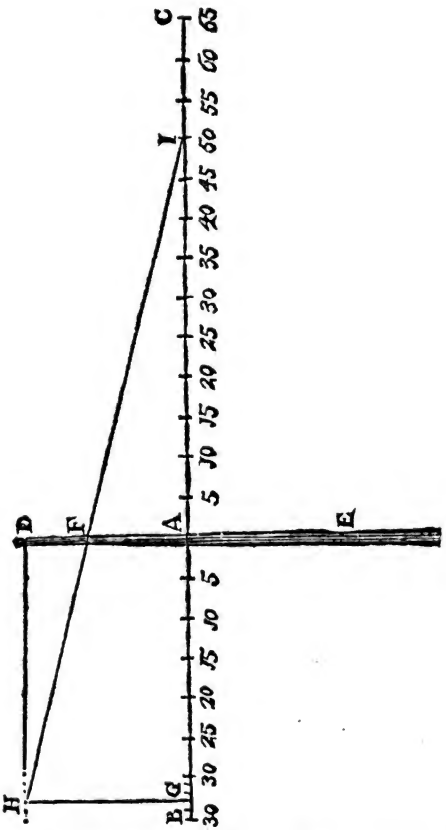
of the ſame Air, when compreſs'd by the Water of the Pipe, and the Difference noted; namely, the Length of the Cylinder of Water thruſt into the Pipe, by the Preſſure. Then to reſolve this Proportion. As the Length of the Cylinder of Water thus compreſs'd, is to the Length of the Cylinder of Water ſo thruſt in; ſo the Height of the Standard of Water, at the Time of Trial, to the Height of the Cylinder of Water preſſing in the Pipe, which is equal to the Height to which the Water of that Pipe, ſo preſs'd, will aſcend above the Surface of the Water in the ſmall Pipe.

THE Height of the Standard of Water, at the Time of Trial, is eaſily known by the Height of the Mercurial Standard at that Time; which, being now grown very common and uſeful, is almoſt every where to be met with, and may otherwiſe be eaſily ſupply'd; for as the Weight of Water, to the Weight of Quickſilver, ſo the Mercurial Standard, to the Height of the Water Standard.

THE Weight of Water, to that of *Mercury*, is by many Trials found to be near as 1000 to 13593, or as 1 to 15, according to his Account following Numb. - - - -

THE

THE Geometrical, or Mechanical Way, was this. Upon a Table, or Plane



draw a Line, as B A C; then croſs it at Right Angles, with another Right Line, as D A E, then divide A B, into thirty-ſix Parts, and continue the ſame Diviſion, from A towards C, ſo far as you have Occaſion of Foot Heights of Preſſure; as ſuppoſe to 100; then

then ſubdivide one of theſe Parts, lying next to A into twelve equal Parts. Then knowing the preſent Water Standard, count, from A towards B, ſo many Parts and Duodecimals, as it is then Feet and Inches: Croſs the Line, at that Point at Right Angles, with another Line, as G H, and from G, ſet off the Length of the Cylinder of Air in your Glaſs, before Compreſſion; then ſet off the Length of the additional Cylinder of Water, from A towards D, as ſuppoſe to E, and laying a Rule over the Points H and F, ſee where it croſſeth the Line A C, as at I, then count the Parts and Duodecimals from A, and that ſhall give the Preſſure or additional Height of the Water, above the Level of the Water in your Water Poiſer in Feet and Inches: The Reaſon of all which depends upon the reciprocal Proportion of the Strengths of Air to the Extenſions thereof.

THE ſecond Experiment, was made, to ſhew a Way, how to find the true and comparative Expanſion of any Metal, when melted, and ſo to compare it both with the Expanſion of the ſame Metal, when ſolid, and likewiſe with the Expanſion of any other, either fluid or ſolid Body. An accurate Account of which is neceſſary, to compleat a Hiſtory of Expanſion or Gravitation. The Method of trying it was, by having a Veſſel full of melted Lead, and alſo a ſolid Body of Iron to be ſunk into it; this ſolid Piece of Iron was about 1 ¼ Inch Cubical, and into it, was faſtened a very ſmall Wire of Iron, big enough to thruſt it under the Surface of the melted Lead, and make it ſink therein, (for, of itſelf, it ſwam upon the Lead, as Wood upon Water). This Wire was faſtened perpendicularly, under a Scale, and ſo much Weight put into the Scale as ſerved to make it ſink under the Surface of the Lead; then taking it out of the Lead, and ſeeing by the additional Weights, put in

into the other Scale, to counterpoiſe it, firſt in the Air, then in Water, or any other Liquor, the comparative Weight of each of them was eaſily diſcoverable.

THE Reaſon of the making of which Experiment was, to hint the Neceſſity there is, in all Experiments fit to be made Uſe of for any Philoſophical Theories, of reducing them to a Certainty of Quantity; without which, no certain and unqueſtionable Concluſion can be made. Now tho' a certain Standard of Weight, Meaſure, Expanſion, Power, Motion, *&c.* be not made Uſe of; yet if ſome one determinate Meaſure for each of them be pitched upon, 'twill be enough to make the comparative Trials uſeful; though it were to be wiſh'd, that ſome univerſal, natural Standard of Meaſure for all Things were found out, thoſe that have hitherto been thought of, having been doubted of, as to their Univerſality and Certainty, at all Places and in all Times.

Not knowing when the following Experiments were made, I inſert them after the foregoing, by reaſon of ſome Congruity between them.

W. DERHAM.

An Account of ſome Trials for the finding out the Preſſure of the Parts of Water one upon another ; and the elaſtical Power of the Air.

FOR the making theſe Experiments, there was prepar'd a long Tube of Glaſs, ſeal'd at one End, and being erected perpendicularly, with the ſeal'd End downwards, it was fill'd with Water, and ſo faſtened againſt the Side of a Wall ; then there was taken another ſmall Tube of Glaſs, very even drawn, and ſmall enough to be let down within the former Tube ; this Tube was 12 Inches long, and was ſeal'd at one End, and divided into Inches, Halfs, and Quarters ; then, to the open End of this Tube, was hung a ſmall, long Plummet of Lead, which would eaſily ſlip down to the Bottom of the longer Tube, and draw down the ſmall Pipe with it ; both which were gently ſo let down by a ſmall Thread, as the Experiment requir'd, which afforded theſe Obſervations. The Pipe, when the lower and open End firſt touch'd the Water, being full with Air, not heated by the touching the Pipe with a warm Hand, or otherwiſe, was obſerv'd by Degrees, as it deſcended, to be in part fill'd with Water, and ſo much the more by how much the deeper it deſcended. And obſerving the Degrees of Condenſation of Air in the Pipe produced at ſeveral Depths, we found them to be theſe. At *Greſham* College, the 24 half Inches of Air loſt one half Inch of its Extenſion

at

at 2 Halfs at 3 Halfs at 4 Halfs at which is therefore a 5th Part of a Cylinder of Water able to counter-balance the Preſſure of the Air. The whole therefore may hypothetically be judg'd to be - - - - - -

I DID, ſince that, erect a Tube ſome 13 Foot high; and fitting all Things as in the former Experiment, I collected this Table A, whoſe firſt Row of Numbers ſhews the equal Spaces into which the Air was extended; and the laſt ſhews the Height of the Water above the under Surface of the Air. Since that, in the ſame Tube ſtanding in the ſame Place, I reiterated the Experiment, and collected this following Table B.

ALL which three Tables, being ſo different one from another, may ſeem to overthrow each other, and the Certainty of this Kind of Experiment in general. But as I cannot vindicate the Trials from ſome Errors (it being almoſt impoſſible to make theſe Kind of Trials ſo accurate, that there ſhall be no Miſtake committed) ſo neither do I believe, that theſe ſeeming Contrarieties do wholly proceed from the Unaccurateneſs in the Proceſs. For ſince the Air is ſometimes under a greater, and ſometimes a leſs Preſſure, the Degrees of Force, requiſite to promote the Condenſation further, muſt neceſſarily be differing.

B	
24	00
23	13
22	31
21	52
20	76
19	101⅛
18	127½
17½	142

A	
48	00
47	08⅛
46	17
45	27
44	36
43	45½
42	58
41	68⅛
40	80
39	91⅛
38	105½
37	117
36	130½

AND hence by the firſt Table, I judge the Height of a Cylinder of Water, able to balance the Preſſure of the Air, when that Experiment was

made to be by the Second Experiment I judge the counter-balancing Pillar, then to be between 390 and 400 Inches, or near about 33 Foot; by the third, I gueſs it to be about 382 Inches, or near about 32 Foot. This Experiment therefore, if accurately made, at ſeveral Seaſons and Times of the Year, may afford us a very eaſy Way of knowing the Preſſure of the Air at that Time, and this more accurately and nicely, than can be perform'd with *Mercury* the ordinary Way. For whereas the Shortening and Increaſe of the Mercurial Cylinder, is at moſt not above 2 or 3 Inches, in this Experiment, the aqueous Cylinder will change fourteen times as much.

NEXT, this Experiment may help us to gueſs at the Preſſure of the Sea Water againſt the Air, let down to the Bottom of it in a diving Engine, by knowing the Proportion between the Gravity of ſalt and freſh Water. But it were very deſirable that ſuch, as have the Opportunity of making Trials at Sea, would be diligent in it. For though there ſeems to be no Doubt, but that Water proportionably preſſeth according to its perpendicular Height; yet it is not eaſy to predict, how much it may vary from that Hypotheſis; which Deviation may be cauſed, either from the extreme Cold at the Bottom of the Sea, which may weaken the Spring of the Air, or from the differing Gravity of the upper and lower Parts of ſalt Water; or from ſomewhat elſe, whereof we may be yet ignorant. Now for the more accurate making of theſe Trials, I think it were very requiſite to have ſome ſuch Engine as this.

TAKE a good ſtrong Glaſs Bottle, that will hold about a Gallon; and let there be fitted to it a handſome Screw Cover of Braſs, and ſhap'd like thoſe Covers that are uſually put upon Chirurgeons Bottles, that are made of Pewter. Let the Cover be

be very well cemented on, and the Screw be made to go very cloſe through the Top of this Cover; let there be made ſeveral very ſmall Holes with a Needle Drill, then hang a good Weight under the Bottle, and let it down with this Cover upmoſt, for by this Means, by drawing it up from ſeveral Depths, and weighing the Quantities of Water it brings up, it will be eaſy to know the Weight of the incumbent Column of Water.

THERE might be many other Ways, but this I take to be the moſt cheap, eaſy, and certain of any; nor is there any Danger of breaking the Bottle, either inward or outward; for as the Bottle deſcends, the Water ruſhes in, and as it is drawn up, the Air goes out.

The following Experiments are here inſerted, by reaſon of their Congruity with the foregoing.

WILLIAM DERHAM.

More Experiments of Preſſure.

Fig. I.

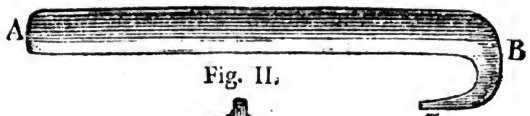


Fig. II.

THERE was taken a Glaſs Tube A B C, (*Fig.* I.) about 23 Inches long, and near ¼ of an Inch over; this was cloſe ſeal'd up at one End A, and the other End B was drawn into a very ſmall Pipe C, and bended according to the Shape in the Figure. This Pipe was found to weigh $1\frac{13}{16}$ ℥ + 4 gr. or 874 Grains, being fill'd with ſalt Water, and the Outſide wiped dry (which was conſtantly done in all the ſubſequent Trials) it weighed $4\frac{7}{16}$ ℥ + 10 gr. or 2140 Grains, whence if we deduct the Weight of the Pipe 874, we have 1266 Grains for the Weight of the Water that fill'd the Pipe. This Glaſs Tube being faſten'd to a Line, to the End of which was hang'd a Plummet of Lead, to make it ſink; 'twas fitted ſo as to be let down perpendicularly into the Water with the ſeal'd End A foremoſt, by which Means the ſmall Hole of the Pipe C was open downwards (that Hole being made purpoſely ſmall, that the Air could not get out at it whilſt the Water got in, nor the Water get in whilſt the Air was paſſing out.) Then the Glaſs was, for a ſhort Time, ſo held in the Water, that all of it, except the ſmall bended Pipe, was cover'd and inclos'd with the Water (which was ob-

obſerv'd in every Trial, to the End that the Air within the Pipe, might be well cooled) and being let down to the Bottom, and there ſuffer'd to ſtay for a ſhort Space. Afterwards being drawn up, looſened from the Line, dried, and exactly weighed; its Weight was found $3\frac{13}{16}{}^{3} + 3$ Grains or 1833 Grains; whence, deducting the Weight of the Tube 874, we have 959 Grains for the Weight of the Water it brought up. The Place was in the Channel to the North of *Quinborough*, the Depth of the Water 16 Fathom and a Foot, or 97 Foot, where we made the ſubſequent Trials which are rang'd in this Table.

Top full		2140 — 874 = 1266	
At 97 Foot deep — 2		1833 — 874 = 959.	307
At 97 Ft. deep — 2		1832 — 874 = 958.	308
At 8 Ft. 3 In. — 2 Ft.		1060 — 874 = 186.	1080
At 16 Ft. 6 In. — 2 Ft.		1257 — 874 = 383.	883
At 33 Ft. — 2 Ft.		1500 — 874 = 626.	640
At 66 Ft.		1737 — 874 = 863.	403
At 66	From the Mouth of the Tube	1734 — 874 = 860.	406
At 33		1530 — 874 = 656.	610
At 16½		1296 — 874 = 422.	844
At 8¼		1131 — 874 = 257.	1009

A Bundle of Corks being knit up in a Handkerchief, and faſten'd to the Line at 33 Foot from the ſmall End of the Glaſs, the Tube was again let down to the ſame Depth, and the Corks, floating upon the Water, ſuſpended it at that Depth; for, a good while afterwards. Then drawing up the Cylinder, by meaſuring, the Cylinder was found to have taken in juſt as much Water, as it had in the laſt Trial, but the Weight of the Glaſs was not examin'd. Other Trials were made the next Day with the ſame Glaſs Cylinder, *viz.*

At 8½ Foot from the Top 1172 — 874 = 298.
Juſt at high Water, the Water being at a ſtand.

At 8½ Foot	1131 — 874	= 257
At 16½ Foot	1300 — 874	= 426
At 33 Foot	1510 — 874	= 636
At 49½ Foot	1635 — 874	= 761
At 66 Foot	1712 — 874	= 838

THE Trials did agree, by Meaſure, with ſome I had made in the Morning.

ANOTHER Trial was made of the laſt Experiment, becauſe it was done when the Water had ſome Current, and the String ſeem'd to ſtream a good Way from the Perpendicular; to prevent which Inconvenience, the Boat was ſuffer'd to drive with the Current, by which Means, the Line ſeem'd to go down perpendicularly into the Water. So the Cane being pull'd up, after it had ſtaid ſome time at the Depth of 66 Foot, it weigh'd 1719 — 874 845. At 82½ Foot, and left to drive perpendicularly 1883 — 874 = 1009.

Wedneſday, March *the* 11*th, in the Afternoon, near the ſame Place, where the former Trials were made, there were made theſe following Experiments of Compreſſion.*

UNTO the Neck, or Mouth, of a common Quart Glaſs Bottle, was fitted a Valve, that opened inwards, and ſhut outwards; this Bottle was ſo let down into the Water, that the Mouth went foremoſt, by which Means, the Water had, as the Bottle was ſinking, a free Paſſage into the Body

Body of it, to compreſs the Air; but by the ſhutting of the Valve, when the Bottle was again drawn up, it was hinder'd from getting out. This Bottle, when empty, weigh'd 37 $\frac{1}{2}$ Ounces, and 24 Grains, or 18204 Grains; fill'd with ſalt Water, it weigh'd 78$\frac{3}{4}$ Ounces and 3 Grains, or 37563 Grains; whence, taking the Weight of the Bottle 18204, we have 19359 Grains, for the Weight of the Water, that fill'd the Bottle. This Bottle being let down 13$\frac{1}{2}$ Fathoms by the Ship's Plumb Line, or 81 Foot, the Valve was ſo hard ſhut, when it was taken up again, that it was difficult to be thruſt open. Though when the ſmall End, or Mouth, of the Bottle, was ſet upward, the Valve being made of Braſs, without Leather, was found to leak a little, by the hiſſing Noiſe the Air made at it. And when by a Knock, the Valve was beaten down, the Air made a Noiſe in ruſhing out like that of a Bottle of Ale when it flies; the Bottle, and the Water it brought up, weigh'd 65$\frac{19}{20}$ Ounces, or 31656 Grains; whence, deducting the Weight of the Bottle 18204, we have 13452 Grains for the Weight of the Water. This Bottle was again let down to the Depth of 14 Fathom, or 84 Foot; and, being drawn up, was found to weigh, whilſt the compreſs'd Air remain'd in it, 65$\frac{1}{8}$ Ounces, and 19 Grains, or 31279 Grains; when the Air was let out, it loſt 21 Grains of its former Weight, counterpoiſing only 31258 Grains, which was ſuppos'd to proceed partly from the freeing of the compreſs'd Air, and partly from the Loſs of a little Water, that the violent Eruption of the Air had blown away; from which laſt Sum, by deducting the Weight of the Bottle 18204, we have 13054 for the Weight of the Water.

March the 13th, another Experiment was made with another Bottle of the ſame Faſhion, which empty, weigh'd 37$\frac{11}{16}$ Ounces and 12 Grains, or 18162 Grains; fi l'd with ſalt Water to the Valve, it weigh'd 77$\frac{11}{16}$ Ounces and 3 Grains, or 37353 Grains; whence, deducting the Weight of the Bottle 18162, we have 19191 the Weight of the Water that fill'd it; this Bottle being let down 8 Fathom, or 48 Foot, the Bottle, compreſs'd Air, and Water together, weigh'd 60$\frac{11}{16}$ Ounces and 12 Grains, or 29142 Grains; the Air being let out ſoftly, which requir'd a long time, and the Bottle, and Water afterwards weigh'd, was found 24 Grains lighter, *viz.* 29118 Grains; whence, deducting the Bottle 18162, we have 10956 Grains for the Water. The Experiments are ranged together in this Table.

THE Bottle, with a bended Copper Pipe at the Top, being let down 8$\frac{1}{4}$ Foot deep, brought up in it 4$\frac{11}{16}$ Ounces, and 24 Grains of Water, the Bottle being weigh'd before-hand with a dead Weight, or counterpois'd; the ſame Bottle, kept longer at the ſame Depth, brought up 8$\frac{3}{16}$ Ounces and 25 Grains of Water; the ſame Bottle, kept yet longer a great deal, brought up 9$\frac{7}{16}$ Ounces and 6 Grains; the Water that fill'd the Bottle, weigh'd 41$\frac{11}{16}$ Ounces and 24 Grains; which different Proportions of Water, taken in, we judg'd to proceed, either from the leaking of the Veſſel at the Screw, by which Means, the Water had a Paſſage into the Bottle below the Mouth of the bended Pipe, which would therefore ſerve for a Vent-hole for the Air to get out at; or elſe that the Motion of the Top of the Water being a little uneven, the Preſſure upon the Bottle muſt conſequently alter, there being ſometimes a greater, ſometimes a ſhorter Pillar of the Water above it; ſecondly, the Bottle itſelf was, by the cockling of the Boat, ſome-

ſometimes lifted higher, then depreſs'd lower, which did alſo alter the Height of the preſſing Pillar; whence, as the Preſſure was a little increas'd, the Water got in; and, as it decreas'd, the Air got out; and, being held a long while in that Poſture, many of thoſe Changes did very much augment the Quantity of Water within the Glaſs.

Experiments of the Weight of Water.

A WHITE Glaſs Viol, made in the Manner deſcrib'd in Figure II. with a ſmall ſhort Neck, was, by Trial, found to weigh, when empty, 1425 Grains; when fill'd exactly full with ſalt Water, it weigh'd 5247 Grains; whence, deducting the Bottle 1425, we have 3822 Grains, the Weight of the ſalt Water. The ſame fill'd with freſh Water taken out of the *Thames* at *Greenwich*, about low Water, weigh'd 5164; whence, deducting 1425, we have 3739, the Weight of that freſh Water. And weighing afterwards the Water, wherewith the Strong Ale at *Margat* is brew'd, we found it exactly the ſame with the Water taken up at *Greenwich*; whence we conclude, the Proportion of theſe freſh Waters, to this ſalt, to be as 3739 to 3822; that is, near as 45 to 46.

Trials of the Heat and Cold of the Water.

A SEAL'D Thermometer was let down to the Bottom of the Water, at 16 Fathom and a Foot, with the great Ball upwards, and the Stem downward, to the End that, if the Cold were extreme, it might have ſo far condenſed the Spirit of Wine, as to have admitted the Air to have got in out of the Neck. And ſo by pulling it to the Top, we might have known the Cold at Bottom; but though the Thermometer was ſuffer'd to remain

a long Time at that Depth, and were ſuddenly pull'd up, we could not find that it had any whit more condens'd the Spirit of Wine, than it was by keeping the ſame Thermometer a pretty while juſt under the Water, at the Top, when we judg'd the Temperature of this Water, both at the Top, in in the Middle, (for, by other Trials, we found the ſame at other Depths) and at the Bottom, to be all the ſame.

N. B. *The Inſtrument deſcrib'd in the* Nuntius ad Abyſſum, *much better for the Purpoſe than this.*

R. W.

Obſervations of Sound.

Being at a Place of the *Thames*, about four Miles above *Graveſend*, there happen'd to be ſhot off ſeveral ſmall Pieces of Ordnance, by a Ship that was about half a Mile farther up the River; the Multitudes of the Echoes of each of which Shots, made a Noiſe among the ſeveral Hills, Woods, and Banks, on both Sides of us, juſt like Thunder. And could they have been number'd, they would, queſtionleſs, have exceeded an Hundred. And having ſince had the Opportunity to obſerve the Noiſe of Thunder, it ſeem'd to me to be deducible partly from Echoes; which would yet ſeem more probable, if we could, by any Experiment, find that the Clouds would rebound or echo a Sound. A Gun being afterwards ſhot off by the Veſſel we were in, when we were near the Mouth of the *Thames*, and ſeveral Ships being on this and that Side of us, we could very ſenſibly hear ſeveral Echoes rebounded from them.

Dr.

Dr. HOOK's *Contrivance of a very commodious Windmill; communicated to the* Royal Society, July, 11, 1683.

JULY the 11th, I read the preceding Diſcourſe and Accounts of the two Experiments ſhew'd on *July* the 4th; and further explain'd each of them by verbal Diſcourſes. Then I ſhew'd theſe two Experiments following, which I explain'd by Diſcourſes, ſomewhat in the following Manner.

THE Firſt, was the Module of a Windmill, in which were thoſe Particulars following conſiderable, not to be found in any other yet made uſe of.

1. THAT it had no Need of any Houſe, but what might be placed, either immediately upon the Ground, or under the Ground, according to the ſeveral Uſes to which it might be apply'd. Whence follow'd,

2. THAT the Houſe need not be any Impediment to the Force of the Wind, which it uſually is in all other Windmills.

3. THAT it doth of itſelf turn to all Winds, and ſo needs not the Attendance, Watching, and Labour of Men to ſet it, which is neceſſary in other Mills.

4. THAT the Vanes are contriv'd of the moſt perfect Form, to receive the whole Power of the Wind, for the Cylinder thereof it is expoſed to: Which is effected by the particular Slope of the Vanes thereof, whereby the Force of the Wind becomes equal upon every Part of the Vane, from the Center to the Tip, or Extremity thereof. An equal Progreſſion of Wind cauſing every Point of the whole Vane to make an equal Arch of Rotation, or an equal Angle at the Axis.

5. FOR

5. For that it needeth not ſo big an Axis, nor ſo ſtrong Vanes as other Mills, the greateſt Strength of this being in the Way of pulling, the other in the Way of thruſting; and this being capable of being ſtrengthen'd by Ropes, like the Tackling of a Ship.

6. For the eaſy Way of producing a circular Motion below, without the Help of Trundles or Cog-wheels, which are both a great Impediment to its Motion, and do wear, and often need Repair.

7. For the eaſy Way of communicating a reciprocating perpendicular Motion, which is uſually perform'd by the Help of Wheels.

8. For the Cheapneſs of it, there being ſo many Particulars not neceſſary to this, omitted, which are uſually done in other Kinds, and not without Neceſſity.

All which Particulars conſider'd, it makes it to be the moſt plain, ſimple, cheap, and eaſy to be made and uſed, that has been yet made; and yet the moſt powerful in its Effects, and the moſt univerſally applicable to all Purpoſes; (as grinding, bruiſing, beating, ſawing, pumping, placing, twiſting, drawing, turning, lifting, *&c.*) that can be made of equal Bigneſs.

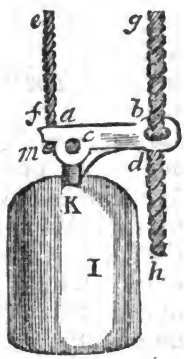
I have thought worth while, to inſert this Account of the Windmill (although ſcarcely intelligible without Figures, or a Module, which I never could meet with) becauſe ſomebody, or other, may be ſo fortunate to find the Module, or, by the Hints here given, contrive a Windmill like this.

W. Derham.

Dr.

Dr. HOOK's *Contrivance to ſtop great Weights falling*, July 11, 1683.

THE ſecond Experiment was a very plain and eaſy Way, how to ſtay a Weight from falling, when the Rope, or Chain, by which it is drawn up or let down, ſhall chance to break. This was effected by a ſmall Arm extended out from the Top of the Weight to the Side, with a Hand, or Pipe, at the End thereof, which graſped, or incloſed, another Rope or Chain, extended from the Top to the Bottom; which Hand, or Pipe, was ſo wide, as to ſlip freely upon the ſaid Rope, ſo long as the Weight was ſuſpended by its own Rope; but ſo ſoon as that any way fail'd, the Hand graſped the Side Rope faſt, and hinder'd the Weight from deſcending to the Bottom. This was one of the plaineſt, eaſieſt, and moſt ſimple Ways of effecting this End, though the ſame may be effected divers other Ways, as certainly, which I have alſo contriv'd. The explicating it, by a Scheme, makes it the more intelligible. I repreſents the Weight, *a b* the Arm, moving with a Joint at *c*, upon the other Part of it *k*, faſt into the Weight, *e f* repreſents the Rope, by which the Weight is either drawn up or let down, faſten'd to the Elbow *m*; by which Means the Wriſt, and Hand of the Arm, is kept at Right Angles with the Part faſt in the Weight, and ſo the Hand ſlips freely upon the greater Rope *g h*, extended from the Top to the Bottom, to which the Weight can deſcend; *d* repreſents a Spring, by which,



which, ſo ſoon as the Rope of the Weight, which holds by the Elbow *c*, fails, the Arm is extended ſtreight; by which the Hand *b*, preſently holds faſt the Rope, or Chain G *b*, by being made oblique to the Perpendicular, and, ſo creeking the Rope, and ſo hinders it from falling; as, by the Experiment ſhewn, plainly appear'd.

THE Uſe of which Contrivance, though poſſibly it might, to ſome, ſeem very trivial and inſignificant, as ſeeming to be calculated for keeping a Clock, or Chime Weight, from falling, is not altogether ſo ſlight and fooliſh; for even for that Uſe it may ſometime or other poſſibly ſave 100 Pound Expence, and the Lives of ſome Men. But if apply'd, in general, for the hindering Weights to fall, it may deſerve a ſomewhat better Value, and be found very conſiderable, ſince it may be very inſtrumental to ſave many Mens Lives, and much Charge, and great Inconveniences, which do very often now, for the Want thereof, happen. For whereas, in many Mines, the Men themſelves are often drawn up and let down in Buckets; and generally the Ores, Stones, Waters, and divers other Things, belonging to thoſe Works neceſſary for procuring Ores, or other Minerals, are ſo conveyed; and upon the failing of the Rope, Chain, or other Part of the Engine, do often fall from Top to Bottom, and ſo are not only daſhed in Pieces themſelves, but deſtroy, and do oftentimes irreparable Injury to Men, or what elſe they meet with in their Fall. By this Means, all ſuch Bodies are ſecured from the Fall, and kept hanging at the Place where they were when the Rope brake, or other Part of the Engine fail'd, and thereby the Bodies themſelves are preſerv'd intire, and no other Harm done by their Fall. The ſame Thing is applicable alſo to Men, aſcending, or deſcending, by Ropes or Rope-Ladders, and to Stones, Timber, or Materials for a high Building.

Dr.

Dr. Hook's *Way to take the Impreſſions of Medals, &c. imparted to the* Royal Society. *Octob,* 31, 1683.

HAVING been ſhewn, by Mr. *Frazier*, the Impreſſions of ſeveral of the King of *France*'s Medals, in a certain thin tranſparent Subſtance, much like *Muſcovy* Glaſs, but much more tough; on which, on the one Side, appear'd the perfect Impreſſion of the Medal, in *Entaglio*, or ſunk in; and, on the oppoſite Side, the very Figure of the ſaid Medal in *Baſſo Relievo*, or ſwelling out. And, conſidering what Way this might be done, having formerly taken off the Figure of certain Carvings, by Glue, ſo as to be able to caſt them in Plaiſter of *Paris*, or burnt Alabaſter; upon making Trial with a Glue made of *Ictheocolla*, diſſolv'd over a gentle Heat, in courſe Spirit of Wine, by laying it upon a fair ſtamp'd Crown Piece, and ſuffering it to lie a conſiderable Time, till it was thorough dry, cold, and hard; I found that it afforded me the ſame Kind of Subſtance, both for Toughneſs, Tranſparency, and Fitneſs, to receive and retain the Impreſſion of the Coin upon which it was laid, as the Subſtance ſhew'd me, containing the Impreſſion of the *French* Medal. This I ſhew'd the Society, and explain'd to them the Way of doing it. And alſo related, that the ſame Impreſſions might be ſo taken with common Joyners Glue; but the Plate would not be ſo tough, nor ſo tranſparent.

THE Preſident mention'd, that there had been a certain *Frenchman* here in *England*, ſome time ſince, who had certain tranſparent Plates like *Muſcovy* Glaſs; with which, he could eaſily copy out any Picture or Print, by laying it upon the

ſame, and writing upon it with Ink, as on Paper; the ſame being very tranſparent; and ſo cauſing the Print, on which it was laid to appear very plain through it: And inquiring, whether I could do the ſame, upon my affirming that I could, he deſir'd that I would ſhew the Experiment of it at the next Meeting.

N. B. *Dr.* Liſter *mention'd the Way of contracting Seals with Mouth-Glue.*

Dr. Hook *imparted to the* Royal Society *this Preparation, to copy any Picture,* &c. Novemb. 7, 1683.

I PRODUCED a Plate, made according to the preceding Deſire; which had the ſame Properties with that which was made by the *French* Gentleman. This was very thin, and as tranſparent as *Muſcovy* Glaſs, or *Selenitis*. It was alſo tough, and would bear Ink as well as any Paper, and ſo was fit to make uſe of, for any Experiments for drawing, or copying Pictures or Maps. The Manner of making it, I explain'd to the Society, to be thus. Firſt, I prepar'd a very thick Ciſe of *Ichthyocolla*, well diſſolv'd in Spirit of Wine, and then clear'd from all its Rags and Foulneſs, by ſtraining it through a clean Cloth; then taking a Looking-Glaſs Plate, well ſmooth'd and poliſh'd, I rubbed the ſame all over with a fine Rag, moiſtened a little with pure Sallad Oil; but ſo as only to hinder the Subſtance that was to be pour'd on it from ſticking to it, but not to make it foul or uneven. Having ſo prepar'd theſe Things, I heated the Siſe, and, when again pretty cold, I pour'd it upon the oiled Side of the Glaſs Plate, and

and ſo taking the Plate, and inclining it this Way or that Way, till the whole Plate was cover'd by the Siſe, I laid the Plate horizontal, and ſuffer'd it to lie ſo till it was thoroughly dry.

Dr. HOOK*'s ſeveral Diſcourſes of Improvements of Scales, Beams, and other Inſtruments, for weighing Bodies more nicely; and firſt, one to find any deſired Part of a Weight, or Body to be weigh'd.* Dec. 5, 1683.

I PRODUCED *an Inſtrument for the ſpeedy and exact finding any deſir'd Part of any Weight given, whether Commenſurate, or Incommenſurate.* The Inſtrument, (being only a Module, and to ſerve only for Explication and Experiment, and not for conſtant and continual Uſe) was a ſlender Fiſhing-Cane, ſtreightened very well, of about four Foot in Length, and tapering from one End to the other; this Material I made uſe of upon a double Account; *Firſt*, for its Stiffneſs; and, *Secondly*, for its Lightneſs, that I might, as near as poſſible, make it to be without Weight, and bending, and ſo approach to, or repreſent, a *mathematical Line.* Now the Part, I propos'd to find, being a *Decimal*, *Centeſimal*, *Milleſimal*, or the Powers of the *Decimal Fractions*, I divided the Cane into eleven equal Parts; at one of which, from the greater End, I, with a Needle, drew through it a ſmall Silk Thread, by which I ſuſpended it; and by adding Lead to the ſhorter End, I pois'd it, until it came to an *Equilibrium*, and ſo it hung *horizontally.* Then I made two *Scales*, with two Rings, whoſe inner Edges were thin and ſharp, by which they might hang upon the Ends of the *horizontal*, or *equilibrated* Cane. The *Scale* and

Ring, for the *greater* and *ſhorter End*, was made ten times as heavy as the other *Scale* and *Ring* for the ſmaller and longer End. Theſe being thus prepar'd, I hung on the *Scale* upon the *greater* or *ſhorter* End, at any Diſtance from the Thread: Then, hanging on the little Scale, upon the leſſer End, moving it nearer and farther from the ſuſpending String, till the Beam hung *in Equilibrio*; the which became an Inſtrument for finding the Decimal, Centeſimal, or Milleſſimal Parts, or Fractions of any Weight given. Suppoſe a Pound be to be ſo divided; Put the Pound into the great Scale, and then counterpoiſe it with Weight, as of Sand, Water, Minium, *&c.* in the leſſer Scale; this ſhall be a tenth Part of a Pound: Remove the Pound, and put the Decimal Counterpoiſe in the greater Scale, then counterpoiſe this in the leſſer, and this ſhall give a Centeſme of a Pound: Remove the Decimal, and put the Centeſme in the Greater, and the Counterpoiſe to it in the Leſs, ſhall give the milleſimal Part of a Pound, and ſo onward for the ten thouſanth, hundred thouſandth, or thouſand thouſandth Part of a Pound; which, this Way, may be moſt exactly found and determin'd: And the like for any other aſſignable Part whatſoever of commenſurate, or incommenſurate Proportion, to the whole Quantity, of what Weight ſoever; the Beams being accordingly proportion'd in Strength and Dimenſions, whether it be for great and maſſy Bodies, or exceeding minute and curious; and, by this Means, with ſome ſmall Addition, the ſmalleſt Bodies may be as certainly weigh'd, as the moſt tractable, even to the thouſand thouſandth Part of a Grain, far beyond the Reach of the Hand, or the naked Eye. And, as the *Microſcope* doth help the *Eye* to make *inviſible Bodies*, and *Parts viſible*, ſo may this help the Hand to make the *intractable Bodies tractable* and *ponderable*

rable, and *comparable*, by other Trutinations than thoſe of Sight; which is of conſiderable Advantage in the Inquiry after the ſeveral Natures of the *Intims of Things*, as I may hereafter ſhew, more particularly. In the mean time, I conceive, there was no great Reaſon for any, either to affirm the Experiment falſe or erroneous, or to ſlight it for its Plainneſs and Obviouſneſs; ſince any, that underſtands *mechanick Principles*, will ſave me the Labour of making a Demonſtration. And how obvious ſoever it be now known, yet I do not find it hath been taken Notice of by any Writer of *Mechanicks*; nor did I ever know any that had uſed it, or taken Notice of it, for this Purpoſe; and though it may be ſaid to be a *Stilyard*, yet 'tis as differing from the common Uſe of the *Stilyard*, as that is from a *common Beam*. I mention'd alſo, how neceſſary an *Inſtrument* this was in almoſt all *Philoſophical Examinations*, eſpecially in all Trials that concern the Limits and Bounds of Powers, in the *Intims of Bodies*. This *Proportional Balance*, will be of general Uſe, and to ſuch, particularly where Weights are troubleſome to carry and remove; and, I ſuppoſe, the only Reaſon, why it has not been uſed, is, becauſe it has not been thought of; though it were altogether as obvious, as to ſet an Egg on End.

This Inſtrument being eaſily underſtood without a Figure, I have therefore omitted the giving any.

A Second Inſtrument for weighing; or, a Sort of Eſſay-Scale.

December 12, 1683, I produced another Experiment, which was alſo an Inſtrument for weighing, which might alſo be of very general Uſe; and that was not only for examining the Weight of any Sort of Gold or Silver Coin, or any other Veſſels or Pieces of thoſe Metals: But alſo for examining and eſſaying the Nature of the Metal itſelf, of which thoſe Pieces, or Veſſels, ſhould be made, both as to the Species of the Metal, and alſo as to Fineneſs, Purity, or the contrary Qualifications of them. Now though this be to be done by means of ordinary Gold Scales and Weights; yet, I dare affirm this Way to be altogether as ſure as the other, and abundantly more eaſy, both for Carriage and Uſe. And there might as well have been Objections made againſt the Art of Printing, becauſe a Writer was able, before that Art was found, to have wrote Letters, and Words, as fair as they could, by that Art, be printed. The Invention of the Inſtrument was grounded upon the Theory of the Nature of Springs, which I have formerly ſhew'd, and explain'd in this Place; and the Way of examining the Goodneſs or Badneſs, of this Kind of Metal, and of diſcovering the Species of the Metal itſelf, was grounded upon the Experiment of *Archimedes*, improv'd and explain'd by *Getaldus*; which two Theories, being rightly underſtood, will take off all Objections againſt the Truth and Reality thereof, with all impartial Perſons.

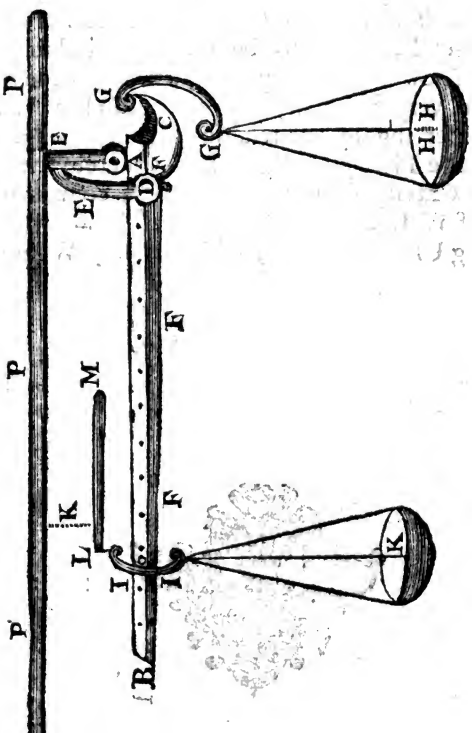
The Inſtrument was made of a Coyle of Braſs Wire, one End of which, was held in the Hand; and, to the other End, was faſten'd a ſmall Net of Hair, in which Net, the Piece of Metal to be exa-

examined was put; and then the whole was lifted up by the Hand, and, by means of a ſmall Top of a Feather, faſten'd to the lower Part of the Wire, the Length of the whole Spring augmented by the Weight of the Piece try'd, was obſerv'd, and by the Diviſion on the ſaid Feather, the Number of Grains were to be taken Notice of; this gave the Quantity or Weight of the Piece itſelf in Grains. Then, for the ſecond Qualification of the ſaid Metals, it was to be found by holding the Piece (now weigh'd, and in the Scales made of a Net of very fine Hair) into fair and clear Water, and obſerving by the relaxing of the Spring, how much the Piece grew lighter; for thereby the ſpecifick Gravity of the Metal itſelf, compar'd to that of Water, was exhibited; and this without making Uſe of differing, or indeed any Weight at all.

A Third Inſtrument for the ſame Purpoſe.

JAN. 9, 168$\frac{3}{4}$.

Scales copied from the *Royal Society*, Regiſt. Numb. VI. p. 136.



I SHEW'D a Module of a *Beam, whereby readily to find any aliquot, or aliquant Part of any Weight given.* The Beam was made in the ſame Manner

Manner as the firſt that was ſhewn; namely, that with a Cane; but whereas that was only then divided and deſign'd for Decimation, or Decuplation, the longer End of this was divided into 12 equal Parts, and the Face of the Beam was made ſo wide, as to be capable of admitting Subdiviſion by Diagonals. The ſhorter End was one twelfth Part of the longer; at which Diſtance, the great Scale was properly fixed, wherein the Weight, to be ſubdivided, was to be put: This Scale, when empty, counterpoiſed the longer End, without any Scale ſuſpended on it: And that the removing of a Scale might make no Alteration of the former *Equilibrium*, the Weight of the ſame was wholly taken off by a proper Counterpoiſe, ſo that the Scale had no Weight at all upon the Beam. The Way of finding any deſirable Part of a Weight given, was thus; If the Part were not ſmaller than a twelfth Part, then the ſame might be eaſily found by one Operation, by placing the Scale at ſuch a Diſtance from the Axis of the Beam, on the longer End, that the ſame ſhall be in ſuch Proportion to the ſhorter End, as the whole Weight is to the Part deſign'd; for Inſtance, having a Lump of *Ambergreaſe*, of an unknown Weight, but 'tis to be divided into three Shares, which are to be in Proportion, one to another, as 345, 234, and 123, to find each of theſe, I thus proceed; adding all the Proportions together, I find they make 702; then, by a Sector, by the Line of Lines, I open the Compaſſes to the Length of the ſhorter Shank of the Beam; and, by that, open the Sector to 345; then, on the ſame Sector ſo opened, I open the Compaſſes to 702, and ſet off that Diſtance on the longer Shank of the Beam, and there place the leſſer Scale; then putting in the Lump into the greater Scale, I counterpoiſe it in the leſs, and that gives

me the firſt Share, which is as 345 to 234, and 123; this Weight I lay by.

Then upon the ſame opening of the Sector, I take off 234, and ſetting it on the longer Shank, I place the leſſer Scale, and proceed as before; and this gives me the Weight of the ſecond Part, namely, 234. Then the Difference between the Sum of theſe two, and the whole, in a common Balance, gives me the third, *viz.* 123.

If the Part, to be found, be leſs than a twelfth Part, and not leſs than a one hundred forty fourth Part, by ſome previous Diviſion of it, by once weighing, I reduce it to ſuch a Part, as, by the ſecond weighing, I find the Part, to be found, will not be leſs than a twelfth; and then I proceed as before. This may be perform'd, either by finding two Dividers of the Part, both which ſhall fall within the Compaſs of 12; or, if it be a prime Number, then by extracting the Root of it; which may be done arithmetically in Decimals, to what Accurateneſs ſhall be deſir'd, or by a Line of Superficies on a Sector, or by a Table of Logarithms.

If the Part to be found, be leſs than a one hundred forty fourth Part, and not leſs than a ſeventeen hundred twenty eighth Part, then it muſt be perform'd by three Dividers, if ſuch can be found, that will fall to be each not leſs than a 12th, or elſe, by the Extraction of the Cubick Root. If the Part be leſs than a 1728th, and not leſs than a 20736th Part; then, by finding four Dividers, each, within the Compaſs of a twelfth, or by extracting the quadrato, quadratick Root, the Part may be obtain'd by four Operations.

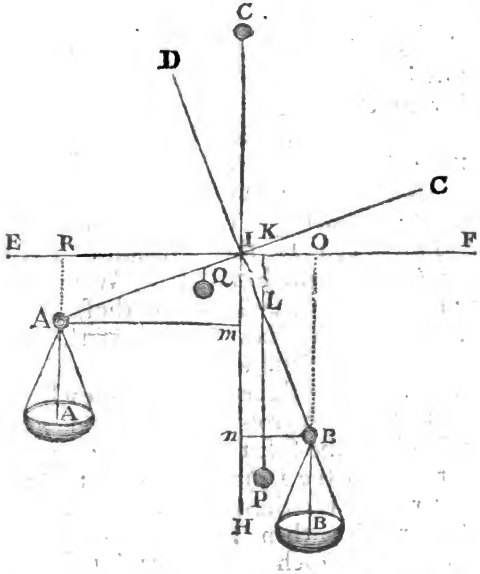
The

The fourth Inſtrument for weighing.

JAN. 16, 168½.

I SHEW'D a new *Inſtrument I had invented, by which, immediately, and without any Trouble, the comparative Weights of any two Bodies given, might be found; if, at leaſt, the Beam were of Bigneſs enough to bear them.* The Beam was made in the Form of a Croſs, equilibrated upon a ſharp Edge in the Center; the Scales were hung upon two Ends (not oppoſite, but) next together, which were alſo equilibrated; the ſmalleſt Weight, in either of the Scales, would make the Arm, by which it hung, to ſtand perpendicular, and, conſequently, the Arm that bore the other Scale, to lie horizontal. The Bodies to be weigh'd, were each of them put into the Scales, one in the one, and the other in the other; and ſo ſuffer'd to take their Poſture (which they would preſently do) by putting the Beam in ſuch a Poſture, that the Diſtances of their Points of bearing, from the Perpendicular under the Center, would be in reciprocal Proportion to their Weight. Dividing then the Arm, on which the greater Weight hung, into ten equal Parts, and each of thoſe into ten, and, if the Beam will bear it, each of thoſe again into ten, all of which, will make one thouſand equal Parts, I place three Pins upon each of the other Arms, which croſs the aforeſaid Arm at Right Angles; the firſt two, at the Extremities, the next two, at the Diſtance of one tenth from the Center, and the third Pair, at the Diſtance of one hundredth; then I provide two Bullets, equiponderant to each other when fitted, the one with a ſmall Clew, ſomewhat more than the Length of the longeſt Diagonal of the two ſuſpending

ing Arms, with a Ring at the End to hang upon one of the Pins, the other, with a Ring only. Then, according to the Difference of the Bodies counterpoiſing each other, I hang on the Plummet and Line upon that Pin of the Arm over the heavier Body, and is neareſt to the Extremity; from which the Plumb Line may fall upon the Diviſions of the Arm, and counterpoiſe it alſo with the Ring and Bullet hung upon the correſponding Pin on the oppoſite Arm, then ſhall the Plumb Line ſhew, upon the Diviſions, the proportionate Weight of thoſe two Bodies. I need not ſhew the great Uſe and Benefit there may be made of this Beam in all Philoſophical Inquiries, ſince they are obvious enough.



Let A C, B D, repreſent the Croſs Beam, moving

on

on I, the Scales hanging at A and B. The Weights being put, the heavier in B, the lighter in A, the Croſs poſiteth itſelf as in the Scheme in reſpect of the horizontal Line E F, and the Perpendicular G H; and their comparative Weight is found by their ſeveral Diſtances from the Perpendicular I H, that is, as B N to A M, ſo the Weight at A, to the Weight at B. Thus far is clear from the Principle of Staticks. Let K P repreſent the Plumb Line, ſuſpended at K; I ſay then, that I K, is to I L, as B N is to A M, or, as the leſſer to the bigger Weight; for A M, is equal to I N, and the Angle N I L, is equal to I L K, therefore K I L, is ſimilar to B N I, therefore as K I to I L, ſo B N to N I = to A M, ſo the Weight at A to the Weight at B. Q. E. D.

The Deſcription of a Pair of Japan *Scales, and a* Japan *Stilyard.*

JAN. 23, 168¾.

I PRODUCED, and ſhew'd three ſeveral Kinds of Beams, for weighing the Gravity of Bodies; the firſt, was a Pair of *Japan* Scales and Weights, made and adjuſted in that Country, and that with very great Care and Curioſity. The Beam was made of a round Rod of Braſs, tapering a little from the Middle towards the Ends; which were flatted perpendicularly, and had each of them a ſmall Hole drill'd through it, tapering both Ways to the Middle, leaving an Edge round the Middle of the Hole, through each of theſe Holes, was put a Braſs Ring of Wire, by which the Diſhes were ſuſpended by four Strings. The Cock, or Tongue of the Beam, was neatly ſoder'd into

into the Middle of the Beam, about two Inches broad below, and ⅓ of an Inch at the Top; and about the Middle, between the Beam and the Top, was put the Pin, upon which the Beam play'd; the Handle of the Beam was alſo made of a Kind of Ring of Braſs, and the lower Part thereof, was ſlit ſo as to receive the Cock, that it might juſt freely move between its Sides and no more; and the Pin reſted upon two Holes made in the Sides of the ſaid Handle; the Top of this Handle had a ſmall Tongue of Braſs, of the ſame Breadth with the Top of the Cock of the Beam, and pointing ſo directly at it, when in *Equilibrio*, and ſo near approaching it, as juſt not to touch it. This Beam was ſuſpended by a convenient Frame of Wood, as to hold it ſteady whilſt it was made uſe of; and to find exactly the *Equilibrium*, by giving a little Knock with a ſmall wooden Mall, upon the Handle, there was cauſed ſuch a ſhaking, as made every Thing ſettle into its due Place; and, by the Ends of the two oppoſing Cocks, or Tongues, the Agreement, or Difference, was diſcoverable. The Weights were all curiouſly, and very exactly, made of Braſs; which, that they might not be adulterated, were, all over the Surface of it, ſtamped with the Emperor's Seal, and the Quality of each engraven upon it in the *Chineſe*, or *Japaniſh* Characters. Theſe are, by a ſevere Penalty, prohibited to be exported into any other Place, and are of great Value in the Country itſelf. The Weights are *Cunderines*, *Maces*, and *Tales*; ten *Cunderines*, making a *Mace*; ten *Mace*, a *Tale*; and 10 *Tale*, one Pound *Troy*.

The Second was a *Japan* Stilyard, made upon the ſame Principle as our common Stilyards, but with greater Curioſity, and for ſmaller Weights, than we generally uſe them, ſerving to weigh any Weight from a *Cunderine*, to two Pound *Troy*, or twenty

twenty *Tale*. The Beam was made of a tapering Rod of Ivory; the Scale, or Diſh, at the greater End, was hung by a ſtrong Thread of Silk, which paſs'd through a Hole in the bigger End of it; inſtead of Handles alſo, there were three ſtrong Threads of Silk, at ſeveral Diſtances from the former, which paſs'd through three ſeveral Holes in the Beam; and to each of thoſe three handling Threads, was adjuſted a Line of Diviſions upon the Sides of the tapering longer Arm; the Weight was of Braſs, and ſuſpended on the longer Arm, by a ſmall Bow of Silk, which might be eaſily ſlipped to and fro, as Occaſion required. The whole Inſtrument was very compleat, and nice enough for the Purpoſes it was deſign'd for, to wit, for weighing Silver and Gold, *&c.* in the Way of Trade.

THE third, was a Stilyard of my own Invention, by which the Weight of any Body, that could be weigh'd in it, might be found without the Trouble of removing the Weight, as in the common Stilyard; and, by Means of a Plumb Line, after the Manner of the Balance I ſhew'd *January* the 16th, the particular Weight of whatever was put in the Scale, was preſently manifeſted; and it had this great Conveniency in it, that the Diviſions, by which the Weights were determin'd, were altogether as great at laſt, as at firſt, being all equal. The Conveniencies and Uſes, are obvious enough in the weighing, either of great or ſmall Bodies, all being to be perform'd with great Speed, and as great Exactneſs, and with much leſs Trouble.

[*In*

[*In the Minutes of the* Royal Society, *of* Octob. 25, 1677, *I find an Experiment made that Day, by Dr.* Hook, *which, for Congruity, I ſhall inſert here.*]

IT was a very eaſy Way to examine the comparative Weight of Liquors, and that to ſo great a Niceneſs, as very ſenſibly and manifeſtly to exhibit ſuch Weight of two Liquors, though they differ'd from one another, but a 1000000th Part of their Weight.

This was performed by the Help of a large Glaſs, of a Pear-like Form, equalling in Bulk about three Pound of Water; which, by Shot included in it, was made almoſt equiponderant to Water; but yet ſomewhat heavier, that it might juſt ſink to the Bottom; but by the fineſt Hair, tied to the Stalk, could be ſuſpended in the Water. This Hair was tied to the Scale of a Beam; and this Poiſe, by a Counterpoiſe in the other Scale, was made to ſwim in the Water, ſo as neither to touch the Bottom, nor the Top. And when ſo poiſed, it was found, that a 5th Part of a Grain added to, or taken from the Scale, would make the Glaſs-Pear riſe to the Top, or ſink to the Bottom. Whence it was evident, that the whole Glaſs, weighing about four Pounds (which amounts to 22040 Grains, or 220400 tenth Parts of Grains) and that one ſingle tenth Part of a Grain would turn it. And the Glaſs, when ſuſpended, being always equal to an equal Bulk of Water, if that Weight be alter'd a 220400th Part, the Poiſe muſt be alter'd, and conſequently, by Help of the Scales, be made ſenſible.

This

THIS Experiment, and the Nicety thereof being underſtood by the Company, it was deſir'd, that Trials might be made the next Day upon ſeveral Sorts of Water, as Pump-Water, new River Water, *Thames* Water, and Rain-Water, that ſo they might be experimentally ſatisfied of the Exactneſs of this new Inſtrument: Which is new upon this Account, that it hath not been taken Notice of by any of thoſe who have written on this Subject; as *Ghetaldus*, *Stivinus*, *Paſchal*, &c. they having only taken the comparative Weight of ſome ſmall Counterpoiſe within, and out of the ſame Liquor, which they have always perform'd with the ſame Scales, which are no Ways fit for exhibiting the Niceneſs and Curioſity of this Experiment.

ON *November* the firſt following, the Experiment was accordingly made, and it was found, that two Grains of Salt, being put into two Gallons of Water, caus'd the Counterpoiſe to be conſiderably lighter: Which was found to be ſo, upon repeated Trials.

Dr.

Dr. Hook's *Experiment before the* Royal Society *Feb. 6, 168⅘, concerning Magnetism in Drills, &c.*

THEN produced the *Apparatus* for the Experiment appointed me laſt Meeting, in order to make out my Aſſertion, that the magnetical Virtue in Steel might be excited, and conſiderably increaſed by a Body not generally accounted magnetical; and therefore, that the affirming a Body to be magnetical, becauſe it excited that Virtue would not always hold good. The Experiment I made, to examine this Opinion, was this. I took a Drill made of Steel; and, leſt it ſhould have had any determinate Virtue in it, as to Polarity, I heated it red hot in the Fire, and ſo ſuffer'd it to cool, quenching only the very drilling Point of it in cold Water: When it was perfectly cool, I apply'd a Needle to it, and found, that which End ſoever I turn'd downward, it would attract the South End of the Needle, and the upper End would attract the North; and this, as often as I repeated the turning of the Drill, and apply'd the Needle to the Ends of it. So that it plainly appear'd to have no determinate Polarity at all, as a Drill, or the like Piece of Steel, touch'd by the Loadſtone. Then I cauſed a Piece of Braſs to be put upon a Table, and holding the Drill very near with the ſame Inclination, and in the ſame Line, that a Dipping Needle left free, when well poiſed, would ſituate itſelf; I cauſed the Drill to be mov'd with a Drill Bow, ſo as to drill a pretty deep Hole in the ſaid Piece of Braſs, and thereby to warm or heat the Top thereof. Then, examining it again with the Needle, as I had done before, I found that the Drill by this Boring, or Agitation,

gitation, had acquired a Polarity or directive Virtue, as well as an attractive for the Point of the Drill, which, in drilling, reſpected the North, whether it were held downwards or upwards, always attracted the South End of the Needle; and the contrary End in like Manner, in either Poſture, attracted the North, in the ſame Manner as if the Point thereof had been really touched with the Needle. In the like Manner, I found by trying with a Steel Chizzel by ſtriking of its End, when placed in the proper Poſition of the Dipping Needle, that much the ſame Effects would be produced.

HEREUPON it was objected, that Braſs itſelf was a magnetical Body, and therefore that this was not a ſufficient Eviction; whereunto I replied, that I conceiv'd any other hard Body, placed inſtead of the Braſs, would produce much the ſame Effect.

I DID therefore propound to have the ſame tried with hard Wood, Ivory, Bone, Glaſs, or Stone, which have not hitherto been accounted magnetical Bodies, to ſee whether they would not be a Means of exciting this magnetical Virtue; for if ſo, then either all Bodies, that are hard, muſt be ſaid to be ſaid to be magnetical, or elſe it will not neceſſarily follow, that every Body that excites this Virtue, is therefore to be eſteem'd magnetical. And this the rather, becauſe as I have, in Part, ſhewn in this Place, and as I ſhall hereafter make out more at large, there may be produced in other Bodies, as well as Steel, Iron, or the like, a Quality much reſembling that of the magnetical; wherein, notwithſtanding, neither the Magnet, Steel, Iron, or the magnetical Virtue, or Power of the Earth, is any Way concerned.

Dr. Hook's *Experiment, about the Strength of Ice.*

Next, I gave an Account of an Experiment, which I had cauſed to be tried in the Preſence of Mr. *Meredith*, and Dr. *Aglionby*, of a Piece of Ice, plain'd true Square, of about fifteen Inches in Length, four Inches broad, and $3\frac{1}{2}$ Inches thick; this was pretty ſolid, having no more Blebs in it than common Ice uſually hath. This Piece of Ice, ſo ſquar'd, was plac'd upon the Engine made on Purpoſe for examining the Strength of Bodies, as to bearing. The Places, whereon the two Ends reſted, were juſt twelve Inches aſunder, and the Bar, whereon the Weights reſted, was juſt placed in the Middle of the Piece of Ice, between the two bearing Cheeks, ſo that the Line of Preſſure, the Bar being round, was at ſix Inches Diſtance from each of the bearing Cheeks; the broader Part of the Ice, was placed horizontal, and the narrower, was placed perpendicular. All Things being thus fitted, we applied the Weight to the two Leavers of the Engine, and began at fifty Pounds; then mov'd them to 100, 150, 200, 250, and 300, ſuffering the Weights to preſs the Ice for ſome Time, at every of theſe Poſitions, the Ice ſtill bearing them, without breaking, or in the leaſt cruſhing, either by the bearing Cheeks, on which it reſted, or under the round Iron Bar that reſted on it; then removing the Weights to 350, and ſuffering them to reſt upon it, in a very ſhort Time, the Ice broke ſhort in two, juſt under the Iron Bar, though it did not appear at all to be cruſhed, at any of the three bearing Places.

This

THIS Experiment was tried, in order to find, firſt, the Hardneſs of this Body, which is produced by Cold, out of the fluid Body of Water, without the Mixture of any ſenſible ſolid Body, or, is rather the primitive Body, out of which, the fluid Body of Water is made, by a very ſmall Degree of Heat, the Difference between the greateſt Degree of Heat, it will ſuſtain without being thawed, and the leaſt Degree it will ſuſtain without being frozen, being ſo very near the ſame, that one's Senſe will not diſcover it, and even a Thermometer, but very little. So that if Heat and Cold, only, be the Cauſes of theſe Mutations, it is the greateſt Inſtance in Nature of ſo conſiderable a Change of Texture, upon ſo inconſiderable an Alteration of the Cauſes.

Secondly, In order to find the Tenacity or Strength of this Body for bearing, and thence, to give ſome Reaſon, how it comes to bear ſo great Weights, moved, or reſting upon it, without being broken, when it covers the Top of a River or Pond, as has been now ſufficienetly experimented upon the *Thames*. And though the Manner of bearing, when the Ice floats upon the Water, be very differing from the Way of bearing in this Experiment, and ſo the Calculation holds not the ſame in the one and the other; yet this Way of Trial is a neceſſary Ingredient of ſuch a Calculation; ſince, without knowing the Stiffneſs of Ice, as to bending or breaking, and the Hardneſs of Ice, as to cruſhing, ſuch a Calculation cannot be perform'd. The Caſe alſo varies very much from the Manner of the Boundings, and the Bigneſs of the Piece of Ice, whoſe Strength is to be calculated. For in a Pond, where the Edges of the Ice are firſt frozen to the Ground, and ſo the Water underneath being pent in from being able to get out, the Reſiſtence of the Water hinders the breaking

of it, even till the reſting Weight begins to cruſh it. And 'tis much the ſame, where the Surface of the Ice is very large, though it no where toucheth or reſteth upon a ſolid Body at its Brims, there being ſo great a Length of Water to be moved, before the Water underneath can give Way to the breaking of the Ice. We muſt alſo conſider the Weight, as bearing in the Center of a round Flake, which is very differing from that of an oblong Shape. To this Calculation we muſt likewiſe take in the riſing of thoſe ambient Parts of the Ice, which at a Diſtance encompaſs the bearing Center, ſince the Ice can hardly deſcend in the Center, without at the ſame Time raiſing ſome circumferential Parts, which are more difficult to be broken upwards, than the Center to be broken downwards.

An Experiment of Dr. HOOK's, *concerning the ſwelling of Water by Freezing.*

THE third Experiment I tried was upon Occaſion of a Report of Dr. *Crone*, of an Experiment try'd by himſelf, of applying the freezing Mixture to a Glaſs of Water, and obſerving the Water to riſe in the Neck of the Glaſs, before any Part of the Water was frozen. Whence he conceived that the Water itſelf did actually expand by its Application, before it came to freezing. The Reaſon of which Phænomena I conceived to proceed only from the ſhrinking of the containing Veſſel, and not from the expanding of the Water, before freezing: To elucidate which, I tried the Experiments I had formerly ſhewn, to prove the ſwelling of Glaſs by Heat, and the ſhrinking of it by Cold; as alſo divers other Phænomena, which are manifeſtly to be aſcribed to the ſhrinking

ing and ſwelling of the containing Glaſs Veſſel, and not at all to the ſwelling and ſhrinking of the Liquor contain'd; as the dipping ſuch a Glaſs of Water, in hot Water, will preſently make the Water deſcend in the Neck; and the dipping the ſame in Water colder, then the Water in the Glaſs, or then the Glaſs it ſelf, will make the ſame Water riſe for ſome Time in the Neck of the Veſſel. However, tho' ſome Trials were made, whoſe Effects ſeem'd, to me, plainly to concur with this Explication, yet the Doctor, and ſome others, ſeem'd yet to doubt, whether the Water it ſelf did not actually ſwell by the Application of the freezing Mixture, before it actually began to freeze; which if theſe Trials do not ſatisfy, there may be ſeveral other Ways made uſe of to find the ſwelling of the Glaſs by Heat, and the ſhrinking of it by Cold. But I conceive no Experiment can be made that will prove Water, without freezing, to be dilated or expanded by Cold, or contracted or condenſed by Heat.

Dr. Hook's *Experiments*, Feb. 13. 1683-4. *Shewing the Specifick Gravity of Ice*, &c.

I Took then a Piece of Metal big enough to ſink the Piece of Ice, I deſigned to examine, to the Bottom of the Water, that ſo the compound Body of Ice and Iron might have a ſenſible Gravity in the Water. Then letting it down into the Water, which I had ſet conveniently in a Glaſs, that I might ſee this Compound freely to ſwim to and fro clear below the Surface; the Scales being conveniently ſuſtained by a Frame, I counterpoiſed it exactly to an Equilibrium, and found it to amount to 1933$\frac{4}{7}$ of 3000 Parts of a Pound Troy, which were the Weights to which I reduced this, and all the other Counterpoiſes. Then I ſuddenly lifted up the Ice and Iron into the Scale, and ſo counterpoiſed it in the Air, and found the ſame to be 2567$\frac{6}{7}$ of the ſame Parts; then I took off the Ice, dry'd the Scale, and let the Iron Weight hang by the ſame String in the Water; and counterpoiſing it, I found it to amount to 1984$\frac{1}{7}$ of the ſame Parts; then lifting the Iron out of the Water, and putting it into the Scale, I found it to be counterpoiſed by 2209$\frac{1}{7}$ of the ſame Parts. Thence the Weight of the Water, equal in Bulk to the Ice and Iron, was 634$\frac{2}{7}$ of the like Parts, and the Weight of the Water, equal to the Ball, was 224$\frac{6}{7}$; thence the Weight of the Water, equal to the Ice was 409$\frac{4}{7}$, and the Weight of the Ice in the Air was 358$\frac{5}{7}$, and conſequently the Weight of the Ice in Water was 50$\frac{7}{7}$; that is, the Weight of the Ice, to that of the Water, was very near, as 7 to 8; that is, the Ice was lighter than the Water, by an eighth Part of the Weight of the Water; or the Water heavier than the Ice, by a ſeventh Part of

the Weight of the Ice. So that the Expanſion of the Ice, to the Expanſion of the Water, was as the Weight of the Water, to the Weight of the Ice; that is, as 8 to 7: So that the Water, by its freezing, becomes expanded one ſeventh Part of its Bulk, and conſequently that 7th Part muſt float above the Surface of the Water, and ⅞ of the Bulk of Ice muſt remain immerſed in the Water, ⅛ Part of the Bulk of the Ice floating above it.

The Ice I made uſe of, in this Experiment, was not very full of Blebs, or Bubbles; nor was it perfectly free of them, but of a middling Nature, which may pretty well hold, as a Standard, or common Meaſure of a great Congeries of ſeveral Sorts of Ice, ſome of which may be much more porous, and ſome much leſs, as I have had Occaſion ſeveral times to obſerve, in this great Froſt. The Time, in which I try'd this, was pretty warm, and ſo it thawed; and the Water having ſtood all the Day, expoſed to the Air, was conſequently much of the ſame Temper; and thence I counterpoiſed the Ice and Iron firſt in the Water, and then preſently lifted it out of the Water into the Scale, ſo that all that levitated in the Water was immediately put in the Scale: The Water was ordinary Pump, or Well-Water, and is accounted a pretty good freſh Water; which Circumſtances I mention, as having Significancy, as will by and by appear.

For from this Experiment it plainly appears, that the common Opinion that the Ice, upon a ſudden Thaw, ſinks to the Bottom, is falſe, tho' never ſo confidently aſſerted by the Water-men: For in this Experiment, where the Water was pretty warm, in reſpect of Ice, and thawed the Ice very faſt; yet an eighth Part of the Ice floated above the Water, and Water by Heat, without boiling, will not expand near that Proportion:

Nay, I have found, that throwing in a Piece of Ice into Water boiling, it ſtill floated, and ſunk not, much leſs can it ſink in a tepid Water upon a Thaw.

Next, from hence we may collect, that in the Northern Seas, at leaſt one Eighth Part of the Bulk of any Body of Ice floats above the Water: I ſay, at leaſt an Eighth; for poſſibly it may be one Seventh; for firſt (as is affirmed by many Voyagers to the Northern Seas) the Ice is found to be pretty freſh, and to have little or no Taſte of Brackiſhneſs; and ſo, one Part taken with another, not heavier than this Ice I made uſe of. Next, the Water, notwithſtanding, in which it floats, is ſalt, and and conſequently about a 40th Part heavier than common freſh Water. Thirdly, This ſalt Water, tho' it do not freeze, is yet pretty near the ſame Degree of Coldneſs with the Ice that floats in it, and conſequently yet more heavy than the ſame Water when more tepid. For as I ſhall hereafter prove, Bodies that freeze not, are yet not leſs cold than other Bodies that do freeze. Fourthly, That the Sea-Water, near the Bottom, is yet much more cold, and much more ſalt, than in the ſame Place it is near the Top, and conſequently muſt much contribute to the floating of a greater Part of the Ice. That the Water is colder at the Bottom, than above, was poſitively affirmed by Mr. *Roachford*, who try'd it in the *Sound*; and that ſalt Water is ſalter at the Bottom, than at the Top, any one may find.

All which Particulars conſider'd, it will not ſeem altogether ſo incredible, or indeed ſtrange, that there ſhould be floating Iſlands of Ice in the frigid Zones, of ſo great a Height above the Surface of the Sea: For, ſuppoſing it to be globular, above a 4th Part of its Diameter muſt float above the Water, to make a 7th Part of its Bulk to float,

and

and conſequently the Depth of the Ice under Water need not be ſo very great, to make ſo great a Height above the Water; but if the upper Parts of it above the Water are yet much higher, and more ſpongy than ſolid Ice, as conſiſting, in great Part, of Accumulations of Snow, then may that Height, above the Water be raiſed much higher, and be made poſſibly to equalize, if not exceed, even the Depth of the Ice below the Surface of the Water, eſpecially if the Bottom of the ſaid Iſland be flat, as moſt probably it is, and as broad, if not broader, than the Compaſs of it at the Surface of the Water; as alſo if Parts above the Water be tapering, like a Pyramid, to the Top. Again, If the lower Parts of the Sea, in thoſe Parts, are colder than at the Top, as probably it may be in the Spring, the freſher Parts of the Water may be congealed, even at the Bottom, and ſo augment the Bulk of it by new Accretions underneath, and ſo continue to buoy it up more and more, and ſo raiſe the upper Parts more and more into the Air. And conſonant to this we find, that the greateſt Iſlands of Ice are found in the Spring, after the Winter is paſt, and the Air begins to have a Tepidneſs in it; and not ſo much, if at all, in the former Part of the Winter, when it freezes more violently at the Top of the Water.

As to the Reaſon why Water, when of ſuch a Degree of Temperature, becomes ſo ſolid a Body; and why, when of another Temperature, it becomes ſo fluid, I ſhall not now ſpend your Time in explaining, deſigning to do it in my General Theory of natural Operations. This only I ſhall mention here, by the by, that the Body of Ice, tho' very hard, is very little ſonorous, in reſpect of Glaſs, which to the Sight it ſo much reſembles: That the Blebs in it are not Vacuities, but a Kind of

of Air, which has its expanſive Power, or Elaſticity, as well as common Air: That this Air does not, upon the Thaw, retreat into the Water, as it ſeems to come out of it upon the freezing, as by Experiment I have found.

Farther Experiments, made Feb. 20. 1683-4. *by Dr.* Hook, *before the* Royal Society, *concerning the Phænomena of Ice.*

The proceeding Diſcourſe was read, and ſome Matters therein more particularly explained by Deſcription partly, and partly alſo by Experiments.

The Experiments were firſt to ſhew, that the Blebs in Ice (ſuppoſed by ſome to be Vacuities, like the Blebs in Glaſs Drops) are filled with Air, which has the ſame Properties with common Air. I took then a Piece of Ice, and putting it into Water, which was tepid, as having ſtood in a warm Room, by which the outward Parts of the Ice quickly thawed, and ſo there remained nothing at all of Air ſticking to the Outſide of it; then whelming a Cup-Glaſs clear over it, which was perfectly filled with Water, and had no Air included in it, I ſuffered it to remain, covering the Lump of Ice, till the whole was thawed, or melted into Water; and it was plain to be ſeen, that as the Ice thawed, the Blebs that were viſible in it, before the Thaw, did aſcend to the Top of the whelmed Glaſs, and then unite with one another into a conſiderable Body of Air.

The ſecond was to ſhew that Water, though boiling hot, would yet be ponderous enough to make the Ice to ſwim and float in it. This was done by putting a Piece of Ice into a Veſſel of boiling

boiling Water: And the Ice continued to float upon it till it was all melted.

The Reaſon of the Experiment was in order to find out the Nature of the Expanſion of freezing Water, and the true Cauſe thereof; which ſeems to contain as many difficult Phænomena in it to be explain'd, as any other in Nature: For firſt, this Body of Ice ſeems heterogeneous to all other Bodies; which being melted, and ſuffered to cool and grow hard, are ſtill condenſed and ſhrunk into a leſſer and leſſer Room, as they grow colder; as is very obſervable in all Sorts of Metals, as Gold, Silver, Copper, Tin and Lead, every of which, when they are melted, take up more Space, or are more expanded, than when they are grown cold and hardened; as one may preſently find, by caſting any of them into a Mould, and obſerving the ſetting, or ſhrinking of the Gitt, by which the Mould is fill'd; or by ſuffering the whole Body, ſo melted, to remain, and grow cold and ſolid in the Ladle or Crucible; for 'tis evident that the top Surface, which, when melted, is protuberant, and ſwelling upwards; when cold, it is flatted, and very often concave. And ſometimes alſo, in ſome Metals, it is crumpled, and ſhrunk into curious Figures; as is very remarkable in *Regulus Martis*, made with *Antimony*, which is therefore called *Stellatry*, for that it hath ſome Reſemblance to the Figure we generally make for a Star, *viz.* ſix Radiations from its Center. 'Tis evident alſo in Tin and Lead; Wax alſo, and ſome reſinous Subſtances, ſhrink upon hardening after the ſame Manner, and Fatt, or Tallow of Animals; ſo all Sorts of Vitrifications and Glaſſes, and all Sorts of Oils, that will harden, and Butter, which alſo grow opaque. But Water, when it paſſes from Fluidity to Solidity, proceeds very differing; *Firſt*, In its inſtantaneous Change.

2dly,

2dly, In its Expanſion, or Rarefaction. *3dly*, In its Tranſparency. *4thly*, In its Refractiveneſs. *5thly*, in its Generation of Blebs, or Bubbles. *6thly*, In its Power of Expanſion: tearing and rending to Pieces the ſtrongeſt metalline Bodies that impriſon it; when, as yet, it leaves Room enough for the ſmall Particles of Air to expand, if at the ſame time it may not be ſaid to ſuck it in; for I do not find that the impriſon'd Blebs are at all preſs'd, nor is their Spring at all the Cauſe of this Expanſion; for by obſerving the thawing of a Bleb in the Ice, I did not find the Bubble that roſe from it to be any bigger in Bulk, than the Bleb that contain'd it; whereas if the Air in the Bleb ſhould be preſſed with as great a Force, as the Strength of the Inſide of the containing Veſſel amounts unto, it muſt of Neceſſity reduce the Air to near a thouſandth Part of its natural Extenſion; and conſequently, when the Bleb comes to be thawed, and ſo ſet at Liberty, it muſt at leaſt, I ſay at leaſt (by reaſon it then ſuffers a greater Degree of Heat, than when it is frozen) expand itſelf into a Bulk a thouſand Times bigger; but there is no ſuch Appearance that I could obſerve. Several Authors have endeavour'd to give Solutions of this Phænomenon, as particularly the ingenious Mr. *Des Cartes*, who ſuppoſing the Particles of Water to be very long and limber Bodies, like ſo many Eels, whilſt, as it were, kept alive, and agitated by this *Materia Subtilis*, are limber, and ſo eaſily complicate and ſlide one within another, and ſuffer the *Materia Subtilis* to have its Paſſage free through them every Way; but when there is leſs Agitation of this *Materia Subtilis*, they do, as it were, die, and grow ſtiff and rigid, and ſo will not ſo eaſily comply to the Figures of each other, but grow ſolid and hard: But then 'tis to be conſider'd, that the greater Plenty there is of the *Materia Subtilis*, the

the greater muſt be the Agitation of them; as he aſſerts in the Explication of the Particles of the Air, and conſequently the more Room muſt they take up, and ſo be more expanded when fluid, then when ſolid. Another late Author ſuppoſes, that Congelation is made by a *Sal Armoniack*, breathed, or exhaled from Animals, which, in cold, froſty Weather, is very copious in the Air, which *Sal Armoniack* does then inſinuate into the Pores of the Water, and ſo wedge up all the Pores, and widen them, and ſo make the Parts of the Water to coaleſce into a hard Body. But this I conceive to be alſo hypothetical, and not experimentally proved; for tho' there may be ſome volatile Salts in the Air, yet 'tis pretty difficult to conceive there ſhould be ſo great a Quantity, as at once to wedge up all the Water of the Northern Part of the Earth, and yet, at the ſame Time, we ſhould not ſmell it; beſides, we do not find that the *Sal Armoniack* Spirit does perform this Effect, when it is raiſed in the Air at other Times; nor does the *Sal Armoniack* it ſelf, when mixed with Water or Ice, do it; for we find that *Sal Armoniack*, ſtrow'd on Ice, will the ſooner make it thaw, and reſolve again into Water, than make it freeze harder: Others have given differing Explanations, but I have not met with any yet, that, in my Opinion, give a clear and ſatisfactory Solution of it. Nor ſhall I at preſent trouble you with Theories, or Speculations, which ſome may poſſibly have a Prejudice againſt; only ſuffer me to acquaint you with a Phænomenon or two, which, if you think any of them worth ſeeing, you may have tried, for they are very obvious, plain, and neither difficult nor chargeable Experiments, tho' poſſibly as inſtructive as the moſt difcult, chargeable, or pompous Experiments, to ſhew ſome Sorts of Expanſion.

TAKE

TAKE then a Urinal, and fit into it a Stopple of a dry Piece of Wood; then put the End of this Stopple into a Diſh of Water, and you will find, in a little Time, the Stopple will grow ſo much bigger, as to break the Urinal.

Secondly, TAKE another Urinal, and fill the ſame with Peaſe; then filling it up with Water, ſtop the ſame with a Cork, which you may tie down faſt with a Packthread; then let it remain ſome Time, and you will find the Peaſe will ſwell and break the Glaſs.

Thirdly, TAKE Plaiſter of *Paris*, or burnt Alabaſter, and put it into a wooden Diſh, and temper it with Water, till it be very ſoft and fluid, that it may be eaſily poured out; then with this Mixture fill a Urinal or Vial top-full, ſuffer it to ſtand upright till it ſets into a ſolid Body, and you will find it ſwell and break the Glaſs.

Dr. HOOK's *Diſcourſe to the* Royal Society, May 21. 1684. *ſhewing a Way how to communicate one's Mind at great Diſtances.*

THAT which I now propound, is what I have ſome Years ſince diſcourſed of; but being then laid by, the great Siege of *Vienna*, the laſt Year, by the *Turks*, did again revive in my Memory; and that was a Method of diſcourſing at a Diſtance, not by Sound, but by Sight. I ſay therefore 'tis poſſible to convey Intelligence from any one high and eminent Place, to any other that lies in Sight of it, tho' 30 or 40 Miles diſtant, in as ſhort a Time almoſt, as a Man can write what he would have ſent, and as ſuddenly to receive an Anſwer, as he that receives it hath a Mind

Mind to return it, or can write it down in Paper. Nay, by the Help of three, four, or more, of ſuch eminent Places, viſible to each other, lying next it in a ſtreight Line, 'tis poſſible to convey Intelligence, almoſt in a Moment, to twice, thrice, or more Times that Diſtance, with as great a Certainty, as by Writing.

For the Performance of this, we muſt be beholden to a late Invention, which we do not find any of the Antients knew; that is, the Eye muſt be aſſiſted with Teleſcopes, of Lengths appropriated to the reſpective Diſtances, that whatever Characters are expoſed at one Station, may be made plain and diſtinguiſhable at the other that reſpect it.

Firſt, For the Stations; if they be far diſtant, it will be neceſſary that they ſhould be high, and lie expoſed to the Sky, that there be no higher Hill, or Part of the Earth beyond them, that may hinder the Diſtinctneſs of the Characters which are to appear dark, the Sky beyond them appearing white: By which Means alſo, the thick and vaporous Air, near the Ground, will be paſſed over and avoided; for it many Times happens, that the Tops of Hills are very clear and conſpicuous to each other, when as the whole interjacent Vale, or Country, lies drowned in a Fog. Next, becauſe a much greater Diſtance and Space of Ground becomes viſible, inſomuch that I have been informed by ſuch, who have been at the Top of ſome very high Mountains, as particularly at the Top of the *Pike of Teneriff*, that the Iſland of the *Grand Canaries*, which lies above 60 Miles diſtant, appears ſo clear, as if it were hard by; and I myſelf have often taken Notice of the great Difference there is between the appearing Diſtance of Objects ſeen from the Tops and Bottoms of pret-

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ty high Hills, the ſame Objects from the Top appearing nearer and clearer by half, and more than they do when viewed from lower Stations of the Hills; and this not only when the Space between them was Land, but where it was nothing but Sea. I have taken Notice alſo of the ſame Difference from the Proſpect of Places from the Top of the Column at *Fiſh-ſtreet-Hill*, where the Eye is, in good Part, raiſed above the ſmoaky Air below.

NEXT, the Height of the Stations is advantageous, upon the Account of the Refractions or Inflections of the Air; which Inflections of the Air are many and very great, ſometimes in an Air which ſeems, to the naked Eye, the moſt clear and ſerene. Inſomuch that That alone does wholly confound the Diſtinctneſs of Objects appearing at a Diſtance; now the greateſt Part of theſe ariſe from Commotions of the more denſe Air that is near the Surface of the Earth, by the Rarefactions of ſome Parts of it, cauſed by Heat; which rarified Parts aſcending, do make the Objects ſeen through it, to ſeem to dance and undulate, which is in great Part avoided, if the Proſpect be from an higher Place. Beſides, the Nature of the Air itſelf, at great Heights, approaches nearer to the Nature of the *Æther*, which more powerfully propagates the Impulſes of Light.

NEXT, in chuſing of theſe Stations, Care muſt be taken, as near as may be, that there be no Hill that interpoſes between them, that is almoſt high enough to touch the viſible Ray; becauſe in ſuch Caſes, the Refraction of the Air of that Hill will be very apt to diſturb the clear Appearance of the Object, as I have often obſerv'd.

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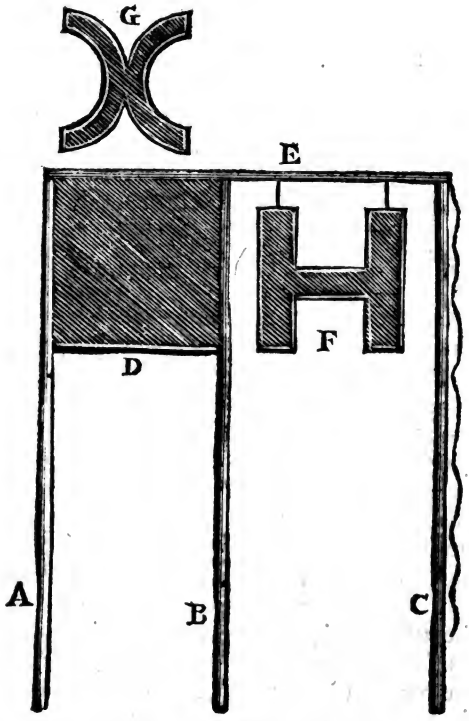
THE Stations being found convenient, the next Thing to be conſider'd, is, what Teleſcopes will be neceſſary for ſuch Stations. And though 'tis true in all, that the longer the Teleſcopes are, provided they are good, the better they will be for this Effect; yet ſomewhat of Limitation is requiſite, at leaſt, that they be not ſhorter than certain Limits for ſeveral Diſtances. Theſe may be as follows: For 1 Mile, 1 Foot; for 2 Miles, 2 Foot; for 3 Miles, $3\frac{1}{4}$ Foot; for 4 Miles, $4\frac{1}{2}$ Foot; for 5 Miles, 5 Foot 10 Inch. for 6, $7\frac{3}{4}$ Foot; for for 7 Miles, 8 Foot 9 Inch. for 8, $10\frac{1}{2}$ Foot; for for 10 Miles, 13 Foot, and ſo forward. One of theſe Teleſcopes muſt be fix'd at each extreme Station, and two of them in each intermediate; ſo that a Man, for each Glaſs, ſitting and looking through them, may plainly diſcover what is done in the next adjoining Station; and, with his Pen, write down on Paper the Character there expoſed, in their due Order; ſo that there ought to be two Perſons at each extreme Station, and three at each intermediate; ſo that, at the ſame Time, Intelligence may be convey'd forwards and backwards.

NEXT, there muſt be certain Times agreed on, when the Correſpondents are to expect; or elſe there muſt be ſet at the Top of the Pole, in the Morning, the Hour appointed by either of the Correſpondents, for acting that Day; if the Hour be appointed, Pendulum Clocks may adjuſt the Moment of Expectation and Obſerving. And the ſame may ſerve for all the other intermediate Correſpondents.

NEXT, there muſt be a convenient *Apparatus* of Characters, whereby to communicate any Thing with great Eaſe, Diſtinctneſs and Secrecy. There muſt be therefore, at leaſt, as many diſtinct Characters, as there are neceſſary Letters in the Alphabet that is made uſe of, (as is expreſſed in *Fig.*

1.) And thoſe muſt be either Day Characters, or Night Characters: If they are to be made uſe of in the Day-time, they may all be made of three ſlit Deals, moving in the Manner I here ſhew, and of Bigneſs convenient for the ſeveral



Diſtances of the Stations for which they are made, that they may be viſible through the Teleſcope of the next Station. Any one of which Characters may ſignify any one Letter of the Alphabet, and the whole Alphabet may be varied 10000 Ways; ſo that none but the two extreme Correſpondents ſhall

ſhall be able to diſcover the Information convey'd; which I ſhall not now inſiſt on, becauſe it doth more properly belong to Cruptography. If the Characters are for the Night, then they may be made with Links, or other Lights, diſpoſed in a certain Order, which may be veiled, or diſcovered, according to the Method of the Character agreed on; by which, all Sorts of Letters may be diſcovered clearly, and without Ambiguity.

THERE may be various Contrivances to facilitate and expedite the Way of diſplaying and expoſing theſe Characters to View, and of withdrawing, or hiding them from the Sight; but this I here ſhew, I conceive, will be as eaſy and ſimple as any: All which may be expoſed at the Top of a high Pole, and by two ſmall Lines moved at the Bottom, ſo as to repreſent any Character.

BY theſe Contrivances, the Characters may be ſhifted almoſt as faſt, as the ſame may be written; ſo that a great Quantity of Intelligence may be, in a very ſhort Time, communicated.

THERE will be alſo requiſite ſeveral other Characters, which may, for Expedition, expreſs a whole Sentence, to be continually made uſe of, whilſt the Correſpondents are attentive and communicating. The Sentences, to be expreſs'd by one Character, may be ſuch as theſe, in *Fig.* 2. O *I am ready to communicate.*)(*I am ready to obſerve.* (*I ſhall be ready preſently.*) *I ſee plainly what you ſhew.* ◡ *Shew the laſt again.* ◠ *Not too faſt. Shew faſter. Anſwer me preſently.* Dixi. *Make Haſte to communicate this to the next Correſpondent. I ſtay for an Anſwer*; and the like.

ALL which may be expreſs'd by ſeveral ſingle Characters, to be expos'd on the Top of the Poles, by themſelves, in the following Manner, ſo as no Confuſion may be created thereby.

I COULD inſtance in a hundred Ways of facilitating the Method of performing this Deſign with the more Dexterity and Quickneſs, and with little Charge; but that, I think, will be needleſs at preſent, ſince whenſoever ſuch a Way of Correſpondence ſhall be put into Practice, thoſe, and many more than I can think of at preſent, will of themſelves occur; ſo that I do not in the leaſt doubt, but that with a little Practice thereof, all Things may be made ſo convenient, that the ſame Character may be ſeen at *Paris*, within a Minute after it hath been expoſed at *London*, and the like in Proportion for greater Diſtances; and that the Characters may be expoſed ſo quick after one another, that a Compoſer ſhall not much exceed the Expoſer in Swiftneſs. And ſo great Expedition may not only be performed at the Diſtance of one Station, but of a hundred; for ſuppoſing all Things ready, at all thoſe ſeveral Stations, for Obſerving and Expoſing, as faſt as the ſecond Obſerver doth read the Characters of the firſt Expoſer; the ſecond Expoſer will diſplay them to the Obſerver of the 3d Station, whoſe Expoſer will likewiſe diſplay them for the 4th Obſerver, as faſt as his Obſerver doth name them to him, or write them down.

THERE may be many Objections brought againſt this Way of Communication; and ſo many the more, becauſe the Thing has not yet been put in Practice. But, I think, there can hardly be any ſo great, as may not eaſily be anſwered and obviated.

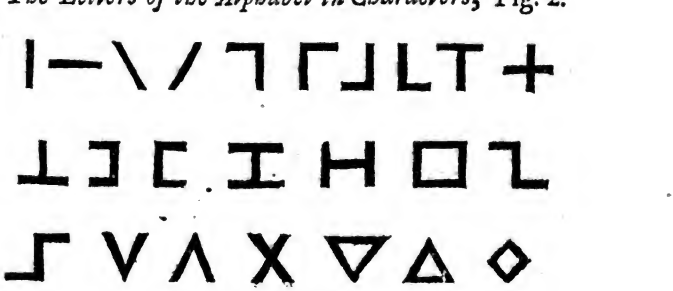
THERE

THERE may be many Uſes made of this Contrivance, wherein it will exceed any Thing of this Kind yet practiſed; but I ſhall not now ſpend Time to enumerate them; only in two Caſes, it may be of ineſtimable Uſe. The firſt is for Cities or Towns beſieged; and the ſecond for Ships upon the Sea; in both which Caſes, it may be practiſed with great Certainty, Security, and Expedition.

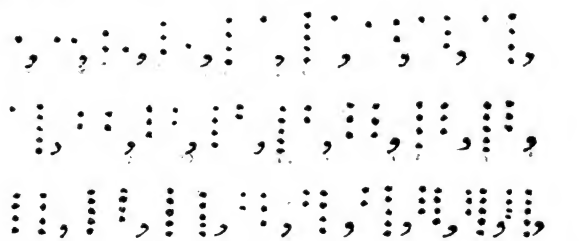
A farther Explication of the Figures.

LET ABC *(Fig.* 1.*)* repreſent three very long Maſts or Poles erected. E the Top-piece, that joins them all together. D, a Screen, behind which, all the Deal-board Characters hang upon certain Rods or Lines, and may (by the Help of ſmall Lines coming down from the Bottom of each of them) be expoſed at F, or drawn back again behind D, as Occaſion ſhall be. G is the Character for a Sentence agreed on, *&c.*

The Letters of the Alphabet in Characters, Fig. 2.



All the Alphabet, or requiſite Characters, may be diſtinctly, and without Ambiguity, expreſſed. Such a Diſpoſition as this, which I have here deſcrib'd, I think, will be ſufficient.



Dr. Hook's *Diſcourſe of Carriages before the* Royal Society, *on* Feb. 25. 1684-5. *with a Deſcription of* Stevin's *Sailing Chariot, made for the Prince of* Orange.

THE Occaſion of this following Diſcourſe was from the Module of a *Waggon*, ſhew'd to the *Royal Society*; upon which Dr. *Hook* diſcourſed largely on the various Ways of Conveyance. Among all which he ſaith, But that which excelled any, that has hitherto been done of that Kind, was the Sailing Chariot, made by *Simon Stevin*, for the Prince of *Orange*, which, in two Hour's Time, ran upon the Sand, on the Sea-Shoar, by the Strength of the Wind, forty two Miles, carrying in it no leſs than 28 Men, with Safety and Security: Of which I have ſeen the Deſcription, and have had the full Account. But this being only accommodated for ſuch ſmooth Ways, as the Sand on the Sea-Shoar, could not be made for common Uſe, and has therefore been laid aſide and diſus'd. However, ſince there is a Poſſibility of

ſuch

ſuch a Performance, it may, perhaps, be worthy Conſideration and further Enquiry, whether it may not be poſſible to contrive, and make ſome other Kind of Chariot, or Carriage, which may perform as much in any other paſſable Ways; which, I conceive, would be of vaſt Benefit to Mankind.

THE Principal Matter, wherein it differ'd from all other Sorts of Land-Carriage, was this, That inſtead of making Uſe of the *Strength* of *Men*, or of any Sort of *Animal*, he made Uſe only of the *Strength* of the *Wind*, and that after the ſame Manner as it was then made Uſe of, for the moving of Veſſels upon the Water; namely, by having Maſts, Sails, and other convenient Rigging, as Shrouds, Stays, Sheets, Booms, and all other Rigging, as was neceſſary for the Management of thoſe Sails. Then, for guiding this Engine, he ſo order'd his Contrivance, that he could, by turning the Axle-tree of the Wheels, make it go this Way, or that Way, at Pleaſure, with as much Eaſe and Certainty, nay, very much more than 'tis poſſible to ſteer a Ship, or any other Veſſel upon the Water. To keep it ſafe and ſecure from overturning, though on ſo ſmooth a Plain as that paſſed over, there was little Danger from the riſing of the Wheels on one Side; yet, in the firſt Attempts, it being better to over-do, in making Proviſions againſt any Thing of Danger, he placed the Wheels at a great Diſtance, or Breadth, one from another; and, as I judge by the Draught, very near half the Length of the whole Carriage; by which Means there could be no Manner of Danger in over-ſetting; and ſtill the leſs, the more the Carriage was loaden, if the Danger of over-ſetting were to be feared from the Strength of a ſide Wind upon the Sails; for the Wheels being placed at a pretty Diſtance without

the Body of the Carriage, all the Weight of the Carriage, together with the Weight of two of the Wheels, and all the Weight of the Men muſt be lifted up, and reſt upon the two Wheels on the Leeward Side, which neither Sails nor Maſts would be able to do.

THE Way of ſteering or guiding this Carriage, was much the ſame with that which is, and has always been practiſed in Carriages upon 4 Wheels; namely, an Helm, or Pole, ſo faſtened to the *Axis*. that by the Means thereof, as by a Leaver, the Axis could be ſwaſhed either this Way, or that Way, upon a Center-Pin, as is now in Uſe in Coaches and Waggons, for the turning or ſwaſhing of the Fore-Axis; only, whereas the Pole, in theſe, is turned and extended Outwards, before the Carriage, in this, it was turned Inwards. The Wheels are about a middle Size, between the uſual Size of the fore and hind Wheels of a Coach, and were made very ſtrong and ſubſtantial; and what was peculiar in them, was, that the Rims of them were 18 Inches, or 2 Foot broad, and the Spokes were made to ſtrengthen the whole Breadth; the Reaſon of which I ſuppoſe was, that they might thereby be the better able to reſt upon the ſandy Shoar, without ſinking, or making Rotes in it, which would have made it move very much heavier, the Wheels being thereby always in a riſing Motion; for the Weight of the whole Carriage, and the Weight within it (which muſt be very conſiderable, there being 28 Perſons in it) reſting only upon the four Points of the Wheels; if they had been made with narrow Rims, muſt neceſſarily have ſunk pretty deep into the Sand; but being broad, and the Sand very ſmooth, as it is generally left by the Sea, a ſmall ſinking of the touching Line of the Breadth of the Wheel, doth preſently make a very broad Footing, to reſt upon the Sand.

THERE

THERE were two of theſe Chariots made, the one a larger, of about 30 Foot long, and the other a ſmaller, about 10 or 12 Foot long: The larger had two Maſts and two Sails, proportionable to the Sails of a Boat, much about the ſame Bigneſs. The leſſer had only one Maſt and one Sail, proportioned likewiſe to its Bigneſs. Each of the Sails had two Yards, the one at the Top, and the other at the Bottom, with proper Rigging to work them. The Bottom Yard, I conceive, was put upon a double Account, Firſt, to keep the Sail more flat and plain, that it might, when the Carriage was to ſail near a Wind, be kept more ſharp and trim; the great Advantages of which I endeavour to prove upon another Occaſion. And Secondly, That the Sails might be the eaſier managed, and tacked, as Occaſions ſhould require. And though I cannot find, whether this Engine was ever tried, or made Uſe of, for Sailing by a Wind; yet, I doubt not, but that it would have far exceeded any Veſſel whatſoever, that ſails upon the Sea, in going near a Wind; becauſe, that in this, there could be no falling to *Leeward*, (which the beſt Veſſels on the Sea do more or leſs) the Wheels, in this, keeping it directly in the Line, or Plain of the Wheels.

THE greater Carriage was guided, or ſteered, by moving the hinder Wheels by a Pole, like the Helm in a Ship, and the End of it had Tackles to bend it towards this or that Side; and the Rule of Steering was the ſame as in a Ship. The leſſer Carriage was ſteered by moving or turning the Axis of the fore Wheels; the Pole or Helm being turned backward into the Carriage, and the Rule of Moving it was alſo the ſame as the former.

THE laſt Thing to be conſidered in theſe Carriages, is the great Swiftneſs of their Courſe, which was ſo conſiderable, that no Horſes, in their full Speed,

Speed, could long keep Pace with them; and Veſſels on the Sea, ſailing the ſame Way, ſeem to be carried backwards very ſwiftly. This, had it not been atteſted by Teſtimonies of undoubted Credit, would have ſeem'd very difficult to be aſſented to. But, on the other Side, if we conſider the advantageous Circumſtances for its Promotion, and ſpeeding forward, and the ſmall Impediments for the hindering theſe Carriages had, beyond any other, we ſhall find much leſs Reaſon to doubt the Hiſtory of it: For, if we compare it with Veſſels ſailing upon the Sea, we ſhall find that this Carriage has firſt a plain, hard and even Surface of the Shoar to paſs over, without any Rub or Impediment; ſo that it is moved in a Plain without riſing or falling, without any unequal Impediment, ſave only ſome ſmall Matter in the rubbing of the Ends of the Axes in the Naves of the Wheels, which, being well oiled, will be very little; whereas a Ship at Sea, when there blows a ſtiff Gale (which is abſolutely neceſſary, when much Speed is deſired) is firſt clogg'd in its Motion by the Lentor and Difficulty of yeilding in the Medium of Water; by the unequal Stoppings of the riſing Waves, which create an undulating and unſteady Motion Upwards and Downwards, as well as Side-ways; beſides the Slope falling and ſliding away to Leeward, which muſt be allow'd for in all Side-Winds, by ſteering ſome Point nearer the Wind, than the direct Way; and conſequently the Length, paſſed by the Veſſel, will be as much longer than the direct Diſtance, as the Secant of ſuch an Angle is than the Radius. On the other Side, if we compare its Motion with that of a Carriage drawn by Horſes, or other living Creatures, it plainly appears that theſe were moved by an unwearied Strength, whereas the Horſes were not long able to hold that Pace. So that

that upon the whole, it ſeems to be the ſwifteſt Carriage yet known, for ſo great a Burthen, and ſo long a Way.

But the great Objection againſt this Invention is, that it is hardly practicable in any other Place, and even there but at certain Times, which poſſibly have been the Reaſons, why it has been ſo long diſuſed, and almoſt forgotten. To which I anſwer, That ſcarce any other Invention for Carriage is practicable in all Places: Land Carriage cannot be practiſed at Sea, nor Sea Carriage by Land; Carts and Coaches cannot be uſed in ſome Places, by reaſon of the Inconvenience of the Ways, as in *Cornwall*. But this Invention, I conceive, is not to be thought confined only to the ſmooth Sands on the Sea Shoar; for I doubt not, but that if Trial were made (as I hope it will ſhortly be) it might be much more practicable upon the plain Downs of *England*, than where it was uſed, by Reaſon they are much more expoſed to the Wind, and alſo much more hard, ſo that the Wheels need not be of ſo great a Breadth. I conceive farther, that the Carriage may be improved much in its Lightneſs, and alſo in the Eaſineſs of moving. If ſuch a Chariot were made for *Salisbury Plains*, *Banſtead Downs*, *Wincheſter Downs*, *Newmarket Row*, or ſome ſuch ſmooth Plains, and the Wheels, (which need be but three) were moved upon ſmall Steel *Pevots* or *Gudgeons*, in *Bell-Metal Sockets*, well oiled, inſtead of being moved upon the large End of an *Axle-tree*. Next, if inſtead of 4 Wheels, 3 only were made uſe of, placed in the Form of a Triangle, the ſteering Wheel being that which went foremoſt, and the Place of the Maſt in the Center of the Triangle, the Weight carried, to be all placed behind the Maſt, to which I would alſo have added a Contrivance to retard and ſtop its Motion,

Motion, whenever there ſhall be Occaſion, which is eaſily to be done; ſomewhat after the ſame Manner as Windmills are ſtay'd, when there is Need. By ſuch a Contrivance, I doubt not, but a Chariot may be made to out-run even the ſwifteſt Race-Horſe, eſpecially where the Courſe is long and plain; and with a Side Wind may be carried back again to the Place from whence it ſet out; and both forward and backward may be carried with as great a Swiftneſs, even as the Wind moves, which will not be unpleaſant to ſuch as have ſuitable Conveniences near their Habitations; with which may be tried as many Experiments of ſailing near a Wind, as can be tried upon the Sea; the Contrivance of the Wheels making the Motion as eaſy, as the Water of the Sea or Rivers in others; and to a very ſwift Motion, having much leſs of Impediment, eſpecially if the Wheels be order'd to the beſt Advantage, all Manner of rubbing or ſliding being thereby taken off, and even the Inequality of the Ways themſelves may be in a great Meaſure removed. I have been the more particular in deſcribing this Carriage, becauſe it was the ſwifteſt that has poſſibly yet been made, and therefore, on this Occaſion, deſerved more than a tranſient Mention, tho' I do not look upon it as an Invention of the higheſt Perfection, for this Effect; but may be as much exceeded, as that exceeded a Man that leiſurely walks. Who it was that firſt invented the Wheel, is not recorded in Hiſtory, it having been long before any Hiſtory extant (except that of the Bible) and the firſt Mention we find of it there, is *Pharaoh's Chariot*, in which *Joſeph* was exalted to ride: Of whoſe Form we know nothing but the Name, tho' it had, in Probability, been known long before that Time; which, notwithſtanding, long preceded any Heathen Writings now extant.

1 *Hyginus*

Hyginus relates, in his 2d Book, where he treats *De Ophiucho*, that *Ceres* invented an One-Wheel'd Chariot, which *Triptolemus* (whoſe Nurſe ſhe was) firſt made uſe of, for to make Speed, to inform the World of her Bounty. *Ceres cum ſua beneficia largiretur hominibus, Triptolemum cujus ipſa fuerat nutrix (qui primus hominum una rota dicitur uſus ne curſum moraretur) juſſit omnium nationum agros circumeuntem ſemine partiri. In Gloſſis Iſiodori, Vehiculum unius rotæ*, is called *Pabo*. But how this One-Wheel'd Chariot was contrived, or uſed, is not to be found in Hiſtory; Mention there is, of other Chariots, with more Wheels, in the ancient Authors; ſo that 'tis clear, it was known and practiſed long before any Hiſtories of Heathen Writers were publiſh'd. An Invention of ſo great Uſe, that it ſeems impoſſible ever to be loſt by Mankind, after it be once known: Which Conſideration makes me very much wonder whence thoſe Men came, that inhabited *America*, before the *Spaniards* over-running and conquering of it; ſince it ſeems probable, that if they, or their Anceſtors, had ſprung from any People here, on this Side of the World, *viz.* from *Europe*, *Aſia* or *Africa*, they muſt needs have carried along with them the uſeful Invention of the Wheel; but it has been obſerved, that they knew nothing at all concerning it, nor any the leaſt Uſe of it, throughout all *America*, before the *Europeans* came among them. So that we muſt conclude, either that they were made Inhabitants before the Invention of the Wheels was found, or that they never had any Origination from any Generation of Men in thoſe Parts of the World, at leaſt not from the *Tartars*, who, of all People, do moſt frequently uſe them; but this by the by. The firſt, and moſt ſimple of *Carriages* by Land, was this Invention with one Wheel, and may poſſibly be moſt accommo-

accommodate, for attaining the End we are now inquiring after, which is Swiftneſs, it having the leaſt Impediment to its Motion, and the leaſt Incumbrance of any other; and may therefore, in the next Place, deſerve to be conſidered, and poſſibly be brought into common Uſe, at leaſt to be experimented, as was that of the Sailing Chariot.

But before I come to the more particular Deſcription thereof, I think it will not be impertinent to examine the Contrivance of the Wheel, as it is applied to Carriages, for the facilitating of their Motion. One of the greateſt Obſtructions to Swiftneſs of Motion being the Inequality of the Ways, and the rubbing or grating of thoſe Ways againſt thoſe Bodies or Weights, that are drawn or ſlid upon them.

The Wheel being then a round Body, and moving forwards, only by its Rollings, doth not at all rub, grate, or ſlide upon the Way; and ſo hath no Impediment at all to its Motion forward, where the Way is even, plain and horizontal, or level, there being no Impediment, or very little, from the Medium of the Air it paſſes through, and ſo hath no Impediment to be moved with the ſwifteſt Motion, like that of the Reſiſtence of Water to Veſſels moving through it: So that the only Impediment ſeems to be that of its own Bulk, (of which I ſhall ſpeak hereafter) becauſe the outward Rim of the Wheel, in its rolling Motion, doth uniformly apply its Parts to the Parts of the Plain, by deſcending down, and riſing up from them perpendicularly; and the touching Part is always quieſcent upon the Plain, and moves not either forward or backward; and conſequently all Impediment from rubbing upon the Ground or Way is wholly taken off, as 'twill be evident to any one who ſhall examine the Motion of any one Point of the Verge of the Wheel; for he will find

find that every Point of this Verge doth, by the compounding the circular and progreſſive Motions together, move itſelf in a true *Cycloidal* Line, and that, in the Point of touching it, reſteth or ſtandeth ſtill in the Boundary between two ſuch Lines. So that where the Plain and the Wheel is perfectly hard and ſmooth, the Wheel receives no Impediment to its compounded Motion; but it may be thought that the circular Motion of the Wheel is an Impediment to the progreſſive Motion, becauſe by Means of this Compoſition, the Parts of the Wheel do, in ſeveral Poſitions therein, receive ſeveral Degrees of progreſſive Motion, and ſo ſeem to go, as it were, by Starts, for that the Points, whereby they touch, have no progreſſive Motion at all; and when they are at the Top, or at the greateſt Diſtance from the Plain, they have a double Velocity forwards, compared to that of the Center, and, in every intermediate Poſition, a differing Degree of Velocity forward. But this is no Impediment at all to the progreſſive Motion of the Whole, each Motion being ſeverally uniform, equal, and continued. For a *Pendulum*, whoſe Weight at the End is a Globe of Lead, or any other ponderous Body, ſuſpended by a String, receives the ſame *impetus* from the Power of Gravity, (which is the ſame in both Caſes) whether this Globe, ſo ſuſpended, be ſuffer'd to vibrate, whilſt it be ſwiftly whirled round upon its Center, or whether it be not ſo whirled at all, the compounding of Motions not at all intermeddling with one another; but every one keeping its diſtinct *impetus*, as may be eaſily found by Experiment, if Trial be made in the Way I propoſe. Whence I conceive alſo, that the periodical Motion of the Earth, or any other Planet about the Sun, would be the ſame, whether the Body of any of them were gyrated round their own Centers, or not, and

and whether the *Axis* of that Gyration were at right Angles with the Plain, in which they are mov'd or not, the Motion or Influence of the one not at all interfering, or diſturbing that of the other. But this only by the by. However, I think it may be pertinent to be conſider'd in the Examination of an *Hypotheſis* of *Gravity*, propounded by the learned Dr. *Voſſius*, in his lately publiſh'd *Miſcellaneous Treatiſe*, wherein he lays great Streſs upon the Poſition of the *Axis*, in reſpect of the Plain of its circular, or direct Motion.

NEXT, we are to conſider, what Impediment to its Motion, a Wheel, thus roll'd upon a Floor, receives from that Floor. There may be two impediments then, that a Wheel, ſo roll'd, may receive from a Floor according to the Nature thereof. The firſt and chiefeſt, is the yielding, or opening of that Floor, by the Weight of the Wheel ſo rolling and preſſing; and the ſecond, is the ſticking and adhering of the Parts of it to the Wheel; to which two may be referr'd all others, all of which proceed from the yielding or giving Way of the Parts of the Floor, and the not returning again to their bended Poſture; for, if the Floor be perfectly hard (as alſo the Parts of the Wheel) tho' it be very unequal, yet is there little or no Loſs, or conſiderable Impediment to be accounted for; for whatever Force is loſt, in raiſing or making a Wheel paſs over a Rub, is gain'd again by the Wheel's deſcending from that Rub, in the ſame Nature as a Ship on the Sea is promoted by the deſcending down of a Wave, as much as impeded by its aſcending, or a *Pendulum* is promoted by its Deſcent, as much as impeded by its Aſcent.

NOR is the yielding of the Floor any Impediment, if it returns and riſes againſt the Wheel, for the ſame Reaſon; but the yielding, or ſinking of the Floor, and its not returning again, is the great

great Impediment from the Floor; for ſo much of Motion is loſt thereby, as there is Force requiſite to ſink ſuch a Rut into the ſaid Floor by any other Means; whether by Weight, Preſſure or thruſting directly down, or any Ways obliquely.

And it may alſo be calculated, by drawing on the Wheel, whoſe Weight, at the mean Time, ſinks the Floor it rolls over. Either Way it will be eaſy to bring it under Calculation, which is the Deſign of this Diſcourſe.

The Second Impediment it receives from a Floor, or Way, is the ſticking and adhering of the Parts of the Way to it; for by that Means, there is a new Force requiſite to pull it off, or raiſe the hinder Part of the Wheel from the Floor, or Way, to which it ſticks, which is moſt conſiderable in moiſt clayie Ways, and in a broad rimm'd Wheel. For in ſuch Ways, the Wheel doth not only loſe a Part of its Motion, by the yielding and preſſing of the Clay againſt the fore Parts of the Wheel, but by the cleaving to, and holding of it to the hinder Parts, which makes all Carriages move very ſluggiſhly and heavily in ſuch Ways.

Thus much I thought neceſſary to conſider, as to the Goodneſs or Badneſs of the Floor, or Ways over which Carriages are to paſs, whereof, in the general, this may be affirm'd, that the harder the Ways are, the leſs Impediment they give to the Motion of Carriages over them; and the more even they are, the more equal is the Motion.

Hitherto I have conſider'd the Wheel only as free, and, of itſelf, burthen'd only by its own Weight. I ſhall next conſider it as burthen'd by another Weight. There are two Ways then of burthening a Wheel. The firſt is, by laying the Weight at the Top of it; the ſecond is, by laying it upon the Center, or Axis of it.

The firſt Way was poſſibly the firſt invented, being of great Uſe for tranſporting of very great Weights ſome ſhort Way, and is generally practiſed for removing of Obelisks, Columns, great Stones, or Great Beams of Timber; and, for that Uſe, the Rollers, or Wheels, are generally ſolid Pieces of hard Timber, cut or turn'd round; and are very long or broad, call'd Rollers; this, of all Ways, is the eaſieſt for removing ſuch Weights; but then they muſt be continually chang'd by being remov'd from behind the Weight, and plac'd before; for as they roll forwards upon the Floor, ſo they roll backwards under the Weight, or rather promote the ſame with a double Velocity to that of their own upon the Floor. By the Way, it ſeems very ſtrange, that the *Weſt-Indians*, tho' in their Buildings they made uſe of ſuch vaſt Stones, and dragged them on the Ground for ſo great a Diſtance, yet that they ſhould not underſtand the Uſe of theſe Wheels, or Rollers, which, Hiſtories ſay, they did not, they performing thoſe Tranſportations only, by the main Strength of Men pulling at the Ends of a great Number of Ropes. By this Way, a vaſt Weight may be moved by a very ſmall Strength, if all Things be hard and ſmooth, approaching much to the moving of a Bulk upon the Water; but this being more proper to be inlarged upon under the Head of Strength, and not ſo adapted for Speed, I ſhall leave at preſent, till I ſpeak of that Part.

The ſecond Way then of burthening Wheels, is, by reſting ſuch Weight upon the Axis, or Center of them; This may be, and has been practiſed alſo two Ways; that is, either firſt, by making the Wheel move round upon the Axis fixed to the Carriage; or, ſecondly, by fixing the Axis to the Wheel, and making the Axis to turn round in a Socket of the Carriage; the firſt of theſe

theſe Ways is new, and has always been the Way of uſing Wheels for Chariots, Carts, Waggons, and ſuch other Kinds of Carriages; the ſecond, is uſed in Wheel-Barrows, and ſuch other Carriages and Uſes, where the Wheel runs within the Frame. Of theſe two Ways, the laſt (where it can be applied) is much the beſt; for that the Axis can be much better fixed in the Wheel, ſo as to make it run true in a Plain; and next, for that the Axis may be kept more firm and ſteady to that Motion, by having the two Ends of the Axis, by Means of its Gudgeons, kept in the Sockets fitted for it; and thirdly, becauſe the Gudgeons, halving the Weight, may be made very much ſmaller, and ſo will not cauſe a tenth Part of the Friction which is neceſſary in the other Way. This ſecond Way, therefore, is much better accommodated for Speed than the former, and may alſo be well enough contriv'd, to be made applicable to ſeveral Sorts of Carriages fit for that Purpoſe, of which I ſhall hereafter ſpeak.

THE next Thing to be conſider'd, is the Make of the Wheel itſelf; which has been ſeveral Ways contriv'd, and made uſe of in differing Ages of the World, and for differing Occaſions. The firſt and moſt ſimple, was that which was made of a round Piece of Timber for Rollers, as I noted before, in which there ſeem'd to be little of Art, but only ſawing it off with a Saw; theſe were of the ſmalleſt Sort, and are ſtill uſed for Truckles and ſmaller Carriages.

THE Second, was that of a ſomewhat bigger Sort, and that was either cut out of a whole Plank, where it cou'd be procur'd broad enough, or elſe was made of two or more Planks join'd together, and faſten'd by two or more, croſs Ledges, and that was call'd *Tympanum*, and the ſame is ſtill uſed for the Carriages of Guns at Sea. The

third Way, was of bending a Piece of pliable Timber, as we now do for Hoops, and thereby making the Rim of the Wheel all of one Piece, and fixing the Spokes to it, which were alſo fix'd into a Nave in the Middle; which Nave was alſo turn'd and bor'd, as the Naves, we now uſe, are.

THE laſt, and moſt practicable of all, was that we now uſe, whereof the Rim was made with ſeveral Fellows join'd and yok'd together with Pins, and ſometimes with Joints, and ſtrengthen'd alſo by the Sides with Irons, and, after all, bound round with Iron Streaks and Nails. This Way is uſed for all Sort of Carriages, whether heavier or lighter; and Wheels, thus made, are differenced only by being made either bigger or leſs in Compaſs, or ſtronger and weaker in Subſtance or Bulk; whence they become alſo thicker or thinner, in Breadth or Thickneſs, and alſo heavier or lighter, according to the various Deſigns and Uſes they are apply'd unto; the Circumſtances and Accidents, that concomitate their deſign'd Uſe, beſt directing the Artiſt in the Contrivance of their Form and Make.

I SHALL not now inſiſt upon explaining, which Sort is moſt proper for every of theſe Deſigns, becauſe I ſhall do that under each proper Head; but ſhall only conſider at preſent, which Kind of theſe are beſt for Speed and Celerity, that being the Head I am now explaining.

FOR making of Speed then, thoſe Sorts of Wheels are beſt which are the biggeſt in Circumference or Diameter, becauſe firſt, a much greater Part of the Rim doth bear at once, than in a Wheel of a leſs Circumference; for the Way being always more or leſs yielding, the bigger Wheel ſinks in ſo much leſs to come to its bearing, than the leſſer Wheel, by how much the greater Circle approaches nearer to a ſtreight Line, or the Tangent

gent of the Floor. Secondly, Becauſe the greater the Arch, the more eaſy is the Riſe of the Wheel over any Irregularity, or Rub in the Way, and the eaſier the Fall, and thereby approaches nearer to the evening and plaining of the Way, and makes leſs Inequality in the Draught. On the Contrary, the ſmaller the Wheel, the worſe, for that it introduces all the contrary Inconveniences. Thirdly, The larger the Wheel is in Circumference, the leſs is the Impediment of the rubbing and wearing.

For *Firſt*, the Leaver of the Spoke is ſo much the longer, and ſo the Nave will turn ſo much the eaſier upon the End of the Axle; the Weight born, in both Caſes, being the ſame, and conſequently the Bigneſs, both of the one and the other, needing not to be differing.

Secondly, The lighter the Wheel be (provided it be made ſtrong enough to perform the Buſineſs it is deſign'd for) the better it is; and therefore all Manner of Contrivance that tends to the making the Wheel ſtrong, and yet large and light, is to be made uſe of, for that thereby a leſs Weight is neceſſary to be moved, and conſequently the ſame Strength will have the greater Effect.

Thirdly, The leſs rubbing there be of the Axle, the better it is for this Effect; upon which Account, Steel Axes, and Bell-Metal Sockets, are much better than Wood, clamped, or ſhod with Iron; and Gudgeons of hardened Steel, running in Bell-Metal Sockets, yet much better, if there be Proviſion made to keep out Duſt and Dirt, and conſtantly to ſupply and feed them with Oil, to keep them from eating one another; but the beſt Way of all is, to make the Gudgeons run on large Truckles, which wholly prevents gnawing, rubbing, and fretting.

THESE are ſome of the good Qualifications of Wheels, prepared and adapted for the Deſign of Speed, which I am now diſcourſing of: There are ſome other Qualifications that yet exceed theſe, of which I ſhall treat ſome other Time, where I ſhall have Occaſion to apply them.

HAVING thus far conſider'd of the Properties and Qualifications of Wheels, fit for ſuch Carriage, I ſhall next conſider what Kind of Carriage is beſt for this Purpoſe, and what Number of Wheels are fitteſt to be applied.

Firſt, FOR the Properties of the Carriage. That which is of the ſmalleſt Bulk, and of the lighteſt Weight, and of the ſimpleſt, plaineſt, and yet ſtrongeſt and moſt durable Structure, is the beſt; provided ſtill, that, in every Particular, it be ſufficient for performing what is required of it. That Carriage, which is only deſign'd for carrying a ſingle Man, ſhould not be made either large enough, or ſtrong enough, or heavy enough, to carry two; that, which can be born by one or two Wheels, ſhould not be loaden, or clogged, with two, three, or four. So that upon the whole Matter of the Inſtrument, fit for Conveyance of one ſingle Perſon, I ſee none can be better than a certain Carriage or Chariot, and for the convenient Reception of one Man, and reſting or moving upon one ſingle Wheel. I do not find this to be in Practice any where, but in *China*, of which there is a ſhort Account in *Martinius* his *Atlas Sinicus*. But this is not ſo well adapted for Swiftneſs, being moved by the Strength of Men, and, for the moſt Part, by one, and ſo is only a Chair, or Sedan, with one Man and a Wheel, inſtead of a ſecond Man; but might be contrived much better, both for Eaſe and Speed, if there were two Men made uſe of with one ſingle Wheel, which I ſhall elſewhere deſcribe;

but ſtill it will come ſhort, as to Speed, in Compariſon to one, wherein the Strength of Horſes, or ſome ſuch ſwift and powerful Mover, is applied for its Acceleration.

THE next Thing then to be conſidered, in an Engine for Speed, is the Application of Strength for the moving thereof, which is the Life of the whole; and without which, all the reſt is motionleſs. This I ſhall diſcourſe of the next Time.

[*I do not find any Account, among Dr.* Hook's *Papers, of the Matters here promiſed.*]

WILLIAM DERHAM.

The Number of Houſes paying Chimney-Money in every County of England *and* Wales, *in the Year* 1685.

Bedfordſhire	12170	*Nottingham*	17554
Berks	16906	*Oxford*	19007
Bucks	18390	*Rutland*	3263
Cambridge	17347	*Salop*	23284
Cheſhire	24054	*Somerſet*	44686
Cornwall	25374	*Suffolk*	34422
Cumberland	14825	*Surrey*	14273
Derbyſhire	21155	*Suſſex*	21537
Devonſhire	56310	*Stafford*	23747
Dorſetſhire	21944	*Warwick*	21973
Durham	15984	*Wilts*	27093
Eſſex	34819	*Worceſter*	20634
Glouceſterſhire	26764	*Weſtmorland*	6501
Hampſhire	26851	*York*	106151
Hertfordſhire	16569		
Herefordſhire	15006		986765
Huntington	8217		
Kent	29242	*Wales*	42565
Lancaſhire	40202	*London*	30997
Leiceſter	18702	*Middleſex*	54287
Lincoln	40590	*Weſtminſter*	14852
Monmouth	6490	*Southwark*	19945
Northampton	24808	*Briſtol*	5122
Norfolk	47180		
Northumberland	22741	Total	1154533

Experiments

Experiments and Obſervations for the Improvement of the Barometer, *by Dr.* Hook, *read before the* Royal Society, Feb. 3, 1685-6.

THE Experiments I have now ſhewn, are no Ways pompous and ſurpriſing. Such poſſibly may better ſuit a Stage or Theatre, for vulgar Spectators to admire and gaze at, who are moſt taken with Shew. But theſe are plain and obvious, and only valuable, as they diſcover ſome Truth, that may be either uſeful of itſelf to be known, or has a Tendency to the making ſome farther Diſcovery, or of being uſeful, as preparatory to ſome other Experiment or Invention, which may be made or founded thereupon. And indeed the greateſt Part of Experiments, if they be not made for ſome ſuch Deſign; and the material Circumſtances, uſeful thereunto, diligently enquired after, and ſtrictly obſerv'd, and brought to a Calculation for that Purpoſe, do ſerve for little elſe than to hint an Experiment to ſome other to try, who may have ſome Uſe or Application for it.

THE Experiments, as they have been made, do exhibit the ſpecifick Weight of the fluid Bodies; together with their comparative Weight with Water: That theſe three Fluids are in ſpecifick Gravity to one another, as follows.

Water, 5997.
So Water to *Mercury*, as 1 to 15.

Spirit of Wine, 5102.
Oil of *Turpentine* to *Mercury*, as 1 to 17¼.

Oil of *Turpentine*, 5209.
Spirit of Wine to *Mercury*, as 1 to 17.

FURTHER

FURTHER Obſervables are,

Firſt, THE great Lightneſs of Spirit of Wine, and Oil of *Turpentine*, they being, Spirit of Wine but as 51. Oil of *Turpentine*, 52, whereas common Water is 60; that is, almoſt a ſixth Part lighter than Water.

Secondly, THE Nearneſs of their ſpecifick Gravity to one another, which may be yet made as much nearer, as ſhall be requiſite, or deſired, by the intermingling Water, or Flegm, with the Spirit of Wine; for the Spirit of Wine being lighter, and the Oil of Turpentine heavier, ſome Mixture of Water, with the Spirit of Wine, will bring the Spirit of Wine to be as near of the ſame Weight, with the Oil of Turpentine, as ſhall be required.

Thirdly, THE differing Nature of theſe ſo ſeemingly ſimilar Liquors.

Firſt, IN that they will not mix with each other, but will float the one upon the other.

Secondly, IN that they will not eaſily receive the ſame Tincture, but differing; the Spirit of Wine readily imbibing a Red, from Cocheneel, which that, and the Spirit of Turpentine, a Green.

THE Uſe, or Application of theſe Experiments, is in Order to the Solution of this following mechanical *Problem*.

HOW to make a *Barometer*, or Inſtrument, to try and find the Weight of the Air, at all Times, which ſhall riſe and fall ſteadily, and without jumping or ſtarting, otherwiſe than as influenced by the Air, and the hitherto unknown Alterations thereof; whoſe Limits, between the greateſt and the leaſt Height, ſhall be 10, 20, 30, 40, 50, or more Feet in Perpendicular; and the Motion, in every Inch

Inch of the ſaid Height, as plainly viſible, as the Riſing and Falling of an Inch in the common ſingle *Barometer*.

It is about 7 or 8 Years ſince I propounded ſuch a *Barometer* to this Society; and I cannot expect that many ſuch will be made; however, poſſibly it might not be amiſs, that this Society, or ſome curious obſerving Perſon, would make one, and diligently remark the Changes and Motions thereof. For it might poſſibly diſcover ſuch Changes and Motions of the Air, as we have hitherto no Notion or Conjecture of; for I did once obſerve, that the Wheel-*Barometer*, a little before a great Storm of Thunder, Lightening and Rain, did appear to have a tremulous Motion, as if the Room, or Poſt it hung upon, had ſhook, when yet the Clouds were but gathering, and were far enough off from this Place, where I obſerved it; of which I have, long ſince, acquainted this Society, and, I conceive, it may be found in the Journal. But there are many other Changes in the Air, that none of the Inſtruments, we yet have, will detect; and therefore there may be Scope enough for Inventions, of other Kinds, to detect them, which may give a farther Light to the Diſcovery of that moſt ſignificant, and moſt uſeful, Body of the Air. And tho' poſſibly the Invention of a mechanical Inſtrument may be looked upon as a trivial Thing, yet, as it may be contrived and applied, it may furniſh us with a new Senſe, by which we may be able to know ſome Properties of Bodies, of which we have now no more Notion, than one born blind has of Colours, or one deaf of muſical Sounds; or than the whole World hath ever had, of the differing Gravitation of the Air, before the *Barometer* was invented and obſerved.

The

THE Reaſon of my contriving this Inſtrument, was, that I might ſhew a Way how the Examination, or weighing of the Air's Preſſure, might be carried to the Extreams, or as far as could well be deſired; for ſo it may be, by this Method, if any one will be at the Charge of making it.

AND indeed if we conſider, and a little more ſtrictly examine into the Nature of Things, we ſhall find, that moſt of the Operations of Nature are out of the Reach of our Senſes, and cannot be plainly, if at all, diſcover'd by them, and we are left to gueſs at the Conſultations and Deſigns of the Privy Council of Nature, only by the publick Acts and Effects that are produced thereby; whereas, if we could by Senſe be informed of the Agents, and of the Method or Way of acting, uſed by thoſe Agents, we ſhould be much better able to give a right Judgment of the Effects.

NOW there is no Method of Information ſo certain and infallible, as that of Senſe, if rightly and judiciouſly made uſe of. And though the Senſes themſelves are limited in their Power and Extent, when conſidered barely in themſelves, as naturally conſtituted, yet their Power may be much enlarged, and their Limits much farther extended, by the Helps that Art may afford, and, moſt eſpecially, by Mechanicks; by Means of which, not only each of them may be made more Powerful in the Diſcovery of the proper Objects of thoſe ſeveral Senſes; but each of them may be made a *Genus*, as it were, of new Sorts of Senſe, compriſed under them, of which we have yet no Notion, nor any Senſe or Method of Diſcovery; at leaſt they are yet unheeded. I might inſtance, in the Body of the Air itſelf, but I ſhall reſerve it to another Opportunity.

In

In Air, 133, ½3.
In Water, ½3. *gr.* 83.
In Spirit of Wine, 2½3, 28 *gr.*
In Spirit of *Turpentine* 23, 23, 41 *gr.*

Air	105½.			
Water	5½,	3 *gr.* ——	1003	— 3 *gr.*
Spirit of Wine	20,	28 *gr.* ——	853	+ 2 *gr.*
Ole. Tereb.	18½	11 *gr.* ——	863	+ 4 *gr.*

WHEREFORE I find that Spirit of Wine may eaſily be made to be 16 Times lighter than Mercury; if then the Spirit of Wine be made of this ſpecifick Weight, by intermingling Water with it, and the Height of the Pipes, or the Cylinder of Spirit of Wine be deſigned to play 32 Foot perpendicular; then muſt the mercurial be 2 Foot more in Height, than the common *Barometer*; which I have found ſometimes (as particularly on *Wedneſday* laſt) to be 30,6; and conſequently the mercurial Cylinder to counterpoiſe the Gravity of the Air, and the Gravity of a Cylinder of 32 Foot in Height of Spirit of Wine, of ſuch a Rectification as I have ſpecified. Now, the Cylinder of the Spirit of Wine being always the ſame, that is, 32 Foot, the Counterpoiſe to it of Mercury will be always the ſame 2 Foot; and the Cylinder of the Air only altering the Cylinder of the Mercury alſo, that counterbalances that alſo, will only be alter'd, and that the ſame, as in the common *Barometer*. Now if the Oil of Turpentine be $\frac{1}{16}$ Part lighter than that, then a Cylinder of Mercury $\frac{1}{16}$ ſhorter than two Foot, will counterpoiſe it; which is but one Quarter of an Inch Difference in the counterpoiſing Cylinders.

Although

ALthough I find, by the Minutes of the Royal Society, *that the learned Dr.* Slare *had, long before the Year* 1677, *ſhewed a* Phoſphorus; *yet it being chiefly about this Time, that moſt of the Accounts of the* Phoſphori *were ſent, I therefore chuſe to inſert here ſuch Preparations as I have of them. And firſt of the*

W. DERHAM.

Bolognian Phoſphorus.

THIS Stone is found in three Places near the City of *Bologna*; the firſt is called *Pradalbino*; the ſecond is a ſmall Brook near the Village *Roncaria*; the third is call'd *Monte Paterno*, and is moſt noted for theſe Stones; not only as having the greateſt Quantity, but a Sort moſt eaſy to be prepared. The Ground thereabouts is barren, yielding Pieces of yellow *Marcaſite* of the Bigneſs of a Nut.

THE propereſt Time to gather it, is after Rain, when the Surface of the Ground is a little waſh'd away. It's known by a Glittering (like that of burniſh'd Silver) which ſurprizes the Eye.

IT was firſt found out by one of that City, call'd *Vincenzo Caſciarolo*, a Cobler, but ingenious, and a Lover of Chymiſtry; who, trying ſeveral Experiments with theſe Stones, by Chance happened on this Way of preparing them, ſo as to make them ſhine in the Dark, after they had been ſome Time expoſed to the Sun.

IT has no certain Figure, ſome being cylindrical, others round or lenticular; and theſe laſt are often the beſt, as being moſt ſhining and tranſparent.

IT's

It's uſually no bigger than an Orange; and tho' *Licetus* affirms, there never was any greater than that in *Androvandus*'s *Muſæum*, weighing about two Pound and half; yet the Author hath had of five Pound.

It's very heavy, conſidering the Bulk, as being probably compounded of ſeveral mineral Subſtances.

The Colour is various, as Aſh, Ruſty, Sky, Yellow, Earthy and White; but the beſt for Uſe are Sky-colour and White.

When it's well prepared, it leaves a Luſtre in the Superficies, and is enlightened, not only by the Sun, but the Moon, and a Fire; but by theſe not ſo ſtrongly, as the Sun.

The Light, tho' it appear like a Coal, yet is not ſufficient to read with, unleſs applied cloſe to the Word.

It will not retain the Light very long, at one Time, nor its Vertue above five or ſix Years.

The Preparation is thus: Take a Cylinder, whoſe Circumference is about two *Roman* Architect Palms, and $\frac{4}{12}$ (of our Meaſure, almoſt two Feet) the Height about $\frac{9}{12}$; ſpread the Inſide of the Cylinder with ſtiff Clay, till the Diameter of the Aperture come to be but $\frac{8}{12}$; on the Top of the Cylinder make four equidiſtant Notches, about $\frac{1}{12}$ deep, and $\frac{2}{12}$ broad: This being done, take another Cylinder of equal Dimenſions with the former, or ſomething taller; at the Bottom, make two Port-holes, oppoſite to one another, and capable to receive a Hand; make a Bottom of the ſame Clay, which may reflect the Heat. This Veſſel being cover'd with a thick Wire Grate, that the Air may eaſily paſs through, and the other Part of the Furnace placed upon it; lay upon the Grate ſome lighted Charcoal, and then other not lighted, but well charr'd, and free from Earth, Stones,

Stones, and other ſulphurous Matters, breaking the Coal into Pieces no bigger than a Nut; when you have made your Bed, as high as the Notches, put upon it your Stones, to be calcined, ſo cloſe, that they muſt touch; but firſt beat ſome of the Stones to Powder, and ſearce it in a fine Hair Searce, that it may come out very fine; when you have wet your Stones, that are to be calcined, in good ſtrong *Aqua Vitæ*, roll them in that Powder, and lay them, as before, on the Charcoal, and make another Bed of Charcoal over them, to the Top of the Furnace, which you cover with a round cloſe Head. When the Coals are ſpent, and the Stones cool, take the Cruſt away from them, and wrap them in Silk, putting them in a cloſe Box, till you make uſe of them.

If you would make Figures and Repreſentations with this Light, as is often done, take the Cruſt, which comes off the Stones, and beat it ſmall, ſearcing it as before; then when you have made your Figure, or Image, wet it with the White of an Egg, and ſprinkle upon it your fine Powder, which will ſhine like the Stone.

This Sort of Furnace is not abſolutely neceſſary, but convenient, as well in determining the Time, as the Degree of Heat; which, if more, might diffuſe that Luſtre which is in the Superficies of the Stones; if leſs, not raiſe it.

The Author, occaſionally ſpeaking of ſhining Woods, delivers this Rule, for the ſure finding of them. That an Apple-Tree is the beſt Wood; that it muſt be very dry, or rotten; that being ſo, and lying under Ground, that Part under Ground will partake of a ſhining Quality, which will not laſt above three Days, nor be recover'd again, when loſt.

Phoſphorus Liquidus.

SUme ſalem alcali *v. g.* cinerum clavellatorum bene purificatum per diverſas ſolutiones & filtrationes, & ab omnibus impuritatibus in unum ; deinde in crucibulo novo ad ſalem albiſſimum calcinetur, tum in mortario polito & calido in minutiſſimas partes teratur ; deinde indatur retortæ vitreæ cum ſpiritu urinæ rectificatiſſimo imbibitus, cui applicetur recipiens bene agglutinatus ; tum ignis per gradus admoveatur : hic operatione factâ debet pluries cohobari, addito ſemper novo ſpiritu urinæ in unaquaque cohobatione, atque ſic tandem ſal alcali cum ſpiritu urinæ tranſit in recipiens in forma butyri antimonii.

Nullius eſt ſaporis, lucet tamen ſcintillatione continua inſtar luminis ſtellaris, & eſt ultra modum volatile ac fortis odoris, quaſi ſulphuris accenſi ; ideo conſervari debet in vaſe vitreo clauſo, infuſa aqua communi deſuper, atque tum radios emittit per aquam, & fulgura, quæ totum occupant vitrum quando agitatur ; ſi enim ſit extra aquam in aere libero, evaneſcit, tantæ extenſionis eſt capax ut lentis magnitudine ſufficiat ad illinendum totum corpus, quod luminoſum apparebit, quaſi igne & flammis circumdatum, abſque minima eroſione ; nihil aliud accendere poteſt quantum hucuſque ſcitur niſi pulverem pyrium.

Phoſphoros Metallorum.

TAKE *Lapis Smaragdi Mineralis* (ſuch as is found in the Mines of *Saxony*) ; beat it into a very fine Powder.

IF you ſtrew this, very fine, on a Plate, of any Metal, and in any Figure, and ſet the Plate on hot Coals; in a ſhort Time you will perceive, in the Dark, a Light to ſhine; which will (ſaith my Author) laſt as long as you continue the hot Coals: And if you beat out the Fire, it may do again, for once or twice; but then the Vertue will fade.

Phoſphoros Elementaris, *by Dr.* Brandt *of* Hamburgh.

TAKE a Quantity of Urine (not leſs for one Experiment than 50 or 60 Pails full); let it lie ſteeping in one or more Tubs, or an Hogſhead of oaken Wood, till it putrify and breed Worms, as it will do in 14 or 15 Days. Then, in a large Kettle, ſet ſome of it to boil on a ſtrong Fire, and, as it conſumes and evaporates, pour in more, and ſo on, till, at laſt, the whole Quantity be reduced to a Paſte, or rather a hard Coal, or Cruſt, which it will reſemble; and this may be done in two or three Days, if the Fire be well tended, but elſe it may be doing a Fortnight or more. Then take the ſaid Paſte, or Coal; powder it, and add thereto ſome fair Water, about 15 Fingers high, or four Times as high as the Powder, and boil them together for ¼ of an Hour. Then ſtrain the Liquor and all through a woollen Cloth; that which ſticks behind, may be thrown away, but the Liquor that paſſes, muſt be taken and boil'd till it come to a Salt, which it will be in a few Hours. Then take off the *Caput Mortuum* (which you have at any Apothecary's, being the Remainder of *Aqua Fortis* from Vitriol and Salt of Niter) and add a Pound thereof to half a Pound of the ſaid Salt, both

both of them being firſt finely pulverized. And then for 24 Hours ſteep'd in the moſt rectify'd Spirit of Wine, two or three Fingers high, ſo as it will become a Kind of Pap.

THEN evaporate all in warm Sand, and there will remain a red, or reddiſh, Salt. Take this Salt, put it into a Retort, and, for the firſt Hour, begin with a ſmall Fire; more the next, a greater the 3d, and more the 4th; and then continue it, as high as you can, for 24 Hours. Sometimes, by the Force of the Fire, 12 Hours proves enough; for when you ſee the Recipient white, and ſhining with the Fire, and that there are no more Flaſhes, or, as it were, Blaſts of Wind, coming from Time to Time from the Retort, then the Work is finiſhed. And you may, with a Feather, gather the Fire together, or ſcrape it off with a Knife, where it ſticks.

THE Fire is beſt preſerved in a Veſſel of Lead, cloſed up from the Air: But to be ſeen, 'tis alſo put into a Glaſs, in Water, where it will ſhine in the Dark, but muſt be cloſe ſtopp'd. Some of this Fire, placed in the Beams of the Sun, will kindle Gun-powder: I ſaw ſome of it, preſs'd with a Quill that was cut, and it fired Gun-powder about it. Mr. *Concle* writ alſo with it on Paper, and the Letters all ſhined in the Dark, and when they decayed, the rubbing the Paper, with the Fingers, revived it again, and this after two Days.

MY Author ſays, he had once wrapp'd up a Knob in Wax, at *Hanover*, and it being in his Pocket, and he buſy near the Fire, the very Heat ſet it in Flame, and burn'd all his Cloaths, and his Fingers alſo; for though he rubbed them in the Dirt, nothing would quench it, unleſs he had had Water; he was ill for 15 Days, and the Skin came off. You may write herewith on Paper, a Wall, or any Wood, *&c.*

N. B. THAT to make this Fire join in Knobs, you muſt, after gathering it from the Recipient, put it into a Glaſs (like a Urinal) and putting it *in Balneo*, or warm Sand, there will evaporate ſome Humidity that lies within it, and thereupon it will ſtick the better together.

N. B. THE Retort muſt be very well luted, to reſiſt the continued Heat: Take therefore, to 50 Pound of fat Clay, as much white Tartar, as much fine Sand, waſh'd and dry'd, and 1 Pound of Cow's Hair; all theſe, mix'd and beat together, will cloſe it Hermetically.

N. B. THAT, when the Operation is done, you muſt take off the Retort, and ſtop it with ſome of the ſame Clay, well warmed, immediately, that the Air enter not; for in Caſe you ſhould leave all to cool, with the Retort on, the Fire, deſired, would retire thereinto.

N. B. THAT ſome do give a little Vent to the Retort, or Recipient, becauſe of the violent Heat in the Operation, but he never does it.

Phoſphoros Baldwini.

RE*c.* Spiritus nitri optimi, qui quodammodo ad flavedinem inclinat. q. pl. hunc mitiga cum dimidia parte *Aq. Fortis*; poſtmodum ſolve in hoc cretam optimam albiſſimam & ſicciſſimam, & quidem tantum quantum hic liquor admittit: unde tandem acquirit odorem ſuavem, fere inſtar olei amygdalarum. Hoc ſolutum filtra, filtratum infunde in cucurbitam, & igne leniſſimo abſtrahe phlegma: fortiore dein urge, ut bene fluat, & quaſi ebulliat: hoc facto, ſine ut ignis extinguatur, exime nitri diſtillati caput mortuum & in aere ſolve; ſolutum in loco calido exſicca, & habebis p. ſe. ſplendens

dens quidpiam. Vel ſi vis ut ſplendeat in quodam fracto fictili (∴ pſrrbus ∴) tunc accipe Verdig. & [hanc materiam] pone ſuper fruſtum fictilis cujuſdam in fornacem probatoriam, aut ſub veteri olla. Da vehementiſſimum ignem, ut bene fluat; exime & verte ſeu move fruſtum in omnes partes, ut liquor fluens ubivis fictili adhæreat. Reponas in loco quodam, ubi ab aere ſit immune, & habebis quod quæris.

I *Shall here inſert the preceding* Recipe, *as I met with it in* Engliſh, *by reaſon it contains ſeveral remarkable Things that are not in the* Latin.

W. DERHAM.

Baldwyn's Modus præparandi Phoſphori Hermetici.

TAKE *Spiritus Nitri*, about a Pound; put it into a Glaſs Body, and put into the ſame, as much as you can take up, with the Point of a Knife, of the common powder'd *Creta Alba*, then it will begin to ferment, or hiſs; and when it has done Hiſſing, put ſome more of the ſame powder'd *Creta*, and continue to do ſo, till it be ſatiated; hereupon the ſaid fermented Spirit, by reaſon of precipitating many *Fæces*, is to be filtrated *per Chartam Bibulam*, and afterward diſtilled off, by a Retort in Sand, untill it coagulate itſelf, *in Fundo*, into a white Salt: Which muſt be kept carefully from the Air, becauſe otherwiſe it very eaſily runs into an Oil. Afterwards, when you would prepare it for the *Phoſphorus*, there muſt be a Proof-Furnace, with a Muffel, well heated, till it be red-hot.

N. B. In the Government of the Fire lies the main Buſineſs; for if the Proof-Furnace be not

hot enough, then the Salt flows, or aſcends, not orderly high enough; but if it be too hot, then the Sulphurous Niter evaporates; then there is put, of the aforeſaid Salt, two Lote (an Ounce) in Proportion of the Space, into a Proof-Pot, (or Crucible, wherein they make Ore to boil) and ſet it again into the Proof-Furnace, under the Muffel, and then the Salt doth preſently run into a Water, but ſoon hardens again, and then runs and mounts up again, that the whole Proof-Pot, in the mean Time, is cover'd; but ſoon after that, the Gold will more and more conſume it ſelf, that only in the Midſt of the Crucible, the Powder 1, 7, 5, 19, 2, remains only with a little Moiſture, wherein it muſt be well obſerv'd, that as ſoon as the Border of the Crucible is dry, though in the Middle there appear ſome Moiſture, the ſame Crucible be ſuddenly taken off, and let cool of itſelf. If the Work ſucceeds well, then the Brim will be yellow altogether; which (Firſt,) *Ex Aere* attracts the Fire, and in the Dark caſts it off again. (2dly,) In the Night, when you hold it to your warm Body, in your Bed, it ſhines. And, (3dly,) When in the Evening you ſtrike it with a Bruſh, or Feather, or ſmall Piece of Wood, ſomewhat hard, it cauſes very bright, fiery Sparks. But the ſame Crucibles will not laſt long, becauſe they attract ſo much of Air and Moiſture, *Magnetice*, and moulder at laſt: Therefore, at the Beginning, I ſet it in a Pewter Box, covered with Glaſs, half the Body cut off from the Neck, and well luted, the ſame to make it keep the longer. But if you would have the *Phoſphorus* in the Figure of a Star, then you muſt not only have the *Sal*, but many Crucibles; and when the ſame are prepared, as formerly, then only that, which is yellow and ſhining at Top, muſt be ſcraped off, upon white Paper,

Paper, till there be a pretty Quantity of the ſaid Powder together, according to the Proportion of the Star intended: Hereupon one takes a ſmall round Looking-Glaſs, whoſe Foil is not made of *Mercury*, but *Lead*, in which cut therein a Star; then, after the Powder is mixed with a little white Wax, melted and heated in a Silver Spoon, over the Coal Fire, well ſtirred, with a little Stick; then this Mixture, while yet melted, is poured on the back Side, or hollow Side of the Glaſs, (which alſo muſt be warm'd, leſt it break). Now as ſoon as the *Phoſphorus* is prepared, in Manner aforeſaid, then it is to be put into a Pewter, or Silver Box, and the Edges of the Looking-Glaſs are well ſecured with Sealing-Wax dropp'd upon it, round about; and then the Wax muſt be made handſome, and ſmoothed, and covered with Paper, either blue or gilded.

De Germinatione Metalli.

REQUIRITUR ad germinationem metalli. 1. Terra apta, in qua fiat germinatio, quæ eſt regulus ſtellatus, vel etiam regulus ſimplex. 2. Color. 3. Humor, quo fit imbibitio.

REGULUS conficitur ex antimonio, nitro, ſale communi & tartaro, æqualibus partibus, toties repetita fuſione, donec regulus fiat albiſſimus, inſtar lunæ. Regulo ſive terra philoſophica habita, itur poſtmodum ad praxin ſequenti modo. 1. Fiat amalgama terræ philoſophicæ & mercurii, qui eſt humor, ad germinationem metallicam pertinens; in hac unione proportio talis eſt obſervanda. Si vis germinationem *ſolis*, recipe *ſolis* ʒj, terræ philoſophicæ ʒx, fundantur ſimul & uniantur. Eadem doſis eſt *martis* germinandi. Argenti vero doſis

℥j, terræ philoſophicæ ʒv, eandemque doſin obtinent *ſaturnus*, *jupiter* & *venus*.

Hac unione facta ſequitur cum ea unio mercurialis hoc modo. *Rec.* Fruſtulum terræ philoſophicæ, idque craſſiuſcule contunde, nunquam enim uniretur, ſi redigeretur in pulverem. Huic terræ greſſo modo ſic contuſæ adjicias tantundem mercurii, miſceaſq; optime in mortario æneo tamdiu, donec totum ſit unitum. Dehinc accipe vas vitreum oblongi colli, ſed ventre inſtar pilæ rotundo, in ſui parte ſuperiore recurvum, non autem in ſui collo dilatatum, quia ad germinationem requiritur circulatio, non autem ſublimatio. In hac pila tumulabis materialia prædicta; inque pila aperta humidum mercurii excrementitium ſive ſuperfluum evaporabis. Facta evaporatione, pila hermetice figilletur; dein ponatur in furno clauſo, hypocauſti calore inſtructo, inque eo per menſem relinquatur: tum videbis metallum ramuſculos ſurſum emittere jucundos, cavitatem pilæ occupantes. Germinatione facta frangatur vas, & ramuſculi e ſua terra eradicentur, inque ignem denuo exponantur, ac denique cum aquis cordialibus abluantur, ſiccentur, & in vitreo vaſe ad uſum ſerventur.

Pro regulo etiam ſumere potes Electrum, quod fit hoc modo. *Rec.* Solis ʒij, lunæ ʒiiij, martis ʒiij, veneris ʒiiij, jovis ʒviij, ſaturni ʒxvj. Primo fundatur ♄, 2 ♃, 3 ♀, 4 ☽, 5 ♂. Sed adverte, chalybem limatum prius eſſe debere, & mixtum cum mercurio ſublimato & nitro, alias cum reliquis non uniretur: tandem & ſol funditur. Atque hoc ex omnibus mixtum conficitur electrum.

Hic pulvis blande admodum purgatus, obſtructiones contumaces domat, & viſcera roborat, ideoque in affixo hypochondriaco, hydrope & ſimilibus morbis prodeſt. Doſis eſt a gr. 2. ad 4. in ſyrupo, conſerva, & aqua appropriata. Eadem eſt doſis electri.

Toge-

Together with the Preparation of Baldwyn's Phoſphoros, *I find that of making what we call* Tin, *or* Latten-Plates; *which probably was communicated by* Baldwyn.

W. DERHAM.

The Way of making Latten-Plates.

TAKE tough Iron, that will bear the Hammer well; and having hammer'd it thin, ply it into the Size you would have cut your *Latten*; then put this Iron into a Mixture of Clay and Water, of a pretty Conſiſtence, and let it ſtand two or three Days; then take it out and hammer it again, as thin as you will have it for your Purpoſe; the aforeſaid Mixture, that ſticketh between the Iron Leaves, keeping them from being beaten into one another; then cut thoſe iron Leaves aſunder, with ſtrong Sheers, and throw by the Cuttings, as uſeleſs; then put theſe Iron Leaves into a Mixture of Rye-Meal, coarſly ground, and common Water, pretty thick, the Clay being firſt rubbed off, and let them ſteep therein four Days; then take them out, and dip them into a Kettle of melted Tin, but draw them quickly out again; then put theſe tinn'd Leaves between the Wires of an Iron Bar, made with Wires fit for this Purpoſe, that the ſuperfluous Tin may run off, into a Pan to receive it underneath. And becauſe the Tin will grow cold at the lower End, and ſo thicker, in an Iron, an Inch deep, filled with melted Tin, dip the thicker Ends of your Leaves, one after another, and the hot Tin will melt down the Exceſs of Thickneſs, but you muſt take them out again quickly; and, with a woollen Cloath, between your

your two Fingers, wipe them off beneath; which you will ſee to have been done, in all *Latten-Plates*, by certain Strokes appearing at one End. Theſe are made ſhining, by rubbing them all over with woollen Rags.

In Dr. Hook's *Diary*, Dec. 26. 1673. *I find this Remark*, viz. *Mr.* Yarrington, *who had ſeen the* Latten-*making Works*, *near* Leipſick, *ſaid*, *many Plates are beat under the Hammer*, *at once*, *like Leaf-Gold*, *or Tin-Foil. The great Difficulty is*, *how to turn them under the Hammer quick enough.*

W. DERHAM.

The Genuine Recept for making Orvietano.

RE*c.* Fol. Dictamni cretenſis recentior. herb. Cardui benedict. Pulegii regalis Hyperici & Scordii; radi. Ariſtolo. long. & rotund. Biſtortæ, Tormentillæ, Gentianæ. Imperator. Carlin. Scorzoner. Aſclepiad, contrayervæ Valerianæ, Angelicæ veræ, petaſitidis, bacc. Lauri & ſeminis Petroſelini & Dauci cretenſis ana partes æquales; & unicuique lib. pulveris, adde theriacæ Andro veteris & mithridati veri an ℥ij Poſtea reducatur in electuarium molle cum Extracto Juniperi baccar. vino albo parat. & in mellis cocti conſiſtentiam reducatur, redacti addendo ſub finem pro quaq; libræ electuarii ſemidrach. vitrioli cyprei in pulv. tenuiſſ. triti; & carnis viperarum exſiccatæ, pro quaque libra, ℥j. Hoc electuarium quotidie bis movere debes, per integrum menſem; deinde ad uſum repone.

This I tranſlate out of the Paris *Mountebank's Paper*, *in* French *more at large.*

N. LE FEBURE.

THIS

THIS is the Secret of *Orvietano*, and it is made by the Heirs of *Heronimo Ferranti*, who was the firſt Inventor of this rare *Recipe*. It is now come, by the Marriage of a Daughter, to the *Contugi*, the famous Mountebank, at preſent, at *Paris*; but it was given by *John Vitrario*, the Succeſſor of *Ferranti*, to the Great Duke of *Tuſcany*, for a Sum of Money; by whom it was ſent, fairly written, and put into a great Box, unto the late Monſieur *de Guiſe*; and by him, as a great Curioſity, to the Duke of *Bouillon*; from whoſe Phyſician, Monſieur *la Febure*, my good Friend and Correſpondent, that had often made Trial thereof, with great Succeſs, I receiv'd it as a choice Secret, at my laſt being in *France*, 1652.

J. EVELYN.

The Virtues.

TO expel Poiſon: Take the Quantity of a Bean, mix'd with Oil Olive, Butter-Milk, or Broth hot; drink three or four Times, till all the Venom be expell'd by Vomit.

After which, let the Patient ſup up a good Draught of Broth, very fat, with an Ounce of *Mel Roſarum*. If any be bitten with a mad Dog, or Serpent, take of *Orvietan*, as before, in Wine; then ſcarify the Bite, and draw Blood, *per cucurbitam*, to which apply *Orvietan*, keeping the Patient waking 12 Hours.

IN Agues, Fevers, Exanthems, and all Contagions, *Rec. Orvietan* in ſome Borage, or Scabious Water, the Weight of a Crown in Gold; but to a Child, in a Fever, cauſed by the Small Pox, not exceeding the Weight of a Bean, taken in White-Wine; the Child well cover'd.

IT

It preſerves from the Peſtilence, taking the Quantity of a ſmall Button. Taken alſo in Wine, Broth, or a Pill, in the Morning, it corroborates the natural Heat, aids Digeſtion, hinders Pains in the Stomach, Difficulty of Reſpiration, ſtinking Breath; cures cataractical Vapours and Diſtillations, the Cholick, windy and rhenal Spleen, *Dolores Matricis* (except *in Gravidis*) kills Worms in Children.

For Cattle that have Swelling, and Pains in the Belly, 'tis very excellent, giving them a Drench in half a Pint of White-Wine, warm. *Orvietan* will keep 25 Years, and more, in a cold Place; or it may be reſerved in Powder, and put into a Conſiſtency, with *Mel Roſarum* at Pleaſure.

Ink for the Rolling-Preſs.

The beſt Black is the *German* Black, and comes from *Frankfort*; it looks like Velvet, and eaſily crumbles betwixt the Fingers, like Chalk. Of this there is a Counterfeit, made of Lees of Wine burn'd, which is full of Gravel, and very pernicious to Plates.

Take excellent Nutt-Oil, and put a good Quantity thereof into a large Iron Pot (which has a Cover exactly fitted to it) ſo as to fill it within three or four Fingers Breadth of the Top; cover it, and hang the Pot, or ſet it on a Trivet, over a good Fire, till it has boil'd; but have a Care that at firſt it boil not over, nor yet when it boils; for 'twould indanger the Houſe. Therefore, diligently obſerve it, and frequently ſtir it with an Iron *Spatula*. Then being very hot, kindle it with a Piece of Paper, lighted. Having thus taken Fire, remove it from the Trivet, into the Chimney-